GULF OF MEXICO FISHERY MANAGEMENT COUNCIL

MEETING OF THE STANDING & SPECIAL REEF FISH, SOCIOECONOMIC & ECOSYSTEM SCIENTIFIC AND STATISTICAL COMMITTEES

WEBINAR

SEPTEMBER 27-30, 2021

STANDING SSC VOTING MEMBERS

James Nance.....
Lee Anderson.....
Luiz Barbieri.....
Harry Blanchet.....
David Chagaris.....
Roy Crabtree.....
Benny Gallaway.....
Douglas Gregory.....
David Griffith.....
Paul Mickle.....
Trevor Moncrief.....
Will Patterson.....
Sean Powers.....
Steven Scyphers.....
Jim Tolan.....
Richard Woodward.....

SPECIAL REEF FISH SSC VOTING MEMBERS

Jason Adriance.....
Michael Allen.....
John Mareska.....

SPECIAL SOCIOECONOMIC SSC VOTING MEMBERS

Luke Fairbanks.....
Cynthia Grace-McCaskey.....
Jack Isaacs.....

SPECIAL ECOSYSTEM SSC VOTING MEMBERS

Mandy Karnauskas.....
Joshua Kilborn.....
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Lynne Stokes.....
Nathan Vaughan.....SEFSC
John Walter.....SEFSC
Bob Zales.....Panama City, FL

- - -

TABLE OF CONTENTS

TABLE OF MOTIONS

- - -

The Meeting of the Gulf of Mexico Fishery Management Council Standing and Special Reef Fish, Special Socioeconomic & Special Ecosystem Scientific and Statistical Committees convened on Monday morning, September 27, 2021, and was called to order by Chairman Jim Nance.

INTRODUCTIONS
ADOPTION OF AGENDA

CHAIRMAN JIM NANCE: Good morning. My name is Jim Nance, and I am the Chair of the Scientific and Statistical Committee of the Gulf of Mexico Fishery Management Council. We appreciate your attendance on this webinar and input into this meeting. Representing the council is Martha Guyas. Council staff in attendance include Carrie Simmons, John Froeschke, Ryan Rindone, Lisa Hollensead, and Jessica Matos.

Notice of this meeting was provided to the Federal Register, email to subscribers of the council's press release email list, and was posted on the council's website. This webinar is open to the public and is being streamed live and recorded. A summary of the meeting and verbatim minutes will be produced and made available to the public via the council's website.

For the purpose of voice identification, and to ensure that you are able to mute and unmute your line, please identify yourself by stating your full name when your name is called for attendance. Once you have identified yourself, please re-mute your line. To signal you wish to speak during the meeting, please use your hand-raise function, and the staff will display your name. Please remember to identify yourself before speaking and to also to re-mute your line each time you finish. We'll go ahead and do that now, Jessica.

MS. JESSICA MATOS: Lee Anderson.

DR. LEE ANDERSON: Lee Anderson.

MS. MATOS: Luiz Barbieri.

DR. LUIZ BARBIERI: Luiz Barbieri.

MS. MATOS: Harry Blanchet.

MR. HARRY BLANCHET: Harry Blanchet.

MS. MATOS: David Chagaris.

DR. DAVID CHAGARIS: David Chagaris.

MS. MATOS: Roy Crabtree.

DR. CRABTREE: Roy Crabtree.

MS. MATOS: Benny Gallaway. Doug Gregory.

MR. DOUGLAS GREGORY: Doug Gregory.

MS. MATOS: David Griffith.

DR. DAVID GRIFFITH: David Griffith.

MS. MATOS: Paul Mickle.

DR. PAUL MICKLE: Paul Mickle.

MS. MATOS: Trevor Moncrief.

MR. TREVOR MONCRIEF: Trevor Moncrief.

MS. MATOS: Jim Nance.

DR. JIM NANCE: Jim Nance.

MS. MATOS: Will Patterson.

DR. WILL PATTERSON: Will Patterson.

MS. MATOS: Sean Powers.

DR. SEAN POWERS: Sean Powers.

MS. MATOS: Steven Scyphers.

DR. STEVEN SCYPHERS: Steven Scyphers.

MS. MATOS: Jim Tolan.

DR. JIM TOLAN: Jim Tolan.

MS. MATOS: Rich Woodward.

DR. RICH WOODWARD: Rich Woodward.

MS. MATOS: Jason Adriance.

MR. JASON ADRIANCE: Jason Adriance.

MS. MATOS: Michael Allen.

DR. MICHAEL ALLEN: Mike Allen.

MS. MATOS: John Mareska.

MR. JOHN MARESKA: John Mareska.

MS. MATOS: Luke Fairbanks.

DR. LUKE FAIRBANKS: Luke Fairbanks.

MS. MATOS: Cynthia Grace-McCaskey.

DR. CYNTHIA GRACE-MCCASKEY: Cindy Grace-McCaskey.

MS. MATOS: Jack Isaacs.

DR. JACK ISAACS: Jack Isaacs.

MS. MATOS: Mandy Karnauskas.

DR. MANDY KARNAUSKAS: Mandy Karnauskas.

MS. MATOS: Josh Kilborn.

DR. JOSH KILBORN: Josh Kilborn.

MS. MATOS: Steven Saul.

DR. STEVEN SAUL: Steven Saul.

MS. MATOS: Martha Guyas.

MS. MARTHA GUYAS: Martha Guyas.

MS. MATOS: Thank you.

CHAIRMAN NANCE: Okay. Thank you, Jessica. We'll go ahead and adopt the agenda. I do have one thing to add to the Other Business. We have --

MR. RYAN RINDONE: The National SSC topics.

CHAIRMAN NANCE: Yes, and so we'll add that to Other Business, and is there anything else to add for this meeting to Other Business?

Trevor, anything for change of agenda?

MR. MONCRIEF: I had a quick question on -- We covered a topic at the last meeting on the review of king mackerel historical harvest and catch limits, and we did have some questions on one of the models, where there was a little bit of disparity. I was wondering if this was going to be a subject that we cover at the next meeting, or do we ever receive clarification on it?

MR. RINDONE: You're talking about the presentation that we received that had looked at basically running the MRIP numbers through the old version?

We had received an updated version of that presentation that we had put back onto the website, and it essentially was a copy-and-paste error, and so Model 1 was the original model that used CHTS and all the SEDAR 38 from 2014, all of its terminal year of 2012, the old shrimp bycatch, et cetera, and then Model 2 used the same terminal year, the same shrimp bycatch, but changed everything over to FES, and that's what had resulted in an approximate doubling of what the ABC would have been.

Then, as you change the terminal year to 2017, and then, as you change the shrimp bycatch to the new median, using the new data, that ticked everything down, to where we ended up with the SEDAR 38 update, and so the logical progression of it held true, and it was just copy-and-paste through tables, and that was all.

CHAIRMAN NANCE: We did present that to the council at their meeting, and the presentation is on there for you to look at, Trevor, if you want to, but it really was just a copy-and-paste error, and there wasn't any issue with the models or the numbers that were associated with those. Thank you for bringing that up though. Any issues with the agenda, as it's been posted? Do we move to adopt the agenda?

DR. BARBIERI: So moved, Mr. Chairman.

CHAIRMAN NANCE: Thank you. Is there a second?

SSC MEMBER: Second.

CHAIRMAN NANCE: Thank you. It's moved to adopt, and is there any opposition to that? Thank you, and so we accept the agenda as suggested. Do we do Scope of Work now, Ryan?

SCOPE OF WORK

MR. RINDONE: Under normal circumstances, we would just kind of look at that as we went, but I think, given the size of this meeting, I kind of do want to walk through it a little bit. After this is going to be selecting a volunteer for representing the SSC's discussions at this meeting at the council meeting from October 25 to 28 in Orange Beach. You can attend in-person or via webinar. If you decide to volunteer, just let us know how you would like to attend, and you will need to put together a presentation, with the help of staff, based on the discussions that are had, and you can beg, borrow, and steal from any of the materials that are provided during this meeting.

We're going to talk about our final draft of the SSC's voting practices that the council was able to look at at its August meeting, and it's mostly pertaining to peer review and making sure that the SSC's peer review practices are in keeping with National Standard 2.

The Science Center is going to talk about a decision tree for making informed decisions on parameters for yield projections, and the idea is to get you guys thinking critically about the effects of different decisions and like the effects of those decisions on the projections, and ultimately on the stock, and Dr. Nathan Vaughan is also going to detail the new code that they're using for modeling yield projections, and that was briefly discussed at the last meeting.

Then that will lead us into the greater amberjack stock assessment re-review, and so Dr. Siegfried is going to go into the revised projections from that assessment and discuss how the new code affects everything and answer any questions that you guys have, and, if you guys like what you see, then you can recommend revised OFL and ABC recommendations to the council, as you think are appropriate.

We're going to talk about the terms of reference for the Gulf Spanish mackerel operational assessment, and this is going to be done in 2022, using data through 2021, and you guys have already approved the scope of work for this, and the last assessment found Spanish to be healthy. We're not presently recommending any topical working groups for this assessment, but, if you guys think that needs to be revisited, we would need to know that pretty soon.

We'll talk about the scope of work for the cobia operational assessment, which is scheduled to occur in 2025, using data through 2023, and, of course, we've talked a lot about cobia, and it's not in the best of places right now, and so the council is taking some actions, through Amendment 32 to the CMP FMP, to try to address

cobia's fishing effort needs. You guys will work out that scope of work, and then I will submit it to SEDAR and the Science Center for consideration for developing terms of reference.

Dave Chagaris is going to talk to us about some red tide ecosystem modeling work that he's been doing, and it's a NOAA RESTORE project, and you guys should ask him lots of very difficult questions. He loves those. Then we're going to go into SEDAR 72, which is the Gulf of Mexico gag stock assessment report, and Dr. Lisa Ailloud with the Science Center is going to walk us through all her hard work, and her team's hard work, with gag.

Then you guys will provide feedback to the Science Center team, and, if there's any modifications that you would like to see, and we do have a couple of days between when this presentation happens and the end of the meeting, but anything, obviously, that is too far in-depth may require us to have to come back to this again, but, if you like what you see, then you guys can make OFL and ABC recommendations as you think is appropriate.

Then Dr. Alexander Gordon from the Science Center is going to talk about a pilot project using field experiments to evaluate alternative methods for distributing fish to the recreational sector, and this project was designed to better inform distributions of fish that are being allocated to that sector, and so you guys should ask him questions and provide feedback.

Then, on the third day, on Wednesday, is Benny Gallaway day, and we're going to talk about the project that LGL Ecological Associates completed for the Louisiana Department of Wildlife and Fisheries for estimating total red snapper abundance in Louisiana and adjacent federal waters, and Benny and his team will walk you through everything that they did, where they did it, how they did it, their results, their responses to reviewer comments, et cetera.

On Thursday, Dr. Stunz will give you guys a presentation on the Great Red Snapper Count team's response to the peer reviewer comments from the March/April 2021 meeting that many of you were participants in, and he'll talk about the revised estimates for absolute abundance for that project and its associated variance, and you guys will need to consider, for providing advice to the Science Center for generating options for catch advice for our November 18 meeting, you will need to consider what you actually want to do, or what you recommend be done, for the State of Louisiana. Do you want to use the abundance data from the Great Red Snapper Count or the abundance data generated by the LGL study, or LDWF?

After that, on Thursday, we'll have a presentation on the Essential Fish Habitat Consultation process by Mr. David Dale from the NMFS Southeast Regional Office, and we'll be providing some insight on how EFH is identified in that whole consultation process, and you guys should ask questions, and I will review the SEDAR stock assessment schedule and the council's interim analysis schedule, and you guys can make any modifications and recommendations to the council there, as appropriate.

Then we'll tie things off with public comment and other business, where we'll talk about the main speaking topics for the National SSC Meeting and get some recommendations from you guys on completed research that fits the bill for the topics that that National SSC Meeting expects to cover. Mr. Chair.

CHAIRMAN NANCE: Thank you. We do have four full days of meetings, and I greatly appreciate each of your participation, and, as we have discussions and things, to be able to bring those up and to be able to come to consensus on the different topics that we discuss. Item Number IV, Selection of an SSC Representative for the Council's Meeting in Alabama --

DR. MICKLE: Mr. Chair, did we approve the minutes? Did I miss that?

APPROVAL OF VERBATIM MINUTES AND MEETING SUMMARY: AUGUST 9-11, 2021 HYBRID MEETING

CHAIRMAN NANCE: Thank you. We probably didn't. We skipped Number II. We did the agenda. Anyway, any edits or corrections to the minutes? Yes, sir.

DR. MICKLE: I have an amendment to make to the minutes. On page 313, line 35, we need to insert "restore" between the words "to" and "seagrass". Thank you.

CHAIRMAN NANCE: Any other corrections or edits or comments? Is there a motion to adopt the minutes?

MR. MONCRIEF: Just real quick, but, on my listing, it's listed as "Dr. Moncrief", and I will be "Mr. Moncrief". I just wanted to make that clarification.

CHAIRMAN NANCE: Okay. Any other comments or any other edits?

DR. MICKLE: Motion to accept the minutes. Thank you.

DR. BARBIERI: Second.

SELECTION OF THE SSC REPRESENTATIVE FOR THE COUNCIL'S OCTOBER 25-28, 2021 MEETING IN ORANGE BEACH, ALABAMA

CHAIRMAN NANCE: We have a motion to approve. Okay. Any opposition? Thank you. No opposition. For Item Number IV, Selection of an SSC Representative for the Council, I think I would like to go again, if that's okay with everybody. Okay. I feel it's good for that interaction, and I appreciate being able to do that.

For Item Number V, Final Draft for the Scientific and Statistical Committee's Best Practices and Voting Procedures, Ryan, do you have that, or --

MR. RINDONE: I do, and Jess will bring it up.

CHAIRMAN NANCE: Okay. Thank you.

FINAL DRAFT - SCIENTIFIC AND STATISTICAL COMMITTEE'S BEST PRACTICES AND VOTING PROCEDURES

MR. RINDONE: Okay, and so the council looked at the draft of the best practices and voting procedures that you guys looked at your August 9 to 11 meeting, and so, generally speaking, they liked what had been put together, and so this is kind of short, and this is going to be posted to the SSC's page on the council's website, so that people can see this.

There was some discussion at the council meeting about whether somebody had participated on the analytical team and what that actually meant, and what it meant to be involved in a project, et cetera, and we tried to provide clarification on that to the council, so that there was a good understanding all around of how we're trying to make sure that these voting procedures best align with National Standard 2, which talks about peer review, and, of course, the SSC serves as the peer review body for all of the operational assessments and for a lot of the other material that comes before the council.

You guys have had an opportunity to look at this, and are there any last-minute things that you would like to talk about with respect to this, or do you think that this is good to post? Mr. Chair, I see Doug Gregory has his hand up.

CHAIRMAN NANCE: Doug.

MR. GREGORY: Thank you. My question more has to do with the SEDAR

process, and my understanding is that, at one time, or currently, if you serve on a SEDAR data workshop or assessment workshop, you cannot then serve on a review panel workshop, and that seems quite strict, relative to what we're doing here. Has that changed, or is that still the case, and, if you're on the review workshop panel, what role do you have at the SSC level with that?

CHAIRMAN NANCE: Ryan.

MR. RINDONE: Thank you, Mr. Chair. If you serve on the data or the assessment panels, you can actually serve as the chair of the review workshop panel, but you just can't be a reviewer. As it relates to the SSC meeting, the way that we have interpreted this in the past is that there's been enough variation in participation throughout the process, and no one person is responsible for its entirety, that is on the SSC.

You would be able to talk about it, just as you would anything else SEDAR-related, and so, if you were -- Like, for scamp, if you were in Luiz's -- Well, I will use Luiz, since Luiz is here. Like, for scamp, when scamp comes before the SSC as an operational assessment, Luiz was on the review panel for the SEDAR 68 research track for scamp, and the council would fully expect that Luiz would contribute to all discussions, voting, et cetera, as it relates to scamp when it's brought before the SSC as an operational assessment. Does that make sense?

MR. GREGORY: Yes. Thank you.

CHAIRMAN NANCE: He would also be able to vote on whether it was the best available science or not.

MR. RINDONE: Correct. Luiz would be able to vote on BSIA, catch limits, et cetera.

CHAIRMAN NANCE: Okay. Thank you. Sean.

DR. POWERS: I understand what this is saying, as far as I was a PI, for example, for me, on the red snapper -- On the Great Red Snapper Count, and I shouldn't be voting on whether it's best available science or not, and I can later vote on catch advice, but, with your scope of work, it seems to open up another possibility, and that is should I be voting on whether the LGL study is best available science, because the way you presented it is, for Louisiana, we have to give advice on which studies we should use.

For the PIs, I wasn't involved in the Louisiana component of the

Great Red Snapper Count, but it still seems, to me, to be -- To present as a potential conflict, because, if we -- I would like some advice on that. How far to extend, and I know we can't vote on our own study, but what happens when there is a study that -- At least the way you set it up in the summary is it's an either/or, and that could potentially be a conflict as well.

CHAIRMAN NANCE: Sean, thank you for that. You know, I've been thinking, and this is just my own thoughts here, and we can have a little discussion here, but, as I was looking at the agenda and looking at what we had to do this week, in my mind, the Great Red Snapper Count is four different research -- While it's under one report, it's four different research projects.

There is the Florida component, there is the Mississippi-Alabama component, and there is the Louisiana component, and then there's a Texas component. In my mind, those are -- While they are incorporated in one giant report, in my mind, those are four separate overseen projects, and so I would think, and we can -- You guys can correct me if I'm wrong here, but I would think that, for the Florida stuff, when we're voting on Florida, that, Sean, you would be able to vote on that, but Will would not. For the Texas component, you would be able to vote on, and others that were directly associated with the Texas one would not.

That way, each of those four things can be voted on separately, and so Louisiana, the Louisiana, component, we're either going to look at the Great Red Snapper Count numbers or the LGL numbers for Louisiana, and that's just kind of, in my mind, what we would like to do, and are there any comments or discussion on that? Luiz.

DR. POWERS: I will respond, and I understand where you're going, Jim, and I think that is the case for Louisiana, because of the way that we had to deal with Louisiana and the Great Red Snapper Count, and it has different -- I guess I disagree with that, because, I mean, all the PIs signed-off on the Great Red Snapper Count, and that's not to say that some didn't have concerns about other regions besides theirs, but it's -- The thing the reviewers really pounded us on, or got caught up on, was this issue of additive, whether the regions were additive.

I think all the PIs agreed to the whole project, but I wouldn't parse it region-by-region, and I would feel uncomfortable that way. I realize that means that, if we're deciding between the LGL study and the Great Red Snapper study, versus other sources of information, that really limits the number of SSC members that can vote, but I wouldn't treat it as piecemeal like that, because the whole point was the statistical design and things that were common

throughout the region were agreed upon by the PIs, and the question of additivity was one of the main ones the reviewers looked at.

CHAIRMAN NANCE: Okay. Thank you, but, in my mind, Sean, if we voted on the LGL study as the best available science, would that then preclude anything in the Great Red Snapper Count study? Do you see what I'm saying?

DR. POWERS: Well, the numbers are different. I mean, I don't know, if we said that LGL was best available, and we also had previously said that the Great Red Snapper Count is best available -- I don't know where that would leave the analysts.

CHAIRMAN NANCE: Luiz.

DR. BARBIERI: Thank you, Mr. Chairman. Actually, Sean already answered the question that I had. Thank you.

CHAIRMAN NANCE: Will.

DR. PATTERSON: Sean raises an important point here, but, as far as this idea about best available, the LGL results weren't available when the red snapper count report was reviewed, and so I think that alleviates some of the issue there.

As far as the sort of blanket endorsement comment that Sean made, there's lots of discussion within our group about issues that weren't solvable, based on how the study was conducted or produced, and I certainly didn't sign a blanket statement that says that I fully endorse every aspect of this report.

I think, even in your own work, people understand that there are potential issues that couldn't be addressed or should be revisited, perhaps, with future research, and so I'm not sure that that conflict exists either. I think this is an important thing to discuss, but perhaps we should review the new work done off of Louisiana first, and then we can decide on some of that.

CHAIRMAN NANCE: Okay, and I appreciate that. Thank you, Will. Ryan.

MR. RINDONE: Thank you, Mr. Chair. Under National Standard 2, there is a provision for a peer review that allows for exceptions to be made under circumstances where the professional experience necessary to provide an adequate review is narrow, and the review itself could only benefit from the additional participation of those whose need for a recusal may be questionable. I think, under the circumstances that we're talking about here, and Will brings

up a good point. At the time, when the Great Red Snapper Count was reviewed, the LGL study didn't exist, and it wasn't completed, and so it wasn't available to be considered for BSIA.

Picking on Dr. Gallaway, Dr. Gallaway was available to vote on that, and so, under these circumstances now, the Great Red Snapper Count has been completed, and you guys have already passed catch recommendations, at least partially, based upon it. I think, under these circumstances, it's an easy argument to make that this review of the LGL study, and its comparison to the Great Red Snapper Count study, it may only benefit from increased participation.

I mean, at this point, we're not -- Like with the response to reviewer comments for the Great Red Snapper Count, we're not asking you guys to redeliberate that topic, and it's more informative than anything, and so the Great Red Snapper Count is done, and the response to reviewer comments are completed, and the report is published. There are no more changes that are to be expected from it, and so, even if you guys did have any other recommendations, it would purely at the pleasure of that team to do anything different, which I don't expect to be something that they're in a position to do at this point.

If the participants from the Great Red Snapper Count were to be involved in the voting for whether to use the data from the Great Red Snapper Count or to use the data from the LGL study for Louisiana, based on National Standard 2 and the body of people that we have that are directly knowledgeable and have directly applicable expertise to look at the biology and ecology and the physical oceanographic characteristics and everything else that's involved with these studies, and it's considerable, I think we would be remiss to exclude any expertise, aside from those directly involved in the LGL study. I just offer those comments for discussion to you guys.

CHAIRMAN NANCE: Thank you, and I think we'll get there on Wednesday and Thursday. Sean.

DR. POWERS: That's fine. I mean, my question was -- I didn't mean for a long discussion, but it was more towards -- Ryan answered it, essentially, and I just wanted that, in my mind at least, before we went into the LGL discussion that will be later, but Ryan's comments are essentially all I wanted to hear. Thank you.

CHAIRMAN NANCE: Thank you, Sean. I appreciate that. Ryan.

MR. RINDONE: Thank you, Mr. Chair. Sean, a lot of this is just

developing a record for why certain decisions are being made with respect to that and making sure that we have some discussion on the record and justification for those decisions.

CHAIRMAN NANCE: Okay. Benny.

DR. BENNY GALLAWAY: I was just hoping that everyone would in fact vote -- Review and vote on the Louisiana study that participated in the Great Red Snapper Count, and I assume that will be the case.

CHAIRMAN NANCE: Yes, and thank you for that. Let's go ahead, and we've all read this, and can we move to adopt this as written?

DR. BARBIERI: If you're looking for a motion to adopt, so moved.

CHAIRMAN NANCE: Okay.

MR. MARESKA: I will second.

CHAIRMAN NANCE: John, thank you. **Any opposition to that motion?** The motion is to adopt the best practices and voting procedures.

MR. RINDONE: As written.

CHAIRMAN NANCE: The motion is to adopt the SSC best practices and voting procedures as written. Any opposition to that? It's accepted without opposition. Thank you. Now we'll move into Item Number VI, and, Katie, you have the floor.

DECISION TREE FOR MAKING INFORMED DECISIONS ON PARAMETERS FOR YIELD PROJECTIONS

PRESENTATION: DECISION TREE

DR. KATIE SIEGFRIED: Hello, everyone. I'm sorry that we can't be there for another SSC meeting. Hopefully it will happen in the new year. I hear you have doughnuts though, and so we'll jealous in the virtual world.

Today, we want to show you some work that we've been doing at the Center to provide some -- The beginning of a discussion, really, about what decisions we want to make explicit and eventually decide on for our yield projections.

I have an outline for you here, and the first thing we want to do is make the decisions about projection settings more explicit to you all. It became obvious to us, after the last SSC meeting, that sometimes these things get glossed over a bit, and we don't have a full discussion about the assumptions and decisions made in

our projections, and so we want to make that more explicit, now and going into the future.

The next part is to show the implications of those decisions, and we'll show that using an example set of projections with our greater amberjack case study, and then we want to explain and demonstrate our new code, which is Dr. Vaughan's code, to supplement the forecasting capabilities in Stock Synthesis when considering allocations, and so I think some of the issues that we didn't present that well last time, but, really, his code is most important when we're considering those allocations. Then we had a request from the SSC to add MSY into the mix here, and so we will do that at the end.

What decisions need to be made? We have this table on the right, and this is generally provided in all of our stock assessment reports, at the projection -- Once we get to the projections at the end of a stock assessment, and the parameters that we have to decide on are the left-hand column of that table, and this is the actual table that you were provided for amberjack, and the decisions are as follows. We have to decide on the years over which to average our fishing mortality, and that's that first row, and the next two rows are the years over which we want to average the selectivity and retention parameters.

The fourth row is how to set our recruitment, which has a huge impact, as I will show later, and what to use for interim landings is second-to-last there. For amberjack, we used 2019 to 2020 landings, and then it was modified at the SSC for 2021, and then whether we have an allocation ratio, and so, for amberjack, for this example, it's 27/73.

The ones that we most frequently discuss are the ones that I have highlighted here in green, and we often don't talk about the years over which to average our fishing mortality, although we could, and I have seen it done before, but, in the Gulf, in all of my searching back into the decisions made, we rarely have done that in the Gulf, and so, really, we need to look at selectivity and retention, how to set our recruitment, what interim landings to use, interim meaning the years between the terminal year of the assessment and the first year of our projections, or, well, the first year that we expect to actually be able to implement management and our allocations.

The impacts of our management and changes in fishing, or fishing behavior, affects that first decision about selectivity and retention, and so we regularly use the last three years of the model to inform selectivity and retention patterns into the future.

This has been what we've done most often, though I have seen deviations from this, and then that decision should be made thoughtfully, by considering whether recent management may have changed the retention pattern.

You will see, later in gag, there is some retention blocks closer to the terminal year than three years out, and so we might want to -- We did recommend this, but we might want to actually look at management in the last three years of the assessment, to make sure we're not just blindly applying three years because that's what has been done before, and so has there been a more recent size limit change, could we even consider a bag limit change, or even the effect of regulations on other species to the one that we're assessing, and then has fishing changed recently that would affect fishing behavior, something like the multispecies fishery considerations.

Then the next decision is how do we think about future recruitment, and so I'm going to go through these like as a thought process, and then I'll show you an actual example.

If we have a well-estimated, or informed, steepness value, we do think we can consider using recruits from the stock-recruit curve. If it's not well estimated, or we may not have a basis to inform steepness, we will have to rely on the model-estimated recruits. It's kind of an either/or, and so that's a branch in our decision tree.

Then, if we're going to use the model-estimated recruits, what time period should be used to pull those estimated recruits? We have several choices, obviously. We can use the whole model timeline, or we can use just the period over which we have rich data, and so when composition data come in or when we think we have good sample sizes in the model for those comps.

The more recent period, which is a very common setting that I noticed in all of the recent assessments, but we have to be cautious of this, because it could imply a regime shift, and I will explain that in a minute, and then we recommend that the recruits from the model be plotted to show what each assumption provides.

This is what I mean by plotting the recruitment, and so we want to consider the uncertainties and whether using a sub-sample of the recruits is implying a change in future productivity, and so I have a plot here by year of the number of recruits that is estimated from the model, the early time period, and this is actually an example from cobia, and the early time period has, you know, a

pretty heavy level of uncertainty and is really just fixed at equilibrium, and then the last few years of the assessment model estimate quite high uncertainty around the recruits as well, and so it is important for us to take a look at the uncertainty around the recruits that we're averaging over.

Here, we've shown an average from 1982 to 2018 that is very similar to the recent estimated average, and so 2014 wasn't the terminal year of the model here, but it was the terminal year of estimated recruits, and so this is just showing you that, in this case, you weren't assuming a regime shift, if you look at recent averages, because it's not that different form the average over a longer time period.

Then a question that was brought up is should we use MSY or a proxy, and, if we have a well-estimated or informed steepness, and, again, steepness is key here, we can consider using an MSY. Oftentimes, we have a benchmark that's in the books, and that matters. We can't deviate. As we're doing the assessment, the analyst can't say, oh, we're going to use MSY, because, oftentimes, the TOR says you're using the proxy that was established at the last assessment, or maybe a few assessments previously.

If an SPR proxy is specified in the TORs, say, or on the books, and a new assessment can estimate steepness, the SSC does have the ability to decide whether to make a new recommendation about the use of a proxy, and we'll show that later as well.

Then how do we decide whether the key parameter of steepness is well estimated? That's something we did for amberjack, and we have done for all of the assessments that I looked at recently that are on the books. We used diagnostics such as a likelihood profile, or contour plots, which is a two-dimensional likelihood profile, and we can take the stock-recruit parameters and plot them in two dimensions and look and see if we can find some minimum in that likelihood surface.

We want to consider all three stock-recruit parameters when we're making these decisions about recruits or making decisions about MSY, and all of this can be shown prior to making decisions about these projection settings, and so steepness, virgin recruits, and the uncertainty of the estimates of recruits, sigma R, for the three parameters that I'm talking about, and then, across that likelihood surface, we're looking for the minimum of the total likelihood.

If that minimum agrees with what the model has estimated, then we have our evidence that the parameter is well estimated, or, if

there's one value, or perhaps a range of values, for each parameter, we want to look at the correlation between them.

On the bottom-left, we have the contour plot from vermilion snapper, and the steepness is on the X-axis. The sigma R is on the Y-axis, and I circled, in red here, because the shades of blue are a little bit difficult to differentiate, and the red shows that, between about 0.5 and 0.3 of sigma R, at about between 0.7 and 0.9, we have a minimum inner-likelihood surface.

That still may not be sufficient in people's minds, but it's important to show where the minimum in this contour plot is, and then, on the right-hand plot, we have a profile likelihood, where we're looking at virgin recruitment, and, overall, those values, the minimum of that dark-blue line, or maybe black line, that is where the model estimated our R0 to be, and so we did see strong evidence, in this model, for the R0 estimate, and so we want to look across all of these and decide whether we think we have good information in the model to move forward with something like MSY, that is linked so intimately to the stock-recruit parameters.

We also want to think about what our target is and what the implication of that target is, which Dr. Vaughan will talk a lot more about this in detail, but, in the past, in our projections, and our presentations to the SSC, we have used the SSB ratio, or depletion, which is SSB over SSB_0, or the SPR ratio, which is SSB over SSB SPR 30 percent, depending on the stock, if it's 30 percent.

There has been very little discussion about the differences, or merits, of each approach, and we'll do that in Nathan's part of the presentation, and, when projected recruitment is close to R0, these two targets yield similar results, and so maybe the lack of discussion has been because, in a case or two, it's pretty similar, and so we hadn't discussed it openly in all of the other assessments, where we might have used one or the other.

I have just been talking, using words, to explain what these decisions are, but I do think it's important to look at the quantitative implications of each of these assumptions, and so what we want to do, and I did talk to Ryan a little bit about this, because the two scheduled presentations, or I guess three, mine and Nathan's and then the review of SEDAR 70, it's kind of hard for us to just shift over to SEDAR 70, because our projection decisions decide the benchmark, and so there's going to be a little bit of a flow between these two presentations, and hopefully we answer all the questions, but we can always come back to this, if we want to revisit for greater amberjack in particular.

What we want to do is demonstrate the effects of all our assumptions using the greater amberjack assessment and projections as our case study. We want to show you the benchmarks, the expected stock size, the expected short-term and long-term yield that you get when you make certain assumptions or decisions, and then what our rebuilding target looks like.

Then the final piece of the puzzle is how the allocations change those benchmarks and the resulting yield, and so we had a request for allocation projections, and that led to the presentation last time, and so the final piece will be to fulfill that request with new projections that include all of these decisions that we have made by both Science Center suggestions and the SSC's discussion and decisions.

I would like to pause, in case there is any questions, because I did go on quite a lot, and I can't see your faces, and does anybody have any questions before I show you the greater amberjack case study?

CHAIRMAN NANCE: Any questions? Roy.

DR. CRABTREE: Hi, Katie. Thank you for the presentation. I am wondering how you -- Have you thought about how to handle fisheries where the allocations are much more complex than just recreational and commercial? For example, in red snapper, you have allocations specified for the for-hire versus the private and then the private fishery is allocated out to each of the states, and so have you all talked about that, or is this able to handle that?

DR. SIEGFRIED: That's an interesting question, Roy. I mean, I think that Nathan would be better suited to answer whether his code can do that. I think that what we have right now, split up into just commercial and recreational, I think it's just a matter of sub-setting it, but, if Nathan can answer that. I think his code can, but I would like to ask him how complicated it would be to implement.

DR. NATHAN VAUGHAN: Yes, we can very much handle that with the new code, and, to a degree, with the old code, and so, yes, we can handle the allocation specifically amongst fleets that are within the stock assessment, and so the place that that gets complicated is with the state-specific allocations within a single fleet, and we don't have any estimates on what the different selectivities are amongst those fleets, and so there is no way to split that out in a meaningful way, and so it's limited to what is in the stock assessment. We can handle any allocation, and we can handle

separate allocations to every single fleet that the stock assessment specifies.

DR. CRABTREE: Okay. Thank you.

CHAIRMAN NANCE: Doug Gregory.

MR. GREGORY: Katie, that was a good presentation, and this is very interesting. My question, or questions, are about the recruitment. In the graph you showed, in this instance, the last estimate of recruitment had a lot of uncertainty, and you kind of emphasized that, but isn't that always the case, that the last year or two that you're going to have greater uncertainty than the previous years?

DR. SIEGFRIED: Yes.

MR. GREGORY: If so, why? What's the big deal about it? It's just the function of the modeling itself.

DR. SIEGFRIED: Yes, the terminal year estimate of recruits definitely is more uncertain, and we have a lot of literature out there that talks about whether we should estimate recruitment all the way out to the last year. Sometimes we scale that back, if there is composition data that -- Okay, and so it's two things.

It's whether we have a good amount of composition data and also when the stock is selected for, and so we want to scale that back, so that the last year -- If we have a fleet that actually catches age-zeroes, or age-ones, it's okay, it's more okay, to estimate recruits to the last year, but, if we don't, say for amberjack or -- I'm sorry, but for cobia that I showed there, and if it's three or four, then we don't want, necessarily, to estimate recruits out to the last period, and we want to assume equilibrium recruits for those years.

MR. GREGORY: My inclination would be that unless that estimate, the median estimate, is outrageously different than recent years, then it's an acceptable value, and I wouldn't be concerned about the uncertainty.

My other question about this graph is you've got a dashed line going back to 1982. Is that the range of years that you consider data rich, because I would consider data rich to really not begin until about 1990.

DR. SIEGFRIED: This is all up for discussion. This is just a matter of we need to take a look at these things, and so, for that

one, if you go back a slide, that was probably -- It's 1981 there, and that's the first year of MRIP, and so 1982 might have been -- For cobia, there might be an issue with the 1981 estimate or something, and we look at when the data come in.

I would argue that we need to look at when the comp data come in and when we think our comp data are most representative, and so I am not arguing that this is what we should use, but this is just what the SSC has seen in the past, and we need to be much more thoughtful about the way that we consider these, and so I totally agree with you, Doug. If we don't think our comp data are as good until 1990, that's probably when we have the best estimates of recruits.

MR. GREGORY: Right, because I am harking back to the extremely variability in the MRIP numbers, MRFSS numbers, in the 1980s and the troubles that were happening with dockside sampling back then that prompted the states to invest a lot of money into increasing dockside sampling after that. Okay. Thank you very much.

DR. SIEGFRIED: Sure.

CHAIRMAN NANCE: Thank you. Dave.

DR. CHAGARIS: I also have a question about the recruitment. You said that it typically had to be either the stock-recruit curve or some average level of recruitment could be used in the projections, but it seems like, in most cases, you could still use the stock-recruit curve, and I understand that maybe the parameters might not be estimated very well, but, if you did use the stock-recruit curve with the standard deviations, given the sigma R, couldn't you -- Wouldn't that allow us to incorporate more of the uncertainty into the projections, and understanding that you may have to do more stochastic runs with the recruitment deviations, but you could capture that uncertainty.

You could also then inform those future recruitment deviations, either with like time series forecasting from the terminal year, or even an environmental signal, and so I guess a question is should we always be throwing out the stock-recruit relationship and those parameters and going with a new recruitment, because that stock-recruit curve is what the model is calibrating, under that assumption, and carrying that through to the projections would seem like a reasonable way. Thank you.

DR. SIEGFRIED: Dave, I think that's right on point, actually. When Nathan talks, he's going to talk about some funded work that is going to look at incorporating recruitment deviations, rather

than pulling the estimates of recruitment, and that's what other regions do, in some instances.

I am purely showing you what our capabilities are at the moment and what the decisions have been in the past, but, I mean, I think recruitment deviations are a good way to go, and I don't think the stock-recruitment curve should be ignored a priori, and I think it's something that needs to be discussed. It certainly shouldn't be used if we're doing the fixing it at 0.99, just for computational convenience, because then we don't think that the stock-recruit curve -- That there is a real relationship between stock and recruits, but, yes, and we just talked about this at the scamp review.

If we have a steepness, even from literature values, doesn't that provide some information about the potential for recruits in the population? If we believe it enough to fix the steepness at that value, then maybe we shouldn't be throwing out the stock-recruit curve just because we can't estimate steepness. It's certainly up for debate and not something that we have done in the Gulf, historically, but it's important to have this discussion, but we just haven't talked about it like this before.

CHAIRMAN NANCE: Thank you. Yes, I agree. It's one of those things that, instead of just doing it the same way for each species, we need to really look at the data and what mechanisms we need to have. Rich.

DR. WOODWARD: Thank you, Mr. Chair. I would like to follow-up on Roy Crabtree's question earlier about the allocations. When you analyze alternative allocations, do you also -- Are you able to estimate the economic value of those alternative allocations to the different sectors and evaluate what is the appropriate economic allocation across the sectors, and just a related question, and I would think that allocations would also affect fishing mortality, due to discards and bycatch and things like that, and so I'm wondering whether those issues are taken into account.

DR. SIEGFRIED: Thanks, Rich. The discards and bycatch are considered insofar as we just assume similar levels of discarding, and that is estimated at the end of the assessment, and that's how the retention function informs that.

We are completely separate from the economic analysis though. It is something that we're -- Since I've been on down here, I've had a number of conversations with the socioeconomic group about, okay, so how can we start to work together, and what I've heard is that it tends to come in sort of at the IPT level, when we start, like

you're saying, calculating the economic cost of these different allocations, but I am not part of that.

The folks in the Gulf Fisheries Branch are not part of that, at this point, and that's not saying that we shouldn't be, but we just haven't been up to this point, and that group is presenting at this meeting, and we are -- I mean, the Science Center is making a big effort to at least start communicating across groups and figure out ways that our modeling can inform the others.

DR. WOODWARD: Okay, and it's difficult, for me, as an economist, to even imagine talking about allocation without talking about the economic consequences of the allocation, and that's implicit in how those are done, and I think it needs to be explicit.

CHAIRMAN NANCE: Thank you, Rich. It is from a biology standpoint, from the modeling, and I think those allocations are made and then put into the assessments. Nathan.

DR. VAUGHAN: I just wanted to add, on top of that, talk of the allocations and the economic side, as was said, this was very much -- Our tools are designed to help estimate what that OFL should be under a specific allocation scenario and not to inform which allocation you should choose, and there are, obviously, a lot more decisions to be made into that.

From the economic side, we have done some investigation, in my work with John Walter on the decision support tool in incorporating those kinds of numbers into the projections. There was a fair amount of pushback, as we've worked with the social sciences group, that a single-species simple economic analysis are not necessarily that robust, and so it becomes a much bigger question of how the sector as a whole -- If you change the quota on amberjack or red snapper individually, how is that going to affect the fishery that's targeting a large number of fish simultaneously, and so it's somewhat bigger than a single assessment, but it is, on the face of it, not that difficult to incorporate a second layer of cost-benefit analysis on top of those allocations, but it's not something we're currently doing.

DR. WOODWARD: Thanks.

CHAIRMAN NANCE: Thank you, Nathan. Doug Gregory.

MR. GREGORY: Thank you. I just had one more brief comment about recruitment, and I inferred, from the reference to implying a change in future productivity, some caution there, but, to me, that's exciting, in that climate changes will probably show up

first in recruitment estimates and recruitment to the population, before we see other changes, and so considering that in our decision of what recruitment range of years to use could be very important. Thank you very much.

DR. SIEGFRIED: I agree, Doug, and I use caution there, not because I disagree with the use, or because there's something bad, but it's just, if we say, okay, we're using recent average recruitment for all of our assessments, we don't necessarily actually look at this plot all the time and say, oh, we do think we're going to have fewer recruits going in the future, and that's fine, and that's what we think, and to consider all the implications of that choice, and so it actually has quite a big impact, and so that's why I said we should use caution.

The other thing that I wanted to mention, to Rich's question, is I certainly -- I mean, I think that the economic analysis is important, and I do see that there is a lot of space between the assessment folks and the economists, and I just wanted to tell you that, in practice, when we get the allocation projection requests, it's after the model is completed, and they just give us a set of numbers, and we rerun our projections with it, and we might be on the IPT, to understand where those sets of numbers come from, but it's certainly not something that we're part of informing those choices beforehand.

I would really like to understand more about the impacts of those allocations on the economics of the fisheries, but it's just not what we're modeling at this point, and it's a separate group effort, or like Nathan, who is a researcher, and he's not one of the people that we task with an assessment, and so those people have more time to do that type of research, and so I think it's valuable, and I'm not trying to skirt the question, but I just don't know, and a lot of people on my team don't have the time to do that research while they're doing the assessment, and so we have to all work together to get all those different parts done.

DR. WOODWARD: Thanks. At the beginning of the presentation, one of the green bullets was allocation, and so it's sort of what caught my eye, and it seemed like that was a decision that was being asked of us.

DR. SIEGFRIED: Sure.

CHAIRMAN NANCE: I know, from what Katie is saying, is, during the assessment, the allocations are just given to them, and that's what they use, and there's a whole separate process that's being used to come up with the allocations for recreational and

commercial. Any other questions for Katie at this time? Harry.

MR. BLANCHET: The two options that were provided of either using a steepness or some sort of average recruitment, one of the things that seems to always come up with red snapper is that we manage to have the stock get better, even with some overruns, because we tended, in the model, to underestimate what happens in the earliest years of the projection period with regard to recruitment.

Basically, it sounds like we are on a lucky streak, but, with red snapper, we have a trawl index that informs recruitment going into the model. Has there been any thought, in terms of at least in the first couple of years of the projection, of feeding in some of these data streams that are going into the assessment as part of the projection of recruitment, and that might be the long way of talking about it, but that's my context. Thank you.

DR. SIEGFRIED: Sure, Harry, and so we don't often have the privilege of having a recruitment index. When we do have a recruitment index, I think it's a good idea to pay attention to what that index is doing, but it's still projecting -- If we projected that index, that's kind of like what we're -- Well, no.

What we're doing for interim analyses is look at what an index is doing rather than project the entire model forward, and so the thoughtful choice of which index to use is another presentation that we could probably provide, but I guess you could then look at the terminal years of that recruitment index and compare it to the recruitment estimates and see if somehow they're mismatched.

If they're not, then assuming some average of the estimated recruits would be consistent with that index, but, without projecting that index say into the future, I'm not sure how we would use it as any more than just a check, or the final results that came out of the assessment, and does that make sense?

MR. BLANCHET: Yes, and, obviously, it's not something that is going to be useful for more than one or two years out, at the best, and there's always some deviation from whatever that index provided and then what, a few years later, the fishery and the assessment observed, and so I'm not saying that we should jump on a good index and ride that horse as far as it will go, because I'm afraid that we're going to get bucked off into a barbed-wire fence, but it's just one piece of information that could be included in this projection that could help inform where we were going, and that was the thought.

DR. SIEGFRIED: I mean, I think, if we have a recruitment index,

that's even another branch of this decision tree that we could add, and certainly it's more valuable information in the terminal years, and I think that there has been that CMS index that's being considered, or has been used, for red snapper projections, but I think it would brought up, during the stock ID and the data scoping, that that would be available, and so that's certainly an explicit decision that we need to make when it gets to red snapper projection time.

CHAIRMAN NANCE: Thank you. Jack.

DR. ISAACS: Katie, I'm an economist, and that means that I'm one of the slower people on the panel, probably. You mentioned a recruitment index, and, if you wanted to do a recruitment index, or put one together, and this is for my own edification, but what variables would you want to collect first?

DR. SIEGFRIED: By recruitment index, I would say that we would have to collect the species that we're assessing at the right ages, age-zeroes or age-ones, to inform what's happening with those young-of-the-years, and so it would be just abundance, or catch rates, of those in the effort of that survey.

That would then help us scale that recruitment, and I don't actually think that it's true what you say about economists, and I cannot follow economics papers, and so I think that's being a little bit bashful, but that's what we would want to do, is collect those species at the right ages, so that we could say that that was a recruitment index that would be indicative of the trend of abundance of that age.

DR. ISAACS: Thank you very much.

CHAIRMAN NANCE: Thank you. Any other questions? Okay. Katie, let's go ahead and continue on.

DR. SIEGFRIED: Okay. We did have a few things that we need to clear up from the last SSC meeting, and so I wanted to take you through that, along with this case study approach, and I think that it will work, but we can also break it down, if we need to.

What you saw, or not all of you were on the SSC in 2021, and so, for those of you that weren't there, I just put it here again for you, and this is the table that was provided about those decisions, and we did not change the relative F, and we did not change the allocations for the base run projections from the model. We did discuss, with the panel, what the other assumptions were going to be, and then we discussed the 2019-2020 landings with the SSC.

The selectivity for the greater amberjack model projections was the average from 2016 to 2018, and then the retention was the same. The recruitment, what we presented was that we applied an average of 2009 to 2018, and then the landings are listed here. We needed to update those with recent landings, because, when the assessment process ended, we could add one more year of landings after the fact, and so we modified those interim landings a bit, as follows.

We had the commercial vertical line, commercial longline, charter/private updated, and headboat updated, and then we used the average over -- I think it was just 2021 was the average. Sorry. 2020 and 2021 were averaged, and 2019 was actual, and the allocation was held constant.

The decision there about what years to average selectivity and retention was actually informed by which years we had retention blocks in for SEDAR 70, and so, if you look at each of these, and so the commercial vertical line is the first bullet. The last time block, 2011 to 2018, you didn't see any problem there, where we would have had a time block in 2017 or something like that, and the same applies for commercial longline.

The last time block encompassed 1990 through 2018, and charter and private added together, and headboat, the last time block is 2016 to 2018, and so none of these time blocks indicate any argument against using those current years, the three-year average, for selectivity and retention and projections, and so that was a -- I think that was a good decision.

Then we moved to recruitment in our landings, and so, for the landings in the stock assessment report, I have just pulled straight -- I have just copied and pasted what was in the stock assessment report, and then the second bullet is the January SSC meeting, and we changed that, and so you can take a look at the commercial vertical line was reduced. The commercial longline went up slightly, and the charter private went down, based on the actual data in 2019, and then the headboat stayed pretty similar.

Overall, it was definitely a reduction, and I would say the biggest reduction was the commercial vertical line, and it reduced the assumed take in 2019, 2020, and 2021, actually.

Then we wanted to look at whether steepness was estimated, to decide what to do with our recruits, and so, in the model, our steepness was initially estimated, along with R0 and sigma R. We provided the SSC, and the panel, by the way, the profile on steepness and sigma R and that they were generally flat in the

area of that MLE estimate, though there was a contour on the surface, and so it wasn't -- What we're looking for is a few units of AIC, or a likelihood, to determine whether it's actually a minimum in that likelihood surface.

No prior was used for the model-estimated steepness, and it was still able to -- The model was still able to find that minimum in our likelihood surface.

This is what was shown to the SSC and the panel, to show that it was generally flat in this region, and so the profile, which is -- The steepness is on the X-axis, and the change in R likelihood is on the Y-axis, and you will see there is something strange happening at the 0.5 value, and there is probably some non-convergence going on, but that steepness generally reduces, from 0.4 down to around 0.7, and then it flattens out, and we wanted to look and see whether that minimum estimate of 0.77 was truly a minimum in our profile likelihood.

Between 0.7 and 0.8 is around equally likely, if you look at the likelihood values, and the lowest likelihood was at the steepness that was estimated in the model. This was from Figure 62 of the stock assessment report, and so steepness was carried through at 0.7, and I think we fixed it at 0.78, because it's not well estimated, as the panel decided, because it's not that different in likelihood, and it is a few units difference, if you look very closely, which I have another plot of that that I can pull up, if we need to, to look just between 0.7 and 0.9. This is the type of information that we need to look at, in general, and so is steepness well estimated.

Then, if we think, well, okay, and so the model estimated steepness, and what kind of information do we really have about the relationship between stock size and the number of recruits, and so the standard output, the standard plotting that we get from SS is the bottom-left, and this is showing that the green dots are not estimated, and only the blue dots are, and so this is a plot of spawning output on the X-axis, and recruitment on the Y.

Then I just pulled out the estimated recruits from the right-hand plot, and so this is in the data-rich period, and this is when we assume the bias ramp occurs, and this is when we have better composition data, although you could argue that there is even some issues with the comp data, like Doug pointed out, in the early 1980s, but this is when we assume that the data-rich period was for amberjack. You will see here, and you kind of have to squint to see if there's a relationship between spawning output on the X-axis and recruitment on the Y.

Then I don't think -- It wasn't explicitly discussed, but the SSC did move forward saying, okay, we need an estimate of -- We want to project with an average recruitment, and we presented to them this plot that showed the recent average from 2009 to 2018, which is what we propose moving forward, if a recent average was desired, and that's what had been done in multiple assessments beforehand.

However, you will see here that assuming a recent average is much lower than a longer-term average, and I provided a few comparison values here for you, and so the recent average is -- This is in millions of fish, and you will see 1,650. The long-term average, which is 1950 to 2018, which isn't that different from 1970 to 2018, is 2,805. Then our data-rich period, which we have as 1984 to 2018, is less than long-term and greater than recent, and that's 2,156.

That is when, like I said, the panel decided that the bias ramp should come in, and that's when we have our good comp data, but there wasn't year-to-year comparisons of what was going on in the early 1980s, to really address like Doug's question, and then I did want to note that there is high uncertainty in the historic and terminal recruitment estimates, and so there could also be an argument made that you would want to exclude the terminal years, and you could want to use an average when you thought that the uncertainty was lower, and so excluding the terminal years, terminal three to five years, would certainly be something that would be in the purview of the SSC to decide.

To get to Doug's question, and so the regime shift, and so, using a recent average for greater amberjack, we are inferring a regime shift, and, by that, I'm saying that we're allowing - We're telling the projections of the recruitment and, therefore, the productivity of the stock is lower and is expected to be lower and will always be lower into the future for this projection.

The expected recruits go down, and the affected stock size goes down, and, thus, our yield in the projections will be reduced into the future. A lower expected stock size will be easier to achieve and so the status is -- It recovers more quickly, and it may not be below MSST anymore if we don't expect it to reach a higher stock size, and so that lower stock size will be easier to achieve, and so the rebuilding will be faster at the same F, because that stock doesn't have to reach the higher threshold anymore, but the yield that the stock produces will always remain lower.

Then it's important to note this table on the right is something that you all receive in the stock assessment reports, and I didn't

provide the values, because it depends on the projections. I wanted to show you what, in this table, are actually dependent on the projections.

In the red boxes, we've got anything that's related to a proxy or a target is -- Those are key quantities in the status determination criteria that will change based on our assumptions of the projections here, and so, if we assume lower recruitment, you're going to get a different FMSY proxy, depending on which target you go for. Are there any questions about the regime shift point that I'm trying to make? I can answer any questions about what we're saying when we say "regime shift".

CHAIRMAN NANCE: Doug.

MR. GREGORY: Thank you. I got tripped up on the slide before this, where it says we're telling the projections that recruitment, and therefore the productivity, of the stock is lower. Okay. That makes sense, but then I thought I heard you say that therein the stock can be rebuilt more quickly, because our goal has changed. To me, that seems counterintuitive, if the productivity of the stock is lower because of overfishing or because of environmental change. That, in my mind, if it's less than what it's been in the past, it's a negative situation, and so it doesn't seem like we would be rebuilding the stock more quickly. Can you clarify that for me, or did I misunderstand something?

DR. SIEGFRIED: Sure. If we're assuming a lower level of recruits, but we have a set stock-recruit relationship, we're going to assume a lower level of stock is required in the future, if that's the amount of recruits we're going to be getting, and a lower stock size reduces the potential yield as well.

MR. GREGORY: But does it reduce -- It reduces the potential yield, but shouldn't we be comparing that future state to historical state? I mean, it seems like we're going down the hill with the population, and we're chasing it, and we're not rebuilding it.

DR. SIEGFRIED: Right, and that's exactly what I am -- I'm sorry.

MR. GREGORY: Go ahead. I just -- I have to think about this more.

DR. SIEGFRIED: Right, and so if you can go back to the slide with the recruits on it, and so, if we're assuming that we're never going to get to those levels of recruits that are estimated, like say before 1990 -- I mean, that's what assuming a recent average tells the projection model. It's telling us that, okay, we're never going to get above that, and so the stock size that was

required to get that level of recruits is no longer expected either.

I don't think this is something that you should immediately click in your brain or anything, and there's a lot more in Nathan's presentation that goes over this, and it actually matters quite a bit with our target as well, whether we assume that we need to get back to an SPR level or an SSB level, but allowing for projections to assume a low recent average basically tells the stock it's less productive and tells the stock, okay, you don't need to get as large as you used to be, because it doesn't have a used-to-be. It just has that recent time period, and so that's what it's telling the projection model is expected into the future. The projection model is separate from the assessment, and so it doesn't even see the past if we don't give it the past, and does that make sense?

CHAIRMAN NANCE: It does. This is something I'm having a hard time too with, Doug. Luiz.

DR. BARBIERI: Thank you, Mr. Chairman. Thank you for the presentation, Katie. This is very interesting, and I think super helpful to the SSC. I think of this as something that needs to be looked at differently if we are talking about providing -- Developing projections to provide catch advice basically for the immediate future, and so the next three years, to stay in line with how the SSC usually provides yield streams for OFL and ABC to the council, versus, when you're looking at a longer rebuilding trajectory for the stock as a whole, and so, when you are looking at this for the rebuilding plan --

I can see how looking at that entire history of information on recruitment, that takes into account all levels of stock productivity, and averages over the long time, would be more appropriate, because, of course, we are projecting, way into the future, and we know that that is highly, highly uncertain.

The longer forward that we are projecting, right, the higher the uncertainty that we know about the future, but -- So I can see that side of things, using the longer time series and accounting for everything that we know about productivity of the stock, but, and this is one of the reasons why we use dynamic models, is because we don't want to be basing catch advice just on an average, kind of like on a per-recruit basis.

I mean, we want to account for the fact that recruitment will be episodic, and it will be uncertain, and for the immediate future, let's say three to five years, and, in that case, to me, the most immediate future should be based on the recent past, if that makes

sense, and so, if we think -- I mean, I looked at your presentation. If we think, okay, there is -- We have found some level here of stock-recruitment relationship, and so we are accepting that recruitment, to some extent, is dependent on stock size, stock biomass, and so not accounting for the fact that the stock is changing in biomass over time, given, I guess, response to exploitation versus other factors, is not really correct.

To me, if we have a stock -- In your case study here, we have a stock that has failed to rebuild, for quite some time, and so we set up a rebuilding plan, and the stock failed to rebuild, and there has to be something that is preventing recruitment to be at its most productive level, and so, to me, in that case, using, I don't know, the last five or ten years, whatever time period, is correct for the immediate future, and, to me, that's different than looking at that long-term trajectory. If something is going to take twenty years to rebuild, we should be basing that on the entire time series of recruitment, reflecting the full range of productivity for the stock. I have some other comments for later that continue this discussion, but thank you, Katie.

CHAIRMAN NANCE: Steven.

DR. SAUL: Thank you, Mr. Chair, and thank you, Katie, for the presentation, and thanks, Luiz, for that feedback. I very much agree with that, and, along those similar lines, my question was that it seems that -- Well, first, I just wanted to say that I believe it's really important that these kinds of decisions about how we use recruitment -- Yes, it's useful to have a sort of set of standards that we can sort of refer to across stock assessments, but I think it's also critical that these sort of choices be made on a per-assessment basis, given the sort of idiosyncrasies of each assessment and the ability to sort of evaluate how well some of these things fit and then make choices based on that.

I guess, along those lines, it seems that, in this model, that you all were able to predict recruitment fairly well and leave those parameters free for the model to be fit, and so I was curious to get your take on whether, given that you were able to achieve what for most assessments would be considered a pretty good fit to recruitment, why not just project kind of using that relationship forward, rather than the last three years, or a five-year period, or whatever kind of average? Thank you.

DR. SIEGFRIED: Steve, your question is why didn't we use the stock-recruit curve, since we were able to estimate?

DR. SAUL: Yes. Essentially, yes.

DR. SIEGFRIED: That was something we discussed with the panel, and we discussed it internally, kind of at the last minute, and I think we -- I don't think we actually sufficiently covered that when we presented the greater amberjack model to the SSC the first time, and so it's something -- I know that Dave Chagaris asked us to show that this time, which we did, which we have later on in this presentation, but it's -- There is not a better answer for you there. We went with what the panel and then internal discussions came up with, and we presented it to the SSC, and we didn't really get any consternation about it, and so it certainly could be revisited at this point, and, like I said, we do have table of those values for you to take a look at.

DR. SAUL: Okay. Thanks, Katie.

CHAIRMAN NANCE: Thank you. Will.

DR. PATTERSON: Luiz asked and discuss things that I was interested in, and so I yield.

CHAIRMAN NANCE: Thank you. Katie, I have one question, and I'm just trying to wrap my head around it, because, at the end of the assessment, we come up with some type of stock size, biomass, that we're treating as our standard, and so we have a determination for that, and we have a stock size and an F that we're trying to get back to.

Then, if we use the recent average, as you've shown, which has a regime shift, and so we're having a real low recruitment, and that's what we're projecting, wouldn't the model -- We're having low recruitment, and wouldn't the model even further try to get F down, in order to get back to that original biomass, or does it just pick this new biomass and say that's as big as we're going to get to?

DR. SIEGFRIED: It picks the new biomass and tells you that's the biggest you're going to get to, and that's the short version of it, certainly, Jim. The reason it's that way is because -- What I was trying to get across before is that we finished the stock assessment, and the stock assessment does not have status in it yet, and we don't have the target yet, like an SSB or MSY or proxy.

When we do our projection, we put in those assumptions, the recruitment being a big one, and, if we give it a low recruitment to expect into the future, it's going to calculate, based on our equilibrium projection, what our SSB proxy should be, what our FMSY proxy should be, all of those things, based on the assumptions

we've given in the projection, and so that's one of the things that I was trying to clear up with this presentation, is the stock assessment ends, but it doesn't give status.

We actually have to get status through the projection. We actually have to get our equilibrium values that give us all those benchmarks through the projection, and so that's why it's even a different stock size assumed when we give it a lower recent average.

CHAIRMAN NANCE: Okay, and so, for each time we do a different projection, the other parameters will change, and is that -- The benchmarks would change?

DR. SIEGFRIED: The benchmarks would change and the target.

CHAIRMAN NANCE: Okay.

DR. SIEGFRIED: There is one version that's produced at the end of SS, but it doesn't have allocations, and it doesn't produce what we need to truly get at those proxies, and that's why we have to carry out those projections.

CHAIRMAN NANCE: Luiz and then Roy.

DR. BARBIERI: Thank you, Mr. Chairman. Thank you for that, Katie, but all of what you just mentioned -- I mean, isn't this really the result of us not really knowing about the future, and so it's uncertainty about the future, in terms of, okay, if we use the lower recruitment for the last five to ten years, and that causes us to have an equilibrium biomass into the future that will impact estimation of reference points, well, that's what we know now, and so I would not have a problem with that, considering that, hopefully, in three to five years, we're going to have another assessment that will help us adjust.

If things change, and if recruitment actually increased, in the next three to five years, we push that reset button, in a way, and that's why we receive those assessments every three to five years, right, and so, to me, there is no other way, really, to handle this, because we have to explicitly accept that we don't know what the productivity of the stock will be going into the future, and we're going to have to accept those equilibrium values being at a lower productivity level, because that's what we know right now.

I think, to me, this is like, for example, setting up that rebuilding plan, but then failing to rebuild, right, in the case of amberjack specifically, and I feel that, well, that's because

we expected a level of productivity that was not really realized, and maybe we set expectations for a level of productivity that was not really achievable, based on the most recent past. Thank you.

DR. SIEGFRIED: If I can respond to that, Mr. Chair?

CHAIRMAN NANCE: Yes, absolutely.

DR. SIEGFRIED: I see what you're saying, Luiz, and I see sort of what you're getting out of this and how you're pulling those thoughts together. I think there's a few things to address. One of them I think somebody else brought up earlier, and that's sort of why are ignoring -- Why aren't we -- I think it was Steve, but why didn't we use the stock-recruit curve, and so what allowed us to estimate these recruits in the first place was the fact that we had a stock-recruit curve, right, and so you're very much decoupling what happened in the model and what is going on in the projection if we ignore the stock-recruitment relationship and go with average recruitment.

It's possible that is reasonable, and it's possible that it's not, but I just wanted to make that explicit. The other thing is whether you are considering, Luiz, when you're making these comments, what management has been able to do, whether the rebuilding has happened, or whether it hasn't happened, and maybe why.

If you look at the time series of recruits, we really don't see as high recruits in the later time period, maybe from 1990 on, and so you may not be as uncomfortable, assuming the regime shift, and the thing that's important to note, which I think Doug was getting at before, was, well, do we ever think that the stock is going to realize the stock-recruit curve again and be able to produce recruits at the level that the stock-recruit relationship would predict if we used that for projections, and that's something to consider, and also the impact on the fishing community, which I think Rich brought up, in terms of allocations.

The impact here is that, if you're assuming that the stock is going to stay low, because you're telling the projections that the recent average recruits are all they can expect, then the yield, even in the short-term, is lower, depending on your target, which Nathan will go over, but, if you're looking at depletion, your depletion is very low, and it's okay with you if you assume that recent average and you assume that, okay, well, we've rebuilt, because we can't get any bigger than this, and so it does have an impact on yield and not just whether we've rebuilt, and I think I show that in the next slide to where I stopped, but we can finish the

questions before we go to that slide.

CHAIRMAN NANCE: Luiz, to that point?

DR. BARBIERI: Yes, and thank you, Mr. Chairman. Thank you, Katie. I understand those points, and it's not an easy decision to be made, and there are consequences that we need to account for, by all means, and that makes sense, but, you know, think about the fact that when -- I think that, when we use the stock-recruitment relationship for an assessment that we felt that function was not properly estimated, I think we're ignoring a lot of the uncertainty, and I am thinking about this, Katie, as an SSC member.

Our ABC Control Rule explicitly tries to account for the uncertainties in the assessment, what we can actually properly predict in providing management advice to the council, and so, when I see an assessment, and I can understand, from a theoretical perspective, that there is information content in that SR function there that we want to use, but, really, when we use, when we choose, an SPR-based reference point, we are explicitly acknowledging that recruitment is not something that we could really -- The recruitment dynamics cannot be properly captured by this assessment, for whatever data deficiencies that weas due to, and so to not really accept the MSY-based reference points, but then use the SR function to estimate recruitment into the future, I think -- I see an inconsistency there that I can't reconcile.

Just one other point, real quickly, Katie, because you brought this up earlier, and so I just wanted to weigh-in on that, but, in terms of using the last let's say three to five years at the end there of the recruitment graph that we see there, and so the recent average, it's highly uncertain, and why wouldn't we use that, if we're actually acknowledging that this is something we don't know very well, and so we expect to be highly uncertain?

If we explicitly said, no, recruitment dynamics is not being properly captured here, given data deficiencies, I think that having highly uncertain levels of recruitment -- Putting some average there and adding big CVs to that average I think is the most appropriate way for us to recognize that uncertainty is properly accounted for in the projections, and that's just my opinion there.

DR. SIEGFRIED: Thanks, Luiz. I want to hear from the other people that have hands up, but I think this is just the beginning. We have a whole bunch more information to show you that will help you with these things that you're struggling with, and others are struggling with, and it's not an easy problem to solve, or even a

one-dimensional, by any stretch.

CHAIRMAN NANCE: Thank you, Katie. We're going to go with these questions, and then we'll move on. Roy.

DR. CRABTREE: I have seen these situations in a number of fisheries in the Gulf, and in the South Atlantic as well, and I am always pretty skeptical of recruitment estimates from forty years ago. You see the variability in the recruitments, and I expect the recreational catches were extremely variable, and I know they were back in the early 1980s, and so -- The other thing is, to me, the most reasonable expectation of what is recruitment likely to be in the next few years is it's probably going to be pretty close to what it's been in the last few years.

When you shift to the stock-recruitment curve estimate of recruitment, it can be quite a bit different, and I've seen times when it's quite a bit higher than anything we've seen, and, if you use that in setting your ABCs, they go up very quickly, and that can get you in trouble.

I have also seen situations where we had pretty good recruitment for a number of years, five or six years in a row, and the stock-recruitment estimate was much, much lower, and then, when you did the projections, it created a paper collapse of a fishery that didn't happen at all, because the recruitment stayed up there, but it does make it very difficult to do rebuilding projections, and, a lot of times, what you will find is, if we recruitment is close to where it's been recently, you can never rebuild, no matter that you do, and that can cause you to constrain a fishery more than you should, because you're trying to achieve something that, realistically, you're probably never going to be able to do.

CHAIRMAN NANCE: Thank you, Roy. Dave.

DR. CHAGARIS: Katie, I think you sort of answered my question, but the last comment you made about you can't do the equilibrium projections, or the equilibrium analysis, I guess the time hindcast model, and you only do that in projections, because of how the allocations and selectivity and all are handled, but it still seemed like the projections should be based off a long-term average, as far as setting the reference points.

The thing that concerns me the most is the issue of kind of the moving goalpost, and so what would happen if, ten years from now, recruitment was even lower and we estimated a lower recent average? If I understand correctly, the effect of that would be that you would get a lower minimum stock size threshold, which would make

it easier than to rebuild to that, but, if that were to keep happening, how would that play out over time, if we were to keep kind of following these lower recruitment signals? Would we just keep lowering the goalpost for this stock and never get anywhere close to the historical reference points?

DR. SIEGFRIED: Yes, and you would lose a lot of yield along the way, which, I mean, you're asking the question, Dave, about basically B zero, which we have started to talk with the larger stock assessment community as to who is using that, is anybody using that, and it's not actually well used at this point, because of exactly the thing you just said, Dave, and, I mean, it's certainly something we would need to see an economic analysis on as well, but, yes, that's exactly the way it would go, the way you described.

CHAIRMAN NANCE: Thank you. Doug.

MR. GREGORY: Okay, and I know this is more related, I guess, to what Nathan is going to show us, but this is all mixed up, and I just wanted to address it. I agree with Dave, and the thing that concerns me is what if this low recruitment from 1990 is the result of a failure of management, and we go forward with this new approach, and we're going to have small ABCs, because we're expecting lower productivity, and so, in one way, that might --

That will help the situation, but we will also not acknowledge that overfishing has been occurring, and so we're stuck with this conundrum of is this a regime change or is this a failure of management, and so, in trying to understand the new approach, and I don't have working knowledge of SS, but, up until now, SS would give our status determination criteria, and then there would be a separate program, in R, that would do projections, and is that correct?

DR. SIEGFRIED: No, and we've always -- With SS, in order to get our benchmarks, we have had to run a long-term, a hundred-year, basically, equilibrium -- Projections out to equilibrium, in order to get our benchmarks, and so there wasn't status determined before we carried out our projections.

CHAIRMAN NANCE: But, Katie, the projections you used were -- You're saying a hundred-year average for those benchmark projections, right?

DR. SIEGFRIED: Right. Well, hundred-year equilibrium. It wasn't using an average of the -- I want to not misspeak. It's not the recruits. I'm saying we carried the projections out a hundred

years to get to equilibrium with the assumptions we made, and, out there, we would calculate what our SSB MSY proxy was and what our FMSY proxy was, in order to determine status, and so projections are when we determine status and not at the end of the assessment.

CHAIRMAN NANCE: Okay. Thank you. I misspoke, and thanks for correcting me.

DR. SIEGFRIED: No problem, and I just had one more thing to Doug's point. What we said during scamp is that, for the Gulf model, if we're not carrying out projections, then we don't actually know status yet. We just have model fits, and the way to evaluate the assessment is diagnostics of how well the model fit the data and not -- We aren't able to actually show the status until we've carried out projections.

MR. GREGORY: That was also the premise of the research track, is you don't come to the meeting with status determination criteria, and you're just fitting the model, and so that's what I assumed that was all about, and it's not really related to this, but I will save my thoughts until later. Thank you.

CHAIRMAN NANCE: Thank you, Doug. Harry.

MR. BLANCHET: A lot of people have touched on this one way or another, but my concern over using the long-term is that, in the case like amberjack, where we have basically determined that -- As soon as we got good data, the assessment showed that we're already in an overfished situation, and so our basis of what our stock should be is all in the historic time series, but then we are, under Magnuson, having to rebuild to that, and I don't know if we can use this recent average approach for recruitment and still be consistent with Magnuson, and has that been discussed?

DR. SIEGFRIED: No. Actually, Harry, we haven't discussed it, in terms of what -- We hadn't considered that yet. We do have a well-estimated R0, which is our virgin recruitment, and we do have a steepness estimate, which shows that there is some data in the model that supports the stock-recruit curve, and so it's definitely something we have to explicitly discuss.

If we have those estimated parameters, we think that this is the stock-recruit relationship, and we haven't been able to rebuild, and is it something that we want to, in some way, consider a dynamic B0, where we don't think it's actually that way anymore? I think that's something that we need to review the literature and discuss more thoroughly, Harry, as to whether we're off-track with Magnuson, and we hadn't discussed it that way.

MR. BLANCHET: It seems like this, in reality, could come out on either side. Either we have had remarkable changes very early on in the fishery, or there were deficiencies in data, early on in the data collection process, or a lot of other possibilities that we could discuss forever, and, as you may remember, I'm not a fan of the current model, but the challenge is this is accepted as our best information, and so now we have to make sure that we're using it appropriately, and that's all.

CHAIRMAN NANCE: Thank you, Harry. Nathan.

DR. VAUGHAN: Thanks, and mine was just piping in on that comment before about the SS versus our hundred-year projections, and I think it gets a little confusing when you think of it as a hundred-year projection, like we think we know what's going to happen a hundred years from now, and that obviously is very uncertain, and we don't know where we'll be, but what SS is doing internally is very much the same thing, but just with an assumption of constant effort between fleets moving in the future, as opposed to the more complex assumptions that we need to take to understand how allocations and other changes are going to affect the fleet.

If we think about what the Science Center is doing in their long-term forecast not as predicting what the stock will be doing in a hundred years, but just predicting an equilibrium approximation under our complex assumptions, that's more what we're trying to achieve, and, under the similar assumptions, that's exactly what SS is doing, and it's just hidden inside the calculations.

CHAIRMAN NANCE: Thank you. We're going to take a fifteen-minute break. We'll take our morning break now, and it seems like an appropriate time, and then we will come back for some of the rest of this. Katie, thank you so much for the presentation thus far.

DR. SIEGFRIED: Sure.

CHAIRMAN NANCE: So a fifteen-minute break.

(Whereupon, a brief recess was taken.)

CHAIRMAN NANCE: We'll go ahead and come back from our break. Katie, go ahead and, as soon as the presentation is up, go ahead and start with the rest of the presentation.

DR. SIEGFRIED: Sure. I think I just have this last slide, and then can move over to Nathan's presentation. This is sort of the end, the culmination of all of the decision discussion that we had

before for amberjack as a case study. Let me walk you through all of the things that are in this one plot, and so, by year, the OFL is plotted in millions of pounds for five different scenarios.

We'll start with the base run that uses depletion, which is what we showed, which is SSB over SSB_0, and a long-term mean for R, and so, rather than the 2009 to 2018, we had accidentally used 1950 to 2018, and so I am showing you what you received at the January meeting, and it's in orange here. This is sort of moving towards correcting that and then showing you the decisions that I have shown in the previous slides of this presentation.

The next one is the base run with recent recruits, which is gray, which is what we said we were showing you, but you can see that's quite a large drop in the OFL, even in the short-term, and it's not just affecting the long-term yield, and it's affecting very much the short-term yield, and we get slightly over half-a-million pounds in 2022, as opposed to two-and-a-quarter-million pounds. I'm sorry. One-and-three-quarter-million pounds for the orange line, and so it's a substantial difference in the yield between the orange line and the gray line. The gray is what you should have seen with respect to recruits at the January meeting.

The blue line, the dark-blue line, is the base run, if we were to use the SSB SPR 30 ratio, rather than depletion. The recruits are still the long-term mean, and so that's the dark line, and so you see using just the SPR, as opposed to the orange line, and so the dark-blue and the orange is what you want to compare there.

Using the SPR ratio, it increases the allowable yield for OFL, and then the yellow line is using both that SPR ratio and recent recruits, and so you can see it's a drop, and, each time we change the recent recruits, we have a drop in the yield projected for OFL and ABC, and so, for that one, you want to compare the dark-blue to the yellow.

Then, which is sort of our proposal, although the years could potentially be adjusted based on what the SSC thought about data-rich, is the light-blue line, and so we've been telling you that we've been using this SPR ratio and what our recruits should have been, and so we have that base run, which is the light-blue, using SPR, and the data-rich recruits, and so from 1984 to 2018.

The message that we're trying to illustrate for you here is that dropping recruits to that average recent time period drops the total yield that you can take, or sorry. Not you, but the stakeholders can take. Using the SPR ratio, it makes the recruits less relevant in that calculation, like Luiz mentioned earlier,

but, later on, when I present, you'll see that the biomass ratio is at 13 percent when you have your SPR at 30 percent, and so that's something the SSC needs to consider, whether they're okay with that little of depletion.

The SPR ratio makes recruits, low recruits, less impactful. Lowering recruits has a large negative effect on the total allowable yield, and then there's a different between using the recent long-term or data-rich recruit period. You might have to soak this in for a minute, and did you have any initial questions?

CHAIRMAN NANCE: This is an excellent presentation graph to show the effects of different decisions that are made. Luiz had a question.

DR. BARBIERI: Thank you, Mr. Chairman, and thank you, Katie. Yes, this is super helpful, but my questions are, one, what are these projections being developed for? What is the goal of these projections?

DR. SIEGFRIED: This one shows the OFL projection for greater amberjack from the base run, and then we would have to also show the F rebuild projections, but with the same decisions.

DR. BARBIERI: Right, but I am thinking -- Are you actually obtaining revised reference points from these projections?

DR. SIEGFRIED: Yes.

DR. BARBIERI: Because what I am thinking is, Katie, I mean -- This is old-fashioned, in a way, but the assessment itself is retrospective in nature, right, and so it's based on the history of exploitation of the fishery, and it's looking at how the stock dynamics changed over time, and productivity over time, and it has -- It produces estimates of reference points based on that retrospective -- Up to the terminal year of the data in the assessment, right, and so that's what we usually use for stock status determination, and so I am thinking as projections being used for two purposes.

One is to provide catch advice and generate yield streams of OFL and ABC for the council over the next several years, basically so we have catch advice until the next assessment is conducted, or if you have to set a rebuilding plan, that gives you a rebuilding target, so you can build your trajectory and estimate your F rebuild. According to those two purposes, what would those projections here be used for?

DR. SIEGFRIED: Luiz, I do have to offer a correction there. Our status determination -- Our status determination criteria are based on benchmarks that are derived during projections. The only way out of that is to run an internal projection in SS that ignores allocation and ignores our bycatch and discards into the future, and so all of our benchmarks are calculated during the projection phase and not at the end of an assessment.

DR. BARBIERI: Right. Fine. I can see that, in a way, that this is the new and improved process that accounts for all of that, but, in years past, we weren't able to, and so I see this as an improvement, and I am okay with that, but I am thinking that, if you have projections to 2030, how would those then reference points be reflective of what is going to be seen over the next three to five years, in terms of stock status? That is my question.

DR. SIEGFRIED: 2030 is just plotted here because it's shortly after the rebuilding timeframe, so that you all can see what changes with the yield and the Fs after 2027, and so the rebuilding plan is already in place for this species, and so we didn't -- It's not comparing what status looks like at this point, and we're just starting with yield.

With respect to how does this reflect where we should get back to, and I hope that I'm interpreting your question right, it's kind of the point that we were making before the issue that you all were discussing, and that is where do you want to rebuild to, and so the rebuilding here is already set, and that's something we maybe should consider, what time period was used when the rebuilding year was set to 2027, and that might be consistent, moving forward, for rebuilding projections.

I think that was the long-term average, but I am checking on that during breaks, and so there's not really -- There's not really any getting around what the recruits were for the projections used to set the rebuilding year. At this point, what these are meant to show you, and this is using old code, and this isn't even using the new code yet, and this is just using a simple F projection that we've had in R for years and years, and I think this particular code has been used for six-plus years, and so this isn't actually new.

The thing that's new now is that we're showing each individual decision and its impact on the projections, and I can also provide a table of the impact on the benchmarks, so that you can see how status might change when you assume SPR, or when you assume depletion, or when you assume what years to use for recruits.

DR. BARBIERI: Okay. Thank you, Katie. That helped a lot. Thank you.

DR. SIEGFRIED: Sure.

CHAIRMAN NANCE: Katie, for my edification, like the blue line, the dark-blue line, each of these, at 2030, is rebuilding to the desired target, and would that be fair to say?

DR. SIEGFRIED: These are just OFL projections, and so we're fishing at the FMSY proxy, and so I haven't shown you the effect on rebuilding.

CHAIRMAN NANCE: Okay.

DR. SIEGFRIED: The relative effect is the same though, and so you can get an idea of what that would look like.

CHAIRMAN NANCE: Okay. Roy, you had a question?

DR. CRABTREE: Yes, and so, Ryan, is the council being told that they have to revise the rebuilding plan at this point, or are we simply providing them with an ABC to set catch levels?

MR. RINDONE: So F rebuild is still set at 2027, and so the projections still endeavor to see the stock rebuilt to -- Based on the projection settings that you guys determine, those catch limits would still rebuild the stock to that level at 2027, but, again, like Dr. Siegfried said, if you use the more recent, like 2009 to 2018 time period, for recruitment, then you're going to rebuild -- Assuming a regime shift that estimates that the stock is not as productive, and so you're never going to get a level that we've been trying to rebuild to all this time, or if you use a longer time period.

DR. CRABTREE: I don't know what to do about any of that, because I don't know what recruitments are going to do, but it seems to me, if you use any recruitment scenario other than the most recent years, the low one, than the projections are assuming that recruitment jumps up to a higher level than we've probably seen in years, and, if you set your catches based on that, you may be catching too many fish.

If you use the lower recruitments, and set your catches on that, and the recruitment does jump up, then you're probably being very conservative, and hopefully you will figure that out, when you get an assessment in another couple of years or so, but I'm just trying to get at do we have to make a decision about the long-term

productivity of the stock now, or is our task that we need to give the council some guidance on what they ought to set the -- What the ABC is for the next few years.

If we set the ABC based on the lower-recruitment scenario, that would be a very conservative place to set it, if recruitment does in fact increase. If we recruitment -- If we come back in in five years, and recruitment is continuing to be low, then I just see no reason to think we're going to see those recruitments again, and, at that point, you could recalibrate the rebuilding plan.

CHAIRMAN NANCE: John.

DR. JOHN FROESCHKE: I have a follow-up on that point. Based on what I have seen of the results of this, depending on the assumption here, it changes stock status, and so, originally, in January, it was determined that the stock was overfished and overfishing. Depending on how this is handled, it could be that the stock is no longer overfished, and so, in terms of if you're modifying the rebuilding plan or not, I think that has implications there.

The other thing that we're going to have to think about is how we're going to fill those gap-year landings, and I know we did this before, and we're probably going to need another year, and we'll probably need to fill 2019, 2020, and 2021, and what the assumptions are about what we would use there, and I'm not sure what those would be.

DR. SIEGFRIED: Mr. Chair, to that point?

CHAIRMAN NANCE: Yes, please, Katie.

DR. SIEGFRIED: I do have a very large spreadsheet with all of that information, and there is no -- None of the projections that I have run show that the stock is not overfished, and so we didn't -- That's something I have, but that's not something we presented, because it doesn't change the status now.

It does change the speed with which the stock rebuilds, and that's something that we show in the allocation projections and that we can provide more information on, given the decisions, but I just wanted to clarify that, and so there's not a status change, at this point, that would make it so that rebuilding no longer has to be continued.

CHAIRMAN NANCE: Okay. Thank you very much for that. Luiz.

DR. BARBIERI: Thank you, Mr. Chairman. Katie, just to confirm, I understand your point here, and I don't disagree with what you said, and what is shown here, because you are really dealing with a rebuilding trajectory, and so, to me, in that case, yes, you have a long-term goal that you want to achieve, and you want that long-term goal to reflect the long-term productivity of the stock, or the capacity of the stock to be more productive if we rebuild it, and so the yield stream that's going to come out of this is going to be based on F rebuild.

All of this to me makes sense, but I was just trying to think about situations where we are dealing with projections that are not taking a rebuilding plan into account, and you're actually going with yield streams of OFL and ABC and not necessarily looking at that long-term rebuilding trajectory, and so what you have presented here to me makes perfect sense. Thank you, Katie.

DR. SIEGFRIED: I appreciate that, Luiz, and I do want to -- We wanted to present this first, because it does have an impact on what you all can, or should, consider, in our opinion, for gag, and so, if the status of that stock is going to be determined by projections, and it's very important that all of these decisions are carefully considered when we complete those projections for you. Thanks.

CHAIRMAN NANCE: That's perfect. Benny.

DR. GALLAWAY: I accidentally hit the button, Jim. I don't have a question at this time.

CHAIRMAN NANCE: Okay. Thank you. Katie, we greatly appreciate that presentation. Dave.

DR. CHAGARIS: One more just quick question, just for my own clarification. Katie, is it possible, and would it be reasonable, to set the targets, the reference points, based off of the long-term average recruitment, but then use the short-term recruitment in the yield stream projections?

DR. SIEGFRIED: I mean, that's certainly possible. I would need to look at the coding capabilities at this point, to see how heavy of a lift that would be. I don't think it's -- Again, Nathan can weigh-in on that, and so I think it is certainly possible to do, and I think that they do that in the South Atlantic.

DR. CHAGARIS: To me, it would seem like -- If you have this case here, where you could use this yield stream, the gray line, but, if you also change the reference point based off of that short-

term recruitment, then your stock size threshold would be lower, whereas you could still maintain that higher minimum stock size threshold based off of the long-term recruitment, and then this yield stream would actually get you there quicker, because you would be more conservative based off of the lower productivity cycle that it's in now. That's kind of where I was coming from with that question, but thank you for that.

DR. SIEGFRIED: Would you mind saying that one more time, because I think I heard it wrong.

DR. CHAGARIS: Like, if you had another figure up here to show the biomass trajectories for all these runs, and the reference points associated with each of these, as far as the targets would go, I would expect to see the biomass recover faster under the gray line, with the lower yield, assuming that you have the reference points based off of the long-term average.

It would take longer for it to get there, but, because you have lowered -- I don't know if I'm doing a good job explaining myself, but, if you set those reference points based off the long-term average, then you would have a higher minimum stock size threshold, and, in order to get there with your lower recent recruitment, you would need a lower yield stream, and so it seems like, if you could do both, use the long-term to set the targets, but then account for the lower productivity in your yield stream determination, that it would help get you to the long-term target.

DR. SIEGFRIED: I mean, there's two things that come to mind there. We would probably have to flesh that out a little bit more, but if say the gray line here -- We just are assuming -- Because this is not showing status, and this is just what you could capture if you used the lower recent recruitment, but, if you used longer-term recruitment to set your targets, you would actually be trying to still recover to that higher value, and so I think it would just -- I think I would have to take a look a little bit, to see if it would actually speed that up, because it speeds it up here, because the future recruitment is assumed low forever, but, if you set those targets based on long-term recruitment, it would be higher, I think.

CHAIRMAN NANCE: Okay. Roy and then Will.

DR. CRABTREE: I think, if you do that -- So you will have a rebuilding projection that is for the purposes of the rebuilding plan, and then you're going to set the catches though based on recent recruitment continuing, and that's going to give you a pretty big gap between the rebuilding plan OFL and the yields and

where you're setting the catches.

The trick there is to be to explain to everyone where that is, and then you've got to recognize that you may come back in, in a few years, and be making inadequate progress towards rebuilding, and so, eventually, you will have to face the reality that it may be that rebuilding is simply not possible, because you can't get back to those old levels of recruits, but I am not convinced that we have to resolve that problem now, but you are going to have to be able to explain to people why you're setting the catches here, but the yield stream is up here.

CHAIRMAN NANCE: Okay. Will.

DR. PATTERSON: Roy made part of the point that I was going to make, in response to Dave's comments. The second is that this idea about speed of recovery is totally dependent on whether recruitment stays at the recent average or those earlier, highly-uncertain recruitments are ever realized again.

Not only do you have this gap that Roy was talking about between the long-term projection of OFL and what the near-term catch advice might be, but then you may never actually get there, and so you have -- I wouldn't assume that the speed of recovery is necessarily going to be affected by this, because of that issue of recruitment.

CHAIRMAN NANCE: Good point, Will. Harry.

MR. BLANCHET: Maybe Katie can help me, but I am struggling. When we look at the gray line, and we look in 2022, and then we look in 2030, our OFL estimate has increased about one-and-a-half times, or something like that, while, if we look at the gold line, it has increased about 20 or 25 percent, and so this seems to be based upon just what are you using for your threshold. Can you explain what the difference in that change is, in the rates of increase?

DR. SIEGFRIED: Sure, and so the more shallow slopes are the ones where we used SPR ratio, rather than the biomass ratio, and the SPR ratio is actually less sensitive to the assumption about recruits than the biomass ratio is, and so you see that doesn't have as big of an effect on the gold line and the gray line and the light-blue line. The steeper -- It has to do with SPR is less sensitive to our assumption about recruits than the biomass ratio is.

MR. BLANCHET: Okay. Thank you.

DR. SIEGFRIED: Sure.

CHAIRMAN NANCE: Thank you. Let's go ahead and move on to Nathan's presentation.

PRESENTATION: EVALUATION OF A NOVEL PROJECTION METHOD TO STREAMLINE ALLOCATION-INFORMED YIELDS

DR. VAUGHAN: Thank you, Chair. To that previous point, while we're waiting --

CHAIRMAN NANCE: We need to grab a new presentation. Is this the one you want, Nathan?

DR. VAUGHAN: Yes, that's the one. Thank you very much. I am going to present on this, and it's not say a new approach, and it's really just an evolution of what we've been doing already, and I will go through that transition of how all these different projection sequences and approaches are all related, and they're all very incremental changes.

I'm also going to try to help clear up some of the questions that we've been discussing by going back over what it is we're trying to target when we're doing projections and what are some of those key assumptions and how these are all interrelated and how they're acting on the results.

The question is what are we really trying to achieve when we're going our projections and our forecasts and setting OFLs, et cetera, and so, really, the priority goal is to achieve some target benchmark at equilibrium, and that may be MSY, and it may be an MSY proxy, such as SPR 30 percent, or SSB 30 percent, but the main goal is to achieve that at equilibrium and to figure out what F, and that's important here, is distinguishing between multiple Fs.

We look at these Fs as the total biomass removed each year relative to the population biomass, and that's the total fishery F that we're going to be trying to target, and we're going to estimate what that would be at equilibrium, and then we're going to try to fish the fishery -- In our OFL projections, we're going to apply that F every single year, and some of the complexity there comes back to how that total F relates back to what we call at fleet-specific F, which is really an effort for that specific fleet.

Then, also, we want to achieve catch fractions between different sector allocations, and we discussed that that could be commercial and recreational, and it could, in theory, be split up finite into different fleet-specific catch allocations, and part of what promoted this new code to be developed was the specific council

request, the specific request for amberjack, and it was to fix the actual catch for the commercial, while also -- While still adjusting the recreational fraction, in order to still achieve those Fs, and complex questions like that, that you can ask if you go into a more dynamic fitting approach.

Then, also, we want to maintain those fleet-specific efforts, still the same within a sector, and so we don't think that -- We don't have any reason to believe that effort will change between the different components of the recreational fishery if we're only fixing the catch for that one fishery, for that one sector.

This is just kind of giving an example of what that is, and so, on the top row there, you've got SSB, and that's going to transition each year. Unless you're perfectly at your equilibrium, it's likely going to be transitioning to some new value, and that value on the left, that 3,000 in year 100, is reflecting what we're trying to achieve, and so that's going to be some benchmark target, and so what is our SSB that we're trying to achieve? We're always going to be approaching one, but, as you can see there, we're at 78 percent of that in the first year of our management.

The next row, you're seeing that total F, and this is what we've been talking about, and we're trying to keep that constant every year, and that's one of the main factors that also inspired this new code, and, as you can see, that will result in a different yield. Even at the same F, you've got a transitioning yield, because your population biomass is increasing, and then that yield is segmented out by sector, and so this is Sector 1 and Sector 2, and those ratios are -- It's hard to see there, actually, where I put in the ratio, but ratios are the same. This is amberjack, and that's the 27/73 allocation in the projection.

Then you finally have these fleet-specific fishing efforts, and those are what traditionally have been kept constant in the projections, these individual fleet Fs, but, as you can see, if we maintain everything else the same, those are slowly going to transition during the projection period, and figuring out what that transition should be to achieve all of our other goals in those top value rows, that's what this new code is trying to achieve and what we've slowly been evolving towards with the different changes in the existing procedure.

If we go to the next slide, we can see a lot of the assumptions that are going to influence those trajectories and how we go about getting to our final goal, and so we've got future recruitment patterns that we've talked about a lot, and we're currently at kind of a baseline of using recent means, or historic means, and

you've got a lot of decisions you could make there that are going to affect the productivity.

As we improve our stock assessment models, we can also expect to move on towards using stock-recruitment curves, and potentially MSY estimates, and then, also, we've spoken about the possibility of including deviations around that stock-recruitment curve to bring in both uncertainty, to better quantify our uncertainty in the future, and, also, at the long-term, potentially we can start bringing in some of these other approaches, such as non-linear time series or climate models, to come up with some predictions about how these deviations are expected to drift or how the total population productivity is potentially expected to drift in the future.

Everything we do in these assumptions is going to feed into our benchmarks by how that affects the long-term stock structure, and we also have fleet selectivity and retention functions, and we would like to be able to include the correct selectivity in the future, and that's really what is driving this, is what is the selectivity of the future and how are they targeting different age and size classes through time, and that could be just whether we average three or less years of the recent fishery, if there was a recent change in selectivity say, or how we -- If there is an expected change in minimum size or retention or discard mortality, and those things need to be incorporated into those projections.

Then fishery sector allocations are going to adjust effort between the fleets, and then, through that, affect the selectivity of the total fishery, which is what's important, and then, also, the benchmark targets, and I will look at how these different possible benchmarks are going to impact our results.

We have historically been mostly looking at stock assessments that have an assumed steepness close to one, and, in those cases, these results are not going to be very influential, because the SSB versus SPR target are going to be identical, and, even if you use a recent average of recruitment, if you had an estimate of one for your steepness, those are unlikely to vary very far from that long-term mean, and so there's going to have a much lower impact.

The reason we got to this is we've had these questions roll in on amberjack, that had a fairly low steepness, and, hence, we have now current recruitment that is very far, or relatively far, away from the equilibrium expectation. It's, I think, something like 75 percent right now, and that's why we're seeing such big impacts on this.

We can start to look at this through the lens of this global MSY, which we've presented before for red snapper, and it looks at basically fishing a lot of different scenarios where you fish just a single age class, and so, if you look at this plot, there is nine different points there along those lines, and these are really -- These are each a different simulation through SS that it performs, where it fishes the stock only at say age-one, age-two, age-three, and you're fishing solely that age class and figuring out what would the maximum MSY be if you only fished that one age cohort, and you can see that you have differing optimal levels of F, which is the orange line there, and different actual MSYs that you achieve, which is the yellow line, for all those different targets.

You will see, basically, that those SPR and SSB lines start to trend after age-five, and that's once you get beyond the age of sexual maturity, where you're starting to see spawning biomass, and so you can progressively catch a small fraction, and these are all basically fishing as hard as they can after that point, which is why it ramps up, but you can see here that there are very different expectations for how much yield you can take for any one given age class, and, in this case, it's around about ages-five to seven have fairly high yields, or maybe four to eight, but then, once you go below that, you're getting much less yield from that particular age cohort.

If we go to the next slide, we'll see that this is for amberjack again, and these are the different fleet selectivities, and you can see that your total is going to be some average of these, but your commercial, in this case, tend to be much more selecting fisheries above that age-six, which generally had a larger sustainable yield that could be harvested from them, and the recreational sector there targets a lot smaller fish, and so, if you shift effort to them, you're going to be support a relatively smaller maximum sustainable yield from the total fishery.

This is why we're so worried about allocation and why it's important to incorporate it into our projections, because, if your stock is going to transition a lot, or if you've got a lot of rebuilding that's expected to go on, you can expect that the selectivity of the fleet, as you move forward in time and the size structure of the population changes, you're going to harvest more or less of different age cohorts, and it's important to keep these fractions constant, rather than just assume that they're going to fish at a constant effort between the fleets.

I think that, at the moment, we can pause, if anyone has got some questions up to this point, before I go on to the impacts of

different benchmarks and targets and why we're doing these modifications.

CHAIRMAN NANCE: Thank you, Nathan. Any questions thus far on the presentation? It looks like everyone is happy, Nathan.

DR. VAUGHAN: Okay, and so we're all much onboard as to why we're doing allocations and the importance of these changing selectivities, and, as we said, it's relative, and so the other big thing that we need to ask is what are we targeting in the benchmark, and that was the primary difference between a lot of those slides, or those lines, that Katie showed, is that we have two different -- Primarily, if we're not able to estimate MSY, we have two primary options that we could do, and these have been conflated in the past, which is a spawning biomass or a raw fraction of SSB_0 that we're trying to target, or are we trying to target SPR, which is that spawning biomass divided by the recruitment?

Those potentially, if you've got a recovering stock and if you don't have a steepness of one, those are likely to have very different repercussions, and so, if we go on to the next slide, I'm going to show here how these recruitment assumptions are going to impact our estimates of the fishery status, and also our future OFL projections, if you use either one of these two assumptions, and, now that we're using these in a fishery that has a high steepness and does have very depleted recruitment currently -- In this one scenario for greater amberjack, they are having a relatively large effect.

These plots, and I know they're a little bit hard to see, but, really, we want to look at the difference between that gray, orange, and blue lines. We're not so much interested in the exact values. This is under a projection where we projected to an SSB of 30 percent, and so we're using different average recruitments, and, over there on the left, the gray line at the top, in recruits, it's showing a large recruitment, and this is your long-term average for the whole time series, and this is what was originally presented to the SSC.

We have in there the orange line in the middle, and that is what Katie presented as the recent data-rich average, and so nineteen-eighty-something to the present, and so that's our data-rich, and then we have the blue line, which is our recent data, recent mean, which is the last ten years, from 2009 onwards.

The issue that we're having is that these are all, taken in isolation, fairly reasonable assumptions that you could make in

setting your forecast projections, and so any one looked at in isolation is reasonable, but there are fairly large implications for what they do, and so, in this case, we're fishing to an SSB 30 percent target, and so you can see, on the right, all those three different assumptions about recruitment have very little different impact on where the population biomass goes here, because we are fishing with a direct intention to achieve SSB 30 percent, and so we achieve the same biomass.

Importantly, you will see there that, in the historic period, prior to -- Well, I guess the non-fixed period prior to 2022, where management would kick in, they're all like exactly the same, and so we have the same spawning biomass, we have the same estimate of spawning biomass SSB_0, and we have the same estimate of spawning biomass over SSB 30 percent, which is our benchmark, and so, in this case, our overfished status determination would be identical, no matter what we assumed for recruitment.

However, on the left, with SPR and F, you can see that there are very different estimates there of what the sustainable F is, because, under those different scenarios, you're going to have to fish much, much less if you would like to achieve a 30 percent SSB ratio, under the low-recruitment scenario, and you can fish much harder under the high-recruitment scenario, if you're also choosing that same SSB, and it's easier to get there.

In that case, you're going to have very -- If say this recruitment changed, either within -- You're proposing different alternatives to the council, or if this is year-to-year, or assessment-to-assessment, and you're making different determinations about what you should use for recruitment, or your recruitment average is changing, if this was a recent ten years that you always used, you're going to see that you have relatively unstable and changing estimates of what is FMSY, what is your overfishing determination.

Consequently, because the population biomass is the same, those different Fs are going to result in very different yield streams for the OFL, rebuilding F, et cetera, very different estimates of yield in those recent years, and those are the years, really, that are important, because that's what is happening before we have a new assessment or an interim update to adjust these catches.

We're going to end up with very different estimates of landings between assessments and between assumptions, and so that is your primary thing that you have to think about if you use SSB, is we're going to get very different yields.

If we go to the next slide, this is going to look at what we

intended to show to the council last time and what we're seeing now, is if we used SPR, which is that spawning biomass discounted by the recruitment that is expected, and so divided by that recruitment.

You have, in the top-left corner, that same projection of recruitment, and you have that same deviation, but now, because we're fitting to this SPR 30 percent, you can see the two plots below that, the SPR and F, and they are identical between runs, because we're targeting that same -- We're targeting SPR every year, and we're achieving that, and now your F, which is going to affect your OFL catches, that is now very stable, and it's equal every year, or between each scenario of recruitment, and, if they change year to year, that would be equal annually.

You're going to have a similar estimate of whether you're overfishing and whether you were overfished in the past, as well as you're going to have a stable quota, OFL, to some degree, between years, at least with regard to what you choose for recruitment.

Unfortunately, you have all of these things are a tradeoff, and the primary goal of these discussions is to help elucidate the fact that, no matter what we choose, we're going to have to have some of these tradeoffs, and so, if we're go to the right on spawning biomass, we're seeing that we have very different targets, as we pointed out, and this is what Katie's slides were showing, basically, is you've got different -- Depending on where you go, you've got different targets of what your spawning biomass will achieve.

We can see that, before 2022, the spawning biomass is the same, because that's the historic period. Your SSB over SSB_0, this SSB_0 is fixed, and those are also very different. If you're got high recruitment, you're going to get to a much larger population biomass, and, as you reduce your recruitment assumption, it's going to go down.

Unfortunately, the big tradeoff comes here in the SSB over SPR. Because your historic values are fixed, and because you are now changing what your population benchmark is going to be, what we think we're going to get to in biomass, you can get very different estimates for what the current overfished status is, and, in this case, they are fairly large between those different alternatives, and so there is always going to be this tradeoff as to what are you trying to hold static and what would you like stability in when you're picking your benchmarks.

I think, before I go on to the differences between the code and the progression in that, if there are any questions on these sort of tradeoffs between different benchmark targets and what is happening, this would be a great time to jump to questions, seeing as there were a lot before.

CHAIRMAN NANCE: Any questions on these? Doug Gregory, please.

MR. GREGORY: Thank you. Thank you, Nathan. I'm not sure that I followed everything well, but my concern, or question, is relating to the changing MSY based on selectivities, and I know that concept has been around for a couple of decades now, versus there being a single global MSY, as envisioned by say Congress in the beginning.

It seems to me that, by using a varying MSY, based on the selectivity of the fishery at that point in time, you can be on a slippery slope. What if the fishery evolved into let's say a fishery that just captured one-year-old fish, and there's an MSY based on that, and the MSST is based on the MSY, and so, even though you could be harming the overall productivity of the stock, is my impression, your benchmarks are all going to look good.

DR. VAUGHAN: In that case -- So that is the point of this, is that it's very important to incorporate that, because that is all we have. The fishery is catching what it's catching, and it's not ideal if they caught all age-one fish, but, as we saw, if you went back a couple of slides, that would be very low.

In this scenario for amberjack, if you did just say we wanted to have a fishery that only caught age-one fish, your MSY is nowhere near that global, and so it's going to be 30 percent, it looks like, 30 percent of the global MSY that we're handling, and, in that case, your spawning stock biomass does maintain the same threshold, and so we're at 40 percent, and so you're going to maintain that, because of what our benchmarks do, in that they're targeting an amount of spawning biomass that's available.

If you fish at those age-one cohorts, you're going to get a really low yield, because you still have to maintain that 30 percent, and you're only catching them when they're small, and so we are -- The fact that we're doing those benchmarks is inherently accounting for that low selectivity, and so you're going to get a very low yield, because you're going to have to leave all those fish to live the whole rest of the time period.

You may only get to kill them at age-one, but you're just going to catch a lot of small fish for your efforts, and so having that selectivity-dependent MSY -- That's what helps with the global

MSY, is we can show people where they could get to if we were able to optimize the fishery to only fish exclusively at the most optimum size, but, in this case, for amberjack, it would suggest that it's best if we could adjust the selectivity of our fishery to say target only age-four to eight fish, and that's where we're going to get the most bang for our buck, in terms of yield.

I don't think that there's actually a slippery slope to reducing the stock, because that's inherently tied up with our benchmark, but, as we talked about before, with that sliding averaging of recruitment, that is potentially a slippery slope, if you went to an SPR target, because you would be constantly be dividing by that smaller value, and so you are, in that case then, able to potentially shift the fishery, slowly.

MR. GREGORY: Thank you, and I guess I'm showing my age. I come from the yield per recruit era, where you want to maximum yield in the fishery, and, by letting MSY be whatever the selectivity is, there's no incentive. There is no direction to try to maximize yield, and so I guess that's my conundrum. Thank you very much.

DR. VAUGHAN: By having it adjust to the selectivity, that should still -- We can show people this global MSY, and that should help encourage them to adjust selectivity, but we do need to -- We couldn't set that global MSY as our MSY, because that would -- Like, if we were to then -- What would achieve that is dependent on the selectivity of the fishery, I guess. At those lower selectivities, there is no way to ever achieve that large catch without completely depleting the stock, which is why we need to adjust that selectivity in our calculations, if that makes sense.

MR. GREGORY: Yes. Thank you.

CHAIRMAN NANCE: Thank you. Mike.

DR. ALLEN: Thank you, Mr. Chairman. This has been really helpful, to work through the example with amberjack in this discussion. I wonder though how it would vary if we looked across species and across stock assessments. In this case, there's a really higher virgin recruitment predicted in the past, and the stock is heavily fished, and I just wondered how this discussion might change if we looked at some other species examples, which I think might be useful down the road.

That being said, I do think that we typically have a huge amount of uncertainty in what the virgin recruitment was and a huge amount of uncertainty in the latest recruitments, and so the idea of using kind of the intermediate data-rich time series makes a lot of sense

to me, but I would like to see what the group thinks about what if we looked at this for some other species, because I think it could influence our thinking some. Thank you.

DR. VAUGHAN: Thanks, Mike. That is very true, and I think what makes greater amberjack, in this case, such a good example, and what has brought it to the fore here, is that, in this scenario, it is a relatively large impact. We're seeing a stock assessment that has an estimated relatively low steepness, and so we're getting a lot of these impacts of recruitment, and we're seeing recruitments that are very different than the historic scenario, and so this is a relatively large impact scenario.

A lot of the reason that these changes may not have been developed as heavily and prioritized in the past is that, for a lot of assessments, it hasn't been a big impact. Like we've stated, they've had assumptions of a steepness of one, and they've had relatively constant recruitment that weren't impacted in the future, and so all of these changes -- You are correct that it's very species specific, but it's these scenarios that are very heavily impacted that have driven the need to develop these adjustments.

DR. ALLEN: Yes, I agree. It is a good example, for those reasons, Nathan. Thank you.

CHAIRMAN NANCE: Also, as we go through gag tomorrow, we'll be able to use this knowledge that we're gaining today to be able to do a good job when we talk about it tomorrow. Harry.

MR. BLANCHET: Can you go back to your page number 3? I'm a bit confused. For 2022, 2023, 2024, you've got yields that average up to the -- That sum up to the total yield, but, when you go out to 2119, the yields do not add up to 1,100, and they are not in the same proportion as they were for the first three years, the first three columns, and can you explain that to me?

DR. VAUGHAN: You're certainly correct, and that must have been a copy error when I moved these values over. They should 100 percent be adding up to 1,162 and in that same proportion, that value. I copied this out of the report table for one of the projections, and so I must have copied the wrong cell number there, and I probably copied 2025, maybe, or, no, and even that doesn't make sense. It's something that I pulled two of the wrong numbers, and so that's what is happening there.

MR. BLANCHET: Okay. I was looking at it, and I had no idea, and I was totally lost there.

DR. VAUGHAN: That's a good catch. I can figure out what I did there afterwards and maybe submit a corrected slide for the council.

CHAIRMAN NANCE: That would be great. Thank you, Harry. Will.

DR. PATTERSON: Thanks, and thanks, Nate, for the presentation. Thinking about this presentation and then Katie's before, it seems to me, and maybe this is oversimplifying, but what it kind of boils down to is that, if we don't use a stock-recruit relationship, and we have an SPR proxy for FMSY, then it matters what we're given, as far as the projected biomass unit or measurements are in the ratio, in that needs to actually show us SPR and not some other closely related but not the same values.

The second issue is that it matters whether we select lower versus higher recruitment to project forward, and that affects productivity and estimates of the stock, and I think we were pretty aware of that, and we purposefully, in the past, used the more recent time period, because we were only projecting for a few years, and I think the SSC has felt more comfortable using the more recent estimates of recruitment.

Now in the sort of regime shift scenario that I think that Katie was presented, but more in the -- We know that there is decadal variability in climate that can have density-independent effects in recruitment, as one example, but, just on shorter time periods, we can have other factors that are driving recruitment, and so we projected the more recent values forward, because we felt like those would probably be more likely than historical higher values or historical lower values to reflect stock productivity in the near term. I am less worried about that component of this, and it seems like the consistency of what we're actually measuring and projecting is probably a pretty easy fix as well.

CHAIRMAN NANCE: I agree, Will. That was a great synopsis.

DR. VAUGHAN: For those projections, it's just whether they're -- The assumptions of what we choose for recruitment is it's just that long-term impact on what is a sustainable level, and, when we use SPR, we make that assumption that's what we're going with, which is what we currently have set on the books.

It doesn't have a large impact, obviously, if F stays the same, and so, for our recent yields, it's not going to matter what assumption we put in there, because the F is always identical, and the population is going to be what it's going to be, but it does

have a big impact on that current overfished status, but it may not be the most precautionary, in that it's --

We think that it's precautionary in the near term, if we set Fs based on that, but that generally doesn't have a big impact, because those first year classes are not well represented in the fishery, and so it somewhat is, because we have a three-year gap now between management and the final year of the assessment model. If that was closer in, we would not likely be seeing those small fish in the landings.

CHAIRMAN NANCE: Okay. Thank you, Nathan. If there's no more questions, we can go on to the presentation.

DR. VAUGHAN: Thank you. Just one final note, and that is that, we make that assumption, if we were to go down the road of attempting to do our projections for equilibrium benchmarks separately from the catch projections, as far as using different recruitment levels, it is going to need -- There will need to be decisions made on which projections do and don't include those recent versus historic recruitment, as far as it a benchmark --

Are the rebuilding projections using the recent low recruitments, versus the target of what we're trying to get to, and that's going to have a very different outcome, and we may get ourselves into the situation, and not if we're using the SPR, because we're allowing that to reduce our target, if we change our recruitment level, but, with the use of biomass, you could get into a situation where that low recruitment is never able to recover to your target of say 30 percent spawning biomass ratio.

The recent recruitment may be so low that, no matter how you fish, you can't get there, and that's kind of what we were getting to with that bottom plot on Katie's presentation, that bottom gray line. Catch was so low, because you basically have to stop the fishery just to recover to the SSB 30 percent, and there's the potential that maybe the recruitment could be so low that we can't, even if we shut down the fishery, and so it's just those sort of interactions that may cause trouble in the future, and they can have unanticipated feedbacks.

CHAIRMAN NANCE: We do have one more question from John.

DR. FROESCHKE: Nathan, thanks. Contemplating a different scenario that we've talked about so far, what would happen in the event where you calculated, based on the long-term, as you explained, but used the short-term recruitment for the recent catch, where short-term recruitment was very much higher than the long term?

It would seem that you would get OFLs, or ABC catch advice, that is higher than what your long-term OFL would be.

DR. VAUGHAN: Your recent recruitments are higher than the long-term expectation, and did I get that right?

DR. FROESCHKE: Yes, and it's the opposite of what we've discussed so far.

DR. VAUGHAN: I probably didn't mention that enough. That is important, in that, if we have recruitment -- As we've presented here, the recruitment is lower than the long-term R0, and we're expecting the SPR to increase our Fs, to give relatively larger F allowances that the SSB method.

If we were to switch that up and say, okay, the recent recruitments are higher, we're actually going to find that SPR tells us that we can fish less than the SSB, because it's not giving us credit for that large -- It's thinking that recruitment is going to go on forever, and we're going to end up with much larger recruitment, and so it's going to expect you to fish less, but whether those are good ways to do these projections or not is a question for the SSC, for you guys, to answer, but they can have potentially counterintuitive impacts on what your benchmark is going to be and how that's going to affect it, but it will reduce the catch relative to what would get from the biomass. These trends would flip.

CHAIRMAN NANCE: Okay. Thank you, Nathan. Would you go ahead and go on with the presentation?

DR. VAUGHAN: Thank you. If we flip to the next slide, we're going to move on to the progression of how we got to these results, and so I will give you some kind of sense of exactly what has been going on, and maybe not exactly, but a rough sense of what's been going on under the hood within Stock Synthesis and then the subsequent adjustments that we have made to continue building and evolving this projection approach that we are currently using.

The biggest difference is the somewhat default approach to fishery projection is to assume that this annual effort, or F between fleets, is going to stay constant into the indefinite future. The reason that works so well is that it allows you to do an equilibrium approximation that doesn't -- Rather than needing to use this hundred-year projection, using old Fs of all the different fleets, you can simply sum up the fleets as they stand, with their relative efforts and their relative F at-age, and then do a single cohort projection, and so you say how many fish are there, how many

recruits are coming, what is the F on age-one, and reduce the number of recruits into the age-one class, and then what is the next F at age-two, and so on and so forth through all the ages of your population.

That would give you the final structure of the population, which would be your equilibrium, and a catch at each of those ages that you can sum up to find your total OFL, and so it's very fast, and it doesn't require any projection, and it doesn't require estimation, but it does make very big assumptions on the exact selectivity of the fishery, which may not mesh with what we're assuming versus the constant catch allocations and bycatch fleets and so on.

Once it's made, that simple projection, it calculates how it needs to adjust each year, and it does a hundred years of just F and OFL, and it projects that forward, and so it's just single passes that makes it so fast, but it doesn't incorporate any of those values.

What happened was SS said how are we going to account for allocations, and it does that single-run projection, and then it simply goes in and adjusts the catch between each of the fleets, after the fact, and that is why you no longer -- You get an output that has the relatively catches correct amongst allocation groups, but I was just trying to highlight here where in the code we're adjusting that, so you can get a feel for the complexity of SS's inputs and what we're doing, but it simply adjusts them after the fact, after the Fs have been determined, and so, in the future, you're going to see trajectories, under these new allocations, that both have different Fs in each year, which is the total biomass removed from the total population biomass, and that is our global F, and it's also going to have a different achievement in the final year, at a hundred years say, of its benchmark.

If you targeted a benchmark of 30 percent SPR, it might finally end up with an SPR of 25 percent or 35 percent or some other value. This is how did we attempt to adjust that, and so SS has an input to say what SPR, or what biomass target, do you want to achieve.

In this case, it's 30 percent, but you can adjust that, and so the approach that was previously taken, and this is what's been used for a good long while now, the last ten years, I think, at least, has been to adjust that target, iteratively, and look at the achieved value in the projection period, at 100 years, to say did we achieve that, and, if we didn't, make an adjustment.

In the case of amberjack, you may have put in a target of 30

percent and then achieved a final value of, once you put in the allocations, a final value of 40 percent, and so we'll say, okay, we achieved 40, and we don't need to be as -- The population is better than we expected, and so we'll move our target down to 25 and rerun the stock assessment, rerun the projections.

That would run again, and we might find that we've got now a value of 32 percent achieved SPR, and we drop a little bit more, to 23 percent, in our projections, and so we converge in on something that achieves our desired benchmark at equilibrium.

That was relatively fast, and it's only a single adjustment that needs to be made, in that one projection value, and it now achieves that SPR, or SSB, benchmark proxy that we wanted to. The important things are that this doesn't iteratively adjust specific years, and so we're not able to always achieve those F targets, and this has been presented to the SSC before for other assessments, where you've used an average over the catches in the first five years of OFL estimates, to try and bring down the impact of that slight skew in Fs in the recent years, but it was -- Again, we're evolving, to try to improve what we're getting out of these assessments, so that, as we can, given the computational power and software that we have available to do this.

Also, this approach is not applicable and couldn't be extended to doing MSY or Fmax projections, because that is all internal to SS and relies on those assumptions. There is no way to say, well, I want to achieve -- Fish a little bit harder than what SS thinks my MSY is, and so that wasn't -- You couldn't adjust this approach to that, and so, if we move on, that was the recent -- Up until this last assessment that you say, that was the Southeast Center's approach.

In order to incorporate the increased complexity of fixed catches for commercial fleets and changing recreational fleet catches and to also correct this percent F deviation, we have developed this new approach, which SS allows you to put in fixed catches, and so this is how we input the recent catch history for these fixed years, and so 2019 through 2021.

When we have those interim year catches that we need to put in, we usually fix it through that, but we have extended upon this to now put in fixed values of -- We'll adjust and put in values for all those years, for a hundred years out, and so what you're seeing on the right is fixed catches to 2025, but that column actually extends all the way down to 2118, and it's a hundred years of projections, and so 2119.

We put in starting estimates of F, and that's -- On the side there, those three values that you see, that's saying that we're putting in fixed catches, and then the 99 means that we're putting in an effort value for those groups, and so we came up with this approach where we would input a recent average F and then iteratively adjust those values to try to achieve all of our benchmarks at once.

To do that, we're really looking at three benchmark types, or three targets. We're looking at our benchmark, and so our stock status we're trying to achieve at a hundred-year equilibrium, and we're looking at annual Fs every year, and we're trying to make sure that that F is equal in all the years, the OFL F, or rebuilding F, if we do a rebuild projection, or ABC, and then also the allocations amongst the fleets, and we're trying to keep those catch, landed catch, allocations constant in all the future projection years, and then, within a sector, we're trying to keep the effort constant, because that's our best estimate of how those fleets are going to react to the fishery.

If you move to the next slide, in order to achieve these three benchmarks, we have three different scalars that are going to be used to adjust those annual fleet-specifics Fs, and those Fs are an effort F, as opposed to the global biomass removal F.

We have this first equilibrium benchmark scalar, and we're basically going in here, and you don't have to follow all the code, but it's here, and, if people have questions on any of that, jump in, and I can answer them.

We have this first little section of code here that basically looks at -- In this case, it's depletion, or SPR, and it looks at the value you've achieved in the final years of the fishery. In this case, I took a median of the last thirty years, just to make sure that you don't have any search issues within SS. If you overfish a stock, say, things can go a little weird in the projection period.

It looks at that achieved value and takes the ratio of that to the target, which would be 30 percent in this case, and there is some code that does an adjustment based on X, but that's not really germane to this, and then it makes an adjustment, and so it says what is that ratio of where we are fishing to where we would like to be, and then it takes that single value and applies to every single F, and so F gets multiplied by that scalar, and that single value might be multiplied by 1.3 times, to increase the fishing effort, and you multiple every single F in the forecast period by that 1.3.

If we go on to the next slide, we now have a more refined scalar, and we have annual F, and so we're trying to achieve the same annual F in every year, and we want to do that without -- We want to search for that value without impacting the benchmark target scalar, and so we now take the median value of all the years of the forecast period that were fixed, and so we throw out those first three years, figure out the median F from all those years, and then we add a multiplier that's annual, and so we compare each year's achieved F to the medium F over the whole time period, and we do a ratio of that, and we multiply each year, to bring them closer back to the median value, and so that will -- Eventually, as you can think about it, as you're approaching the correct value, they will sort of slowly converge on the OFL F, by making those adjustments.

Then, if we go to the next slide, we're also trying to come up with correct catch allocations between sectors, and so we're going to look at how much catch was landed by each fleet and compare the total for a sector to the total for all fleets and then do another scalar on each of those fleets that adjust. If one is too high, and the other is too low, it will bring one up and one down a little bit, to bring them closer to their target, and so that is going to be applied, and that multiplier is going to be applied to each fleet within a sector and within a year, and so that is an annually-specific value and also a sector-specific value.

Then, by default, we manage to achieve that constant effort between fleets, because all of these multipliers have been applied equally to any fleet within a given sector, and so those will stay the same, and so our starting values are what define those relative fleet-specific efforts.

Then combining all of those together, this is just sort of -- At the top there, you've got those three multipliers, F mult 1, 2, and 3. We just multiply all of those together to get a combined multiplier for the entire fishery, and then we go in and we multiply all those values to that forecast file F input, and then we override it and then rerun SS again, and we just keep iteratively doing this until all of our targets have been achieved, and there's a threshold value you can put in there, into the code, that is set at a default of like 0.001.

Once your deviations between your target and your achieved values are less than that threshold, it will finally -- For everything, it will say "stop running", and that will be your final output.

The first loop will be for OFL, our new given target, and I will go on now to the next slide, and we also have a built-in capability

to do this for ABC and rebuild, so that those can be also achieved with all constant Fs and constant allocations correctly distributed, and so those three loops, and the first loop, as I just described, is the OFL, and that's targeting a benchmark and F every year and allocations.

Once you have achieved that, that becomes your F OFL, and we've now got an estimate of what that target of fishing limit, F, is, and then I've got an ABC option that, if you input a value that is some percentage multiplier, say F ABC of 75 percent of F OFL, you can input that, and there is no longer a benchmark target to achieve, because we don't have a fixed stock status that we want to get to, but you know have -- So you've set that benchmark scalar at one, and now you just target -- Rather than targeting the median F, we are now targeting that fraction of F FOL, and you're doing the scalars on annual Fs and allocation.

Then the same thing for rebuild, except now you take an additional proportion that you say -- All the user puts in is a rebuilding year, say 2027, and so all years leading up to and include that 2027, that will be able to adjust that F in order to achieve rebuilding, the original SSB at OFL fraction, and then years after that will be fished back at the original F OFL. Will, if you want to jump in, this is probably a good point for a question.

DR. PATTERSON: Sorry, and I don't mean to interrupt you. I was just getting my name on the list, but, while we're here, I am curious about total kill versus harvest and you how track that for various sectors.

These are fishing to the landed -- These are fishing to landings, and so retained catch, as opposed to total kill. All of this, again, is happening inside of Stock Synthesis, and kind of the overarching thing is Stock Synthesis is doing the projections for all of these things, and so everything that was in the stock assessment model, all the selectivities, the retention, the discarding, the discard mortality, that is all being maintained by Stock Synthesis, but all of these projections are operating off of the retained catch, and so the sector allocation is the retained catch by the sectors and not the total mortality, but the total mortality is impacting the fishery.

DR. PATTERSON: Can you output that within your code, or do you have to go back into SS to get those different aspects of the total catch?

DR. VAUGHAN: You would need to -- Within SS, within the outputs to SS, to see what was the total catch and what was the -- This is

not particularly outputting anything. All those results that you see that come out, and the results that Katie showed, they are -- Within the Southeast Center, we have functions that can go in and read the SS files and extract all the values that we're interested in, and so the total discards, the total retained catch, the population status, all of that is being pulled out by separate code. This code is merely rerunning Stock Synthesis and adjusting the input values to that forecast file, in order to achieve the benchmarks that we're trying to achieve simultaneously.

DR. PATTERSON: Okay. Thanks.

DR. VAUGHAN: There will be output, but it's from a separate code to this, and so it's not necessary, and then, as I said, all of these are saved into that OFL, ABC, and rebuild folder, and that's where our other functions would be run, inside that folder, to extract all the values that you're interested in, and, each of these, obviously, the OFL needs to be run to get your benchmark, and the ABC and rebuild could be run -- Both of them could be turned off as these -- You could just input the value.

If we go to the next slide, there was some differentiation that I wanted to point out between what you're setting as your benchmark, your forecasting targets, and how this operates, and it may be more than you need to know, but I just wanted to be transparent in how this is doing it.

We've got SPR and SSB benchmarks, these proxies, and they are relatively simple to achieve. We have a known value. Each time you run SS, you get a value for SSB, or SPR, in every year, and you also -- You know what your target is. It's 30 percent, or it's 26 percent, and so you have -- There is a direct search that I can say, okay, we're targeting 30, but we've only achieved 25 percent, and so we should decrease F a little bit, in order to increase our spawning biomass, and so that's a straightforward search.

This came back to that question that we had before, and my bottom point here is that, when we're projecting recruitment that is less than R0, the F is going to be greater under the SPR approach percentage target rather than SSB, but, if the recruitment is actually larger than that, we're expecting SPR to be less than it would have been under the SSB scenario, and there are a number of implications for those. The point was that this is a relatively direct search, and it's a single loop.

For MSY, that's a lot more complicated, and this is why it's not as efficient. You're trying to find MSY, and we're trying to keep

allocations constant, and we're trying to fish at a fixed F and do all these things, but we don't know what MSY is eventually going to be, and so there is no way to directly search to find your MSY, because we don't know what we're actually trying to achieve, and so, to do this, we come up with a two-stage search that basically searches first for an SSB target, and it starts out with a default value and slowly iterates on that, but it searches for an SSB target and will achieve that with all of its allocations.

This is some changes we made, and it's very important that you sort of incorporate those together, because your allocations will affect what your MSY is likely to be, or what yields are possible, and so we do each in an iterative first stage, where we search for a default SSB percentage, and achieve that search, with all the correct allocations, and then adjust the target a little bit.

If we've got an SSB of 0.25, we move by a step of -- We move by a bit and we go, okay, 0.3, or 0.35, and there's a step size, and my default stocks all had a step size of 0.1, but it moves up a little bit and runs that whole search again and finds out what MSY would be achieved at the 0.3 SSB percentage, and that might be higher, or it might be a bit lower, but there is a dome-shaped pattern between the percentage spawning biomass you achieve and the total MSY, or the achieved yield at that spawning biomass percentage.

We slowly adjust those SSBs, iterating backwards and forwards through the search, until we hone-in on what is the maximum yield, what SSB achieves the maximum yield, and so that's the two stages.

This is the first bit of code, but it's hard to see on these, probably, for everyone, is that outer loop, and it's trying to achieve -- Or the inner loop is trying to achieve that SSB fraction, and this is where we're adjusting our target SSB, in order to hone-in on the actual MSY that we're trying to achieve, and so that's how we are performing that search.

If we go to the next slide, this is validation. As I said to Will's question, all of these results -- We're not actually changing how projections are done so much as just trying to make sure the values that are returned in the projection outputs are what we're targeting, and so all of these are very fixed things, the SSB achieved in each year, the SPR achieved in each year, catch allocations, and you can go back into the forecast values, and these are the values that we pull out for the reports, and so you can directly compare these, straight one-to-one, between what you tried to target, and did I try to target SSB 30 percent, and then you can go straight to that report file and say what did I achieve, and I did achieve 30 percent, and so it's inherently directly

validated every time we present these results, in that they are what we said that -- As long as the results that come out are what we said we wanted to return from the report, we can see that F every year is constant, and that's how we're validating the outputs from this.

This is just a plug for a project that we just got funded. As I said a couple of times, and Katie has, this is an evolution of how we're doing these projections and how we're doing forecasts in general, and so we now have a project funded for myself, John Walter, Katie, and Skyler at the Science Center, and then Ryan on the council, and Nick Farmer at SERO.

We're all going to work on this project together, to try to figure out, for the next generation, how do we want to improve on our forecasts further, and so what -- In three phases, basically, and what things would we would like to incorporate into future forecast projections, what better data could we incorporate, how could we improve on our assumptions, and then Phase 2 is really how do we incorporate this, collaborating with all of you on the SSC and the council, to say how do we incorporate this directly into management.

We can say that we're going to fix allocations, but what tools do we need to better enable you to understand what the impacts of the different allocation decisions would be, and should we give you a tool that enables you to adjust that yourself, or the council to adjust that at the endpoint, and it automatically gives them an OFL, rather than this sort of repeating one number, and it has to continually be iterated, and so how can we figure out that workflow, and then, also, what software, and so interfaces, do we need to do that.

It's going to be over the next year that we're planning to work on this, and we look forward to collaborating more to figure out how to make all of these questions that we're discussing today more transparent and more easily interpretable, not just to you on the SSC, but also to the council and to the fishery community, because they're the ones who really have to understand and believe this, if they're going to implement these and feel confident in the OFLs and the quotas that we're setting, and so that's really the goal of this.

Then the next slide is just questions, and so the code is out there, for those inclined to read about it, and I am more than happy to answer questions now, and, if you have bigger, longer questions, that is my email, if you would like to dig into the details.

CHAIRMAN NANCE: Thank you, Nathan. If we have any specific questions on the presentation, we'll take those now, but I don't want to have a long discussion right now, but are there any questions on Nathan's presentation? Okay. Nathan, thank you for that. It was a great job. Now we're going to go ahead and take our lunch break, and we'll come back at 1:15 Eastern Daylight Time.

(Whereupon, the meeting recessed for lunch on September 27, 2021.)

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September 27, 2021

MONDAY AFTERNOON SESSION

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The Meeting of the Gulf of Mexico Fishery Management Council Standing and Special Reef Fish, Special Socioeconomic & Special Ecosystem Scientific and Statistical Committees reconvened on Monday afternoon, September 27, 2021, and was called to order by Chairman Jim Nance.

CHAIRMAN NANCE: It looks like we're back live again. I guess I'm going to start with, over lunch, did anybody come up with any questions or concerns from Nathan's presentation? If not, Katie, I think you have the floor now.

DR. SIEGFRIED: All right. I was going to start on Slide 25 of my presentation, but Nathan might want to speak first.

DR. VAUGHAN: There's an error on the slide, and it was supposed to be 848 and not 484, and so it was just a dyslexic copy-over error.

CHAIRMAN NANCE: On the one that Harry was talking about?

DR. VAUGHAN: Yes, exactly.

CHAIRMAN NANCE: Thank you so much.

REVIEW OF SEDAR 70: GULF OF MEXICO GREATER AMBERJACK STOCK ASSESSMENT

DR. SIEGFRIED: All right, and so, after Nathan's very thorough presentation about how the code works, I have to tell you that it's slightly anticlimactic what I have to show you for that, and

so what I have put into the yellow boxes is where the differences lie, or the values that we need to pay attention to, and so this is comparing the same runs, and so the same decisions, with and without the new code, and so the first table of values is our base run from SEDAR 70 with SPR 30 as our target and recent recruitment, and so 2009 to 2018, as was reported, but not shown, and then the base run with those same specs with Nathan's code.

This new code accomplishes two straightforward tasks. We have to achieve that SPR 30, and then we have to keep our Fs fixed at F SPR 30, and so what I have highlighted here, in the top matrix, is the yellow that shows you that, with the old code, we're not actually getting to that 0.242, which is our F SPR 30, like we are in the second table, where the F is held constant, which is what we should have with constant F projections.

Because we are the Southeast, we have lots of peculiar data streams and conditions and very complicated management, and SS has just not figured out how to do this in the past, and the Science Center has been trying to get that code that will do this, and we finally have that.

This is what we should have shown you last time, and it's an apples-to-apples comparison, instead of apples-to-oranges, and so the top table shows you that we get to an SSB over SSB_0, which is our depletion ratio, and that is, or I'm sorry, the depletion, or the biomass ratio, is 13.3 percent when we get to an SPR of 30 percent, which is very low.

We thought it was important to point that out to you, because I am not sure that the SSC was really aware that that's what the tradeoff is here, when you get -- Nathan showed that in his last -- I forget which slide it was in the presentation, but where he showed that if you use SPR 30, you're going to get that really low depletion.

You can also compare the OFLs that are projected in 2022 to 2030, and, as we showed with our presentation in July, they're, overall, really not that different, which is why we didn't really pick up on this being a major issue with the previous base run projections, but it does solve the problem where we're not getting our fixed Fs in our old code.

The purpose of this was to show you that things are held the way that we want them to with the new code and that the difference is purely the F in each year. Are there any questions about this? I know that you all had that complicated presentation from Nathan, and I talked earlier in the morning, and you just had lunch, but

do you have any questions about this apples-to-apples comparison?

CHAIRMAN NANCE: Doug.

MR. GREGORY: Thank you. The column, the seventh and eighth column, where it says like "SSB_SSB SPR 30", is that a ratio?

DR. SIEGFRIED: Yes.

MR. GREGORY: Okay, and this is the base model without the new code.

DR. SIEGFRIED: The top one.

MR. GREGORY: Why are these numbers so different than the numbers that were in SEDAR 70?

DR. SIEGFRIED: Because there is two big differences. The base run before was shown with an SPR -- With a biomass ratio, the depletion rather than the target being SPR 30, which is showed you in my figure, that that makes a big difference when you assume SPR 30 as your target, and, also, the recent recruitment is actually realized here, and you will see the 1,650, rather than the 2,800 that was shown in January for the base run projections, and so those two corrections were made, and this is what we should have shown you in January, is the top table.

MR. GREGORY: My concern, and the thing that has prompted a lot of my scrutiny, is, in January, the analysis showed that overfishing was occurring, dramatic overfishing, and the spawning stock biomass was only 68 percent of MSST, or like 20 percent of MSY, and that's grossly overfished, and dangerously overfished, and that overfishing was occurring, but the presentation today shows the opposite, and so, if that's not the new code, then we need to be scrutinizing the differences between the January analysis and what's being recommended now, because it's just disconcerting to have the world turned upside down from one analysis to the next. Thank you.

DR. SIEGFRIED: I understand that, Doug, and that's why we wanted to walk you through each of these potential implications of the decisions, because -- I remember Joe Powers saying something, when we showed this in January, that it should have looked worse, that our numbers for R should have been lower, and so maybe he was picking that up, but we didn't run it with the correct level of recruits, or at least the level that we told you that we were running it at, and so that makes a huge difference with respect to our benchmarks and then the ratios.

This is still showing that the stock is overfished, because it hasn't gotten to SSB over SSB proxy, SSB MSY proxy, and so you see that that occurs -- At OFL, it's going to occur in that last year, if you round up, but so it's still in an overfished state, and we're showing this to you just the OFL, and so we do think that it needs to be reconsidered. The projections need to be reconsidered and that the SSC should tell us their newly-informed decisions about these settings. We will rerun the projections with those settings and provide those tables and all the standard output for your consideration.

MR. GREGORY: So am I misunderstanding Column 8, the SSB_MSST SPR, or are you saying that the analysis in SEDAR 70 was not based on SPR?

DR. SIEGFRIED: The projections were not based on SPR. They were based on the biomass ratio, the depletion.

MR. GREGORY: Okay. I didn't catch that.

DR. SIEGFRIED: That is something that we're being more explicit about here, and so the MSST -- It's been MSST and the SSB MSY ratio, but it's still in an overfished state, and so this is -- Then, if we show you the rebuild projections, which we have in later slides, it will show you the different time tables for recovery, but these decisions were not discussed, and we may not have been explicit about this, and we did have to make the correction about the recruits, and so that's why we wanted to take you through all those decisions and show you this, and then we're going to have to discuss the projection settings.

CHAIRMAN NANCE: Ryan and then Roy.

MR. RINDONE: Thank you, Mr. Chair. Whether or not greater amberjack is considered to be overfished is based on the ratio of the spawning stock biomass to the minimum stock size threshold, and so, under both of these circumstances, greater amberjack would no longer be considered to be overfished, because that fraction is greater than one.

Relative to the old metric for determining whether or not the stock was overfished, which would be against SSB at MSY, then it would be a different story, but, under Amendment 44 to the Reef Fish FMP, those status determination criteria were revised for several species, including greater amberjack, such that, so long as the ratio of -- Excuse me. The minimum stock size threshold was defined as being equal to 50 percent of the spawning stock biomass

at the MSY proxy.

For that column there, that SSB_MSST, and assuming that SPR proxy of 30 percent, under both of these scenarios, greater amberjack is no longer considered to be overfished. Something that I do have a question about though, Katie, is for the column immediately to the left of that, for SSB at the SSB at MSY proxy, and it doesn't appear that we ever really get to a value there that is greater than or equal to one, which would indicate that the stock is rebuilt, because the rebuilding plan is based on that value of SSB compared to SSB at MSY.

I was wondering if maybe you could elaborate on that, if that's just a function of the projections, or what we're looking at, or what we would have to do to see that value by 2027 actually be greater than or equal to one, to indicate that the rebuilding plan had been achieved.

DR. SIEGFRIED: These are OFL projections, and we have rebuild projections, and I was under the impression that it was still under the rebuilding plan until it reached SSB SPR 30 and not -- I guess, if it's not considered overfished, because it's been the two, it's still in a rebuilding plan, and is that what you're saying, Ryan?

MR. RINDONE: Yes, Katie. That is exactly correct, and I realize that that can be a -- That's been a source of confusion for all of us at different points in time.

DR. SIEGFRIED: Okay. Well, our rebuild projections are -- We have that available, so that we can show you which level of F is required in order to be rebuilt by 2027, but this is -- I mean, this is the status that we were showing you here, and this is whether it has reached MSST and whether it has reached the SSB SPR proxy. It's also important to note that, if that's the case, then we would be down to six-and-a-half percent of virgin stock size to be considered overfished, and so what I'm saying is, when we reach -- Right now, we're at 13 percent, approximately, once we are rebuilt, and so that means that we would be down to around six-and-a-half percent of virgin biomass, or virgin SSB, sorry, to be considered overfished.

MR. RINDONE: That's all under this recent recruitment scenario, and so I would presume that those values would change, depending on your assumptions about recruitment, correct?

DR. SIEGFRIED: Yes.

CHAIRMAN NANCE: Roy.

DR. CRABTREE: The virgin biomass that we would be at 13 percent of, is that virgin biomass figure using the stock-recruitment relationship recruitment, or what recruitment is that based on?

DR. SIEGFRIED: The SSB_0 -- Nathan, that doesn't change, right?

DR. VAUGHAN: No. The SSB_0 is a fixed value.

DR. CRABTREE: What level of recruitment is it assuming?

DR. VAUGHAN: That is assuming recruitment at the initial virgin population, and so, for us, that's R0, and so it's assuming R0. If you had a changing R0 throughout time, it would be the R0 at the beginning of the model.

DR. CRABTREE: Okay, but the question is whether that level of recruitment is possible at this point. If it's not -- The fact that we're at 13 percent of that, I don't know if that's really real or not, and, Ryan, the reference point on the books for this is 30 percent SPR?

MR. RINDONE: That's correct.

DR. CRABTREE: Okay, and so what we would -- The rebuilding plan ends, the current one ends, in --

MR. RINDONE: 2027. This is the second revision of the second rebuilding plan for greater amberjack.

DR. CRABTREE: So I don't know if the 13 percent value is really that concerning or not, because, if the productivity of the stock is less, than it wouldn't really be 13 percent, because the unfished biomass now would not be as high as what this SSB0 is, and I don't know how real any of that is anyway, because that has so much to do with what was going on forty or fifty years ago, when there was a lot of uncertainty to it.

CHAIRMAN NANCE: So you're saying that 13 percent SSB is using the old --

DR. CRABTREE: That's how I am understanding it. Now, that's using the --

CHAIRMAN NANCE: That's the original biomass clear back in --

DR. CRABTREE: If I'm understanding this right.

CHAIRMAN NANCE: Is that right, Katie?

DR. SIEGFRIED: Can you say that one more time?

CHAIRMAN NANCE: It's the SSB current over SSB_0 -- The SSB_0 is the historical virgin stock back at the beginning and not the stock when we get to the data that we're more comfortable with.

DR. CRABTREE: I suspect, even if you close the fishery completely, and had zero removals, if the recruitments stay at recent levels, I mean, you're clearly never going to get back to that level of biomass with these recruitments.

CHAIRMAN NANCE: Okay. Luiz, you had a question?

DR. BARBIERI: Thank you, Mr. Chairman. Katie, just to confirm something here, to make sure that I am thinking about this correctly, what you're saying here is that this basically recalculates the rebuilding trajectory after this assessment, but this is, I guess because of an SSC decision, or recommendation, at the last meeting, this was based on that low recruitment for the most recent period?

DR. SIEGFRIED: By rebuilding trajectory --

DR. BARBIERI: You have here this table from 2022 to 2030, and this is at F -- The F value there, on the third column, that is the value of F that corresponds to the FMSY proxy?

DR. SIEGFRIED: If it's 0.242, yes. The other values were the problem in the old code.

DR. BARBIERI: Right. But then you say that you have another table that has the same trajectory, but at the F rebuild?

DR. SIEGFRIED: Yes.

DR. BARBIERI: Thank you. That's what I was trying to ask.

CHAIRMAN NANCE: Okay. Thank you. Doug.

MR. GREGORY: Okay. Thank you. At the risk, I guess, of beating a dead horse, or exposing my total ignorance, going back to SEDAR 70, I see, in one place, where there's a recommendation from the assessment that we change our goal from SPR to MSY, or at least that's the impression I get, but yet, as I read through the document, I see spawning stock biomass at SPR 30 percent throughout, and that's what our motion was based on, and so I still

don't understand why SEDAR 70 is so different than the tables that you're presenting.

DR. SIEGFRIED: Can you tell me where that motion for MSY -- I'm confused about that, because I thought that TORs had F SPR 30 in them and not FMSY.

MR. GREGORY: I don't know what the terms of reference had, but our motion to accept the OFLs was based on a projection of SPR 30 percent, which is what is at the top-most part of this table.

DR. SIEGFRIED: Yes, and both of them are, and so the error in those projections were the recruitment and the fact that we weren't using SPR 30 as our target. That is why these are so different from what you saw in January. Those are the two adjustments we had to make to be consistent with what we said we were presenting and what you made the motion on. As far as -- I am confused still about MSY, because I am pretty sure that we kept F SPR 30 throughout.

MR. GREGORY: I am looking at, I guess, one of the early tables in SEDAR 70 that says what the current definitions are and what the proposed definitions are, and the -- If I can find it. I will just leave it alone, for the time being.

CHAIRMAN NANCE: Doug, are you talking about at our meeting in January?

MR. GREGORY: Yes.

CHAIRMAN NANCE: Carrie, to that point?

EXECUTIVE DIRECTOR CARRIE SIMMONS: I will try. Thanks, Mr. Chair. In that report, Katie, it says recruitment for the projection period was fixed at the mean of 2009 to 2018, and what you're saying is that was not done, and is that right?

DR. SIEGFRIED: That's right.

EXECUTIVE DIRECTOR SIMMONS: Okay, and so that's the first difference. The other question I had is it says in the report that the 2019 preliminary landings were used, and those were classified as final, and how was 2020 and 2021 handled for these projections?

DR. SIEGFRIED: That was an average of 2017 through 2019, I am nearly 100 percent sure, but I will check with Nancie. It used to be 2016 to 2018, but, when we got the real value for 2019, it was

shifted to 2017 to 2019 for 2020 and 2021.

EXECUTIVE DIRECTOR SIMMONS: Okay. Thank you.

CHAIRMAN NANCE: Nancie, did you want to comment on that?

DR. NANCIE CUMMINGS: Katie was accurate in the last response, and, also, it was done at the January meeting, at the request of Mr. Rindone.

CHAIRMAN NANCE: Nancie, would you reiterate that, please, at the January meeting?

DR. CUMMINGS: Regarding the change. When the report was submitted, we used 2016, 2017, 2018 to inform 2019 and 2020, but then we were asked, at the council meeting, and this was after the report had already gone out, to use 2017, 2018, and 2019, because 2019 was now available, and so I don't have the page in front of me, I think, that Katie gave that informed those landings, but what she said was accurate.

MR. RINDONE: Nancie is right. I did say that, because the data were available, and usually we ask that most recent data be used.

CHAIRMAN NANCE: Okay. Thank you very much, Nancie, for that. Benny.

DR. GALLAWAY: What was the 2018 spawning stock biomass, Gulf-wide? I don't have it handy, and so do you have it handy?

DR. SIEGFRIED: I can find it here in just a moment.

DR. GALLAWAY: I'm assuming these are in units of thousands of pounds.

CHAIRMAN NANCE: It looks like they're in millions.

DR. SIEGFRIED: Sorry?

CHAIRMAN NANCE: Aren't these in millions of pounds?

DR. SIEGFRIED: SSB is in metric tons, and it was 2,433 metric tons in 2018.

DR. GALLAWAY: Okay. I will have to do some -- I struggle with the spawning stock biomass estimates. Our study in the western Gulf on petroleum platforms alone had like three-hundred-plus-thousand fish, weighing, on average, twelve to fourteen pounds, on

the platforms, mainly in deep water. When you add that up, it comes up to a very large number compared to the total Gulf number, and so what I am struggling with is how confident are we in the spawning stock biomass numbers, and I believe there is an amberjack study going on that will provide additional resolution, and is that correct?

DR. SIEGFRIED: Yes, there is, Benny, and we didn't have -- That study was just getting started when we were completing this assessment, and so that was something that the SSC discussed when they were getting ready to vote on the SEDAR 70 stock assessment, that that would be available for the next go-round.

DR. GALLAWAY: It's now been accepted for publication, and it should be out soon, and the technical report is final and out.

DR. SIEGFRIED: Okay, and I think something that is important to reiterate here is that we're trying to show you that there is a very sharp distinction between the assessment and the acceptance of the assessment and then the settings and the acceptance of the projections, and so we needed to correct this for you, and we needed to make the decisions clear, and we needed to provide a new set of projections for your consideration, but what we saw that the assessment was accepted based on the fits to the data in that process, and so we aren't going back and sort of going through the assessment step-by-step. We are just showing you the effects on the projections, because that's how benchmarks are set, and that's really where the management advice comes through.

The assessment results should be considered in light of things like the regime shift, because, whether you believe the depletion or not, this is what is estimated, given the data that we have, and it's certainly within the purview of the SSC to decide whether they think that a certain set of recruits is more reasonable, going into the future, but it doesn't really open up whether we fit those data well in the assessment process, and it's really two separate scenarios.

In fact, in other regions, it's completely separated from the stock assessment, because they are two separate procedures. One is fitting a bunch of data, and one is optimizing results based on calculations.

DR. GALLAWAY: Are the available data dominated by data from the eastern Gulf?

DR. SIEGFRIED: Dominated -- Do you mean like informed mostly by the eastern Gulf?

DR. GALLAWAY: Yes. "Informed" is a better word. Yes.

DR. SIEGFRIED: Let me take a look, real quick. I mean, it would seem that that's the case, because it seems like there's less information, I guess, until now, about the western Gulf, and so the next assessment will be better informed by data from the western Gulf.

DR. GALLAWAY: This is taken to be representative of the Gulf as a whole, correct?

DR. SIEGFRIED: Yes, because that's what we had at the time.

DR. GALLAWAY: Right, but it's -- Okay. I believe it's probably a very good estimate for the eastern Gulf.

CHAIRMAN NANCE: I think that, Katie, you have done an excellent job in showing us what you had proposed to show us, and it's becoming clear what was happening, and so I appreciate you being able to do that. Ryan, you had a point?

DR. GALLAWAY: I agree.

MR. RINDONE: Yes, Mr. Chair. I just wanted to echo, on this last exchange between Dr. Siegfried and Dr. Gallaway, the only fishery-independent index that was used as part of SEDAR 70 was the joint video survey, which was looking at the Panama City, Pascagoula, and FWRI video indices, and those are primarily functioning in the eastern Gulf of Mexico, and so the bulk of the remaining data would be coming from Gulf-wide indices that are fishery-dependent, like the commercial, recreational, and for-hire surveys.

CHAIRMAN NANCE: Thank you. Shannon.

DR. SHANNON CALAY: Thank you. I just wanted to kind of remind the SSC that this all came about because we were asked to prepare rebuild ABC values based on F rebuild for a variety of allocations to the fishery, and that's where we realized that we did not have concrete decisions that were well documented by this SSC to complete those projections as part of the management process. Right now, I still don't have an idea of what the concrete decisions are from this SSC, and so, honestly, at this point, I don't know how we would proceed to compute rebuild ABCs.

CHAIRMAN NANCE: Luiz.

DR. BARBIERI: Thank you, Mr. Chairman. Katie, Shannon, and

Nathan, and I also want to thank you for the presentations and the discussion today, and this has been very helpful. As you can see, several of us have been somewhat confused about some of these issues and how they were handled, and I think our decision-making wasn't really that well informed before, and so I think that this has been really, really helpful.

To put in my two-cents' worth here, trying to stay consistent with the comment that I made before, I am looking at this as -- If we're talking about a rebuilding plan, and so developing estimates of equilibrium values over the long-term, for those, I think that using the long-term recruitment values, maybe the entire time series of values that exist, to reflect the entire range of productivity of the stock, would be the most appropriate.

In this case, the yield streams, at least for ABC, are going to be based on F rebuild, which is going to come out of that trajectory there, rebuilding trajectory, that is set. Now, for stocks that are not under rebuilding plans, and, therefore, we don't have that, necessarily, long-term anchor of an equilibrium value way out there, then we provide, I would say, management advice, and we want to have yield streams that are based on the most recent past, whatever that would decide to be, depending on the species and the case and the data availability, and that is simply to generate those yield streams of ABC for those most recent values. Does that make sense, Katie?

DR. SIEGFRIED: Yes, and so kind of the way we did it in the South Atlantic, where we used the benchmarks as they come out, but we adjust the recruitment only, and not the way the benchmarks are calculated, right?

DR. BARBIERI: Right.

DR. SIEGFRIED: Yes.

CHAIRMAN NANCE: Okay. Doug.

MR. GREGORY: Thank you. I have two questions. I guess one is for the Center and one is for Ryan. The one for the Center is, last year, I believe, we were looking at different allocations for red grouper, and we made recommendations for ABC based on that, and do those analyses have to be revisited with this new approach?

I see the Center strongly endorsing this new approach, and, to me, it looks like a big improvement on the overall process, but do we need to revisit the red grouper, which I think is the only one we've done so far where we do have a number of allocation

discussions coming up, and so this change to the projection process seems reasonable.

DR. SIEGFRIED: No, we don't have to redo red grouper, and that was actually the first time that this adjusted code was used, and Nathan and Skyler worked on that together, and they were able to hold the Fs constant, and then the difference between the SPR and the biomass ratio was not -- There wasn't a difference, and so there isn't a reason to revisit those.

MR. GREGORY: Thank you.

CHAIRMAN NANCE: Nathan, to that point?

DR. VAUGHAN: Katie covered it. There was no difference between the SPR and the SSB trajectories for that, at least no substantial difference.

CHAIRMAN NANCE: Thank you. Doug, go ahead.

MR. GREGORY: My next question is I don't recall us discussing anything about different allocations for amberjack, and where did that come from? Did that come from the council or the IPT or what?

MR. RINDONE: Doug, that came from the council. The council has several proposed allocation scenarios that they wanted examined for consideration.

MR. GREGORY: Okay. Thanks.

CHAIRMAN NANCE: Okay. Dave.

DR. CHAGARIS: Thank you. In these tables here, just so I'm clear, the spawning stock -- Under spawning stock biomass, it's the same as is in SEDAR 70, which is the twenty-three-thousand-seven-hundred-and-some metric tons, but the minimum stock size threshold, the SSB SPR 30 percent, or the MSST SPR 30 percent, is less than half now of what it was in SEDAR 70 with the new recruitment, and we're attributing this basically lowering the bar, and calling it a regime shift, and what I'm having a hard time with is, if this recruitment is low because the stock has been recruitment overfished, I don't think that qualifies as a regime shift, in my mind.

To me, that's recruitment overfishing, and, if we were to follow this path, as I mentioned before, then you would just recruitment overfish, call it a regime shift, lower the minimum stock size threshold again, and so I guess -- I don't know if this really a

question directed at anybody, but just what qualifies as a regime shift?

I mean, if there was some type of ecosystem-level productivity or something like that, that, in my mind, is more of a regime shift than recruitment overfishing, to the point that recruits are low, and then saying, well, now it's a lower productive stock, and that's kind of where I'm at with this.

DR. SIEGFRIED: I understand that, Dave. I mean, we've had lots of internal debate about what a regime shift is too, but that's partly because we have oceanographers and other scientists for whom that means something very different, but, the last time we discussed this internally, we pulled out a paper, and I would like to get it to someone to put on the SSC's website for today, and I know we missed that deadline for that, and I don't want to not adhere to the briefing book deadlines, but there is a paper that we can pass out about when should we consider a regime shift.

By regime shift, all we're meaning is just -- I mean, I think recruitment overfishing is not necessarily incorrect, but it's just what we're allowing the model to assume for recruits, and that's all we mean by that, and so perhaps our language could be adjusted so that it makes sense to everyone, but I'm sure you could appreciate that this is a difficult thing for just the fisheries community, fisheries management community, to come to grips with, what we're even doing here with this assumption.

I am happy to pass that paper out, and it's sort of guiding our discussion about when we should consider even suggesting that the SSC consider a lower recruitment stanza, and maybe we could call it that instead of a regime shift. Does that make sense, Dave?

DR. CHAGARIS: Yes, and I would like to see the paper and kind of understand maybe where you all are coming from with that, because a regime shift can mean different things, and I guess what I am most concerned about is moving the goalpost, or lowering the bar here, which would then change the stock status and whether that is appropriate or not, I guess. Thank you for your response.

DR. SIEGFRIED: Sure.

CHAIRMAN NANCE: Luiz.

DR. BARBIERI: Thank you, Mr. Chairman. Katie, I think it would be great to see that paper, if you can post that there, or send it to council staff, and then, irrespective of what actions we have to take at this meeting, I think that having all of you come and

bring up these issues and have this discussion -- I mean, you can see that we are going back and forth and asking a bunch of questions and engaging in this discussion, because I think it's being taken that way, as a conversation between us and you, and I think we are learning a lot through that process.

I would encourage, with Mr. Chairman's approval, pending whatever decision we make here today regarding yield streams for greater amberjack, those recommendations, that perhaps we revisit this issue at the next SSC meeting, Katie, where we can have a discussion, perhaps, of that paper, or the implications of that and how that would have on the projections and the interpretations of a regime shift, because I think it's been a very healthy and interesting process for us.

DR. SIEGFRIED: Thanks, Luiz. I mean, that's exactly what we were hoping for here, is to get a discussion going with the SSC, between the SSC and the Science Center, and even SERO, and everybody involved that contributes to this management process, and put together a document of what we hear as well, and that's -- So you will note that we just provided a presentation for this, but there's not a document, because it's not just the Science Center dictating this, and so it was meant to be a discussion and then a document put together about sort of where we landed.

Then, I mean, I do have more in this presentation, and I do want to come to just the decisions for what we can provide specific to amberjack as well, but we will put together that larger decision document, and we can certainly discuss it at the next SSC meeting, along with all the literature that we provide.

CHAIRMAN NANCE: Okay. Thank you, and this really has been an excellent discussion. Roy.

DR. CRABTREE: We'll have to come back to this again, because it seems to me that what we need now is we need to know what F rebuild is, to get us to 30 percent SPR by 2027, and then we need to see what the associated yields would be there, and then that would be what we derive an ABC from, right?

DR. SIEGFRIED: Yes, and we only give you one set of decisions. This is sort of where we landed, based on -- I mean --

CHAIRMAN NANCE: Katie?

MS. MATOS: Katie, we can't hear you.

DR. VAUGHAN: Katie dropped off the line.

MR. RINDONE: Shannon, would it be possible to get kind of like a decision breakdown? I am kind of thinking like -- You can tell me what you think might be most appropriate, but you know how you guys will go through the model progression of where you started with the last model and all the changes that you made, until you got to the current model, and whether you wanted to do something like that or something like the table that was used in Michael's kingfish analysis, and I think that would work also, with like the incremental changes from one model to the next, just to kind of be able to visually demonstrate what the changes were from where we started with the projections in January to where we are now.

I think that will be useful for how we disseminate this information moving forward, and it will also help us, I think, to be able to better tie what scenario is going to correlate with what projections yield.

DR. CALAY: So Katie is back. Go ahead, Katie.

DR. SIEGFRIED: Sorry about that. I have two computers going, and one of them died.

CHAIRMAN NANCE: Well, at least you didn't.

DR. SIEGFRIED: At least I didn't, yes, and so I heard, Ryan, most of what you said, and I think we have most of that here. I showed the progression through the old code, and then I'm going to show the progression through the new code in the upcoming slides, and then we also have the table of decisions that is provided in each stock assessment report that we can go through in this presentation.

CHAIRMAN NANCE: Katie, I think let's go ahead and do that, and then we can make some decisions.

DR. SIEGFRIED: Okay. The other question that we had, and I think Dave brought it up, is what if MSY were used, and then I think Steve brought it up today, and this is just showing one comparison run. This is the base, using SPR 30, but with the data-rich recruits, because there's been a lot of discussion about whether those early recruits were reasonable, and so we did kind of make the executive decision here just to show what data-rich recruits would look like, as opposed to this is the new code, and these are both -- Sorry. This is a little confusing, and I should have corrected that.

This is with the new base, using the SSB SPR 30 and the new code

base, with the stock recruit curve, and so this is showing you that they really only deviate after we get some selectivity in the fishery, and so it's a few years away from that first year of management, and then we start to see the curves diverge, because the stock-recruit relationship, as the stock builds, is going to estimate a larger recruitment, as opposed to a fixed level of recruitment in the blue line, and so this is what would happen if we used the stock-recruit curve, as opposed to just an average, even if it's over the data-rich period.

I think this is the table, and I have a mislabel here. It's F over FMSY in the fourth column, but so this is a similar format to the table that you've seen previously. The R, you will note, the recruits, is pulled from the stock-recruit curve, and it's no longer fixed. The F is the FMSY, and so this is an OFL projection, and so the FMSY is slightly different from F SPR 30, and then the SSB and then the benchmarks around SSB are listed here as well, and so, if it needs to be over SSB MSY to be rebuilt, then that doesn't occur, under this OFL scenario, in this timeline.

However, if you -- I am not showing this, but I will put it in the report, but, if we use the stock-recruit curve with an F rebuild, it is going to rebuild in 2027 with an F of 0.122, and so that gives you an idea that it's about half the F, but it's not half the yield in each year, but I will provide that in the report.

Then you will note, in the second-to-last column, the SPR is greater than 30 percent, and it's 32.5 percent, at the end of the time period. Then you can see the corresponding OFL yield stream, and so this was asked for to compare -- We could put it in light of all the other discussion that we've had, as to whether MSY is more appropriate than F SPR 30, but it certainly is possible for the SSC to determine that.

Then the comparisons that Ryan was talking about, and this is sort of grouping all of those comparisons, as well as the allocations, in one slide, and so, at this point on, we're using -- Right after Nathan's presentation, from that point on, we're using all of that new code, all of Nathan's code, and so I have these runs labeled as Greater Amberjack Allocation Scenario 1, 2, 3, and then the fixed commercial landings.

We were asked to compare the allocations on the side with commercial versus recreational and then a commercial fixed allocation, and I think it's fixed at recent take, and then just to show what the proportion of recreational would be, and it's actually pretty difficult to see the difference between Allocation 1, which is 1984 to 2016, and the yellow line, which is the fixed

commercial take, and this is under the base run scenario that I showed right after Nathan's presentation, and so these decisions are using SPR 30 and the recent recruitment.

Then these are the rebuild projections under the different allocation scenarios, and you will notice the jump up after 2027, and so, with rebuilding, and we will rebuild by 2027, and then we're allowed to fish at the FMSY proxy in the years 2028 and on. Again, there's not a big difference between these scenarios and we tend to have the grouping of the green and the yellow, which is the higher proportion to commercial and the fixed commercial, and 2 and 3 tend to be more closely linked, and that's the slightly lower allocation to commercial.

As I've said before, we would like to capture the discussion that we've had so far, so that we can provide a document that incorporates what we've presented and what you've said and sort of the decision points that can be made, and we also want to cite all of these papers that we're talking about, anything available in the literature about steepness, say, and the regime shifts. Then, based on these discussions and decisions, we will provide the updated projections to the SSC.

I will be taking notes, or we can actively, on a Word doc or whatever you all want to use on the screen, list the decisions for the amberjack projections. They can be informed by the table that I provided at the beginning of the case study, and then we would carry those out not only for the base run, which is the 27/73, 73 commercial and 27 recreational, that's in the base run and the allocation scenarios requested by the council.

We want to continue, or begin and continue, this discussion about best practices for how to treat recruitment for this assessment. Then gag is going to be tomorrow, and then all of the stocks into the future, and then, finally, we do want to discuss whether the SPR proxy is appropriate here, and it should be a discussion each time that proxy is put into place, and even if a new assessment finds that they can estimate more of the stock-recruit parameters, the SPR proxy should probably be revisited, as to whether to use those instead of depletion, say, or the MSY as a target.

I think that is it, and so is it possible to put up a sheet to take notes about the decisions? Does it have to be done through motions, or can we just brainstorm here and then talk about motions, or how does it go from here for you all?

CHAIRMAN NANCE: What I would like to do is maybe bring that table up, so that we have that to look at while we're making our -- I

think it's the discussion from the -- It's the very, very first one. I think, Katie, it's the table in the very beginning.

DR. SIEGFRIED: Slide 3.

CHAIRMAN NANCE: Slide 3, I think. I think it's that table, but it has the whole thing on it. I think that's it, isn't it, Katie?

DR. SIEGFRIED: No, and this is all of the quantities of interest. It would be Slide 3, or it would be Slide -- For the case study, it would be Slides 13 or 15.

CHAIRMAN NANCE: There it is right there. Okay. During our discussions, this is a critical table to look at when we're making our decisions and our discussions.

DR. SIEGFRIED: Mr. Chair, can I add one more thing?

CHAIRMAN NANCE: Absolutely. You bet.

DR. SIEGFRIED: John Froeschke did note this before, that it's likely that we would need to have 2023 be the first year of management, and so we might want to add that to our interim landings discussion, and I think it sounds like we're going to need an extra year of interim landings.

CHAIRMAN NANCE: Okay. Thank you. Right now, we have up through 2021, I guess. I am trying to see on there. This looks like an older table. I see it. It's 2019 through 2020, and so we would want that to be 2020/2021 and recruitment through 2019, and is that correct? Is that correct, Katie?

DR. SIEGFRIED: We would still have 2018 as the terminal year of the assessment, and so we would need interim landings from 2019 through 2022, and our recruitment would stay -- The last year it's estimated is 2018.

CHAIRMAN NANCE: Okay. Thank you. Okay. Let's open it up. Harry.

MR. BLANCHET: Just I would say my two-cents' worth, but maybe it's only one-cent worth, but my concern with using recruitment from the stock-recruit curve is we all are very aware of what those recruitment variations are annually, and I would hate to be so optimistic as to suggest that, over just a few years, we would actually see some measurable benefit in recruitment in the future, so that we would be comfortable enough to increase allowable harvest based on that relationship.

To me, that is a whole other level beyond using something like recent average recruitment, however that is defined, and I am just uncomfortable with trying to add that into our projections. As I said, it's just one-cent worth.

CHAIRMAN NANCE: Thank you for that. I think it would be beneficial if we went down each of these parameters, and so let's start with relative F. Right now, we have the average value from 2016 through 2018. Any issues with that value? Doug.

MR. GREGORY: I think, beginning back in 2008 or 2009, we started using the geometric average the last three years, and I know we haven't done it consistently, but I think that the geometric average is what we've been doing, instead of just the plain average.

CHAIRMAN NANCE: Katie, is that average or geometric average?

DR. SIEGFRIED: It should be the geo mean, but I was just checking with Nancie.

CHAIRMAN NANCE: Okay, and Ryan just said that too, and so, instead of having the average, probably the geometric mean, 2016 through 2018, and is that correct?

DR. SIEGFRIED: Yes.

CHAIRMAN NANCE: Okay. Any issues with using that value for the relative F? I don't hear any objections. Luiz.

DR. BARBIERI: Not an objection, Mr. Chairman, but, by relative F, what you mean here, Katie, is F current, an estimate of F current?

DR. SIEGFRIED: Yes, as compared to the benchmarks, yes.

CHAIRMAN NANCE: Right. John.

DR. FROESCHKE: Is there some way in this spreadsheet or something that we could note which things are the same as the current one and which ones are different, and so, for example, this value is the same as what was used before.

CHAIRMAN NANCE: In other words, is this the value that was used in SEDAR 70?

DR. FROESCHKE: Yes, and so we're not changing the methodology. We're carrying that through, but all the other ones probably will

change.

CHAIRMAN NANCE: Maybe, in that comment over there, just say "same as SEDAR 70"? How is that? Okay. Thank you. I am glad you suggested that, and so that's F current, is what we're seeing, right? Okay. Go ahead, Roy.

DR. CRABTREE: Sort of. It's our most recent F estimate, and it's not really current. It's several years old.

CHAIRMAN NANCE: It's still the best estimate of that. Selectivity, I think this is probably also the geometric mean, and is that correct?

MR. RINDONE: Yes, and we do it this way just because it represents the most contemporary view of a way that the fisheries are currently operating, considerate of whatever management changes have recently taken place.

CHAIRMAN NANCE: Okay. I just want to make sure we're using the correct terminology.

MR. RINDONE: The same for retention.

CHAIRMAN NANCE: Okay. So this is the same as SEDAR 70, I'm assuming. Okay. Retention, it's the geometric mean of 2016 through 2018. Any change or issues with that value? Okay. Recruitment. Katie, this is where what we said in the -- It's a little different, and is that correct?

DR. SIEGFRIED: Yes, and I do need to -- I need to respond to the relative F comment too, because we do -- We use it for two things. We use it for what the F amongst the fleets is doing through the projections, but we also use the F current as a geometric mean, and I didn't have that in this explicitly, but they are both -- I am just double-checking with Nancie, but the F current is only a geometric mean, but the relative F is an average, and so we need to leave that relative F as an average and add in F current as a geometric mean.

CHAIRMAN NANCE: Okay, and so relative F is just a simple average?

DR. SIEGFRIED: Yes.

CHAIRMAN NANCE: Okay. Let's go ahead and put that in there.

MR. RINDONE: Relative F, selectivity, and retention are all simple averages.

CHAIRMAN NANCE: Okay. So those are simple averages.

MR. RINDONE: F current is the only thing that's using the geometric mean.

DR. SIEGFRIED: I have a wonderful team of people checking code and looking back at things right now, and so I just don't want us to get too far down without being accurate.

CHAIRMAN NANCE: That's good, and I'm glad you have that team. Somewhere, I guess, just for our own edification, let's put F current as the geometric mean.

MR. RINDONE: We can just add a row.

CHAIRMAN NANCE: We can put it there. Paul.

DR. MICKLE: Thank you. Real quick, Ryan, would you mind just jogging our memory on the size change and what year it occurred, when it was implemented, on the recreational side, as well as the seasonal calendar year that changed? It helps, obviously, with these discussions here.

MR. RINDONE: Recently. Hold, please.

CHAIRMAN NANCE: So, Jessica, on F current, you can put "geometric mean 2016 through 2018".

DR. SIEGFRIED: Thank you for typing this, Jessica.

CHAIRMAN NANCE: That was also, I think, SEDAR -- Yes. Perfect. Thank you.

MR. RINDONE: Mr. Chair, effective January 4, 2016, the minimum size limit for the recreational fleet increased from thirty inches to thirty-four inches fork length, and so it's been in effect since 2016.

CHAIRMAN NANCE: Okay. So 2016, and I think that's probably -- Thinking back on discussions of why we picked that year, it was so that we didn't have that conflict of size. Okay. Now, recruitment. Luiz.

DR. BARBIERI: Just a suggestion. Just, first, to have that in front of us, if Jessica could project Slide 20 from Katie's original presentation, and then we'll have those values right there in front of us. The recent average, that's what we had recommended

back in January, I believe, is what Katie said.

CHAIRMAN NANCE: Okay, and so I think there's two different things. We have the recent average for recruitment, which we were saying was 2009 to 2018. Katie, where was the change? is this the data that should have been used but wasn't, or --

DR. SIEGFRIED: Yes. This recent average, the 1,650.66, is what should have been used, but the long-term average, 2,805.57 is what was used.

CHAIRMAN NANCE: Okay. Perfect.

DR. CRABTREE: Jim, when we come back and look at the yield streams and all, the ABC, we're basing it on recent average, right?

CHAIRMAN NANCE: Yes. Martha.

MS. MARTHA GUYAS: Thanks. I was trying to chime in to help answer Paul's question too, and so there's been a bunch of changes. I think the last one, commercial trip limit, was in 2020, and that was reduced, and then we changed the fishing year and the recreational seasons, the open months, in 2018, and so I just wanted to throw that out there, because that was going back to Paul's question, and we were on the same wavelength there. I was going to bring that up.

CHAIRMAN NANCE: Okay. Thank you for adding that, and so there are some changes that have occurred during these timeframes, aren't there. Okay. Martha, thank you for bringing that up. Let's go ahead and go back to that Excel sheet. Perfect. Is there discussion on this? Do we want to use the average of 2009 to 2018, or do we want to use a longer-term? Luiz.

DR. BARBIERI: Just for discussion here, Mr. Chairman, Katie, if we follow the rationale that we were using before, and we say, okay, if we are setting up a long-term rebuilding trajectory, and we are trying to rebuild the stock to that original level of biomass, to a healthy state, and we say, okay, we insert in there the recruitment that would be over the entire time series data, so we can actually set a trajectory that will rebuild to that larger stock size, right, and so I can see that as logical.

At the same time, I just want to bring up this here for discussion, and suppose that, for whatever reason, and I think this is one of the points that Dave Chagaris had brought up before -- Right now, because the stock is in an overfished state, it's not going to be able to produce that level of recruitment, right, and so it might

actually set yields that are higher than can be actually realized, can be achieved, and so, to me, that's one of the issues to grapple with here, is setting the target that we want to reach in the long-term for rebuilding the stock and having that at a higher stock level. If we couple that with the short-term advice, it may not align itself with what can be realized.

CHAIRMAN NANCE: Roy, to that point?

DR. CRABTREE: Yes, and so, but if you use any of the other recruitment estimates, then the yields are going to jump up, because they're going to be based on recruitments already -- We don't have anything to indicate that that's the case, but, even if the recruitments stay at this recent average level, we still see some improvements over time in the biomass.

We get to the 30 percent SPR, and, if we reassess, in a few years, and we have seen increases in recruitment at that point, I think that is telling us that, hey, it is possible to rebuild this to a higher biomass, because the average recruitments have gone up, but we just don't know if that's going to prove to the case or not.

CHAIRMAN NANCE: To that point, would it be better to use a shorter, like the last three years? I am saying that I don't know what -- That is kind of where we're at now, but I'm not sure what that would do with the projections. Ryan.

MR. RINDONE: Thank you, Mr. Chair. I think one thing to caution you guys about using just the last three years is that you have an increase in the CVs around those point estimates for those last three years, specifically the most recent year, because there is nothing following it to provide any contrast. While there may be some discernable trend, your confidence in that should probably be tempered in that way.

CHAIRMAN NANCE: Good point. Steven.

DR. SAUL: Thank you, Mr. Chair. I echo Luiz's concern, given the sort of history of assessment and management with this stock, that it seems continuously being set to rebuild, but it does not seem to be responding, and, as per the discussion today, obviously, there is something going on with recruitment for this fishery.

While, typically, from a statistical modeling perspective, it's attractive to project forward the fitted spawner-recruit function parameters for projecting, in this case, I -- Given the history of this stock, and I don't remember how the other -- I don't recall, right now, how the other assessments projected things, but it may

-- It's probably worth taking some kind of a recent year averaging approach, like we're discussing, and so I think that's what I would -- Where I would lean.

Then, with that, whether to take the most recent years or the longer sort of timeframe, I echo the sort of concern that Ryan mentioned about the CV jumping up in that last year, and I am concerned that the last year could be poorly specified, or estimated, and so it may be worth not including that, or, if we do, being a little bit precautionary, in terms of what -- How we address that CV. Thanks.

CHAIRMAN NANCE: Jessica, let's put the other graph back up. While we're having the discussion, I think it's good to look at it while we're talking. Thank you, Steven. David.

DR. CHAGARIS: Thanks, and I was just wondering if it was possible to separate out, in the spreadsheet that we were working on there, the recruitment into the decision that we would make to determine the long-term targets and a decision on recruitment for catch projections.

I am still not sure if you could do one and then the other, and it sounds like, Katie, you said they do that in the South Atlantic, but maybe we don't have the ability to do that here yet, but, anyhow, just having that separate in the spreadsheet, so it's clear what is being used for each purpose, if that would help, and I know it might help me, as this comes back up a few months from now.

CHAIRMAN NANCE: I think that's a very good point. John, to that point?

DR. FROESCHKE: Would it be a simple as adding a row for the recruitment at OFL and then recruitment at ABC, for example, if you wanted to use the full series for the OFL and the regime shift for the ABC? Then other request, since I've got the mic, is could we add steepness to the parameter, just so we document what it is, because I am still a little uncertain about how that was done.

CHAIRMAN NANCE: Okay. Yes, we can put that on, and then Jessica can play around with that for a second.

DR. SIEGFRIED: Mr. Chair?

CHAIRMAN NANCE: Katie, go ahead, and I know that was a question for you. Thank you.

DR. SIEGFRIED: I am glad to hear that John said that we can split those up into OFL versus ABC, and we certainly can, Dave, do what you're asking, where we use a different set of recruitment to estimate our benchmarks and then a relatively short-term lower recruitment to look at, in case you want to consider that for ABC.

If you're thinking of OFL, and then decrementing it for ABC, the OFL probably -- I mean, I was just looking, and the virgin recruitment is 3,698, I believe, compared to what you see in that figure. The long-term recruitment says 2,805, something like that, and so, I mean, we're already quite a bit below virgin recruitment, and we're not aiming to get back to virgin recruitment, and so I just wanted to throw that in there, and we can go with getting back to it.

This one is actually cobia, Jessica, and so if you want to go to the other. If we want to make two considerations, one for OFL and one for ABC, and have us look at lower short-term recruitment for that ABC, I think there's justification for that.

CHAIRMAN NANCE: Thank you, and so here's what we're dealing with. Here is the recruitment through time. Here's what we're dealing with, and I think we've alluded to this, through our discussions today, that it may be good to have a recruitment for the OFL estimate and then one for the ABC, and so let's have a discussion on the recruitment time series for the OFL. Any discussions on that? Doug.

MR. GREGORY: Thank you. Is this graph that we're looking at the amberjack graph?

DR. SIEGFRIED: Yes.

MR. GREGORY: Okay. If you look at the long-term, it goes back to 1970, and we've got that great uncertainty, and then we've got a lot of volatility in the 1980s, and the data-rich period is from 1990 to the present. The short-term average and the long-term average, in this graph, are pretty close together. They're maybe a half-million pounds apart, or maybe a million, or maybe three-quarters.

If you took the average from the data-rich period, 1990 to the present, it's going to be somewhat in between, and I think, to me, that makes the most sense, and to separate this and the OFL and the ABC, in some instances, may be worthwhile.

In this instance, I don't think it makes a whole lot of difference, and it would be a lot of work, but this is a good conversation,

and I think we should have this conversation with every assessment, as to what recruitment trend or input we're going to use, but I would vote for 1990 to the present for the projections, given what we see here. Thank you.

CHAIRMAN NANCE: 1990 because we start the data-rich series then?

MR. GREGORY: Yes, and we get through that period with the recreational data that's so variable, and I assume that's why we see this variability in the 1980s with the recruitment, and you've got to remember that the first trip ticket system for the commercial fishery in the Gulf of Mexico didn't start before 1985, and so you would give that a couple of years to get the bugs worked out, and we're up to 1990.

If you look at the trouble that we had in the beginning with the MRFSS survey of dockside sampling, that was in the 1980s, and, when you look at the FES conversions that we've looked at for a number of species, that all gets extremely variable in the 1980s, and I think we had an assessment recently where the CIE people recommended that you start the assessment in 1986, because of all this variability and uncertainty and assumptions when you go back in history. I am just confident with data from 1990 forward, and that is thirty years, and that's a better track record of data than we had when I started my career with the council. Thank you.

CHAIRMAN NANCE: Any other comments on that? Roy.

DR. CRABTREE: Well, I mean, it doesn't look, to me, like it's going to be that much different, and I think the whole idea that you can calculate some poundage level and say that's an overfishing level is nonsense.

The only way to measure overfishing is to get an F estimate, a fishing mortality estimate, because you could have the catches go way up because you did have a great recruitment, or you could have them come down because you had terrible recruitment, and so I think you could do it that way, but, just eyeballing it, from 1990 forward, it looks like it's going to be somewhat less than the data-rich average, but maybe a little bit more than the recent average, but I don't think it's a fundamental difference.

CHAIRMAN NANCE: Okay. I would like to hear a motion, if we have any, on the data to use for the recruitment for the OFL estimation. Bob Gill is not here to make it, and so you guys are going to have to --

DR. BARBIERI: I am still unclear, and I think Doug is the one who

made a recommendation, but I didn't capture the years there, Doug, and was that 1990 to 2018?

MR. GREGORY: Yes.

DR. BARBIERI: Okay. Doug is working on that motion now, right, Doug?

MR. GREGORY: I move that we use the period from 1990 to 2018 for the recruitment, use the average of those years.

CHAIRMAN NANCE: For recruitment for OFL?

MR. GREGORY: I would use it for recruitment for both, OFL and ABC. My biggest concern about projections is not where it's going to go in the future because of a recruitment level that we chose, but it's the uncertainty based in the first two or three years, which is what our recommendations usually are, three to five years, and the uncertainty, due to not knowing what the landings are between the terminal year of the assessment and the time management starts, and so now we're talking, what, 2018 or 2019 to 2022 or 2023.

To me, that's the biggest problem with estimating ABCs for what we use them for, because we don't use ABCs for ten, twenty, or a hundred years out in our recommendations to the council. We use the ABCs in the first three years or the first five years, and that is something I would like to try to put this on in the future, is trying to resolve that confusion that we have in not knowing what the landings are before the projections start. **The motion on the board is to make 1990 to 2018 the period to estimate recruitment for OFL and ABC for greater amberjack.** I will accept that, and I accept any wordsmithing that anybody wants to do.

CHAIRMAN NANCE: Do we have a second for that motion?

DR. BARBIERI: I will second it, Mr. Chairman.

CHAIRMAN NANCE: Okay. Luiz seconds that. While we have discussion, Katie or Nancie, is it possible just to get the average recruitment for 1990 through 2018, so we can kind of see what we're talking about, how different that is than the long-term average, which is 1970 to --

DR. SIEGFRIED: It's 1,848, as opposed to 1,651.

CHAIRMAN NANCE: Okay.

DR. SIEGFRIED: That's in millions, of course.

CHAIRMAN NANCE: Yes. Okay. Thank you. Jim.

DR. TOLAN: Thank you, Mr. Chairman. Given that we're filling out this spreadsheet with recommendations moving forward, does it really need a full motion? I mean, I fully expect to support this, but I'm just curious, and does this really need a full motion for us to vote on? Thank you.

MR. GREGORY: I think I got trapped into that. I will go either way.

CHAIRMAN NANCE: Maybe I am being too strict. I just like to see where we're at, as far as are we all in agreement on this or not, because, as we move forward, it's important that we come to an agreement on using the 1990 through 2018 or those types of things, so that we know where we're at. Katie.

DR. SIEGFRIED: I am thinking of this in the context of a decision tree, and, although I think that Doug is not wrong with 1990, I would like it to be something that is not just by eye, for future stocks, and so I just wanted to provide a little bit of information that we had our first set of length comps available in 1984, and that's why the data-rich period is specified for the bias ramp and that first stab at projections for you is set at 1984.

However, our age comps come into -- I think it's either 1990 or early in the 1990s, and that's why I don't disagree with Doug, in principle, but I do think we should try to work together to come up with more objective ways of deciding on this, instead of make it by eye, and so --

CHAIRMAN NANCE: Absolutely. Harry.

MR. BLANCHET: My primary concern is that we are using a single species and kind of developing a process that we're going to be applying, at some level, across many different species, and my concern is that, while with amberjack, it does not seem to matter a whole lot, we have seen other species where we've had fairly notable changes in recruitment within the recent thirty-year time period, and I don't really want to be getting into taking a bigger chunk of time than is a good characteristic of what is actually going on right now, and I don't see a whole lot of difference between 2016 and 2018, currently. They both seem like reasonable approaches, but there are going to be other species where taking that longer time period might be either overly conservative or overly liberal.

CHAIRMAN NANCE: Luiz, to that point?

DR. BARBIERI: Harry, I agree completely, and I think that perhaps, for the sake of discussion today, we kind of develop this tacit agreement that, at least for a while, we're going to have to look at this on a species-by-species basis, until we kind of get a better feeling of what could be sort of developed as a broader, more stable framework.

CHAIRMAN NANCE: Roy.

DR. CRABTREE: In January, I was recently retired and didn't want to think about having to do with fish, and so I'm not sure what all went on, but was there a reason behind the 2009 to 2018, other than it's ten years and that's a nice round number?

MR. RINDONE: It's ten years, and it's a nice round number.

DR. SIEGFRIED: And it's the period that is estimated, and so it's the last ten years of estimated recruitment.

CHAIRMAN NANCE: Thank you. Dave.

DR. CHAGARIS: I think part of the reason I wanted to separate the OFL to ABC is because I see them as they should be two different decisions, and I sort of agree with this approach of 1990 to 2018, and I don't like the eyeballing that we had to do there, but I am still wondering if that is, one, consistent with Magnuson-Stevens, as Harry brought up earlier, and whether or not we're supposed to be rebuilding based off of virgin reference points, or managing based off of those virgin reference points.

That's on the one hand, with the OFL, and then with the -- What I am suggesting with the OFL is that I might be more comfortable with using the full long-term average for the OFL, but then the ABC -- If we went 1990 to 2018, then we would potentially miss a situation like we have now, where the recruitment is lower in the last ten years, and so that was sort of the reason why I was hoping we could break these apart, because, to me, it's two different things, potentially.

CHAIRMAN NANCE: I think it's wise to break them apart, in my opinion. Sean.

DR. POWERS: I just want to echo what Harry and Luiz were talking about as well, that we don't -- That this will have to be, I think, for a while, a species-by-species issue, and, as far as breaking

the ABC and the OFL, I'm fine with that, and I can see that we might want to be more cautious with one than the other. That's it.

CHAIRMAN NANCE: Thank you. Mike.

DR. ALLEN: Thank you, Mr. Chairman. When I first looked at this, I looked at what Katie is calling the data-rich period, from 1984, and that time period, from 1984 to 2018, has much lower uncertainty than the rest, and earlier than that, and then using on the most recent years, and so it made sense to me, at the start of this discussion, to use the data-rich time period, which includes some of those high recruitments back in the 1980s, and so just a suggestion.

CHAIRMAN NANCE: Thank you. Doug.

MR. GREGORY: I intended this to be just for greater amberjack, because I do think this is something we should examine for each species. I would also like to point out that, if we break them up for OFL and ABC, going forward, we're in complete violation of our ABC Control Rule, which we haven't changed, and so we would have to make a recommendation to change that to something like this, or, on a case-by-case basis, make a point of arguing why we're violating the control rule, or deviating from the control rule, and so those are two points that I wanted to make.

There was a third point that 1990 to 2018 was not just eyeballing. I do have a memory of lots of data changes and lots of data surveys, and 1990 is just a year when things -- Data started coming together in the Gulf of Mexico, and so it wasn't an eyeballing, and it was based on my history of how the data collection systems in the Gulf of Mexico evolved. Thank you.

CHAIRMAN NANCE: Thank you. Steve.

DR. SAUL: Thank you, Mr. Chair. Katie, I just wanted to say that it seems like you're looking for a method that would be standard, or somewhat standard, for all kind of assessments, and I think that's a really useful approach, but it should also just be used with caution and caveats, given the wide range of species and the wide range of life histories in these species that are assessed in this region.

The other point that I wanted to make, or at least direct the SSC towards, is perhaps an approach -- Something like the North Pacific Council is using, like a risk-table-type of approach, could be useful for trying to sort of evaluate the pros and cons of some of

these assumptions and some of these approaches toward computing benchmarks.

CHAIRMAN NANCE: Thank you.

DR. SIEGFRIED: To that point, thanks, Steve and Doug. I take your points, and I agree with a lot of what you said, and I don't think that everybody has the same experience as say Doug, and I think what's important to do is think about when the data streams got good, so to speak, and so I think what I would like to do is be able to write down that, okay, if we have length comps that we think are representative enough to include in the assessment, that should be the beginning of our data-rich period, or, alternatively, when age comp data, which is what happened in 1990 for amberjack, that we started to get logbook information, from 1990 on, with age comps, with corresponding age comps, and so that would be why 1990 would be appropriate for amberjack, an objective reason.

We could say, okay, when the age composition data are turned in at a rate that we think is sufficient to put into the assessment, then that's when our data-rich period should begin, and that's the sort of thing that I think would be useful, as a rule-of-thumb, and there are certainly reasons to deviate from rules-of-thumb, but they just have to be well explained with lots of evidence. That's all.

CHAIRMAN NANCE: Thank you. Ryan.

MR. RINDONE: Thank you, Mr. Chair. I guess I will start first to Katie's point, in looking at the -- Katie, I'm looking at Figure 1 of the SEDAR 70 stock assessment report, and it's on PDF page 123, and it has -- It's the figure with all the data inputs.

If you're looking at that line from 1990, it looks like the age compositions are available more often than they aren't beginning actually in about 2000 for the directed fleets, but the length comps are available back to the mid-1980s, but I also concede that the experience of those that have been around for a while, and knowing when those data, like the length composition data, definitely got better and were also stratified by species and weren't just grouped as, oh, it was some kind of jack or some kind of amberjack, and eventually it was species-specific.

Certainly, through time, those data got better. However, to the previous point about Magnuson, I went into the Act and looked around, and, generally speaking, rebuilding is referred to -- When we're talking about rebuilding a stock that is overfished, we're talking about rebuilding it to a level consistent with producing

the maximum sustainable yield in such fishery.

I guess my question would be, conceptually anyway, is that, if our perception of what the MSY can be for a stock has changed, due to some fundamental say regime shift or state of nature that we now agree is more plausible than what we thought previous, then our perception of what the maximum sustainable yield could be is going to change, and, thus, our rebuilding target, presumptively, would also change.

I don't think that, by doing this, that this exercise, or looking at things in the way that we are, would constitute -- I'm not an attorney, and so if Mara is around, but it doesn't seem that that violates the Act, because it's not saying that you can only define MSY as one thing one time for a stock and that's what you have to adhere to forever, because ecology is dirty and messy and things change.

CHAIRMAN NANCE: Okay. Thanks for that consideration. Shannon.

DR. CALAY: Thank you. When you put the motion on the table, there was a discussion about divorcing the OFL and the ABC and potentially using different recruitment periods on each, and I do think we need to be a little bit cautious about creating advice that would pertain to all stock assessments without looking at the possible effects of that decision, because I think it is possible, if we're not careful, that we could set up a situation where we set up criteria that the ABC yield stream is actually higher than the OFL, which is, of course, not appropriate, and it would be most likely to happen when the recent recruitment is higher than the long-term average.

CHAIRMAN NANCE: Yes, and that would be where we wouldn't want to go, and I am not looking at this as every single assessment we do this, but there are some of them that I think we need to look at in greater detail, and I think that's what we're trying to do here. Steve.

DR. SAUL: Thank you, Mr. Chair. Just to Ryan's point earlier about the timing of data streams and some of the caveats, I would just caution folks to keep that in mind very carefully, in particularly the year 1990. I think most of the data become more stable around 1993, and so that may be something for consideration as well, in terms of when to take that recruitment averaging timeframe. Thank you.

CHAIRMAN NANCE: Thank you. Josh.

DR. KILBORN: Thank you, Mr. Chair. I guess I am a little uncomfortable with the suggested timing of the early 1990s, specifically because, from an ecosystem perspective, the Gulf of Mexico went through a pretty large environmental shift in the mid-1990s, and we can look at just the AMO as a good indicator of that, which went into a completely different phase around 1995 or 1996.

As far as amberjack in particular are concerned, the recruitment has been underestimated, in comparison to the Beverton-Holt estimate, and it's been regularly underestimated based on that curve, since the 2000s. We haven't had an above-average year since the late 1990s.

Getting back to the idea of a regime shift, I think we might be in a completely different regime, and so we might need to consider that, overall, because I just don't know that extending the timing of this averaging period into a previous regime is going to be indicative of what we might expect in the current time, and so, again, I'm a little uncomfortable with the timing that's being suggested, and I think the shorter ten-year average is, again, a little arbitrary, but maybe possible more appropriate. Thank you.

CHAIRMAN NANCE: Thank you very much for that comment. Let's go ahead and go back to the motion. Trevor.

MR. MONCRIEF: I am just going through and trying to listen to everybody's points and comments that everyone has had, and I think we're all flip-flopping in between, with great points had by all, and great evidence supporting it all, and I was wondering, given the differences between opinions and everything else, if it might be worthwhile to entertain maybe changing this timeline from 1990 to 2018 to 2000.

That matches when the age composition really starts up, and it meets what John is talking about with the regime shift in the Gulf of Mexico, and it's a little bit shorter of a time period, and it seems like a happy medium between all the discussions we've had over the last five or ten minutes, if we go 2000 to 2018.

CHAIRMAN NANCE: Thank you. Luiz.

DR. BARBIERI: Thank you, Mr. Chairman. Katie, if you're still listening, let me ask you a question. I am thinking about this, and is this trying to achieve the different things, and I would like to hear your advice, I guess, or recommendation, or at least your thoughts on this.

The long-term sort of rebuilding target that we're trying to set,

and then trying to tie some recruitment values to that target, we're trying to identify a target that is high enough to represent the true productivity of the stock, when it is rebuilt, to that level of biomass at MSY, or thereabouts, but then, separate from that, there is this issue of what's going to happen over the next several years and the fact that, even when we look at the figure, but then we see that -- I think Josh mentioned this, and others have as well, that the recruitment has been below average for quite a while.

Can you help us think through this, in terms of help me reconcile these two goals, and one is trying to identify a target with recruitment values that reflect our ability to rebuild to that healthy side with the short-term concerns that we have with recent recruitment not really being what seems to be very healthy. I think this motion ties to this whole thing, in terms of OFL and ABC, and we're trying to think about those two goals, and I would love to hear your thoughts on this, Katie.

DR. SIEGFRIED: Sure, Luiz, and I'm still here. Don't worry. I'm not going anywhere. I have a lot of thoughts on this, and I don't think that anybody has the right answer, and so all of what people have been talking about is interesting, and I've been trying to absorb it, too.

I think that the first thing I would think about is my target, and so, if you're going to go with SPR, some of this discussion about recent recruitment doesn't have as big of an impact, like I showed with the projections that I ran, and so this careful consideration of 1984 or 1990 or 1993 doesn't have as big of an impact. It does if you're trying to rebuild to a certain depletion level, and so I think I would look at my target first, and then I would consider that impact.

In the model, we have included these length comps, starting in 1984, and we've considered the data-rich period according to the model, what we think that our recruitments are better estimated to be in 1984. If we don't think those data are good enough to be considered data rich, I don't think we would have included them in the model in the first place.

I don't think that I would deviate from that assumption that's made at the assessment phase, unless some new information is brought to light that says that 1990, or 1993, is better, and maybe the consideration of those length comps needs to be, I don't know, made more explicit in the future.

I would also look at the relative level of recruits that I am

assuming, as opposed to virgin, which, right now, if you assume recent recruits, you're at 1,600, and virgin is like 3,600, and so you're quite a lot lower than we think the stock can achieve at maximum productivity. For me, it would be -- Also, if I was an SSC member considering the management, what we've tried to do in the past with management, and whether it's been successful, and whether we believe that the stock can actually produce the amount of recruits at each of these assumed levels.

We have seen, based on the recruits fit by the stock assessment, that it's been relatively lower recruits since about 1990. The SS model also has to have a net zero deviation, and so those early high recruitments are reduced, the deviations, later on, because we do have to have them all sum together, and so I would take with a grain of salt a couple of the highs and a couple of the lows.

I think it is a little bit of overkill to compare 1984 to 1990 to 1993. I think there's the bigger issue of the target and the bigger issue of whether you think the stock can actually produce what the stock-recruit curve or what the historical recruit estimates say it can. Is that helpful at all, or is that too philosophical?

DR. BARBIERI: This is very helpful. Thank you so much, Katie.

CHAIRMAN NANCE: Roy.

DR. CRABTREE: I think that is helpful, Katie, and we can go round-and-round on this time period, and it, in the end, doesn't make much difference. What's important here is that we get the fishing mortality rate and the fishery approximately where we think it needs to be.

The recruitments will be what they're going to be, and I don't know if it can go back up, and I don't see anything to make me think we can get it back to that virgin recruitment level, but they may come up some as the stock recovers, and there's just no way of knowing that, and so I think you can go round-and-round of whether you use 1990 to 2018 or 2000 to 2018 or stick with the 2009 to 2018, and I don't think that's going to make all that much difference in these numbers.

I do think the OFL and the ABC and the F rebuild ought to be done in a consistent fashion, so that we have one period of time that we're going to base these things on, but I really -- If you look at those recruitments, going back to 1990, there's not a noticeable trend in all of that, and so I don't think it's going to make -- Given the error in all of this anyway, I don't think it's going to

make much difference.

CHAIRMAN NANCE: Katie, thank you for that input. Let's go ahead and vote on this motion. **Any opposition?**

DR. BARBIERI: I don't have any opposition, Mr. Chairman, but I am just wondering -- Doug, based on what Katie just said, and to consider -- To keep all of this internally consistent with the assessment, would it give you too much heartburn to change that time period to start in 1984? That way, that would be the data-rich average, based on what the assessment team determined to be the data-rich average.

MR. GREGORY: Not really. I don't know if 1984 is part of that period where you had a lot of volatility, and that was my only concern. I will note that, when you get below 1984, or 1985, and most of the data, except for catches, are speculative, and this assessment goes back to the 1960s, when all recreational harvest was speculative, and I've had a concern about that ever since the assessment people started going back and using the Fish and Wildlife Service data that's been proven to be incorrect.

I know there was a best practices workshop, and the methodology was derived based on the population size of coastal counties in the Gulf of Mexico, to modify that estimate, but there is so much uncertainty and so much speculation in those older data that I certainly would dismiss them, but I have no problem going with 1984, and I have no problem going back to the ten years, but it just seemed like this was almost a compromise, but it had a basis for it, in my mind.

If we want to change that to 1984, and I think Steve said he wanted to change it to 1993, and an amendment would be fine. I would not object to an amendment.

CHAIRMAN NANCE: Okay.

DR. BARBIERI: Well, I don't know if I want to make that substitute motion, because we continue discussing -- For those if you not here in the room, we continue discussing some of the issues about data quality along this time series.

CHAIRMAN NANCE: My only problem with that, with 1993, is we've got what we're terming as data-rich is 1984 through 2018. John.

MR. MARESKA: Along the lines of the discussion, the commercial landings, I think it wasn't until 1995 that they actually really started to break them out between the unclassified and specifically

greater amberjack, and so, in my mind, that may be a good starting point, is 1995, because my understanding is this is derived from the landings information.

CHAIRMAN NANCE: Okay. Thank you, John. Harry.

MR. BLANCHET: This might be off the topic, but my concern is, if we're talking about -- Maybe because we started talking about regimes, but the point about the Atlantic Oscillation -- I don't recall what the exact date was of that, but certainly we have seen some changes in some other species, and blue crab comes to my mind, related to that oceanographic phenomenon, and so, if you are going to -- If that fits within a consistently data-rich time period, then that at least takes one additional parameter off of the table, in terms of trying to estimate what your recruitment might be and any change from that.

CHAIRMAN NANCE: Okay. Thank you. It seems like we're kind of going around in circles on trying to come up with dates, and I think, from listening, all of them have good points, and so what we're trying to do, as has been discussed, is we want to be able to have a time series that we feel comfortable with as our OFL estimate, and that's what we want to get back to, and so we need to come up with that in a way that's consistent with what we're trying to do, and it's consistent with Magnuson, and we, as an SSC, are comfortable with that value. Luiz.

DR. BARBIERI: If I may, Mr. Chairman, and thank you. To one of the points that Katie just made about what target we choose here, if it's SPR-based, then these would not be too relevant, or the relevance wouldn't be as high as it would be otherwise, and so my understanding is that, when we reviewed this assessment back in January, we did not accept the MSY estimate, the steepness estimate, and, therefore, the direct MSY estimate, and so we would go with an SPR target anyway, right, and so that brings me just -- I think that was a helpful comment from Katie, because that will bring me a little more peace of mind, to know that, sure, this is going to be relevant here, to some extent, but not as much as it could have been otherwise.

CHAIRMAN NANCE: Okay. **Is there any objection to this motion?**

SSC MEMBER: I suppose I object to the timing, yes.

MR. GREGORY: I think everybody objects to it, but nobody is offering an alternative for us, and apparently there is no agreement. It ranges from 1984 to 2010.

CHAIRMAN NANCE: Right. Let's go ahead and vote on this motion, since it's there, and so, Jessica, go down and get us the list, and let's vote on this motion, so that we can go on.

MS. MATOS: Steven Saul. Jack Isaacs. John Mareska.

MR. MARESKA: No.

MS. MATOS: Lee Anderson.

DR. ANDERSON: No.

MS. MATOS: Dave Chagaris.

DR. CHAGARIS: No.

MS. MATOS: Doug Gregory.

MR. GREGORY: Emphatic yes.

MS. MATOS: Trevor Moncrief.

MR. MONCRIEF: No.

MS. MATOS: Sean Powers.

DR. POWERS: Yes.

MS. MATOS: Jim Tolan.

DR. TOLAN: -- my original reservations of the motion.

MR. RINDONE: What did you do about the motion?

DR. TOLAN: I abstain for my original reservations, and I didn't think it was needed, and so I'm abstaining.

MR. RINDONE: Thank you.

MS. MATOS: Richard Woodward.

DR. WOODWARD: I will abstain.

MS. MATOS: Will Patterson. Paul Mickle.

DR. MICKLE: No.

MS. MATOS: Benny Gallaway. Harry Blanchet.

MR. BLANCHET: No.

MS. MATOS: Jason Adriance. Luke Fairbanks.

DR. FAIRBANKS: No.

MS. MATOS: Mandy Karnauskas.

DR. KARNAUSKAS: I will abstain.

MS. MATOS: Steven Scyphers.

DR. SCYPHERS: No.

MS. MATOS: Jim Nance.

CHAIRMAN NANCE: Yes.

MS. MATOS: David Griffith.

DR. GRIFFITH: Yes.

MS. MATOS: Roy Crabtree.

DR. CRABTREE: Yes.

MS. MATOS: Luiz Barbieri.

DR. BARBIERI: Yes.

MS. MATOS: Michael Allen.

DR. ALLEN: No.

MS. MATOS: Josh Kilborn.

DR. KILBORN: No.

MS. MATOS: Benny Gallaway.

DR. GALLAWAY: I abstain. I think Katie and Nathan have done an excellent job in defining the tool, but I am abstaining because I don't think the input data are ready to apply to the tool, and so I abstain.

MS. MATOS: Jason Adriance.

MR. ADRIANCE: No.

MS. MATOS: Jack Isaacs. Steven Saul.

DR. SAUL: No.

CHAIRMAN NANCE: The motion failed.

MR. RINDONE: It was six to twelve with four abstentions and one absent.

CHAIRMAN NANCE: Let's go back to that Excel sheet. We know what we're trying to accomplish. Right now, the recruitment that was used was 2009 to 2018, and I think that was for ABC, right, and so is there any issue with continuing with 2009 to 2018 for the ABC? Roy.

DR. CRABTREE: I don't have any issue with that. I don't understand why we would split the timeframe for OFL and ABC differently. We're going to need to use a timeframe for the ABC and the OFL and F rebuild and all of that, and I have no problem with using that recent timeframe, and that seems to be where the SSC came down back in January, and I don't see anything here that leads me to see clearly a better one.

CHAIRMAN NANCE: I think the only issue with our splitting was this 2009 through 2018 and using that, and is there any way to get back to where we were?

DR. CRABTREE: If you mean where we were back with the virgin recruitment and all that, I don't think so, no. You need to have much higher recruitments, and we don't know if that's going to come to pass or not.

CHAIRMAN NANCE: Well, that's kind of the dilemma we're in, is we're trying to come up with a set of years that we're comfortable with to define OFL and come up with a set of years to define ABC. They can be different. They can be different, or they could be the same. Luiz.

DR. BARBIERI: If I might, Mr. Chairman, in this case, because this is under a rebuilding plan, right, and so ABC is going to be set as yield at F rebuild, and so, in that case, it's an automatic value that is going to come out of the F rebuild that's going to be tied to that rebuilding target.

CHAIRMAN NANCE: So are you saying that's already set then?

DR. BARBIERI: Well, if I understand this correctly, the two are tied, because, if you set the target for OFL, which is MSY or its proxy, then you are rebuilding to that target, and the F rebuild that's going to be estimated is going to be more fishing mortality during the --

CHAIRMAN NANCE: Roy.

DR. CRABTREE: I mean, essentially, what you're saying is, given the amount of fish in the water right now, we think, if you fish at the maximum fishing mortality threshold, that's the OFL, and this would be the yield corresponding to that, and this is the yield if you fish at F rebuild, which we believe will get you to where you're going, which is 30 percent, and we would likely reduce for some level of uncertainty, and that would then be the ABC, and all of that would be based on these recent recruitments continuing.

CHAIRMAN NANCE: Katie.

DR. SIEGFRIED: Thanks, and so I wanted to add a little bit to my pontification earlier, and maybe some practical statements, and so the reason that I think separating recruitment into these two options -- Well, it only will work in a few ways, and so, again, we have to assign a set of recruitments or use a stock-recruit curve to get our benchmarks, and so that should be decided, and that could be the OFL.

That same level, those same benchmarks, should be used for the ABC runs, but we could use the recent recruitments just for the projection time period, which I think is what Dave was getting at earlier, in that those recruitments could be considered the recent time period, or any, really, iteration that you want, any version of the recruitment that you've already seen, and, as Roy said, ABC is just a decremented OFL, and so we can't actually carry the recruitment through the full equilibrium to get the benchmarks differently between the OFL and ABC runs, and so it really is just -- You have to pick the one set of recruits to determine the benchmarks and then either use those and just decrement for your ABC, or you can select a separate set for the recruits.

You also can, for the OFL run, assign the set of recruits for your benchmarks and then also assume different recruits just for the projection time period, and so, I mean, it gets a little confusing there, because I don't know how many iterations of average recruits you would actually want to do, and it sounds like mid to early 1980s and 1990s, and there is kind of not one answer there, and so, I mean, the way that we would probably propose, and Nathan or Shannon or somebody can jump in if there's more insight they can

offer to this, but, to do the OFL, you just need to pick what you think the stock could get to at some point, and the recruitment would be a more precautionary, okay, well, we've only seen recent average recruitment at this level, and so we want to add that buffer. Does that make sense? I mean, there's a few different steps that you could go through there.

CHAIRMAN NANCE: That does make sense.

DR. SIEGFRIED: Okay.

CHAIRMAN NANCE: So how do we move forward?

DR. SIEGFRIED: Can I add one more thing, Chair?

CHAIRMAN NANCE: Yes, you may.

DR. SIEGFRIED: For the rebuilding, what was used for the rebuilding plan the first place, I believe, the best I could find, is that it was the whole time period, the average of the whole time period, and that was what was used to set the rebuilding time period in the first place, and that could be a logical first step for the OFL.

CHAIRMAN NANCE: Katie, was that 1970 through --

DR. SIEGFRIED: I think it was 1950 through 2016, and it was the first year of the model to the last year of the model, and that's what was used for SEDAR 33, when the last rebuilding plan was extended to 2027, the best I can tell, and so that's kind of saying, okay, the most productive we think the stock could be is the long-term average, and that's OFL, and then ABC is decremented using some sort of recent average recruitment or based on the reasoning that we all have been talking about for a few hours now.

CHAIRMAN NANCE: Yes. Trevor.

MR. MONCRIEF: I just kind of want to make a comment, to try to move forward on this item, because we've been on it for quite some time, and I'm trying to digest everything that Katie said, and I appreciate all the information that she has provided, because it's certainly starting to clear my head, as we move through this discussion, but I think, from what I've heard from the group and everything else, the consensus is, is there any chance that we would ever get to that pre-1990 recruitment level, and there seems to be a fair amount of doubt, or consensus of doubt, that that would ever happen.

I think the logical next step would be, okay, well, if that's not the case, do we think we could ever make it to the 1990 to 2000 range, where you see the spike in 2000, and I understand that's a little bit arbitrary and everything else, but I think the decision for us to move forward with, on the OFL side is -- I think John had a good point on 1995, when they actually started separating out the seriola.

I think that encompasses that increase in the 2000 timeline, and it seems like a reasonable step to say the stock, at some point, could get back to that level, and then, after that, what Katie was talking about with a different run to the ABC, you could use the recent time series. Like I said, I'm just trying to interject a couple of ideas for us to be able to kind of move forward on this subject and lay some stuff on the table, so that we can wrap our heads around it.

CHAIRMAN NANCE: Thank you. Dave.

DR. CHAGARIS: I think Katie's last scenario is what I would be most in favor for, where you would set the benchmarks based off of the long-term average, but then you can adjust your ABC, if we're in what we perceive to be some low-productivity era, and the reason I'm hesitant to go with the 1990 starting year for the long-term average is because a lot of the depletion in this stocks, across multiple species in the Gulf of Mexico, occurred prior to that, and so what we would be doing would be -- Basically, the stock gets depleted through the 1980s, before modern stock assessments come in and start improving things.

The stock gets depleted, and now we have a depleted stock, and we lower the minimum stock size threshold, and it's almost like we're conceding defeat there on the already depleted stock, and so I think that, to Trevor's question of can we ever get back there, if you base the benchmarks on the recent average recruitment, then the answer is no, because the rebuilding will be rebuilding to that lower minimum stock size threshold, and so, if we use the long-term average recruitment with benchmarks, and then the rebuilding plan is rebuilding towards those targets, even given -- Now, if you're in a period of low recruitment, then you would need a more aggressive rebuilding strategy, but it would eventually get to that target, in theory.

CHAIRMAN NANCE: Okay. Roy.

DR. CRABTREE: Well, I guess some of this is what you mean by benchmarks, and I don't think of OFL in the way that Katie is talking about it right now. To me, the OFL is the yield that you

would get with the biomass in the water now if you fished at the maximum fishing mortality threshold, and so, when you talk about the long-term productivity of the stock and what it could get back to, well, that's more MSY, and that's a benchmark. The proxy we're using, and how does that equilibrate and things, that's a benchmark.

How the OFL will change over time in the projections, well, that's determined based on the recruitments that you use and how the biomass increases, but, if you ask me what is the OFL right now, well, if I fished at the maximum level we're allowed to fish at without overfishing, this is how many pounds of fish it would give me, and so I think I'm just thinking about what these terms mean a little differently.

CHAIRMAN NANCE: Right now, on the books, we have, for recruitment -- I think it's the average of 2009 through 2018, and that's what we have when we first came back in January. We certainly can continue with that, but I think, with our discussion, we weren't comfortable with that, and so is there a reason to change that? There were certainly a lot of comments and a lot of discussion on this, but I want to make sure that we as an SSC are comfortable with the value that we're proposing to go forward with for the Center to run.

DR. SIEGFRIED: Mr. Chair, to that point?

CHAIRMAN NANCE: Yes.

DR. SIEGFRIED: I do need to add something to what I said before, because we're talking about average recruitment, and it is a moot point if the group thinks that that MSY is okay and we don't need a proxy. If we use the stock-recruit curve, it also takes into account the lower SSB and won't produce recruits that are out of range of what that SSB can provide, given the steepness estimated in the model, and so all of this discussion of what average recruitment should be is moot if you decide to use the stock-recruit curve.

CHAIRMAN NANCE: I know we had -- In January we had a long discussion of that, and that's why we went with SPR 30, and so I think it would be not wise to go back with that value, and so I understand exactly what you're saying, Katie, but we need to move forward on this, and so do I hear any recommendation to allow us to move forward, or do we want to keep it at 2009 to 2018? Is there anybody online that wants to interject there? Harry.

MR. BLANCHET: I say keep it at 2009 to 2018.

CHAIRMAN NANCE: Okay. Any opposition with that suggestion? That way, we can have that, and, as Dave suggested, we can use a more recent time for ABC. I think it's been a great discussion, and I've learned a lot, and I think it's moving us forward on the greater amberjack. Unless I hear any opposition for recruitment, we're going to go with the average of 2009 through 2018, and the ABC is taken care of with the rebuilding plan, isn't it?

DR. CRABTREE: We need an F rebuild and then something showing what those yields would be using this recruitment during these years, this average recruitment.

MR. RINDONE: Previously, it was set at 75 percent of SSB at SPR 30 percent, the last time you guys updated the amberjack.

CHAIRMAN NANCE: Okay. Nathan.

DR. VAUGHAN: Thank you, Chair. I just wanted to interject, just while you're making the decision, so you can think about the rebuild, and I don't have the exact numbers, obviously, but, roughly, if you choose that 2009 to 2018, for the OFL projections, it's not going to impact the F. As I showed in the presentation, that F OFL will be the same no matter what recruitment we select, but it will impact, obviously, your spawning biomass at equilibrium, and, hence, what your SSB that we're rebuilding to will be.

It's going to be very close, and so, if you use an ABC target, or rebuilding target, of 75 percent of that, it will have an impact. Due to the really low spawning biomass it's going to predict with that 2009 recruitment though, you're not going to get very much reduction in F under the rebuild plan, because we're so close to the rebuilding target already, and so that's just something that I thought that the SSC should be aware of when they're making this decision, that a rebuild-specific projection decrementing F, in the years up to 2027, will probably not be very much lower than F OFL, with that 2009 assumption.

CHAIRMAN NANCE: Thank you. Okay. John.

DR. FROESCHKE: To that point, my understanding is that, with these settings, that the stock was not overfished anymore, and so we don't need an F rebuild. It's well above MSST, at least according to the presentation.

MR. RINDONE: It's not rebuilt to SSB at the MSY proxy, and so it's no longer overfished, but it's still in a rebuilding period.

It’s the same situation that we’re in with red snapper, and it’s no longer --

DR. FROESCHKE: That’s not how we did it in January though, right?

MR. RINDONE: We didn’t do it that way in January, because the stock was still in the dumpster on all accounts at that point, but, under this revised projection scenario, SSB over MSST is greater than one. Therefore, it’s not overfished anymore, but SSB over SSB at MSY is still less than one, and so it’s not rebuilt, and so we haven’t satisfied the rebuilding plan yet. It’s the same thing as red snapper. Red snapper is not overfished anymore, but it’s also not rebuilt, per the data that we have.

CHAIRMAN NANCE: Correct me if I’m wrong, Katie, but the values that were presented in January were incorrect, and is that --

DR. SIEGFRIED: Yes.

CHAIRMAN NANCE: Okay. So, if we would have gotten what we thought we had in January, it would have been in that not overfished situation in January.

DR. SIEGFRIED: That’s right. Can I add one thing to that?

CHAIRMAN NANCE: Yes, you may.

DR. SIEGFRIED: The F rebuild is not that different from the OFL projection, but I do recall what Ryan said, that they chose the 75 percent F SPR proxy, or FMSY proxy. Sorry. Instead, because that allowed for some space between the ABC and the OFL.

CHAIRMAN NANCE: Yes, and I think, to stay consistent with what we talked about in January, I would propose that we stay with that same ABC. Any opposition to that? The OFL is based on 2009 through 2018, and the ABC is 75 percent SPR 30. Trevor.

MR. MONCRIEF: Just one quick comment. I am kind of building off of what Dave Chagaris and what I brought up and everything else, and this idea of moving goalposts, and what Dave has kind of reiterated, but correct me if I’m wrong, but I was bringing up with 1995 to 2018, or 2000 to 2018, that time period, we wouldn’t necessarily be shifting the goalpost entirely over to an entirely new regime by using the recent recruitment.

Instead, it would be more of a stepwise approach, where we would be moving to a different time period and saying, in this modern time period, plus what Josh had talked about with the ecosystem

and everything else, this is our new goal and what we're moving toward.

Once again, correct me if I'm wrong on it, but it seems like, if we're going to go with a recent recruitment, we are just drastically changing the goalpost at this point, and the only reason that I brought up the recent time period and the potential for it was for us not to get back to that by using the entire time span, right, those goalposts that Dave was supporting, and I feel like we've been going down that route for a very long time, and this stock has stayed where it is, and each assessment has had similar results every time.

My idea was just, if we move it to a different time period, we're incrementally moving the goalpost and saying this is what we've observed over the last twenty or so years, and this is our new goal and what we're trying to build to.

CHAIRMAN NANCE: So would you propose --

MR. MONCRIEF: If I had to make a proposal, I would say 1995 to 2018, because of what John brought up with them identifying out to a species and then the actual comps beginning for the headboat fishery.

CHAIRMAN NANCE: You're saying because that 2000 data point is in there, and it would -- With 1995, it would keep that one in there. Roy.

DR. CRABTREE: But, just looking at it, I don't think the average level of recruitment would be much different.

MR. MONCRIEF: No, I don't think it would be much different, but we're basing it on a longer time period with the additional points in there from the 2000 time period, and so, yes, it's going to increase a little bit, but I think it's worthwhile at least in including that increase a little bit, so that we're not completely shifting our goalposts to the other side of the field.

CHAIRMAN NANCE: The reason that I was suggesting 2009 through 2018 is that's what we came up with in January, after a long discussion, and so, in my mind, I would rather keep it there, but I understand what you're saying, Trevor. Roy.

DR. CRABTREE: To me, it's not much different, in terms of where the goals are, because the recruitment values aren't much different, and so I don't know that it has much practical significance.

CHAIRMAN NANCE: Unless I hear any opposition, we're going to keep it at recruitment at average 2009 through 2018 for the OFL and ABC at 75 percent SPR 30. Okay. It's been a great discussion, and I appreciate all the discussion. Katie and Nathan, thank you for that presentation that you gave us. It was excellent, and it caused a lot of discussion. Ryan.

MR. RINDONE: I think we have a few more things to fill out on your table, Mr. Chair.

CHAIRMAN NANCE: Let's do, for landings --

MR. RINDONE: This is bearing in mind that the initial year of management is likely not going to be until 2023, and so a decision needs to be made with respect to, at a minimum, 2021 and 2022. You could use the data that are available for 2020 at this point, and those data would be considered final.

CHAIRMAN NANCE: Okay, and so we would use data -- I am trying to think of where we're at here, and so we need an average of, let's see, 2019 to 2021 -- No. In 2020, we would have the data, right?

MR. RINDONE: Your most recent three-year period of finalized landings data would be 2018 to 2020. John is throwing up a road flare in the back.

CHAIRMAN NANCE: John.

DR. FROESCHKE: Just on that 2020 data, since that's imputed, that's really recycled data from other years, and so I would just recommend not using that one, because it's already -- That information is really just a repeat of the others.

CHAIRMAN NANCE: So what would you suggest? 2017 through 2019? Okay. So 2017 through 2019, use the average of those, and that's the same thing as we have written down here, isn't it, for coming up with values for 2021 to 2022, and is that what we're saying?

DR. FROESCHKE: That will be used for the years 2019 through 2021, and it will be three years that you've got to fill, right?

MR. RINDONE: No, and you would use 2019 as you have, and then 2020 would use -- 2021 and 2021 would both use 2017 to 2019.

CHAIRMAN NANCE: That's right.

DR. FROESCHKE: Carrie wants to start projections in 2022, and

we'll see if we can get it done.

CHAIRMAN NANCE: Okay, and so we're just going to use the average of those for 2020 and 2021? Okay. We're going to use those averages to come up with the values for 2020 and 2021. That's correct. Okay. Perfect. Allocation ratio. This is where we got into this, isn't it, because every allocation we come up with is going to change it, isn't it, Katie?

DR. SIEGFRIED: Yes, and it changes slightly, based on what I showed you in that plot, in those series of plots.

CHAIRMAN NANCE: But, right now, we have an allocation of 27/73, and is that correct, and so that's what I would suggest we use.

MR. RINDONE: We have to do it for the other allocation scenarios that are in the document, also, and so the base version would be 27/73, and then the other versions would be the other ones that are being considered in the document.

Mr. Chairman, I'm sorry to do this, but, to go back a step, on the landings, I'm looking at -- In 2018, we modified the fishing year for the recreational fleet to begin on August 1 and end on July 30, and it's got a couple of in-season closures, but, bearing that in mind, and looking at the commercial and the recreational landings against the ACL, there is considerable variability in the landings, year over year, and it's not that things are consistent.

In the imputed data, John, to your point about 2020, it's still a smaller fraction for -- If I remember correctly, I think it was like 11 percent or something like that, but -- Was it 7 percent?

MR. MONCRIEF: Yes, it's 7 percent, and I think, really, what that comes down to is the distribution of the fishing season and the time that all the states actually got back in order, and so that's the reason why the percentage is so low.

MR. RINDONE: At that point, there's a seasonal closure from November 1 to April 30, and so that includes Waves 1 and 2, and Wave 2 was the one that was most affected by all the closures and everything, for all species, but, by Wave 3, stuff really started getting back off the ground, as far as the states doing their APAIS intercepts.

The effort surveys never really waned, because those were all done by mail, and so I would just encourage you to think about how you're treating 2020 specifically, because the amount of imputed data seems not substantial.

CHAIRMAN NANCE: Trevor.

MR. MONCRIEF: To that point, I think it's worth at least consideration of that, given the low percentage. I think that should be -- It's there, and it's very low imputation, and it should be fairly consistent with recent years.

CHAIRMAN NANCE: Okay. Jim.

DR. TOLAN: Thank you, Mr. Chairman. The point I was going to make had to do with the next category, the allocation ratios, but Ryan jumped all over that, and I think the defaults ought to be the existing ratio, but, because the council asked specifically for these other permutations, I think it needs to be included in this, but that's the point I was going to make, but Ryan jumped all over that one, and so thanks.

CHAIRMAN NANCE: Okay. Thank you, Jim. Doug.

MR. GREGORY: Thank you. On the same topic, there is three allocation ratios that were presented from the Center, and I guess those three, and the current one, can be analyzed, but I don't see us making a recommendation on anything, because we have no information. We don't know what the basis of these ratios are, and we have no document. We have no information about how they were derived and what the rationale is, and so I don't know when we'll get these analyses done, but, if it's at this meeting, I would be very hesitant to make any -- To have discussion, or recommendation, on any ratios based on very little information.

CHAIRMAN NANCE: It's not going to be at this meeting, for sure, but I think the council came up with some recommended allocation ratios that they wanted to look at, and so, for our base case, we're using 27/73, but then the Center will run those other allocation ratios. Katie.

MR. GREGORY: Before we go on, my understanding, from looking at the motions from the council, is they simply asked staff to come up with some different allocations, and the council itself didn't recommend them, unless it was at a meeting after January.

CHAIRMAN NANCE: Okay. I don't know on that.

MR. GREGORY: I just want some background information as to the basis of it.

CHAIRMAN NANCE: Okay. Katie, do you have any -- Go ahead.

DR. SIEGFRIED: Doug, I don't have an answer as to how those ratios were determined, except for a little bit of information from folks on the IPT. That was a council request that we received to explore those four different scenarios, and so what I assumed would happen is that we would complete those separate from what you all are deciding here, but related in that we've used the same settings for five of those, and so just not the allocation ratio. The rest of these projection settings for the base run we would use in those allocation scenarios.

The other thing I wanted to mention is that, if you're not going to assume different recruitment for the OFL and ABC run, we probably don't want to separate them in this table, and we probably want to -- I mean, do you think that you all will want to see the F rebuild, as classically provided, and the 75 percent F SPR 30, or do you just want the 75 percent SPR 30, because the ABC and the OFL are too close together when we use just rebuild?

CHAIRMAN NANCE: I think we could see the F rebuild and also 75 percent SPR 30.

DR. SIEGFRIED: Okay.

CHAIRMAN NANCE: I know what you're saying about we have it split here, and I think that was just kind of for our discussions. Does that cause you heartburn, to leave it like this?

DR. SIEGFRIED: Not as long as it's captured in the notes. No, I understand. The other thing is the landings, unless it's in the parentheses, the average of 2017 to 2019, used in 2020 to 2021, is consistent with what was used for SEDAR 70, if you wanted to put that in the comments, unless you're going to move to the 2020 value for the data.

CHAIRMAN NANCE: Okay. Let me ask. Are we going to use 2020 as a real data point, or is it going to be calculated? Trevor, did you say use 2020 as its own?

MR. MONCRIEF: I think it's worth at least looking into. It's a very low level of imputed data, and, I mean, at the point when they start running the projections -- It should be final on what date?

MR. RINDONE: They should be final now. I mean, by this point, the commercial data have been received, and the Texas high-use data have been received. It should have a bow on it at this point for 2020.

CHAIRMAN NANCE: Katie, do you have the ability to use 2020?

DR. SIEGFRIED: We would have to query the data providers, but, if it's not due until January -- I believe we could fit that in, but I would have to just check with them.

CHAIRMAN NANCE: Okay. If it's doable, use it. If it's not, then we'll use the average. How is that?

DR. SIEGFRIED: Sounds good.

CHAIRMAN NANCE: Okay. Then allocation ratio, and then steepness is the last one. Did we use steepness at all?

MR. RINDONE: Steepness was profiled and leveled out between 0.7 and 0.8, and, ultimately, the model had converged on a value of 0.777, but, insofar as it functionally matters, basically, the likelihood value had shown that any value between 0.7 and 0.8 had a similar probability of being plausible, but 0.77 is the value that was actually input based on that.

CHAIRMAN NANCE: Let's just go with that, the 0.77. Carrie.

EXECUTIVE DIRECTOR SIMMONS: Thank you, Mr. Chair. Just a question, but I thought that we were going to perhaps look at these later in the meeting, the projections, because I think most of this was already done, and that was my understanding. If we have to wait until January, then we'll be way behind, if we have to do a rebuilding plan for this stock.

CHAIRMAN NANCE: Okay. I don't know, on the Center.

MR. RINDONE: Well, technically, we're still in a rebuilding plan, and we're revising it as a function of all of this, but, for the allocation ratio, we need to include the other stuff that's in the document there too, and not just the 27/73.

EXECUTIVE DIRECTOR SIMMONS: My understanding, from talking to the Science Center, was that we were going to look at this later in the week.

DR. SIEGFRIED: To that, Mr. Chair?

CHAIRMAN NANCE: Yes.

DR. SIEGFRIED: I can certainly provide that if the landings do not change.

CHAIRMAN NANCE: Okay. Let's go ahead and do that then. We'll go with the landings the way they are, and, if you can provide those later in the meeting, we'll go with that. Katie, any questions on these, on the table?

DR. SIEGFRIED: I don't think so, but, if you wouldn't mind, I'm just going to go through this one more time, because it's complicated.

CHAIRMAN NANCE: Absolutely.

DR. SIEGFRIED: I am going to provide -- For the base run of the assessment, I am going to provide projections that will then help us determine status using the average of recent estimated recruitment from 2009 to 2018. I will also provide the ABC projections, using both F rebuild, which I will calculate as an iterative process in Nathan's code, and 75 percent of F SPR 30, to allow you to compare those. The landings will stay the same as were input at the January meeting, and the allocation ratio is what remains on the books.

I can relatively easily provide the other four allocation ratios, but it's two steps, right, and we have to accept the first set for the base run of the model, the projection, and the management advice from SEDAR 70, and then you will be able to take a look at those allocation ratio requests, and is that -- Is my understanding correct?

CHAIRMAN NANCE: Yes, that's correct.

DR. SIEGFRIED: Okay.

CHAIRMAN NANCE: Carrie.

DR. SIEGFRIED: Sorry, but the steepness is actually 0.777, and we have shorthanded it in the document, quite often, as 0.8, and I just don't want that to be confusing for later. Thank you.

CHAIRMAN NANCE: Okay. Then that's perfect. Thank you. Carrie.

EXECUTIVE DIRECTOR SIMMONS: Katie, isn't this what's on Slide 25, the first table, already done, except the different allocation scenarios? Isn't that first-half of that table what's already been done?

DR. SIEGFRIED: For the OFLs, it is. SPR 30 with recent recruitment. Yes, this would be the OFL, but I don't have the

rebuild and the 75 percent F SPR in this presentation.

CHAIRMAN NANCE: It would be the -- Wouldn't it be the bottom part of that, the base, with new code?

DR. SIEGFRIED: Yes.

CHAIRMAN NANCE: Okay. Then the OFL would be those values there that have already been run. Okay. So we have that, and we just need to have the other ones run then.

MR. RINDONE: Well, you need to have the 27/73 version run and then accept that, and then, after that, you can look at the alternative allocation scenarios, and then, at that point, the council would be looking for you to say that these have been done appropriately. Should the council decide to choose one of these, these are all mathematically correct in the way that they have been determined.

DR. SIEGFRIED: This is 27/73, and both of these are the base run allocations.

MR. RINDONE: Right. For the OFL, and so you just need to see the ABC side of that, and then determine whether you think that's appropriate, BSIA, blah, blah, blah, and then, from there, move forward.

CHAIRMAN NANCE: Thank you, Katie. We appreciate it very much. John.

DR. FROESCHKE: Just so I'm crystal clear, that landings row, I don't know what we came in for landing on the landings.

CHAIRMAN NANCE: The landings row, if we want to have it with that, then the landings have to stay the same as they were in January. If we change it, then we're not going to have it until a little bit later. Not this week.

DR. FROESCHKE: So 2017 through 2019.

CHAIRMAN NANCE: That's correct, yes.

DR. FROESCHKE: Okay.

MR. RINDONE: Insofar as that matters, Mr. Chair, we don't have -- I don't have the 2020 data up in front on the NOAA ACL monitoring webpage yet, but I'm working on it, and, I mean, there was definitely a drop in the landings for the 2019/2020 recreational

landings, compared to the previous year.

For 2019/2020, it's showing that only 48 percent of the ACL was landed, but, for those historical data -- It looks like for the previous year, 87 percent -- For 2018/2019, 87 percent of the ACL was landed, and, for 2017/2018, 72 percent was landed, and so considerably fewer fish were estimated to have been landed in the 2019/2020 fishing year for amberjack.

Then the 2020/2021 season would pick up in August of 2020, and so I would normally recommend to you that it doesn't matter, if there's not been some significant change along the way, or anything in particular has happened -- If the average of 2017 to 2019, or 2018 to 2020, isn't going to be that dissimilar, then who cares, but, in this case, it might be, and so that's just something to think about.

CHAIRMAN NANCE: Okay. That took us five hours. That's good. Let's go ahead and do Number VIII and Number IX. Not together, but we'll start with Number VIII, which is Terms of Reference for Gulf of Mexico Migratory Group Spanish Mackerel.

TERMS OF REFERENCE FOR GULF OF MEXICO MIGRATORY GROUP SPANISH MACKEREL OPERATIONAL ASSESSMENT

MR. RINDONE: Okay, and so we are looking to start the Spanish mackerel assessment in 2022, like near the end of 2022, using data through 2021, and this is an operational assessment. You guys have already looked at the scope of work for this, and this operational assessment will see a migration of the recreational catch and effort data for Spanish from CHTS to FES.

The Spanish mackerel assessment is dusty. It was last done in 2013, using data through 2011, and so the data are ten years old at this point. On the fortunate side, from the anecdotal information side of things, we haven't heard much from fishermen about there being any problems with Spanish mackerel, and so hopefully that's a good thing.

We're going to ask if any changes or corrections to the model and input datasets be included. Specific to MRIP, we want to see the changes in the magnitude of catch and effort, since this is the first time we're seeing that migration to FES for this species, and we're asking also to generate a catch equivalency table to describe the catch recommendations which would have resulted had MRIP-FES data been used in SEDAR 28.

I don't expect this to have as much of an impact, particularly for

Spanish, because it's a stock ACL, and there is not sector allocations for Spanish, and the landings have generally been well below the ACL for this species for the last twenty years, at least.

To the degree that some of these things are possible, we're asking that the length frequency for the commercial fleets be included, update life history information, if warranted, consider age-dependent versus constant natural mortality estimates, and characterize any differences in annual commercial and recreational landings data from the SEDAR 28 stock assessment that are greater than 5 percent in any given year, respecting appropriate data currencies.

If there's any shifts, massive shifts, going from the data that were used the last time to the data that are used now, just because it's been ten years, and there is constant QA/QC that goes on with these data, it just would be nice to flag those and have a chance to talk about them.

Something from the last assessment was to consider -- Under TOR Number 3, it was to consider whether steepness could be estimated with or without a prior, and, if it's fixed, evaluate the sensitivity of that assumption. Then our typical gamut of updating model parameter estimates and uncertainties and estimates of stock status, according to our current management benchmarks, and status determination criteria.

Last, but not least, some of these things about the projections, like otherwise -- Unless otherwise recommended, use the geometric mean of the previous three fishing years to determine F current, and I don't know if you guys have any questions, and, I mean, the rest of this is pretty standard. We're not recommending a topical working group for Spanish mackerel though, and that was based off of what we had talked about the last time, when we talked about the scope of work.

CHAIRMAN NANCE: Shannon.

DR. CALAY: Thank you. I believe that Spanish mackerel is one of the few assessments where we've actually used an ensemble modeling approach and used three different steepnesses. Is the intention to continue with that approach, or is there some different treatment of steepness that is more appropriate?

MR. RINDONE: Shannon, I think, based on the research recommendations from SEDAR 28 to consider steepness can be estimated, and perhaps that should be the starting point, to play around with that and see if that's plausible. Again, there's going

to be a certain amount of artistic license afforded to you guys, as far as that's concerned, because it says to consider it, and it's certainly not a requirement to deviate from what was done the last time, but to at least investigate.

CHAIRMAN NANCE: Is that satisfactory, Shannon?

DR. CALAY: Yes. I mean, that sounds satisfactory.

CHAIRMAN NANCE: Okay. Thank you. Any comments or concerns with this?

SCOPE OF WORK FOR GULF OF MEXICO MIGRATORY GROUP COBIA OPERATIONAL ASSESSMENT

MR. RINDONE: Seeing no mutinous movements in the room or online, we can move on to the scope of work, Mr. Chairman, for cobia, and we will put our rubber stamp on these terms of reference for Spanish mackerel, and we'll send them off to SEDAR and the Science Center.

For cobia, the scope of work, and remember that the scope of work is kind of our first step before we get to the terms of reference stage, and this is where you guys have the opportunity to really try to figure out exactly everything that you want to see happen in the next assessment. This will be an operational assessment, to be held in 2025, using data through 2023.

The exact timing of when this will happen in 2025 may change somewhat, and, as such, so could the model's terminal year, and so, typically, the later -- If an assessment begins say like in October or later, then it's possible to consider data from the previous year, but, if not, then it needs to be two years back, because just the pace at which data become available and finalized.

Again, we're going to have an evaluation of the MRIP data, as has become standard, and an evaluation of life history data, if warranted, and, from the SEDAR 28 update assessment, a reexamination of the Stevens and MacCall method for developing a subset of trips that target cobia.

As you guys are aware, cobia are not directly targeted by a majority of the directed fleets, and they're kind of an incidental catch. If you see them, you might throw a lure at them. If you catch one, it's like, oh, that's neat, and there aren't a large number of fishermen that go out and say we're going to catch cobia today and then actually do it, and so being able to better identify those trips that have targeted cobia is important. Dave.

DR. GRIFFITH: I was wondering, and, regarding those kinds of trips, do you have information about the market values of cobia? It seems to me that this is a -- In the Atlantic anyway, and I've not sure about the Gulf, but it seems to me that more and more people are targeting cobia, recreationally and maybe commercially as well, because it's a really, really good eating fish, and it's kind of a fun fish to catch.

MR. RINDONE: So it's a good eating fish, and it is a fun fish to catch, and the issue is more that the abundance of cobia in the Gulf isn't uniform throughout the Gulf, and there are metapopulations that occur throughout the Gulf, and so, from like east Texas to west Louisiana, there's a metapopulation, and there's another one that occurs around the mouth of the river, and there's one that's in the Big Bend area, and then there's another that occurs from about Tampa Bay south through the Keys.

Then there's the Florida East Coast Zone of cobia that are genetically similar to cobia in the Gulf, and they're considered part of Gulf cobia, and they go up to about the St. Augustine area, and, for ease of demarcation and law enforcement, we've just drawn a boundary between the Gulf and Atlantic migratory groups at the Florida-Georgia state line, but the size of those metapopulations, especially compared to the amount of area they cover, definitely varies, and the concentration of where these cobia are often landed is certainly in the northern Gulf.

Say from east-central Louisiana over to about let's call it Destin, there's a large amount of cobia that are landed in the Gulf from that area, and then less so outside of that, and so I don't know how that affects differences in dockside prices for the commercial sector, as an example, but the commercial sector accounts for a very small proportion of the total landings in the Gulf, and so tens of thousands of pounds, at best, and the rest is from the recreational sector, and that can -- That's different for the Florida East Coast Zone and for the Atlantic migratory group, but, for the core of the Gulf stock, the commercial sector makes up a small fraction, and I think Dr. Scyphers has more.

CHAIRMAN NANCE: Steven, please.

DR. SCYPHERS: Thank you, Mr. Chair. I actually just sent a little bit of text to the meetings email address, and, Ryan, please feel free to tell me if this is not within the scope of an operational assessment, but I often suggest that we include a pretty general term of reference there on incorporating social and economic information, as practical, and I think that might get at some of

the things that Dr. Griffith was pointing at, too.

CHAIRMAN NANCE: Doug, please.

MR. GREGORY: Thank you. Ryan, given our recent discussion today about averaging retention parameters and selectivity parameters, as well as the geometric mean for F, would you want to include that in this term of reference, in addition to the geometric mean of fishing mortality, include the average of the other two parameters, and/or, since we have kind of an agreement, going forward, that this is the way it's going to be done, we could take out the requirement for the geometric mean.

MR. RINDONE: We can certainly do that, if that's what you guys want to do, and so it's more what you guys want to do and less what I want to do, unless you guys don't talk about it, and then it becomes what I want to do, and so I encourage you to tell me what you want to do, so I don't end up just speaking for you.

MR. GREGORY: Well, it's my understanding that what Katie said was that the Center wanted our agreement on some of these things that they had been doing, and I was only aware, explicitly, of the geometric mean of fishing mortality of the last three years, and so it just seems, for consistency, that you can include them or not, and I am just bringing it up.

CHAIRMAN NANCE: I think it would be good to include them, because, I mean, we had a long discussion on it, and I think it's better to have it on here than to not have it.

MR. RINDONE: Using the geometric mean for F current is included in here. Using the average of the last three years for selectivity, retention, et cetera, those are things that can be put in.

CHAIRMAN NANCE: I think that would be good to do.

MR. RINDONE: Jess, can you send me Steven's addition?

CHAIRMAN NANCE: Trevor.

MR. MONCRIEF: Real quick, to build off of your comment about the viability of them, I think it's important to note, and I am correct on this, right, Ryan, that the commercial fishery for cobia -- They are held to the same recreational size and bag limit as the recreational guys?

MR. RINDONE: Correct.

MR. MONCRIEF: So they have a very small possession limit, similar to the recreational guys, and the market, I think, in itself, would pay top dollar, but there's not a lot of product moving across the board. In states like Mississippi, it's the only recognized gamefish, and so we don't even allow commercial sale of the species at all.

The other thing I was going to bring up is that we will be five years. Sorry. 2023 is the beginning year, and it will be three years until a length limit change on this species, and is the operational assessment a place to be able to have a bullet in there about evaluating whether or not that regime change is actually corresponding to a drop in fishing mortality, or do we need to do that at another time?

MR. RINDONE: Well, I think we should be able to do that just from the length comps from the landings, and so, when we look at the length comps from the recreational and commercial fleets, that should be pretty apparent, especially for the commercial fleets. Even though the proportion of landings are lower, the data are generally just of higher precision, and so you should be able to see what the effects are with respect to the fish that are actually coming across the dock.

Now, even in 2023, I hesitate to say that you're going to see any sort of effect from recruitment, and I think you're just going to need time, and there's not a terrible amount that's going to help that, I don't think.

MR. MONCRIEF: I agree with you on the recruitment side of things and all that, and it's got to have a little bit more time to get in there, but I think going down that route -- You make management changes, and you ought to be able to look and see what kind of effect they're actually having.

MR. RINDONE: I mean, if you guys wanted to put something in there specifically, I think you can, but I'm just saying those data will be in there, and they will be detailed pretty explicitly as a function of the discussion about the length composition information, and so I think it is taken care of, and you'll be able to see it.

I have, for the purposes of yield projections for selectivity and retention, use the average of the most recent three fishing years, and then, underneath Term of Reference Number 2, we will add Steven's bit, which Jess will have, I'm sure, already sent to me. Is there anything else?

CHAIRMAN NANCE: Any other comments or suggestions? This is our opportunity. Luiz, please.

DR. BARBIERI: Just a minor -- I guess it was a minor issue here, but, under 4, MSY proxy, yield at FMSY or F rebuild, if overfished, I don't think that it would be yield at F rebuild would be the MSY proxy, right?

MR. RINDONE: If it's overfished, the MSY proxy -- Sorry. If it's overfished, the MSY proxy could be set at F rebuild, to make sure that the stock rebuilds by a certain date, like within that ten-year period, and so we would use F rebuild as the MSY proxy, instead of F at 30 percent SPR, which is the difference, I guess, between using the MSY proxy or depletion.

DR. BARBIERI: No, and I think that this would be -- Really, the F rebuild would be the F ABC, right? The MSY proxy would be your OFL, right?

CHAIRMAN NANCE: Katie.

DR. SIEGFRIED: I will yield to Doug first, so he can make his point.

CHAIRMAN NANCE: Okay. Doug, please.

MR. GREGORY: I will be quick. I agree with Luiz on this, and I also have a question. Should we be more explicit about MSY? Since we're going to be estimating steepness, we might be able to estimate F of MSY directly, but should we have in there an MSY proxy of F of 30 percent SPR, or an F of MSY, so we get both?

MR. RINDONE: I can change the MSY proxy to be the yield at FMSY or proxy, and then, if it ends up being maintained at F 30 percent SPR, then that's covered by the proxy.

CHAIRMAN NANCE: Carrie.

EXECUTIVE DIRECTOR SIMMONS: Don't we ask for what's on the books from the council for the MSY or MSY proxy, and so, if it's like Fmax, or 30 percent SPR, that's what we ask for in these, typically?

MR. RINDONE: Yes.

EXECUTIVE DIRECTOR SIMMONS: I just don't want to add confusion to these scopes of work right now, which we'll come back and get the

terms of reference after that, right, Ryan?

MR. RINDONE: Right.

MR. GREGORY: Then, whatever the council has, we should explicitly put it in here.

MR. RINDONE: Okay. I will do that.

CHAIRMAN NANCE: Roy.

DR. CRABTREE: Just but that's a very different thing than the yield at F rebuild, and so I would separate that. My guess is this will be a very data-poor assessment, and I don't have high hopes that we would be able to really estimate MSY or steepness very effectively.

CHAIRMAN NANCE: Katie.

DR. SIEGFRIED: Thanks, Mr. Chair. Everybody has pretty much made my point already, in that I was going to recommend that the MSY proxy be what's on the books, like Carrie said, and separate F rebuild, as a few of the other folks said, with overfished as a sub-bullet, just because I -- Just to keep it cleaner. Thank you.

CHAIRMAN NANCE: Okay. Thank you for those comments.

MR. RINDONE: Okay. I've incorporated Steven's motion, and I've made that edit to the FMSY proxy, and I have the instructions for the projections for selectivity and retention. Is there anything else? Again, a topical working group is not recommended for cobia, unless there is specific issues that you guys want to see discussed more specifically.

CHAIRMAN NANCE: Doug, please.

MR. GREGORY: For Katie, since we just came out of the scamp assessment review workshop, and Max from Sweden told us it was very important, under Item Number 3, the second bullet, explore logistic selectivity for recreational and commercial fisheries, it seems to me that should be explore logistic and double logistic, or double normal, selectivities. What do you think?

DR. SIEGFRIED: I think that what he was pointing out was because we assume logistic, and I think what's pointed out here is -- My guess is because they assume domed, and so --

MR. GREGORY: I don't know. I don't recall.

DR. SIEGFRIED: I don't either, and I was just pulling up the report, but I think certainly explore selectivity functions, as appropriate, or as they fit the data, something like that, would allow us to explore the different functions that fit the data well.

MR. GREGORY: Okay. That sounds good. Thank you.

CHAIRMAN NANCE: I think that's a point well taken.

MR. RINDONE: Okay. I have made that change. Anything else? All right. I will send this via carrier pigeon to SEDAR.

CHAIRMAN NANCE: It might be faster than the U.S. Mail.

MR. RINDONE: Some days I would be inclined to agree. I think that takes care of us for what we can reasonably expect to accomplish this afternoon, Mr. Chair.

CHAIRMAN NANCE: Okay. I appreciate the discussion today. We had five hours slated for our discussion with the Center on greater amberjack, and we used that, and I really thought we had some great discussions that I think will help us in future assessments. I appreciate your time, and we'll go ahead and adjourn until tomorrow at 8:30. Dave Chagaris is going to give us the first presentation on red tide ecosystem modeling. Thank you. We stand adjourned.

(Whereupon, the meeting recessed on September 27, 2021.)

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September 28, 2021

TUESDAY MORNING SESSION

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The Meeting of the Gulf of Mexico Fishery Management Council Standing and Special Reef Fish, Special Socioeconomic & Special Ecosystem Scientific and Statistical Committees reconvened on Tuesday morning, September 28, 2021, and was called to order by Chairman Jim Nance.

CHAIRMAN NANCE: We'll go ahead and start. I appreciate everybody being here for day two. We'll go ahead and start with Dave Chagaris on the red tide ecosystem modeling presentation. Dave, you have the floor.

RED TIDE ECOSYSTEM MODELING

DR. CHAGARIS: Okay. Thank you. Thank you for the opportunity to speak with the group on this project. I will say that I noticed that, last night, that the PDF version that I sent to Ryan did not include the final editorial changes that I was making the night before the deadline, but I am happy to share this updated version, and it's mainly just editorial things, and all the slides are still in the same order, and so I just wanted to mention that.

Today, we'll be talking about red tides and the ecosystem modeling work that we've been doing as part of this project funded by NOAA RESTORE. I would like to acknowledge the co-authors on this, mainly Daniel Vilas, who is a PhD candidate here and a graduate assistant in my lab. He's done a lot of the heavy lifting, as far as running the simulations and processing the output and updating the model, and he's really just been a workhorse on this project.

Joe Buszowski is one of the programmers with the Ecopath international initiative, and he has added some new code to the software that allowed us to do these red tide simulations. Zack Siders is also a research faculty here at UF now, and he helped develop some code that allows us to update the red tide maps and the models in a more timely fashion. There's been a lot of other people involved in this project, either as data providers or just reviewers on some of the preliminary modeling work, and so they're listed there, and I won't call them off by name.

Before I get into the red tide work, I kind of want to take a step back, and I showed this slide when I presented a couple of years ago to the SSC, but just to kind of think broadly about ecosystem models and where their place is, and I think that ecosystem models can provide a range of advice, from strategic and qualitative advice, such as just when to add more precaution, whether or not to adjust stock assessment parameters.

They can also transition into more tactical and quantitative advice, as far as forecasting population fluctuations, evaluating how well management options perform under environmental change, looking at tradeoffs in harvest policies, and all the way up to actually developing quantitative reference points in multispecies fisheries and ecosystems.

In the Gulf, there is really no shortage of applications for ecosystem models. We have multiple stressors in this system, and primary production can change with the Mississippi River, and we have rebuilding predators, but also forage fisheries, and also ecosystem goals and targets that we're hoping to develop in the

future.

Today, I'm going to be talking more about red tide mortality, and I will be providing some quantitative information, but it may be used in a qualitative sense, as far as setting the catch projections, and I will be focusing on gag grouper for this talk, because it's relevant as far as the stock assessment and some of the decisions that may be coming up later in the day.

As you all know, red tides are a commonly-occurring event on the West Florida Shelf, and there are records going back to the sixteenth century, and so they're not entirely new, but they do occur regularly on Florida's Gulf coast, mostly in the southwest region during late summer and early fall, but they have occurred in all months in all regions of Florida.

They tend to have pretty broad impacts across the ecosystem, resulting in large fish kills, even killing large megafauna, and, also, everybody is aware of the impacts on tourism and human health and respiratory distress.

Red tides have also been incorporated into recent stock assessments. Going back about a little over ten years, we've been trying to incorporate red tide into assessments for gag and red grouper, the first case being the SEDAR 10 update, and this was following the 2005 red tide, which was a severe event that occurred on the West Florida Shelf.

Typically, this is done by adding in a pseudo fishing fleet and estimating an F for that fleet for a pre-defined year, and so the committee has to select which years the red tide would occur, and this usually assumes full selectivity across ages, and the estimate of that mortality rate is informed by the trends in abundance, and it has tended to lead to high estimates of mortality, especially for gag in 2005, but it's somewhat variable, at least going from the SEDAR 10 to the Stock Synthesis assessments.

We also get lower estimates in 2014 and 2018, but we're still not entirely sure what's the best way to include this information into Stock Synthesis, or how to account for red tides in SS models and projections.

Now, red tides are also important when it comes to the projection side of the stock assessments, and so the first part is incorporating them into the historical simulations in the assessment, but then also the projections, and this was first apparent with gag grouper in 2014, when the SSC met to set the ABC for this species, and there was an ongoing bloom in the Florida

Big Bend region, and the way that occurred -- The SSC, I believe, selected an ABC yield stream assuming that the 2014 red tide was just as severe as the 2005 red tide, but it didn't actually turn out that way. The bloom remained isolated in the Big Bend and dissipated over a couple of months, and, eventually, the SSC had to come back and revise that recommendation.

This issue of red tides during the projection years also showed up for red grouper after SEDAR 61, and you can see these are the figures from the assessment document showing the different yield streams and depletion levels based on different assumptions about the red tide mortality in 2018, and so I say this just to emphasize that these red tide effects are important not only in the stock assessment phase, but also in the stock projection phase.

The goals of this project are primarily to improve our ability to account for red tides when assessing and managing reef fish stocks, and there is two objectives. These are related, but they are different. One is to provide historical estimates of red tide mortality for the stock assessment, and the second objective is to provide more contemporary estimates for use in the projections, and so, when we have a red tide that occurs during one of the projection years, what information can we provide for the stock assessment model to incorporate that?

What I will talk about today -- First, I will give a brief overview of the West Florida Shelf model and some of the updates that have been made and how we developed the red tide maps and the red tide responses that are used in the model and some of the validation and calibration attempts. Then I will go over the red tide mortality estimates, focusing on three years where we know that red tide occurred, as well as these last couple of years that are important for the projections, and then I'll talk a little bit about where do we go from here.

I won't go into too much detail with the modeling, and we're using Ecopath with Ecosim and Ecospace, which is a freely-available and widely-used software package for marine ecosystem modeling. It basically has three components. You start with the Ecopath model, which is a static snapshot of the system, and the required inputs are biomass, mortality, consumption, diet, and fishery removals, and this is the starting point for all the dynamic simulations, and so these are initial values.

You then have the Ecosim component, which is the time dynamic, and there is no spatial considerations in Ecosim, but, in this case, we're simulating the biomass dynamics over time, as a function of consumption and predation in fishery losses, and we can also

include environmental forcing and calibrate the model to fit time series of abundance, and then the third part is Ecospace, which basically runs the Ecosim model in each grid of a map, but it also accounts for habitat preferences, distribution of fishing effort, and spatial/temporal/environmental drivers, as well as movement amongst the cells and it's in Ecospace where we were incorporating the red tide mortality.

Just to kind of give you a general sense of what's happening under the hood with Ecospace, this is a spatially-explicit food web model that simulates changes in biomass, B, for a group, I, in a given map cell, K, and so that change in biomass is a function of biomass growth, which is how much a fish consumes multiplied by a food conversion efficiency, the G, and so you have biomass growth minus the losses to predation minus the losses to fishing and other mortality and any migration to or from the neighboring cells.

Now, each of these terms in this equation has another set of equations underneath it, and so the consumption is predicted based on foraging arena formulation, and so that's the Q, the biomass growth and predation losses.

The fishing mortality is predicted by allocating effort spatially based on profitability, and so there's an equation that will distribute the effort and give you the F in each cell, and then the movement amongst cells is driven by the relative habitat capacity, or, basically, how much foraging area there is in each cell, and so an animal that has -- A cell that has low foraging capacity will have low consumption in that cell, and it will also drive movement away from that cell.

We're including red tide effects in two areas here, and the first is on the other mortality term, and so this is a direct effect of red tide, and so mortality, and so the second way is through the habitat capacity layer, and so we're accounting for red tides in the foraging capacity of each cell, and so, where red tides are present, fish can feed less, and that will also drive their movement out of that cell, and so it generates these sub-lethal effects as well as avoidance patterns.

The West Florida Shelf model has gone through several iterations. The first iteration -- The model was first generated around 2001, under contract from FWC, and this was Tom Okey and some other folks from UVC that constructed this model.

At that time, it was entirely functional groups, and so he couldn't really do a whole lot of single-species management scenarios with it, and so, in 2013, as part of my PhD dissertation, I adapted

that original model by adding more resolution to some of the key managed species. Then, in 2017, with the NOAA RESTORE project, we've done additional updates. Mainly, we've restructured a few of the groups, adding more age stanzas for groupers and doing a major overhaul of the spatial model.

The current version now has eighty-functional groups and eighteen fishing fleets. Some of those functional groups, as you will see, such as gag grouper, have age stanzas in them, and so we do have age structure built in. The start year is 1985, and we calibrated the time series through 2017. Then we've done a lot of updates to the Ecospace model.

The major updates we did included defining the habitat preferences and species distribution, and so we had to do this for all the groups in the model. In most cases, we were able to develop some of these habitat preference functions based off of survey data, and so their relationships with depth, temperature, and salinity, and we did this through some statistical modeling, and the foraging capacity in each grid cell -- So we set up these habitat preferences, and then we have environmental layers underneath it, and so, for each cell, we get a calculated foraging capacity, which is the product of the preferences for all the environmental variables in that cell.

The second major update was filling out the spatial and temporal environmental drivers, and so, in this case, each monthly time step of the model, the maps will update, and so we'll have a monthly map for temperature, salinity, Chlorophyll-a, surface and bottom temperature, and we develop these maps from the satellite imagery and the HYCOM oceanographic model.

Now I am kind of narrowing in on gag grouper, which will be the focus of the rest of the presentation. Gag are represented in the model as six age stanzas, going from age-zero to five-plus, and this was done to capture their ontogenetic changes in habitat and feeding, as well as the fisheries selectivity pattern.

As I mentioned before, we initialized this for 1985 biomass, landings, and mortality, and that was all taken from the previous stock assessment, the SEDAR 33 update, and the diet composition of gag was based on almost 1,500 stomach samples, and these were primarily collected from FWC's gut lab, and some were also contributed by Will Patterson, and there was some data available in the Gulf of Mexico's species interaction database.

In Ecosim, we calibrated the model to fit the time series of biomass and catch out of the stock assessment, and so, in this

case, we're fitting to the data that were predicted by the stock assessment model and not the observed data, and only in Ecosim are we calibrating any parameters, and I will mention that several times.

For Ecospace, we had to include baseline dispersal rates, and we assumed thirty kilometers per year for ages-zero to one and fifty kilometers per year for ages-two-plus. The fifty kilometers per year for ages-two-plus was based off of some rough estimates coming from tagging studies. The thirty kilometers is a rule-of-thumb that we used, because we didn't have information on those younger ages for movement, and then we included habitat preferences for depth and rugosity.

Things like temperature and salinity didn't really come out as significant in some of the statistical models, and you can see here, in these maps on the bottom-left, the light-gray-shaded area, in the bottom-left corner of these maps, and you'll see this throughout, and that is an excluded area, and we aren't actually modeling the dynamics in the offshore habitat, and just on the shelf, but you can see the computed habitat capacity here for the different age stanzas for gag, where age-zero are really close to shore, age-ones are still very close to shore, but a little bit further out, and age-twos are starting to move even further out. Then, as they get to age-three and four, most of the biomass is distributed along the high-relief habitat further offshore.

As you run the model -- This is the initial computed capacity, but, as we run the model, those biomass distributions tend to converge a little bit more over time, and so how we're incorporating red tide mortality into the Ecospace model, the first step is to develop the monthly red tide maps that will go into the spatial and temporal framework, and so, at each month, there will be a map applied that has red tide cell concentrations.

We do this with a combination of satellite imagery, the cell concentrations from FWC, and we krige those and clip it to a satellite imagery to generate the habitat, the red tide maps. We then put those into the ecosystem simulations, using the spatial-temporal framework, and, because we have these maps now, we're accounting for the spatial overlap of red tide with the species in the model.

We also -- Because the maps are running on a monthly time step, we're accounting for the bloom duration, and they are -- The values in those maps are cell concentrations, and so we have sort of a proxy for severity. We're accounting for the direct mortality, through forcing on the other mortality term, as well as the sub-

legal effects on the foraging capacity, which causes reductions in consumption, and therefore growth, but also drives movement away from red tide, and so we're capturing avoidance effects.

Because this is big food web model, we're also able to account for the food web effects, and so, if the forage base is impacted, that has an effect on the recovery scenarios following the red tide event.

The satellite imagery was based off of normalized fluorescence line height imagery, and this has been used to study and monitor algae blooms for a while now, and it was developed by Shuai Yu down at USF, and the fluorescence line height imagery is an indicator of algal blooms, but not necessarily red tide, and so that's why we have to sort of groundtruth it with the FWC samples.

This imagery is available beginning of July of 2002, and so all the red tide mortality estimates begin in 2002, and we don't have data yet going back before that, and so we haven't been able to piece together, pull together, maps to go before 2002, and we were able to update this through about mid-August of 2021.

There is a threshold cutoff, which is the 0.02, and those are in units of fluorescence and reflectance, and so we do have a cutoff value. In those maps there, you see the little thin red outline that would indicate where that 0.02 is, and, again, this is a good indicator of presence and extent, but not necessarily severity.

To get severity, we have to use the Karenia brevis cell concentrations from the routine monitoring and event response from the FWC HAB data, and these are data that are collected both at the surface and the bottom. They range from no red tide all the way up to really high levels of 162 million cells per liter.

One caveat is that those cell concentrations may not always be correlated with the toxicity, and so you can have high cell concentrations that may not kill a lot of fish, and you can have low cell concentrations that might kill more fish, but, in general, we tend to believe that higher concentrations are more severe and do kill more fish.

We take these red tide data and extrapolate them over the spatial grid, and so you can see here these two months on the bottom-right is September of 2005, and you have the satellite imagery in gray, with the red polygon. The red points are data that were collected with cells per liter greater than 50,000, and the blue points are data less than 50,000 cells per liter, and you can contrast that with the July 2014 event that was more isolated in the Big Bend

region off of Cedar Key.

When we extrapolate these data, there's a couple of different ways you can do it. The two ways that work the best for us are just the simple inverse distance weighting and ordinary kriging. We also tried some other kriging approaches, like including anisotropy and spatial-temporal kriging, but we didn't really have a lot of success with that.

I think, with the anisotropy, it's because of the nature in which the data are collected, and it's not really on a nice grid that would allow us to account for that non-symmetrical variance distribution, and, with the spatial-temporal kriging, there are some months where there was just no significant red tide samples, and so we can't actually string together a time series of months to do that, and so we tried both of those approaches, but they didn't really work out for us, and those were some suggestions that were made at one of the earlier data workshops for red grouper, I believe.

We take these krige maps, and then we then clip them to the polygons from the fluorescent line height imagery, and we get these two maps here on the bottom, which is what is going into the Ecospace model, and so this is kind of showing you going from the data that are collected here to the maps that are going into the model, and I'm just using the September of 2005 and July of 2014 as examples, and this is all done at a ten-minute resolution, which is about a twenty-kilometer grid cell.

Now we've built the maps for red tide, and now we need to incorporate some red tide response functions. The two response functions are a mortality response function and a foraging response function.

The mortality response function is a new feature that we had to add to the model. Previously, you couldn't manipulate that term, that other mortality term, in the model through a response function, and it was just a multiplier, and so now we can actually include these logistic curves that you see down here at the bottom.

What those represent is the proportion of biomass that is killed in each grid cell as a function of the concentrations of red tide in that grid cell. Now, we don't actually know what these response functions are. We have some idea here, and this is kind of the rule-of-thumb, that we believe that fish kills are possible around 10,000 to 100,000 cells per liter, and they get more likely as you go up from there.

Without knowing that, we had to evaluate the model over a range of different response functions for the mortality as well as the foraging. Now, the foraging response is also done with a logistic curve, but it's a declining slope, and so this would say that, as red tide cell concentrations are higher in a given cell, there is less foraging capacity in that cell, and so they will eat less and grow less, but they will also try to move away from that cell, and so, while this is a negative effect in the model, it actually mediates the biomass loss, because it creates this avoidance response, and so it actually played out quite nicely in the model, when you incorporate both of these response functions simultaneously.

I don't show this in the presentation, but we see fish tend to pile up on the sides, on the edges, of a red tide event, as they try to move out. If a red tide event is too big, then they will die, because they can't actually move away from it, and so it works as we were hoping.

As I mentioned, we don't know the actual -- What these response functions mean, and so we evaluated over a range of them, and it ended up being over 160 total runs that we did, specific for the gag grouper now, and we looked at a combination of sensitivity, high, medium, and low sensitivity, to red tide, applying the response functions only to gag, and also to all consumer groups, and applying only the mortality, or the mortality plus the foraging. We did this just to kind of fill out and try to bracket the uncertainty in what we know about those response functions.

One of the critical things that we've really been working on recently is trying to validate the red tide predictions. As I mentioned before, the Ecospace is not an estimation model. We aren't able to estimate any parameters within the model, which is why we were running so many trials. It's a simulation-only model, and so all the comparisons that you will see here are basically done after we've run the model and so we would run the model, compare it to output, and then we would try to thin out some of those runs that were not reasonable.

For example, the first thing we would look at is we could compare the Ecospace predicted red tide mortality in 2005 with the estimate from the stock assessment that was around 0.7, 0.77, or something like that, and we would assume some wiggle room there, and you can see here, on the right, that histogram shows sort of three different groups of runs.

Those on the left are where we had really low sensitive response functions, and those are the right were runs with very high

sensitive response functions, and those in the middle are ones that we believe are probably more reasonable, or at least better aligned, with the estimates coming out of the stock assessment.

The next thing we looked at was the predicted biomass trends with observed indices of abundance, and we thinned out some of those 160 runs based off of the root mean squared error and how those data fit, and I will show more of those plots in a minute, and then the last thing, which I think is really cool and is still sort of a work in progress, is trying to identify situations where we do have data inside or outside or before or after red tide events and compare the predicted response, or difference, between those two with the observed. So far, the run selection has only been based on Numbers 1 and 2, which is the point estimate and the trend.

This just shows the predicted red tide to predicted biomass, vulnerable biomass, corrected for selectivity, to each of the observed indices of abundance coming out of the stock assessment. The yellow line is the SEDAR 72 prediction, and the black line is the Ecospace prediction without any red tide effects. The red line includes only mortality from Ecospace, and the green line includes both mortality and foraging.

You can see how, especially the difference between the black line and the red and the green lines, but incorporating red tide effects in the model tend to capture some more of those dynamics, and I show this to kind of -- Not as much as a comparison with the observed data, but to show that the Ecospace model is generating patterns that are fairly consistent with the stock assessment model, at least over a longer term, and so that gives us some comfort that the model is performing reasonably well and capturing some of the dynamics of this species.

Here is an example of where we've looked at some before and after comparisons, and so we poured through all the different survey data and lined up the samples with the red tide sampling, to try to identify situations where we may have before-and-after comparisons, or inside outside, and one case that we found was in the 2005 bloom.

The FWC baitfish trawl survey, which is no longer in action, but it used to sample in the spring and the fall, and so they had done their spring sample in April of 2005, and this was off the area of Tampa Bay down to Charlotte Harbor, and then they sampled again in November of 2005.

Now, the bloom was still somewhat active in November, but, by that

time, it had mostly dissipated, and so we have what is roughly a before and after comparison that we could look at, and we see here that the difference in the survey data was about a 50 percent decline in biomass from April to November.

However, the Ecospace model predicted a much stronger decline in biomass in this region, and so there's potential here that Ecospace might be overestimating red tide impacts for this event. Now, this is only looking at total fish biomass, and so we can break down this down a little bit further by species, going forward, and so this was one attempt.

Another case where we've tried to do this type of validation was looking at the 2014 event that occurred in the Big Bend region, and this was also based off of this publication by Driggers et al. that really described the effects of this on their catch in the bottom longline survey, and so, here, we see that the bottom longline survey estimated a larger decline, and so about a 75 percent decline, in fish abundance compared to the model, and so, in this case, the Ecospace model might be potentially underestimating the red tide impacts for 2014.

Now, this is something that we're continuing to work on and trying to kind of fine-tune this a little bit, but there's only a handful of cases where we can do this type of before and after and inside and outside comparison, and there's all the caveats that go with that. None of these data were collected in a BACI-type design or with controlled sites, and so you do have to kind of take it a little bit with a grain of salt, but we're looking for all the information we can to try to validate this model.

Now I will show you how those simulations sort of play out, and so I have three cases. I have 2005, and this is -- The top row are the red tide blooms, and the bottom row shows some biomass distributions and dynamics. This will just loop through each year, and so you'll see 2014 cycle through, and 2018, but, just so you get an idea of kind of how the model is working, you get a big red tide event, and you see the biomass goes down in that affected area.

2005 was probably the most extreme and easiest to notice, but, if you watch these sort of play out over time, it's hopefully just to give you an idea of how the dynamics work. Now I will break this down in look at it in snapshots, because it's a little bit easier to digest it, but the 2005 red tide was already present in January of that year, just south of Tampa Bay, off of Sarasota County.

It persisted throughout most of the year. The spatial extent and

severity peaked in September, and there was pretty broad coverage offshore, and still the center of this event was just south of Tampa Bay, but there were -- As you saw in that previous map, there were high concentration samples collected further north and offshore, and this particular event resulted in the highest mortality for most of the species and age groups in the model.

The 2014 event was present in the Big Bend region during July, and so it sort of popped up towards the end of June, and persisted through July, but then it quickly dissipated, and, by August, it was pretty much gone, and so the concern at that time was that this bloom was going to expand further south and become more severe, like a 2005 event, but it didn't actually happen.

Now, this bloom was offshore, and so what's interesting about the 2014 event is that the model predicts a less severe mortality than in 2005 on the juveniles, whereas, in most cases, when these red tide events occur along the shore, you get higher impacts on juveniles.

Then the 2018 event, which was another severe event, again in southwest Florida in July, and it then lasted all the way through November, and so it was a pretty long and severe bloom. It did remain closer to shore, although it moved north a little bit, going from -- Especially later in the year, in September and October, but it was limited to nearshore, and the impacts were highest on the younger ages, because of that.

Now, looking at these, at what red tide has done over the last couple of years, which is -- These are the years that would be in the projections for gag, and so we were able to update these red tide maps and the environmental drivers through mid-August, and so I did this -- When we last ran the model, it was around the first of September, and so, at that point in time, we were able to get the data about two weeks prior, and so there's about a two-week lag in data availability, and that's mainly the satellite and the FWC data that are about two weeks out.

I will mention, just for transparency, in going from the analysis that we presented in the SEDAR working paper to the run that we updated, we did change a little bit in the Chlorophyll-a spatial-temporal layer, a different equation for how we integrate the surface Chlorophyll over the euphotic depth, and it didn't really change much at all with the red tide analysis, but it improved some of the stability of the species in the offshore environment.

In 2019 to 2020, there were relatively small red tide blooms. There was a bloom that popped up toward the end of 2019, and it

then went away, and there was nothing in 2020 until December, and so 2019 and 2020 were fairly quiet years for red tide.

Now, 2021 is a different story. You can see this bloom that was already presented in January, and it persisted all the way through April, and then, in May and June and July, the red tide became quite severe in the Tampa Bay area, as many of you are aware, and it has persisted there through August, and even into current weeks, and so this is the current sampling, looking at the first two weeks of September.

Our data that we'll run -- The model runs that we'll show today had data through mid-August, and I'm just showing you this to show that the current sampling still indicates that this bloom is present. It's a little bit more patchy, but there is still a red tide bloom occurring as we speak.

All right, and so we put all that together, and we run the model, and we get these estimates of red tide mortality for gag, and we also get them for all the other species, but I will focus on gag, and so you see here, by age, the highest red tide mortality was in 2005, and other extreme years include 2006, 2018, 2012, and 2015 and 2016, but it kind of depends on what age stanza you're looking at.

We typically see high red tide mortality for the younger ages, because of their spatial overlap in the nearshore environment, and the oldest age stanzas, the age-four, and mainly age-fives, the worst event was in 2005, and that is about the only detectable red tide event, although we also see a little uptick in 2018 for those older stanzas.

As I mentioned before, there was low mortality in 2019 and 2020, but the model is estimating an increase in red tide mortality for 2021, and we expect that increase to be higher, if we were to update the model and allow this bloom to persist through September, through August and September.

Some of the I think more interesting results came from our attempts to integrate this into the stock assessment models, and so we've been working with Lisa to do some sensitivity runs with the SEDAR 72 assessment, and she'll mention these in a little bit more detail in her talk, but one of the ones that I think showed the most promise was incorporating those red tide mortality estimates that I showed you on the previous slide into age-specific natural mortality deviations within the SEDAR 72 stock assessment model.

When this was done, it overall resulted in better fits to the

indices, as measured by the root mean square error, and especially recruitment, and so you can see the red line is the SEDAR 72 sensitivity run with red tide, the blue line is the base run, and the dots are the observed data. If you look carefully at the age-zero index, we're capturing a lot more of the variability and really fitting those data much better when red tide are included as a mortality deviation.

I think what this is doing is it's helping resolve -- Currently, the stock assessment models really only have recruitment deviations to put at this, and those are probably informed more by the composition data and less by the trends, and, when we also add this other process in, as far as changing mortality rates, it seems to improve the fits to the indices, and you see that with the age-zero survey as well as the charter boat and private survey as well.

That just suggests that our outputs from the ecosystem model are consistent with the other data streams going into the stock assessment. However, there is still an issue when we did this, and you'll see, later in the day, that it resulted in this really high peak of landings in 2014, and so, basically, the stock assessment model was wanting to put some more mortality around the 2014 event, and so we still have to resolve that, but I would say that it's a promising result here and that we are able to improve the fits, at least to the trends, and I think that it will take a little bit of simulation work and some more digging to get at the reason for that peak in landings.

Overall, one of the things that I learned from all this work is that red tides are potentially a bigger driver of population change for gag, at least than I previously thought, and I'm basing this off of just the dynamics in the model. If you compare the Ecospace model to the stock assessment model, where the stock assessment is mainly driven by fishing and recruitment deviations, and Ecospace is driven by fishing and environmental effects and red tide, we tend to get some fairly consistent dynamics.

What may be interpreted as recruitment dynamics by the stock assessment model could also be explained as environmental or red tide effects by the Ecospace model.

Another important finding, I think, was this response in recruitment, and so, here, you see the red line is the Ecospace model prediction with red tide, and this is the model runs that only -- That had the red tide responses applied to the entire food web, and so there has been anecdotal evidence that, after a red tide event occurs, we get high recruitment in groupers, and there has not been much of an explanation for that.

The model predicted that, and this is an emergent property of the model, and we believe this is a trophic driven compensatory response, and so, after a red tide occurs, there is less predators and/or less competitors in the system, and so you have the ability for a stronger year class to come in following the red tide event.

Now, we aren't able actually pin that down yet, as far as which predator or which competitor is the culprit there, because the output generated by the Ecospace model doesn't yet provide all the species-specific predation mortality rates, and that's something we can get out of the model, working with the programming team, and so I can't say any more than we believe this is a trophic-driven response, or at least it is coming out of this model.

Now, there are a lot of uncertainties and limitations with the Ecospace model, or ecosystem models in general. I think some of the relevant uncertainties pertaining to this analysis have to do first with generating the red tide maps. The extrapolation of those HAB data is necessary, but it does introduce a lot of error, and so we have -- There is different ways to try to account for that, by doing different kriging methods, but it is a source of uncertainty.

The habitat maps, and so our species distributions that come out of the model are only as good as the habitat maps that are informing the model, and we're still relying on probably some outdated benthic habitat hardbottom maps, rugosity maps, and those could be improved as more of these mapping products come online.

As I mentioned before, the red tide response functions, we don't really know at what level of sub-concentrations that fish die, and we have some idea, and, also, we don't know how that varies across taxa. In this case, we assume that all fish -- In those runs, every group in the model was assigned the same response function, but, in reality, some are more sensitive than others.

The dispersal rates, as far as good versus bad habitat, that is something that will be very difficult to get tagging studies. We don't generally get that fine of a level of detail, but understanding more about dispersal would help improve the model.

The fishing effort especially, the spatial effort, is unknown for recreational fleets, and so that would have an effect if there was a lot of harvest taken from the area right before a red tide occurred in that area, and then, of course, diet composition is always a source of uncertainty.

Some of the limitations with the model include the timeframe where we have data, and we can only generate these red tide maps back to 2002. As I have mentioned a couple of times, Ecospace is a simulation-only model, and we are not estimating parameters. Another limitation is just the time scale of all these dynamics. The model runs on a monthly time step, but, as we know, red tides do occur on a daily or weekly-dynamic event.

We attempted to introduce the hypoxic condition brought about by red tide into the model, but it was difficult, because of trying to piece together sort of the patchy data that we have on dissolved oxygen, but that is a component that is not yet included.

Some of what we're working on now primarily is trying to improve model fit to data, and so we are working on a likelihood function that can incorporate either trends in abundance, map-to-map comparisons, as well as those empirically-observed effects, like before and after, and so trying to really validate the model and improve the parameters.

We now have the ability to run the model in parallel, on a big workstation that I have in my office, and so what used to take twelve hours can now be done in about thirty minutes, and so we can now literally do thousands of runs per day, and this is exactly where we're at, and we're just evaluating all of these parameter combinations and trying to find configurations that work best with the model.

Then, of course, how do we incorporate that into Stock Synthesis, and I think that that's going to be probably an ongoing topic, at least for the next couple of years.

Now, operationalizing the model -- What I mean by "operationalizing" is basically being able to turn the crank and deliver these outputs as needed on a regular basis. As I mentioned, we do have the capacity for more timely updates, and so we can provide information at about a two-week lag, and that would be done just by updating the red tide maps and the environmental drivers, but longer-term maintenance of the model should probably include periodic sort of benchmark-type updates, where we would incorporate new data streams and update the model to be consistent with all the stock assessments that have occurred.

I think probably the last thing for the SSC to think about is that, if this information is going to be provided and included more regularly into stock assessments, then is a formal review necessary, and do we need the determination of best scientific information available for that routine use in SEDAR assessments?

I am not sure what that might look like.

A few research recommendations include just more comprehensive sampling around red tide events, everything from the HABs all the way up to fish, invertebrates, benthic invertebrates, and just plankton. Tagging studies to understand mortality and movement, and some experimental studies to understand red tide tolerance and thresholds across multiple taxa.

Simulation studies on how to incorporate into the Stock Synthesis models, new algorithms to estimate cell concentrations from satellite imagery indirectly, which would eliminate the need to do the extrapolation, and then some lab studies to understand more about red tide physiology, ecophysiology, and how they relate to their environment and food toxicity.

I would like to thank all of those that have been involved in this work, as well as the NOAA Restore science program, and it's definitely been a team effort that we've been working on for a few years now, and I will leave this slide up for any questions, since this also contains the maps of more recent samples collected by FWC, as well as the red tide time series analysis. Thank you.

CHAIRMAN NANCE: Dave, thank you very much for that excellent presentation. Are there questions or comments? Dave.

DR. GRIFFITH: Thanks a lot for that presentation. It was really interesting. I am curious, and so the -- Evidentially the older gag -- One of their responses is to migrate out of the region and into some other area, and so I am wondering -- Well, a few things.

One is there is this wonderful food web that you have on I think it's Slide 11, and so have you noticed, or would you think there would be changes to the food web following those kinds of migrations? You mentioned, also, that there would be less competition and less predation, or there might be less predation and less competition, following a red tide event, and so I'm wondering -- Does that affect the food web somehow, or have you been able to check that out or model it in any way? Thanks.

DR. CHAGARIS: Thank you for that question. Yes, it definitely impacts the food web, and so there is -- In each grid cell of the model, it's got its own little food web, and so, if you drill down into the impacted areas, you do see drastic changes across multiple trophic levels.

Daniel has been looking at some of that and trying to understand how the trophic level and some of the food web metrics change in

the impacted areas, and so that's something that he's working into his dissertation, but, yes, there is definitely food web effects, because we're impacting the entire food web with red tide.

Now, as far as the migration goes, as fish move around over the spatial grid, they're going to eat what is present in those grid cells, and so, if they're moving offshore, they're going to eat prey items that are available to them offshore, and you do have sub-food webs, spatially.

CHAIRMAN NANCE: Any other questions? Paul.

DR. MICKLE: Thank you. I enjoyed the presentation. I have two brief -- A question and a statement. First, Ecospace, just a question, and I'm not terribly familiar with Ecospace itself, but does variability -- As far as their response function, does it increase or decrease with depth or spatial -- Is there a variability trend that you see, as far as the outputs and response function, that maybe you could look into further to try to understand, maybe for calibration purposes and validation purposes and things like that? Thank you.

DR. CHAGARIS: I don't -- We wouldn't be able to incorporate different responses spatially. Now, for like gag, we have ages-zero through five, and so we could have a -- Those are -- They have different spatial distributions, and so each of those ages could, in theory, have a different response function, and different taxa could have different response functions.

That's something we're in the process of evaluating now, as far as are there combinations of different response functions, with some species being more or less sensitive than others, that would result in better -- More consistency with the observed data, but the response functions themselves are prescribed by the user.

DR. MICKLE: Okay. Thanks, Dave. That gives a little bit more clarity to what I was thinking about, and my last is just a statement. This potential method, as far as Ecospace and the capabilities of what it can be used for, and you presented here today, with kind of the target with Stock Synthesis, is terrific, and it's very noble, and I think we're all excited about it.

Also, when I'm sitting here and thinking about some of the things that are going on in the central Gulf, I can potentially see this as a method for quantifying loss by correlative influence by certain things, and not only red tide, but other things that could potentially come out. Some litigators may look at this as a potential tool, in the future, for their endeavors, and so with

everything we create comes certain things that come out of it, and that's all I will say, and I will leave it there.

CHAIRMAN NANCE: Thank you. Luiz.

DR. BARBIERI: Thank you, Mr. Chairman. Dave, thank you. That was an excellent presentation, and I really liked the way that you structured this, to walk us through the whole process and explain the different components, make it easier on us to understand, and my question is really to help me -- To get your input, your perspective, and it's to get your perspective on this issue of how much of a red tide is enough for us to really consider that to be a population-level impact.

I bring this up in a sense of looking at the indices of abundance that go into the assessment, and so assessments, in general, already, by design, they integrate indices of abundance, so we can actually look at those trends in abundance and how they are influencing the stock.

I think that something like your kind of work would basically help us understand, when you have those decreases in abundance that show up already in the indices, first, to understand that those are due to some ecosystem-level impact. In this case, it would be red tide, and so we can actually have explanatory information there why the abundance is being reduced in those situations.

Often, we see situations where we have, and you can imagine that I have been getting this question a lot for southwest Florida, the Charlotte Harbor area, that we get a lot of red tide events where you see a lot of dead fish floating, and fishermen call, anglers, the public, call concerned, and especially that big event in Tampa Bay, because that was in everybody's backyard, and it was very visible, and people called.

Then, when we look at our fishery-independent monitoring program indices of abundance for the main stocks, we don't see any decrease in abundance and impact that we could attribute to red tide, or anything else, for that matter. Can you help us understand a little bit, and how do we get to that point of understanding the impacts of red tide that perhaps are not being shown in the indices of abundance?

DR. CHAGARIS: That's a great point, Luiz, and my take on that is that, first of all, our fisheries-independent monitoring programs were not designed for this type of event response, and especially an event like red tide, that can be very isolated in space and have a short duration, while it might still have impacts on the

stock.

Most of the sampling that takes place is either not -- Like, if you take say the SEAMAP trawl survey, there might be one or two samples that are collected in an area impacted by red tide, and the same would go with the FWC camera data, and so they just aren't designed to track the effects of red tide, and so, if you're sampling it earlier in the year, before the red tide occurs, and then you sample again a year later, unless it was a really severe event, you are likely to -- There could already be the compensatory response, and so you don't actually detect the decline, because it happened in between your sampling events.

I think a lot of that has to do with the design of the surveys that we're using, and the other issue there too is that we tend to focus a lot on just the extreme red tides that cause a lot of fish kills and create a lot of media attention, but what this analysis shows is that there are sort of smaller red tide events that might not be as apparent or as talked about, and they might not have a lot of fish wash up on the beach, and so you might get less calls on that, depending on the currents and things, and so the approach that we've used in previous assessments of trying to pick out one, two, or three years where red tide occurred, I think it could be inadequate, because there are lower levels of red tide that are probably especially affecting the juvenile age stanzas, which we don't have especially good monitoring of those as well.

I think it's combination of what we believe to be an extreme red tide event, based off of what we observe standing on the beach, or from calls from fishermen, versus what is actually happening and our ability to sample around the red tide event. That third question, as far as what would be an adequate survey design to track impacts of an event like a red tide, I think that would be a neat research question.

DR. BARBIERI: Yes. excellent. Thank you, Dave. That was very helpful. I appreciate it.

CHAIRMAN NANCE: Thank you. Mandy.

DR. KARNAUSKAS: Thanks, Mr. Chair. Great work, Dave. As the others have said, this is a really nice presentation, and thank you for walking through all the excellent work that you guys have done.

One question I have, and it's something that I hear a lot, and it's a concern I have, is these inshore events seem to be sort of increasing in frequency, and so the overall ecosystem impact of

the forage loss that probably occurs with these inshore events, and I wonder if there's sort of a lag response on the overall managed species that could be occurring with the red tides, and I'm just curious if your modeling could inform or provide insights on the question at all.

DR. CHAGARIS: Yes, and I should have included a figure that shows this, but one of the reasons we applied red tide only to gag and to the entire food web was to try to tease that apart, and we absolutely see a faster -- I will say that we see a slower recovery for gag when the entire food web is impacted.

DR. KARNAUSKAS: Thanks. That's really interesting.

CHAIRMAN NANCE: Thank you. Will.

DR. PATTERSON: Thanks, Jim. Great job, Dave. It's really cool to see the evolution of this modeling work, and, obviously, we've used this in the SSC, in different circumstances, with West Florida Shelf reef fishes, but to see it continue to evolve and develop is really great.

One question I had is I didn't really understand, back on Slide 20, when you were talking about the thinning process, and you had, or have -- There is a figure there where you have, it looks like, three different parts of the distribution that are broken in a couple of different places, and you focused on the center part of the distribution, because it matched -- The Ecospace predictions matched the year-specific mortality estimates coming from the stock assessment.

I didn't really understand what you were saying about thinning and how -- The idea here is, if you're going to use this tool to potentially predict the severity, or impacts, of red tide, are those dynamics that mis-estimated, or at least estimated a different mortality, than the assessment for the runs that you're talking about here on Slide 20?

Are those dynamics, or processes, still in the model, and so then, moving forward, although you have discounted them here, this output here on either end of this distribution, or the upper and lower distributions, how does that affect the ability to try to estimate what the potential impacts could be in the future?

DR. CHAGARIS: First, let me try to clarify the thinning process a little bit. We have all these different response curves here, and we have set of high sensitivity, those in red, versus low sensitivity, those in blue. We didn't know what the response curve

-- What the appropriate response curves could be, and we evaluated over a broad range of those, and it ended up being over 160 different runs.

Now, some of those, say for example the ones that had really low sensitivity, either did not match, didn't line up with what the stock assessment was predicting for the 2005 event, and those are the ones on the left of this histogram here with lower mortality rates, and those that had really high sensitivity to red tide had a much higher mortality rate.

Some of those also led to unstable dynamics, and so things crashing out of the model, and things going haywire, because we were really trying to push the -- You know, bracket that uncertainty and the responses, and so, after we did those 160 runs, we thinned out those that either didn't demonstrate a strong enough response to red tide, and that was consistent with the assessment, or those that caused the model to completely crash and go unstable.

Out of those 160 runs, we had a subset that was -- I want to say it was around ninety-four runs, and that is what we generated the median mortality estimate and uncertainty bounds from, and then we also compared to the time series, to make sure that, if it was flatlining, or if the group was crashing, then we would know that that was not a reasonable response function setting.

Moving forward, and making future predictions, I think we now have a smaller range of response functions, and so we could limit the model runs to just include those response functions that we think are reasonable and generate consistency with the observed data and start to kind of narrow-in on what is a better set of response functions. Does that answer your question?

DR. PATTERSON: Yes, definitely. I missed that important part about you were simply looking at the response functions, to figure out what was most plausible, given the assessment.

DR. CHAGARIS: Right.

CHAIRMAN NANCE: Thank you. We've got Steven and then Sean and then Jason. I'm going to cut it off after Jason, so we can continue on with our agenda, but Steven.

DR. SCYPHERS: Thank you, Mr. Chair. Dave, thanks for a really, really great presentation. I am curious about one of your notes on uncertainty and recreational fleet effort, and it's kind of a two-part question.

First, I was curious, and what is it currently set up like? What's the assumption on where that effort is taking place? Then what about temporally? Does this vary in time sequence with the model at the monthly intervals? Then kind of a related question is, based on that, on your comment about uncertainty, what would the best -- What would be the best solution to providing that data? Would you need it within the same grid structure that the model currently operates, at say one-month intervals, or -- I just kind of wanted to know your thoughts on that. Thank you.

DR. CHAGARIS: Currently, the effort dynamics are modeled using basically a gravity model approach, and so there's a sailing-related cost, which creates basically the profitability of fishing in a given grid cell, and so, given the biomass of the target species in that cell, the value of those species in that cell, and those sailing-related costs of that cell.

Now, with the commercial fisheries, we have things like VMS data that we can compare, and we can tune some of the parameters to better fit the known effort distributions, but we don't have those data for the recreational fleets, and so, right now, they're just leaving from shore, and they're going out, and, basically, the effort is concentrated closer to shore, and, as you get further away from shore, there is less fishing effort, and it will be concentrated in cells where fish biomass is higher.

As far as the incorporating recreational effort data into the model, I think it's quite flexible in how we can do it. Ideally, we would have a map for every month of effort distribution, and that can be at -- It would need to be at the same grid cell, but it doesn't necessarily have to be monthly, and it could be bi-monthly, or it could be a map for each year, and so there is flexibility in how we might do that.

Also, just qualitatively looking at the maps and knowing where the effort is occurring, and we could tune the model to that, and so there's flexibility in how we might do that. Right now, we're just sort of letting that gravity model approach play out without any direct tuning to correct it one way or another.

DR. SCYPHERS: Thank you. One quick follow-up, if I may.

CHAIRMAN NANCE: Go ahead.

DR. SCYPHERS: Is that fixed through time? Does the effort vary through time with any kind of index, like the Florida Reef Fish Survey or anything like that, or is it static?

DR. CHAGARIS: Yes, it varies through time.

DR. SCYPHERS: It varies through time, and so, if you had a three-month red tide that was quite severe, it would pick up if there were decreases on effort through that period of time?

DR. CHAGARIS: The way we typically do it is we provide the time series of effort, and then the model will distribute that relative effort spatially. I believe there is an effort response model that could be turned on, and I know there's one in Ecosim, and so the effort dynamics -- Effort would respond to changes in abundance, but I don't know if that's available in Ecospace. I would have to check on that, but, typically, we would provide a time series of effort, and that would control sort of the global magnitude of effort, and it would be distributed spatially according to the model.

DR. SCYPHERS: All right. Great stuff. Thank you very much.

CHAIRMAN NANCE: Thank you. Sean.

DR. POWERS: Thank you. Great presentation, Dave. I learned a lot. I had two questions, and I am going to do one at a time. On the timing issue, I was really impressed that you can turn this around so quickly, when you said you have included, in what you showed us today, August data. That's really impressive. Does that lag -- Is there an additional lag from when the measurements are taken and FWC can process them in the lab? I was wondering about that, but are they pretty quick, or is there more of a lag, because they have to actually do the cell counts?

DR. CHAGARIS: I think FWC is pretty quick about it. I mean, their data are usually available within a week, and so there's a pretty quick turnaround in the samples. I think the -- When we updated this, we had FWC data through I think it was August 16, but the satellite data were only available through August 9, and so I think the --

Now, obviously, satellite data could be brought in in real time, working with the ocean optics person or something, but we were pulling them from a server and waiting for the eight-day composites to come out, and so, yes, I think there's pretty quick turnaround with FWC, and I don't know that they could get those samples out any faster. Two weeks is probably about as good as we could do.

DR. POWERS: That's awesome. It's excellent. This one is a little broader, and so, historically, isn't the mortality already in the model, since it will show up eventually in landings and/or

recruitment, and so I guess the value of this is trying to explain what portion of that mortality is due to red tide?

DR. CHAGARIS: Yes, and this is -- The main thing that these ecosystem models do is partition the mortality, and so it's partitioning it to fishing predation and other mortality and now red tide.

DR. POWERS: So we would then understand, or have the ability, to better predict -- If we have a red tide event of some severity, we could use these results to predict what the bump in natural mortality would be, and I guess that's the -- Because, historically, it's already in there, because of the landings data.

DR. CHAGARIS: I mean, that's exactly right. This model can be used to predict an increase in natural mortality, and we have separated this mortality term out as it's attributed only to the loss from red tide, and so that's why, in the assessment model, it was put in as an additive term in those deviations.

DR. POWERS: Okay. Finally, a comment on SEDAR and how this fits in, and I would think that, and it's just my thoughts, that, if we had a benchmark, or I guess a research now, clearly a data workshop could look at it, along with other indices and things like that, but, if we want to include it quicker, in an operational type of framework, the analysts can decide, and then, ultimately, the SSC will evaluate that, when they accept the assessment, or we could be more proactive and endorse this, so that the analysts feel that they can include it, but I appreciate you bringing it up, because it does look like you're very, very close to, obviously, including it in the assessment framework.

CHAIRMAN NANCE: Thank you. I agree with that, Sean. Jason.

MR. ADRIANCE: Thank you, Mr. Chair. Since you were cutting it off after me, asked and answered, and so I will let us continue.

CHAIRMAN NANCE: Okay. Thank you. We're going to -- Dave, thank you so much for that presentation. It was excellent.

DR. CHAGARIS: You're welcome. Thank you.

CHAIRMAN NANCE: We're going to go ahead and move on to our next agenda item, Review of SEDAR 72. Just a reminder that we need to determine whether the SEDAR 72 assessment represents the best scientific information available, and, if so, inform the council of the stock status of gag based on that, on the status determination criteria.

We also need to determine whether the assessment is suitable for management, and, if so, recommend values for OFL and ABC. That's just a reminder that that's what we're going to be doing here with gag grouper this morning and this afternoon. With that, Lisa, are you ready to start the presentation?

DR. LISA AILLOUD: Yes.

CHAIRMAN NANCE: Thank you for being here.

REVIEW OF SEDAR 72: GULF OF MEXICO GAG STOCK ASSESSMENT REPORT PRESENTATION

DR. AILLOUD: Good morning, everyone. My name is Lisa Ailloud, and I was the lead analyst for SEDAR 72, and so, today, I will walk you through the SEDAR 72 process. I will emphasize that, since this was an update assessment, an operational assessment, I will emphasize the changes from the past assessment, which was the SEDAR 33 update.

I will go over the model development, the fits, the results, and the diagnostics. I will also describe some of the important sensitivity runs that we looked into, and I will draw conclusions and then probably spend a fair amount of time on decisions related to benchmarks, stock status, and projections, since we did run some initial projections, but there is probably going to be some modification, following the discussions we had yesterday. Then, finally, I will go over some research recommendations that came out of this assessment and that would be very useful for the next research track assessment.

Starting with the overview, just a quick reminder from the terms of reference that there were four main things that needed to be looked into. The first related to the way that red tide was defined in addressing the assessment. The second related to the changes in the recreational fisheries, in terms of the update in the MRIP data, and so using the fully-calibrated time series for the recreational fleets.

The third one had to do with the way retention and selectivity was described for the recreational fleets, and the last one relates to the improvement that the Southeast Fisheries Science Center has done with regard to estimating commercial discards, which is something that has been used in other stock assessments already and has been accepted, but hadn't been adopted in gag yet, and so we addressed it here.

There were a number of changes that occurred in SEDAR 72 compared to the last assessment, and I list them here. The first four are directly addressing the terms of reference, and then we also had new information on maturity and hermaphrodism, due to updated data up to 2019, and so this was included. We changed the way the indices worked for the recreational fleets, and there were two separate indices for the private fleet and the charter fleet, and those were combined in SEDAR 72.

We had additional age-length pairs, and so we were able to re-estimate growth and also update variances. There was an update to the methodology used by the Southeast Region Headboat Survey to estimate discards that we adopted here, and then there was a black/gag grouper update for the correction factors earlier in the time series of commercial landings and discards.

Then, lastly, we had a proposed combined video survey index to replace the separate SEAMAP and Panama City video surveys that were used in the past, and we had a lot of really constructive discussions around this new index, which I will go into detail later in the talk, but, ultimately, we decided to keep it as a sensitivity run, due to some difficulties that I will go over later.

Ultimately, the stock was determined to be overfished and undergoing overfishing, both using female-only SSB or SSB combined, and, again, those are topics that came up in the past of how to define spawning stock biomass when we're looking at benchmarks, and I will dive much deeper into detail in those topics later on, when we get to the benchmarks.

Let's go over an overview of the data. The overall structure of the assessment did not change all that much from the last assessment. Obviously, we have additional years, and so the terminal year was 2019, instead of 2015 in the last assessment, and the fleets were separated the same way as in the SEDAR 33 update, and so we had one commercial vertical line fleet that represents both vertical line and other gears, and we have a longline commercial fleet. We have three recreational fleets: the headboat fleet, the charter boat fleet, and then a private shore, and so it's private plus shore catches and discards.

Then we also have this red tide bycatch fleet, which Dave described, and so it's not actually a fleet, and that's just the way it's modeled, but it's just to account for mortality that we can assign to red tide, rather than assigning it to an actual fishing fleet, and then, in terms of abundance indices, we had -- The vertical line has a fishery-dependent CPUE index. The

longline, the headboat, and then, instead of having both separate charter boat and private indices, like we had in the past, they were combined into a single index.

Then the fishery-independent indices, and that was unchanged from the last assessment, and so we had three separate indices. We had the SEAMAP video index, the Panama City video index, and then the age-zero combined index, which is linked to the recruitment in the model.

On the right-hand side, you see the size of the bubbles describe the relative amount of data available within each category, and so you can see here that, in terms of catches, the primary catches are coming from the recreational private inshore fleet, but, in contrast, if you look at the available length and age composition, we actually have relatively less information coming from that primary fleet than we would, for example, coming from the commercial fleets, and so it gives you a little bit of an idea of where we have some data deficiencies and where are the data-rich periods of the assessment.

The life history data, some things were unchanged from the last assessment, and that includes the length-weight conversion used internally to SS, as well as the length equation for converting total length to fork length, and that's used actually externally to the assessment, for things like minimum size limits, and those were unchanged.

For age and growth, like I mentioned, we did have additional age-length pairs, and we actually had 31,000, approximately 32,000, in the last assessment, and now we have almost 54,000 age-length pairs, and so a really nice improvement that allowed us to re-estimate the growth curve, and you can see, on the right-hand side, the difference between the internally-estimated growth curve of the last assessment in blue and then the current assessment, SEDAR 72, in red.

We also were able to re-estimate the variability about the growth curve, using those age-one pairs, but that was done externally to SS and then input as fixed parameters, and the reason is, when we tried to estimate the variability internally to SS, it gave some non-logical results, which were showing very little variability in the large ages, which was not what we were seeing outside of the assessment, and so, with expert advice, we decided to just fix it in the assessment.

Then the natural mortality, and, because we had additional age data, we were able to reevaluate what the maximum age was observed

in the population, and it was increased from thirty-one to thirty-three.

Because our natural mortality vector is based on a regression method that requires an estimate of the maximum age, the group decided to -- The panel decided to adopt this revised maximum age of thirty-three, and they also decided to change the actual regression method used for getting the natural mortality vector, and so, whereas we originally had used the Hoenig et al. method for the last assessment, since then, there's been what is considered an improved estimator, in the sense that it is based on a larger dataset, and so it's a similar approach, but just based on a larger dataset, and, after discussions, it was decided to re-estimate a target mortality using the Then et al. method and using serranid-only data.

The entire vector is scaled using a Lorenzen scaling, which was used last time, but we just updated the growth curve that was used for scaling, by basing it on the growth curve that was internally estimated in the last assessment.

For the natural mortality, there was interest in seeing how that alone affected the results of the update assessment, and so you will see later that we did have a sensitivity run, with the only modification being that we replaced the natural mortality vector with the old SEDAR 33 update vector. On the right-hand side, you can see the differences between those mortality vectors, and so there is a slight shift upward to higher levels of M across all ages.

The maturity, we were lucky enough to get some updated information on maturity, with samples, and it was based on 881 samples collected from 1991 to 2019, and it predicted a 50 percent maturity around 3.9 years, which you can see on the top graph is a slight increase compared to the SEDAR 33 update.

In terms of hermaphrodism, since gag transition from female to male, we, in SS, include a transition function, and this transition function -- On the bottom-right plot, you can see it's actually the dotted line, and this is what is parameterized in SS, but I think the solid lines are a little more intuitive to us all, which is the fraction females at-age at equilibrium, and so that was estimated to be about 50 percent female around 11.6 years, and that was a bit of a change from the last assessment, which estimated it at around ten-and-a-half years of age, but it was based on an increased sample size, again, and due to the best information available.

The fecundity was set to be equivalent to spawning stock biomass, the same as in the last assessment, but there was a debate over whether we should describe it as female-only spawning biomass, which is usually done for a species, but may not be preferable for hermaphrodites, since a decrease in the male spawning stock could have negative effects on the overall fecundity or capability of the stock to reproduce.

There was an alternative run done using combined male and female spawning stock biomass, to define fecundity, and this was considered as a sensitivity run, and I will go into detail later on that, and I also did calculate benchmarks and reference points for both of these alternative scenarios, and that was a term of reference.

Now, the recreational landings, as mentioned previously, we now have the fully-calibrated MRIP estimates available, and so the FES estimates, which were quite different from what was used in the last assessment, and so a few changes here that I am going to walk you through, because there was a few changes made as we were going through the data processing.

If you look at the graphs below, the solid line shows you the landings data from the last assessment, and then the dotted line shows you the data that was handed to us for the recreational landings, and there was a few things that needed alteration, and so the first is there was a noticeable peak, if you look on the -- - There was a noticeable peak here in 1983, in the private fleet, and it was -- It felt really beyond the range of the rest of the data, and it was linked back to being driven largely by a single intercept survey of a single angler trip, with four contributors, to a group catch that harvested thirty-six black groupers.

That, because of the species misidentification from black and gag grouper early on in the time series, those were translated to gag grouper, and this single intercept had a lot of weight in determining the estimate for that one year, and so the panel felt uncomfortable with this peak, and they didn’t think that it was a very realistic one, and it had a lot of uncertainty, and it was impactful, because the historical time series, which is from 1963 to 1980, and that’s a time period where we actually don’t have MRIP data, but we have to hindcast the historical time series, that historical time series is scaled to the average CPUE from 1981 to 1985.

That peak fell in that timeframe that was used as a calibration for the historical time series, and so it had a lot of weight, and, ultimately, the decision was to replace that peak with the

geometric mean of 1981, 1982, 1984, and 1985 and then replace that value and replace it in the private and shore fleet time series. It resulted in a 78 percent decrease in that single point estimate of landings for that year and mode.

Now, the second issue that we noticed is that, even when we adjusted that peak, it adjusted the historical time series, but then we noticed that the way the historical time series is split up between fleets is that we're provided with an overall historical time series of recreational catches, and so it includes all states combined, and we have to partition it.

The way it had been done in SEDAR 33 and the SEDAR 33 update was to calculate the relative ratio of catches between each of the three fleets from the period 1981 to 2012 and look at what was the average ratio of the various fleets, and, at the time, it was 20 percent charter, 5 percent headboat, and 75 percent private.

When we introduced the newly fully-calibrated MRIP time series, you see that your private fleet estimated landings are actually much higher, and so the contribution of the private fleet was also higher, and so, therefore, there was a need to recalculate those ratios so that we didn't have that issue where the headboat and the charter boat historical time series was unrealistically high compared to the rest of the time series.

What we did is we took the same approach, and we looked at 1981 to 2019, and we looked at what is the average proportion of landings by fleet, and it turned out to be 10.5 percent charter, 2 percent headboat, and 87.5 private, and then we applied those ratios back in time.

This is the result on the right-hand side, and so this is really what you can look at if you want to see the changes between the last assessment and SEDAR 72, in red, and, on the left-hand side, it's just an illustration of what happened to that peak as we lowered it down and what happened to the historical time series as the peak was lowered down as well.

Really, the major difference here, and I will reiterate, but it's important, is the private and shore time series is much more dominant now that we have our FES-calibrated time series, and there are less changes observed in the other two fleets.

The recreational discards, again, we have now those fully-calibrated FES time series that we can use -- Let me go back. I forgot to mention one important point, which is that, in the last assessment, the CV was very tight on the recreational landings,

and there was a CV of 0.01, and so the model was forced to track those observed landings quite closely, but the panel decided to increase that CV, to increase it to 0.2, and allow a little more freedom around those catches, given our understanding of the uncertainty in the catches.

I will show you, later, that we do have a sensitivity run that looked into what happens when we tighten those CVs to 0.05, and you will see that we had a less -- The model performance was poor, and so I will go over that illustration later.

Back to the discards, and you can see, on the right-hand side, the differences in the additional years made available to this assessment. Again, unsurprisingly, with the scaling, we now have more discards coming from the recreational private and shore fleet.

The headboat, there are some differences, and they're coming from the fact, mostly, that we moved to the SEDAR best practices approach to estimate the discards, where you look at the ratio of discards to landings in the MRIP time series and the headboat time series for 2008 to 2019, and then you apply -- You take the mean of those discard ratios between the headboat and the MRIP charter and then apply it back in time over the entire time series to calibrate the headboat discards, where the headboat survey was not collecting data on discards, and so it's a proxy method, but it was considered an improvement over what was being done in the past.

Then, because we did not obtain any CVs from -- We didn't have any uncertainty for the headboat survey, and so we fixed those CVs to 0.2. I wrote "after 1986", because, prior to 1986, we had MRIP CVs available for headboat. Then, for the charter and the private and shore, we used the CVs that were provided to us by MRIP to set the uncertainty around those data points.

The assessment has an assumption on recreational discard mortality. That one was unchanged from the past assessment, and so it was set to 12 percent, and so, of the fish discarded, 12 percent automatically die, and the rest are released.

For the commercial landings, there were pretty minimal changes. The one change was a new set of gag/black grouper correction factors, and so, again, there was -- Historically, in the time series, there was some species misidentification, and the Center provided some updated ratios for applying to the observed black groupers, but you can see here, on the right-hand side, that it didn't make a huge difference on the time series, compared to what was available last time, and we do have, obviously, a few additional data points.

No CVs are provided for the commercial landings, and so we do have to make some assumption about uncertainty, and what was done in the last assessment was to tighten the CVs to 0.01, which was done equally for the rec landings and the commercial landings, and, this time around, the decision was to keep it to 0.01 post-IFQ, and the rationale being that we think we have a better understanding of the landings post-IFQ, more accuracy and precision. Then, back in time, increase slightly the CV to 0.05.

Now commercial discards, and this represents quite an improvement over the last assessment. Thanks to the improved methodology for the commercial discard estimates, we now have a much longer time series than was previously available, and so, in the past, the way the discards were estimated was to get a year-stratum-specific discard rate from the observer data and simply multiply it with a year-stratum-specific total effort that was obtained from the coastal logbook program, but, in SEDAR 72, we adopted the best practice of using discard per unit effort observed from the reef fish observer program multiplied by total effort from the logbooks, and this method also has a hindcasting that goes back prior to when the reef fish observer program was in place and allows you to obtain estimates of discard rates all the way back to 1993, I believe. Yes, 1993. You can see, on the right-hand side, that's quite a different time series available, and a much longer timeframe.

The discard mortality, again, in this case, we have a separate discard mortality for the recreational fleets and the commercial fleets, mostly given the fact that they fish at different depths, and so you would expect higher mortality in the commercial fleets that are fishing deeper, and that was set to 25 percent. That was not changed from the last assessment.

In terms of length composition, the data available were from the same sources as were used in the last assessment, simply updated -- Large sample sizes and more QA/QC. The length samples were weighted by the commercial landings for the commercial fleets and the recreational landings for the recreational fleets, at the finest spatial/temporal scales available.

The input sample sizes were input as the number of trips and downweighted according to the percentage of landings represented, and so, when we define a sample size in SS, you have a choice, and you can put the number of fish measured, and you can put the number of trips, but the idea is that, because fish from the same trip are not independent, and are likely to be correlated, it's better to try to match the effective sample size and maybe use something

more conservative, like the number of trips.

In our case, we took it one step further and said how much of our trips are being represented in those length compositions, and, according to that percentage, we down-weighted the sample size, and so those are the numbers that you're going to see in the top-right corner there.

For the commercial discard lengths, those came from the reef fish observer program, as was available already in the last assessment, and the input sample sizes, in that case, were simply the number of trips, and then, for the recreational discard lengths, those were coming from the FWC FWRI at-sea observer program, and input sample sizes as the number of trips.

I will note though that the charter recreational discard lengths were input as nominal lengths, and there is no weighting involved, and those were assumed to be representative of the charter fishery versus the headboat had to be weighted by trip length and region, because the headboat trips were not deemed to have been sampled proportionally to the fishing effort, and so there was an adjustment made so that the overall sample would be more representative.

You see, on the right-hand side here, what that distribution looks like, and, if I go to the next slide, we will see -- You will see the difference between the SEDAR 72 on the left and the SEDAR 33 update on the right, and so, generally, nothing drastically changed, and there were larger samples.

You might see some slight differences, for example, in the commercial longline discards, due to more years with different -- More years post-IFQ, for example, make a difference, and then you can also things like the SEAMAP video survey, and you now have larger sample sizes available, and so it's a little bit less variable and sparse than you see on the right-hand side.

In terms of age compositions, we had ages available for the recreational and the commercial fishery. Those were input as nominal age compositions, and there was no weighting involved, and there was a lot of work done earlier, in the SEDAR 72 process, trying to implement something more sophisticated, and we tried to apply something called a conditional age-at-length, which allows the ages to be linked to the length distributions, but we found out that --

The whole process assumes that each of those age observations are a random sample from the population for a given length group, and,

because of changes in sampling through time, and sometimes not adhering to a random or random stratified design, we were very likely violating that assumption, and so the decision was to stick to nominal distributions as an input, and we will dive into this issue further during the next research track assessment, because it clearly required more work, and it was dangerous to assume that we had random samples, when perhaps we didn't.

Those nominal distributions still had quite interesting information for the model, because you can see, in those bubble plots that represent the proportion of ages observed in each year -- You can see that we're able to track some cohorts, and so the diagonal bubbles show you strong cohorts passing through the fishery, and, alternatively, you can see some weaker cohorts in between, and those bubble plots show your vertical line and your longline samples, and so we had about 4,000 samples from the vertical line, and almost 3,000 from the longline, available, and you track very strong cohorts in 1989 and 1993, and then the last strong cohort observed is in 2010, but, beyond that, we don't really see much strong structure indicative of a strong cohort.

Then the recreational fleets, and the sample sizes are much smaller, and so it's going to look a little more variable, but we still were able to track the same cohorts that were observed in the commercial fleets, and they were apparent in the headboat and charter boats, at younger ages being targeted, of course, but still visible, but you see, on the right-hand side, what was available for the private fleet, which was very sparse. It's 460 samples over thirty years, and, unfortunately, it was not very informative for the assessment, and so that is one data deficiency, certainly, for a fleet that is representing a large amount of the catches, but you can tell, from those samples, that the private fleet is catching younger fish, on average, than the headboat and charter boat.

The other thing that you can see is that, similar to the commercial fleets, we're not really seeing any strong cohorts coming into the fishery in recent years, and the last observed one was in 2010, which is the same thing observed in the commercial fleets.

Moving on to the commercial CPUE, these are the vertical line and the longline indices, on the right hand-side. Now, those were actually unchanged from the last assessment, and the reason is that they define the pre-IFQ period, and so they are truncated in 2009, because we didn't have an ability to account for the changes that were due to the IFQ, and so those time series then change.

However, we did apply a data-weighing process on each and every

one of the indices, which essentially allows the assessment model to estimate additional uncertainty beyond what is input initially, and those uncertainty bands that you observed here, those confidence intervals, represent the original uncertainty plus the estimated uncertainty, and, later on in the talk, I will break down for you how much contribution came from the original uncertainty that was obtained from the standardization process and then how much was actually the SS telling us that there's probably additional variability that we're not accounting for.

For this series, it was a little too busy, and so I am just showing you the SEDAR 72 indices, with their uncertainty, but, later on in the talk, when I show you the fits to the indices, you will see the comparison with the SEDAR 33 update.

Now the recreational indices, and so we have the headboat on the top, which, again, that one is also truncated. It's truncated in 2010, and the reason is that, post-2010, there have been some pretty considerable reductions in the length of the recreational season, which is suspected to affect the level of discarding, and the headboat index, the way it's set up, it actually tracks only landed fish, and we didn't have a good way to adjust that index to account for any change in the length of the recreational fishing season, versus the MRIP charter and private index tracks both landed plus discarded fish, and so, in that case, we can go all the way up to 2019, and there is no issue with the reduced fishing season that can be incorporated and reflected in the level of discards.

As I mentioned before, there were some changes for the charter boat private and an effort to combine the charter boat and the private indices together, and so let me show you here just a quick illustration of the various steps that we're taking, and so, on the right-hand side, you can see the separate -- I don't think that I labeled those, and I apologize. I believe the top one is the private index, and then the bottom one is the charter boat index, and you can see the first step was to update each of them separately, so you can see the comparison between the SEDAR 33 update and the SEDAR 72.

One notable difference is the subsetting of trips that target gag were done using the guild approach in the last assessment, and that was changed to use preferred Stephens and MacCall approach for this assessment, and so that accounts for some of the changes, but you can see that, overall, they seem to have a very similar -- - They seem to have a very similar trend in those two indices, and it was decided that these two indices were probably redundant, and so it was preferable to combine them in a single index.

When that was done -- I don't have all the diagnostics here, but you can refer to the working paper, but, essentially, by combining the indices, we actually got improved diagnostics overall on the index, on the final index, and so it was the basis for deciding to move forward with a combined index.

Now, moving on to fishery-independent surveys, there was an age-zero survey in the assessment, and it's actually a combined survey, and it's a combined index, sorry, which combines multiple inshore surveys, and you can see, on the right-hand side, the different regions covered by the survey, and the combined index is one that is weighted by the aerial coverage of seagrass in each of the sampling regions, and so that was essentially unchanged from the last assessment, except that there is now additional data points that were re-estimated. This survey within the model structure is directly linked to the recruitment, and so it's a recruitment index, and it drives -- So it's very informative for the level of recruitment.

Then we also have the SEAMAP video survey, and we can see, on the bottom-right, it's a little small, but you have a map down here just to show you that it's a survey that operates on the high ridges all the way across the north-south axis, and, in this case, we had additional data points, but there was one change done, which is the type of model used for the index development, and so while, in the last assessment, a negative binomial model was used, this time around, they tested a delta lognormal -- Sorry. The delta lognormal was used in SEDAR 33, and, this time around, they fit a negative binomial model, and that one had better diagnostics and was accepted as the best model.

It resulted in a few small changes from the past assessment, which you will see later on, but, overall, the other major difference is that we allowed more uncertainty around that index with the data-weighing process, and also that we had additional length composition data available, and so you can see here, by year, what's available, and these are fish measured from the cameras, and there was a total of 148 individuals measured over the entire time series, and so not a ton, but good enough that we were able to improve selectivity, which I will show you in the next portion of the talk.

Then, in terms of how we input these into the assessment, those lengths are considered to be a random sample, and, thus, are input directly as nominal lengths, and the sample size -- To try to match the effective sample size, we used the number of camera drops with successful measurements, rather than the number of fish measured.

For the Panama City video survey, the methods were also unchanged from the SEDAR 33 update, but there was one change which was brought to our attention, is that the panel decided to drop the year 2005, which had been included in the past, and the reason was that, in 2005, the entire area had not been surveyed, and it was actually only the eastern part here that had been surveyed, and so it was deemed, in the working paper, that this was probably not representative of the entire area, and so it was safer to just drop the 2005, and so you can see here that it starts in 2006 and goes all the way up to 2019.

Like all the other indices, we have additional uncertainty estimated, and, like the SEAMAP survey, we have fish measured from camera drops, and the sample sizes are the camera drops, and we have a total of 122 individuals, and you can see here what type of lengths are available, and so even less than SEAMAP, and also note that the lengths are only available starting in 2009, and so, if you look at the index, the earlier part of the index, where we're seeing quite a few recruits, where 2006 is a lot of younger fish showing up in the index, and those -- Unfortunately, we do not have a composition for those in the model, and I will come back to that when we discuss selectivity and how it was parameterized for this index.

Then, finally, and importantly, we have the red tide portion of the assessment, and so we had some really interesting papers brought forward regarding red tide. There was some information from the Something's Fishy with gag survey from the GMFMC, which gathered -- I think they looked at over 400 comments, and there was a lot of good information on the perception of the stock.

There was a paper on the link between the hypoxia and the red tides, and, again, very good information on some years where perhaps we may not be -- We may see a red tide event, but maybe the hypoxia event might be detrimental to the gag population and located in a larger spatial area, maybe, than we would see if we just looked at the red tide events.

Local ecological knowledge, and so results from interviews with stakeholders that outline a ton of interesting information on the impact of the red tide, but other things, such as the changes in catchability across time, and then, of course, Dave's paper, which he just presented, showing the estimated mortalities generated from his ecosystem model, and so, with all this information, we -- The panel decided to stick to the method that had been used in the past, which is to model red tide in the assessment as what's called a bycatch-only fleet, and so you account for mortality by

generating essentially an F that is attributed to red tide.

You have to tell SS which years you want to, quote, unquote, turn on the bycatch fleet, and so, while 2005 was the only year selected in the last assessment, thanks to all this new information, we selected two additional years, which always came up as very strong red tide years, and they're clearly impactful to the fisheries, and so those were 2014 and 2018.

Now, one other pretty major change was regarding the selectivity. In the last assessment, the selectivity was set up to not include age-zero fish, and so the selectivity of the red tide was essentially making ages-one-plus fully vulnerable to the red tide, or equally vulnerable, whereas, in this assessment, given all the information coming from the ecosystem model that clearly showed that age-zero were impacted, that assumption was changed, and we put a flat selectivity over all ages.

Now, this was an operational assessment, and so, for the base run, we tried not to deviate too much from what was done in the past. However, we did try some alternative runs, which Dave alluded to, and there were two important sensitivity runs.

The first one was fairly straightforward, which was just look -- To look at the ecosystem model outputs and to look at, given those inputs, what is the relative vulnerability of different age classes to the red tide, and we transformed that information into an empirical selectivity at-age vector for ages-zero and five-plus, and so ages-five and above had the same level of selectivity.

We ran the assessment, and I will show the results later, and then the second one, which was more promising, was to actually remove the bycatch fleet, and so remove the red tide bycatch fleet, and, instead, what you do is you set up some additional mortality, natural mortality, on top of the baseline mortality, but using time blocks, and so, for every age, from zero to five, and every year in which red tide was detected by the ecosystem model, and so that's 2002 to 2018, minus 2010, because it was not detected in 2010, and so, for every age and every year, we tell SS to estimate additional natural mortality, which we attribute to red tide, and that additional parameter is informed by an informative prior that comes directly from the estimated mortality that's coming out of the ecosystem model.

We have a mean, and we have a standard deviation for those priors, and they're just coming straight from the ecosystem model, and so they're quite informative in guiding SS to estimate that additional mortality. I will show those results later on. Next is the base

model run.

CHAIRMAN NANCE: We'll go ahead and stop here, and I'm going to have anybody with questions -- You can kind of put your hand up on that, but we're going to take a ten-minute break, and then we'll come back to the base model runs. After the break, we'll get the questions, and so, if you have a question, go ahead and put your hand up, and we'll get to those after the break, and then, after those questions, Lisa, we can go to the base model runs.

DR. AILLOUD: Sounds good. Thank you.

CHAIRMAN NANCE: Thank you.

(Whereupon, a brief recess was taken.)

CHAIRMAN NANCE: Jim, you're up first for the questions.

DR. TOLAN: Thank you, Mr. Chairman. Great job up to this point, with a huge amount of data, but my question is not really a question, and it's more of an observation, and I just wanted to get your opinion on it, and it goes back to the length comps from the video cameras, and I'm looking at Slide 24, and it just surprises me that all of the fish that were successfully imaged, and, again, this is just successfully imaged, they're all about the same length.

There's very little variability, in terms of the -- A good deal of them are around sixty to a hundred millimeters, and the variability -- To me, it struck me that it wasn't there, and, when I compare it to the Panama City -- Again, they're smaller fish, because they're more inshore, in terms of the camera arrays, and so the next slide over, but that's more of the variability that I would expect to see, and so I'm just curious if you think that the deeper camera drops are recording something that's more of a behavioral thing.

The only reason I bring this up is because these indices are something that are really informing the model, and I just wanted to make sure that we're capturing what we really think we're capturing with these video drops, but that's the only point that I wanted to say, is that I was struck by the lack of variability in the SEAMAP drops, but thank you.

DR. AILLOUD: Thank you, Jim. To your observation, that's exactly right. I think one reason why SEAMAP is less variable is exactly because they are older fish, and so you would expect to see -- To some level, to see less variability in length-at-age from that

survey.

I did have lengthy discussions though with the groups in charge of these surveys, to ask if the measurements coming from those camera drops can truly be deemed random, and they didn't seem concerned. They seemed to go with the idea that, yes, they are random, and I did ask -- For example, for SEAMAP, I did ask, if you're smaller, are you less likely to be picked up, even for Panama City, but they didn't believe so. They didn't see any observation that would make me think so, and so I suspect the -- I suspect it has to do with the range of ages available to the gear. These are all high ridges, and so that's my interpretation.

CHAIRMAN NANCE: Thank you. Jason.

MR. ADRIANCE: Thank you, Mr. Chair. Would you mind going to Slide 11? I just had a question there. I understand how you dealt with the 1983 peak, but I noticed, after all that, in the private shore, there was now a 1982 peak, and I just wanted to see if there was any thoughts on that or explanation for that.

DR. AILLOUD: That peak appears because, now that we've lowered the other one, that one appears more prominent, but, if you look at the scale, you can see, on the left-hand side here, this is the 1982 peak, which we didn't alter, and it is a peak, but it didn't fall so far beyond the range of the rest of the time series, especially if you add the uncertainty bounds, and so it wasn't deemed to be something to be concerned about.

There was also troughs that you could be concerned about, for the same reason, but, if you look at the scale here, that 1983 was really off the charts, and that's why it's the only one that got singled out as being potentially problematic.

MR. ADRIANCE: Okay. I was looking in terms of just the private shore, where it really peaks compared to the rest of it there, on the right-hand side, but I do understand what you're saying. Thank you.

CHAIRMAN NANCE: Lisa, it seems like, also, in our discussions, that 1983 had just a real tiny sample size, whereas the other years around that had a very good sample size, and so 1981 and 1982 and 1984 and 1985 had very good sizes associated with those numbers.

DR. AILLOUD: Yes, you're correct. There was more intercept surveys.

CHAIRMAN NANCE: Okay. Thank you. Will.

DR. PATTERSON: Thanks, Jim. I have a question about some of the life history data, and so, back on Slide 9, where the information on transition from female to male is presented, in the SEDAR 33 update assessment, and so there's been a change, based on new information, on the perception of both the maturity of females as well as the age of transition, but, looking back at SEDAR 33, this is -- The blue lines in the figure on this Slide 9 is what I would expect to see, is that there would be a pretty close agreement between the transition, perhaps, to male, but there -- Well, there's a broader gap that we see here in SEDAR 72, between what we see as far as percent maturity and then the transition.

I am just trying to make sense of what the difference is there and whether we feel -- Obviously, the decision was made that the switch here to the information that we see in the red lines was more appropriate, but can you just tell us a little bit more about the information and how this decision was made?

DR. AILLOUD: Yes, and so this part is a bit confusing, because the data that we get is those solid lines. This is what is being measured in the wild, and it's the proportion of females that are observed on the sites.

The dotted line is a modeling -- It's just the equation used in the assessment, and so we have to do an exercise to go from one to the other, so that, when we input this transition rate, the result is that solid line, and so there's a way, internally to SS, to verify that what we put in translates into 50 percent females at whatever age we're trying to target.

When I went back to the SEDAR 33 update, I actually had a very hard time tracing back exactly how this transition rate was calculated, this blue dashed line, and I could not recreate what was done, and so what I did was look at all the other assessments that used transition functions that had been done since the SEDAR 33 update, and, given those scripts and this information, I was able to formulate my transition rate properly and input it in SS and obtain exactly the result that I was expecting, which is 50 percent female at more than twenty-six years of age.

It's possible that there was a mistake in the SEDAR 33 update for this transition rate. Unfortunately, I'm not 100 percent clear, but all I know is that I was not able to reproduce it, but, to your point, the reason why they differ so much is that the transition rate is -- It's the age-specific fraction that changes sex in each of those age groups, and so it's a little bit hard to wrap your head around, but, as long as you can interpret the solid

line, you can somewhat ignore the transition rate, because they are doing the exact same thing, and they are just working differently inside the assessment, and I hope that makes sense.

MR. RINDONE: Lisa and Will, I think Sue Barbieri is on the phone, and she might be able to shed some light on this, also.

DR. SUSAN LOWERRE-BARBIERI: What we found, in terms of actual age at transition, is it's just a little bit older than what had been reported previously, and I think what Lisa is trying to explain is that the way that SS 3 calculates proportions at transition is different than how biologically we estimate 50 percent at transition. Will, to your question, it wasn't very different than what the actual biological parameter was in the prior stock assessments. Does that make sense?

DR. PATTERSON: The first part made sense. I didn't catch what you said there about the prior assessment and --

DR. S. BARBIERI: This graph makes it look like there's a big difference, but I think -- Luiz, you can jump in if this is wrong, but I think it's because of how SS 3 actually integrates transition rates into the model as opposed to how biologically we estimate 50 percent male transition.

The actual -- In terms of life history and whether it changed a lot, it didn't. It was a little bit higher, but not much, but this is showing the transition rate, as opposed to how we typically estimate 50 percent male transition. I guess this graph is giving the wrong impression. It looks like there's a huge change at some point and that males weren't transitioning until age-fifteen, and that's not the case. It's just the way they're using the rate, as opposed to the way we typically use 50 percent maturity for transition.

DR. PATTERSON: That's the disparity that I don't understand, and what Lisa just said was that we could ignore the dotted line, or at least that's what I thought she said, and so I'm actually still confused.

DR. AILLOUD: It's easier to compare those solid lines, and so, for example, the slope parameter and the parameter that shows the 50 percent proportion of females -- Sorry. It's the 50 percent proportion females at equilibrium, and so that age was originally around 10.4 and now shifted to 11.6, and I think that's where Sue was saying it's not a huge change, from 10.7 to 11.6, and so that's a little shift. There was a bigger shift, I guess, in the slope, when I'm looking at it here, but the overall result was not

drastic.

DR. PATTERSON: I guess my confusion is that it appears to me that the model is perceiving there to be this multi three or four-year lag that animals are going through transition before they become male. That's what this suggests to me, and I don't really understand what the implications of it might before the assessment.

DR. AILLOUD: Sorry, and so that transition rate -- It's not a -- That's where it's misleading. It's not that three years later that same amount of transition happened. That's not how to interpret it. It has to do with the ratios of males to females at equilibrium, and it has to do with, in every age group, what fraction of that age-specific group will change sex, and so I'm not sure -- I'm sorry, but I'm not sure how to express it better.

DR. S. BARBIERI: Will, did you understand that?

DR. PATTERSON: I understand the female part of it. I still don't understand the transition rate component of it. I guess the take-home is don't interpret that as the fraction male, and so they shouldn't necessarily agree, but I don't understand why there would be this difference at age-eleven-point-whatever, but I don't want to hold up the process here, and we can move on, but that just struck me as odd.

DR. S. BARBIERI: Just really quickly, Will, it's because SS 3 estimates the annual transition rate each year, which is different than how we do maturity, and we can send you the details on that, if you want.

DR. PATTERSON: Okay. Great. Thanks.

CHAIRMAN NANCE: Katie.

DR. SIEGFRIED: Thank you, Chair. I just wanted to add, if this will help Will at all, it's actually a conditional probability, rather than a transition rate, and so it's a conditional probability of transitioning to male at those ages. That's the way that it's estimated in the model, rather than it being literally a transition rate.

DR. PATTERSON: Okay. Thanks.

CHAIRMAN NANCE: Sean.

DR. POWERS: Thanks. Can we review the rationale and the need to combine the charter boat and private CPUE, more the rationale

behind why you combined those?

CHAIRMAN NANCE: Do you have a slide number for that, Sean?

DR. POWERS: I did not, but there we go.

DR. AILLOUD: Okay, and so the original discussions were that they were -- The feeling was that they were redundant, and they were tracking a fairly similar portion of the population, due to where the fleets were fishing and due to the trends that were being observed in the index, and the exercise to try to keep them separate or combine them resulted in the model fits and diagnostics being improved in the combined index, and so that was the rationale from the analysts, to propose that a combined index might be preferred, because it had a better behavior, and then, on top of it, it avoided having two indices that might be tracking very similar portions of the population and perhaps putting twice the weight, when we maybe didn't need it.

DR. POWERS: Okay. That helps, but how did you deal with the sample size issues, because I would assume charter boat and the private CPUE -- What was a trip, or what was an effective sampling unit, and were they similar between the two?

DR. AILLOUD: It was fish per angler hour, and I don't recall the exact detail on the development, and I'm sorry.

DR. POWERS: That's fine. You can send whatever later, and I wouldn't expect someone to remember that level of minutia, but I was just interested in -- Essentially, it was parsimony and trying not to get that information overweighted, since they were both collecting the same size.

DR. AILLOUD: Right, and the detail is in Working Paper 7, which I think --

DR. POWERS: That's it for me, Jim.

CHAIRMAN NANCE: Okay. Thank you. Mike Allen.

DR. ALLEN: Lisa, excellent presentation, and it's great that it's such a data-rich species. I had a question about -- I believe it's on Slide 23, the age-zero survey and this recruitment index, and my question is, is the survey collected only in seagrass? My concern is that, if so, changes in seagrass abundance could influence the reliability of that index to actually estimate recruitment as a whole, and so I'm just wondering about that, if that was discussed as part of the SEDAR.

DR. AILLOUD: Well, we didn't really discuss it too much, because I think there was lengthy discussions in the last assessment, and that was accepted as the best practice, but my understanding is that the survey is not only in seagrass areas, but that the seagrass area covered by each individual survey was used to weight the different indices together, and so you have -- Let's see.

There is the Florida State University estuarine gag survey, and there is the Panama City Andrews Bay survey, and then there is the State of Florida estuarine survey, and so all these separately cover, in part, seagrass, which then they looked individually, to see how much of the area covered is covered by seagrass, and that was used to weight them.

DR. ALLEN: All right. Thank you. That's helpful.

CHAIRMAN NANCE: Okay. Thank you. Let's go ahead, and, Lisa, you can proceed with the presentation.

DR. AILLOUD: Okay. Moving on to the base model run, obviously, there were a lot of steps that were taken between starting from the last assessment to building up to all the changes that I described in the data streams and then additional changes due to the tuning stage of the model.

I just broke it down to eleven major steps, so you can get a feel for what steps caused what trends to appear in the final trajectories, for example, of spawning biomass and of fishing mortality and all that, and so, here, I have just general -- To outline these steps, the first one, obviously, is to start with our last assessment, and the second step, which was necessary, was to simply convert to the newest version of Stock Synthesis, and so that's often done, as you know, and that doesn't change any of the results, but it can change some of your likelihood, or gradients, as you can see here.

The next step was just to replace -- No alteration to the terminal year, and we stick to 2015, but we take out those recreational catches and discards, and we put in the FES estimates, just to see if that alone, without altering the years or anything, what that does alone to the results. Again, not changing the CVs, and so keeping that very tight uncertainty around the recreational catches.

Step Number 4 is where you'll see all the new data added and all of the additional years, and so now your end year is 2019, and you now have a slight change in, for example, the indices, and we now

have that combined index. You have all new data, in terms of catches and discards and indices and length composition and age composition.

The next step is Number 5, and now we add all the biology, and so we change the mortality, the maturity, the hermaphrodism, the growth, and we change the time blocks, because you will see, later on, there are some time blocks that were set up originally to account for the changes in the minimum size limits, and, because there has been new regulations since 2015, we have to create new blocks to account for that.

We also had to add new blocks, because the recreational fishing season has undergone some different phases of reduction along the years, and so we had to account for that, to reflect potential differences in retention in the recreational fleets.

Then Step 6 is when we start actually improving the fits, and so we're playing around with the selectivities, and we're adjusting them, given the new data streams. There was a prior, originally, on initial Fs that was from the last assessment, which I removed, and it was not necessary in this case, and I presume it helped with model stability in the past, but, with the new model, it was not deemed necessary, and then recruitment deviations were extended to 2019, because we do have a recruitment survey, and so it was judged that we were able to get a fairly good estimate of recruitment all the way to the end of the time series.

The Step Number 7 was to add on now and turn on that bycatch fleet for 2014 and 2018. As you recall, red tide was only in 2005 for the last assessment base model, and so we add two more years, and then Step 8 is now we're doing some data weighting, and so we're applying what's called the Dirichlet multinomial weighting of the length and age composition, and the idea is that it's an internal weighing of process, internal to SS, where SS can downweight some of the overall sample sizes assigned to the length and age composition to improve the fits. I will show you the result of that weighting later on.

The following step is just a standard bias adjustment, and that's a recommendation from the Stock Synthesis developers, to adjust the recruitment deviations, adjusting using the bias adjustment ramp, and Step 10 is another data-weighting step, but, this time, we're actually weighting the indices, by adjusting the amount of uncertainty around those observations, and so every single index -- For every single index, we estimated some level of additional standard error to be added on to the input standard error. Again, I will show you what levels of standard errors were estimated.

Then, finally, because we did one more adjustment, we needed to apply that recruitment adjustment ramp one more time, to get our final model.

You can see -- Unfortunately, it's hard to follow, and we're used to looking at likelihoods to look at model fit, but, really, once you throw in the Dirichlet parameters, it blows up everything, and it blows up your likelihood, and that's just the way it's parameterized, and so not too informative, but you can see that, beyond Step 8, those little adjustment steps do improve the overall likelihood and the gradient.

Those are the steps, and those are the changes in the estimated quantities that you might be interested in seeing as kind of a summary metric, and so one important one, obviously, is your virgin recruitment here, which went from -- It's a on a logscale, but it went up from 8.52 to 9.34, and so you will see, in the plots, that now we estimated a much higher level of virgin conditions.

The offset, or regime, however it was called in different versions of SS, that is a parameter that allows you to say that, even though I started my time series in 1963, that's not when -- We were not in virgin conditions, and so fishing was already occurring, and so we cannot assume that we were in virgin condition, and so it estimates an offset, to say, well, how far off were we from virgin conditions, and so this is what you see here.

The target M, like I mentioned, we did change our baseline M, from which we did the Lorenzen scaling, and so it's a little bit higher, 0.159, and now, just like recruitment, everything scales up a little bit, and so your virgin spawning stock biomass goes up from 24,000 metric tons up to 36,000.

Virgin recruitment is seen here in numbers, so you can get a better sense of what that translates to, and then, finally, I just put the SSB over SSB_0, and so over virgin conditions, in the start year and in the end year, just so you get a sense of how that shifted, and while, originally, in the previous model, it was observed that the stock was less depleted in the final year than it was in the start year, we have the reverse happening now, and we actually have a less depleted stock in that 1963 and the first year of our time series, at 42 percent versus 29 percent, which was estimated in the last assessment.

How does this translate into your time series of SSB? You see, on the left -- With the base model, we are looking at females-only spawning stock biomass, and we will see, later, how this affects

the combined spawning stock biomass, but, for now, it's females only, and I just want to draw your attention to a few important steps, and so, if we start with our base SEDAR 33 update run, it is the very darkest blue line that you see, and so, as you see in the last assessment, the stock, virgin stock, was lower, almost hitting zero in the 1970s, and then there was an uptick in the last five years of the assessment.

Once we updated and converted -- Sorry. Once we converted it, it didn't change anything, and those lines are on top of each other. However, once we added FES, we have a drastic change, and there is a jump here, and our virgin conditions are now much higher, and, in the terminal year, instead of the uptick, we see this trajectory starting to head down. Now, this is before we add any other information, and we're still ending this data in 2015.

Overall, with FES, we see more variability across the time series, and so larger increases in certain years, and the next big step is when we add the new data, and so those are the catches, the discards, the indices, and that, I guess, teal line is when you add all the new data.

An important thing that we add there is we're allowing those CVs also to be a little bit larger on the recreational survey, and so your uncertainty then becomes more larger as well, and so, with the new data, we're now getting a different estimate of the trajectory between 1963 and about 1990, compared to before we added the data.

Also, there's a bit of a lowering of that virgin condition compared to just having the FES data, but, generally, all the next steps are going to keep those virgin conditions quite close to each other, even after updating the biology and doing the data weighting, but then, if you look at the trajectory from 1963 to 1990, once you update, the next step is to add the biology, and so think about, importantly, natural mortality had an important impact here.

That also causes it to lower down a bit here, and then all the additional runs beyond that don't really have a drastic change in the overall trajectory.

Then, if you look at the recent times, you see this kind of bump, which I will go over, but, essentially, it coincides with your red tide coming in 2014 and then your SSB leveling off, and so that's very different, obviously, than the trajectory that was observed in the terminal year of the last assessment.

Now, just a different way to look at it, on the right-hand side, is to look at the fraction unfished, and so you see, again, major steps are going to cause, again, the most major changes, but you can see here that, really, the fraction unfished in the SEDAR 33 update was really shooting up in the last five years, and keep in mind that there wasn't really much information to point to the impact of the 2014 red tide, because the model ended in 2015, and so I think that what was really important for the change here is that we now have the indices, and we have the length composition, and we have the age composition, and it's all pointing to the fact that the red tide did impact the population, and it's leveling everything off over here.

You can see that even more clearly once you look at the exploitation rate. These exploitation rates are the total biomass killed of age-three-plus, and so the ones that are selected by the fishery divided by the total biomass of age-three-plus, and a few things to point out.

The first is, when I say exploitation rate, in these graphs, it does include the red tide fishing mortality estimate, and so, those peaks that you observed in 2005 and 2014 and 2018, those include red tide, and that's why they become so apparent, but you can really see how the last assessment estimated a pretty low fishing mortality, and, of course, did not include the 2014 red tide in that case, but, now that we have new data, we're really seeing quite a lot of variability in levels of F in recent years that are much closer to the level of F that we were seeing in the late 2000s.

Then, finally, the recruitment, and so, unsurprisingly, as we're scaling up the population, with virgin conditions being higher, then the total time series kind of scales up, but what you see is fairly large recruitment in -- I should specify that what's considered the data-rich period is when the length composition comes in, and so it's 1984, in this case, and so, from 1984 to 2019, that's where the recruitment deviations are estimated and centered to zero, and so what you're seeing is more recruitment, stronger recruitments, in the early 2000s and then a drop down to 2011, and there's less variability in recruitment in the more recent years after that. I will move on to the fits, and then I will break before going into the diagnostics.

Model fits, now we're looking at -- You're going to see a series of plots that are set up similarly, and you have the expected, and so the predicted, values in blue, and the observed data is in black, and then, on the left-hand side columns, it's going to be your SEDAR 72, and then your right-hand side is going to be the

SEDAR 33 update, and so you can see the differences not only in the data inputs, but also in the fits.

This is the landings, and so you have your recreational landings in numbers, and so here in thousands, and then you have your commercial landings in metric tons gutted weight.

The main take-away here is that, because we allowed more uncertainty, a larger CV, around the rec landings, we're now seeing more departures, and I will show you, in later slides, that what ended up happening is that the model was allowed to fit the discards more closely for the rec weights, which are larger in proportion, at the expense of maybe departing a little more from those landings, versus in SEDAR 33, and the model was forced to follow those observed values very closely, and so that's why you can't even tell the difference between observed and expected.

Then, if we looked at our two commercial fleets, the CVs are tighter. We do believe that we have a better handle on our commercial landings estimates than we do on our recreational landings, which was the reason to do so, but you see a little bit of departure here and there in a few years.

I mentioned before that we did look into what would happen if we lowered the CV on the recreational landings and forced the model to fit more closely to these observed values, and the result was an overall worse fit.

The total likelihood indicated worse fit, and the model ended fitting more tightly to the catches, and that degraded the fit to the discards, and I show here the likelihood profile on R0, and so these are the different components of the likelihood, or the different colors, and what you would want is them to converge on the same value, so that you don't see any disagreement, or strong disagreement, between different datasets, and clearly you see that, on the right-hand side, when you allow the CV to be a little bit larger, the model is generally happier with the datasets being more in agreement.

When you look on the left-hand side, there's a bit of disagreement here, with the discard data pulling towards a higher R0, the age data pulling towards a lower R0, and so the conclusion was to adopt that CV of 0.2 as an improvement over the last assessment.

Just to show you the implication, the sensitivity run, and so the red lines, show the tighter CVs, and so, if you look at the uncertainty bands, for the most part, they overlap, but there are some differences in the estimates of the time series, and, notably,

in the recruits in recent years.

Discards, like I mentioned previously, now you will see the differences between SEDAR 72 and the 33 update, and so, in the 33 update, you see that fitting the landings closely did come at the expense of greater misfits to the discard data, and you see this in the charter boat, and there is an odd peak happening here in 2012, I think, and so, if you look on the left-hand-side, we have better tracking of the discards. Recreational, private, and shore, obviously the magnitudes are higher, and so you see that difference from the left panel to the right, and then, on the right-hand side, you see now your expanded time series of discards for the commercial fleets, and there were some -- So the vertical line was tracked fairly well.

It was a bit harder to fit the commercial longline time series, and, with the relatively higher CVs, it was allowed to depart from it. Keep in mind though -- So two things. One is I do think this is indicative maybe of -- There might have been some catchability changes due to the IFQ, some behavioral changes in the fishery.

However, keep in mind that we're looking at thousands of fish here, and so, really, it's the difference between a thousand fish and 4,000 fish, and these are not very important on the final results of the assessment, is I guess my point. It's something that I noted in the research recommendations, but it's probably not something to worry too hard about. Then, finally, the recreational headboat here, again, we're able to track the observed values a little bit closely.

Okay. Now the indices, and so this gives you a chance to see the different data inputs, as well as the different fits between the assessments, and there is a few things that I want to bring your attention to.

First, obviously, I did mention that we did do some data weighting in the tuning process, and so we did allow more uncertainty around those indices than were in the past, and you can see it's very drastic with the age-zero survey, where those uncertainties -- Those CVs are really, really tight, and so that's one big difference, but the other major difference that I noted is that the last --

If you look at the last three or four or five years of all these time series, if you look at the independent surveys, and so the SEAMAP and the Panama City video, it's really not doing a very good job of tracking those last few data points, and it's overestimating, in both cases, and you see a similar pattern

happening in the private index, where it's really overshooting beyond those uncertainty lines.

If you recall, the trajectory of the spawning biomass in the last five years of the previous assessment, was really kind of an upward ramp, and so it seems like it was not in agreement at least with the indices and with the update, and we got some improved fits, especially here with the SEAMAP video survey, and, overall, similar fits in the commercial fleets, and similar fits in the age-zero survey, and so, really, that is the one kind of major difference that I noticed, is the last five years of the time series.

Then I mentioned model weighting, data weighting, and so this is to give you a sense of how much extra uncertainty was added on, and so the first column here would be the different fleets, and so we have our commercial handline, our longline, our headboat, our charter private CPUE, our age-zero survey, our SEAMAP survey, and our Panama City survey.

Those are the input CVs, and so what SS does is it takes an average standard error over the entire time series, and then it adds on to that average and gives you this estimated additional uncertainty, and then you end up with a new average SE.

Just to give you an idea, it doubled the uncertainty around commercial handline, and it didn't really change it for the commercial longline, but the big changes that I see are also age-zero, which makes sense, because there is a lot of values near zero, and so those standard errors were very small, and then not much addition on the headboat, the charter private, and the video.

Now I will go over the selectivities, and so that was a term of reference, and it was in the last SEDAR 33 update, and so it's the panel on the right-hand side. There were a few things to note, and so one is that the private, the headboat, and the charter fleet -- There were some issues getting the selectivities to converge, and so one of the things that was imposed on the selectivities was a decline to zero at the largest length, and so that's why you kind of see that those lines kind of follow each other, and they all kind of decline gradually to zero, which is -- It was predetermined by the shape of the selectivity curve.

The longline was a logistic, and the commercial handline was a double normal, and the SEAMAP survey was a logistic. You see it all the way on the far-right here, and I will come back to that. Age-zero is not shown here, and age-zero is only selecting age-zero, and so it doesn't have a selectivity, and that's it.

On the left side, those are the selectivities estimated in SEDAR 72, and so a few differences is we allowed a little bit more freedom at estimation in the charter and headboat fleets, and so, instead of pinning down the selectivities to go to zero on the right-hand side of the largest length, we did allow the model to see if it could estimate either plateauing or -- It was free to estimate it however it wanted, and it could do to a logistic, it plateau, or it could go to zero, and it found this intermediate selectivity, and so selecting some of the older fish, but not all of them, and mostly selecting around sixty centimeters, with the private fleet selecting -- Well, remember that it's private and shore, but it's selecting those fish as well around sixty centimeters, but also selecting more of the smaller fish than you would select through the charter boat and the headboat fleets.

Then you have here your SEAMAP survey, which is much shifted to the left, and I will come back to that, and then your PC video is kind of a square shape, and that one was something we had to force in a little bit, which I will discuss as well.

The red lines here will show your SEDAR 72 estimated selectivities compared to the blue lines, which are the past assessment. The left-hand side is the selectivity at-length, and so the way the model is set up -- In SS, you can specify selectivity at-length or at-age or both, and I will let you know when I specified either or both, but, on the right-hand side, it's what internally is done in SS, is to translate all of these selectivities into selectivity at-age, and so you can kind of get an understanding of how that translates to the ages, and there's a little bit of an oddity here in the legend.

On the left-hand side, the first one is the vertical line, and so I did try to keep that selectivity flexible, and, by that, I mean to give it a double normal shape, where SS would be allowed to go to logistic if it wanted to, or it could be allowed to diverge from it, as it did in the last assessment, but I ran into issues. It was very unstable, and, in some runs, it would go down to essentially a selectivity of zero at a hundred centimeters, which is not what we know about the vertical line.

We know it fishes out on grounds that overlap with the commercial longline, at a much higher depth than the recreational fleets, and so the decision was to make it a logistic curve, and, with that, it was much more stable. I did have -- I did do a quick run, just to check that it didn't have a huge impact on the results, and it did not, but it was mostly model stability.

The commercial longline, nothing really changed here, and it was

still a logistic, and so it just got estimated and shifted slightly to the right, and so slightly to those larger fish, with the additional data. The headboat, I already mentioned. Instead of pinning it down to zero, it was really estimating the selectivity at the largest length classes, and so this is how the newly-estimated curve looks, the red one. Then, on the right-hand side, you just kind of get a sense of how that translates into the age-based selectivity.

Now, on the charter and private and shore, again, charter is similar to headboat, and it's given a little more freedom, and it actually preferred to select more of those larger fish, to match with the length composition, and the recreational and private and shore -- That one was really difficult, and I did try to diverge from the last assessment, and I tried more flexible shapes and lines and other forms, but the issue is that we don't have high quality length data, or age data, for the private fleet, and so anything I tried was a bit unstable, and, in the end, I decided to land on a double normal, which is what was used in the past, and just force it -- As was done in the past, force it to go to zero, because we know the private fleet does not fish as far as the other recreational fleets, and so that was a good assumption, that it does not -- It will not get out to those larger fish, and that was the agreement of the group and the decision of the panel.

MR. RINDONE: Lisa, can we go back to the previous slide, please? Just about that assumption that the recreational, private recreational, fleets don't fish as far, and I know we had talked about this a little bit in some of the webinars, that, these days, there are definitely recreational vessels that can fish as far and as deep as any of the commercial fleets.

The kind of technology that's being built into some of these larger vessels, these larger recreational vessels, rivals, and, in some cases, it could be even better than that that is used by the commercial fleets. Just insofar as that assumption is concerned, I think maybe we should be cautious about that.

DR. AILLOUD: Thank you for that comment. I definitely agree, and I remember discussions around that. I think, ultimately, the limitation was simply a data limitation, and there was no fish sampled at those lengths, and so the model could not -- Once I asked it to estimate selectivity -- If there are no fish in the composition, it will not believe that the fleet is selecting them, and the only alternative is to really fix those selectivities, but we didn't have kind of a good prior knowledge to do that.

I totally agree, and I think I put it in the recommendations, that

we really need more length data on this private fleet, because it's been very hard to correctly describe it in the model.

MR. RINDONE: I agree with that completely, and I think part of the reason why we don't have the length data for those vessels is most of those vessels are going to be leaving from private access points, or private marinas, and so the probability of them being intercepted is likely reliant to them being intercepted directly on the water, because they are going back home, essentially, and so there isn't going to be somebody there to measure those fish, and so it would seem highly unlikely that we would get those data, unless they were collected through some other way.

DR. AILLOUD: Okay. Good to know. Thank you. Related to selectivity is the notion of fleet retention, and so, in SS, you can specify a shape for the retention of each fleet, and so it's essentially saying which lengths are discarded by the fishery, and you can allow it to change through time, and so that was a term of reference. It had already been looked into in the past assessments, and we just kind of expanded upon what was already done to account for the additional years that were made available.

Just so you have it kind of in the back of your mind, I did put here the major changes in regulation from 1990 to 2019, and so 1990 is the first year with a minimum size limit, and so you can see the changes here from twenty inches to twenty-two to twenty-four. You can see, in the commercial fleet, that there was a very recent change in the minimum size limit in the terminal year of the assessment, and so that will become relevant when we discuss benchmarks and when we decide how we want to assume retention projecting forward.

There is also -- There were some aggregated grouper catch limits, bag limits, sorry, and trip limits, and those are listed here, and, obviously, the IFQ started in 2010 for the commercial sector.

What we were able to include? We were able to create time blocks based on changes in the minimum size limits, and the assumption is, if a size limit came in midway through the year, the time block would come in during January of the following year, assuming that we might not see the full change in behavior until the full year.

The other thing we did include, which was also included last time, is the change due to the IFQ, and so we have evidence from the length composition of retained and discarded catch from the commercial sector that, after the IFQ was imposed, there were fish that were above the size limit that were being returned to the sea, and so those were included in a retention function, and I

will visually show you what that looks like in the next slide.

Then, finally, in a similar vein, for the recreational fleet, after 2011, there has been some pretty major restrictions on the length of the recreational fishing seasons, and it changed from year to year, and it changed between state and federal waters, but what we decided is that we observed mainly three different times that were uniform within each other that had similar lengths of recreational fishing season, and those were a time block for 2011 and 2012, which was the most restrictive fishing season, and so you would suspect more fish to be discarded if they're caught outside that fishing season.

The second would be from 2013 to 2015, and that was mildly restrictive, and then more days were opened up in 2016 to 2019, and so we determined it was the least restrictive, because it's still not fully open, and so we do expect that discards will happen due to it, discards of fish that are legal-sized fish that would need to be discarded, and so those are three time periods.

How does that translate? In the assessment, we have this retention curve, and you see there that it's a logistic shape, and what you can define is the inflection point and the slope, and so you see here that we have the vertical line and the longline, and then we see the different assessments.

For the commercial fleets, we're now adding additional layers, right, and we're adding additional minimum size limits, because we increased the time period covered, but, overall, we have the same shape, and so the logic is that the slope of the curve -- When there is a minimum size limit in place, we assume it's knife-edge, and so we assume that, if there's a minimum size limit in place, but there's no IFQ, the fish that are below the size limit are discarded and the fish that are above are kept.

Post-IFQ, we let the model estimate an asymptote, which you see here in the vertical line longline, and so it allows it to go less than one, saying that a certain percentage of fish that are of legal size are going to be returned to the water.

As expected, the vertical line asymptote was estimated to be much higher than the longline. When we look at the length data, the longline showed a lot more discard of legal-size fish relative to fish that were below the size limit compared to the vertical line, and so it did match our expectations, and it was similarly observed in the last assessment, but the vertical line asymptote was a little lower, and I think it's just due to additional years informing that asymptote.

Then, finally, I forgot to mention that the situation before any size limit was in place is that gray line, and it's what we call an effective minimum size limit, and so it was not in place. However, we do expect that the fishery was not retaining all the fish, especially those fisheries targeting larger fish, and so this is the shape of that effective minimum size limit, which was fixed.

Now the same idea goes for the recreational fleets, but, in this situation, we have -- Instead of an IFQ, we have a reduced fishing season, and so we have three different asymptotes being estimated. In the last assessment, there was only one being estimated, because the terminal year was 2015, and the decision was to have a single asymptote across all those years.

You can see here the headboat, and the most restrictive season was in 2011, and so that's that dark-green line. That's the lowest one, and you see it with the charter boat as well. That's the lowest line here, and so the most fish being discarded, and then, in the private fleet, it was estimated around 0.5.

With the next season, which was 2013 to 2015, it's that dotted line, and so it's higher, and so now you actually are retaining more fish, which makes sense, because the season is open for a little longer, and the same with headboat, and now I will get back to private at the end, because private was only able to estimate an asymptote in the most restrictive fishing season.

Then, if you look at headboat and charter boat, for the most recent time period, which is 2016 to 2019, the asymptote is around 0.5 for headboat and 0.7, or 0.65, for charter boat, and so that's the least restrictive fishing season.

Now, because the private fleet -- We tried to let the model estimate those asymptotes in all three seasons for the private fleet, but remember that we don't have length composition of discards for the private fleet, and so, really, the model -- The only thing it has to go with is the ratio of discards to landings and the length composition of the retained catch, and so it's much more difficult to get an estimate of that retention than it is with headboat and charter boat, and what happened is the asymptotes for the less-restrictive fishing season would bound at one, and so it would not be able to estimate anything lower than one.

We did do a sensitivity run just to check what would happen if we assumed that the retention functions of the private fleet matched the headboat, and so you say what if the private and the headboat

fleets behaved the same, and we fixed those curves, those asymptotes, from the headboat to match the private fleet, and the result, overall, was a degraded fit, and one of the things we notice is a misfit to the length composition of the retained private share catch, where it was creating more larger fish than were being observed.

If you look at the actual overall trends through time, the biggest difference was this virgin condition, and the sensitivity run showed much higher virgin biomass than was estimated in the base run. Just by changing this private fleet retention, there was larger estimates of exploitation rates and because the spawning stock was scaled up, as was the recruits.

We had discussions about this, and similar issues were observed actually in the last assessment, and they had done a similar exercise and had seen similar results. The discussions, conclusions, were the people didn't think that the private fleet operated exactly like the headboat and that they had different mechanisms to react to the fishing seasons being restricted.

The conclusion -- The panel, in the end, decided to stick with letting SS estimate retention in the private fleet and, again, adding this topic to a future research track, because it's, obviously, important, but we are, again, in a data-limited situation, and it's difficult to make a decision without more data, more information, on the behavior of the fleets.

Moving on to the surveys, the selectivity -- Again, I'm showing you the selectivity of length and then what's translated into age on the right-hand side. One thing I wanted to point out is that the blue line here shows you the selectivity of the SEAMAP survey that was estimated in the last assessment, and so, in both the last assessment and this one, we specified a logistic selectivity, because that survey targets high ridges, and it is targeting the largest size and age classes.

However, with the updated data, you see that the curve shifts all the way to the left, and so I looked into -- If you pull the actual mean total length that come out of the camera drops, the survey, you have here total length. SS is in fork length, but it's quite similar, and then the east region is the region that we build the index on, is the red line, and so the fish are sixty to eighty, and Jim pointed that out, that it's a pretty restrictive length group.

When you look at the selectivity that was used in the last assessment, it's really not selecting anything until about ninety

centimeters, and, even then, it's ramping up pretty slowly, and so I think there was probably an issue of low sample size, and now we're improving it just by improving the data points, and so this new curve we think matches our understanding of what the index is tracking. That's what it was built to track, the survey was built to track, and so we feel more confident about this new selectivity.

Now, interestingly, the opposite happened with the Panama City video survey, where, in the last assessment, you see that blue line, and it's a double normal selectivity, and the parameters are estimated.

In the last assessment, it estimated something that was quite aligned with our understanding of the survey, and the survey is inshore, and it's not tracking high ridges, and it's not tracking the largest fish, and it's capturing some of the smallest fish as well in the population.

Now, when I tried to do the same thing and let SS estimate all these parameters, it would go to a logistic, and we don't believe the Panama City video survey should be logistic. It's not observing those fish, but the issue is that we have such a small sample size that it's very sensitive to the amount of -- As soon as it sees some large fish in the sample, it's kind of pushing the selectivity to say, well, if we see one, then we probably are selecting a lot, because that sample size is so small.

With a lot of back-and-forth discussion with the group, we decided to tighten, or essentially inform, the selectivity, but putting priors and then letting SS estimate with those priors, and now that red line is what you see, and so it's now more aligned with our understanding of the survey and more aligned with what we've estimated in the past.

One other change here, if you look on the right-hand side, is, in the last assessment, the selectivity at-age for this survey was set to zero for age-zero, and so it's saying that none of the age-zero recruits are being captured in this index. However, the index survey description shows that the original intent was to cover gag age-zero through three, and it is going quite inshore, and it is picking up on recruitment signals, which we noticed resembled 2006, that pretty high point, and that's indicative of a large recruitment event.

Therefore, I relied on this assumption as well, and I removed any notion of age selectivity and just let our length selectivity be translated into an age selectivity, and so letting the model capture some of those age-zero fish in the selectivity of the video

survey.

Here are the overall fits, and on the left-hand side is the new assessment, and so a few notable changes that I want to point out. Overall, the fits were actually pretty good in the last assessment. Overall, some are quite similar, but there are differences, for example, in the vertical line, and those are the discarded fish, on the top-left panel here, and that misfit occurred when we added the data sources, and so, even after trying to adjust the selectivity, I could not get a good fit to the discards without negatively affecting the fit to the retained catch for the vertical line. That is something that will need to be looked into in future assessments.

A lot of this misfits, if we break it down by year, happened in the last three years, and so it's possible that there was a change and that we're not capturing the retention curve quite a well, and maybe there was a change in the behavior, and so the discarding has changed, the behavior of discarding by length, and then the other thing I want to point out is the Panama City video survey, and so the reason why there's an uptick here is because I allowed age-zeros to be selected, but there are no --

There are almost no age-zeroes in the samples, in the length samples, and one of the thoughts we had is that the length sample started in 2009, and that is after that large recruitment event, and so it's not entirely representative of the entire time series of the index, and so, even though there's a misfit to the actual data inputs, we do think that it's a better characterization of the index and of the survey and that the population is trying to track.

Now, just another way to look at it, breaking it down by year, those are the Pearson residuals, and it's important to look at the scale here on the left-hand side. Residuals go form negative-two to two.

On the right-hand side, it blows up to a hundred, and that was in the last assessment, and so part of it is have been some improvements in the selectivity and retention and fits to other -- - One of the improvements is also the modeling and how the fish in the very smallest length classes are dealt with, and so, when you have sparse data, you can get those residuals going up, and so there were some improvements that had to do with the modeling as well, but you can see, on the left-hand side, we have a better behaved model.

Then, again, it's hard to see here, with all the very large

residuals in the private fleet, but you see, on the left-hand side, the residuals.

Now the age composition, and the fits to the age composition did not change all the much, and they're not great, and they were already not great in the last assessment either, and, like I mentioned, part of it is I really think we need to dig more into these datasets and figure out exactly how the sampling scheme is affecting our assumptions inside the model and if we're matching what was actually intended in the data collection, and so more work is needed here, and that is definitely something that I put in the research recommendations, but nothing terrible.

You can see the Pearson residuals, and some patterns are apparent, with positive residuals in the commercial longline over certain years of the oldest fish, and so some things will need to be looked into in more detail, but, overall, nothing too detrimental.

The data weighting, I did mention already the weighting of the indices, and now I am going to mention the weighting of the compositions, and so the way the Dirichlet multinomial works is you can set up a parameter for every fleet, or you can put a parameter and share between fleets, which I did in some cases, where the sampling is similar, and then the SS internally estimates a multinomial -- What's called a theta parameter, but I scaled it to something more interpretable.

This is a number from zero to one, and it gives you the weight that you should assign to those compositions, and so, if it's one, it's saying that your input sample sizes are a good reflection of the effective sample sizes, no change needed, and then, if it's lower -- In all case, those were all high, but, if it was 50 percent, 0.5, then it would tell you that the effective sample size should be much lower.

The reason why those are really high is probably because our input sample sizes were already downweighted, in a sense. The fact that I used trips, number of trips, or camera drops, and that, in certain instances, was able to even downweight those numbers of trips to the representative portion of trips, and so it's a smaller number, and so I think that's why we're not really seeing a huge amount of post-hoc weighting done by SS.

The annual exploitation rates estimated by SS are shown here, and, again, keep in mind that, when I show those summary plots, I do have red tide included, and so it's not just the fleets. In 2005, obviously, you're seeing this crazy jump here, which we observed in the last assessment, but now we're seeing 2014 and 2018, and

you can see there's a fair amount of uncertainty around those data points, which makes sense, and there is more uncertainty in the recent red tides, which also makes sense, because, as we add more information, for example age composition, that's really informative to the red tide events, if an age class suddenly disappears or some signal comes out of the indices.

You're seeing here, again, that it's estimated that the 2005 red tide was the most impactful one, and then gradually lowering here from -- If you look at 2018, you will find that there is a fair amount of uncertainty, but, overall, the trends in this exploitation rate are similar between the last assessment, in the sense that it ramps up from the 1970s to the 1980s, and then it plateaus, and then it gradually increases again, but the big difference, again, is in those last few years.

Whereas, in the current model, we're seeing fairly similar exploitation rates in the last five years than we see over the 2000s, in the past assessment, there was a big drop, and so there was a big difference between exploitation rates prior to 2011, essentially, to more recent ones.

Now this is just to show the relative exploitation rates of the different fleets, and the biggest thing to note here is, after FES, obviously, this private fleet now has a lot more of an impact, much higher catches, and so it's more prevalent in our assessment compared to the other fleets, and it's responsible for a higher proportion of the exploitation rate than it was in the last assessment, where the commercial vertical line was actually quite above it in the earlier years of the time series.

Then the other thing is I did note the kill from the red tide, and so the estimated kill from SS, which is 13.67 million fish in 2005, three million in 2014, and one million in 2018, versus, in the last assessment, it was 3.4 million killed, and now two major differences.

One is we have scaled the population up with FES, and so now you would expect a similar exploitation rate to have a larger number of fish associated with it, but the other thing is I also allowed the red tide to affect age-zero fish, and so that's part of why this number is much larger as well. In the last assessment, it was assumed that red tide did not impact age-zero fish. I am almost done with this section, and then we can open it up to questions.

These graphs show your total biomass on the left and your female-only SSB on the right, and, again, we saw it in the step-by-step

virgin analysis process, but the big difference is that the virgin condition -- The uncertainty is much greater now around especially those historical estimates, and, in the recent years, we're seeing a very different trajectory of SSB leveling off and being impacted by that 2014 red tide, which we did not see in the last assessment.

Now these are the estimated recruits and the recruitment deviations, and so in black is the main recruitment phase, and so that's the data-rich portion, and then these are the early recruitments, and, on top of it, we have the bias adjustment ramp that is applied at the end. Importantly, steepness is fixed, and so productivity is set to steepness to 0.855. The sigma R is also fixed to 0.6.

There were some explorations of steepness, estimating steepness, in SEDAR 33, which failed, with steepness bounding to one, and so this is actually -- This figures comes from the -- Now I am blanking on the source, but it's a prior that was developed that is believed to be most appropriate for groupers, and that is a decision that was adopted in SEDAR 33, and that was reiterated in the SEDAR 33 update, and that we kept in this assessment.

I did add a couple of slides here, and so I can redistribute this PowerPoint, and I got a little bit of feedback, and there was figures people were interested in seeing, and so this is the sex ratios, and so you can see here what SS estimates as the unexploited sex ratio, and so 32 percent male, and what we are at today, which is 1.4 percent male.

This is all estimated internally to SS, and, obviously, we're feeding in the transition rates and the different selectivities, and so this is the result of the sex ratio that is observed, and, interestingly, it actually matches our observations fairly well, which, again, I guess makes sense if we're inputting the transition rates, but the exploitation history is showing 1.4 percent male in recent years, and, from recent observations in situ, it's finding less than 1 percent males in the non-MPA regions, and I believe about 5 percent, 3 to 5 percent, males in the MPA regions, and so it seems to be in line with our understanding of where we stand.

Then, obviously, this big drop in males is a reflection of the ramping-up of the commercial fleets and the recreational fleets and the relative targeting of males through time, which increases as, obviously, we're increasing the minimum size limit and affecting females disproportionately compared to males.

Then, finally, I just added that in, because I know it's been discussed a lot in past assessments, if anybody was curious to see

the SS predictions on the mean weights for each of the recreational fleets compared to the observed data, which I obtained from either MRIP and then the headboat survey, and so, as expected, you see more variability in the data. SS is smoothing it out, but you do see what you would expect to see, and it's matching quite well with our understanding, or our observation, of the mean weights for these different fleets. Okay. I think that's a good place to pause.

CHAIRMAN NANCE: I think it's a good place to pause, and we will have questions after lunch, and so think about this last part of the presentation, and get your questions ready, and we'll come back at ten after one, Eastern Daylight Time.

DR. TOLAN: Mr. Chairman, if you will indulge me for just a second?

CHAIRMAN NANCE: Okay, Jim. Go ahead.

DR. TOLAND: Thank you. Lisa, when I had asked that question earlier about the SEAMAP cameras, I inadvertently called you Jessica, and so I wanted to correct that and to apologize. That's all, Mr. Chairman.

CHAIRMAN NANCE: Okay. Thank you, Jim. Have a good lunch.

DR. AILLOUD: Thank you.

(Whereupon, the meeting recessed for lunch on September 28, 2021.)

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September 28, 2021

TUESDAY AFTERNOON SESSION

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The Meeting of the Gulf of Mexico Fishery Management Council Standing and Special Reef Fish, Special Socioeconomic & Special Ecosystem Scientific and Statistical Committees reconvened on Tuesday afternoon, September 28, 2021, and was called to order by Chairman Jim Nance.

CHAIRMAN NANCE: Let's go ahead, and, anybody that would like for questions and things, please raise your hand. Roy first, and then Katie, Skyler, and Lisa --

DR. CRABTREE: I was just curious, Lisa, because it seemed odd to

me to see the red tide mortality characterized as an exploitation rate, because it's really not an exploitation rate, but I guess that's just because of the way that SS handles it as a fishing fleet?

DR. AILLOUD: Yes, you're correct, and it's confusing, and we brought it up to the Stock Synthesis team, and they are going to change that, actually, and so, in the newer iterations, it will come out as an actual mortality added in, most likely, and so I'm sorry, and that's very confusing to look at, and the reason that I was pointing it out is because, when we're calculating the benchmark, we have to be careful also that we're not -- I mean, it's up for discussion, but probably you don't want to include that F in your benchmark, if you're looking at your exploitation rate in the last year of the assessment, or, if you're averaging over years that have a red tide, you need to be careful, and so that's a point that I was going to bring up later.

DR. CRABTREE: That was the concern that came to me, is if, somewhere in all of this, the red tide mortality is getting lumped in with fishing mortality, because it shouldn't be. Okay, and so we'll come back to that then.

DR. AILLOUD: Yes.

CHAIRMAN NANCE: Benny, please.

DR. GALLAWAY: I just wanted to point out and reinforce the comment that Ryan made during the presentation. First, Lisa, that was a great presentation. I really enjoyed that, and, in fact, I advised my staff to download a copy of that, and that was really good.

The concern that I have is not from your presentation, but it's about how well we're censusing the recreational catch, given the number of people living at the coast, at least during the summer, with large vessels that I fear are probably not being censused. I think this is a subject that we all take up and come up with a solution to, because I think they comprise an important part of the fishery, and I'm not sure they're being intercepted appropriately for their strength. Thanks.

CHAIRMAN NANCE: Thank you, Benny. I think, Doug, you had your hand up?

MR. GREGORY: Yes. Lisa, I agree with everybody else. All these presentations, I'm just in total admiration of the stock assessment scientists and what they're doing now with Stock Synthesis and what Stock Synthesis can do and pulling everything apart.

My question is did the assessment panel discuss about how far back in time to go with the assessment? I know it's going back to 1963, and we expressed concern yesterday about that, and I noticed, in the red grouper, both SEDAR 42 and SEDAR 61, only went back to 1986, and 1986 is the beginning of the data-rich period, and I think that was done in -- Either done before the review workshop or done during the review workshop in SEDAR 42, but I am really concerned about going back in time when the data is basically generated through a bunch of gyrations, because of a desire to go back in time.

When we started doing assessments, that was necessary, but now we have thirty-five years under our belts, since 1985, or 1986, and I don't see the need to reach back in history and gin-up numbers, and I am curious as to what effect that would have on the status determination criteria if we were to run this assessment beginning in 1986.

I say that kind of thinking that it's not possible to do now, but I am really concerned about this model results that reach back, and amberjack was the same way, that reach back to 1963, where the data was faulty, and the data was inaccurate, and then adjustments were made through I guess a best practices workshop that tried to figure out how to adjust it, to make it seem more realistic, but it's still not good data, and so my question is was there any discussion about starting the assessment in 1986, like we do with red grouper?

DR. AILLOUD: Thank you for that comment. It did come up in the discussions, and the long story short is that, because it was an operational assessment, we did not feel it was the right time to play around with the start of the time series, because it would require a lot of thought and a lot of work to check what we're doing, because you're correct that, for example, for the recreational fleets, the data are less -- They are less precisely known from 1963 to 1980, compared with the MRIP period, which starts in 1981.

However, we do have vertical line commercial landings data that starts in 1963, and so that was actually the reasoning at the time, back in I think it's even SEDAR 10, for starting the time series in 1963.

To your point, I think it's a valid point, and I think it's probably something that will be considered in a research track assessment, but it was just outside of our purview for this operational, and then, as to speculating on how it could affect stock status, I

don’t think I can speculate.

I will say though that, even if you start your time series later, we do know that we’re starting it in an already fished state, and so there is some level, obviously, of hindcasting that has to happen, where we do have to estimate initial Fs, for example, for the fleets that were operating, and so we still have to figure out what was that state of the fishery at the time we started the assessment, and there is no really good way to get away with that, unless you have a time series that starts all the way back before the fisheries even started, and so I hope that helps answer your question.

MR. GREGORY: Right. Is it possible to -- Not this week, but is it possible in the near-term, to do a sensitivity run like that, without worrying about diagnostics and other stuff, and just see what the overall effect of that change in the starting year would have?

DR. AILLOUD: For SEDAR 72?

MR. GREGORY: Yes.

DR. AILLOUD: The issue is -- The problem is, if I do, I wouldn’t want to do strictly a sensitivity run, and I think it would have to come with all sorts of checks on like diagnostics and everything else, and so I don’t think that would be straightforward. I think it would need to be done with a lot more care than a quick --

MR. GREGORY: I understand.

DR. AILLOUD: It would require projections, also, to get the benchmarks.

MR. GREGORY: I certainly have concerns about this assessment without -- With the historical information included, when we really don’t need it, as far as I can tell. We did need it twenty years ago, or thirty years ago, when that data was first discovered. Okay. Thank you very much.

CHAIRMAN NANCE: Thank you, Doug. Benny, do you still have your hand up?

DR. GALLAWAY: Is it down now?

CHAIRMAN NANCE: Did you have another question?

DR. GALLAWAY: No, and the green arrow points up, which I assumed

was raising the hand, but the red arrow points down, and so it's down now, correct?

CHAIRMAN NANCE: It is down now, yes. Thanks. Okay. I don't see any other hands up, and so, Lisa, let's go ahead and continue on with the presentation. Harry, is your hand up? Okay. Harry.

MR. BLANCHET: This is to follow-up on Doug's question. It seems very similar to what we were talking about with the last one, which is looking at essentially what is your fishing rate with regard to the recruitment that you're currently getting, and so those SPR 30 type of analyses, and, yes, that's a very different issue, and so I think that, if you just carry forward all of those discussions that we had under amberjack, and just tuck them under gag, and it does seem like something that is necessary to have a research track, but it also needs to have the discussion about is this something that is feasible to do and appropriate.

CHAIRMAN NANCE: Thank you, Harry. With no other hands, Lisa, let's go ahead and move on.

DR. AILLOUD: Okay. This next section is looking at the diagnostics of the base model, and so we carried out a few kind of standard diagnostics. The first one is a likelihood profile, which I had brought up earlier, when I was talking about our choice to choose a larger CV.

You can see, on the right-hand side, the profiles, and I just zoomed in, and so it's the same plot, but I just zoomed-in on the bottom, because you really want to see is if there is more than two loglikelihood units difference in the choice of -- In the MLE of the different components that were brought into the assessment.

Here, you see that quite good agreement. You have maybe a slightly higher R0 suggested through the index data than you would through the catch, but, generally, those differences are just a few units of loglikelihood, and there is pretty good agreement between the data sources, and so that was a positive diagnostic on the model.

The second one we did was a standard jitter analysis, where you just -- You randomly change the starting values of your parameters within 10 percent of the base model fit values, and what you want to see -- Here, I have all the different model components, and the red line is indicative of the actual likelihood value in the base run, and what you really want to look for is in this bottom total likelihood. You don't want to find a run that would have a lower overall likelihood, or a negative loglikelihood, than your base model, and, indeed, we didn't see that.

A few of them go above, which is fine, and there is some parameters, and a lot of parameters are freely estimated, and so you would expect a little bit of movement, but you do not want to find a better model, at the end of the day, and so this was also a sign of stability in the base model.

Then, finally, we did a retrospective analysis. Here, you're peeling off one year of data at a time, and you're seeing how it changes the overall trajectory, and so I am showing you spawning biomass, and so, here, it's females only on the left-hand side, and then your recruits on the right, and then I will show you also the exploitation rate.

What you do not want to see is something systematic, and so you don't want to see, for example, that, for every year you peel off, you get better and better -- Higher and higher SSB or lower and lower SSB. What we're seeing here is no cause for concern, and there's nothing, really.

Sometimes the points fall below, and sometimes above, the base level of 2019, and so there is no pattern that should be alarming us here, and I also added Mohn's rho here, which is just a metric that you can look at if you're looking for kind of a number to say, okay, does it pass the retrospective analysis test, and, if you're interested, I put the literature here, the citation, for Hurtadi-Ferro et al. 2015, and you want your Mohn's rho to be between 0.15 and 0.2 for something like gag grouper, and so we're in good shape here as well. Then this one is for the exploitation rate.

I do want to point out that, for the SEDAR 33 update, there was an issue in the retrospective, at the time, and, mainly, if you looked at the SSB at the time, what was happening is that, for every peel, the SSB was going higher and higher and higher, and so, essentially, it was saying that we had a higher -- We thought SSB was higher than you would if you added one more year of data, and so that always raises cause for concern, and so it's good to see that that pattern at least has gone away now in this update assessment. I don't know if anybody has questions on the diagnostics before I move on to the sensitivity runs.

CHAIRMAN NANCE: We'll take any questions. There's none in here.

DR. AILLOUD: Okay. Then I will go through sensitivity runs and then take another break for questions. The first one is very straightforward. The panel wanted to see what was the effect of the new mortality vector, and so, as you recall, and I put it on

the top-right here, but our Ms are -- They were pushed up a little bit by updating the regression, the methodology, and the maximum age.

The result, unsurprisingly, is you have a bit of a scaling difference here, and so, when you increase your M, you are scaling -- That got scaled up a little bit, and the overall fit was improved with the lower M, but then there is nothing really drastically different, and, as you can see, the confidence intervals do overlap.

Combined video, we spent a fair amount of time on this one, because we were really interested in including the combined video in this update assessment and replacing our Panama City and our SEAMAP time series with this updated index, but I will walk through our thoughts and why we decided to not include it in the base run, in the end.

It's been successfully used for a number of stocks, but, with gag grouper, we have a big challenge. The challenge is that, if you look at the map, the surveys we're trying to combine in this combined index come from very different areas off the coast of Florida, and particularly from different depths, and so Panama City targets the inner shelf, and you can see those red dots, and Pascagoula purposely targets the high relief. They are looking for the largest fish, and then FWRI covers kind of the whole extent, but it's restricted to that middle part of Florida, which is not covered by Panama City.

The idea was, if we combine them all into one index, that would be really great, because then we would have a much more representative index that covers more of the area, but the challenge we ran into is that, because the surveys were covering different depths, they were actually tracking different parts of the population, and particularly your Panama City survey was picking up on the recruitment signal, whereas Pascagoula was really tracking fish of older ages, and so the original thought was --

What we tried to do is we included it as a combined index, but we broke up the selectivity. We put time blocks on the selectivity, to say that the selectivity of that index changed through time, and we based those time blocks on when the different surveys came into the picture.

On this plot, you can see the selectivity logistic here that is closest to us, and that would be the selectivity that defines the era where only the Pascagoula survey was in the combined index, and then you have a little sliver where you see that the

selectivity kind of moves to the left, and so it's starting to pick up on some younger, or I should say smaller, fish.

That is when Panama City comes in, but FWRI has not yet started, and you can see that FWRI starts in 2010, and then the further one, that is hard to see, is another logistic curve that is estimated when all three surveys are operating at the same time and they are combined, and so, in that time block, your selectivity -- We estimated it to be, again, slightly pushed to the right, and so a little bit closer to what it is at the beginning of the time series.

It was kind of a modeling solution to a problem that we were observing, but we want to make sure that our model knows what we're tracking.

In the end, it worked fairly well, but what you do when you break up an index, when you break up the selectivity, it's kind of the equivalent of breaking up the index, and you're losing information, and so I think that will be a very interesting point to look at, again, in the next research assessment, because we talked about a prospect, for example, of, instead of combining all the survey data points, maybe splitting into an offshore and an inshore index, so that it has more information that the assessment can make use of, without having to break it up like that.

If you're interested to see the overall fit, it was actually pretty good, and it followed the -- It tracked the index pretty well, and it really didn't change anything all that much, except perhaps that, in the sensitivity run with the combined index, there was no detection of that recent red tide, which is -- You see between the blue here and the red, and so that is one difference, and, obviously, we do know that 2018 had a red tide impact, and so it was interesting to see that that was not being picked up. Our conclusion was to keep this as a sensitivity run and to keep the Panama City survey index and the SEAMAP survey index separate in the base run.

Now we're moving on to our red tide selectivity runs. This first one, if you recall, all I did was I kept my bycatch fleet, and so the red tide has a fishing mortality that is associated with it, but what I'm doing now is I am providing it information on the selectivity, and that is coming straight from the Ecosim model.

Instead of selecting all ages uniformly from zero to maximum age, I am now shaping the selectivity, and you can see, on the left here, that it's really selecting mostly age-zero through five, and it really drops from four to five, and then, at five-plus, I have

set it as an equal selectivity. Selectivity changes for every year the red tide comes in, because that comes from the ecosystem model.

Unfortunately, this run did not go as we would have expected. We actually ended up -- If you look here, these are the apical Fs, but just to show you like which years the model was picking up a red tide, and you see that it's very different from what we were -- The information we gathered from stakeholders. We're starting to see something in 2011, and then not see it in 2014, and it was picking up multiple years around 2005, and so it was very messy and not very -- It was very hard to interpret, and, when we looked at the overall trends, the overall trend in SSB wasn't too different.

Maybe it was observing a little bit -- The differences in certain years around the red tide, the years 2005 and 2014, but, really what concerned us here is we just weren't very happy, or comfortable, with the model results, and we just decided that it's probably not the best approach to red tide, and one likely issue we're having here is we're now opening up the red tide to a lot of years and not giving the model a lot of direction, and so it can kind of move around, and this probably, I would guess, not a stable model, if I were to do diagnostics on it.

We looked at kind of an improved way to do it, and that is the one that Dave talked about, and that was a really interesting one, because what we got from Ecospace is on the left here, which Dave showed you, but you can see the red tide mortality estimates by age group and by year.

What I did is, in SS, just like we put time blocks on those minimum size limits, you can put time blocks on other parameters, and so what I did is I took my -- I took out the bycatch fleet, and so red tide is no longer a model of the fleet, but what it is modeled as is additional sources of mortality on the natural mortality vector, and so we have our baseline natural mortality vector that is fixed, that Lorenzen curve, and, on top of it, I allow SS to add mortality in certain years.

To give SS guidance, I put priors on those additional mortality parameters, and so, for each age, and each year, I allowed SS to estimate a little bit more natural mortality that would be explained by red tide, and the priors were constructed by simply taking the means from the ecosystem model and the uncertainty and creating a normal prior around those additional Ms.

Two things to keep in mind, and one is Ecospace goes from -- The

red tide goes from 2002 to 2018, and it did not detect a red tide in 2010, and so those were the years where I set those blocks, and so I am not putting any red tide before 2002, and the other limitation is that I could only -- I added blocks on each age group up to age five, and so I am assuming, in this run, that age-six and above are not affected by red tide, which is probably not the best assumption, and something we would need to improve, but I was just trying to be parsimonious. It's a lot of parameters every time you add a time block.

What did the results show? They showed that the biggest improvement was the fit to the indices, and particularly the age-zero is really catching on some peaks here that were not being tracked in the base model as well, and so that was really encouraging to see, and the same with the charter private, tracking that index more closely in the late 2000s, and then also in the early 2000s here, and we can see the differences, and so that was really encouraging.

Unfortunately, there were other things that we could not explain, and so that's one of the reasons why we kept it as a sensitivity run, but suggested that it needs more work, and it's promising, and it could possibly be included in the future in a base run, if we can deal with some of these issues.

One issue, which Dave talked about, and he said 2014, and that's my fault, because I think I told him 2014, but it's actually 2013. If you look at the expected catches here for the private fleet, there is a -- The expected catches in 2013 are really, really high compared to the observed, and the same with the discards, and they're higher.

We are not sure what is going on, but clearly the model has to kill some fish off, and it has to do it before the 2014 red tide, and so seeing this issue -- There could be a lot of explanations for it, and one could be maybe the 2014 red tide was stronger than what is seen from satellite data, what we can interpret from it in the ecosystem model, and it could be other things. Maybe there is hypoxia happening, or maybe it's just model misbehavior, but, in any case, it kind of caused us to keep this at a sensitivity run and just think some more about it.

These are the overall trajectories, if you want to see the effect on the spawning stock biomass trajectories. Again, it's slightly higher virgin conditions estimated, but the uncertainty bands overlap here, and then, post-2010, we see a little bit more of a recovery in SSB than you would in the base run, and then you see a larger drop, due to the 2014, and these are the differences in

the recruitment as well. Then, finally --

MR. RINDONE: Lisa, can we interrupt you just for a second? We've got a couple of questions.

DR. AILLOUD: Yes.

MR. RINDONE: Thanks.

DR. CRABTREE: Lisa, maybe I got a little confused. I heard you say that some of the red tide peaks weren't matching up with what you had heard from the fishermen and when we know the red tide was there, but that was with reference to the sensitivity run and not the base run, right?

DR. AILLOUD: That's correct.

DR. CRABTREE: Okay. Thank you.

CHAIRMAN NANCE: Harry.

MR. BLANCHET: I did not realize that I had my hand up. Sorry.

CHAIRMAN NANCE: Okay. That's fine. Thanks. Okay. Go ahead, Lisa.

DR. CHAGARIS: Mr. Chair, I have a question.

CHAIRMAN NANCE: Dave, I'm sorry.

DR. CHAGARIS: For some reason, my raise-hand feature went away.

CHAIRMAN NANCE: I think that's what we did, but go ahead. No, I'm just kidding.

DR. CHAGARIS: I just had a question on this topic, or maybe just some speculation, but I am wondering if, because we didn't assign the -- We didn't place any red tide mortality on fish six years old or older, if maybe it was really wanting to bump up the landings in that year to account for mortality in those older ages, if that was maybe a potential explanation there.

DR. AILLOUD: Thank you. That is correct, and that is also another hypothesis. Yes, it's probably not a fair characterization to say that anything above age-six is immune to red tide, and so certainly that could be one of the reasons why the model is trying to kill more fish.

That could probably be improved by -- If we don't want to add too many parameters, it could probably be improved by doing some mirroring, and so borrowing parameters from age-five and mirroring it over older ages. The initial thought was just that it was way too many parameters to estimate to put it on every single age group, but that's a fair point.

CHAIRMAN NANCE: Okay. Thank you. Dave, because you're a panelist, I'm being told that you can't raise your hand, and so just go and just interrupt, when you see an opportunity, and we'll be able to recognize you.

DR. CHAGARIS: Okay. Thank you.

MR. RINDONE: That goes for anybody that's a presenter. When you're changed over to a presenter, or a panelist, in the system, it removes your raise-hand function.

CHAIRMAN NANCE: Okay, Lisa. Thank you. You can go ahead. Jim, did you have your hand up?

DR. TOLAN: Thank you, Mr. Chairman. Lisa, can you go back just one slide? I just wanted to get something clear in my head. On the right-hand panel, when it's looking at age-zero recruits relative to the virgin condition based on the red tide sensitivity run, am I interpreting this correctly, in that, in times where we have red tide, that's in the model, basically the 2000s forward, the reason why those numbers are bigger than the virgin is what we get these big -- What the model thinks is big inputs of recruits after a red tie, and I just want to make sure that I understand that.

DR. AILLOUD: That's correct, and that's something that Dave observed in his Ecosim model and hypothesized that it's decreasing competition, perhaps. Dave can probably elaborate, but we seem to see those signals in the Ecosim model after 2005, and then you see it here again after 2014.

DR. TOLAN: Okay. Thank you very much.

CHAIRMAN NANCE: Okay. Thank you. Go ahead, Lisa.

MR. GREGORY: I have a question.

CHAIRMAN NANCE: I'm not seeing your hand. I'm sorry. Doug.

MR. GREGORY: Just an observation, but, since we've got this recruitment slide here, and you can look at any of the recruitment

trends, throughout this timeframe of this assessment -- I read somewhere, I think, that the sex ratio of male/female continued to reduce, and there's been concern about sperm limitation, and the lack of males, going back to the late 1990s or early 2000s.

What is curious to me here is that we've got this large number of years of an increased recruitment, which, to me, kind of refutes that maybe sperm limitation is a problem. If it was, I would expect recruitment to be getting worse and worse as the ratio of males to females decreases, and that's all. It's just an observation and not a question.

CHAIRMAN NANCE: Thank you, Doug.

DR. AILLOUD: Thank you, Doug.

CHAIRMAN NANCE: You can go ahead, Lisa, with the rest of your presentation, please.

DR. AILLOUD: Yes. Okay. GRFS, and so just to make a note here, and an apology, because, as I was going through all my runs, I didn't realize that there was an issue and a mistake that I made in the sensitivity run for this.

I picked -- There was a lot of back and forth on getting this time series, and, unfortunately, I did not use the final, final version, and so I reran it, and this is correct. This is the correct sensitivity run. Thankfully, if you have the old slide, you will see that it did not change the end time series, the end results, but you will see some differences in the catch, especially in the earlier years, because what I messed up was not getting the time series that was corrected for black grouper misidentification, and so just a note here, so that you're not confused of why you're seeing different numbers.

For GRFS, a few steps needed to be taken. The calibrated time series that you see in green, this is not the raw GRFS data. This is the private and shore fleet, and so what I had to do, because I had a fleet that was -- It wasn't just Florida private, which is what GRFS is tracking, but it was private plus shore, and it had other areas than Florida, even though it's a majority Florida.

What you're seeing here is that the GRFS time series is essentially a lower catch series, and lower discards, in terms of magnitude, compared to the base run, and I also -- Because I was given 1981 to 2019, I did have to make some assumption about the historical time series, because I needed to match the assessment, and I couldn't just say there was no catches prior to 1980.

What I did was I took the same approach that was taken in the base run, meaning that I looked at the average CPUE from the first five years, 1981 to 1985, and used that as a scalar for the historical time period, and then I reapportioned my historical time series according to fleets according to the new ratios of fleets, and so how much private there was, compared to the charter boat, compared to headboat.

Then, on the right-hand side, you see the results and how it affects the overall model-estimated time series, and so you have your fraction of unfished, and then you have here your spawning biomass, which is female only, your exploitation rate, and the age-zero recruits.

Notably, which is not surprising, is the scaling of your spawning stock biomass and your age-zero recruits. When you're using the sensitivity run with a lower -- With the lower amount of catches and discards from GRFS, everything scales down, right, and so it's the same that we saw when I was doing the state-by-state process and going from the old MRIP time series to the FES, and so that was really the major difference here, and then there's some smaller differences, if you look at that year-to-year basis. We'll pause here to see if there's any other questions on the sensitivity runs.

CHAIRMAN NANCE: You bet. John.

MR. MARESKA: Lisa, thank you for the level of explanation that you're providing in your presentation. I have a question in regard to the Ecospace model, and I may have missed it in your explanation, and so, those mean mortality rates that were coming out of the Ecospace model, how did you restrict them in SS so that they were only applicable to the cells from which Dave was able to estimate them from? I'm just making sure that that additional mortality wasn't applied across the entire stock.

DR. AILLOUD: The outcome -- Those are mean estimates coming by age and by year, and so they are already averaged over all the cells of the Ecospace model, which -- I guess I'm trying to see where you're getting at. Are you referring to the fact that the Ecospace model has a kind of gray zone, where it's restricted to only part of offshore Florida?

MR. MARESKA: Yes.

DR. AILLOUD: Versus the catches could -- Okay. Well, that I did not -- I don't know how much that would affect it, and I did not make -- I made the assumption that those mortalities estimated

from the ecosystem model would be applied in my overall Gulf of Mexico model, which, for ages-zero through three, given where they are in space, is probably a good assumption, but maybe Dave can speak to whether that might be an issue for older ages.

DR. CHAGARIS: For gag, we're pretty comfortable assuming that most of the population is on the West Florida Shelf. That's where all the landings come from. I'm sure there is probably a portion of the population outside of our model domain, but these are -- The mortality rates coming out of the Ecospace model are over the entire population, and so we took the total biomass loss divided by the average biomass across the years, providing the overall cells, to get the mortality, and so this is a -- Those were meant to be population-wide estimates of mortality.

MR. MARESKA: Thank you, Dave, for that explanation.

CHAIRMAN NANCE: Any other questions? Okay. Lisa, let's go -- Harry.

MR. BLANCHET: Sorry, but this goes back to the retrospective analyses, and the comment was made that the Mohn's parameters were in the acceptable range, and correct me if I'm wrong, but, when you're looking at those parameters, that is considering what happens to essentially the history of the stock parameter, whether it's biomass or recruitment or whatever, over the range of the assessment.

For a lot of what we are doing -- Where I see that parameter as most useful is how much change is going on, especially in those early years of the assessment, where we really don't have a whole lot of information, and is the model relatively consistent in those estimation parameters.

Not so much where we typically look, which is the most recent years of the assessment, and so, if we look at the spawning biomass, with that retrospective, what we see is that, at least in the final year of the prior assessment, our estimate of spawning biomass was a lot more optimistic than our current estimate is, and that, to me, is -- When I look at it, and we're talking about what kind of advice can we give to the council, it's that additional years showed a different picture of the current status than what we saw when we last ran that assessment.

While the parameter might be acceptable for the particular purpose that we're using it, I think that's something more than needs to be said, and that's all.

DR. AILLOUD: Thank you for your comment. You're correct, and I actually think that -- I am actually not surprised that we were being too optimistic, because you really can't see the impact of that red tide on the stock until the age composition and the length composition and everything comes in to indicate whether SSB was impacted, but I guess what I meant by there is nothing alarming here is that it is not directional.

It's not like every year it goes lower and lower, and it actually comes back under in the 2017 peel, and then it comes back up in 2016, and so that's where I was saying that I didn't think it was cause for concern, because it's not a systematic decline.

MR. MARESKA: Okay. It's not very easy to see in that particular graph there. It's a lot easier sometimes to look at the tables.

DR. AILLOUD: Yes.

CHAIRMAN NANCE: Okay. Thank you, Harry. Let's go ahead and -- There we go.

DR. AILLOUD: Okay, and so I just have one quick slide on overall conclusions, and I brought it up before, and so I will go quickly, but I did have some text here from the last assessment, because I do know that, obviously, the stock status and the end year estimates of spawning stock and the trajectories are quite different from the last assessment, and so I did want to just bring up some text showing that there was some instability in the last assessment, that there was some concern over the retrospective pattern.

I do think, in that sense, we have taken this model one step closer to stability, and we have addressed the TORs in the timeframe that we had available. We're seeing improved fits in a lot of the diagnostics, and, that being said, I also think we uncovered a lot of questions and a lot of things that could be influential and really need to be looked into, and so we do have quite a long list of recommendations of aspects that could be more thoroughly explored in a research track and take this a step further in our improvements.

CHAIRMAN NANCE: Go ahead, Lisa.

DR. AILLOUD: Thank you. I brought up the terms of reference, just so people have it, and the last assessment used Fmax as the status determination criteria, and so these are the guidelines that I followed here that you're going to see, and then we can discuss the assumptions that come into the projections.

Before I go into showing you the differences between assuming a female-only contribution to reproductive potential versus a combined male and female, I just want to lay out some arguments that were brought up way back, even in SEDAR 33, or even SEDAR 10, and were already topics of debate.

To summarize what we know, and what we understand, we have two options in the modeling framework. We can characterize the reproductive potential as female-only mature biomass or males-plus-females mature biomass.

Now, if you assume female-only, which is often done for non-hermaphrodites, you're basically saying, in a hermaphroditic framework, that it doesn't matter how hard you fish the males, but the females will not -- The reproductive potential of the stock will not be affected.

The assumption you're making is that decreased fertilization is weak, and, if you do not believe that to be the case, of if you have evidence to show that maybe there is sperm limitation, or that the stock is negatively affected by the fishing out of the male population, and you believe that there is potential for decreased fertility to be either moderate or unknown, the suggestion from Brooks et al. is to use combined SSB.

Now, what we're seeing in the sex ratio, which was brought up, is that we are pretty far from what were the conditions in the virgin stock, where we had about 32 percent males, and now we're down to 1.4 percent, according to the estimates, or 1 to 5 percent, according to field observations, and so there is a risk that we have reduced fertilization going on.

In terms of what evidence we have, well, we do see that it doesn't appear that gag grouper is compensating for what is happening, and we're not -- It's not regaining, or changing, the age of transition enough to counter the impact of the fishing, and so we're still at 1 percent to 5 percent. We also have evidence that we don't have a lot of strong year classes since 2006. There is one in 2010, and so that text is -- I probably should have updated, and there is one in 2010 that you can see in our bubble plots, but, since then, not much at all, and so, in nine years, we're not seeing a strong signal.

Then the relationship between the sex ratio and the fertilization success is generally poorly understood in gag grouper, and so, keeping that in mind, I can stop here, if there's questions. Otherwise, I can go ahead and show you the implications of each of

the assumptions.

CHAIRMAN NANCE: There are a couple of questions here. Roy.

DR. CRABTREE: Lisa, I think I heard you say that the estimate of the percent of males in the unfished state was thirty-something percent?

DR. AILLOUD: Yes, 32.

DR. CRABTREE: Where does that come from, that number?

DR. AILLOUD: In SS, we're giving it a transition rate, and, obviously, it's seeing the age composition in modern times, where we have age information, and so it's able to attribute how much -- - In numbers of fish, how many fish we have that are males and how many are females, just by using that transition rate, and, when you go back in time, knowing the selectivity of the fleets and whether they were fishing out the smaller ages or the older ages, that's where that sex ratio comes from. It's the history of the fishing, and then it's scaled -- For the virgin biomass, it's scaled from that 1963 year to an unfished condition.

DR. CRABTREE: I remember one study, and I think it was Peter Hood that did it, but I don't remember when it was done exactly, but it was many, many years ago, and they looked at the percentage of males. Thank you.

DR. AILLOUD: I think there was one -- The literature I found was from 1996, Coleman, where they estimated the sex ratio to be around 17 percent in the late 1970s, and then down to 1 percent in the early 1990s.

DR. CRABTREE: Well, there is a Hood and Schlitter paper, I think, and I don't remember the year, and I don't remember what species they looked at, but they looked at one of the grouper species with an old dataset and looked at the sex ratio. Okay. Thanks.

CHAIRMAN NANCE: Okay. Any other comments? Lisa, let's go ahead then on to the projection settings.

DR. AILLOUD: Okay, and so these are the projection settings that you've seen in the report, and so, in terms of relative F, and so that is the relative F between the fleets that is projected forward, and we took the average from 2017 to 2019, and so the average of the last three years.

Red tide is excluded, and so back to the concern about the bycatch

fleet that is not included in there, and selectivity -- Typically, or at least in the last few assessments, it was averaged out over multiple years for selectivity and retention, and now the issue we're having here is that there's a time block in 2019, because there is a minimum size limit that came into place from the commercial fleet, and so the initial setup was to just take selectivity and retention from 2019, and so that can be open to discussion, if you have opinions on this decision.

Next is recruitment, which we've talked about for greater amberjack, and so probably something that will need to be discussed, but, for at least these figures that I am showing you, we took the -- We derived the recruitments from the Beverton-Holt stock-recruitment relationship, which keep in mind that steepness is fixed in this case, but there is a relationship embedded in the assessment.

The interim landings, I actually did have landings provided for up to 2020, and I was informed that there might be a certain level of imputation in the MRIP catches, due to some issues in being able to survey fully during COVID times, and I'm not sure how it affected the commercial fleets, but, in any case, I do have data that was provided until 2020.

For 2021 and 2022, the panel decided to use a three-year average, which was 2018 to 2020, and so you see the numbers on the middle panel here, and, finally, allocation ratio, which I did not change from the last assessment, but it was 39/61, the commercial to recreational.

CHAIRMAN NANCE: Thank you. Any comments to the projection settings that are here? Roy.

DR. CRABTREE: On the F reference point, Fmax, how different is Fmax from F 30 percent SPR, and I remember that it was Fmax, but I can't remember why we used Fmax to begin with, but do you know how close those two Fs are?

DR. AILLOUD: Unfortunately, Fmax is very different than SPR, if you consider the two different SSB scenarios, female-only to SSB combined, and so, if you look at this figure here, in this case, the Fmax itself is not that different between scenarios.

However, an SPR would be much more sensitive to your definition of your spawning stock, and, therefore, the SPR equivalent of your Fmax is going to be very different if you're looking at a female-only scenario, where it's SPR 31, and so, like you say, in this case, female-only Fmax and SPR, percent SPR, are very similar, 30

percent SPR, I mean, or 31. However, in the case where we're doing SSB combined, because Fmax is not sensitive to that spawning stock, you're going to get a very different answer, and so we're now at 16 percent SPR.

CHAIRMAN NANCE: Okay. Thank you. Doug Gregory, please.

MR. GREGORY: Again, just to follow-up with Roy's question, Fmax came from the very first stock assessment done with gag. Why it was done different than red grouper, I don't recall, but it's just stuck all these years. Thank you.

CHAIRMAN NANCE: Thank you. Luiz.

DR. BARBIERI: Thank you, Doug, and just to add, from memory, as I'm drawing from memory here as well, but my recollection is that, when that happened, because we are looking for an appropriate proxy for MSY, for FMSY, and the F 30 percent SPR was, for some weird reason that I cannot really fully explain, and I can't remember what the reason was now, but it was coming up much higher, unreasonably higher, than would be expected, in terms of being a proxy for FMSY, and the value of Fmax was coming up much more reasonable. It might have to do -- If it's explained with the differences in sex ratios, and, one way or the other, it was --

DR. CRABTREE: That's consistent with my memory as well, but do you know -- In the past, have we used female-only for gag, or have we done sexes combined?

DR. BARBIERI: As far as I can remember, we used only female-only, and we haven't been able to. I think we're going to get into that discussion in more detail a little later, but yes.

CHAIRMAN NANCE: Will, please.

DR. PATTERSON: Luiz's recollection about the Fmax versus F 30 percent SPR is how I remember it as well. In the last benchmark assessment for gag, to Roy's question, when we discussed this within the SSC, the issue was there was some sentiment expressed that we should be using combined sex, because of the issue of potential sperm limitation that had been discussed, although there wasn't any direct evidence of it, but there was concern, given the very low percentage of males estimated currently in the population.

There were really stark differences between the results from the combined sex versus female-only, and one of them was really, really pessimistic, and one of them was much more optimistic, and we picked the female-only model, which, in hindsight, may have been

a mistake.

CHAIRMAN NANCE: Luiz, to that point?

DR. BARBIERI: Just to that point, real quickly, exactly, Will, and my recollection with that discussion was it was from the original SEDAR 33, and then, of course, we had to revisit it during the update. The reason was that the sexes combined, even though we felt that that would be the most appropriate way to go, was not providing plausible results.

It was showing some results that were completely outside the realm of reality, we felt, and so we just felt that we didn't have really any other choice but to go with the model that was more reasonable, in terms of results.

CHAIRMAN NANCE: Okay. Any other questions? John, please.

MR. MARESKA: So the selectivity and retention, it looks like, in 2018, that size limit went into the commercial sector, probably August 1 or somewhere around that time that it was passed, and so I'm just wondering -- Maybe we should consider 2018 and 2019, because that gives you two years of the recreational sector and then a year-and-a-half of the commercial sector under the current regulations.

I remember, maybe in a presentation that Sue Barbieri gave, about -- I guess there was a timing when there was the ontogenetic shift, where some of those four-year-olds were beginning to move offshore to the spawning grounds, and I think that was in the latter part of the year, and so there may be more reasons to also select that 2018, the latter half of the year, and I don't think, in any of the years -- No one is catching their allocations, and so the effort is still ongoing, and so that's just for consideration.

CHAIRMAN NANCE: Thank you. Will, go ahead, please.

DR. PATTERSON: I just have a question here about the percent SPR equivalent that is being calculated here, and so we had some discussion yesterday about what is SPR, and different metrics have been proposed as being functionally equivalent to SPR, and Nathan Vaughan showed us that's not really true, and that we need to be very careful in what we're calculating and putting forward. It appears here that this SPR that is being proposed is total biomass and not based on egg production. Is that correct, or am I missing something?

DR. AILLOUD: That is correct. We don't have egg production in

this model, and so our fecundity equivalent is our spawning stock biomass, but the reason why you're seeing such a difference between your Fmax and your SPR is that SPR, in a sense, is trying to preserve the reproductive potential of the population, and Fmax doesn't do that. It's looking at your yield, and so that is the root of why they are so different.

CHAIRMAN NANCE: Okay. Thank you for that explanation. Any comments on these, on the benchmarks here and the reference points? Roy.

DR. CRABTREE: Lisa, when I look at F current, it says geometric mean of the last three years of the assessment, including red tide mortality, and that does not seem appropriate to me. The F current should not include red tide mortality, because the red tide mortality is natural mortality.

DR. AILLOUD: Yes, and I agree with that, and I actually just pulled this from the report, but, yes, after discussions on that, after the report was submitted, we had the similar discussions, and, yes, I think you're correct. I don't think it should be included in the F current.

DR. CRABTREE: Okay, and so then that leads to how different is the F value, and how does it affect the overfishing status in the next column?

DR. AILLOUD: It does not affect the overfishing status. It's slightly different, but it's not very different, but it still makes the stock overfished, with overfishing occurring, in both SSB female and combined scenarios.

DR. CRABTREE: But, I mean, it would affect the value in this table for F current over MFMT, right, because that's --

DR. AILLOUD: Yes.

DR. CRABTREE: Okay.

DR. AILLOUD: I can pull the number during break, but it's a small difference.

DR. CRABTREE: Okay.

CHAIRMAN NANCE: Okay, but it should cause it to come up a little bit, right, F current?

DR. AILLOUD: Yes.

CHAIRMAN NANCE: Okay.

DR. AILLOUD: No. Your F current will come down a little bit, and, therefore, your F current over MFMT will be slightly lower, because it does not change Fmax, because that is calculated excluding the red tide.

CHAIRMAN NANCE: Okay. Let's go to the next slide then.

DR. AILLOUD: Okay, and so, here, I have plotted the projection time period, as well as the assessment time period, just to show you, and it's what Nathan illustrated, is that, when we're projecting forward, we're trying to reach this SSB Fmax on the long run, and so you see here what those SSB Fmax values are for each scenario, and the MSSTs, and, like we noted, if you are using Fmax, you're actually not going to see a drastic difference in the time series of being overfished in both of the scenarios.

However, I suspect that, if we were to do an SPR benchmark, then the picture would be different, which is what happened in what I believe was the benchmark, where an SPR equivalent was extracted from the SSB female-only run and applied then in a combined scenario, and so I don't think that was done quite right, because, instead of recalculating the SPR for the combined scenario, it actually just borrowed the SPR from the female-only, and it brought you a completely different picture.

Just a grain of salt here that, because we're using Fmax, you're not going to see a huge difference in the history here, and so what we're seeing is that SSB has been -- In the SSB female scenario, SSB has been overfished since 2006, with the exception of 2011/2012, and overfishing occurring since 2001, although there are a few years that fall right below Fmax. Then, in the combined scenario, SSB has been overfished since 2006 as well, and overfishing has been occurring consistently since 2001.

Again, a word of caution, because these are pulled from the report, but the Kobe matrices do include red tide, which is why you're seeing some peaks, but this is the general trajectory of the stock through the Kobe matrix here, and so the stock experienced overfishing for twenty-two of the fifty-seven assessment years, and consistently since 2016. Should I pause here?

CHAIRMAN NANCE: Yes, let's go ahead and pause here. Thank you. Let's go ahead -- We need to know, I think -- We've seen the presentation to this point, and we can have a discussion on whether this assessment represents the best scientific information

available, and does it provide us a status of the stock to present to the council, and so that's the discussion we need to have. Roy.

DR. CRABTREE: I have a question on when you're doing the projections. We've got a fishery where we know we have these periodic red tides that kill a substantial number of fish, and so, in fact, natural mortality is higher than the base natural mortality rate. When you do the projections though, you're not factoring in any additional natural mortality resulting from future red tides, and so doesn't that mean --

CHAIRMAN NANCE: Lisa, go ahead and comment on that, please.

DR. AILLOUD: Sorry. Yes, you're correct, and that is something that I was going to bring up for discussion, because, according to Dave's presentation, there does seem to be an impact in 2021, and so we might want to think about how to include that in the projections.

DR. CRABTREE: Yes, and it seems to me that we're -- We're essentially saying that the cumulative natural mortality rate that is in the assessment is lowered in the projections, and I don't know how much of a factor that is, but that would effectively be what we're doing.

CHAIRMAN NANCE: Luiz, did you have a --

DR. BARBIERI: Yes, and I have several points that I think -- For us to bring up for discussion as we consider the base model and then make our decisions, in terms of accepting the assessment as-is or whatever recommendations we have.

I think one of the main decision points here is this SSB females only versus SSB combined, right, and, in my view, going with the combined would be the way to go. I think that Lisa and the Center were able to really structure this model in a way that it handled the SSB combined in a better way and produced results that, to me, are credible, that are not outside the realm of reality, and so I would feel more comfortable going with the SSB combined, considering all the issues that we have been discussing regarding sex ratios and the potential problems with low numbers of males out there and the sex ratios that have been depressed. That is just one issue, Mr. Chairman, and maybe we start there.

I mean, another point, and it would be good if Katie is also still on, just to help us think this through, in terms of workload for the Center and the issue of this being an operational assessment, versus something more -- With more detail, like a research

assessment, but I think that this issue of Fmax versus F 30 percent SPR needs to be revisited. For some reason, we have evolved from using old models to now being an integrated model, like SS, and being able to account for all these different things, and we're still using Fmax, which doesn't seem to align well with the reference points that we use for any other stocks that we provide catch advice on.

To me, this is something that deserves to be discussed, and, if there is a way for us to see what the results would be if we're using the 30 percent SPR proxy for FMSY, I think that would be extremely informative, and I bring this up because I was looking at the SEDAR schedule, and I was thinking, if this schedule stays as-is, we're not going to have another gag assessment until 2026, at the earliest.

Whatever management advice we provide now, it's going to stick for a while, and so it's not something that we can just expect to have corrected next year or the year after, and then I have some additional comments, but I will stop there, so we can handle those two.

CHAIRMAN NANCE: Let's go ahead with the issue of Fmax and F SPR, and also on whether we do the female only or the combined. Steve.

DR. SAUL: Thank you, Mr. Chair, and thank you for that presentation and for the hard work on assessing this species, and I know that this particular one comes with a lot of caveats and a lot of challenges, and this update, this new operational assessment, represents quite an improvement over past assessments, and so kudos to all that were involved in this work, and so that's great, and I think that lays a solid foundation for us, as the SSC, to discuss management based on it.

Like Luiz, I also wanted to echo his concern for using Fmax versus something like an SPR 30 or 40 percent benchmark, and so that is something that I think is worth us, as an SSC, discussing and whether we want to, as Luiz said, sort of continue in that sort of way with other species or not.

I personally feel that other reference points are a little bit better both estimated as well as better metrics, and so a little bit more conservative, in terms of not worrying about overages and stuff, and so I was leaning towards advocating for more of an SPR-based reference point, if that's possible.

Then I was also -- I also really commend the methodology of sort of iteratively building the stock assessment model, and so that

was really useful, sort of with the analysis that you all went through, and I would be curious to see like a table of likelihoods kind of throughout those iterative steps of building the model, if that's the best available. I think I will stop there. I have a couple other comments, but they are unrelated, and so I will stop there for now, for discussion. Thank you, Mr. Chair.

CHAIRMAN NANCE: Thank you. Let me ask you this, just as a -- In the terms of reference, we're talking about MSY proxy equal to Fmax or F rebuild, and so does that preclude us from looking at other things?

MR. RINDONE: For this assessment, yes. Using a different proxy for FMSY is a pretty remarkable deviation from our terms of reference, and so it would change an awful lot to do that, and so, at least for this operational assessment, Fmax and F rebuild are what you're looking at, but that certainly doesn't stop you guys from making a recommendation for the next time the stock is examined that alternative proxies for FMSY be explored.

CHAIRMAN NANCE: Okay. Any other comments on what we're talking about from the virtual world? Jim Tolan.

DR. TOLAN: Thank you, Mr. Chairman. Before we get too far away from the point that Dr. Crabtree had raised, I just wanted to reiterate the point of folding that red tide mortality into the natural mortality coefficient as it goes to the projections, because we see a very similar situation here on the Texas coast, in terms of red tide, and, from the 1970s to the 1980s and to the 1990s, in what we're calling sort of this data-rich period for most of our stock assessments, we've seen an increased frequency over the decades in this red tide.

If that's going to be the case going forward, how does that get folded into the projections, and so I don't want to get too far away from that, but I commend Dr. Crabtree for bringing that up. Thank you.

CHAIRMAN NANCE: Thank you. Katie. Then I've got a question for you. Are you going to be able to walk us through the decision tree for gag?

DR. SIEGFRIED: Thank you, Mr. Chair. I can certainly do that, and I can tag-team that with Lisa. I was just going to make a few comments, in response to what's already been said.

CHAIRMAN NANCE: Absolutely.

DR. SIEGFRIED: A number of you were either attendees or on the actual panel for gag, and you will know that the total -- The combined SSB is what was recommended by the panel, and we had Sue Barbieri as our reproductive biology expert on that panel. We thought that was a pretty well-informed decision, but Lisa then did the extra work of carrying it through, so that you all would be able to compare, of course, with what was provided in SEDAR 33 and then what was recommended by the panel.

I very much appreciate her doing that, so that you guys can go back and forth between those two options, with everything that you have there, and it's not quite the same as some of our -- As staff noted in review, that the total biomass and the female-only biomass, and there's not the same SPR assumptions that you can make, but we calculated what you would need to compare, I think, in that table.

As far as the Fmax versus SPR, and I'm not going to argue with Ryan about that, but it is something that I thought was within the purview of the SSC to recommend a difference, and, if it requires that we run some extra projections at those targets, we can do that pretty easily with Nathan's code. I know that Lisa has been working quite closely with Nathan, to make sure that the code is flexible enough to accommodate Fmax and any SPR proxies that want to be explored.

We certainly wouldn't want that to turn into a rabbit hole that she jumps down, but we would be able to provide some comparisons for you all, and so that's the workload comment.

CHAIRMAN NANCE: How soon would that be able to be run, Katie, just out of curiosity?

DR. SIEGFRIED: If I could let Lisa answer that, since she would be doing it, and I know she was prepared to run more at this meeting, but I am not certain how long it would exactly take if we get quite a few requests. Lisa.

DR. AILLOUD: During the projections, getting the benchmarks is pretty quick. The problem is getting the uncertainty, and that's really slow, and so I can certainly get you the benchmarks and the status, without the uncertainty around the OFL, for now.

MR. RINDONE: Katie, if you can guys can generate the requisite information to support the discussion for alternative FMSY proxies, then the SSC can discuss those. I guess the caveat, to the Science Center and to the SSC, is just to remember that the establishment of an FMSY proxy, from a codified perspective, is a

council decision, and so, ultimately, it would be on the council to decide whether to modify the current FMSY proxy for gag.

DR. SIEGFRIED: Certainly, Ryan, and we were just offering to provide a couple of additional runs, if you all wanted to have those in your report.

MR. RINDONE: Well, I mean, if you guys can do it then okay. I am certainly not going to stand in the way of that, but I just wanted to make sure that we were all understood on how that FMSY proxy would have to be changed.

DR. BARBIERI: Right. Exactly, but, even recognizing that, I think that the regulatory amendment that discusses stock status determination criteria actually has a comment there that would allow the SSC the flexibility to recommend, as new stock assessments are completed, an update to the existing -- Because sometimes there are issues between using a proxy versus using the actual MSY, right, value. Go ahead, John.

DR. FROESCHKE: That's in Reef Fish Amendment 48, and there is a section in there that would allow the council to adopt an MSY proxy, if they chose to, as recommended by the SSC. However, that hasn't been implemented yet. It could be implemented soon, but not yet.

DR. CRABTREE: So all of this is going to take a council action to put it in place anyway. They're going to have to put in place the new catch levels and everything else, and so I think, if we think a 30 percent SPR is a more appropriate proxy, then that's what we should recommend to the council. I guess, if they disagree --

CHAIRMAN NANCE: We need to see it though, too.

DR. CRABTREE: Then we would have to redo it, but, yes, we need to see what does it mean and what does it --

CHAIRMAN NANCE: Right. Doug.

MR. GREGORY: I think what I was going to say has been said, that we could recommend something to the council, and, unless there's been a change, and maybe that's what John is referring to, such a change in proxy would have to be a plan amendment, rather than a regulatory amendment, and that's the only hiccup, but I think the council is working toward making it easier to change, and so it definitely starts with us making a recommendation and going from there, and then, if the council either cannot do it, or chooses not to do, then it gets kicked back to us to do what's on the books

now.

CHAIRMAN NANCE: Okay. Thank you, Doug. Carrie, please.

EXECUTIVE DIRECTOR SIMMONS: Thank you, Mr. Chair, and I missed some of the presentation, and I apologize, because I had to step out for a call, but I think just, if you're going to ask the Science Center to make any of these additional runs, and perhaps this was presented, but some rationale of why we think a proxy is better, and then talking about these other items that we went through the decision tree with yesterday, regarding recruitment, and would the virgin recruitment have to change and that type of stuff, to rerun all those projections. Thanks.

MR. RINDONE: Mr. Chair, just to continue piling on from the council staff perspective, we would need to see -- We would need to be able to provide projections both in Fmax and in whatever the alternative FMSY proxy is, as part of the development of any plan amendment to address all of this.

CHAIRMAN NANCE: Yes.

DR. SIEGFRIED: Mr. Chair?

CHAIRMAN NANCE: Katie.

DR. SIEGFRIED: You wanted us to go through the decision tree for projection settings, and is this a good time, or are there -- I can't see you all, and so I don't know if it's a good time.

CHAIRMAN NANCE: I think it's a good time, yes.

DR. SIEGFRIED: Okay, and so I think it will be a little bit more complicated for this, because of red tide. Lisa already explained why the selectivity and retention are just the terminal year. Is there any -- Go ahead.

MR. RINDONE: You guys still have the outstanding decision to make on the assessment itself, and so the projections, of course, are based on the information generated from the assessment and back-feed into it, but as far as does the assessment represent the best scientific information available, and do you consider the assessment --

Secondarily, after that decision, do you consider the assessment to be appropriate for recommending management advice to the council? Those should be two separate motions, which sometimes we've made together, in the past, but an effort that we're trying

to do, moving forward, is to make them separately.

CHAIRMAN NANCE: Here is going to be the -- If we use Fmax, or if we use an SPR value, an F SPR.

MR. RINDONE: I think that your recommendation should be tied to which SPR value you're using, just with the understanding that, if that SPR value is different than Fmax, that both of them are going to need to be examined by the SSC, and your decision, at that point, the way I would view it would be, if this proxy for FMSY is being used, be it Fmax or F 30 percent SPR, then the resultant projections, as the settings have been defined, are appropriate for this proxy, just like you guys did with the different allocation scenarios, et cetera.

CHAIRMAN NANCE: Leann, do you have a question?

MS. LEANN BOSARGE: Yes, sir, but I am going to -- I will tell you what my question is, and then you all address it whenever you think it's appropriate. You had one slide in there which referenced the GRFS time series, and, since I was the council member that asked you, like at the last minute before you started this assessment, if you could at least examine the historical time series for GRFS relative to FES, I just wondered if the stock assessment panel found that helpful at all.

Were you able to utilize that to interpret anything from FES or make any deviations, and I was just wondering if there was some more color that could be added to the discussion on that, to see if it was helpful at all or if it was just one more thing for you to have to do that really didn't add anything for you, add any value.

CHAIRMAN NANCE: Okay. We will probably discuss that in a minute, Leann, but thanks for bringing that up. I would like to entertain a motion on the assessment itself.

DR. BARBIERI: I have one ready, Mr. Chairman, but --

CHAIRMAN NANCE: But you combined everything into one?

DR. BARBIERI: No, I did not, but, to some extent, I mean, I think that, if we're going to consider what Leann just brought up -- I mean, this is something that, to me, this would be another bridge to cross here, that potentially we would consider a base model that uses the Florida State Reef Fish Survey, instead of the MRIP-FES-based, because it's something that was developed together with the MRIP program specifically to address these more specialized

fisheries, and it's certified by the service, and I don't see -- If this was a stock that had Gulf-wide distribution, but the landings outside of Florida represent less than one-tenth of 1 percent of the total landings.

This would not conflict in any way, and it's a fully-calibrated model that has been reviewed by the MRIP program, and it has been accepted and certified for this purpose.

There is a meeting coming up this fall, and it's in the process of being scheduled, by NMFS S&T that is the Gulf implementation team sub-group meeting, to discuss specifically these types of issues, just because the Fisheries Service is actually moving forward with developing full implementation of these supplemental surveys, as they become -- As they are deemed appropriate to be integrated into stock assessments.

The guidance that we have received, through the implementation team, the Gulf implementation team, was that, instead of having this decision be made by the Fisheries Service in isolation, that it would be made by the regional SEDAR, or stock assessment, processes, the regional stock assessment processes, as they consider which data is appropriate to be integrated into different stock assessments, and so this is why I waited until this point to have this discussion.

However, having said all that, if this is a bridge that is too long to cross at this point -- I mean, we don't have all the full model diagnostics, and, I mean, this was really run as a sensitivity at this point, and I understand that, but I would like to make this point here, that, as a formal recommendation -- When we get to that point, as a formal recommendation, that this be considered as a base run model for the next stock assessment of gag, because I feel that that would be the most appropriate survey to represent those recreational landings. **With that, Mr. Chairman, I actually just emailed the motion.**

CHAIRMAN NANCE: Thank you.

DR. CRABTREE: Can I make a comment, while we're waiting on his motion?

CHAIRMAN NANCE: Yes.

DR. CRABTREE: I guess, Luiz, I don't have an opinion one way or another, GRFS or whatever, and I tend to agree with you that that's a bridge too far at the moment, but I -- That's something that I would kick to the council. If they want to monitor the fishery

with GRFS, then tell us. I mean, I have a lot of concerns with all of these state surveys, that we're going to end up with a mess on our -- Well, we already have ended up with a mess on our hands in red snapper, but, at some point, if the council wants to use GRFS on this, then that's what I assume will happen.

CHAIRMAN NANCE: Okay. So we have a motion offered by Luiz. Do we have a second?

DR. MICKLE: I second.

CHAIRMAN NANCE: Paul seconds. Is there discussion? Martha, please.

MS. GUYAS: Thanks, Mr. Chair. Just to what Roy said, I think the council is interested in going in this direction. Obviously, I am biased, calling in from FWC here, but, I mean, this is why the council wrote the letter asking for the GRFS, or SRFS, data to be incorporated in this assessment, and so, I mean, I would be -- If you all choose not to go that route this time, I would be interested in hearing why. It is a dataset that addresses a number of issues, and it does have less uncertainty than the MRIP-FES values.

Anyway, I'm just piping in there, plus we haven't had a formal vote, like thou shall include this dataset, but I thought -- At least I was thinking that, really, it's up to the SSC and the SEDAR panel to kind of kick those considerations around, but, if it's the council, then I'm sure the council can take that up as well. Thanks.

CHAIRMAN NANCE: Thank you. Paul.

DR. MICKLE: This will be my opinion only, but it just seems very distracting that we're talking about GRFS data and things like that, and we have a motion on the board that has nothing to do with that, and could I respectfully request that we address the motion in our discussion, and vote on the motion, and then take up GRFS?

CHAIRMAN NANCE: Yes.

DR. MICKLE: I know I don't have the power to do that, but --

CHAIRMAN NANCE: No, that would be --

DR. MICKLE: It just seems very distracting.

CHAIRMAN NANCE: We're going to vote on this motion first, and

then we can certainly talk about the GRFS. Doug.

MR. GREGORY: Thank you. I will save my comment about Luiz's proposal until later, and I will just comment on this motion. Given the problems I have with the historical recreational data being used for this assessment, and concern that that use -- When you look at all the graphs going back into the 1960s, the stock size is a lot bigger, and the exploitation is a lot smaller, all based on those numbers that are not real in any respect, it might be worth it for us to have a good look at it, or just read the best practices paper that came out, but, as long as that's included, this is not the best available scientific information, as far as I'm concerned, and so I will vote against the motion.

CHAIRMAN NANCE: Can you explain just a little further on that, please, Doug?

MR. GREGORY: Okay. I think, starting in the 1950s, or maybe the 1940s, the U.S. Fish and Wildlife Service did a mail survey, country-wide, asking people about their fishing activities and what they caught and hunting activities and what they shot and all that.

Every five years, they would come out with an estimate of harvest levels by species, and California had their own independent study, much like we're talking about with the state, and the California fishing study said, no, no, no, we don't catch nearly as much as this Fish and Wildlife mail survey is saying. We just don't do it.

They basically refuted and said the survey was incorrect, and sometime back -- I came across a paper by a gentleman out of Virginia University who summarized all of this and said that that Fish and Wildlife survey was not realistic, and so I brought this to the SEDAR attention, when SEDAR first got started and assessments were going, because of the need to reach back in history, if we can, to get as much information as possible.

What SEDAR did, to try to address this overestimate of harvest, was to go back and look at the historical trend of population size in coastal counties and adjust this U.S. Fish and Wildlife Service estimate accordingly, and there is a paper in the SEDAR documents, and it comes up every now and then, and it's in the best practices, on how they did that, and I applaud the effort, because they were trying to correct something that was wrong, because they felt like they needed this 1960s data to have a stock assessment.

I contend that, thirty-five years later, we've got thirty-five

years of data, and we don't need to be capturing this ephemeral data that is not accurate at all, and I think it has a big impact on the stock assessments, and that's what I am trying to get at. With red grouper, we didn't go back to grab those years. With gag, we did, and why are we doing it differently?

That's kind of a summary of it, and I'm sure that somebody within NMFS could give you a more accurate picture of that analysis that they did with the county population data and the adjustments. It was all in a good-faith effort, but I think the basis of all that is just plain incorrect information, and it's overblown, overestimated, landings, grossly overestimated landings from the 1960s. That's my memory of it all, and I think the guy's name from Virginia University is McCue.

CHAIRMAN NANCE: Okay. Thank you, Doug. Shannon.

MR. GREGORY: There we go.

DR. CALAY: Hi, Doug, and thank you, Chair. I don't think that I have a convincing result from this assessment to provide, but we have done analyses for the previous gag assessments that have removed the historical data, and, in those analyses, the stock status, the current stock status, was not impacted much, and so we have seen this several times before with the gag grouper assessment, and frankly with some others, that how you actually get to the historical data period helps the model kind of tune-in that initial fishing mortality, or that initial depletion estimate, but it has very little to do with the current stock status estimate that results from the model. It would be ideal if we had that to show you today, but we do not.

CHAIRMAN NANCE: Thank you, and I know, in our discussions -- Since this was an operational assessment, to not deviate tremendously from what had been done in the past, and, as we go forward with a research track, then those things can be explored in more detail. Luiz, please.

DR. BARBIERI: Thank you, Mr. Chairman. Doug, I am looking at Slide 7 in Lisa's presentation, and I don't think that the data that you are talking about -- That data was not included in this assessment. The U.S. Fish and Wildlife Service historic estimates --

MR. RINDONE: No, she said -- Lisa, didn't you say that you used the 1981 to 1985 CPUE for hindcasting the recreational catch and effort?

DR. BARBIERI: Right.

DR. AILLOUD: Yes, and so that was based on a fishing and hunting survey, and so I think that's the one that Doug was referring to, and there is an estimate of effort every five years, and that is what is used, and it's scaled by the CPUE of 1981 to 1985, and so the scaling is the CPUE, but the effort comes from the survey.

MR. GREGORY: But, Luiz, the green bubbles go all the way to the beginning of the time series, and that's private shore mode, and charter boat, also.

CHAIRMAN NANCE: Katie, do you have a comment on this one?

DR. SIEGFRIED: Yes, I do. Thank you, Mr. Chair. The method that Doug is referring to is what is used to create those bubbles pre-1981 for the recreational private shore mode, and so, yes, those are in there, and I am just sort of saying the same things that have been said, but I think they're worth saying again, and this was a SEDAR best practice to use that method, and it was accepted in the last assessment, and this is an operational assessment, and we have sensitivity from the last time that showed, when we excluded those years, we didn't have a substantial change in the terminal year status.

It's just not something that we can explore at this time, and it's certainly not something that I think is a giant red flag for the status, because we have those 1963 commercial data, and those are actual logbook data coming in for the first year of the assessment, and so the first year of the assessment isn't even scaled by those recreational data only. They are scaled by the actual logbook data as well.

Then we would have had problems in things like R0, in our likelihood profiles, if we would have had a problem with the initialization of the model, and so all of the other data in our likelihood gibes with using those recreational data back in time, but I certainly think it's something to reconsider when we have a research track assessment.

CHAIRMAN NANCE: Absolutely. Thank you very much for that.

MR. GREGORY: To that point, I know we had commercial catches going back to that time, and we had them recorded, but logbooks wasn't created until the early 1990s, and the Trip Information Program, or the TIP program, wasn't started until the late 1980s or early 1990s, but I just wanted to make that point. I am concerned with that old data, and I have been since it was first being used, and

I will go back and look at the previous assessment and just see --

MR. RINDONE: It's SEDAR 33, Doug.

MR. GREGORY: I will look at that, because I don't remember seeing that before. Thank you.

CHAIRMAN NANCE: Thank you. Roy.

DR. CRABTREE: I appreciate that, Doug, and I think we're all concerned about the data and the quality of those early years, and I'm not sure how we'll resolve that, because it's well back in the past, but, I mean, I tend to -- I think I agree with Katie and company that, given where we are, and the ground rules of SEDAR and what can be done, and given the problems we've had in this fishery, and continue to have, I think we need to move forward with action based on this assessment.

I am comfortable with and will support the motion, because I don't think we can just stop everything now and say, all right, let's do a research track, and so we can come back to this next time, but, for now, I think this is the best we have.

CHAIRMAN NANCE: Thank you. Let's go ahead and put the motion back up. Steve.

DR. SAUL: Thank you, Mr. Chair. Just thinking about the past and the early part of the time series, and you may have mentioned this during the presentation, and maybe I missed it, but I was curious to know how starting fishing mortality was specified and if the model assumed that the population started at virgin in 1963, and, if not, kind of how or what that starting path was deemed to be. Thank you.

DR. AILLOUD: Mr. Chair, can I answer?

CHAIRMAN NANCE: Absolutely. Thank you.

DR. AILLOUD: Thank you. It is not considered virgin condition in 1963, and so you're correct that there is an initial fishing mortality that is estimated. There is one estimated for the vertical line, because that one was in operation, and there is none for the longline, because that one did not start until after, and then there's one for the private and shore fleet.

There is not separate ones for charter boat and headboat, because the private fleet was considered to be a larger portion, and that

is sufficient to account for the recreational catches. When you start to add more initial fishing mortalities, it gets more difficult to get to the unfished state and the offset.

So, to answer your question, the model internally estimates an offset to determine how far off we are from virgin conditions in 1963, and the way it does that is it takes in all the other information we've given it, and so it looks at the age composition that comes in in the 1990s, and, well, what does that age composition look like, and how should it look like, if it was unfished, given everything we know about the biology of that stock.

It tracks back in time, and it looks at what was the fishing from 1963 to 1991, when this age composition came in, and so it backtracks everything and tries to account for that and then comes up with an estimate of that offset from virgin conditions.

CHAIRMAN NANCE: Okay. Thank you. Any other comments or questions on this motion? Benny, please.

DR. GALLAWAY: What was the final resolution on assigning the red tide mortality to natural mortality versus fishing mortality? Was there additional runs going to be made, and will that be discussed in the next version of the report? Thank you.

CHAIRMAN NANCE: Go ahead, Lisa.

DR. AILLOUD: I think I should just clarify something, because I know it's always confusing. Even though I set up my red tide as a bycatch fleet, and it has an F, it doesn't -- In the model, it doesn't really matter if I call it an F or an M. It's just removing fish from the fishable population, and so, on the other side of this discussion, I think was the comment on how do we include red tide in the projections, and that's another issue, and that's entirely doable, and there is no problem with that, but I just need to know what is the magnitude of that red tide and when do I put it -- What year do I put it in? I hope that helps.

CHAIRMAN NANCE: That does, and, on that point, since it's not an F, per se, does having it in there affect the projections?

DR. AILLOUD: It does not, because the relative F excludes red tide F mortality.

CHAIRMAN NANCE: Thank you. Perfect. Thank you for that clarification. Michael.

DR. ALLEN: Thank you, Mr. Chairman. You know, we've always

struggled in dealing with these hermaphroditic fishes in stock assessment models, and I think this is a really good advance, and I'm in support of the motion and the combined SSB.

A comment, and it brings a question to my mind of, in the future, what percent of males -- The question is would you ever switch back to a female-only model, or should you use a combined model in the future, and so, for example, if males went from 1 percent to 3 percent to 10 percent -- I think it's something for us to think about, as an SSC, under what conditions you would potentially go back to not including males in the assessment, but, in this case, and with 1 percent males, and the way this one was handled, I think it's a really nice advance. Thank you.

CHAIRMAN NANCE: Thank you. Katie.

DR. SIEGFRIED: Thank you, Chair. It's just a small point about the motion that goes to the projection decision matrix that we presented yesterday, and you can do with this comment what you will. I am very happy to hear Ryan's take that we want to start moving towards just accepting or not the assessment first and then moving on to the other parts of this, and this kind of does tie the status to acceptance of the assessment, and, depending on the --

Well, I mean, all of the proxies that we've looked at, it's still overfished, with overfishing, but it may be better to separate the stock status from this motion and include it after the projections have been completed and you have reviewed those, because that's really where you find out the stock status. Apologies, Doug, and I should have not said logbooks, and that is incorrect. Thank you.

CHAIRMAN NANCE: Okay. I see what you're saying, and so we would like, for this one, just for the SSC to determine the operational assessment is best scientific information available.

DR. BARBIERI: I understand that. I completely understand Katie's comments, and I don't think that this motion is necessary then.

CHAIRMAN NANCE: This is necessary, because of the model itself, because the projections then are a separate issue.

MR. RINDONE: Well, this motion is necessary because the agency ultimately is responsible for defending the determination of the best scientific information available, and one of the things upon which it leans heavily in that determination is what you guys say about the assessment, because -- Right now, this review that we're

doing, this is the peer review of this stock assessment, and so there's not going to be another one. This is it.

What happens here directly affects NMFS' ability to defend the decisions that follow as the best scientific information available, and so this motion is absolutely necessary.

DR. BARBIERI: Right. Then, just for me to fully understand it, but we haven't seen the whole thing just yet, and isn't that what the --

MR. RINDONE: Well, you're approaching things in smaller packages, right? Instead of saying SEDAR 72 and its projections and the yields and everything -- Like, front to back, this is all the best science, and you're saying the assessment itself, the data that went into it, the way that things were handled, represents the best scientific information available. You pass that or you don't pass that.

Let's say you do pass that, and Katie walks you guys through the decision tree for the projections. Blah, blah, blah, you make some decisions, and it turns out that the stock is overfished and undergoing overfishing, as she and Lisa have indicated that, regardless of what SSB metric you use, that's what you're looking at.

You make that determination then, and then, after that -- Again, I'm just assuming your decisions, but then you make OFL and ABC recommendations based on the yield projections that are generated from the decisions you make to inform the projections, and so I see some hands have shot up.

CHAIRMAN NANCE: Paul, is that acceptable? Okay. Let's go ahead and go back to the period there. Mandy.

DR. KARNAUSKAS: I just quickly wanted to go back to Michael's question about the reference points and when you consider going back to a female-only versus combined, and I just wanted to point out there is some literature -- There is a simulation study that was done by Brooks et al. in 2008, and that might provide some useful guidance on when those different reference points would be considered.

CHAIRMAN NANCE: Okay. Thank you. Katie.

DR. SIEGFRIED: Thanks. I think Ryan and Mandy both just said what I wanted to say. Ryan said the thing about stock assessments, and so I appreciate that.

CHAIRMAN NANCE: Yes, and I appreciate you bringing that up, because this is a little bit of a change from what we've done in the past, and so I appreciate you bringing that to our attention. Jim, please.

DR. TOLAN: Thank you, Mr. Chairman. I am actually of the opinion that that second sentence is beneficial to this motion, because, otherwise, us, as an SSC, are just simply saying we looked at the operational assessment, and it's good science.

Well, at the end of that report, it becomes a status determination, and either we accept it or we don't. If we accept it, and it's overfished and undergoing overfishing, that's part of the report. The projections and all that comes out of the agency and the OFL, that's all very, very different, but I am of the opinion that this second sentence that got deleted is really needed for this motion. Thank you.

CHAIRMAN NANCE: Thank you for that comment, and I -- In the old days, I agreed. The thing about the projections are that it depends on the projections that you use, and the status may change from the base to something else, and it may not be overfished and those types of things, and so, based on projections, some of those criteria can change.

DR. TOLAN: To that point, Mr. Chairman?

CHAIRMAN NANCE: Yes, absolutely.

DR. TOLAN: I'm keeping the projections completely separate and out of this motion, but my reading of the report is it came with a status determination for this species, under all of these conditions, and we accept it or we don't accept it, and so I think the rest of that sentence is really necessary. Thank you.

CHAIRMAN NANCE: Okay. Ryan, to that point?

MR. RINDONE: Thank you, Mr. Chair. Can we go to Lisa's initial projections page, where she had the assumptions that were made for retention and selectivity, et cetera? It's Slide 77, please. Jim, these are the settings that were used, leading up to this meeting, and this is where that stock status determination of overfished and undergoing overfishing for those different SSB metrics comes from, and it's assuming all the different values, respective of the comments there in that right-hand column.

This is kind of like the default that was provided for this

assessment, and that's where that stock status that Lisa presented comes from. The way that I am asking you guys to consider making these motions, with respect to the stock assessments, is similar to the way that they would be made say considerate of like a research track assessment.

The first part of it is the model and the data that are used and building everything up, and then the second part is using the -- Parameterizing the projections and determining the stock status and then making that declaration. Then the third part is recommending OFL and ABC to the council, and so those three decisions made in that order.

There is no point in generating projections off a model that you don't have any faith in, and there's no point in generating catch advice over projections that you don't think are properly parameterized, and it's just kind of like one thing is building on top of another.

CHAIRMAN NANCE: It is different than how we've done it in the past, but I think, from what we're seeing now, this motion is the appropriate starting point for the other two motions that we will have.

DR. TOLAN: Not to belabor the point, Mr. Chairman, and Ryan's explanation is helping a lot, but I just think that, given the report as a whole, we're accepting it in this motion with the internal projections that come with it, and I am having trouble separating those two things out, and I am trying to wrap my head around what Ryan just mentioned and this sort of new way of doing things, but I'm not going to belabor the point anymore. Thank you.

CHAIRMAN NANCE: Okay. Thank you, and I have had trouble with that too, Jim. Sue.

DR. S. BARBIERI: I am not sure if this was the right time, but I was going to address a couple of things that Mike brought up about the hermaphrodites and when you really need to have combined sexes, and there are a series of reasons why it's really important here for this particular species, but there is also more known in general, but maybe this is not the time for that, since you're very close to dealing with this motion.

CHAIRMAN NANCE: We'll deal with this motion, and then we'll come back to that. How's that?

DR. S. BARBIERI: Sure. Sounds good.

CHAIRMAN NANCE: Thank you. Let's go ahead and deal with this motion, and I know there is opposition, and so let's go ahead and do a -- Doug.

MR. GREGORY: I just wanted to acknowledge that I did go back to SEDAR 33, and what Shannon told us was correct, that they did the different runs, and they came out very similar, but so did the run that started in 1880, and so I don't know what to make of that, but it was -- All I can say is that, in 2014, I was not on the SSC, and I was otherwise preoccupied, and so I didn't raise the issue at that point in time.

CHAIRMAN NANCE: Thank you, Doug. Let's go ahead and take a vote on this motion. **The motion is the SSC determined that the SEDAR 72 operational assessment for Gulf of Mexico gag (based on the combined sexes SSB) represents the best scientific information available.** Jessica.

MS. MATOS: Rich Woodward.

DR. WOODWARD: Yes.

MS. MATOS: Will Patterson.

DR. PATTERSON: Yes.

MS. MATOS: Paul Mickle.

DR. MICKLE: Yes.

MS. MATOS: Benny Gallaway.

DR. GALLAWAY: Yes.

MS. MATOS: Harry Blanchet.

MR. BLANCHET: Yes.

MS. MATOS: Jason Adriance.

MR. ADRIANCE: Yes.

MS. MATOS: Luke Fairbanks.

DR. FAIRBANKS: Yes.

MS. MATOS: Mandy Karnauskas.

DR. KARNAUSKAS: Yes.

MS. MATOS: Jim Tolan.

DR. TOLAN: Yes.

MS. MATOS: Sean Powers. I think he's not on there. Trevor Moncrief.

MR. MONCRIEF: Yes.

MS. MATOS: Doug Gregory.

MR. GREGORY: No.

MS. MATOS: Dave Chagaris.

DR. CHAGARIS: Yes.

MS. MATOS: Lee Anderson.

DR. ANDERSON: Yes.

MS. MATOS: John Mareska.

MR. MARESKA: Yes.

MS. MATOS: Jack Isaacs.

DR. ISAACS: Yes.

MS. MATOS: Steven Saul.

DR. SAUL: Yes.

MS. MATOS: Steven Scyphers.

DR. SCYPHERS: Yes.

MS. MATOS: Jim Nance.

CHAIRMAN NANCE: Yes.

MS. MATOS: David Griffith.

DR. GRIFFITH: Yes.

MS. MATOS: Roy Crabtree.

DR. CRABTREE: Yes.

MS. MATOS: Luiz Barbieri.

DR. BARBIERI: Yes.

MS. MATOS: Michael Allen.

DR. ALLEN: Yes.

MS. MATOS: Cindy.

CHAIRMAN NANCE: She should be on now, I think.

DR. GRACE-MCCASKEY: Yes.

MS. MATOS: Josh Kilborn.

DR. KILBORN: Yes.

CHAIRMAN NANCE: Thank you. **The motion carries with one opposed and one absent.** Just so we can go on to the next section without having an issue, let's go ahead and take a five-minute break. Five minutes, you guys, and you've got to be quick. Then we'll come back at whatever five minutes is, Jessica. 3:41.

(Whereupon, a brief recess was taken.)

CHAIRMAN NANCE: We'll start, and, Katie, would you walk us -- You and Lisa, I guess, together, would you walk us through the decision tree?

DR. SIEGFRIED: Sure. We can do that.

CHAIRMAN NANCE: Thank you.

DR. SIEGFRIED: Sure. I think it would be helpful if Lisa could get control back, so we can look at the recruits and look at her original table and all of that good stuff.

The relative F, the only issue that really came up was whether to exclude red tide and which years to use. The last three years are sort of our standard default, and then to exclude red tide -- It sounded like we agreed upon that. Chair, do you want me to pause for a while after each piece, to make sure that people have a chance to weigh-in?

CHAIRMAN NANCE: I think that probably would be great, Katie. I will look around the room. We need to go through it quickly, but we need to make sure that everybody has their opinion. Roy has already got his hand up.

DR. CRABTREE: I think, yes, absolutely exclude red tide.

DR. SIEGFRIED: Okay. Yes, we think that's appropriate, because this is meant to represent the fishing mortality of the fleets. If somebody disagrees, just bring it up. If you have an issue when we're down the list that popped up from before, go ahead and bring it up. I'm not always a linear thinker either. The selectivity and retention seem appropriate, because of the recent size limit changes, and is that right, Lisa?

DR. AILLOUD: Yes, and so there is a time block just on 2019, even though the size limit came in partway through 2018, and so, essentially, if you decide to select more years, it's just going to average out between different retention curves, and so that's the only thing to keep in mind.

CHAIRMAN NANCE: John did bring up that it was in the later part of 2018, and is that right?

DR. AILLOUD: August, I believe.

MR. MARESKA: Maybe Lisa could indicate if it's really going to be that much significant difference. If it's not going to be that different, then we leave it at 2019.

CHAIRMAN NANCE: Okay. Did you hear that question, Lisa?

DR. AILLOUD: Yes, and it will not be that different. What it will probably do is average between a retention curve of, what is it, twenty-two inches to twenty-four inches, because that's really the only difference.

CHAIRMAN NANCE: I think that certainly is acceptable. Okay.

DR. SIEGFRIED: I think we've got those two handled. We'll do the easier ones, and so allocation should stay the same, and that's the base run, and we do assume that there will be further allocation projection requests, but that's after this base case, and, for the interim landings, is there any disagreement about the way that Lisa handled this initially, based on the table that you see in front of you?

CHAIRMAN NANCE: No, and those are in FES values, correct?

DR. AILLOUD: Correct.

CHAIRMAN NANCE: So, if we wanted to use the Florida numbers in there, and we used them as a sensitivity run.

DR. SIEGFRIED: There would be issues with that, in that we didn't -- So there are a few issues that should be brought up, if that is a desire of the group, because we didn't actually carry out any diagnostics, and we don't know about the effects of that selectivity on other parameters, and we just showed status, basically, or not status, but abundance trends based on that sensitivity.

We also didn't have a full historical time period, and Lisa can go into this more, if it's necessary, but the GRFS dataset was 1981 on, and so had to back-calculate it, in order to put it -- To actually replace the private and shore modes, and so it wasn't entirely the GRFS dataset that is in that sensitivity.

CHAIRMAN NANCE: Okay. Thank you. Luiz, did you have a comment?

DR. BARBIERI: Well, yes. Katie, I don't disagree with you, and I'm kind of thinking through this. I am trying to think about, as we start considering some of these specialized surveys to be used in support of assessment and management in the region, when would be the best time to include them, and I think what I understood, in terms of guidance from S&T, was that this decision would be left to the regional stock assessment process, and so, through data, assessment, and review workshops, that discussion -- Because all the data that goes into an assessment, all of those decisions are made, right, during the stock assessment process.

Then the SSCs would review those recommendations and then accept or not, and so this is why I brought it up, is this is something that, looking at the results of the assessment, looking at the outputs there that came out using the GRFS data, they didn't seem to be too much off what would be expected.

Now, having said that, I think you're right that we haven't looked at the full set of diagnostics, and none of this has really fully explored that dataset as we would have, and did, for the base model, and so I recognize that, and I'm not trying to push the issue here.

If you feel, Katie, that it would be best to handle this at the next assessment for gag, I would be okay with that, and I think

that you guys have been already very flexible in giving us the separate sexes versus the combined, for us to be able to make that decision, and then you are now willing to look at the different MSY proxies, and so I understand that there is some amount of workload here.

As Roy mentioned, we're trying to make some decisions here and move forward, right, with management advice to the council regarding gag. At the same time, council members have been asking this question, right, and this is something that we were waiting to cross this bridge, basically, to get to this decision point here, to decide, okay, is this something that can be done at this stage or not. I will leave that there and see, Mr. Chairman, if we can hear from others, and I understand Katie's point completely, and I appreciate all her points.

CHAIRMAN NANCE: Any comments on what we've talked about thus far? Doug.

MR. GREGORY: Thank you. I agree with Luiz, and, particularly, we need to look at the impact of making such a change very carefully, and I've got a question for Luiz. I didn't know that your survey went back to the 1980s. I thought that was something you just started about eight or ten years ago.

DR. BARBIERI: That is correct, but remember that FES doesn't either, right, and so there is a calibration that was done.

MR. GREGORY: Oh. Okay. I love all the calibrations. Thank you.

CHAIRMAN NANCE: You're welcome. Any other comments or questions on that? Okay.

DR. SIEGFRIED: To that point, Chair?

CHAIRMAN NANCE: Absolutely.

DR. SIEGFRIED: I mean, I'm sure that everybody could talk about this for a whole SSC meeting, and that may come around, but I think the best place to explore the state data in the assessment is in a research track assessment, and the Science Center is working now to develop basically guidance for the states on how to get their data in the format that we can explore it in the red snapper research track assessment.

What we did here is meet the TOR by including it as a sensitivity, and Lisa worked with the State of Florida folks to get it in the best shape possible, but we would still need the historical data

in order to really replace what's in the model now, the FES time series.

We would need to, as Doug said, and Luiz said, really consider this carefully, and all we did was pull one time series out and stick the other one in, with no further exploration, and so it would be really ill-advised, because we don't have the diagnostics, to use those in projection settings at this point, but we have every intention of exploring those data and what we could and what we can't do with them in the research track for red snapper. We still don't know which format we're going to get them in and how we're going to compare the states' formats. We have a lot of questions, and everybody is going to have to work with us on that, as we go through that research track process.

CHAIRMAN NANCE: Okay. Thank you, Katie. Roy.

DR. CRABTREE: I don't have an opinion, really, one way or another, because this didn't come up at the last SSC meeting, and that was my first SSC meeting, but I guess what Katie says makes sense, and I guess the question is, Luiz, are you okay with approaching it in that way, and it does seem like something we need to make sure that we've explored the angles and understand where we're going.

DR. BARBIERI: No, and I'm comfortable proceeding as-is. I think we're going to be --

DR. CRABTREE: Okay, and so we get there to using the state survey, but we do it through the research track approach, when we have all of the diagnostics and all of the things that we need to do that.

DR. BARBIERI: I am comfortable with that, but, that, to me, it's something that -- It's a decision that we are making deliberately here, and I think Jim is going to get that question when he presents this to the council, and I am positive that you will, and we have got to make sure that we have the appropriate answers to provide, and I think we just develop those, and we have those in our report.

DR. CRABTREE: That's fine, and it's quite clear to me that we are going to end up using GRFS with this and a number of the other Florida fisheries, and I don't think anybody has any problems with that, but it's just that we're not quite able to do it just yet.

CHAIRMAN NANCE: Thank you. I think we're done on projection settings.

DR. SIEGFRIED: We still have recruitment. That's a big one.

CHAIRMAN NANCE: Sorry. Okay. Will.

DR. PATTERSON: I was just going to point out that we haven't talked about the stock-recruit relationship yet.

CHAIRMAN NANCE: Okay. Thank you. Harry.

MR. BLANCHET: My comment was on the stock-recruit relationship.

CHAIRMAN NANCE: Okay. So I guess, Katie, we better get to that one.

DR. SIEGFRIED: Yes. Okay, and so what we did for this -- Maybe you could pull up the recruitment time series and/or the stock-recruit curve, Lisa, in your presentation? Okay, and so what I said in my presentation is that we should consider whether steepness is estimated or whether there is good information to use for steepness, and so what was used here is a fixed value from the last assessment, and it came from the benchmark, which is Shertzer and Conn, right, Lisa?

DR. AILLOUD: Yes.

DR. SIEGFRIED: Yes, and so it's a published paper on a meta-analysis of steepness, and that can be debated, certainly, but that is the steepness value that is used, and the sigma-R is fixed at 0.6. The R0 was well estimated, and we did look that, and I don't know if you have an R0 likelihood profile in your presentation, but we did review one at one of the webinars.

We wanted to look and see if -- Okay, and so then that's what we know about steepness. We know that it's informed by a published paper that is a meta-analysis of some snapper grouper and then other species in that paper, and we know that our sigma-R is fixed at a common value, and our R0 is well estimated.

What Lisa is showing you here on the left is the dark dots, just like in my presentation, are the estimated, and then the green and yellow are the early time period, and so you can kind of take a look at whether you see that there's a relationship, and you have a lot more spawning biomass with the low recruits, and then you have those higher spawning biomass producing larger recruits. That is going to be forced by that fixed steepness. Then the recruitment time series -- The options that you have are to use the data-rich, which begins in 1981, or is that right?

DR. AILLOUD: I think 1984.

DR. SIEGFRIED: 1984 to the terminal year, or to a year before the terminal year, if you think the terminal year estimates are not estimated well. What we did to start was to use that 0.855 estimated steepness value and use the stock-recruit curve to generate recruits, because we thought there was a relationship between stocks and recruits, but that is up for discussion. Did you have anything else to add that, Lisa, while I'm looking at this other slide?

DR. AILLOUD: No, and I think you covered it.

DR. SIEGFRIED: Okay.

CHAIRMAN NANCE: Okay. Will, please.

DR. PATTERSON: Thanks, Jim. I meant to ask questions about this when Lisa went through the presentation, the first time through, but I failed to do so. It says here that steepness is fixed at 0.855. In the Shertzer and Conn paper, the value that they put forward is 0.84, and so, first, I'm curious about the difference between the 0.84 and the 0.855. It's not a huge difference, but maybe there was some way that 0.855 was estimated differently than what they proposed.

They also proposed 0.84 as a prior on steepness, and here it says that steepness was fixed, and so I am just curious why it wasn't treated as a prior, whether it's a loose or informative prior, but, instead, here, it's a fixed parameter.

DR. AILLOUD: I looked back, and so this number here comes from the SEDAR 33 update, and so 0.855, which, as I said, we followed SEDAR 33, and so, going back to SEDAR 33, what they did at the time was try to estimate it, and then also look at a suite of them, and they looked a 0.7, 0.85, and I think 0.9, and, in their justification for 0.85, that's where there a reference to that prior from Shertzer.

I will be honest, and why it's 0.855, I'm not sure. It was 0.85 in SEDAR 33, which probably was just a rounding, and then the reason why I didn't make it a prior is just because I was keeping with what was done in the last assessment, and so, in the last assessment, it was fixed at that value.

I should say, in SEDAR 33, when they did explore 0.7, the model behaved really poorly, and so they excluded that from the options, and the thought was that there was evidence for this stock and that there should be some sort of relationship and that we should not have a steepness at one, and so they reverted to 0.85.

DR. PATTERSON: Jim, I put my hand back up. I don't know if you see that.

CHAIRMAN NANCE: I see it. Go ahead, Will.

DR. PATTERSON: Okay. Thanks. I am just curious if you had done any exploration during your model fitting, and, obviously, there are some other things that have changed that improve the ability to fit the model in this round versus the 33 update, and so I'm just curious if you did any type of likelihood profiling to see or try to fit this as a prior, instead of just a fixed value, with a prior instead of a fixed value.

The idea that there is probably some information in the stock-recruit relationship not to go to a steepness of 0.99 makes sense, just looking at the estimated data here, but I am just curious if you explored any of that, but we haven't seen it.

DR. AILLOUD: The only exploration I did was try to estimate it, and it did bound to one, and that's as far as I took it, and I did not look at any likelihood profiles or anything.

DR. PATTERSON: Okay. Thanks.

CHAIRMAN NANCE: Okay.

DR. SIEGFRIED: Are there any other questions, Mr. Chair?

CHAIRMAN NANCE: No, not that I see. Harry.

MR. BLANCHET: The recommendation then is to use this spawner-recruit relationship, and am I reading this correctly, that, if you look in the upper-right-hand corner, essentially since about 2008, we've had one recruitment estimate that is above the expected, and about ten that are below?

DR. AILLOUD: You are looking at the recruitment deviations, and is that correct, at this spot?

MR. BLANCHET: That's correct.

DR. AILLOUD: Okay, and so the way that SS works is that we define a data-rich period, which is all those black points, and it forces the deviations to center on zero, and so that's why you're seeing some above and some below, and you're correct that the deviations are all negative since about -- I mean, I guess some of the uncertainty overlaps with the zero line, but, yes, at least in

2018 it's below, and then from 2011 to 2012, and, if you look at the spawning stock trajectory -- If your spawning stock goes down, those recruitments, those deviations, are going to go down as well, right, because we did fix the steepness, and so there is a relationship, and that's why you're seeing the deviations are positive when the stock biomass was stronger, and then, as the stock biomass goes down, starting in 2008, then the recruitment follows and you see those negative deviations.

MR. BLANCHET: Okay, and so that is not saying that those are below the estimates that you would get from the stock-recruit relationship? Okay.

CHAIRMAN NANCE: Any other hands? Sue, please.

DR. S. BARBIERI: Hi, Lisa, and thanks so much for all this, and thanks that I can actually speak normally now, but just to make sure, so I understand the -- Because I also wondered about these recruit deviations, but how well does this pattern agree with the indices of recruitment, in terms of the last -- I think you said there hasn't been a strong year class since 2010.

This seems quite concerning, and, just in terms of the sex ratios, I have heard a lot of people talk about spawner-recruit relationships, and that typically they look like a shotgun, like this one does, but that they're very important, because, if you have no spawners, you have no recruitment, and, yet, we don't seem to be thinking the same thing when we think about males.

I mean, we are, and we're using combined sexes, which is awesome, but, in terms of reproductive resilience, you often have more variability, and then you may have a crash, and so I'm very concerned about this stock, and I wondered how well the actual indices -- What do they suggest, in terms of recruitment in the most recent -- Maybe from 2010 on.

DR. AILLOUD: Let me pull up the index. If you look at the age-zero survey, you do see that, in the last ten years, those points, on average, are lower than the previous ten-year time period. I don't know if that answers your --

DR. SIEGFRIED: Sue, I think it tracks really closely there, doesn't it?

DR. S. BARBIERI: Yes, it looks like it. Thanks, Lisa. That's exactly what I was wondering, and I just hadn't kept track well enough, in terms of what you presented before, but thank you. That was exactly what I was wondering, and I think that we should be

concerned.

DR. SIEGFRIED: Mr. Chair, are there any other questions around the table?

CHAIRMAN NANCE: Not that I'm aware of.

DR. SIEGFRIED: Okay, and so that's certainly different than for amberjack. We didn't have an age-zero survey, right, and so the age-zero survey is helping us compare the estimates of recruits, and we have a fixed steepness value, and then I wanted to just add that what I looked up in the SEDAR 33 report says that, for the -- - It doesn't quite make sense, but forecast recruitments are derived from the model estimated by the original stock-recruitment relationship based on the recent time period of 1984 to 2015.

That's not exactly what we would say is recent anymore, but that's basically the data-rich period, and so, for 33, what they assumed was -- The data-rich period informs the decision of how to configure our projections, and so what we started with, what we decided to have Lisa provide initially, was pulling the recruits off the stock-recruit relationship.

The farther back in time you go, the more like the stock-recruit relationship we hope it should be, if we actually are estimating well, but those are the options, to either pull off that stock-recruit relationship or to take some average of the time period, and so what we started with is what you have on the screen.

CHAIRMAN NANCE: Any -- This is what we have, and there are what the projections are based on. Any changes to those? Any recommended changes?

MR. RINDONE: John Mareska, you had a recommendation that you had made earlier about selectivity and retention, and did you want to revisit that?

MR. MARESKA: Mr. Chairman, I do have one question, and so this original steepness was based on female-only, and so is there going to be a substantial change in steepness if this is sexes combined?

DR. AILLOUD: No.

CHAIRMAN NANCE: Okay. Lisa, that was a no?

DR. AILLOUD: I mean, I don't have -- That was not discussed, and so, no, I don't have any other alternative thought on how steepness would be affected if we wanted to do something more appropriate

for combined sexes.

CHAIRMAN NANCE: Harry, please.

MR. BLANCHET: I am just going back and forth with -- When we were talking about amberjack, we were talking about using an average over the most recent time period for our recruitment, and, here, we're using a stock-recruit relationship. I think we need to carefully explain why we're doing something different here, versus with amberjacks, and I realize that, in this case, we have indices of age-zeroes, or at least early fish, from the videos and so forth, but I think we should be explicit about why we're making this selection rather than average of the last several years.

CHAIRMAN NANCE: Okay. Any comment on that, Lisa?

DR. AILLOUD: Maybe Katie, if she wanted to --

DR. SIEGFRIED: One of the things that we're preparing for you is -- We're still going to show you the difference between the recent time period and a longer period of recruits, when we show you the amberjack results, so that you can compare. This is, again, the regime shift idea, and sort of the stuff that Dave was talking about with moving the goalpost.

Right now, we don't have a goalpost, because we don't have a rebuilding plan set yet, and so it seems like it's either going to be pulling from the stock-recruit relationship or doing a longer-term -- Like was done for SEDAR 33, a longer-term average of the estimated recruits, which are even better informed, because we have an age-zero trawl index, and so I do think there's differences between the two, and I do think the amberjack decision will be discussed when you see the projections that you have asked me to provide.

CHAIRMAN NANCE: Okay, and it seems like this one has a pretty good relationship, stock-recruit relationship. Doug.

MR. GREGORY: Thank you. By fixing steepness and getting an MSY, doesn't that kind of force us into looking at F of MSY and MSY as one of our status determination criteria, as well as 30 percent SPR and Fmax, so we can kind of choose between the three? Otherwise, why would we fix steepness? Just to estimate recruitment?

DR. SIEGFRIED: That's a good question, Doug, and that may be something that I wasn't clear enough about yesterday. We don't have to use MSY when we have fixed steepness in the model. It is

used to relate stocks to recruits when we don't have estimable steepness with the data that are in the model. We can still use a proxy, if we use the stock-recruit relationship to estimate those recruits, and then, whether we estimate the recruits and then use them in the projections directly, or we just pull from the stock-recruit curve, we can still use a proxy. We don't have to use MSY.

MR. GREGORY: But we could now. I mean, if steepness was 0.99, we can't use MSY, and we're stuck with a proxy, but, if we fix steepness, we can estimate MSY directly.

DR. SIEGFRIED: You can, but that hasn't been the way that the SSC has operated in the past, because the steepness wasn't well estimated enough to believe MSY, and that was what I understood from my reading of the previous materials, but certainly there is no reason that you can't use a proxy along with the stock-recruit curve, and then, yes, of course, MSY can be estimated when you fix steepness.

MR. GREGORY: Right. Okay.

CHAIRMAN NANCE: Okay. Luiz.

DR. BARBIERI: Katie, I think I'm going to have to pile on here, but help me understand this. If we are using the stock-recruitment relationship to estimate those recruits, that means the parameters that we estimate, that were estimated, are considered valid, right, and there is enough information in the data, in this assessment model, to estimate those parameters, and then you can use the stock-recruitment relationship to then estimate the recruits.

Then, if you don't -- I am finding that difficult to understand, and like how can we choose a proxy for MSY, because we're saying that we cannot directly estimate MSY, but we use the stock-recruitment relationship anyway, and then a little more, because then you can see the nature of my question here, and I thought that, when you fixed steepness, that corresponded to a value of SPR, right, and there is some level of correspondence between the two, and so there is consistency within the model.

We could either fix steepness and identify the SPR proxy that corresponds to that steepness or the other way around, and can you help me understand that, Katie?

DR. SIEGFRIED: We might be moving between species just a little bit here, and so the amberjack -- We were able to estimate steepness for amberjack, and you could argue how well estimated it

is, but we could estimate it without a prior. Here, it's fixed at a meta-analysis value, and so, by doing that, we're saying we don't have average recruitment around some mean, and we actually think there's a shape to the stock-recruit relationship, and we are fixing that by using a fixed steepness for gag.

If we don't think that -- Then, with that fixed steepness, comes a corresponding estimate, or value, for MSY. If we aren't estimating MSY, there's been talk, quite often, in the SSCs and the councils that, well, we don't trust MSY, because the steepness isn't well estimated, or estimated at all, and so that's why a proxy would be pulled in.

Yes, you can calculate a proxy for the value, whatever the proxy is, whatever SPR percentage it is, for the MSY that is estimated with the steepness that you have used to fix the stock-recruit relationship in the model, and so there might have been a little bit of confusion because of the two species and how different the steepness values are derived, or arrived at, and did that help at all?

DR. BARBIERI: Well, a little bit. I guess I'm going to have to go and re-read some of these papers between that relationship, but I am thinking, if we have a value of steepness, then we have -- You're saying that we have an MSY estimate that we can use, right, and why are we choosing a proxy reference point? I am talking gag.

DR. SIEGFRIED: I can look back at the SSC documentation, to figure out the thought process then, and my question about amberjack is because it's an estimated value. Let me pull up what the rationale was.

DR. BARBIERI: In that case, if it was estimated, but the SSC decided not to accept it, as the review body, and recommend a proxy reference point, to me, that would remove the use of the steepness and the S-R relationship to develop those recruitment values, or, I mean, in this case here, if we say, well, we fixed steepness, then I would like to see what is the SPR value that corresponds to that value of steepness of 0.855.

DR. SIEGFRIED: So that is -- Let's see. For the last SSC meeting, the steepness that was used, that was fixed, is 0.85, like we said, and the FMSY proxy -- Okay, and so the SSB current, at that time, over SSB 30 percent, was 0.471, and so that's what was used. Let me see if I can find what the actual SPR value was. It's hard for me to justify the decision that I wasn't there for, but I am just trying to go through the explanation.

DR. BARBIERI: I'm sorry, and, Katie, I don't mean to belabor this point, and perhaps this is my own misunderstanding of how this works.

DR. SIEGFRIED: No, I don't think that it's a -- I mean, I think it's a valid question, and I think other people probably have the same question, and it would appear inconsistent if an estimated steepness value would be rejected and then a proxy was used, but a fixed steepness value was used and a proxy is -- In both instances, the steepness is fixed, but different decisions, apparently, were made. Would it be possible, while I look through this report, for the next person to ask a question, so that I can find this? I don't want to hold things up.

CHAIRMAN NANCE: Sure. Roy.

DR. CRABTREE: Just, to me, I mean, because the steepness is fixed here, we told it what the steepness is, and it didn't estimate it, and so that's the reason that I wouldn't use an MSY that comes out of this, because it didn't estimate it. That's like you told it what MSY is and then said, okay, that's MSY. That's why you used a proxy here, because you don't have it.

Now, with amberjack, yes, it could estimate the steepness, and so I guess, arguably, you could make the case that we should use the MSY estimate with amberjack, but the problem, in that case, was all the higher recruitments were in the early, prior to data collection, portion of it, and that's not the case here, but it does seem, to me, because the steepness was not estimated, it's appropriate to use a proxy and not the MSY.

DR. BARBIERI: Right, but, since we chose that value of steepness, we chose it, but that value is determining the values of recruitment that are being estimated, correct, and so made that choice, and it couldn't be estimated by the data available. The model didn't estimate it. The model failed to estimate it, and we added that value, but we used -- To me, that's awfully circular. We fixed a value so that then we can define the productivity of the stock, to actually determine whatever the value of recruitment will be, correct?

DR. CRABTREE: Okay, but, in my history of thinking with these sort of things, if you don't estimate the steepness, then you don't have a real estimate of MSY, and it's appropriate to use a proxy, and that's the case here with gag.

DR. BARBIERI: Right, and I'm fine with that, with using the proxy.

What I am saying is that we accept that we did not estimate steepness, but then choose a value of steepness that we chose, and that value of steepness will determine the value of recruitment that we actually are going to be producing now.

CHAIRMAN NANCE: But that's always true.

DR. CRABTREE: Is the point that you then want to see what SPR corresponds to that steepness, and then that would be the proxy we would use? I am not sure where you're going.

DR. BARBIERI: I am trying to think about how do you come up with the recruitment values for here, and, I mean, right there, it's saying value is based on a Beverton-Holt stock-recruitment relationship, but that relationship was not accepted, correct?

CHAIRMAN NANCE: The parameter, the Beverton-Holt relationship, is, with a steepness of 0.855.

DR. PATTERSON: And the sigma value, right, and so the recruitment deviations is also being fixed here, and so I think what Luiz's issue is -- It doesn't have to do with the proxy or MSY, which, like Roy has indicated, we're setting the MSY by fixing the steepness at 0.855, but I think what Luiz's concern is, it's more so projecting recruitment forward to predict equilibrium values, or the near-term projections for OFL, and then estimating ABC.

The two alternatives then would be to use the 0.855 and the sigma at 0.6 to project forward or to take an average of recent recruitment, whatever you set that time series, and, I mean, that seems like the choice that we would have to make.

CHAIRMAN NANCE: Or the projections, Will.

DR. PATTERSON: Yes.

CHAIRMAN NANCE: Yes. Okay. Sue.

DR. S. BARBIERI: I just wanted to share -- Because this was from the presentation that I gave a couple of months ago, or a year ago, maybe, but from the SEDAR 33, and I believe the update, what was there was that data suggested the steepness was about one, but the concern, and I believe that was concern from the SSC, I thought, was that gag grouper biology does not suggest that steepness would be one, and that, based on Conn et al., it would have been about 0.68, but it was fixed, for I'm not quite sure what reasons, at 0.85.

I do think, like Will was saying and what Luiz was saying, obviously this has huge impacts, because we're setting that steepness ahead of time, and, in terms of the original question about whether steepness would vary -- I mean, it won't vary in this case, because we set steepness, but, in terms of if you're estimating steepness, yes, it has to come out differently, if you use combined sexes, I believe, and, Luiz, you would know better than I, and so I think that is a huge point. I hope that's helpful.

CHAIRMAN NANCE: Yes, it is. Will.

DR. PATTERSON: Sorry. I jumped in there without waiting my turn.

CHAIRMAN NANCE: Did you have any other comment on that?

DR. PATTERSON: No. I'm good. Thanks.

CHAIRMAN NANCE: Okay. Steve.

DR. SAUL: Thank you, Mr. Chair. To that point, I agree, and I echo Luiz's concern regarding steepness, and I wonder -- At the risk of, obviously, adding more workload to you all, and having been on the other side of this, at times still being on the other side, where you're cranking the model, I wonder if -- It sounds like we're not going to be able to really make an informed projection-related decision and set appropriate ABCs and OFLs without possibly like a sensitivity analysis to sort of better explore the implications of the value being slightly different from what it's been fixed at, given that it has been fixed.

I wonder if that would be a useful sort of decision tool for our SSC members, to sort of help guide how we set our limit, in addition to if we can sort of define a range of steepness values that could be explored, and then the range of values coming out of that could be used to help establish or set uncertainty bounds as well, and so that's just something for folks to think of as we're sort of considering how to use this appropriately.

CHAIRMAN NANCE: Okay. Doug.

MR. GREGORY: Thank you. I initially raised this, because it seems like we're in a transition. For a long time, according to my memory, most of the models estimated steepness at 0.99, and we just said, okay, that's what it is, and so we go with a proxy, and we didn't really worry about it.

In the recent scamp review workshop, the international CIE people were pretty adamant that you need to have a steepness, and 0.99 is

not reasonable, because there is no fish population on earth that has that kind of unlimited reproductive potential over the entire range of the population. We all know that, intuitively.

They were insisting on a steepness, and, for a long time, in the early years of the assessments, the proxy was because we couldn't do an MSY, but, if we decide to use a steepness, and we can estimate an MSY, I guess the question I'm raising is, is that better than a proxy? I think Luiz -- It's a philosophical question at this point, and Luiz is saying the same thing.

If we can do it, should we? It doesn't matter if we set it or if it's estimated with a prior. If it can be estimated within the model, with or without a prior, that's great, but, even if we set it, that's no different than other things that we set and estimate, based on our best knowledge, and we've got lots of metadata on this. I think, going forward, this is something for us to continue to talk about. Thank you.

CHAIRMAN NANCE: Thank you. There's a table that appeared on the screen. Katie, is that from you?

DR. SIEGFRIED: That's from you.

CHAIRMAN NANCE: Okay. I will take Will's question.

DR. PATTERSON: Thanks, Jim. To Doug's point, in the past, when we had steepness values of 0.99 coming out of the assessments, that wasn't -- It wasn't estimated at 0.99, but it just went to the upper bound, and we fixed steepness at 0.99, which is quite different than what the South Atlantic Council and SSC typically have done, in which they put the prior on steepness from Shertzer and Conn, and they try to estimate MSY, with that prior and the information that's in the assessment, to indicate where steepness actually is.

In our case, we've said there's not enough information to predict recruitment from spawning stock biomass. It's not that there's no compensation or density dependence in that relationship, and recruitment can increase without bounds, but it's simply saying that we don't have enough information to actually estimate what steepness is, and so we're just going to say there is no relationship, and then the workaround for projections is to estimate recruitment from the recent time period, or a recent time period, and use that to project forward, especially given the fact that we're only projecting over the near-term and not five, ten, twenty years down the road, as far as OFL and ABC. It's not that we've estimated steepness to be 0.99. We set it there, saying we

can’t estimate it, given the information in the assessment.

DR. SIEGFRIED: Mr. Chair, to that point?

CHAIRMAN NANCE: Yes, please.

DR. SIEGFRIED: Will is exactly right, and there have been examples using average recruitment, where the computational convenience of fixing steepness at 0.99 is used in both regions, and, actually, I did it for cobia in the South Atlantic, and it’s been done quite often in the Gulf.

That is never meant to imply, as Will said, that it’s unlimited potential productivity, and I am glad that Doug brought up the scamp issue, because one of the things that they pointed out, which I think should get to Luiz’s initial consternation, is that, when we use that steepness value, we are basically telling it what level of recruits to estimate, on average, based on the size of the stock.

It may not be something that this SSC, or others, trust for the MSY, because the MSY is such an important benchmark, and it would be -- They seem to be more reliant on a well-estimated steepness, but it certainly is something that informs recruits, and those CIE reviewers were quite comfortable with that, because they didn’t think it was a truly average recruitment just around some mean that had nothing to do with the stock size.

I think it’s two different questions, whether we use the MSY from the fixed steepness and whether we trust that stock-recruitment relationship enough to either use average recruitment or just pull from the stock-recruit curve.

CHAIRMAN NANCE: Thank you. Lisa, go ahead and take us through this table.

DR. AILLOUD: I have pulled this from -- This is the SEDAR 33 benchmark assessment SSC review, and it’s just to show that they did look at how those reference points changed between the different steepness assumptions, and so you can -- Obviously, those numbers would change in this case, but I think it gives you an idea between SSB female and SSB combined.

If we look at 0.85, at least in this case, it appears that, in the SSB combined scenario, your FMSY and F SPR 30 are quite similar, but Fmax is very different, which I did show in mine, that my Fmax and my SPR 30 diverged quite a bit in the SSB combined scenario as well. I don’t know if this helps a little bit, to think about how

they are related.

CHAIRMAN NANCE: I think this helps greatly. Dave.

DR. CHAGARIS: One of the things that hasn't been mentioned, but I kind of struggle with, is the idea of fitting the model with a stock-recruit curve, whether steepness is estimated or not, but the projecting with a model that does not have a stock-recruit relationship, and it would seem to me that all the other parameters in the estimated model are conditioned on your decision, or the estimate, of steepness in the recruitment parameters.

That should be carried forward into the projections, I mean, just based on principles, and so I guess I'm a little bit concerned that, if we don't like the stock-recruit relationship enough to use it in projections, then why is it okay to use it in the estimation?

I am also concerned that, in doing one different from the other, that you have two slightly different models, at least in theory, even though the behavior might not be that different, and so that's just kind of a concern and thought that I had, and I don't know if others feel the same or not, but I just wanted to share that.

CHAIRMAN NANCE: Thank you. What do we want to use here? Will, go ahead.

DR. PATTERSON: I agree, Dave, and that's something that I was thinking about as well, but I wonder if you could set the steepness at 0.855, but then estimate the sigma parameter, the deviations from the actual recruitment estimates, and, that way, you would have perhaps a more realistic projection of the uncertainty in recruitment, based on that estimated component, the variance, and not the actual value of steepness.

CHAIRMAN NANCE: Will, you're saying, for the base model, use the 0.855. For projections, use a variance around that?

DR. PATTERSON: No, and what I'm saying is both the steepness value and the sigma were fixed in the base model, and so I'm just kind of thinking out loud here about whether that makes sense to fix the variance, when you actually have the estimated variability in recruitment around the function that's fit by a steepness of 0.855, and so why fix the variance, and then that has implications then for projections. I am thinking out loud here, but it's kind of also a question for Lisa and Katie.

CHAIRMAN NANCE: Yes. Lisa, do you have that? Do you have the

sigma that would be able to be carried out through a projection?

DR. AILLOUD: Estimating it would require rerunning the assessment and checking all the diagnostics. I get the point, and I think it's definitely a good idea, but it's just not something that we revisited during the process, and so that would be quite a bit step, to rerun everything and try to see if we can estimate sigma R.

DR. PATTERSON: Of course. I wasn't thinking about all the diagnostic stuff that goes with it.

CHAIRMAN NANCE: So would there -- Let me ask you this. Would there be a major issue with anybody to use 0.855, sigma 0.6, for the base, but also carry those two parameters for the projection?

DR. SIEGFRIED: Mr. Chair, sorry for interrupting, but I'm just making sure what you're asking is if pulling from the stock-recruit curve, using the steepness of 0.855, is appropriate, as we've done in our initial runs, and is that what you're asking?

CHAIRMAN NANCE: Yes.

DR. SIEGFRIED: Okay. Thank you.

CHAIRMAN NANCE: You're welcome. I am trying to make it where we're using 0.855 for steepness, sigma 0.6, if I'm remembering correctly, and we use that in the base model, but also use those two parameters for the projections. Is there any SSC member that has an issue with that? Will.

DR. PATTERSON: My hand was just still up.

CHAIRMAN NANCE: Okay. Seeing none, that's acceptable for the recruits. Katie, go ahead.

DR. SIEGFRIED: That is it for this table, but we do have the issue of how to treat red tide for gag. We have all the settings here, and this will be what we use to determine our benchmarks and status, but what we do with red tide is important. We have excluded the red tide fishing mortality from our relative Fs, knowing that there was a large red tide event in 2021. We should probably consider it, and we have red grouper as an example of potentially how to consider it, but we also have the convenience, I believe, Lisa, that we can treat it as an M instead of those punctuated exploitation rates in each of those years, right?

DR. AILLOUD: I am not sure about that. I don't know if -- The

way it was done was just we can have the bycatch fleet in the prediction years and just set a level of kill for the red tide in a certain year, and this could come from the ecosystem model, or it could be a range.

CHAIRMAN NANCE: Will.

DR. PATTERSON: My hand is down. Sorry.

CHAIRMAN NANCE: Okay. For the projections that I have seen, how was red tide treated in those?

DR. AILLOUD: There is no red tide in the projection.

CHAIRMAN NANCE: Okay.

DR. AILLOUD: We just think that we should include it for 2021, knowing that it did occur.

CHAIRMAN NANCE: Right, and so I guess -- In 2021, we have landings there, and we have projected landings for 2021 through 2022, using the three-year average, and so it is something that we would want to do for 2021, to take out some of those landings, based on red tide?

DR. SIEGFRIED: I think we would just apply a fishing mortality for the bycatch fleet, as opposed to -- I think the M was just a selectivity, or sorry, a sensitivity, that I had in my brain, but sorry, Lisa. Were you about to say something?

DR. AILLOUD: No, and that's exactly right. We can specify the F for the bycatch fleet, and it will just kill off fish, and so it's independent from the interim landings. It's an additional source of mortality.

CHAIRMAN NANCE: Okay, and that would be -- The red tide would be set at what value?

DR. AILLOUD: That is the question.

DR. SIEGFRIED: Which of the current red tide years would it be relative to, or could we scale it to one of those? Is there any input from the group as to what we should use for that year? Maybe you can go to that relative F plot, Lisa, so we could take a look at those peaks?

CHAIRMAN NANCE: Dave.

DR. CHAGARIS: Thank you, and so, I guess, in the past, what was done, and correct me if I'm wrong, but there was some assumption made about the red tide fishing mortality in the bycatch fleet and the level of that relative to a year in the model where the red tide F was estimated.

In the past, we didn't really have any information to inform that, and, oftentimes, you would say, well, is it equal to 2005, or is it half of 2005, or is it a quarter of 2005, but I think, here, we could use the output from the ecosystem model to provide a relative -- A relative red tide mortality to some year that was estimated in the model. If that's a direction that you guys want to go -- I believe, Lisa, you have the table of estimates, but, if you need me to provide those as ratios to an estimated year, just let me know.

DR. AILLOUD: Yes, and I think that would be very helpful, to see how it scales to the other years, but you're correct. In the past, what we did is have a range, and so it was either it was as bad as 2005, or half as bad, or twice as worse, or something to that effect.

DR. SIEGFRIED: Is a range still desirable, or is it --

CHAIRMAN NANCE: I think let's do a range and one that Dave would supply, and I think that we could have those different ones, and then we can look at them, to provide us some numbers there.

DR. AILLOUD: One question for Dave. With the range of uncertainty that you obtain, do you think that would be a proper way to define the range?

DR. CHAGARIS: I think so, because I feel like all the data that has gone into that model, as far as where the red tide occurred, how long it lasted, and how severe it was, and the overlap with the age stanzas, I think it would be a reasonable approach to do it.

CHAIRMAN NANCE: Is that something that's doable within a good timeframe?

DR. CHAGARIS: I mean, we wouldn't have to rerun the model. It's just a matter of how we scale the 2021 red tide estimate to we'll just say the 2005 estimate, and so that would be scaled relative to the estimates out of the Ecospace model, and so it would basically be a fraction of the red tide in 2005.

The thing I'm wondering is, with the age-specific age estimates,

how to handle that, but, yes, we would be able to say, okay, based off of our confidence values of those various runs that we did, that the red tide mortality in 2021 would range between A and B, relative to 2005, and I could do that. Do you need it tonight, or what's the timeframe?

CHAIRMAN NANCE: Well, I think what we're looking at is we've got greater amberjack on Friday, and we would, I think, finish this one up on Friday, also. Thursday. I don't know why I am saying Friday. So Thursday, and so we have, on Thursday, sometime in the afternoon, but we would have the model and the projections for that and this one, gag, with blessing the model for management and coming up with the OFL and the ABCs. I think both of those are doable, and we would need to give each of those probably an hour to be able to do that, but I think that's doable.

DR. SIEGFRIED: Yes, but, to your question, Dave, the sooner the better, and so is tonight or tomorrow morning possible?

DR. CHAGARIS: Yes, I can do that.

DR. SIEGFRIED: Thank you so much.

DR. CHAGARIS: No problem.

DR. SIEGFRIED: Okay, and so we've got all of our settings, and we've got -- The main difference between what Lisa has done up to this point and what we will show on Thursday is the red tide, and is that right, Lisa?

DR. AILLOUD: Yes.

DR. SIEGFRIED: Okay, and so I just wanted to -- We do have to set up a rebuilding timeframe, and this is something that I have talked with council staff about, about how to approach this, and so Lisa did put together F equals zero projections, but they don't yet have the red tide in them, and so I'm not sure that it's helpful to look at those at this point, but we would have to -- When we get to her presentation, we would have to take a look at how all these decisions affected that rebuilding timeframe under that F equals zero scenario.

The way I understand it, and, Ryan or others, correct me if I'm wrong, but we have to show that, determine how quickly it recovers under F equals zero, and then, based on that, decide if a generation time can be added or if further discussion has to be had at that point, but we're only running these projections for total biomass, and is that the agreement of the group, because

that was the motion, and so we don't have to do them for female only?

CHAIRMAN NANCE: That's right. It's just total biomass, yes.

DR. SIEGFRIED: Then we would -- Go ahead, Lisa.

DR. AILLOUD: Sorry. I just want to clarify, because I got a little lost. Are we doing Fmax projections, or are we considering an SPR alternative, or --

DR. SIEGFRIED: We have to do Fmax, because that's in the TORs, and then we could provide another run with like F SPR 30.

CHAIRMAN NANCE: Yes, and I think both of those would be appropriate.

DR. SIEGFRIED: Is SPR 30 what the SSC is asking for, for consideration?

CHAIRMAN NANCE: Is there any issue with not using SPR 30?

MR. RINDONE: Again, you're just looking at it at this point.

CHAIRMAN NANCE: Right. That's right, but is there -- Are we going to use the SPR 30, or is there another one that we want to look at, while we're doing this, or -- I am just opening it up, just to make sure, but I think that's what we've used in the past, and so we have Fmax and F SPR 30. Those are the two that we're going to look at, using the total biomass model and with the red tide put into 2021.

DR. SIEGFRIED: We can't provide an F rebuild until we know how many years into the future the stock needs to be rebuilt by, and so that would be the next step, which means we're not going to come to a final answer at the SSC meeting, but it seems like it's something that could be done in a follow-up, because these decisions, particularly like what I showed you for amberjack, the decision about which recruits to use when setting our benchmarks, has implications on that rebuilding timeline. Like Lisa showed in her table, and I forget which one it's one, but the one where you showed the difference between the female and total biomass, and you showed the difference between the F SPRs.

MR. RINDONE: Katie, two more things. For the rebuilding timeline, Magnuson dictates that we have to try to rebuild the stock within ten years or a generation time, and so I think seeing what it looks like to be at F zero and then within ten years and a generation

time are your F rebuild trajectories.

The other thing is that we are likely to have the council ask what the allocations would look like if rebalanced under FES, and so I know that's additional work to run through the projections, but is that something that can also be looked at, and so basically using FES under the past allocation scenario?

DR. SIEGFRIED: You're talking about the way that we've shown the equilibrium yield under CHTS versus FES, or are you talking about rewriting what the regulations would have been?

MR. RINDONE: What would the allocations have been if we had used FES?

DR. FROESCHKE: I will jump in. The current one is based on the landings from 1986 through 2005, and so I guess the ones we were thinking is the one that's on the books, and then whatever the calculation would be using the 1986 through 2005 landings, and then likely this option of keeping the commercial quota constant and then giving the rest to the recreational, whatever that percentage would be.

DR. SIEGFRIED: Okay. Well, what I would like to propose then is that we have Lisa do the immediate projection needs, and then, if there is time, we can do that, but that's usually in a follow-up request, and we just have the two days, at this point, to complete this, and you all have two other big documents to review, and so I am not sure that we can get that last part done, but if I could just talk with you, John, offline about that and see whether it's even possible to consider that at this point, but we do need to do the projections first, rather than provide that, right?

MR. RINDONE: Well, those would all be different -- Those would all yield, potentially, different fractions for F current over MFMT and SSB current over MSST, correct?

DR. SIEGFRIED: I mean, the effects of allocations is -- We haven't looked at the effect of allocations yet, and we still haven't finished up just the first day's projections.

MR. RINDONE: I understand that, and I think my point in that is that these are things that we know that the council is going to request, and I think that, yes, we need to resolve the issue in front of us, based on the specifications for the projections that the SSC has already given forward, but we also know that these are things that are likely to be requested, and so, if we can accomplish them by this meeting -- I think the expectation should

be that it's going to be requested at some point in the very near-term.

DR. SIEGFRIED: Absolutely, yes, and, if we can do them in the next few days, we will, but, if it means not doing the projections well, because we have too many things on our plate, we need to prioritize the projections and do the other requests after the meeting.

MR. RINDONE: Of course. Yes, the priority should be using the status quo allocations, as things are, and then to write down on the to-do napkin to hold the commercial poundage constant at where it is now, and then reexamining what the allocations would have been using FES data from 1986 through 2005.

DR. SIEGFRIED: Okay.

CHAIRMAN NANCE: Shannon, I would like to hear your comment.

DR. CALAY: Not to add to the confusion, and thank you very much, Chair, but there was some question of whether or not in fact this SPR 30 that was derived from this assessment is higher than the Fmax. We have seen that in previous assessments of gag grouper, although that may have arisen from using female-only, or because of the hermaphroditic transition, and so, if it is -- My point is, if we're in a situation again where F SPR 30 is actually higher than Fmax, then that would not be a reliable proxy, in our minds.

CHAIRMAN NANCE: Okay. If SPR 30 is higher than Fmax, we would then revert to Fmax.

DR. CALAY: Or, alternatively, you would have to choose a proxy that is -- Fmax is meant to represent kind of a high estimate of FMSY, and so certainly, if F SPR 30 should not be considered, it would not exclude considering F SPR 40 or a higher value.

CHAIRMAN NANCE: Okay. I agree with that. Are you planning to run 40? That's just a question. Will.

DR. PATTERSON: I am not in favor of looking at the results and then picking which one we prefer, necessarily. However, Steve Saul earlier floated the idea, very subtly, I thought, about F 40 percent SPR, and I think it warrants some discussion, given the life history of gag in particular, that we do talk about 40 percent SPR.

CHAIRMAN NANCE: Will, let me ask you this. Would it be better, for the run, to do Fmax and F 40 SPR?

DR. PATTERSON: I think Steve was going to say something there, and I would like to hear what he has to say.

CHAIRMAN NANCE: Okay. I'm sorry.

DR. SAUL: Thanks. I think it would be useful, and so we used to manage -- There was a time, I believe, and correct me if my memory is wrong, but I believe there was a time when we managed most of the groupers at 40 percent.

Given that this particular species is a little bit unconventional, given that Fmax may be -- Sorry. That the SPR 30 may be higher than Fmax, personally, it would be interested in seeing -- I would lean more towards an SPR 40 percent recommendation for most grouper species, particularly those that aggregate to spawn, like gag and if we were doing Nassau groupers.

With red grouper, I am not as concerned, because they spawn pretty continuously, but, for these things that get together that spawn at a fixed time of year, it might be worth exploring. Then I also just -- With that, it kind of brings us back to the steepness conversation a little and sort of the caution there of just projecting with one fixed steepness value. Thank you. Again, all of that while being sensitive to the Center's workload and to the amount of work that it takes to really explore all of these questions.

DR. SIEGFRIED: To that point, Chair?

CHAIRMAN NANCE: Yes, please.

DR. SIEGFRIED: I would tend to agree with Steve and Will, in that, because of that hermaphroditic transition, that we probably shouldn't consider F 30, in addition to the fact that, the last time we considered F 30, it was greater than Fmax, meaning that there is something going on there that -- Anyway, I think that it would be better to do it on these principles of what type of species we're looking at, what the literature says, like the Harford et al. paper, rather than just giving you both and letting you choose based on the results or some sort of -- I am not really sure what you else you have to choose from, if we just provided you three options without any other complementary materials.

I did want to ask about the rebuilding timeline that Ryan brought up earlier, because I think we maybe glossed over that just a little bit too, in that we need to calculate F equals zero for ten years. If the stock rebuilds, then that's one answer. If it

doesn't, then we add generation time and calculate an F rebuild at the end of that, and so it would basically be eighteen years, because what Lisa is showing you is a generation time is about eight years. You take the ten, and you add the eight, and you have an eighteen-year timeline, and is that correct, Ryan?

MR. RINDONE: It's how long does it take the stock to rebuild at F equals zero, what F is required to rebuild the stock within ten years, and then the generation time, and so those three circumstances when a stock is overfished.

DR. SIEGFRIED: A generation time is added to --

MR. RINDONE: The ten-year rebuilding period, and is that correct, John? We will have to look up the generation time, but the first two are dead givens.

CHAIRMAN NANCE: I am going to throw this out here, just for a second. For Thursday, let's not look at any different allocations, okay? That's going to be off the table. That's just too much trying to do, and so I want to be able to see SPR 30, and that's what we have used before, and Fmax, and those are the two scenarios, I think, that are appropriate. Harry.

MR. BLANCHET: We have said that Fmax might be more appropriate than F 30, and the question that I have is, if Fmax comes in as below F 30, is that also going to be considered?

CHAIRMAN NANCE: It probably is unlikely, but at least we would be able to consider that, Harry. Mandy.

DR. KARNAUSKAS: I think Katie already said what I was going to say, but just to pile on the comments of what Will and Steve were alluded to, in an attempt to not be arbitrary with the proxies, and I just wanted to point out that the Harford paper did recommend SPR 40 percent as the highest probability of achieving long-term MSY.

CHAIRMAN NANCE: I think I understand, and so, Doug.

MR. GREGORY: Thank you. If we know that the council is going to ask for different allocation scenarios, and each one is going to have a different ABC, can we simplify things and not do ABC at this meeting and wait until we do the full run?

CHAIRMAN NANCE: I would rather, at this meeting, and Carrie can correct me if I'm wrong -- I will hear from Carrie first.

EXECUTIVE DIRECTOR SIMMONS: Thank you, Mr. Chair. We just talked about that, Doug, and I think there's some things that I think the SSC kind of needs to wrap up on this, perhaps, and then with the current allocation, and then we take that information to the council, perhaps with an idea of what allocations the council may want to look at, and then we take that back to the Science Center and then the council at this point, but I just feel like we have quite a few things up in the air regarding Fmax and some of the projections and the things the SSC wants to see moving forward, and, if we could at least take a look at that, we might have a better understanding of where we need to go at the next meeting. Thanks.

CHAIRMAN NANCE: I agree with that.

MR. GREGORY: If we calculate the ABC, we can take it to the council saying this is what it would be under the current allocation, and this is not a recommendation, and what do you want us to do as the next step? Boom.

EXECUTIVE DIRECTOR SIMMONS: Right, and, ideally, we would have at least what that new yield stream would be in the CHTS equivalent as well, if possible.

DR. CRABTREE: It's linked to the GRFS decision, because, if they want to use GRFS, then that's a different allocation set than if they use FES, and I don't think they want to change the allocations to FES and then switch them back to GRFS.

CHAIRMAN NANCE: I think, like Carrie is saying, we go with this for Thursday, and we see where we're at and present that to the council, and then we can move forward with council recommendations.

DR. CRABTREE: Jim, do we still want to look at F 30 percent, or do we want to just leave this at Fmax, because the workload is getting so out of control?

CHAIRMAN NANCE: There's only two runs there. There is Fmax and F 30. Katie and Lisa, is that acceptable, to look at those two different ones.

DR. AILLOUD: Yes.

CHAIRMAN NANCE: Okay. So, for Thursday, those are the two we'll look at, and also with the red tide. Okay. Anything else from the SSC? We're not going to be able to get to Dr. Gordon today. Ryan will talk with him, and we'll move him to Thursday, also.

MR. RINDONE: Yes, and I've talked with Dr. Diagne, who will reach out to Dr. Gordon and move him to first thing Thursday morning. I have talked to Dr. Stunz, and he and his team will be prepared to begin one hour after they were originally scheduled, and then your regularly-scheduled programming will resume after that point.

CHAIRMAN NANCE: Lisa, thank you for that excellent presentation. It was very helpful to be able to run through gag grouper with that presentation, and I appreciate all the SSC being able to be here and be able to participate in that, and I thought it was a very productive day. We'll see you tomorrow at 8:30.

(Whereupon, the meeting recessed on September 28, 2021.)

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September 29, 2021

WEDNESDAY MORNING SESSION

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The Meeting of the Gulf of Mexico Fishery Management Council Standing and Special Reef Fish, Special Socioeconomic & Special Ecosystem Scientific and Statistical Committees reconvened on Wednesday morning, September 29, 2021, and was called to order by Chairman Jim Nance.

CHAIRMAN NANCE: Good morning, everyone. I appreciate you being here on day three of our SSC committee meeting. Today, we have, for our agenda item, it's estimation of total red snapper abundance in Louisiana and adjacent federal waters, and so, as we proceed through that presentation, we certainly want to be active in a committee, asking questions and those types of things.

There was a peer review that was done on this study, and, as we get to that point in the agenda, we're going to have Louisiana Wildlife and Fisheries, the ones that funded this project, speak a little bit about the review process, so we have a better familiarity with what was going on there. Dr. Gallaway, I guess I will turn the time over to you, for your presentation.

LGL: INTRODUCTION: ESTIMATING ABSOLUTE ABUNDANCE OF RED SNAPPER OFF LOUISIANA PRESENTATION

DR. GALLAWAY: Thank you, Jim. As Jim stated, this is a study for the State of Louisiana that was conducted by LGL. On the first of

November 2019, Louisiana entered into a contract with LGL to estimate total red snapper abundance in Louisiana waters. The specific objectives of the study were to, one, determine species composition at 106 sampling sites at predetermined locations in the Gulf of Mexico, per approved sampling methodology.

At these sites, our goal was to conduct hydroacoustic submersible rotating video and biological sampling for finfish at these same 106 sites in Louisiana waters. In addition to this, we conducted water column surveys with each sampling period, and the last item on the objectives was to conduct a mark-recapture study at a subset of six sites, one platform, and one artificial reef site in each of three regions. Of importance, this study was required to be compatible with the Great Red Snapper Count.

I have added this slide, and I realize that I had not included an organizational chart, and so I added it to the presentation. I was the principal investigator. I joined LGL in 1974, and I am president of that company. Scott Raborn was our Chief Biostatistician. He joined LGL in 2007, and he is our Vice President. Scott is well qualified in statistics. He received his doctorate in 2000 from Mississippi State University, majoring in fisheries ecology and minoring in statistics. He serves as statistician throughout the entire LGL company.

Jack Egerton was our hydroacoustics advisor. Jack worked with us on the BOEM platform study, as a subcontractor through the Marine Science Institute of the University of Texas at Port Aransas. He has recently joined us as a formal external associate, and so these were our three main leaders of the project, and we will be doing the presentations today.

In addition, we have Kyle McCain, who joined LGL in 2013, and Taylor Beyea, who joined us in 2018, who led these same types of tasks for the BOEM platform studies in 2017 and 2018. We also had Dr. Will Heyman, who joined LGL in 2013, who led the biological sampling at open bottom sites, or uncharacterized bottom.

I wanted to move, if I can, to Slide 122 forward, is the next slide. I think a great deal of credit for the success of this program was based -- We gathered 100 percent of the data at the 106 sites, and that was -- Our success is due, in large part, to our captains that we used from the charter boat industry. We'll have them up here in just a second, and it was a pleasure to work with these guys. The vessels were excellent.

Here they are, and going from left to right is Jamie Gaspard from Louisiana, Hans Guindon, Mike Jennings, Buddy Guindon, Scott

Hickman, and Bill Butler. These guys, it was a pleasure to work with. They worked hard, and their boats were fast and comfortable. Well, maybe not comfortable. We worked hard on the boats, but they were perfect for this work, and a large part of our success is due to them.

The study was believed to be necessary when it was learned that unforeseen circumstances had curtailed the plan for sampling in Louisiana. The Stunz et al. 2021a, which is the initial Great Red Snapper Count study, developed an ad hoc estimate for Louisiana, based largely on sampling in waters adjacent to Louisiana, but they were actually in Texas.

However, it was determined that sampling directly in Louisiana waters was needed, and the contract was subsequently let to LGL to sample the 106 preselected sites. These sites had been selected by the Louisiana Department of Wildlife and Fisheries, following extensive collaboration with researchers throughout the Gulf of Mexico.

Some milestones in the project included, one, a proof-of-concept study conducted in March and April of 2020 and reported in May, to finalize the final field sampling and logistic plans prior to conducting the field studies. All 106 sites were successfully sampled over the period of May 18, 2020, to September 6, 2020, despite COVID and a bad hurricane season. Sample and data analysis occurred from September of 2020 to June of 2021.

The original Great Red Snapper Count was first released in March, followed by a revised report on March 15, and the report was reviewed by the Scientific and Statistical Committee of the Gulf of Mexico Fishery Management Council and by outside reviewers during March 30 through April 2, 2021. We did not receive the final version of this report, which I have referenced as Stunz et al. 2021b, which was published in August of 2021, and we did not get it until early September, and so we relied mainly on the draft report. However, we did look at it, and we did modify our report in critical areas, based on the final Great Red Snapper Count published in August of 2021. Sorry for going so fast, but we've got a long day, and so I'm trying to get through these introductory materials quite quick.

Our draft final report was submitted to the state on June 2, 2021, and the corresponding data report was submitted on hard drive on June 11, 2021. It was submitted separately, and the reason for this, I have since learned, for submitting it separately was to enable the state to enter these data into their database, complete with instructions, and the reason for this was to facilitate its

distribution to people who requested the data, and so those data are, or will shortly be, available in electronic format to the general public. All you have to do is request these data.

The draft final report was sent out by the state for outside peer review shortly after submittal, and it was reviewed by two external reviewers and one internal or state reviewer. Comments were received from External Reviewer 1 on August 16, 2021, from External Reviewer 2 on August 25, and from the internal Louisiana reviewer on August 29. Our responses to the reviewer comments were submitted to the state and the SSC on September 9, 2021. The revised report was submitted to the state and the SSC on September 13.

Here is where, Jim, I think you wanted to defer to the state to talk about the peer review comments, and then, Jessica, we will pull up the overview of the peer review comments in the second presentation, and so, Jim, I return it to you for the state.

CHAIRMAN NANCE: Thank you. We would like to have -- Is Harry on?

MR. BLANCHET: I'm on.

CHAIRMAN NANCE: I'm glad you're on, and so could you just provide a general oversight of what the reviewers were asked to do and those types of things, please?

MR. BLANCHET: Sure. I don't have -- I wasn't really prepared for this, and so I'm going to kind of wing it, and so feel free to ask some questions. The general request -- We were aware that the SSC was going to be reviewing this document, and we wanted to have some peer review from some people who were not on the SSC, because that would be redundant, and we wanted to have people who were proficient or highly experienced in some of the same kinds of work as this was, and with the Gulf of Mexico and Louisiana.

Having those parameters limited a lot of the available people, because, as you know, a lot of the work that gets done in the Gulf of Mexico gets done by people on the SSC.

The main charge to the reviewers was please give this a good review, but -- I mean, is there any specific questions about it? I mean, I expect this to be reviewed just like you would any manuscript for a peer-reviewed journal, and so anything from missing a comma to that's an erroneous assumption. I don't know what else to say.

CHAIRMAN NANCE: Will, did you have a question, please?

DR. PATTERSON: I don't really know where is the best place to ask this, but, since Harry has the mic now, maybe he can best answer this, but, in the report, it says, as indicated in the request for proposal, the selection of 106 sites resulted from several months of collaboration between LDWF with researchers throughout the Gulf states, and these data were used by a contracted ecological consulting firm to assist LDWF in the development of a research protocol study design.

In Benny's opening remarks, he indicated that these 106 sites were predetermined by LDWF, but I don't see in the report, or at least I can't find it, how -- What kind of design, sample design, was utilized here to select these 106 sites. Was it haphazard, or was it totally random, or was it stratified random? What was the actual study design?

MR. BLANCHET: I was not directly involved with that part of the study design myself either, and so I can -- If I comment, it's from recalling reading some emails over a year ago, and so I may not be the best person to answer that question, but, in general, the design was random within strata for various habitat types. Does that make sense? Now, there was some pairing of sites that occurred to try to better understand differences between sampling types or habitats, but, beyond that, it was kind of a random draw within.

Now, within some of those, and so, if you look, there are anything from shallow to edge of the shelf, and some of those you may have very small numbers of sites within a category, and so, for instance, deepwater oil rigs, I don't know how many there were to draw from, and so that, I believe, was part of the design, but, again, I was not directly related to it, and I only can tell you what I observed of what came out of that process.

DR. GALLAWAY: Will, I think, as I go through the report, that will become a little more apparent, and so I think, if you can hold this, if we don't answer it successfully, ask it again towards the end, and I will address it again.

DR. PATTERSON: Okay. Thanks, Benny.

CHAIRMAN NANCE: Sean.

DR. POWERS: My question is similar to Will's, and so I will wait to hear the response, but, Harry, relative to the review, was the way the 106 sites were chosen in the overall design part of the review, or, if it wasn't in the report, it wasn't what you sent

the external reviewers?

MR. BLANCHET: That site selection was not part of the review.

DR. POWERS: Okay. Thanks.

MR. BLANCHET: They would not have had access to anything that you guys did not have access to. Now, that being said, if there had been a request from a reviewer for that information, it could have been provided.

CHAIRMAN NANCE: Thank you, Sean. What we're trying to do here is each project that we get, that the SSC looks at, has different levels of peer review, and we would like to be able to just -- If you have questions on that peer review process, what was asked, those types of things, I think this is the place to ask that. We can certainly ask that during the presentation too, as Benny goes through this.

DR. GALLAWAY: I submitted the general results of the peer review, the overview comments, as well as the detailed comments, which was basically marked on the text, prior to the deadline of September 13, and so the general review comments and the detailed comments are in your information materials. I didn't know exactly what was wanted here, and so what I did is summarize -- The next slide will go through the reviewer comments, and I don't know whether you've had a chance to look at these or not, and so we'll briefly and quickly go through the reviewers' comments, if that's appropriate, Jim.

CHAIRMAN NANCE: That is. Let me just add one thing before, and that's just, as we go through this, just like we do with anything, we as a body of the SSC utilize what the reviewers' comments are, what the reviewer process was, what we see the analysis was for the presentation, what the study shows, and things like that. We take all of those things into consideration while we make our deliberations, and so I just wanted to just remind us of that, but go ahead, Benny, please. Thank you.

DR. GALLAWAY: Again, the reviewers were selected by the state, and the reviewers were blind to us. Our Reviewer 1 general comments is the next slide. Reviewer 1, subsequently, in his response, identified himself to us, and so Reviewer 1's comments are that he appreciated our work, and he was generally consistent with our approach, and he liked our approach.

He provided detailed comments on the pages of the report itself, and we addressed all of those, and those are provided in your

information material, and I will not go over those, and they are quite extensive. He chastised us a little bit for not citing some appropriate work. We added that work to our final report, but all these studies have now been referenced.

He asked if there were lights on the camera. There weren't, and they weren't baited. We sampled during daylight hours, and both camera arrays, both stationary and towed, were not baited and were without lights. We had a misspelled word in a figure, which was quite apparent, and that was corrected.

He was quite concerned how we tied up to the rig and how we conducted our fishing operations. We responded that we did not tie to the rig, and we used a dynamic positioning trolling motor to hold position. Orientation of the vessel to sampling sites was dictated by the prevailing wind and wave directions. Bait and hook deployments shifted around and were not held static, as depicted in Figure 12 of the report. Here, I think I will refer to Scott to address his comments on how we addressed age and growth with the von Bertalanffy growth models. Scott, can you take this response?

DR. SCOTT RABORN: He was just wondering why we didn't force the T0 parameter to zero, because you don't have any samples, or fish, young enough to really define that parameter, but a recent paper of, and I can't remember the citation, but it argues that -- It was Sebastian et al. 2013, but, in a von Bertalanffy curve, because all three parameters are so correlated, if you hold one constant, it does affect the other two parameters.

One common pitfall that people fall into when looking at a von Bertalanffy curve is they try to interpret a single parameter in isolation, and it's just difficult to do that, and so the Sebastian et al. demonstrated that you actually get a more biased von Bertalanffy curve when you fix the T0 parameter, and so, for that reason, we just used three parameters.

I would also like to point out that the von Bertalanffy curves that we generated -- We did those quickly, and there are other methods that you can add to it. We can let the variance change by each age, which we did not do, and this is just a very simplistic, quick-and-dirty, fit to those data, and so I will just leave it there.

DR. GALLAWAY: Thank you, Scott. The next slide is simply the references that are included in that response. Dave was commenting on the natural banks, and he thought our estimate of shelf-edge banks red snapper population were low, and that was a change now,

and those have been increased, and he was also very upset with us calling ten to seventeen-year-old red snappers old, since they live to age-fifty-five, and so we agree with his comments.

Just a comment that, when platforms -- When explosives are used, and there is demolition at the bottom, it knocks off all the biota, and they have to recolonize, and so we agree with that comment. He is basically saying that we need more uncharacterized bottom samples to be taken, and we certainly agree with that.

He was comfortable with our estimate of six-million, as compared to twenty-six to twenty-eight million, but he acknowledged that the Great Red Snapper Count had very little data from the waters off of Louisiana and that, if more sampling was done by both groups, the estimates would converge somewhat. Scott, do you want to take this one too, that's having to do with --

DR. RABORN: Yes, and that was just -- This is a holdover from our BOEM report, and it's just looking at a way of comparing von Bertalanffy growth curve fits, but that's probably not the best way to do it, and so we just deleted this statistic from the report.

CHAIRMAN NANCE: Benny, just real quick, I'm not sure about going line-by-line for each of them.

DR. GALLAWAY: Okay. I didn't know how you wanted to do this, and so I was just guessing.

CHAIRMAN NANCE: That's fine. I guess, if there are real substantial questions and stuff from the reviewers, let's go through those, but I will let you use your discretion on those.

DR. GALLAWAY: Then I would go to -- Let's start with Reviewer 2, who did not identify himself to us. He provided a summary of his characterization of what we did, and he provided an accurate summary, and he concluded that the LGL Louisiana estimates, since they were based on sampling in Louisiana, provide some prima fascia support for using the LGL data.

On Slide 19 of these sets of slides, at some point, there should be a discussion of the selection by the suppliers of the site locations, and we provided the additional text on page 1 of the report, and we included the RFP as Appendix 9, which provides more detail, and I will be going through more detail as we go through the presentation.

He basically -- He approached how he's going to address this, and

I think, on Slide 21, the next slide, he again outlines his interpretation of what we did, and he characterized it well, and he noted that, and you might want to look at these, if you haven't, but that Tables 1 through 3 in our report are particularly helpful in understanding the relative sampling that has been supplied.

If we go to Slide 23, where he asks some questions about our hydroacoustic sampling and in situ target strength, Jack, I would like if -- Can you address this response on Slide 23, with target strength?

DR. JACK EGERTON: This one was -- It's the source of variance measurable, depending on what thresholds are used. I mean, in theory, it could be, but, in reality, it's very difficult to do. As you will see later in my presentation, there is forty or fifty different operators, where different thresholds are set, and so changing each one of those in turn, and seeing the end results, in terms of numbers, ends up being an almost infinite task.

In practice, really, what people do, both ourselves and also in the Great Red Snapper Count project, is you see what data you've got, and you set the thresholds according to what is actually there in front of you, the whole sort of aim being to filter out those responses, which aren't from the species of interest, and so I think it would be very difficult to do that.

DR. GALLAWAY: Okay. I would like to skip to Slide 27 for another highlight. Reviewer 2 noted that some important tables in the report may provide clarification, and he observed that, while there may have been biases in the sampling, at least our protocols were consistent, and, thus, it was felt, by this reviewer, that catchability effects on red snapper composition would be minimal.

The next slide, Number 28, again, he is explaining what he -- His interpretation of what we did on Slides 28 and 29, and he accurately described our methodology. In Slide 30, he noticed that we did a modeling plus a mean count estimation of total abundance, and he noted that we did not explicitly recommend that the model site method was preferred to the mean site method, but that we focused on the model site, and that's true. Our model site estimate is the estimate that we endorse as being the most appropriate for this study.

On Slide 31, he further provides rationale for using the model site and suggests that we address this topic in more detail, and we will address further, as necessary. He also provided comments on the mark-recapture estimates, to which we will address in the future. Keep in mind that this was a very short-timeframe project,

and our main objective was to estimate the absolute abundance of red snapper in Louisiana waters, and so, really, given the timeframe, we did not go into, or have time to go into, the normal amount of explanation that we would typically do, and we plan on doing this as we go forward with publishing these results.

On Slide 33, he recommends that the preferred estimates of Louisiana red snapper absolute, at this time, be generated by the model site method, and we certainly agree with that, and we added text recommending that model site abundance indeed was the preferred method, and he, in Slide 34, suggests further exploratory analysis, and we certainly agree with that. As I indicated, the short timeframe had a lot to do with the rather abbreviated explanation of some of the results, and these will be addressed as we go forward and look to publish these results.

Reviewer 3, this was the Louisiana internal review, and, here again, I would like to emphasize that the reason for submitting the data electronically, under separate cover, was to enable Louisiana to incorporate these into their database to facilitate distribution to request for these data, and so these data are, or will shortly be, available to anyone who would like to see them, by requesting them directly from Louisiana. Harry, or anyone from the state, would like to make sure that I have got that clear, feel free to speak up at this time.

MR. BLANCHET: The only thing that I would add is that, when we get a data request for fishery-independent data, we provide the data, and we provide the information, any meta-data that we have, and we do not do analyses of that data. We do try to provide the requestor with the information that would give them the context and understanding of how it is -- How that data is possible to be used, and the long-term purpose for this is that, on many occasions, we have had people request data and then attempt to use the data in ways that might not be suitable for the way the data was collected.

Really, it's up to the requestor to be doing whatever analyses, but we try to give whatever guidance we can with regard to sample design or the meaning of various parameters, because, as anybody who has worked with some datasets knows, some parameters might not be self-evident, in terms of what that value means.

DR. GALLAWAY: If we go to Slide 37, Reviewer 3 noted that we used different thresholds than the Stunz et al. work. It's not really evaluated, to see what difference that would make in the final estimates, and I would like to, again, if I can, Jack, have you address this. We do have an appendix in the report directly

comparing it to the Stunz et al. report, and Jack has provided this response, and, Jack, if you can basically speak to this issue as well.

DR. EGERTON: Sure, Benny. I actually cover this a little bit in the presentation that I will be doing shortly, but, essentially, we didn't have the detailed methodology when we were sort of going full-on ahead with the analyses.

There are some differences between the two studies, and there is actually more differences within the Stunz et al. study than there is between ours and what they did in Florida. Ours, effectively, is very similar to the methodology they used in Florida, and what they did in Florida and what we did are both quite different from what they did in Texas, which they extrapolated those numbers from, but, yes, I think I cover this pretty adequately later on in my presentation, and so I think that will probably be dealt with later, if that's okay.

CHAIRMAN NANCE: Yes, that's perfect.

DR. EGERTON: Okay. Thanks.

CHAIRMAN NANCE: Benny, did we lose you?

DR. GALLAWAY: Yes, because I did not unmute myself. What I indicated was these were the review comments that we received, and all but one were blind reviews, and there are our responses, and we provided detailed responses and the explanatory variables, and, if anyone has comments on the peer review, I guess now would be the time to mention those. This is what we received.

CHAIRMAN NANCE: Thank you. Any comments from the SSC, or any questions about the peer review process or the responses to the peer review? Will.

DR. PATTERSON: It's an interesting way to solicit peer reviews, where you have an internal reviewer and then the first reviewer indicated that he had collaborated with LGL, and so I focused most of my attention on trying to understand what Reviewer 2 was saying.

Reviewer 1, the comments that Benny just went through here, one comment that kind of stuck out to me was the idea about the unconsolidated bottom, and he had indicated that there were few samples extrapolated across a large area, and this is where the design question I think really is important, because, if you have -- If you base your design with some information about what the variance in density, or abundance, is, and expansive habitats,

then you can have a small number of samples, if the variance is low among samples and the samples that you take are randomly distributed across the habitat.

That's what sampling design aims to do, is to economically allocate samples randomly based on the variance that you know about that habitat, or region, and what your threshold is that you're shooting for on the backend, and so I disagree with that comment that it's nonsensical, I guess, to take relatively few samples across a large area, and it depends on what the variance is among your sites, and it also relies upon a random distribution of samples, and then you live with the result.

CHAIRMAN NANCE: Very true.

DR. GALLAWAY: I have no problems with that assessment.

CHAIRMAN NANCE: Okay. Any other questions from the SSC?

DR. GALLAWAY: Okay. Then I would like to now move back to our presentation, per se, and this will be what I think is Slide 8. It's following the slide immediately after -- This is it. It's Slide 10 in this version, and I've got an old version.

What I plan on doing now is to first describe our study area and provide a characterization of the major types of habitats that were represented within this area, and then I will describe the 106 designated sampling sites that were located within the study area, and I will then describe the field sampling strategies for each habitat, which were divided into discrete sites, which were artificial reef, petroleum platforms, pipeline crossings, natural banks, and then the second type of was uncharacterized bottom. The field method section is followed by descriptions of data and statistical analysis and then, lastly, by the results and a summary and conclusion section.

The study area and habitats, the Louisiana red snapper management area was divided into three regions, west, central, and east, as indicated in the figure below this comment, each of which was divided into four depth zones. There was shallow, which was ten to twenty-five meters deep, mid-depth was twenty-five to forty-five, and deep was forty-five to 100, and the shelf edge was 100 to 150. We later combined the deep and shelf zones as a single zone, and so that is now forty-five to 150 meters deep, and so these show an overall description of distribution of the sampling sites across the Louisiana waters.

As noted, the 106 sites were sampled in the summer and fall of

2020, and the data for thirty-seven petroleum platforms that were present in the study area and sampled as part of the Bureau of Ocean Energy Management study were also included in the analysis.

Before describing our specific sampling sites, I would like to first provide an overall background for red snapper habitat provided in the study area. Areal coverage of all discrete and UCB habitats within each region and depth zone were determined using GIS, and we used an Albers Conic Projection with North American Datum of 1983, or NAD83, and this projection is centered at 91.5 degrees West, 28 degrees North, on the area of offshore western Louisiana and encompasses the study area. Selection of this point served to reduce distortion when calculating areal coverage of bottom habitat.

The extent of UCB habitat was estimated from the usSEABED Bottom Sediment Database, and the appropriate reference is given, and this is a gridded database that estimates percent coverage of bottom sediment, which is rock, mud, sand, and gravel, within each grid cell, which are 2.2-kilometers-by-1.96-kilometers, and we considered uncharacterized bottom, or UCB, as being those grid cells that had less than 66 percent rock.

This provides a distribution of the uncharacterized bottom, in terms of its composition. As you can see, a large part of the eastern and central is dominated by mud habitat throughout the area, with very little sand and gravel, except when you get over to the west. If you extend this map into Texas, you will see a big area dominated by sand and gravel habitat on the upper Texas coast, and so this is the distribution of the uncharacterized bottom by sediment type that we used in our characterization.

The Louisiana study area was dominated by mud substrates, with lesser amounts of sand and gravel substrate, expect in the west shallow zone. We calculated the total area of UCB in Louisiana, or unconsolidated bottom, to have been on the order of 49,000 square kilometers, and this compares favorably to the Great Red Snapper Count estimate of 53,052 square kilometers. The difference is less than 10 percent.

For our analysis, again, I want to point out that we deleted the shelf edge area, which was not sampled, and so this table provides our three regions and depths, the areas of each of those, the number of sites over uncharacterized bottom, the area sample, and the percent of the area sampled, and so this is a summary of our sampling of uncharacterized bottom.

The natural bank habitat was estimated using the natural bank

coverage obtained from the Gulf States Marine Fisheries Commission, in combination with the areas from the usSEABED dataset having greater than 66 percent rock coverage.

A summary of our estimate of natural bank habitat and the areas sampled shows a total of 724 square kilometers of natural bank habitat offshore in western Louisiana, which is itemized in the table shown below. Our estimate of 724 square kilometers compares quite closely to the Great Red Snapper Count estimate of 821 square kilometers, and there is only about a 12 percent difference.

The next slide shows the distribution of the natural banks, which, as you can see, are mainly on the shelf edge, except for Sonnier Bank, which is shallower and a feature here that we did not sample, and I don't what that is.

Our next slide includes other artificial reef habitat, and what we used for artificial reefs, if you sum them up, was mainly standing platforms, of which, in 2020, when we conducted the study, there were 821 in place, and pipeline crossings, where each of the pipes was greater than twenty inches in diameter, and there are 514 of these, and then reefed platforms by the State of Louisiana.

Collectively, if you add these up, these provide a total of 1,777 discrete habitats, and these estimates are almost identical to the latest Great Red Snapper Count estimate of 1,771 artificial reefs offshore in western Louisiana. In the following maps, I'm going to show the distribution of platforms and pipeline crossings, which occur off all regions and depths, whereas reefed platforms occur mainly in the mid and deep depth zones.

This shows the standing platforms, which are distributed throughout Louisiana, especially in the eastern and central area, and they are less abundant in the western area, and so the next slide shows the pipeline crossings, where the pipes were greater than twenty meters, and, again, as you see, they are distributed across the area, as depicted by this figure.

The next slide shows the artificial reef zone, and there are 442 of these, by our estimates, and these are reefed platforms put in place by the State of Louisiana, having this distribution. In addition to these, which were our main sites, there are some other sites that we did not sample, like small caissons, and there are 147, or documented obstructions and wrecks, and there are 132 charted obstructions and fifty-six wrecks. These were also present, but we did not sample. We did not feel the impact of not including these would be large, or that the impact would be relatively small. I will give a map of the distribution of these

structures, and you will see that most of these are restricted to shallow and mid-depth.

This is the distribution of standing caissons. As you see, they're essentially in shallow zones. The next slide shows the distribution of obstructions. Again, they're in shallow and mid-depth. The wrecks are the next slide, and, again, it's shallow, as it's indicating.

If you look at the 106 total sampling sites, thirty-seven were located in the west region, thirty-three were in the central region, and thirty-six were in the east region, and, of these sites, fifty-five were discrete reef sites, whereas fifty-one were UCB sites. UCB sites included only thirty-nine unique sites, but paired sampling was performed at twelve sites, and these are areas having the same substrate, but with and without pipelines, to enable us to look at the effects of pipelines.

Note also that the UCB hydroacoustic surveys were conducted by Auburn University, and they used alternative site numbers, which were assigned for internal tracking purposes, and, in Index 1 of our report, we provide a key to relate Auburn numbers to the original Louisiana Department of Wildlife and Fisheries site numbers. Our samples for UCB habitat, again, did not include any taken from the shelf zone, which constitutes about 8 percent of the total sample area.

This shows a distribution of the sites that were actually included in the study and were sampled, and so we had pretty good coverage throughout the State of Louisiana. What we attempted to do, as you look at these sites, is provide more detail, and so, if you go to the west region, the next slide provides the location of the site, type of habitat, or the site type, and it shows the sample type, where we collected composition samples, and we used either a vertical hook-and-line or longline samples to collect specimens, and this designates which approach was used for each site and describes what the habitat type is. Again, the region, the depth, and the name, and the schedule for sampling that site.

The next slide provides detail, and I do this so that -- These maps get rather small in the report, and I want you to be able to relate each site to its characteristics, and so these were the west sampling sites. As indicated, we included the BOEM platform sites and the studies over stratified, or randomly stratified -- We selected these, and the platforms are the dominant habitat type in Louisiana, and these were supplemented by other types of samples, including additional platforms across the area, in each of the three depth zones and regions.

The next slide provides the same information for the central region, where there were thirty-three sites. Again, the detail is provided, so that you can see exactly what we did, and so we have a preliminary schedule, and they're also in the report, via completion schedule, which compares the plan to the actual, and so we tried to provide all of the detail about our sampling, warts and all, so that you can objectively utilize and look at these data, if you choose to use them.

The next slide shows a map of these sites in the central region. Again, you see the coverage obtained with the randomly selected plus the supplementary sites that were selected and included, as described, in the RFP. The same detail is provided for the east region, which the next slide I think will -- This is the east region, and, again, the detail and the map, which is the next slide. I don't want to belabor these, and the next slide shows the map of these thirty-six sites. There is a map of the distribution of the sampling in the east region.

These were our station selections, and these were stations that were utilized, which included a combination of the previous study, the BOEM study, as well as the supplementary data provided by this study, and so, with that, I would like to now move to the sampling methods that we used at these sites.

CHAIRMAN NANCE: Benny, we have a couple of questions. John.

MR. MARESKA: Good morning, Benny. As you're going through your presentation here that is covering the 106 sites, I was reading, in the final report, on page 5, where it says the total of 106 sites were sampled in the summer and fall of 2020, and, in addition, data for thirty-seven platforms that were present in the study area and sampled as part of our recent BOEM study, were also included, and I have noticed the BOEM sites are also on your maps that you're presenting.

DR. GALLAWAY: That's correct.

MR. MARESKA: Can you talk about the site selection and distribution of the BOEM sites compared to the 106 sites for the LDWF study, and were there any differences in that study, as far as the work to be done?

DR. GALLAWAY: Those were stratified randomly selected in the BOEM study, and that's described in our BOEM report, which has been submitted, and we also have a paper in press which further defines that, and so similar regions, similar depth zones, and that was

stratified by region and depth and randomly selected within those strata by habitat type.

MR. MARESKA: All right. I will review that paper and compare it further.

DR. GALLAWAY: I can provide that to you, if you will remind me. If you will send me an email, I will send you the final report, if you would like that. You can get it from the government, and I can also send you a condensed version, which is the published, in-press version, which might be more useful.

CHAIRMAN NANCE: Go ahead and send that to all of us, please, Benny.

DR. GALLAWAY: Will do.

CHAIRMAN NANCE: Sean.

DR. POWERS: Benny, correct me if I'm wrong, but this is the only time we're going to be talking about any kind of sample design in your presentation? I don't want to ask a bunch of questions that will then be addressed in a further presentation, but is this the time to talk about sample design?

DR. GALLAWAY: Probably so.

DR. POWERS: Okay. I am trying to figure out the overall design, and so you have three regions, three depth zones, and four habitat types, right? Is that correct?

DR. GALLAWAY: Yes.

DR. POWERS: So what was your replication within all those strata, when you boil it down, the ones you want to resolve, something at a depth, at a region, and at a habitat type, and what is the actual sample, the N, that you're talking about?

DR. GALLAWAY: The N that we're talking about, the numbers of samples?

DR. POWERS: Within all of that, what is the actual level of replication? If it's not balanced, you can just give me a range, and is it four sites, five sites, within each of those?

DR. GALLAWAY: That is provided in detail in the previous tables and maps that I just provided, and that was the intent of that, was to show -- Perhaps I didn't do a good job.

DR. POWERS: I think it's Table 18 or something like that, but I was just wondering if -- I could go through and add them all up, and do it that way, but, if you knew offhand what the individual N were in that combination of strata.

DR. GALLAWAY: If we go back to those initial maps, where we compared the numbers of each habitat, and show the maps of those.

DR. POWERS: Or that table right before you showed the maps.

DR. GALLAWAY: Basically, I attempted to provide the numbers and distribution of those. Here we go. It's starting with uncharacterized bottom, and there is a table for each, showing the numbers and comparing those numbers to the Great Red Snapper Count numbers.

DR. POWERS: Okay, and so, for example, central shallow, with two, in that -- That's just for the uncharacterized bottom?

DR. GALLAWAY: Yes.

DR. POWERS: Okay. I guess I will go through and add those up. The west, you said the natural bank sites in the west and the mid and deep areas, you didn't sample, and is that correct?

DR. GALLAWAY: We sampled the natural banks. If you go to the natural banks slide, which will be a few down the road here, the natural bank habitat, here are the natural banks and their distribution and the total area.

DR. POWERS: Okay, but do you have that -- So I would have to -- To figure out how many sites that is, I would have to go back to the other table?

DR. GALLAWAY: To those other tables. That is correct.

DR. POWERS: Okay, and we'll talk about this later, but --

DR. GALLAWAY: Thank you for your comments, and you're adding things that I will try to add in the next revision, seeing that the --

DR. POWERS: Just one overall table, with the overall design, would be good, and I know that the design was dictated to you and that that wasn't part of your study, but it's just that the western area, that natural banks and all that, if you look at the Karnauskas et al. 2017, it has a lot of the at least high relative

abundance estimates, and so I'm trying to get an idea of actually how many samples were there.

DR. GALLAWAY: I've got you, and that can be derived with the information provided, but we did not -- We provided all of the detail without much of the summary, given the short timeframe of getting this in at this time, and so that's my excuse and apology.

CHAIRMAN NANCE: Josh.

DR. KILBORN: Thank you, Mr. Chair, and thank you for the presentation. I guess I'm maybe a little confused about the intent of the sampling design. It appears that maybe there was some assumptions that went in at the beginning to only sample areas where the fish were presumed to already be, and, if you look at the uncharacterized bottom map, and compare that to the map of the actual study sites, it seems like large areas of like sand, in particular, were just completely avoided.

I know there may not be an assumption that you would encounter a lot of these fish on those habitats, but I'm not quite sure that that makes it statistically appropriate to avoid them in the sampling design, and so could you maybe speak to that rationale a little bit, please?

DR. GALLAWAY: Well, I don't think they were intentionally avoided. I think the platforms, which were the dominant, were stratified randomly selected. UCB habitat, if you go back to that distribution map, I think the map -- Slide 16 shows the distribution, and you're talking about this area, the shallow area of sand here?

DR. KILBORN: Correct. Like, if you look in the western shallow portion up in the top-left area, there is a huge section of that map that was completely unsampled, and so, again, I just -- I don't get a feel for an actual stratified random sampling protocol.

It seems like there was a lot more directed sampling than randomized sampling, and so I just wanted to -- Again, with like the natural banks is another good example, where the majority of the natural banks appear in the deeper depth zone, and that was -- I believe you said that entire depth zone was ultimately truncated from the dataset. I think your table has about less than a half-a-percent of the natural banks was ultimately sampled, and so, again, I am just trying to get a sense for how the randomization and stratification was done and whether or not this actually does represent a representative sampling design. Thank you.

DR. GALLAWAY: Thank you.

CHAIRMAN NANCE: Luiz.

DR. BARBIERI: Thank you, Mr. Chairman. Benny, thank you for the presentation. I think my question expands here on what I think is one of Josh's points. If you go to I think it's the slide before this one, Slide 15, it's the uncharacterized bottom table, that table that had the uncharacterized bottom, or Slide 17. Sorry.

You have different zones there, right, and then you have different areas, and then the number of sites that were sampled, and how were those samples allocated? I am thinking, Benny, in the sense of like how it goes from the design -- If this was using a design-based approach, to go to your estimation procedure, like -- To some extent, like the different weights, and you're going to have to generate different weighted means that are proportional to the number of sites sampled and the different densities in different areas, right, and different sites.

For example, for the central shallow, that row there, are two sites of uncharacterized bottom that were sampled, and the percent sample was two-tenths-of-one-percent of the area. How was that choice made, for example, relative to the east shallow, which had seven samples collected, and so how was that sample allocation assigned to different sites and then those used in your estimation process, so that you can actually expand those to the entire area? Can you help us understand that part?

DR. RABORN: Benny, can I speak to that for a second?

DR. GALLAWAY: Yes, please.

DR. RABORN: You're right that it was unbalanced, and so that's why we ran it through the statistical model, is because, of course, it sort of forces balance, because, in say one region and depth zone, you may have sampled nine sites that were uncharacterized bottom, and then, in another region and depth zone, only two, and then, if you're trying to estimate it all together, it's partially confounded, if you don't run it through a statistical model and hold everything constant and use the marginal means.

What we wound up with is we're advocating the use of our model estimates, and that represents the marginal means, which would take care of that balancing problem, or weighted average problem, which you were referring to. Does that help?

DR. BARBIERI: It did, and so, basically, you did come up with some weighted means that account --

DR. RABORN: Absolutely.

DR. BARBIERI: -- for this different allocation, but did that also account for the different -- That was to Josh's previous point, but to the different densities, and so, for example, we can go more often, for economic reasons and for sampling efficiency, and we can go more often to sites where we know we're going to have a higher probability of having fish, right, than sites that are bare sand.

Then we have to down-weight, right, the sites with the densities we know already, because that was by design, or our intent to downweight those sites to compensate for the fact that we went there, because we knew that we were going to have higher densities there already, and so, if we don't do this, we extrapolate those higher densities to areas where --

DR. RABORN: To lower densities, yes. That's right, and so, within a stratum -- My understanding is that they were randomly allocated. Now, we call it uncharacterized bottom, but it's not completely devoid of structures. On a couple of sites, we happened to hit a site where it had some sort of relief or structure there, but we didn't know. It's not on the map, and we didn't know that, and that's just where the random allocation put us.

I guess you have to assume that that's representative of all uncharacterized bottom in Louisiana, and that's just an assumption we made, or we have to make, because we can only sample so many sites, and we only can spread so much butter with so much bread, and so that's the best we could do.

DR. BARBIERI: Okay. Got it, Scott. Thank you.

CHAIRMAN NANCE: Will, please.

DR. PATTERSON: I just looked at the RFP, to try to get a sense of how the sites were selected, because I think that's really important here to the ultimate estimate, and understanding the variance, obviously, within these strata, and I just see -- It says, in the RFP, that 106 sites need to be sampled and that they would be provided to whoever wins the competition here.

Harry didn't happen to know how that stratification and sample design was created, and so I think it would be helpful to, at least my understanding, and probably other SSC members' understanding of

this, to get somebody from LDWF who was actually responsible or part of the team that selected the 106 sites to actually comment on how that sample design was put forward, and I think that would be really helpful here.

CHAIRMAN NANCE: I think that would be good. Harry, if you could get maybe someone to be able to speak to that today sometime, so we can get a sense of how those different sites were selected, and that would be, I think, appropriate.

MR. BLANCHET: Okay. I will see who is available.

CHAIRMAN NANCE: Thank you very much, Harry. David.

DR. GRIFFITH: I want to second that point, because commercial fishermen are always very leery of the sampling techniques that fisheries biologists use, and I think that, in this explanation, you really need to address the issue that Luiz brought up about the density of species, or red snapper, in some locations versus others, because, again, this is an issue that commercial fishermen are very sensitive about.

CHAIRMAN NANCE: Yes. Thank you. I do have a question too, and this is my own. When we talk about uncharacterized bottom, it seems like there's a lot of different estimations of the size of that, and so I guess I'm just wondering, and is there not just a general areal map that we use to determine those different bottom types, or does everybody have their own little piece?

DR. RABORN: Well, I will say this. We did have the substrate type available as a variable in the model. I had percent sand, percent gravel, percent mud, and none of it panned out to be -- To explain much of the variance, based on what I see, and so they didn't make it into the final model specification, and so that's why we didn't really include substrate type. You would think that it would, but I suspect that it has to do with the limited number of sites.

CHAIRMAN NANCE: Okay. Thank you, Scott. Any other questions on this part?

MR. RINDONE: Scott, were those the Colorado substrate data, the University of Colorado data?

DR. RABORN: Honestly, I don't know. I am not the GIS person in our group, but they were just -- Our GIS analyst just gave me the data associated with each site. Now, what database that was based on, I couldn't tell you.

DR. GALLAWAY: I forget the name, but it's the same database that everyone is using.

DR. RABORN: If Kyle is on the line, he can answer that.

MR. KYLE MCCAIN: We used the usSEABED database for our bottom classifications, and that is the Buczkowski -- I don't remember the year on it.

DR. GALLAWAY: It's referenced in the presentation and in the report.

CHAIRMAN NANCE: Okay. Thank you very much. Let's go ahead then and -- Paul.

DR. MICKLE: I may ask it later, and it's a little different from site selection design and things we've been discussing for the last few minutes, but just, for the group, and I'm sure I'm not the only one, but these last two columns on area sampled, is that the efficiency of the gears and the methodologies to understand the actual area that was sampled by the action of the project?

My second question is how did you create areas from your methodologies? My second question is, is it consistent with the Great Red Snapper Count on how they calculated their quantitative areas sampled?

DR. GALLAWAY: I think that will become -- Those questions will be addressed in the next presentation, which will be given by Jack Egerton, which will describe the hydroacoustic sampling methods and address that question, and that will be -- It's over twenty slides, and so it will be a lengthy presentation by Jack.

CHAIRMAN NANCE: Okay. I think what we'll do is we're going to take a ten-minute break right now, and then we'll come back to that presentation, and so it looks like maybe five after ten, Central Daylight Time, we'll come back. Thank you very much.

(Whereupon, a brief recess was taken.)

CHAIRMAN NANCE: Okay. We're back. Benny, you're welcome to continue on your presentation.

DR. GALLAWAY: Okay. I think the next slide -- If we go to Slide 34, which is sampling methods. This slide, or this presentation, is going to start with the hydroacoustic analysis, which will be given by Dr. Egerton, and it will include both the sampling and

analysis methods, so you'll have it all together, since he was our expert, and so, Jack, I will turn it over to you to present this section. Thanks.

LGL: FIELD SURVEYS AND SAMPLE PROCESSING PRESENTATION

DR. EGERTON: Okay. Thanks, Benny. As he said, my name is Jack Egerton, and I'll be taking you through the hydroacoustic methods and the data processing. As an overview, I will be running through the field methods, the calibration of the echosounder, how the data is visualized on what we call an echogram, and I will then provide overviews on the data processing and multifrequency analyses, and, under these, I will present the data flow in Echoview and then run through noise removal, decibel differencing, single targets and multiple echoes, target strength, and subsequent fish density.

I will then quickly show you how occasionally we had to remove bait schools from the data, and then I will move on to geographical analyses and the fish abundance calculations with the application of SRV data. Then, at the end, I will run through some comparisons with the Great Red Snapper Count project.

For the field methods, a BioSonics DTX echosounder was used and deployed from local fishing vessels. We have three transducers, and these were pole mounted over the side of the vessels, with the faces of the transducers about one meter below the water surface. The calibration occurred on every trip, and a YSI EXO Sonde was used to collect environmental data, the temperature and salinity being the most parameters relevant to the hydroacoustics.

As I said, we had three different transducers used on this project, a thirty-eight kilohertz, seventy kilohertz, and 120 kilohertz, and this enables us to do what is known as multifrequency analysis, and I will chat more about this a bit later on.

As I said, the echosounder and its transducers were calibrated on every trip, and this was done by lowering tungsten carbide spheres, which have a known target strength, under the transducer face, and the local environmental data is applied, and it's all processed in Echoview, and so any differences between the expected value from the spheres and what we recorded in situ is applied in the later processing. The main image here just shows the returns from the sphere under the transducer, on what's known as an echogram, and, on this, you can see -- Depth is on the Y-axis, but also the color scale, the target strength, and so increasing target strength shows more towards the red.

This is another echogram, and so this just shows the raw data, and so what it looks like as it actually comes in, and we have meters, and so depth on the Y-axis, and, again, also the color scale, which shows the strength of the signal return. There is not too much to see on this one, but we can see the seabed, in the orange line, is about sixty meters, and, also a few potential fish returns above the pinnacle there, and so the subsequent processing is all about just getting these potential fish returns out of the data, or leaving them in the data, and getting everything else out, and so removing all the noise and cleaning it all up.

This is a quick overview to the processing in Echoview. The data is filtered to keep swim-bladdered fish in and everything else out, and so, in order to get fish density, we get the acoustic biomass, which is Sv, and so the signal per volume, which is that's energy reflected by all fish present, and it's divided by the target strength from individual fish in situ.

The data is then exported in twenty-meter-long-by-ten-meter-high analysis cells, and these images show here just show a little example of final fish density values in yellow and the target strength values in blue along the survey track.

With the multifrequency analysis, different frequencies produce different target strength responses, depending on the type of target, and so the multifrequency is all about using these different responses from various creatures at different frequencies to help filter out what we don't want and just to leave the swim-bladdered fish in.

In this graphic, you can see that, although the responses from swim-bladdered fish are fairly consistent across the different frequencies of transducers that we used, responses from plankton -- A good example to look at here would be the euphausiid shrimp. That changes quite markedly, and so we use the differences between the responses with the different transducers to get rid of plankton and non-swim-bladdered fish and sharks and such.

The processing of the hydroacoustics is not a simple process, and there are a multitude of different ways that one could go about trying to get a final number of fish out of the data, and this shows what is known as a data flow in Echoview, and each of these boxes, or operators, have various different functions. It's all quite complex, but I'm going to try and run through some of the main sections of this data flow in the next few slides of this presentation.

This is from the top-left of the data flow, and this shows the raw data that comes in from the different transducers, and so the thirty-eight is on the left-hand side, seventy kilohertz is the bottom-right, and then 120 kilohertz at the top, and so you can see the different strengths and return, depending on the frequency from the raw data.

You can also see, if you look very carefully on these echograms, that it's been split up into analysis cells, which are ten meters high and ten meters deep and twenty meters long, and so these are Sv echograms, and so the returns from all fish present, and that is different from single-target echograms, which are where we get target strength from. Again, you can see, on the right-hand side of the echograms, the strength of return shown by the color scale.

The next step is the noise removal process. We have impulse noise removal, which takes away noise spikes, and then we have transient noise removal, which is quite similar to the impulse noise removal, and this removes values that are significantly higher than the neighbors, and then the mean of those neighbors is used instead, and then background noise removal, where general background noise, such as noises from a ship engine or things like that, and that's all removed from each sample.

Any remaining noise, which might be from things such as bubbles at the surface, under the transducers, or coming up from the seabed, is also removed, and, also, the platform structure, when we come across that, that's also removed from the echograms.

Smoothing then occurs, to remove stochastic variation in the noise signals, following the literature, and also data within three meters of the transducer face and one meter from the seabed is also removed, and so, ultimately, we end with an echogram which looks like the one on the right here, and that's done with all different frequencies of data.

Then this moves us on to the multifrequency analyses, which are also known as decibel differencing, and we used values, following the literature, to filter everything but the swim-bladdered fish, and, in doing this, we got the seventy-kilohertz data, and this was subtracted from the 120-kilohertz data, and this had to satisfy a criteria between minus-fifteen and one decibel to be categorized as swim-bladdered fish, or, with the thirty-eight kilohertz data subtracted from the 120, it had to be less than three decibels.

Data also had to satisfy a summation of less than minus-170 decibels, and so all of the frequencies added together to be categorized as swim-bladdered fish, and this all goes to create a

mask so that only fish remain, and this mask is shown here on the left-hand side, and so anything in the dark gray is not fish, and everything in the light gray is fish, and this is applied over the noise-removed echogram for the Sv data.

The other data was also thresholded at minus-fifty decibels, and it could be seen that some particulate matter was still making it through, and so it's all about filtering stuff that isn't our target species, or our target type of species, swim-bladdered fish.

That mask that we produced is also applied to single targets, so that single targets are responses from fish that can be definitely categorized as being individual fish, and, here, we also used a minus-fifty decibel threshold for target strength, and that mask was applied over this, so that we're only using target strengths from what we defined as swim-bladdered fish with the Sv echograms. In this echogram here, you can just see the sort of small little flecks. If you look very closely, you see little flecks of color, and these are data results from single fish.

It's from these single targets that we derive target strength. However, these have to be further filtered, to remove what is known as multiple echoes, and these can occur when two fish are exactly the same distance from a transducer and can overinflate the target strength that we get.

These have to, therefore, be accounted for. If the target strength is overestimated, then final density values will be underestimated. To do this, it's all quite a complicated process, but we followed the literature and used a criteria of the Nv index has to be less than 0.1, and the M percent of multiple echoes had to be less than 70 percent, and this is all follows the standards, follows the literature, on this.

The top echogram here shows the results in mean target strength per cell, and the bottom echogram shows what remains following this process to get rid of the multiple echoes.

In order to fill any gaps of target strength, i.e., in areas where have Sv, but we have no valid single targets, the data has to be smoothed, and so, ideally, we would target strength to be taken from the same cell, but, if this wasn't possible, if we had no valid single targets there, then target strength had to be taken from the adjacent cells, and, if it's still not available, then the mean target strength from the layer is used. Even if there wasn't anything in the layer, then, ultimately, we had to use a site mean target strength. Finally, we end up with an echogram that looks like this, with no gaps in it for target strength.

After all that, we can finally get towards fish density, through what is known as echo integration, and so, here, as I said before, we take the Sv data, which is shown in the top-left here, and that's divided on a cell-by-cell basis by target strength on the bottom there, to result in fish density, and so that's number of fish per cubic meter, and that is shown in the echogram on the right-hand side there.

This is a little bit of an aside, and I wasn't quite sure of the best place to put this, but it's just to say that, on occasion, bait schools are recorded, and they are removed, because they can really skew abundance values high, and we defined these as having a dense monotone appearance and when the individual fish within them could not be discerned, and so when we couldn't get any valid single targets to divide the amount of energy being reflected by the whole school. The top shows one that was removed, and, below that, it shows you one that remained in, and this remained in because we could pluck out individual fish within it, or moving in and out of that school.

We have density values along the track of each of the habitat types, sites per habitat type, and then the mean values of these is multiplied by the grid cell size to get the abundance. Really, the main thing on this slide is the grid cell size at each of the habitat type sites, and so, for example, a platform is fifty-meters-by-fifty-meters, and so the sampling area is essentially divided by five, and so you have five smaller cells within that, five cells going in X direction and five cells in the Y, and it's from that grid cell size that we multiply out the fish density values, which I will talk a little bit more about in the next slide.

Also, on this, one thing to note is the Aglen ratio, and that has been derived from empirical studies which show that it has to be higher than six to be thought of as to have adequate coverage within an area. Essentially, it's calculated by the track length divided by the square roots of the survey area, but I think suffice it to say that we have that adequately covered in each of the zones, in each of the sites. Sorry. Also, on that, if it was completely covered during like the parallel transects at platform sites, we also did spiral transects, in order to maneuver around any standing structures, when that was necessary.

This kind of summarizes it all up, but, as I said before, we get the final density estimates through the division of Sv by in situ target strength, and these are then exported to a spreadsheet, and the values were converted to a number per meter squared by

multiplying by the depth of the layer, which is normally ten meters, and it only wasn't ten meters very close to the seabed.

Also, at this point, where there was no data, and so where areas had to be blanked out, such as on the rig structure, these values were replaced by the mean of the horizontally-adjacent cells. The data is then exported to GIS, and we use Quantum GIS here, and a spatial join is performed, so that all points within each of the grid cells were exported back into a spreadsheet, and abundance calculations were then performed, through areal density, multiplied by that grid cell area.

A mean abundance is therefore gained per grid cell for every depth layer, and so every ten-meter depth layer, and then this is later proportioned out by the species composition, which is gained from the SRV cameras, which is think Benny will talk about in a little bit.

I am going to run through some comparisons with our methods versus that of the Great Red Snapper Count project. Overall, the methods were actually pretty similar, and fish density was calculated and then converted to abundance through multiplication of the volumes to water investigated.

These were then both proportioned out by -- The total abundance was then proportioned out through application of the SRV data and use of the MaxN metric. Primarily, both studies used echo integration, and so the Sv divided by the TS method, although they did some echo counting at times, when the fish were adequately dispersed enough to allow this.

In terms of differences, they used different methods in each of their study regions, and so our methods were very similar to what they did in Florida, in that they also did multifrequency analyses here with different frequencies of echosounders. They did a summation of these frequencies, to proportion swim-bladdered fish, which we also did, but we also did the decibel differencing technique, as described earlier.

With their Texas data, which was extrapolated into Louisiana, and they only had one frequency of echosounder, and so they weren't able to do this decibel differencing multifrequency analysis. The target strength thresholds were the same between us and them in Florida. In the Texas region, the thresholds they used were a bit less conservative, and the Sv threshold that they used in Florida was also slightly less conservative. Finally, they used a kriging style of interpolation to analyze the final densities, as opposed to our sort of grid analysis.

That's what I've got, and so I'm going to pass it back to Benny for the other methods. I am happy to take any questions now or at the end of this session, and I don't mind either. Thank you.

DR. GALLAWAY: I think questions now would be appropriate.

CHAIRMAN NANCE: Okay. Let's go ahead and have questions. Trevor, please.

MR. MONCRIEF: I just had a quick question. I was trying to follow through. I read through the report earlier, and I'm trying to link back to it, but, essentially, my question is the picture you showed early on, on the acoustic side, it was UCB Site A13 showed a small structure, or something else like that, with a few fish on it, and I looked forward in the presentation, and I saw a couple of sites that were uncharacterized that had fish on it, and my question is, when you all did -- When you all conducted this over the uncharacterized bottom, were there any sites that you sampled that you had to basically throw out or move, because you ran over a structure that wasn't previously identified?

DR. EGERTON: From my point of view, no. I kind of just analyzed the data as-is, as what I saw. I'm not sure if that was later taken into consideration in the modeling or not, and that might be a question for Scott, but I was just sort of analyzing these on a what is there sort of basis, which is almost -- It's not blind, but it's almost blind in comparison to what should have been there.

MR. MONCRIEF: Okay, and so I guess the question might be for Benny more so than, and was there any like prescreening that happened at these sites?

DR. GALLAWAY: We did the transects, and, where we encountered a structure, we went back and applied our vertical sampling techniques to that structure site. We went back and did vertical lowering of the SRV to get a count of fish associated with that structure and composition of the species, using vertical drop and using it as if were a discrete method, or a discrete site.

MR. MONCRIEF: Okay.

CHAIRMAN NANCE: Any other questions? Okay. Let's go ahead and move on then.

DR. GALLAWAY: On the next slide, we go on to the SRV, the submersible rotating video. For discrete sites, we dropped the camera near the structure. At each depth, we did five minutes,

ten rotations of 360 degrees, and we recorded at every ten-meter layer of water column at predetermined depths to match the hydroacoustic data. For the target drops, we opportunistically dropped the camera to capture fish assemblages and points of interest on discrete and UCB sites.

In the SRV analysis, we identified all fish to the lowest possible taxon, and we recorded relative abundance of each species using MaxN, the maximum number of a species in any single frame, and we recorded these data for every ten-meter depth layer.

Relative species abundances were converted to composition percentages to apportion hydroacoustic abundances, and the proportions were then applied to the fish abundances at each ten-meter depth layer.

For vertical hook-and-line sampling, for length, weight, sex, and age composition, we followed these methods. We conducted vertical hook-and-line sampling at each discrete habitat included in the study, and these included platforms, artificial reefs, pipeline crossings, and natural banks, as well as these obstructions that were observed over uncharacterized bottom.

Our sampling gear consisted of two hook sizes, six and eleven, and two bait types, which were squid and menhaden, allowing for four bait/hook combinations. Only one bait/hook combination was fished on an individual pole, and each bait/hook combination was fished an equal amount of time in each site. These fish were caught and brought onboard, and they were placed in corresponding shrimp baskets, indicating which combination of bait/hook type had been used.

I would like to point out that the large fish were not rare, especially at pipeline crossings and artificial reef sites, and this particularly relates to not only red snapper, but also to amberjack.

The samples that were retained, the fish were returned to the dock for processing on the same day that they were collected, and, typically, this occurred late in the day or night, and we had dinner available when the crew got in, and everyone ate and processed the fish.

Typically, the boat captains would get some rest, and the biologists would stay and weigh and measure and sex the fish obtained, and, for red snapper, otoliths were extracted, cleaned, and stored in a labeled envelope. Then the rest for the crew was the rest of the night, and then, of course, they were able to rest

in transit out to the next day's site. Once the fish were processed, they were stored on ice and transported to charity organizations.

We conducted mark-recapture methods at sites, at six sites, three platforms and three artificial reefs, and one set of experiments was performed in each of the three regions in the mid-depth zone. For this study, we used doubly-tagged -- Fish were doubly-tagged dorsally, and fish were cage released, following the methods developed by Dr. Szedlmayer.

For the towed underwater video methods, as depicted in this figure, Number 1 shows the vertical deployment, and the towing was conducted at three to five knots, with no additional scope and the provided camera position arc, and filming occurred at zero to ten meters above the bottom, and then 4 is the vertical retrieval at the end of the tow.

Our underwater video sled detail, and this is something we put together, and we used a video camera, and it was a GoPro Hero 7 black, and the housing was rated to 200 meters. It had 1440 resolution and sixty FPS. It was four-by-three wide and no zoom. It had HyperSmooth video stabilization, and the field of view was 94.4 vertical and 122.6, and the camera system is shown, a side and front view, on the left, and this camera system was quite rugged and worked quite well.

A few notes, and the sled was towed without adding additional scope, to avoid bottom snags while capturing the focal depth area between zero and ten meters above the bottom, depending on water depth and water clarity, and so this provides detail to the comments that I've already made.

For the bottom longline method at UCB sites, we deployed one mile of longline, with weights and floats at each end, and we had a 1,400-pound test monofilament mainline, with three-foot gangions of eighty-pound test, and we had seventy-four hooks baited with squid, and alternating 6/0 and 11/0 circle hooks were used on the structure, on the longline.

This shows the set. Number 1 is the buoy. If you go down to 2, that's the mainline and the 1,400-pound test. The weights were twenty to forty pounds, and we had snag clips, and the gangions were three-feet and eighty-pound test, and the circle hooks were 11/0 and 6/0, and this is just a diagrammatic presentation of what I just presented.

I would like to characterize our comment on there were lots of

large red snapper over uncharacterized bottom. We sampled, or obtained, 183 red snapper used for composition sampling, and the ages ranged from two to twenty-five years. The average age was 8.6 years, and the mean weight of these fish was 9.4. The maximum weight was up to 18.4 pounds, and the mean length was nearly twenty-six inches, or 64.9 millimeters.

You will notice that we use inches and pounds a lot in this presentation. Mainly it's, when we get to the bottom line, to compare landings in pounds and metric tons, et cetera.

The next slide shows the same process was used on the dockside catch sampling, and we returned and sampled on the same day, and the measurements were taken in the same way as described before for discrete sites. Again, the red snapper that were caught were carefully iced from capture through processing, after which they were donated to charities.

Laboratory analysis, most of the laboratory time was devoted to analyzing videotapes of the fish within habitat and analyzing otoliths to determine red snapper age, and so this provides pictures of that activity.

Here, I would like to turn the mic over to Scott Raborn, who will elaborate on the statistical modeling that was used to estimate red snapper abundance. I will turn it over to Scott and let him explain. Scott.

CHAIRMAN NANCE: Just on that, are there any questions on what Benny just presented? John.

MR. MARESKA: Benny, I've got a couple of questions. The mark-recapture site selection, how was that determined, which sites were going to be used for that? Then another question, and is the bottom longline -- Were there any differences in the CPUEs between the different hook sizes? I looked through the report, and I don't see any length frequencies related to the different hook sizes for the gear.

DR. GALLAWAY: For the latter question, I will defer that to Scott, and then I will come back to your first question after he answers that.

DR. RABORN: At this point, we haven't looked at hook selectivity, but it's something that we have on the horizon and that we're going to do before we publish, but, yes, we used two different hook sizes, 6/0 and 11/0.

We used two different bait types for most sites, squid and menhaden, but, for the uncharacterized bottom, I think we only used squid, and what we found, or what I just saw, just looking at the cursory plots of the data, is the smaller hook caught as many large fish as the larger hook, but the larger hook did not catch as many smaller fish as the small hook. We caught more fish on the 6/0 hook, and that was true at other sites as well, but, at other sites, like I said, we used two different bait types, and just, upon initial inspection, it looked to me like the bait type had as much to do with selectivity as the hook size.

CHAIRMAN NANCE: Thank you. John, any more? Benny.

DR. GALLAWAY: For the first question, yes. We selected sites for which we had SRV and hydroacoustic data, and those were basically selected one of each type within the depth zone, the two types. It was not randomly selected, and they were handpicked to correspond to where we felt like we had good estimates.

CHAIRMAN NANCE: John, a follow-up, please?

MR. MARESKA: Thanks, Benny, and then one additional question. Your SRVs, in reading the report, it looks like they were dropped in proximity to noted fish densities, and so around your platforms and rigs, was this just on the fish-heavy side, or was it randomly determined before you got to the site, and did you do multiple sites at these larger platforms?

DR. GALLAWAY: We were doing our transects around the platform, and, out away from the platform, we observed large schools of fish, and we went back to take a look at identifying what those fish were, attempting to relocate that school and drop the camera among them, to determine exactly the species composition of these schools out around the periphery of the platform. In other words, you would have large schools of fish circling the platform that you would encounter, and we attempted to determine what those schools were.

MR. MARESKA: Okay, and so you did that at several locations around these larger rigs and not just at one site?

DR. GALLAWAY: That's correct. Anywhere we encountered those large schools, we went back and tried to look at the composition of those schools that were encountered.

MR. MARESKA: Okay. Thank you.

CHAIRMAN NANCE: Any other questions? If not, Scott, we'll turn

the time over to you. Will.

DR. PATTERSON: I'm sorry, Jim. Thank you. I just have one quick question here about the SRV data and its use in estimating the species composition to convert the targets perceived with the hydroacoustics into red snapper estimates.

John asked part of what I was curious about, as far as whether there was a random sample around the platform, because red snapper don't always occur in this large aggregations of other species, or even within its own species, but the other question I had is visibility in the western Gulf can be problematic, and so I'm curious. Like what percentage of the sites were you unable to extract optical data using the SRVs, and how did you deal with water clarity issues with that gear?

DR. GALLAWAY: I will first defer that to Scott, and, in the meantime, Jessica, if you could unmute Kyle and Taylor, who did the analysis, and they might want to address that, but Scott first, and then Kyle, and then Taylor, if you all wish to comment.

DR. RABORN: Thanks, Benny. First of all, at every discrete site, there was a standardized SRV drop. Then, in addition to that, if they happened to come across a large school of fish, then they did directed drops, and then those data were all -- The directed and the standardized drops were all pooled together to determine MaxN for each species at each site. I think that was one of your first questions. Then -- Forgive me. I lost my train of thought, and what was your other question?

DR. PATTERSON: I will re-ask the other question, but, while we're still thinking about this one, you did a standardized drop, and then you did sort of targeted sampling if you saw an aggregation of fish that you're interested in, and then you said you pooled the MaxN, the species-specific MaxN, across -- Between those two types, to look at the proportion red snapper within the community, but did you ever examine, before you pooled the data, whether there were differences in the community composition estimates from those two types of sampling, your standardized sampling versus your targeted sampling?

DR. RABORN: No, I didn't.

DR. PATTERSON: Okay, and so the second part of this was looking at visibility, and so, in the western Gulf, water clarity can be problematic for optical methods and gears, and so I am just curious how -- Among these various samples, were there times that the SRV data -- That you had detectability issues, and there's also

potential reactive behavior differences among species that could exist, and so I am just curious how you attempted to account for any of those confounding effects.

DR. GALLAWAY: I would like to also unmute Will Heyman to address that on the towed video, and so I think I will start with Kyle and then Taylor and then Will.

MR. MCCAIN: On the SRV stuff, for the most part, we were able to detect fish. I really don't recall many instances where there was zero visibility across any of the regions. Taylor, do you have anything?

MS. TAYLOR BEYEA: Certainly there were -- It certainly varied, as far as visibility went. You definitely have that nepheloid layer at the bottom in most places, but we could still detect fish, most of the time, at every layer. I'm not sure that I can give you a percentage offhand, and we could look into that, but, primarily, it was not prohibitive, in my opinion, for most of our sites. There are, undoubtedly, some behavioral biases as well for certain fish species, but I would think that's not substantial, given the visibility that we did see.

DR. PATTERSON: Jim, can I follow-up, just real quick?

CHAIRMAN NANCE: Yes, please, Will.

DR. PATTERSON: I am curious, and you've both indicated that there were no sites with zero visibility, and that you could always see some fish, but it might be worth exploring in the data, if you haven't to this point, whether the estimated visibility, which is going to be sort of a qualitative -- If you don't have turbidity estimates or transmissivity estimates from something like a CTD, to actually examine how the perception of visibility, even qualitative, affects the proportion of red snapper that you're estimating from your SRV data.

Different species react differently to the gear, and some have negative, and some have positive reactions to even small, towed cameras, and so I think it would be worth investigating whether that has an effect on your estimates of proportion red snapper, which then would scale in your acoustic data.

CHAIRMAN NANCE: Benny, you said you wanted Will Heyman to speak?

DR. RABORN: I can speak to that, but I don't know if Will has anything he wants to add.

DR. GALLAWAY: Will, do you want to add anything on the vertical tow visibility effects?

DR. WILL HEYMAN: We did have some areas with very low visibility, but I think Scott is going to address this, because that was taken into effect with the statistical modeling.

DR. RABORN: All right, and so, Will Patterson, your point is well made. Obviously, you have done some SRV work, and what do you do when you can't see very well, and you only see ten fish, but your hydroacoustic estimate says there's thousands down there? We first ran into this problem when we did our BOEM study estimating the number of federally-managed species on platforms.

When we tried to get site-specific estimates, you simply take your hydroacoustic density or abundance and then you apportion it among the species based on the SRV data, and sometimes we would only see three or four fish, and they would all be cobia, and you would have an estimate of a thousand cobia on a platform, and you know that's not right, and so what we found was that, instead of getting species-apportioned estimates per site, you treat -- I will go into this in just a second, but, in the modeling exercise, we treated the hydroacoustics data and SRV data and modeled them separately, and we came up with estimates for each stratum.

Then, once we had that, we combined them, and so, if you underestimated the proportion of red snapper on one site, and overestimated it on another site, the idea is that the areas cancel, and, when we did that, we got reasonable estimates that seem to match the mark-recapture estimates, which we did as a verification exercise, but I can go through the modeling stuff, and then, if you still have questions, we can address it then, unless you want to --

DR. PATTERSON: That sounds good. I think there's probably some data exploration that could be done just with the empirical data, to maybe get a sense of how much of an effect that you might be able to glean just from those data, and, yes, modeling can handle this, to an extent, unless the bias is all in one direction, and then you have no way to account for that. I mean, if you have a random distribution of measurement error, then perhaps you can handle it there, but, if it's all in one direction, then it becomes more problematic.

DR. RABORN: Yes, that's right, and that's what we were worried about, which is why we employed the -- We couldn't do mark-recapture at every site, but we could use it to spot-check here and there, and it was close enough.

DR. GALLAWAY: Ryan, I would like to point out that, over the break, I sent you the paper that's in press that provides more detail on these subjects of bias and using the camera SRV data, and can you distribute that to the SSC?

CHAIRMAN NANCE: Yes, he will do that. Thank you. Will, any more questions?

DR. PATTERSON: No. Thank you.

CHAIRMAN NANCE: You're welcome. Sean, please.

DR. POWERS: Benny, was there any hypoxia while you all were doing this survey, and would that complicate any of the observations that you did, either with the acoustics or with the underwater cameras?

DR. GALLAWAY: Yes, I believe there was hypoxia, and I think Scott will address that.

DR. RABORN: Probably not. I don't remember which sites -- We used dissolved oxygen as a covariate in the models, but I can't recall which sites exactly. These are all good suggestions and very pertinent summaries that we need to include, and so, yes, we can include -- We need a table summarizing the environmentals by site, and including water clarity and how that may have affected the SRV data and the distribution of fish in general.

DR. POWERS: I was just wondering, because if -- So you did the field work in 2020, and I think Rabalais found a pretty large dead zone out there in 2020, in the summer, and so I was just wondering how much of that area was represented in your sampling area, because I think she does shallow to mid-shelf in her surveys.

DR. RABORN: I don't know exactly, and it would be nice if we could get that coverage and overlay it onto ours and look at it, we do have the environmentals, the site-specific environmentals, of course.

CHAIRMAN NANCE: Kyle, Taylor, and Will, would you all like to comment on that as well, starting with Kyle and then Taylor and then Will?

MR. MCCAIN: Can you repeat the question?

DR. RABORN: Hypoxia, and how often did you have low dissolved oxygen levels? I mean, we have the data. Give me thirty minutes,

and I can create a table, but, in real time, I can't do that.

MR. MCCAIN: I don't have an answer off-hand.

MS. BEYEA: I'm not sure on specific sites off-hand either, and we could do some data exploration there and look at some overlap, and that sounds interesting.

DR. HEYMAN: I am looking at the data right here, and I am not seeing anything below four in the sites that I sampled, but, again, we can get back to you on that.

CHAIRMAN NANCE: Thank you, Sean. Will.

DR. PATTERSON: I think Sean raises an interesting question, or point, here, and I thought about this when looking at the toppled platform versus standing platform estimates.

I think Reviewer 1 pointed out that maybe it's because the toppled platforms have lost a bit of their community, and it would take time for that reestablish, but it could also be that the really low dissolved oxygen, the hypoxic or anoxic portion of the water column, is closer to the seabed.

I am wondering how hypoxia, anoxia, potentially could be affecting their perceptions of communities and how ephemeral they may be seasonally, given the dead zone sets up and then dissipates and sets up and dissipates.

DR. GALLAWAY: The distribution on the toppled platforms needs to be taken into account relative to the area of hypoxia, and I think most will probably fall outside, but I'm not sure.

DR. RABORN: I can take a look at that further, too. Like I said, I did use dissolved oxygen as a covariate. It didn't make it into the proportion of red snapper model, but it did make it into the hydroacoustic side of things, but whether that's fully accounted for --

I mean, maybe you need to include it like that, but you maybe also need to include a binary variable, categorical variable, that says it was hypoxic or not at a certain depth. I didn't think about exploring that, but it's a good suggestion.

CHAIRMAN NANCE: Okay. Any other questions? I think it would be good to move into -- Maybe some of these will be answered in this next presentation, but, Scott, let's go ahead and do the statistical modeling and estimation.

LGL: STATISTICAL ANALYSES AND MODELING PRESENTATION

DR. RABORN: All right. By now, I'm sure you have gleaned that the response of interest is red snapper abundance in the State of Louisiana and the waters offshore of Louisiana, and, to get that, it's basically the product of three responses. There's the total fish density, as measured with the hydroacoustics, the proportion of that density for red snapper, and then, once you have that, given your stratum that you were estimating it for, you need to extrapolate it to the entire stratum.

For natural banks and uncharacterized bottom, we did that based on area, and, for the discrete sites, like the artificial reefs and platforms and natural banks, we did that based on -- I'm sorry. Not natural banks. We did that based on the number of structures in that stratum.

First, how do we define each stratum? We had three categorical variables. We had longitudinal region, east, central, and west, and Benny showed you a map of where those were delineated earlier, and then we had the depth zones, ten to twenty-five, twenty-five to forty-five, and forty-five to, ultimately, 150 meters. As he told you, through this modeling exercise, we combined the deepest two depth zones.

Habitat type, you had natural and manmade, and the natural habitats were the natural banks and uncharacterized bottom, and the manmade were artificial reefs, which were purposely put there as reefs to attract or promote fish production, but then you had standing platforms and then pipeline crossings. Other people refer to artificial reefs, artificial structures, as all of these combined, but just to make clear that we're delineating between actual dedicated artificial reefs that were planned versus standing platforms and the pipeline crossings.

Then we had this fourth variable that Jack was talking about, and the hydroacoustics gave us an estimate of fish density for every ten-meter depth band, and that's categorical, ordinal, in nature, but, ultimately, a model that is a continuous variable, and we used the midpoint of each band to the bottom as a metric of distance off the bottom, and that is a continuous variable, or a covariate.

There were a couple of approaches to estimating red snapper abundance, as I briefly mentioned. You could get a site-specific estimate, where you just multiplied total fish density times the proportion that were red snapper and then extrapolate that to the

entire stratum, and we call that mean site abundance approach, but that's not what we're advocating be used as our abundance estimate.

The second approach is what we're advocating, and that's what we're calling the modeling approach, where you model the total fish density and the proportion red snapper separately for each stratum before you combine them and then extrapolate them, and the reason for that is, as I mentioned just a second ago, you can get this sampling measurement area magnification, and so let's say, as Will Patterson pointed out, you had poor visibility, and you went over it with hydroacoustics, and you had 2,000 fish, and half were red snapper and half were say Atlantic bumper, but you don't know this, and you drop the SRV camera, and you count ten fish, nine of which were Atlantic bumper.

If you did it this way, you would get an erroneous estimate of 200 red snapper, instead of what you should have gotten, which would be 1,000 red snapper, and so that would be what would happen on a site-specific basis.

Any errors in the SRV get magnified when you multiply it times the total fish density, and then, when that gets weighed in with all the site-specific estimates, it can give you an incorrect idea of what's going on in that particular stratum, and so, for that reason, we're arguing that the better approach is to use all these categorical variables and then the appropriate environmental covariates to just model these things separately and then take the model output and multiply them together.

The idea, again, is that, if you do it this way, any errors in the red snapper proportion, up or down, would cancel each other before they get magnified by their total fish density. I mostly argue for this approach because, if we don't use this approach, then they really don't need me. That was my attempt at humor. Sorry.

First of all, we'll look at the final model specification for the proportion of fish that were red snapper, based on the SRV data, and the most parsimonious model, based on AIC, was the longitudinal region, temperature, the habitat type, and distance from the bottom.

The small "z" in front of temperature means that, for all the covariates, we just standardize them to their z scores before entering the model, and the term "habitat type" and distance from bottom just means that that's an interaction between habitat type and distance from bottom and all the subsequent main effects, and then "s" in parentheses means that I used a smoothing spline to relax the linear assumption for the vertical distribution.

Basically, this constitutes a generalized additive model for the binomial response of whether or not it was a red snapper or not, and we did this within the MGCV package for R.

The final model specification for the hydroacoustic part of total fish density, this time, dissolved oxygen was shown to be important, based on the AIC, and it included temperature as well, and several other interactions. Again, habitat type was interacted with distance from the bottom, but then also region and the depth zone.

We used the same fixed effects variables considered for the proportion for the red snapper model, and it would have been convenient and tidy if you had exactly the same model specifications for both responses, but that didn't occur, and so we just had to live with it. You would think that salinity would be important, and it no doubt is, but it was also very correlated with temperature and dissolved oxygen, and so we just removed that, to prevent collinearity.

Then there was one more complication. When you're looking at the distance from the bottom, and those depth bands, sometimes the very deepest depth band didn't fully make up ten meters. Sometimes it was only four meters, or two meters, and so it wasn't sampling the same volume of water as the layers, or bands, above it or at other sites. To account for that, we used the volume of water at each site-specific depth band combination as an offset in the model.

We allowed the intercept for the hydroacoustic model to vary randomly across sites, and so this created a generalized additive mixed model, and so we get -- Which gave us the fish per cubic meter, and the distribution I chose to represent total fish density was a tweedie distribution, and it has a parameter to adjust the type of distribution that it represents. One would represent a Poisson, and two would be completely gamma, but they both handle zero values, which occurred in our dataset, and so several other publications have shown that the tweedie distribution is often the most accurate, or most robust, used for fisheries-type data. Sometimes a negative binomial is as good, but it seldom is better, and so, since it was more flexible, this is what we went with.

When we combine the model outputs, for each of these strata, and that was the region, depth zone, habitat type, vertical depth band combination, we predicted the proportion red snapper and total fish density, and so we held all the covariates, the temperature, or dissolved oxygen, as it were, at what we observed, the average, for each of those strata. Then the red snapper density was just

estimated as a product of those two model outputs.

To get an overall variance around just red snapper abundance and the confidence intervals, we had to get the arithmetic variance for the total fish density, which we did with the method of moments, on mu and sigma, and it was lognormal function, and so that's how we got the arithmetic variance, and then we added that to the variance from the proportion red snapper, based on Goodman's variance of products estimator, and, fortunately, the total fish density and the proportion red snapper were not correlated, and so we didn't have to worry about incorporating the covariance aspect of it.

We converted the red snapper per cubic meter now to red snapper per square meter, based on the average width of the vertical depth band for each of the strata, and then we propagated the variances accordingly. The total abundance for natural banks and uncharacterized bottom were extrapolated using total areas for these habitats, and, for platforms, artificial reefs, and pipeline crossings, these were discrete sites, and it was just the number of structures within each of these strata that we used for the extrapolations.

Just to point out that, for these discrete sites, we assumed that our sampling area subsumed all the red snapper present at that particular site, and I think Jack pointed this out earlier, but I will just reiterate that artificial reefs, the areas were sampled, were 62,500 meters squared, and the same for pipeline crossing, and then 42,000 meters squared for platforms, which, if you were doing a spiral sampling design, that would be like sampling out to a radius of 115 meters from the center.

To go over some model diagnostics, this is from the proportion red snapper model, and this is deviance residuals versus the linear predictor, and, of course, from the logistic regression, the residual plots always look sort of funky, but nothing too egregious here to cause alarm. It seems that the model did overpredict slightly the proportion of red snapper, based on the distribution of the residuals. Then I took the final model output and plotted it against what we observed, and it's not exactly comparing apples-to-apples, but it gives you an idea. If it was way off, or way different, then it would cause you to be suspicious, but it seems to fit the data reasonably well.

This is for the hydroacoustic side, the total fish density, and these are the deviance residuals versus the linear predictor, and you can tell that they're more or less equally distributed around zero, except for the zero values along the bottom, which is very

typical of these type data, and, again, we slightly overpredicted -- The model slightly overpredicted this response, but nothing of major concern.

For the total fish density, we had -- The depth zone actually made the cut in the final model, and so I have the predictions separated by shallow, mid, and deep, and so this is the predicted values, the black line, versus the observed values, the circles, and, again, it seems to fit the data reasonably well, and the same for the mid depth zone. We didn't sample any natural banks in the mid depth zone, and so that part of the graph is missing, but it gives you an idea of just how well it fit. Then, for the deep, the same thing.

Then, finally, the mark-recapture methods, we used an approach developed by Bill Gazey in the mid-1980s, and he used non-informative priors when estimating -- When getting his mark-recapture estimate, and this tends to give you better confidence intervals, and it helps when your number of recaptures are very low. It tends to be less biased, which is the preferred method. The details of that can be found in that publication, and I won't speak to it any further, and I guess that gets us to the results, unless we have any questions.

CHAIRMAN NANCE: Let's go ahead and take any questions on that methodology. I have one. From the averaging, using that model to produce an average number of red snapper, I am not sure it takes care of a directional bias. If red snapper are always less than what you expect, you would have -- I know what you're saying, is that sometimes they're less, and sometimes they're more, and so you hope it averages out, but I don't see how that, in the model -- If it's always less than, if it's always a directional bias, how would the model take care of that?

DR. RABORN: It wouldn't. I mean, if red snapper are just -- If you have a true proportion of red snapper at a given site, a true assemblage structure, and if red snapper are always underrepresented, based on SRV data, and, of course, there is variation around that, ups and downs around that, and that's what the model takes care of, this approach takes care of.

It's that error, that random error, that gets multiplied times the total fish density. It gets magnified, and you wind up with some crazy answers for a given site, but, if it's consistently under or overrepresenting, there is no way to handle that, unless you know it, and you just put in an offset there, but that's why we did the mark-recapture sampling, to see if that was the case.

CHAIRMAN NANCE: Okay. Sean.

DR. POWERS: Could you go Slide 83? This is the final model that you used, and I didn't think that depth zone was in there, but it is. Okay. Then can you go to some of your residual plots? Any one would do. If I added up all those circles, it would equal 106? It looks like more than 106 circles.

DR. RABORN: Right, but, at each site, there were multiple layers, and each site had a different number of vertical depth bands, and so it's the number of vertical depth bands times the number of sites.

DR. POWERS: Gotcha, and so that is where your effective sample size comes from and not the 106 sites? Your sample size for your routine extrapolations are 106 times however many depth bins you have?

DR. RABORN: That's right.

DR. POWERS: Do you think those depth bins are independent at the 106 stations?

DR. RABORN: No, and I put a term in there to model the pattern across those depth bands as a continuous variable, as I said, and then, for the -- Not for the proportion of red snapper, but for the total fish density, I let the intercept vary randomly among sites.

DR. POWERS: If it's a continuous response, then it shouldn't really inflate the sample size, and so your sample size should still be about 106, and so I guess that's where I'm getting confused. If depth is categorical, I understand how that would inflate the number of sample -- The sample size, but I am still trying to graph what is 106 plus the continuous variable.

DR. RABORN: I see why you're confused. For each prediction, I used the average temperature and dissolved oxygen, but I didn't use the average depth. I estimated -- We got a prediction for each -- I know I modeled it as a continuous variable, but we got a prediction for each vertical depth band, because we felt that would be more accurate, as the proportion of red snapper could change along the vertical column, as well as total fish density, and so each output was each site depth band combination, and that was each observation.

DR. POWERS: My concern is that, when you do your variance estimate, that that's really inflating your -- It seems to be

artificially inflating your sample size from the 106, which I would argue is the true sample size.

DR. RABORN: Yes. Well, you've got me stumped here for a second, Sean.

DR. POWERS: Okay. You can think about it. We've got all day.

DR. RABORN: I see what you're saying, but, if I'm incorporating the change in the response along this vertical water column, and I have a sample every ten meters, and then I am modeling the change in that across that vertical column, we now let the intercept vary randomly across sites, and it seems like it would handle some of that autocorrelation.

DR. POWERS: The ten-meter bins are not independent at the site.

DR. RABORN: No, and there is a pattern to them, right, and so, at a given site, there may be a pattern in the distribution along the vertical column, and so I put a term in there, distance from the bottom, to capture that pattern. Now, what you're suggesting is that maybe we put a random term in the model to handle the correlation between adjacent depth bins?

DR. POWERS: No, and I wouldn't be so bold as to advocate a solution, because I am not a statistician, but it just seems, to me, that, in the end, when you start computing the variances around your estimate, that this will -- The sample size is larger than it actually is, and that would affect your variance estimates, in the end, but we haven't seen those yet, and so, I mean, we can revisit it when you all show the final numbers and variance estimates.

DR. RABORN: You very well may have a point, but I just can't speak to it right now.

DR. POWERS: Okay.

CHAIRMAN NANCE: Okay. Any other questions? One thing, Benny and Scott, as we go through this, I would like to keep this study, showing what the results of this study are, and not really have -- - Because the Great Red Snapper Count is a whole different study, and so comparisons and things probably are maybe not that appropriate, and so let's just keep on with what we're showing here for this study and the results.

DR. GALLAWAY: Okay, and so we will move directly into the results at this time, and are you ready to do that, Jim?

CHAIRMAN NANCE: Yes, absolutely. Thank you.

LGL: RESULTS PRESENTATION

DR. GALLAWAY: Okay. Our results is that, overall, we encountered eighty-eight unique species across all sampling approaches and habitat. At discrete sites, we viewed sixty species on the SRV and collected twenty-nine of these species using vertical hook-and-line sampling. At UCB sites, only six species were documented using the towed video camera, but another nineteen species were documented in the SRV drops taken over bottom features occurring in the UCB. I hope that's clear. If you've got a feature, you've got more species diversity and abundance over that feature. A total of twenty-one species were documented using bottom longlines over UCB.

The discrete site hydroacoustic SRV sampling summary is provided in this slide, and we had fifty-five sites here, and we had 192.5 kilometers of hydroacoustic transects and seventy-two SRV surveys. We counted 39,000-plus fish, and, of these, 2,813 were red snapper, and that's for our discrete site summary.

We have a diversity of species from the vertical sampling, everything from greater amberjack to black jacks, to scamp, porgies, gray triggerfish, blackfin snapper, and there was quite a diversity of species taken on the vertical lines.

For the UCB towed video and longline sampling survey, we sampled fifty-one sites. Now, of these, thirty-nine were unique sites, and another twelve sites were paired with those, basically on and off pipelines within the same sediment type. Here, we had 702 kilometers of hydroacoustic transects and forty-nine kilometers of towed video transects. The towed video surveys, we counted 3,200-plus fish over small features, of which 1,400-plus were red snapper. In contrast, towed video surveys on UCB without features yield observations on just 101 fish, of which only eleven were red snapper, and to the right there shows a small feature and a cloud of fish over it, and red snapper are at those sites.

Again, we've got a variety of species taken from longlines, everything from eels to pufferfish to cobia, bluenose rays, snapper, rock sea bass, and the next slide shows that, also, we have lots of sharks on the bottom longline, and they competed for our catches.

In Table 9 of our report, our model results suggested that the total abundance of red snapper offshore of Louisiana during the

summer of 2020 was about six million fish, and that's from the model estimate, and a 95 percent confidence interval of about 4.7 million to 7.8 million fish.

The standard error was 791,199, with a corresponding CV of 13.1 percent. Most of the red snapper that we encountered occurred over UCB, at 63 percent, or 3.8 million fish, followed by standing platforms at 22 percent, or 1.3 million fish. Then natural banks, which the model approach yields 621,000 fish, pipeline crossings with 3 percent, or 196,000, and artificial reefs were at 2 percent, or about 100,000 fish. Scott, would you like to speak to any of those results?

DR. RABORN: No, and I think you've got it.

DR. GALLAWAY: Red snapper were not uniformly distributed across UCB. They were primarily associated with small bottom features of relief within this habitat, as we mentioned before, and red snapper over UCB habitat in the west region were much less abundant, and we got about 737,000 fish, than observed in the central, which had about 1.5 million fish, and the east region, again with about 1.5 million fish, and so that was our distribution.

Red snapper over UCB habitats were typically larger, given our sampling, and a lot over 610 to 888, and older fish, basically. As you see, they're six to twenty-five, but, if you looked at the age, it's basically four out to twenty-five, with most between four and fifteen years of age over UCB habitat. These are large fish.

In the summer, in our sampling, female red snapper from the mid-depth eastern zone, many of the snappers from this area had fully-developed gonads, and many showed signs of imminent spawning.

This is an important slide. Some 1,328,000-plus red snapper, 22 percent of the total population, were estimated to be present on the 821 offshore platforms present in offshore Louisiana in the summer of 2020. This compares to 1,353 red snapper estimated to occur on 882 platforms present in the same area in 2018, and these data are provided in the paper that I sent to Ryan, which he will distribute to you, and this is the in-press BOEM studies.

In both studies, I would like to put out the observed abundance differences by depth, with maximum abundance being about 3,000 fish per platform in the mid-depth range, and so this 3,000 fish, 2,900, it does not occur at all platforms, but it peaks, as you see here, around the fifty-meter depth range, and it builds from the shallows to that peak, and then it declines after that peak.

The size and age of red snapper at Louisiana platforms, they were characterized by a wide size range, ranging from small fish of around 270 millimeters total length to 770 millimeters total length, and those fish were between three and eight years of age. The total biomass of red snapper at platforms in 2020 was estimated to be 6,722,000-plus pounds, and this compares to a recent recreational fishing quota for the study area of 784,332 pounds.

Red snapper at natural bank habitats, in our model estimate, we estimated that there were 621,000 red snapper occurring on natural banks, and this represented 10 percent of the total abundance offshore in Louisiana, and, if you look at Table 9 in our report, the observed regional densities were not markedly different, and they were 766 to 876 fish per kilometer squared. The main difference was related to an area of habitat available within the region. For the west, we had 180 square kilometers, but, if you get to the central, we have 1,521 square kilometers, and in the east twenty-three square kilometers.

Given these observations, a total of 456,500 red snapper occurred on natural banks in the central region, and 146,820 were present on the western natural banks, and only 17,812 occurred on eastern natural banks.

Now we'll look at the size and age of red snapper on Louisiana natural banks, and it typically included large fish, with most between 470 and 690 millimeters total length, and the range was five to nine years in age was the high density. Red snapper biomass on natural banks within the study area was estimated to be on the order of 3,745,866 pounds, based on the size and weight information.

This is abundance of red snapper at pipeline crossings. Here, we got a total of 195,778 red snapper, having an associated biomass of one-million-plus pounds, were estimated to occur on these 514 crossings of large pipelines of greater than twenty inches in diameter, and these fish exhibited a broad size range, from 270 to 770 total length, with most being between two and eight years of age.

Here we have abundance of red snapper at Louisiana artificial reefs, and we got a total of 99,733 red snapper, with a total of 359,241 pounds, that were estimated to occur on Louisiana artificial reefs during the summer of 2020. We had a broad range of sizes that were observed at these habitats, from about 300 to about 750, with most of the fish between three and six years of age.

Overall, our population estimates that we conducted were characterized by small sample sizes, as we expected, given the time to do this, and this shows the sites in each of the west, central, and east zones. Sites 3, 13, 7, 17, 8, and 26 were platform and artificial reef samples, and they were all in the mid-depth. At each of these -- For example, in the west, for the platform, we marked thirty fish, and, at the artificial reef, we marked forty-five and released. The recapture sample size was 117 and forty-five, corresponding, and the tags returned were two and three from these.

In the central region, again, we marked twenty-eight and thirty-one fish, and the recapture sizes were in the eighties, and these included relative high tag returns. Four tags were included in those recapture samples.

In the east, we had difficulty catching fish there, and these samples are not good at all. We were only able to mark less than twenty fish at each site, and the recapture samples were a little bit better, but, as expected, those are not adequate for a population estimate.

The maximum likelihood estimate from the Gazey estimate for the West Platform Site 3 was 1,740 red snapper, as compared to, if you use our mean site abundance, that mean site abundance was 2,700. However, if you use the model approach, which we suggest, or recommend, it was 1,838, which is very close to the maximum likelihood estimate of 740.

For the Central Platform Site 7, the maximum likelihood estimate from the population estimate was 608 red snapper. If you skip down to the last sentence and compare that to the model estimate for this region and depth zone, which was 430 red snapper, again, it's fairly close, and, again, those are markedly different from the 1,491 fish, based on the mean site abundance approach.

The population estimates for individual artificial reefs in the mid-depth zone in the Louisiana area was estimated to be about 1,491 red snapper, based on the mean count estimate, and this estimate contrasts, however, with the population estimates, ranging from 608 to 902 fish, and the model estimates, ranging from 261 to 908 fish. Again, the latter suggests fairly good agreement. Scott, would you address this slide, please?

DR. RABORN: Yes, and thanks, Benny. Here is just a quick-and-dirty von Bertalanffy curve that we did for all the sites combined, just to show you how it compared to SEDAR, but I think a better

way to do it is to let the variance change as function of age, which we didn't do, and we need to, and then we need to do it by site and by stratum, of course, but for now, this is what we have and how it compares.

DR. GALLAWAY: A summary of our estimates of red snapper abundance and biomass by habitat type and/or area are summarized in this table, and you can see that our abundance sums to about six million fish, of which 3.8 million or so were over uncharacterized bottom, 1.6 million were over artificial reefs, and the distribution over other habitat is reflected, along with a corresponding biomass estimate based on the mean weight of fish at these sites.

Jim, I know you didn't want a comparison, but that was part of our charge. Anyway, this slide shows that the latest estimate for Louisiana from the Great Red Snapper Count is about seventeen million fish, which is roughly three-times larger than our estimate, and so I apologize for doing a comparison, because this becomes an important part of our study.

Where the differences come from, and these differences seem to be largely related to catch rates, rather than differences in area of habitat or number of habitats, is, as you can see from these slides, our areas, or numbers of habitat, agree fairly good. Where we get differences is in the catch rates, and these are big differences, and I think we have explained some of those with the preceding slides.

Like, if you apply a 2,900 estimate to all artificial reefs, 2,900 has a very -- That abundance level, or magnitude, has a very restrictive distribution, and so the summary is the area or count of habitats are similar between the two studies, and the catch rates, in the latest Great Red Snapper Count, are from two to five-times, or six-times, higher than the corresponding LGL estimate, and that yields total estimates that ranged from 2.4 to 6.2.

The observed differences in catch rates could be attributed to a number of factors, but, as one reviewer stated, quote, however, since the Louisiana estimates in LGL are based solely on sampling in Louisiana and adjacent federal waters, whereas Stunz et al. used extrapolated samples from outside that area, this provides some prima fascia support for using the LGL results in support of management.

The other external reviewer noted that he was much more comfortable with the LGL estimate of six million of the Louisiana sample than he was with an estimate of twenty-six to twenty-eight. Then again, I think the estimate would converge, if more sampling was conducted

in the Louisiana area.

The next slide, and there may be some bias in these, but these are -- The size differences of red snapper in the eastern Gulf appear to differ greatly from the western Gulf. In Florida, based on the information that I looked at, most of the red snapper are small, and 62 percent of the fish appear to be below the legal size limit of sixteen inches.

In Louisiana, a wide size range was evident in 2020, with 87 percent being above the legal size limit. Of course, there could be biases associated with those estimates, from hook-and-line sampling versus the paired -- Using the paired laser beams using to estimate the size of fish in Florida. I think I will stop here and answer any questions that might have appeared for this part of it.

CHAIRMAN NANCE: Okay. Thank you. Will, please.

DR. PATTERSON: Thanks, Jim. Since this slide is up, I will just comment on it. I think what Benny said there is correct, that a direct comparison between these two plots is not possible, because the plot on the top, which was produced by my group in Florida, at the 749 natural reef sites and then I forget how many dozen artificial reef sites, but there are about 800 data points, sites, sampled here, but with ROV and using stereo cameras.

We have a paper that we published on the stereo camera methodology, that shows it to be accurate in estimating the size of red snapper and other fishes, and so that's in situ, and then the plot below is fish caught with 6/0 and 11/0 circle hooks, which are, in both cases, a medium to a very large-sized circle hook, and there are a handful of studies published in the Gulf, and, again, my lab published a few of them, and there's a recent stock assessment simulation looking at contact selectivity that Steve Garner has published, and there's some work done by Matt Campbell and other folks at Pascagoula, National Marine Fisheries, that have looked at hook selectivity, contact selectivity, for these size range of hooks, and there is some recent work that FWC has done on the east coast examining selectivity as well.

I think it's pretty clear, across all these studies -- There is some slight differences in the estimates. However, in all cases, they show a dome shape for these circle hooks and the size range, and so you are likely underestimating the number of small fish quite substantially, but then you could also be underestimating the largest fish that are present.

Now, this distribution looks like it's pretty even from about 450 out to maybe 750 millimeters, but red snapper can grow to be a meter, and so what fish are you missing on the backend by using this approach, and I know that, in the western Gulf, it's difficult to use optical approaches, whether you use a laser scaler or stereo cameras to estimate fish size in situ, but I just think that this comparison, just for face value, is really apples-to-oranges.

CHAIRMAN NANCE: Thank you, Will.

DR. GALLAWAY: Scott, do you care to comment?

DR. RABORN: Well, we know that there is some selectivity going on with sampling with hook-and-line, and, as of right now, we haven't addressed it. All I can say is that the smallest hooks, the 6/0 hook, caught as many large fish as the largest hook, and it caught a wider range of fish, but that doesn't tell you exactly what the selectivity is.

How selective it is -- Like you said, it tends to be dome-shaped, from what people have published, and it's not as -- It doesn't taper off as much as say a gillnet selectivity curve, but I guess I can't really speak to the magnitude of the differences right now. I mean, I know some of it is due to selectivity, but some of it is due to the fact that it's probably larger fish in the western Gulf, and which one controls, I don't know.

DR. GALLAWAY: The smaller fish, Will, were mainly in the shallower zones, and a large part of our -- A large sample of our habitats were in deeper zones, and those were indeed characterized by larger fish, from looking at the SRV data, as well as the hook-and-line catches, that they were not dominated by small fish at the deeper depths.

DR. RABORN: Yes, and we can say that, within this study, even if it is hook-selectivity biased, the patterns fell out just like you would expect, and you saw larger fish the further you moved offshore in the deeper zones. As you moved from west to east, the fish got smaller. There are larger fish in the west and smaller fish in the east.

You saw smaller fish on artificial reefs and platforms, of course, that are fished more heavily, and then larger fish on pipeline crossings, which are probably not as easy to find by fishermen, and then followed by the uncharacterized bottom and the natural banks, and so those patterns bore out, but, still, it could have been shifted, to some degree.

DR. PATTERSON: I think examining those type of relative differences with using the same gears in Louisiana across the shelf would be informative about what the spatial distribution is of smaller fish, even though you're still likely -- Your catch is underrepresenting less than 300, or maybe even 400, millimeters in your catch, relative to the population, but you can make those relative comparisons.

I mean, the data that are presented here, they're the data, and those are the sizes of fish that were captured in the study, but it's just that next step in the report, and then here in the highlighted text, or the provided text, and they talked about this comparison between Florida estimates and Louisiana estimates, and that's just what I was commenting on, that they're not directly comparable, because of the way the data were collected.

There are other data, like the benthic longline survey that NMFS does, that clearly shows larger, older fish captured in the western Gulf relative to the eastern Gulf, and so I think there are other data that have biases equally distributed in the east versus the west that show, or indicate, there are older, larger fish more in the west than in the east, and there's clearly an absence of larger, older fish in the samples that we collected with stereo cameras off of Florida, which we inferred had to do with the fact that you have a rebuilding population and the age composition and size composition hasn't fully recovered in that system, but, anyway, that's the only thing that I was talking about here, is really that direct comparison and trying to use these two plots to make statements about what's going on in Florida versus the size and age comp in Louisiana, and I just don't think you can make that direct comparison.

DR. GALLAWAY: I think you indicated they were consistent with the overall pattern, based on relative sampling using the same gears across the Gulf, and the detail, the size information, by depth and region, is provided in the appendix of the report, to show those differences, even though we have not addressed it yet in the report. Thanks.

CHAIRMAN NANCE: Thank you. Roy.

DR. CRABTREE: Well, it's an interesting discussion, but it seems like the complexity and the difficulty with all this is we have all these different studies done in different regions, and they use different methods, and we're trying to compare them and somehow add them together, but we don't really understand how comparable they are, and that's the essence, I think of the difficulty.

CHAIRMAN NANCE: Thank you. Steven.

DR. SCYPHERS: Thank you, Mr. Chair, and thank you, Benny, for the presentation. I have two questions. The first one is about the tagging study, and I gather, from the report and the presentation, that there was the intentional recapturing by the project team, but I was wondering if you guys also had recaptures from fishermen reported, and, if so, how that data might feed in.

DR. GALLAWAY: On this one, I will have Kyle, our data manager, speak to that. We had such small samples, and I don't think we had too many, if any, returns from fishermen. Kyle, would you like to speak to that, or can you speak to that?

MR. MCCAIN: I can't speak to that directly, and I would have to talk with Pete, or the Auburn group, and I know they had some returns back from fishermen, but I don't know how much of that -- I'm sure we used that in our analysis, but I can't confirm it.

DR. RABORN: If I could just add something. I don't think those -- If we had tags returned from fishermen, they weren't counted as recaptures. That would just be considered tag loss.

DR. SCYPHERS: Okay. A related follow-up, and I might have missed this part too, but I was curious how many fish were tagged. I saw the site design, and I will admit that I'm also still trying to wrap my mind around the site distribution and the sampling design, but, across the sites where this tagging experiment was done, how many fish were tagged at each site or overall?

DR. RABORN: Don't we have a table in the slide presentation that shows that?

DR. GALLAWAY: If you could pull that table up, and I think it's maybe Slide 117, and it's somewhere in there. There you go. It's not very much. We went and tagged and planned on getting back a couple of weeks later to get a quick return, and so this was -- There were not a lot marked, and these were small sample sizes, and that's why we used the Gazey population estimator, because the small sample size is small tag returns, and so those are the data, in answer to your question, Steve.

DR. SCYPHERS: Okay. Thanks. My second question was just a curiosity question on the SRV data. If you encountered say snapper or other reef fish that you just couldn't identify -- I noticed the composition, that you told what proportion there were, and I was curious if there was some portion of fish that were not identifiable to species within that dataset.

DR. GALLAWAY: Kyle and Taylor, would you address that, please? I believe there's a table in the report that shows that, yes, there indeed were species that were not identified.

MS. BEYEA: That's correct. There was some proportion on some sites that, if you get fish in the distance, you simply can't identify it. We identified everything to as low a taxon as possible, but certainly we had an unidentified component.

DR. SCYPHERS: Okay. I will look back that, too. Thank you very much.

CHAIRMAN NANCE: Thank you. Sean, please.

DR. POWERS: Thanks. I don't want to get into too much discussion about why the numbers are different, because the PIs for the Red Snapper Count and our statisticians aren't on this call, the ones that specifically dealt with Texas and Louisiana, but I am surprised that the estimates are different.

I mean, Roy has got a point that different gear often, whether it's additive, whether it's comparable across regions, and it's surely an issue. Will conducted some comparisons off of Florida between the hydroacoustics and the ROV and found pretty large differences with the hydroacoustics in his case, and they were greatly underestimating, if I remember the results of his study correctly.

The justification for applying Texas to Louisiana, if you look at the Karnauskas manuscript, which used similar sampling techniques, and, now, that was relative abundance, but it's not confounded by differences in gear, and it showed pretty similar relative abundances between the eastern Texas and western Louisiana, and so that really surprised me, that the numbers were so different, just based on Mandy's synthesis of a whole lot of sampling done in that region with very similar gears, or identical gears, and I shouldn't say very similar, and so just a couple of observations there.

DR. GALLAWAY: If you look, extreme western Louisiana does compare very favorably to the upper Texas coast, and that, to me, seems to be the epicenter of red snapper in the western Gulf, is the upper Texas coast, say from Corpus all the way over to the eastern edge of Louisiana, and, if you look at the usSEABED map and look at the distribution of sand and gravel and circle that, I think you'll find that that corresponds to a high density, and so, yes, a certain part of our study area -- In fact, Greg and I compared numbers very early on, where we had just sampled the western

region, extreme western region, and we agreed very closely.

When we expanded our sampling to the west, that radically shifted, when you got over to those large areas of mud bottom with very little structural relief other than petroleum platforms, and so there are some spatial similarities and differences with the extreme western Louisiana tying into what I consider to be the epicenter of the distribution of red snapper, and I will probably disagree with everyone in the room, but I think we'll find out, over the long term, whether or not that's correct or not.

CHAIRMAN NANCE: Mandy, I am going to let you go in here, to that point.

DR. KARNAUSKAS: Thanks. I actually had sort of a different question, and I wasn't going to respond to that, if Will wants to go ahead.

CHAIRMAN NANCE: Okay. I will have Will first then. Thank you for that. Will.

DR. PATTERSON: Thanks. I think Roy raises an interesting point here, about this does add some more uncertainty to the process of trying to find truth in the midst of these various estimates, and we don't have the stock assessment broken down on a state-by-state basis, but we do have information about western versus eastern Gulf of Mexico, and what we know about all of this -- We have an estimate from the assessment about population size, and we have the Great Red Snapper Count estimate, and now we have, for Louisiana, this estimate from LGL, and so I think it's a bit unfortunate that the snapper count, the Gulf-wide study, was called the count.

Whether it's great or not is another issue, but just that it was called a count, because I think it imparts this idea that we went out and counted all the fish, that we took a census, and clearly that wasn't the case. There was a statistical design, and it differed among states, but each had a design, and we employed different gears, acoustic and optical, and Sean in Alabama did the depletion methodology to try to estimate red snapper abundance.

We end up with three different estimates that have different geographic areas that can be broken down to, and, obviously, the LGL estimate only applies to Louisiana. What we know about all of them is that they're wrong, right? None of these are accurate.

We try to minimize bias, and we try to account for bias, but none of them are accurate, and then we have precision, imprecision, in

the estimates that the CVs represent, and I just don't see how -- Thinking ahead, because, tomorrow, Greg will present the final red snapper estimates from the Gulf-wide project, Greg Stunz that is, and then we have this meeting in November that's on the books to try to utilize all this information again for management.

I just don't see how that's possible, given the amount of uncertainty that exists now with this new estimate in Louisiana, and, obviously the imputed estimates from Texas into Louisiana that was in the red snapper project are tough to give a lot of credence to, given the fact that we had to borrow data to fill in gaps in Louisiana, but Sean raises interesting points about the spatial distributions from other studies, and, again, how do we reconcile that with now the reports and spatial distribution of estimates from the LGL work?

There's a lot to reconcile here, and there's also reconciliation that needs to happen between all of the other data sources that are in the stock assessment versus these two other independent estimates. There is $15 million between these studies that has been invested in trying to come up with fishery-independent, or assessment-independent, estimates of red snapper abundance, and I personally, and this is my opinion alone, just don't see how this is possible at this stage, without a SEDAR-like data workshop fit into the current research track assessment for red snapper, so that we can do this right.

We can sit down and we can figure out -- Benny has pointed out some really important differences here in the site-specific estimates of abundance, or density, and these different habitat strata from the snapper count report versus what his group at LGL has estimated.

We need to dig into that. Why is that? Like where are potential sources of bias that haven't been identified? Which maybe has higher veracity, given whatever assumptions that were made with the data and how the application was limited? Anyway, in looking at all this information, I just don't see how, in November, we're going to be able to say, okay, here's the answer. I just don't see how that's possible.

CHAIRMAN NANCE: That's a good summary, Will.

DR. GALLAWAY: Will, I totally agree with that and that a workshop, or a sit-down meeting, similar to a data workshop, needs to be -- I think we could all reach consensus if we had that opportunity. Maybe a consensus on the wrong answer, but at least we would agree on the wrong answer.

DR. PATTERSON: I definitely think it's worth trying.

CHAIRMAN NANCE: Thank you. Mandy, we'll go ahead and have your question, and then we'll break for lunch.

DR. KARNAUSKAS: Thanks. Will's comments are actually a nice lead-in to what I was going to say. I wanted to comment that, essentially, all these studies are taking estimated densities, whether those are relative or total abundance, and multiplying those over an area, an assumed area of habitat, over which its densities are assumed to have some inference, and, Benny, I think you made an important point, if you could go to your Slide 115.

Really, it comes down to a difference in catch rates. It's 117. These catch rates are really important, because they can be really variable, of course, depending on when you sample, the season, hypoxia, et cetera, and, if you look at how they varied -- I think, in the Karnauskas paper, we estimated that the catch rates on artificial reefs were twenty-something-times those on natural banks, and there's other estimates out there that say that they're only two-times as large, and so, really, it's this difference in catch rates that I think influences a lot of these final estimates that we're seeing.

On that point, I am trying to wrap my head around -- On this slide here, you've got areas of natural banks, and then you've got artificial reefs, in numbers, but those artificial reefs have an implied area, and, here, it's a little bit difficult to understand how those catch rates differ, because one is in a total unit of the structure and the other is in a total unit of area. I wonder if those could be converted to area-per-area catch rates, to make them a little bit more comparable.

DR. GALLAWAY: Scott. I think that's possible, but I will let Scott address that, Mandy.

DR. RABORN: Yes, and that's what I modeled, was the number of fish per cubic meter, which was then turned into square meters. For the uncharacterized bottom and the natural banks, we extrapolated based on area, but, for the discrete sites, like the platform, we assume it had -- That all platforms had the same area, which was basically a radius of 115 meters out from the center, and then we just -- That was the absolute abundance, or estimated abundance, for a given site, or platform, and then you multiply that times the number of platforms, but it started out as number of fish per cubic meter and the number of fish per square meter, and so, yes, we have that, and we can compare it.

For a platform, it seems like it's a reasonable thing to do, but, for an artificial reef, like you said, they can vary in their areas, and so we assumed that they were all the same, that they all have the same area, and there was really no other way to do it.

DR. KARNAUSKAS: Okay. Thanks. I agree, and that was, I think, the same assumption I used in the paper that we did. I was downloading the -- I downloaded the report that was online, and I was looking at some of the relative catch rates on some of the different substrates, and some of them raised my eyebrows, and I can't remember specifically, but I do think that we should be looking at some of these comparative catch rates, and that's probably where a lot of these differences are coming from, as Benny pointed out.

DR. GALLAWAY: Again, I think, rather than doing it independently and then trying to reconcile, if we could take Will's suggestion and do it in a joint meeting, a data-workshop-type meeting, that would be much more effective and get to the answer a lot quicker than going back and forth.

CHAIRMAN NANCE: Okay. Thank you. Jack, I am going to come to you after lunch, young man.

DR. ISAACS: Okay. Very good.

CHAIRMAN NANCE: Okay. We'll go ahead and break for lunch and come back at 1:15 Eastern Daylight Time. Thank you.

DR. GALLAWAY: Jim, we're running well ahead of schedule, and so we'll finish early.

CHAIRMAN NANCE: I see that. Thank you, Benny.

(Whereupon, the meeting recessed for lunch on September 29, 2021.)

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September 29, 2021

WEDNESDAY AFTERNOON SESSION

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The Meeting of the Gulf of Mexico Fishery Management Council Standing and Special Reef Fish, Special Socioeconomic & Special

Ecosystem Scientific and Statistical Committees reconvened on Wednesday afternoon, September 29, 2021, and was called to order by Chairman Jim Nance.

CHAIRMAN NANCE: Jack, I think you were up next, and I think Benny is on, and so I think we're all set.

DR. GALLAWAY: I think all of our group should be here.

CHAIRMAN NANCE: Okay. Jack, if you're on, you have the next question.

DR. ISAACS: Thank you. Actually, I just wanted to say that I liked what Will had to say. When I started looking at this thing here, it kind of brought up more uncertainty, in some ways, about the plentitude of the red snapper, rather than less, and it's something that we really need to give some serious consideration to.

Another thing too, and, at the risk of sounding pedantic, it was a very fine presentation that we had here, but it used the wrong language in some places, and it used the Italian, prima fascia, which means "first face", and I think what you wanted to do was use the Latin phrase, "prima facie", rather than "fascia". Thanks. Not terribly important.

DR. GALLAWAY: Thank you. I was quoting a reviewer.

DR. ISAACS: Very good. Not a problem. The dangers of a classical education, I'm afraid.

DR. GALLAWAY: Thanks.

CHAIRMAN NANCE: You caught that, Jack. I'm impressed.

DR. ISAACS: Tell that to my Latin teacher. Thank you.

CHAIRMAN NANCE: You're welcome. Any other questions on the study thus far? You've had lunch to think about questions, and so anything else that you want to bring up now, before we go into Benny's discussion? Any questions on methodology or results or those types of things?

DR. GALLAWAY: Since we have this Slide 117 on the screen, I will just go back to that, briefly, and point out, again, that I think it's very important that the differences in the catch rates -- To address Mandy's question, I think we can, and I think Scott assured is that we can convert the discrete sites to numbers per square or

cubic meter, or square kilometers. We can put them all in the same unit and compare directly, and I think that will not be a problem.

I think, again, the big differences are in the catch rates, and I know that Greg will address the changes in the Great Red Snapper Count tomorrow, but I would like to note that there was -- Between the draft and the final in Louisiana, there was a reduction from twenty-nine to eighteen million, and the existing difference is still related to the catch rates, and so, again, I think, to Will's suggestion, I think, if all the investigators were able to participate in a workshop, we could probably come up with pretty close to a consensus answer, even though it may be the wrong answer, but I think it's -- I think it will get close.

CHAIRMAN NANCE: Okay. Thank you, Benny. If there are no further questions, Benny, why don't you go ahead and continue on with your presentation, please.

LGL: DISCUSSION
PRESENTATION

DR. GALLAWAY: The next slide I have is what 118 or so, and its impact on stock status, and so I would just like to start here. The final abundance from the Stunz et al. 2021b estimate, the final report, the estimate of red snapper in the Gulf of Mexico overall was estimated to be about 118 million fish in 2019, as compared to about 36.7 million fish estimated to be present by the SEDAR 52 stock assessment, and that's the SEDAR 2018. The references, incidentally, are at the back of the presentation.

The SEDAR 52 estimated that 23,643,192 age-two-plus red snapper were present in the western Gulf, which was Texas and Louisiana, from the mouth of the river west, as compared to 39,456,399 estimated to be present in the western Gulf by Stunz. The new estimate for the west Gulf was about 1.7-times as high as suggested by SEDAR 52, but, for the eastern Gulf, the final report from Stunz et al. 2021 estimated that 78,507,594 red snapper were present, nearly six-times as many as estimated for the eastern Gulf by SEDAR 17, which was about thirteen million.

However, if you take our estimates from Louisiana and combine then with the Texas estimates, which I believe the Texas estimates are consistent with what I think the population is there, the overall estimate in the western Gulf was very close to the SEDAR 52 estimate. The SEDAR 52 estimates for the western Gulf, as indicated, was 23.6 million, and the modified estimate, plus this study estimate, was twenty-eight million.

If we go to the next slide, it's a bar graph of the differences, and, in this graph, you see there are three sets of -- There are three bars, and the first one is SEDAR 52, and the second is the present Great Red Snapper Count estimate of Stunz et al. 2021b, and the last one is you take the Texas estimate and add it to the LGL Louisiana as a modifier.

If you look at the western Gulf as the black portion of the bar, and the eastern Gulf is the great portion of the bar, and so you have the total estimates that are shown there, with 118 million in the middle and the 36.7 million from SEDAR 52, but, if you add ours to Louisiana, you see that our western Gulf estimate is not really -- It's twenty-eight million, compared to twenty-three million.

Where the big difference is, it's in the eastern Gulf, where there's something like an estimated seventy-eight million. That's a lot of fish, and so, anyway, that's the results of that comparison. If we go to the next slide, our estimates, the Louisiana study area, we estimate that there is on the order of six-million red snapper, ranging up to I think a maximum confidence interval of about eight million.

We have an associated total biomass of over forty-seven million pounds, and this is based on the sizes that we collected at those sites. About 63 percent of the population was estimated to occur in association with UCB habitat, or features on UCB habitat, and about 27 percent occurred in artificial reefs. Only about 10 percent, in our study, was estimated to occur on natural banks.

Our estimate of six-million fish is lower than the estimated eighteen million red snapper extrapolated recently, but it still suggests a healthy stock is present. Despite our estimate that red snapper population in the study area was only about six million, the biomass was high, due to an abundance of relatively large, old fish.

For context of these numbers, the most recent recreational fishing quota for Louisiana that I looked at was about 784,000 pounds, or about 1.7 percent of the total red snapper stock size in the study area. The red snapper in the Gulf of Mexico, as we all know, are divided into west and east Gulf stocks. The west Gulf stock includes our Louisiana study area, plus Texas, and the eastern Gulf stock includes Louisiana east of the mouth of the Mississippi River, Mississippi, Alabama, and Florida.

When we combine our estimate with Stunz, we obtained a total west

Gulf stock of about twenty-eight million red snapper, and this compares pretty close to the SEDAR 52 stock assessment of 23.6 million fish.

The size distribution for red snapper in western Louisiana reported herein differs greatly from the size estimated in Florida, and we've discussed that and the potential for bias. However, Dr. Patterson has pointed out that independent estimates suggest that larger, older fish are present in the western Gulf, as compared to Louisiana, and offshore in Louisiana -- Offshore oil and gas platforms appear to be the most heavily-fished habitat in our study area.

In 2020, we estimate that this habitat had a standing stock of about 1.3 million red snapper, with an associated biomass of 6.7 million pounds. The average weight of red snapper at platforms is about five pounds, and the average length of fish was about twenty-four inches, well over the minimum size limit.

What we consider important is over 63 percent of the total population west of Louisiana occurred over UCB habitat, and these habitats -- The red snapper present were larger than elsewhere, averaging about twenty-six inches in length, 9.4 pounds in weight, and fishing pressure is likely reduced compared to the more dense aggregations over habitats with structure in charted locations. Thus, UCB habitats may in fact be serving as de facto, and I hope that's correct, marine protected areas.

Overall, I believe the Louisiana red snapper stock appears to be in excellent condition and is experiencing very low levels of recreational fishing mortality. The recreational fishing quota is only about 1.7 percent of our estimate of total stock biomass, and that pretty much concludes -- As I said, the references are provided that were cited in the text.

CHAIRMAN NANCE: Okay. Thank you. Any questions or comments at this point? That's the end of the presentation, Benny?

DR. GALLAWAY: If we get any questions, I may ask some of our other staff to chime in and provide their closing comments about their role in the project, and I particularly would have Scott and Jack, and, actually, I think we should maybe hear from Dr. Peter Mudrak, who participated in the study as a subcontractor under Auburn, but who has since joined LGL, and so I would like our people to have an opportunity to provide any final comments that they may wish to make, as well as help address questions, which I'm sure we'll get. Thanks.

CHAIRMAN NANCE: Okay. Sean.

DR. POWERS: A couple of things. One, I understand the discussion about differences in catch rates, but I am not convinced that's the whole reason for these differences between the different studies. There is clearly a lot we don't know about the sampling design and the rationale behind it, and that will influence the extrapolation. Hopefully we'll find out more about the thoughts behind the sampling design.

I am still concerned about the sample sizes and whether that was calculated correctly and how that will affect both the mean estimates and the variances, and so, while I understand Mandy's points, and Benny's and Will's, on catch rates, I am not entirely convinced that catch rates are the only difference here, and so, like I said, I look forward to hearing more about the design and getting into the analysis and extrapolation part of the questions.

I am still unclear though -- The immediate question I have is the difference between Texas banks and Louisiana banks. They would seem to be very similar, to me, and, now, I'm not an expert in that region of the Gulf, but I still don't fully understand why we would have differences, this large differences, between the eastern Texas natural banks and the Louisiana natural banks, especially with what we think we know about the longline fishery that operated in the Louisiana banks area that had pretty high catch rates, and so your thoughts on that, Benny, the differences between the east Texas and west Louisiana banks and why those numbers are so disparate.

CHAIRMAN NANCE: Benny.

DR. GALLAWAY: If you look at the distribution of the banks -- If you look at Sonnier Bank, the catch rates were really high, and it was an appropriate depth. If you get out to the shelf edge, if you look at the four western Louisiana banks, those catch rates were more similar to the Texas catch rates. There may be differences associated with difficulties in sampling the banks, and we could go over our methods that we used, but I think there are similarities in Texas and far western Louisiana, but, as you move further to the east, those catch rates change.

The platform differences I do believe are stratified by depth, and they are not representative of all the -- The 3,000 is not representative of the catch rates at all the artificial reefs here, and so, again, much of these are kind of difficult to address in this setting, but, again, would be -- I believe, if we all got in the same room for a workshop, we could reconcile a lot of these

differences fairly quickly and more appropriately than what's available in this setting.

DR. POWERS: But what was your sample size on those banks? How many samples is that dependent on, and I know I can put it back together by going to the graphs, the tables, but I was hoping that -- I mean, it seems like, again, when I worked through the math, that there is only two or three samples on each of those bank areas, unique samples.

DR. GALLAWAY: That's correct, and, Kyle and Taylor, can you address this?

MR. MCCAIN: There were fifteen -- There were a total of fifteen discrete-type sites on each of the banks, or for the banks, the natural banks, samples, and so there were -- We picked five banks, and then we did three samples on each one of them.

DR. POWERS: So a total of fifteen out of the 106.

MR. MCCAIN: Correct.

CHAIRMAN NANCE: Sean, did you have another one? I'm not sure -- From a standpoint of sampling design, is there anyone that can speak to that?

DR. POWERS: I think, Jim, we really need to have a sense of what the -- How the sample sizes were picked, what sites were picked, what was the universe that we're talking about, and what was the extrapolation they intended when they sampled those.

CHAIRMAN NANCE: Benny, or is there anyone from the Louisiana Wildlife and Fisheries that can speak to the sampling design protocol and how those were selected?

DR. GALLAWAY: I will open it up, and then I will give my impression. Does anyone from Louisiana want to speak to that? If not, I will speak to it.

CHAIRMAN NANCE: Okay. Benny, I guess.

DR. GALLAWAY: The dominant habitat type was petroleum platforms, and, as I have indicated, those were a stratified random -- Selected on a stratified random basis. The other sites were more or less sampled and stratified again by depth and region and more or less on almost a transect basis, but modified by depth and sediment type and habitat type, and so I don't know that those were randomly stratified selected.

DR. POWERS: Can I follow-up, Jim?

CHAIRMAN NANCE: Yes, Sean, please.

DR. POWERS: So that last issue, that not knowing if they were randomly selected and how the stratification worked, is a major issue. I mean, I would assume that your analysis assumed that they were randomly sampled and they were stratified in some proportion, and this gets to Luiz's earlier comment about whether weighting is appropriate and things like that, but, at its basic level, that's a very important point, and so, if we can ask -- I'm sorry that I don't remember the statistician's name, but was that an assumption in his analysis, that they were random?

DR. GALLAWAY: Scott, if you will speak to that.

DR. RABORN: Yes, it was an assumption that they were randomly sampled.

CHAIRMAN NANCE: Okay. Any other -- John, please.

MR. MARESKA: Benny, I got the paper, and thank you for that. I guess my question is, the work that you did at the BOEM sites in 2017 and 2018 that were included in this study, the hydroacoustic work was in a spiral pattern, which probably isn't that much of a difference, but I also noticed that I didn't see where decibel differing was used in calculating those abundances, and have you all looked to see if there is any differences in estimates from the work done in 2017 and 2018 versus the recent study?

DR. GALLAWAY: I will defer to Jack on that one. Jack, if you can take it.

DR. EGERTON: In the BOEM study, we didn't have the capacity to do the decibel differencing. We only had one echosounder, but I haven't personally checked out the difference between the results, in numbers, of that study versus this study.

MR. MARESKA: Okay. Thank you.

DR. RABORN: I would like to add that the SRV data for the BOEM study -- We did include some species that we didn't include with the samples collected in 2020, because of the differencing, and so, in other words, in the BOEM study, hydroacoustics would have included some individuals and species that the hydroacoustics in the 2020 study did not, and so SRV data was adjusted accordingly.

CHAIRMAN NANCE: Okay. Will.

DR. PATTERSON: Thanks, Jim. Earlier, Harry said he was going to try to check with folks back in Baton Rouge, perhaps, to see if there was more information available about sample site selection and design, and I think that's kind of fundamental to understanding entirely this work, and so I am curious if Harry has got any feedback there or what the prognosis is for getting that information.

CHAIRMAN NANCE: It looks like Harry has got his hand up. Harry.

MR. BLANCHET: Thank you, Mr. Chair. I did circle back with some other folks that were also involved with this, and part of the problem is that I wasn't intimate with the full design, and it was, like a lot of things, done by several different people, and so I hate to get too much into the weeds of the specifics of how this -- You know, why did we choose Site A and not Site B, without more background, but what I would prefer to do is, if you want something that is more definitive, we can write something up and provide it to you, but I think that Benny's characterization is pretty much what I understood. I can get you more detail about that, and if that would work. If not, I can see if I can get somebody else this afternoon to talk about it in some more detail.

DR. PATTERSON: My recollection here is that what Benny said in the report, and in his presentation, is they were provided a list of 106 sites, and that's what is indicated in the RFP, that 106 sites have already been selected, and the contractor, whoever won the competition, would be responsible for providing abundance estimates based on those 106 sites.

There really is no information yet provided about how those sites were selected, and, if it's a question of why was Site A versus Site B selected, then that seems, to me, that it's haphazard and not random, and that's really, fundamentally, what we need to know, is was this as stratified random design, and how was the randomization conducted, versus were these 106 sites that were picked by somebody for some reason other than just a stratified random design.

MR. BLANCHET: As Benny said, and as I said earlier, the basic design is stratified and random. There may be some additional components in there that I don't have full knowledge of, and so I don't want to pretend that I am the expert on it, because there was a group of people that was involved with this development, and I can see if I can get someone to speak in more detail about it, or, if you prefer, I can get something more in a formal working

paper type of a presentation.

DR. GALLAWAY: I can speak more about it as well. Louisiana consulted with a lot of people, including us, as the BOEM representative, and so we had inputs into the selection of the 106, just like a lot of other people did. The selection, the final selection, was approved by the state, and, again, the habitats were stratified by sediment type, habitat type, depth, region, and so forth.

Within that, your selection of sites becomes somewhat influenced by logistics and the distribution of habitat types, or bottom sediment types, within that region, and so your options become less, and so it's not like we didn't have any input. As the BOEM contractor, we did, and as well as other people as well, and that was the final selection that was made, and, yes, it has some warts. Is it representative of -- I don't think so. Thanks.

CHAIRMAN NANCE: Okay.

DR. PATTERSON: Jim, can I just respond to that, real quick?

CHAIRMAN NANCE: Yes, Will, please.

DR. PATTERSON: All that information, and that's great information from Harry and from Benny, and it sounds to me then that the sample design was haphazard, if you're worried about logistics or distance from port. I can understand experts having input in indicating what the habitat distribution is and what your sample frame actually looks like that may feed into the stratification, but, from there, it should have been a random sample of discrete habitats and locations and depths, whatever your strata are, within that.

It sounds to me like that didn't happen, and we just need to have -- I think Harry's idea of getting somebody to write down exactly how the selection process occurred would be really helpful, and so thanks.

CHAIRMAN NANCE: Yes, and I think that would be -- Harry, I think that would be the best solution, is to be able to provide a written paper, if you will, of how the selections were made for each of those sites. I know it won't be today, and I realize that, for sure. Luiz, to that point?

DR. BARBIERI: Yes, and thank you, Mr. Chairman. Harry, if you could add to that document, in terms of sampling design, also how the sampling allocation was arrived at for all the different

strata, and that would be super helpful to understand how that ties into the actual estimation process, at the end.

MR. BLANCHET: When you say sampling allocation, do you mean the number of samples per stratum?

DR. BARBIERI: Exactly, yes.

MR. BLANCHET: Okay.

DR. BARBIERI: To whatever sets, or strata, you have, and so, for example, there is different sample sizes per stratum that you have, right, and so did you use a scheme of like, for example, probability proportional to size, or did you use some other type of sampling allocation, scheme, that would then allocate more samples to sites that are either larger in size or have a higher variability relative to other sites that are less variable.

CHAIRMAN NANCE: Yes, I think that would be an appropriate way. Trevor.

MR. BLANCHET: Okay. Got it.

CHAIRMAN NANCE: Thank you, Harry.

MR. MONCRIEF: My point was just to echo the last sentence that Will had and then what Luiz just said, and I think, if we're going down this route of having a discussion between all of them, I think Harry has got some time to be able to get their stuff together, and we're going to probably rehash this all over again when that meeting comes through, and so that's the only point that I was going to make.

CHAIRMAN NANCE: Okay. Thank you, Trevor. Sean.

DR. POWERS: A written document is fine. In addition to Luiz's comment, if it is -- If it's haphazard, then it doesn't matter, but, if it is random, giving us the universe of where these samples reside, and how big that universe is, would also help, building on Luiz's questions, so we know that, within one strata, there is a thousand platforms, and in another one there are ten, and getting some idea of the size, and not only the sample size, but the size of the universe that you potentially could choose from.

DR. GALLAWAY: In the introductory material, where I gave the maps and numbers by region and depth, that wasn't adequate for that, or you would like that expanded, or give me some advice there.

DR. POWERS: Just if we could put it in the table, and, I mean, the platforms are fairly easy to do, but how did you choose exactly the location on the banks to start your survey on? Did you have a gridded system? I think that's what Rocher used, a gridded system, and then he chose grids at random, and so number of grids in the possible universe versus how you chose those specific starting spots, and so the information on the platforms and the artificial reefs is pretty clear, and those are known locations. The natural banks and uncharacterized bottom, it's a little fuzzy on how they would have been chosen at random.

DR. GALLAWAY: So it's those two that need elaboration, and, Harry, I think how we exactly did that on the banks and UCB -- We would probably be best to address that and provide that to you.

MR. BLANCHET: Yes, I agree.

CHAIRMAN NANCE: Okay, because I think there's things like, if you have an area that's twice as big as another, did you have two samples in that and one in the smaller area, those types of things, and so it's sample allocation, randomness, and things like that. Scott.

DR. RABORN: One thing I would want to point out, that I don't think was made clear, that was perhaps misleading, is that we had 106 sites that we sampled, but we used an additional thirty-seven platform sites from our BOEM study, and so 106 plus thirty-seven, and, in the BOEM study, those sites were chosen randomly within strata, and they were allocated based on the number of platforms present at the time we were sampling, and so they were allocated proportional to the number that were there, and it was truly a stratified random sampling.

CHAIRMAN NANCE: So they were chosen -- If you had a hundred platforms in a certain area, and you had 300 out here, you then had three samples in the outer one and one in the --

DR. RABORN: Correct.

CHAIRMAN NANCE: Okay. Those types of things for the platforms.

DR. RABORN: Yes.

CHAIRMAN NANCE: Okay, and so the platforms are one thing, and that seems to be reasonable, but I think it's the other questions on the other sampling regime and how those were randomly selected within each of those different strata. Benny, that would be great to have.

DR. GALLAWAY: Yes, and, Harry, we can work with you on development of that paper that we're talking about.

CHAIRMAN NANCE: Okay. Thank you.

MR. BLANCHET: Thank you.

CHAIRMAN NANCE: Okay. Any other questions on this part? Roy.

DR. CRABTREE: Just sort of a comment on the western Gulf part of all this. The notion that the western Gulf is in good shape I think is pretty consistent with the last assessment, which I think projected out that the western Gulf was likely rebuilt at this point.

The one concern that I think we need to look into a little more -- I mean, I have some concerns, given the Louisiana results, and it causes me to wonder if the Texas Great Red Snapper Count estimate might not have some issues and potentially be biased as well, and then, if you use the Louisiana portion of it, then it matches reasonably well with the assessment. It's a little higher, but we have a lot of reason to think that the catches in the western Gulf, recreationally, have been underestimated, and that would tend to do that.

The eastern Gulf differences really are concerning, and I guess we'll get into all of that tomorrow, but I think, somehow, we've got to come to some resolution for that, but it's certainly clear to me that there are still an awful lot of questions and things that need to be figured out before we're going to really know what to do with this.

CHAIRMAN NANCE: Josh.

DR. KILBORN: Thank you. I just wanted to clarify that the written document that -- I'm sorry, but going back to the sampling design issue. The written document that is proposed for being worked up, are we -- I would like to see more detail about the selection of the artificial reefs and the other fixed structures as well and not only the uncharacterized bottom and natural reefs, and so I just wanted to clarify, and is that everyone's understanding, that we're going to get a full workup of how all of these sites were selected, as far as the allocations of numbers and what the actual stratification design was?

CHAIRMAN NANCE: I think that probably would be appropriate. Benny, is that possible to do? It sounds like the platforms were

out of your other study.

DR. GALLAWAY: There are additional platforms here, but, yes, predominantly they came from the other study, and, yes, we can address -- What I am hearing is the focus should be on UCB, the Louisiana artificial reefs, and the natural banks, and is that correct?

CHAIRMAN NANCE: I think what we're asking is, for the sampling design, we need a good, detailed map of how the samples were -- How the sites were selected for each of the samples.

MR. RINDONE: Yes, for everything.

DR. KILBORN: Correct.

DR. GALLAWAY: Including platforms, and so you want the entire --

CHAIRMAN NANCE: Yes, sir.

MR. RINDONE: The entire sampling frame for all strata for all habitats for the entire study, I think is what the SSC is asking for.

DR. GALLAWAY: Okay. That won't happen overnight, but, yes, that can be provided.

MR. RINDONE: That's not acceptable, Benny. Tonight.

DR. GALLAWAY: Okay. I will get on it.

MR. RINDONE: Thank you, Benny.

CHAIRMAN NANCE: Not tonight, but maybe tomorrow at ten. No, I'm just kidding. I understand that it's going to take a little time to do. Let me ask, from a -- Any other specific questions on the study? I would like to hear discussion on how to move forward.

DR. KILBORN: I actually did have one more question, if you will allow it.

CHAIRMAN NANCE: Yes, Josh. Absolutely.

DR. KILBORN: I was wondering -- Getting back to Will's point about the potential underestimation of the smaller size classes, are we going to deal with that, or did we just kind of forget about that, because I do feel like that may have -- That may be playing a role here, to some degree, in the difference between the eastern numbers

and the western numbers, and it does kind of feel like maybe this study has the potential to have underestimated the smaller size classes. I mean, I don't know that we can really do much about it at this point, but are we going to address it in any more detail, or is it kind of an it-is-what-it-is kind of a thing?

CHAIRMAN NANCE: Well, Scott, I will go ahead and let you -- He's got his hand up to address that, Josh.

DR. RABORN: When you say the smaller fish were underrepresented, the age frequency histogram, and the length frequency histogram, may be biased, if we underestimated the smaller fish, but that doesn't have anything to do with the absolute abundance. The hydroacoustics were picking up smaller fish, and you would have seen them on the SRV, and so let's be clear about what it's representing, or what it's biasing, and it's not biasing the absolute abundance estimate. It would bias our age composition or size frequency composition.

DR. KILBORN: Thank you for that clarification.

CHAIRMAN NANCE: Okay. Is that --

DR. GALLAWAY: I would point out too that independent surveys, like the longline surveys, there is additional information that went out in the size distribution between the eastern and western Gulf, and so, as a generalization, I think it's correct, and so, the degree of the bias, I'm not sure, but I think the generalization can be successfully defended.

CHAIRMAN NANCE: Okay, and so we've heard this presentation. Sean, go ahead.

DR. POWERS: Sorry, but just to follow-up on Josh's point, which I don't want to dismiss necessarily so quickly, when you say that the hydroacoustics, which was the basis of the count, is not size selective, and, therefore, your number is fine. You might have a problem parsing it into the correct age categories, or size categories, but your number is okay. Is that the rationale?

DR. RABORN: That's my rationale, but I'm not a hydroacoustics expert, and so you had all sizes of fish, and different species of fish, and I guess you're getting at target strength, and so I guess I would defer to Jack on that, to what extent would hydroacoustics be biased towards different sizes of red snapper.

DR. EGERTON: Well, I think, really, we've got to work on the assumption that, if the fish had a swim bladder, it was counted in

the hydroacoustic abundance numbers, and so they should have been picked up, but, if something is bigger, then it's going to have a stronger target strength return, and so potentially there's a slight bias that way, but I think we've got to work on the assumption that, if they are swim-bladdered, we would have picked them up.

DR. POWERS: Okay, and then your species composition, and so that's the numbers part, but then your species composition -- Would that not be size selective, based on your catches in the longlines and in the cameras, because the cameras are where you get your species comp from, correct?

DR. RABORN: That's correct. We didn't use the hook-and-line for species composition at all.

DR. POWERS: Well, I don't know the answer, and I'm not a hydroacoustics person either, and I don't know whether the assumption of no size selectivity goes for hydroacoustics.

DR. EGERTON: I think, really, of all the sort of available methods, that the hydroacoustics should be pretty robust against size selectivity, and so I think we're all right, from that respect.

DR. RABORN: As far as size selectivity goes -- Like I said, if you're talking about gillnet selectivity, those slopes are pretty steep on either side of the maximum selectivity, but, from what I've seen from hook selectivities, it's rather broad, and, if you're using multiple hook sizes, it sort of guards against some of that selectivity, but you can't quantify and exactly say there was no selectivity, and, obviously, there was.

You would tend to think that it would be biased towards larger fish, and I don't know that, but you would think that, but you have to ask yourself -- If you see a substantial difference in age composition between the western Gulf and say off of Florida, is all of that really due to hook selectivity? I mean, certainly that contributes to it, but, you know, you would think --

DR. POWERS: No, and I don't think that's what we're saying. I think we all recognize the difference in the eastern and western sizes and the expectation that the fish are larger in the west, but it would influence your -- Since both counts, I assume, are supposed to be two-plus-year-olds, is the difference in the two to four-year-olds -- Does that drive a lot of the high numbers, at least in the east?

Are you all picking up the two to four-year-olds in the abundance they are, or is there something with the size selectivity, and I think that's the only issue, and, like Josh said, I think it should be explored a little bit, and essentially give us the argument of why you don't think that's the case.

DR. GALLAWAY: In the appendix to the report, you will find size by region, depth, and habitat that shows a shift in size distribution from onshore to offshore and from east to west, and so there is more detail in the report than has been formally discussed.

DR. RABORN: It's interesting. If you look at the catch data from the hook-and-line database, clearly -- We used squid and menhaden, and we used two different sized hooks, the 6/0 and 11/0, and, unlike other studies, we didn't -- We maintained the same size bait for both hook sizes, and so we didn't confound bait size with hook size.

We caught way more fish on squid, regardless of hook size, and they caught as many big fish as did menhaden, and I almost wonder if adding in the catches from the menhaden biased the size or age composition, because squid caught the whole range, a broader range, of fish and caught a lot more smaller fish. Anyway, all of that is to say that there is work to be done on that, and we need to look at hook selectivity, and we need to do it by bait type, and I don't know how exactly, but, if we can get at bait selectivity, that would be nice too, but it's something we're currently working on, and I do appreciate your comment.

CHAIRMAN NANCE: Thank you. Will.

DR. PATTERSON: Thanks, Jim. This issue of the size, potential size, bias is important, obviously, and I take the comments from the LGL folks about the acoustics shouldn't have the same issues that hook selectivity likely had with the hook-and-line sampling. However, you know, and another reason why a workshop could pull people with lots of different expertise together is Kevin Boswell, on the Gulf-wide team, has the most hydroacoustics experience, and he worked with -- He ran the eastern Gulf, at least the Florida, sampling, and he worked with the western Gulf, to go through post-processing of the data to come up with estimates.

One thing that was done, that I didn't see in the methods here, or see in the report, and they were -- The acoustic methods and data extraction and estimation are well detailed, and so I assume it wasn't there, but it was that Kevin truncated the target sizes distribution more so than I think was done here, as an added layer

of filtering, to try to --

As far as the community composition of potential targets that could be red snapper, and so that there's an extra step of filtering I believe that occurred, but, again, having Kevin present to go through that, to try to figure out some of the methodological differences that may have existed. I'm not really sure how much of an impact that potentially could have had in different estimation processes, but I think that was present.

As far as the size composition from the hooks, and so the catch, when I look at the data, I can see three potential areas where there is potential bias. The first is underrepresenting the smaller, younger fish, and the second is underrepresenting the larger, older fish that are likely present in the population, but not seen in the catch, and then, third, you have this distribution that's relatively flat over about a 400 or 500-millimeter range.

That's not likely to exist in the population. We have natural mortality and fishing mortality that is removing individuals as they age, and so that should be more of a logarithmic decline than this just kind of smooth, flat peak and then almost a knife-edge at around 750 or so millimeters, and I don't have the slide pulled up, and so I'm doing that from memory.

Anyway, it does matter, and I'm not sure there is a way to fully account for that, but just, when we're making comparisons, we need to have that in mind, that, even when talking about inshore to offshore, smaller, younger fish inshore and larger, older fish offshore and what have you, we're still missing quite a bit, probably, of the smallest, youngest fish that occur in these habitats in any place, and so I just think we have to keep that in mind.

CHAIRMAN NANCE: Okay. Thank you, Will. I would like SSC recommendations, and there's been some things that have been proposed, workshops and things like that, and we've got this study that we looked at today, which gives us estimations of red snapper in the Louisiana area, and we have the Great Red Snapper Count study that is going to be presented tomorrow, and we've seen that before. We saw that in detail in April, and so how do we, or do we want -- How does integration of these things happen? That's what we're looking for in the end, if that's possible.

Do we wait until have more comfortable numbers from this Louisiana study, or do we say we want to do this under a SEDAR process, and we've got the data workshop coming up in May, and is that an appropriate place for this, or do we need to have something before

that? I would like recommendations of how we move forward on this. Jim Tolan, please.

DR. TOLAN: I am pretty good at punting, because of some of the really pointed questions about the sampling design that we brought up and the selectivities of the hooks, and there's just a number of things that I think the way forward isn't quite here yet, and so I think it's a great study, but I think the way forward, as far as the SSC incorporating this, I don't think we're quite there yet, and so I just wanted to throw that out there.

CHAIRMAN NANCE: What I was suggesting, Jim, is not incorporating it this second, but to come back at another meeting, say in a SEDAR meeting in the data workshop, and have this presented with all of the other numbers, so we can see that, or do we need to see that before that, so we can recommend whether we want to see that in the SEDAR process?

DR. TOLAN: To that point, Mr. Chairman.

CHAIRMAN NANCE: Yes, please.

DR. TOLAN: I totally misunderstood what your definition of a way forward would be, and I am really happy just to put this in the context of a workshop coming up, once we get some of these different answers of the really pointed questions that have been raised so far, and so I misunderstood what you meant by, from the SSC members, what you think is a good way forward, but that's all I have.

CHAIRMAN NANCE: Okay. Thank you, and sometimes I don't talk well, and so, if you misinterpreted it, that's fine. Carrie.

EXECUTIVE DIRECTOR SIMMONS: Thanks, Mr. Chair. I am looking at an email from Patrick Banks, and he is saying they do have, I guess, a staff member or someone, Jason Froeba, who is available to talk about the study design, but apparently he's not on, and so I don't know if we want to take a quick break and try to get him on, and if he can answer some of those concerns today, or if we absolutely need something in writing, but I just wanted to bring that up.

CHAIRMAN NANCE: I think he could answer maybe some, but I think the questions we're asking probably are a little more detailed. They're probably going to take a little time to work into a paper, and that's my impression of what we're asking for.

DR. GALLAWAY: I will speak to that, too. I would like to work

with Jason and Harry and our staff and put together a more comprehensive discussion and description that would be available from any one person giving their recollection, and so I think we should all get together and provide you the most detailed explanation possible, which would include contributions from all of us.

CHAIRMAN NANCE: I think that would be the best way. Like Jim said, there are a lot of things, right now, that we're kind of unsure on, and so our path forward is a little blocked at this time, until we see that, and so, when we get that, I think we're able to then make some determinations on the study and things like that. Trevor, please.

MR. MONCRIEF: Real quick, Ryan, what's the current dates on the SEDAR data workshop?

MR. RINDONE: Right now, we're looking to have this thing rescheduled for May 2 to 6.

MR. MONCRIEF: That's multiple days, designed to go over all the other questions that we have, correct?

MR. RINDONE: It's five straight days. There will be plenty of opportunity between now and then to try to have some more webinar opportunities to talk about things in chunks, leading up to the meeting, to make sure that everyone is adequately prepared for the meeting. The data deadlines have remained unchanged for data preparation and provision to the Science Center, which will help with them being able to package everything together and take some initial looks at different things.

The data for the Great Red Snapper Count have already been provided to the Center, and they're going to work with Greg et al. to ask any questions about specific things, like exploitation rate by strata and anything like that that they might be interested in. There is plenty of time, I think, between now and that workshop to have an extra webinar or two, if it's necessary, to try to flesh out some things and get some questions answered.

MR. MONCRIEF: Yes, and that's kind of what I was getting to, Mr. Chair, is that we're -- I mean, I think that data workshop for May is going to be heavy. There's going to be a lot of stuff, because there's going to be a lot of stuff going on, as there always is for red snapper, and so I think having something that's separate and specifically for this, prior to that data workshop, is probably the most appropriate thing.

CHAIRMAN NANCE: I think that's a wise -- Having been to those workshops, those data workshops, in the past, there's a lot of things that go on, and so having this as a separate thing before that, so that we come to the data workshop having it already fully decided is probably the best way forward. Sean.

DR. POWERS: Trevor and Jim covered it, and I lowered my hand. I agree with them that we need to get this -- We need more information on this study before we can advance to a workshop setting.

CHAIRMAN NANCE: Yes, but, Sean, I do think that, once we get that data and look at it, a workshop probably would be the next step. Is that --

DR. POWERS: Yes, depending on what we find out from the information, and, either way, whether we -- For both studies, whether we take the extrapolations, in the end, for the assessment, or whether we just use the data, and they will take the estimate, and those are all up in the air, and so, hopefully, a workshop, either agreeing to what number we feel comfortable with for both studies, or just agree that here is the data and here is the compatible data that we could use going forward, and either one of those requires a workshop.

CHAIRMAN NANCE: Okay. Thank you, Sean. Luiz.

DR. BARBIERI: Thank you, Mr. Chairman, but Trevor and Sean already addressed my points, and so thank you.

CHAIRMAN NANCE: Any other points? I think what we're recommending is get this new data, and when would we look at that, Ryan? We would have to have it for the next SSC meeting, right?

MR. RINDONE: We have a meeting scheduled for November 18, but I don't think that is -- I don't think that's the appropriate time. We only have one day for that, and so --

CHAIRMAN NANCE: Let me ask this, and this is just me thinking here. Maybe November 18, if we get all this information about the study, the methodology and things like that, we could use that day to go over that. Then, from that, we could determine whether the data is -- If we can use it or not, and then we can plan a workshop where we get together with both, to be able to determine the best steps forward, and would that be something that's doable?

MR. RINDONE: Well, I mean, it sounds like, to me, that there's more at play here than just one or both of these surveys, based on what you guys are talking about, and so is that what you want to

do, is --

CHAIRMAN NANCE: I guess I'm talking about -- Right now, we have some questions on the study that we just looked at today, some methodological questions, and so would the November meeting be a time where we can get that information and have discussions on it? That's kind of what I'm --

MR. RINDONE: So, I mean, we have that scheduled already, and we haven't issued a Federal Register notice on what's actually going to be discussed yet. I mean, you guys looked at an agenda that I put together that was predicated on this study and the Great Red Snapper Count being comparable and you being able to move forward with some sort of recommendation about catch limits, if you thought it appropriate to do so.

We can pivot to additional consideration for the LGL study and talking about some of the things that have been discussed here, talking about the sample frame and anything else that you guys want to bring up. I mean, it can be used for that, sure, but I am just letting you know that we only have one day set aside to be able to do that, and so it will be constrained to a day. Our plan would be to do a hybrid again, just like we've been doing.

After that point, your next meeting is the January SSC meeting that is -- I am anticipating it being three days and it being, if you break out your calendars, being the week of the 10th, and so like Tuesday, Wednesday, Thursday, depending on you guys' availability for that, but there's a lot of stuff in that one already of things that we need to get knocked out for that meeting already, and so it may require carving out some additional time somewhere else, and, again, like, if all this information is intended to be submitted to SEDAR, for consideration for SEDAR 74, then there will be also opportunities for things to be discussed as part of that, and so there may be some resolution that can be had and documented through that process, and that would be advantageous to the SSC's deliberations as well.

CHAIRMAN NANCE: I just see that, like Trevor is indicating, that May data workshop is full, and I don't see how these two could be discussed in that setting in a timely manner.

MR. RINDONE: No, and I'm not saying that these two things would be discussed only there. There had originally been planned a webinar to talk specifically about the data available, and the subsequent use of those data, from the Great Red Snapper Count into SEDAR 74, but that webinar has been delayed, and it will be rescheduled. I see that Julie has her hand up, and I would kind

of like to hear her perspective, Mr. Chair.

CHAIRMAN NANCE: Okay. Julie, we'll let you go first, and then I will do Jim and Sean.

DR. JULIE NEER: Hi. I just wanted to echo what Trevor and others have said, is that, from your discussions today, and your discussions on the previous Great Red Snapper Count presentation that you received in April, and I'm assuming that what you will receive tomorrow as well will generate discussion, and I would not leave this for the data workshop.

In addition to these new data sources, we also have to figure out how to incorporate the state data, if it is going to be used, and we also have to figure out how -- There is a variety of new data, and we have new stock ID definitions, which are going to require getting -- Different from the previous assessments, which are going to require some re-jiggering and discussions of how that data is going to be partitioned, and so I would bet that the data workshop is going to be incredibly busy.

I don't see that you're going to have the quality time you need to talk about this, and, additionally, it sounds, to me, like this group wants input from a variety of people who probably won't be at the data workshop, such as all the statisticians that were involved in some of these Great Red Snapper Count and Louisiana LGL studies, who are not probably going to be at the data workshop.

I would strongly suggest, given your conversations thus far, that you guys plan on something to bring in those people who have the expertise on some of these sampling design questions that you're bringing up, because it doesn't sound like, from the names you guys are mentioning -- Those people are not currently coming to the data workshop for red snapper. We do have about fifty people coming, but a lot of those people that have been mentioned are not on that list, currently.

CHAIRMAN NANCE: Okay. Thank you.

DR. NEER: I want to make sure you have the appropriate people you need for those discussions.

CHAIRMAN NANCE: Julie, thank you. I appreciate that. Jim.

DR. TOLAN: Thank you, Mr. Chairman. As soon as I saw Julie's hand go up, I figured that she was going to jump on the couple of points that I was going to raise, but, in terms of this upcoming one-day workshop, as it deals with the issues before us now, my

suggestion is we treat this kind of like SEDAR working papers and that the folks from Louisiana and Harry's staff, and we get these other questions about sampling design hashed out and written down.

If they could get it to the rest of the SSC ahead of time, like they do the working papers, we could go into this one-day workshop fully prepared to hammer it out and not be swamped with a bunch of stuff on that very day of the workshop. If we're limited it in time, I would like to treat it like working papers. Thank you.

CHAIRMAN NANCE: Good suggestion, and I think let's do that. We would have to get it ahead of time, and, Jim, that's a very good suggestion. That way, we come to that meeting and are prepared to be able to sit there and discuss.

MR. RINDONE: So that would mean getting your stuff in by October 27, and really no later than November 3.

DR. TOLAN: It's certainly better than Benny's deadline of 10:00 a.m. tomorrow.

CHAIRMAN NANCE: Yes, that is.

MR. RINDONE: No, and Benny still has a 10:00 a.m. tomorrow deadline.

CHAIRMAN NANCE: Okay. Sean.

MR. RINDONE: So October 27, with the final briefing book deadline being November 3 for that November 18 SSC hybrid meeting. Benny, knowing those dates, or Benny et al., knowing those dates, are those -- Is that deadline realistic for what you're being asked to provide?

DR. GALLAWAY: I would like to say that November is really problematic for me, and so it would have to be either sometime in October or after November. I will be unavailable in November.

CHAIRMAN NANCE: Okay. Carrie is messing her hair up, and so I will let her talk.

EXECUTIVE DIRECTOR SIMMONS: Thank you, Mr. Chair. I guess, before we give away that November date, should we perhaps see what happens tomorrow morning with the final Red Snapper Count and see if we still would want to have that November meeting to perhaps go through any other integration scenarios that you may want to consider for catch advice in the near term, after we get through that information tomorrow, in addition to the other fishery-

independent indices you asked for.

I know we didn't have a whole lot of time, and I think it was during the March/April SSC meeting, to go through some of the analyses that I think John Walter and others had done on that, looking at fishing effort on the uncharacterized bottom. I guess just, perhaps, after Benny saying that he might not be able to meet that date, we don't want to completely -- Maybe wait a little bit on that.

CHAIRMAN NANCE: We had that meeting that was scheduled to do something, and I'm not sure we're going to be able to accomplish all of that, but you're saying there's other things that we were going to do at that meeting?

EXECUTIVE DIRECTOR SIMMONS: We still have the final Red Snapper Count, right, to get through tomorrow.

CHAIRMAN NANCE: Yes, we do.

EXECUTIVE DIRECTOR SIMMONS: I don't know if we still want to keep that November date, to look at any of that catch information, or not, I guess, and so I would just see if we want to wait until we get through that tomorrow, perhaps, to make that final recommendation.

MR. RINDONE: I was just going to add to what Dr. Simmons said and just to remind the SSC -- I guess, because there are a couple of new folks on here also, but that, the last time you guys made a catch recommendation for red snapper, you based the overfishing limit on the Great Red Snapper Count and the acceptable biological catch on the NMFS bottom longline survey, and the council has asked that you consider the video survey, the combined video survey, that's being offered for SEDAR 74, along with the revised Great Red Snapper Count numbers, as components of things to look at for considering catch limits, and just so that -- Mapping what you're done versus what's been asked.

CHAIRMAN NANCE: Okay. Now I am -- Sometimes my brain freezes on certain things, but it's -- We're also, in November, going to recommend new catch limits for snapper, potentially, and certainly, if we do that, that's a little while of discussion. Okay.

EXECUTIVE DIRECTOR SIMMONS: I am just suggesting that we wait to give up that date until how we see how tomorrow goes, Mr. Chair. That was all my suggestion was.

CHAIRMAN NANCE: I appreciate that, because I had it where we were doing one thing, but I forgot we were trying to do that other too, and that's a complex discussion. Roy.

DR. CRABTREE: Yes, and there are some of us who weren't on the SSC at that time, and so this will be our first time to see the Great Red Snapper Count material, and I think, tomorrow, there are going to be a lot of questions about the eastern Gulf numbers and the Florida numbers, and those are going to have to be figured out and resolved, and so I think that getting to a new ABC or something in November is going to be very difficult to do. I think there's just too many loose ends and questions and things that are going to need to be figured out, and so I guess we figure out what we think is the most productive way to use our day in November.

CHAIRMAN NANCE: Trevor.

MR. MONCRIEF: I was just going to echo Roy's point, and I agree with that fully. I wasn't on the SSC when it went through, but I was certainly listening in, and that was a very robust and lengthy discussion, and I think going over this information and catch advice in the same day is a tall task, and so I just want to echo what Roy said.

CHAIRMAN NANCE: Thank you for that. Sean.

DR. POWERS: I would like to go back to a point that I was going to make, and Julie made it as well, and it might delay our process even further, but, given that we're going to look more closely into sample design and allocation of sampling in the LGL study, I think it would be great to have the same stat reviewers that we used look at this one as well, and it should be a shorter, quicker task for them, I hope, but Mary Christman was one, and forgot the other gentleman's name who did it, but they provided a lot of very useful information and dove into the sample design and told us the limitations of the design and our extrapolation, and so I would really advocate that we engage them, and maybe not --

Maybe just for a written review and not facetime and all of that other stuff, but just send it to them to review, and I know that's a staff issue, because that involves probably compensating the reviewers and things like that, but I would like to offer that up as a suggestion.

CHAIRMAN NANCE: Sean, thank you. That has been discussed, and so certainly we'll look to the council for that type of discussion.

MR. RINDONE: Thank you, Mr. Chair, and I just wanted to clarify

what you guys are looking at for the Great Red Snapper Count presentation tomorrow, and so, as we mentioned on Monday, that study is done, and so what you guys are seeing tomorrow isn't -- It's not another opportunity like for another tear-down or anything like that.

It's their response to the reviewer comments and the SSC comments and everything, and it's the work that they have completed since that point up to now, and they have published and submitted that report, and so that study is completed. There's not like another rehash of it that's going to happen tomorrow, and so that's what we've detailed in the scope of work, and that's just based on where that project currently stands.

CHAIRMAN NANCE: Okay. Thank you, Ryan. Will.

DR. PATTERSON: I had another comment, but, just responding to what Ryan just stated, peer review is never really done, even after you've published something. Now just more people get to see it, and so, while there may not be another process like we had back in March and early April to go through step-by-step the sample design, the methodology, the estimation processes for the Great Red Snapper Count estimation, we still, as a group, can comment on how that information is used and whether some areas that are stated in the report maybe do or don't reflect the full uncertainty that exists in the data.

I mean, a lot of people can have different opinions beyond just what we as a group expressed in that report, and, I mean, I think all scientists appreciate that that's going to happen, even after something is published, and so the real question is how do we use that information?

We as a group feel like we have sincerely responded to reviewer comments, both from the SSC and from the external peer reviews, and so now this is our response. There is actually one slight change, and, actually, it's not really that slight of a change, but I will leave that for tomorrow, for Greg to go over.

However, you know, it's still -- You know, this body, the SSC, has to utilize that information, perhaps, to make management advice to the council, and so it still is going to be -- We still would need to go through it as a group, especially with the new membership on the SSC, I think, to go over that.

To Carrie's point about maybe we should defer recommendations about having this panel until we get perhaps some more information tomorrow, I'm fine with that approach. The one thing that we're

not going to have by tomorrow is sort of clarification on some of the LGL questions that have been asked here, and Benny kind of hinted at perhaps one path forward, because most of the Louisiana sample, or information, data, came from imputed values from Texas in the Great Red Snapper Count, and now we actually have a process, a project, which a sampling design was created, and sampling was performed, and estimates were produced from Louisiana with Louisiana samples.

The question is do we simply substitute that information, those estimates, into the Gulf-wide estimation that was done in the broader study, and I think we can't quite get to that point by tomorrow, and so it still would speak to having this sort of reconciliation process that I think is needed, whether that's one day or November or whether it's a two or three-day deal.

I was kind of envisioning it being a little bit broader and actually looking at the assessment issues and information, and maybe that won't happen until later into the research track assessment, but, you know, one of the things that has been talked about is the higher estimates in Florida.

Well, you know, when the last snapper assessment was done, using a couple-year-old data, we didn't have FES recalibration, and we didn't have the new release mortality estimates that have come from a couple of different studies that are higher than earlier estimates. There are new approaches to estimating M that weren't around when that assessment was done, and so there's lots of things that could scale productivity in the model.

The second thing is, you know, as Benny pointed out in his presentation, the stereo-camera-estimated size distribution for red snapper in Florida, in the Great Red Snapper Count study, is predominantly small, young fish. Most of those fish, in that first mode that you see in the data there, they weren't alive when the last red snapper data were collected for the previous assessment, and we've seen fishery-independent measures that show an increasing population abundance trends in Florida.

We're not going to get to that level of reconciliation and scratching our heads about does this actually make sense, given other information, unless we have a workshop, where we can actually sit down and look at all of this information and put it together.

CHAIRMAN NANCE: Will, let me ask you this. For looking at that, would that be -- Would you want something like that pre-SEDAR data workshop?

DR. PATTERSON: I think it's needed. I mean, Julie made some great points here, and Trevor as well, about the amount of information. This is the most data-intense, most complicated assessment, in the country, and probably the world, and it's always a full week, when there is a red snapper benchmark, or now research track, assessment, to go through all of the millions and -- Just in these two studies, it's fifteen-million-dollars' worth of research, but there is millions of other dollars that states and the federal government have invested in understanding red snapper population ecology and population connectivity and population dynamics in the intervening years.

There is always so much new information, for this particular species, to go over, and I just don't see how you could put this in addition to the traditional data workshop. It just doesn't seem feasible.

CHAIRMAN NANCE: I agree. I think something in March or something like that, where we could look at the Great Red Snapper numbers, and also the LGL numbers, where we could kind of come to consensus on those and be able to bring that to the data workshop, I think, would be advisable, because it sounds like after the November meeting, but the November part is going to be for something different, but that's something we can think about, for sure, Will. Benny.

DR. GALLAWAY: I'm assuming that, tomorrow, the sampling design for extrapolating the Texas data to Louisiana will also be discussed, and how exactly that was done and considered to be representative for Louisiana.

Our study may have some warts, in terms of sampling design, and we'll see how bad we think those are, and I'm not sure they're that bad, but I would like to emphasize that we took great pains to document everything that we did in great detail, where at least it can be repeated and modified in the future, and I think, again, sitting down in a workshop, to discuss this among investigators, including statisticians and hydroacoustic people, would be the ideal way to go.

It's just finding time to do that in a reasonable manner, and so I am a strong proponent of the workshop approach, and also looking at more detail on how the eighteen million was derived for Louisiana from Texas data, and so that's my comment.

CHAIRMAN NANCE: Thank you, Benny. Doug.

MR. GREGORY: Thank you. I agree with a lot of the recent comments.

The scope of what is needed to be done I think is beyond the purview of the SSC. I think us vetting this, like we're doing, and maybe not participating in the workshop, but getting reports from the workshop, as we're talking would be good, but National Marine Fisheries Service needs to take the lead.

This is not a simple task, just trying to understand these studies, which are monumental, and maybe the first of their kind, in a large ecosystem basis, and the red snapper research track is the more appropriate way to try to flesh all this out, and not the SSC. How are we going to come up with annual catch limits based on a count, one sample or two samples?

I think that's way beyond our purview, and I would even dare say, since we've got another study coming along behind, the amberjack, and maybe another one after that, this is something for the National Academy of Sciences to address. How do you incorporate and integrate these types of studies into a stock assessment that can produce annual catch limits that are safe? Thank you.

CHAIRMAN NANCE: Thank you, Doug. Any other -- John, it looks like you have your hand up.

DR. JOHN WALTER: I do. Good afternoon, everyone, and thanks, Doug, for pointing out that NMFS has to take the lead here. I would say that this is actually a joint process. Coming up with catch advice on something that we've never done before is a team effort, and for the agency --

To put it all on the agency, we are seeking the guidance and wisdom of the SSC in this, and we would like some guidance as to how the SSC might see us using this information, and I think that's going to be a request from us coming out tomorrow, if we get tasked with doing something for the November meeting, and I guess that remains to be seen, but I think it is a -- No stock assessment is done by the agency alone, and that's just been really evident in the process, and so we do seek your guidance and council, as SSC members, as well as all of our other partners, in how best to incorporate this in advice. I think that is what people want, and I don't think people want the agency to act unilaterally on these, and so I hope we can work together on it. Thanks.

CHAIRMAN NANCE: John, we will.

MR. GREGORY: I'm sorry, and I didn't intend to imply that we just kick it to NMFS, but we're doing our part here, and vetting these things further is, I think, our job, and then reviewing other analyses from other people, but I agree with you that this is a

team effort, and it always has been. Thank you.

CHAIRMAN NANCE: I agree with both of you. John, after tomorrow's discussion with Dr. Stunz and his group, we'll be able, I think, to flesh out better the November meeting, what we plan to do there. Any other comments on the LGL study?

Dr. Gallaway and team, I greatly appreciate the time and effort that you have spent in the presentation and talking with us today, and I appreciate all the efforts, and thanks, Scott, for being here. Benny, with that, we'll go ahead, and we're going to take our break now. We'll come back at 3:00, and Ryan and I will decide what we're going to talk about.

DR. GALLAWAY: Thank you.

(Whereupon, a brief recess was taken.)

CHAIRMAN NANCE: We're back online here. It's a little bit later than I thought, but -- What we're going to do is we have some items that we've moved up on the agenda, and we're going to do Item Number XX, which is Review of the SEDAR Schedule, and Item XXI, which is the Interim Analysis Schedule, and then I'm also going to have Other Business, where I have a couple of items that I want to talk about there. Ryan, I will go ahead and turn the time over to you for Number XX.

REVIEW: SEDAR SCHEDULE

MR. RINDONE: Thank you, Mr. Chair. All right, and so gag is an operational assessment, and so we're looking at 2021 here, and so the gag operational assessment, obviously, you guys are wrapping that one up now.

The scamp research track, Dr. Neer sent out the final report from the research track, and she sent out a dissemination email to us, and so that's available on the SEDAR website now, and the red snapper research track is going on from now through 2023. Later this year, FWC will pick up the baton on a mutton snapper benchmark assessment, and Luiz is very excited.

Then, moving into 2022, we have the scamp operational assessment, and so, when we do a research track, it's followed by an operational assessment, and that's where the management advice is expected to be generated. We also have, on the list, an operational assessment for Spanish mackerel, to wipe some dust off of that one. That last assessment is quite dated, and then an operational assessment for gray snapper, and that will start later

that year.

Then Luiz will personally put the icing on top of mutton snapper assessment, and so I'm sure that Shanae would love to hear that, and then, in 2023, we see, hopefully, the red snapper research track wrapping up and the conduction of the subsequent operational assessment, to get management advice out of that, and then we have an operational assessment for yellowedge grouper, and that will also start that year, and so yellowedge grouper is another one of our more dated assessments, and its last terminal year was 2009, and so this will be a good one to get updated. Then FWC will hit the ground running, later in 2023, with west Florida hogfish, which I think, at this point, I think about four or five years old.

In 2024, we propose to have a research track assessment for gray triggerfish, and you guys might recall the last assessment for gray triggerfish was cut short, due to some irreconcilable data issues, and so we're operating still off of SEDAR 43, and so, for a stock that's been in a rebuilding plan, we definitely want to see what's going on there with gray triggerfish.

You guys had looked at an interim analysis for that stock, and it looks like it's improving, and so hopefully this assessment shows some more of the same, and so I guess we'll see. Remembering though that, of course, the research track isn't going to generate management advice, but this will provide a good opportunity for the collective science community to really work on some of the outstanding issues that we've had with assessing triggerfish for a while.

We have a red grouper operational assessment and a vermilion snapper operational assessment listed for 2024, and then we have an open slot here, and so something that Luiz had mentioned, on Monday, was when the next time was that we were going to assess gag. We just did the scope of work for cobia, and is Julie unmuted? I would kind of like her to be able to free-flow into this, too. Julie, are you there?

DR. NEER: Yes, sir, I am.

MR. RINDONE: Good. I just wanted to make sure that you can cut me off when you need to. We've done the scope of work for cobia, and so I don't know if you guys would want to -- If you guys wanted to put gag on this current schedule, one option might be to talk about moving cobia up to like 2024 and then put gag in 2025.

The reason why I say that is because of the approval timelines that we work off of for SEDAR, for getting scopes of work in and

approved, and it has a lot to do with being able to provide the Center especially with enough time to be able to set up getting all the data in place and the staff workloads and everything else, and so I think trying to plug gag in too early would be difficult to do inside of that framework. Mr. Chair, I see Will has his hand up.

CHAIRMAN NANCE: Will.

DR. PATTERSON: Thanks, Jim. In our discussions yesterday, we talked about gag and the timeline, and it's been a while since there's been a benchmark assessment for gag, and I know that the research track timelines are a little bit more drawn out, and they're not meant to produce management advice, but it just seems like, for one of the marquee species in the Gulf, and there's a lot of uncertainty and a lot of issues about its life history and how do we effectively assess and manage a fish that has its life history, if there is an opening for an assessment, could that be -- Could a research track assessment be accommodated for gag? I mean, it seems like we're overdue for that.

MR. RINDONE: Well, we're in the proposed part of the schedule, technically, and so, I mean, it's something that could be put on there. I would say that it couldn't be put on for 2024, but perhaps in 2025, and we are positioned, though we would have to move kind of quickly about it, to be able to move cobia up to 2024, since we have an accepted scope of work from the SSC, and so I think that meets our timing deadlines, as long as I get that in by November 1.

CHAIRMAN NANCE: Let me ask this question. Right now, we have an open slot in 2024?

MR. RINDONE: Yes.

CHAIRMAN NANCE: Okay.

DR. NEER: I just want to note that the guidance we were provided from the Science Center is to have no more than two research tracks operating region-wide, and we are far above that, and so the research track for gray trigger is starting in 2024, and it will role extensively into 2025, and so I agree with Ryan that there is probably no way that you could get gag in 2024 without bumping triggerfish.

It would have to certainly be up for discussion whether the Center could accommodate you guys having two research tracks underway at the same time, and one would be in data and one would be in

assessment, and so it's possible, and the data is always the bottleneck still, currently, and so 2025, I would agree with Ryan, would be probably the earliest that you could get a gag research track underway, unless you were willing to suggest moving triggerfish. Yes, I would -- Since you guys are ready for cobia, and you have approved that scope of work, Ryan could certainly put that up and make that up for consideration for 2024, as an operational.

CHAIRMAN NANCE: Okay. Katie.

DR. SIEGFRIED: Thank you, Mr. Chair. I just wanted to throw in something that we're going to be discussing hopefully at the SEDAR Steering Committee meeting, is the request from the council for the tilefish data triage. That is going to need to be -- I don't know if that's what the tilefish complex evaluation is or if that's the assessment based on that data triage. Julie, do you know?

DR. NEER: I would punt to Ryan, but I think, since it says, "evaluation", that that's what they're considering to do the triage part, but Ryan would know better than me.

MR. RINDONE: Yes, and so we were asked to -- The SSC asked for a letter to be written to the Science Center to talk about the -- To look into the data triage, to see like what was going to be possible, and is it going to be possible to assess these different species independently, or was it still better to assess them all together.

We got a response back from the Science Center that said that the Science Center had considered our request for a data triage for Gulf tilefish and said that, because it's not a routine data extraction, and it's essentially a stock identification process, that they were not able to accommodate that request and that they thought that it would be best to do it as a research track, with one slot required for each species, and there are three tilefish species, and so that would be three assessment slots that they would want required to do that. That would, obviously, be very taxing on the council's SEDAR schedule and its assessment needs.

DR. SIEGFRIED: Thanks, Ryan, and so I wanted to ask about that, so that we could better evaluate the schedule, because that was my understanding.

MR. RINDONE: Well, and I think part of what we were looking at was trying to figure out what to do with tilefish, because, the last time around, the assessment wasn't used for management, and we ended up going with Tier 3 of the control rule and average catch

with standard deviation.

I don't know that that data condition has improved dramatically enough to result in an opportunity to fold something as data-poor as tilefish into something as hungry as an age-structured model, but perhaps something a little bit more rudimentary could have been explored, and so that was one of the things that the council was thinking about with respect to this tilefish complex evaluation. Trevor.

MR. MONCRIEF: I guess maybe I was a little bit confused when we said data triage, and I really thought that what we might have been getting was just a list of available data sources and how data-rich they were, and I didn't think it was going to be too complex of an evaluation, but just kind of looking at what was available and what could possibly be gleaned from it.

CHAIRMAN NANCE: Carrie.

EXECUTIVE DIRECTOR SIMMONS: Thank you, Mr. Chair. I guess that was kind of what we were hoping as well, but it sounds like that's something that maybe our staff need to do instead, because I think tilefish was on the 2024 accepted schedule, and so I haven't had a chance to talk to council leadership about the letter just yet, but I would gander to estimate, assume, that they're not going to want to fill up that many slots for tilefish, and so probably what we'll do is staff will just work on a statement of work for golden tilefish, and we'll try to come up with what we think is available data-wise, and we'll try to get that to the SSC as soon as possible for 2025.

CHAIRMAN NANCE: Okay. Roy.

DR. CRABTREE: Ryan, I see a greater amberjack operational assessment in 2025, but I don't see a research track anywhere. Is there an update of the -- Was it 70?

MR. RINDONE: Yes. Again, one of the situations that we're running into is that, for a lot of the issues that are identified with some of these assessments, we're being told that that needs to go through a research track assessment. A research track can take a shorter amount of time, but, given the scope of issues with some of these species, it's a little naïve, I think, to think that we would be able to get through a research track process in a matter of months for any of our more often assessed and discussed species.

All of these things are going to be very resource intensive, in terms of time, people, et cetera, and so we have to think pretty

critically about when we're going to ask for what. The council has been pretty adamant about having gray triggerfish on the schedule and getting that one dealt with, especially since the last assessment was not completed, and, of course, we, obviously, have recognized needs for doing some similar work for greater amberjack and for gag and for red grouper, frankly, but we can't do all of these things at the same time.

Some prioritization will be necessary, and, in the interim, we can use things like interim analyses to heat-check how a stock might be doing. If it's appropriate, you guys can always recommend updated catch limits, based on those interim analyses, and so it's not that we're leaving you guys, or the council, completely in the dark either.

CHAIRMAN NANCE: Okay. Carrie.

EXECUTIVE DIRECTOR SIMMONS: Thank you, Mr. Chair. Just the other thing to think about is that greater amberjack operational assessment -- I thought we were looking at that for a research track, because that's when the greater amberjack abundance study might be available, and so I'm not sure that actually is correct, but, that being said, we can't have more than two research tracks in the region, and we may need to think about that some more. I'm not sure when the results of the amberjack study are going to be available.

MR. RINDONE: To that point, Mr. Chair, I was going to say that I didn't have that listed as a research track, because, under the rules for an operational assessment, we can include new data, and so, if that study is available at that time, presumptively, it's a new data stream, and so that's why I had it listed as that, but, if we're going to have to have a research track to include new data, then, obviously, that's going to be -- That's going to make a lot more things difficult.

CHAIRMAN NANCE: Julie, to that point?

DR. NEER: Yes, Mr. Chair. With regard to specifically that point, with regard to greater amberjack, we do have it currently listed on the SEDAR planning schedule as operational/research track, because it's also going to come to the South Atlantic in the same way, and we're hoping to get some more guidance on what sort of products might be available for inclusion before we have to finalize what type that will be in 2025.

Ryan is correct that certain things can be added in an operational, and you don't have to do a research track, and it really depends

on the volume and the datasets itself, and so 2025 won't be finalized until 2022, and so we have a little bit of time to see how things play out with that project and then determine whether it will be an operational or a research track.

CHAIRMAN NANCE: Okay. Thank you. Doug.

MR. GREGORY: Thank you. I think we need to take a look at -- You might recall, some of you all, back ten or twelve years ago, we created some data for ACLs, by taking the average of ten years of landings. Just recently, one of those groups of species was closed, because that ACL had been exceeded, and I'm talking about blackfin snapper, queen snapper, silk snapper, and wenchman, as a group.

It would probably behoove us to take another look at that, and I don't think that it needs to be an assessment, or take up an assessment spot, but it's something that the SSC can do, like we did earlier, and we probably have some indices now, like the video index, and maybe the longline index, that have these species in it.

If we had indices, that would better inform the way we might want to change the ACL from what we did ten or twelve years ago. This is the first time one of these data-poor -- I forgot what we called them, but complexes, I think, has had a closure, but I don't think it requires a stock assessment spot with NMFS.

CHAIRMAN NANCE: That's a good suggestion, Doug. Thank you. Shannon.

DR. CALAY: Thank you, Chair. I wanted to make essentially the same intervention that Julie did, and so I will just emphasize that it's not having new data that triggers a research track stock assessment. A new data input can be considered in the operational assessment with a term of reference, essentially.

What triggers a research track assessment, for example, is a major restructuring of a stock assessment model, such as that that would happen if we get new information about stock ID or the species that might be represented in a complex. That's what triggers a research track stock assessment.

To Doug's point, I do think there is utility in considering some data-limited approaches. We did recently have success in the Caribbean, using a very data-limited version of Stock Synthesis, which only required catch information, length composition data, live history, and that's it, pretty much, and so there may be

utility in reconsidering doing some unassessed species through data-limited processes, but I would say that that would take up a stock assessment slot, and we might be able to do a few stocks, but not an extraordinary number, because there is still the issue of data triage aspects to consider, even though the analytical processes aren't as complicated.

CHAIRMAN NANCE: Okay. Thank you. Jason.

MR. ADRIANCE: Thank you, Mr. Chair. I was actually going to wait until Other Business, but, since Doug brought up the mid-water snappers, I'm wondering -- Prior to this, I think we had only seen that complex reach 60 percent in recent years, maybe, and I'm wondering if, along with when we look at it, or if we put it on an agenda, but if we can get some sort of presentation, on how those landings are calculated in recent years, and what's the imputation level, the QA/QC involved with those landings, something along those lines, to help us see what caused that dramatic shift in catch there as well. Thank you.

CHAIRMAN NANCE: John.

DR. FROESCHKE: Just as a follow-up to Doug's comment about the mid-water snapper complex and the closures, that is something that -- Council staff has been looking at this, and we are interested in this, and this has come up for a number of -- Well, at least a few other species, like lane snapper and perhaps some other ones, and I think what we've talked about is give us a little time to go through and look at the wave data, and just see if there's a sampling issue, or an anomaly, and perhaps we could bring that back in a presentation to the SSC to look at before we take up a SEDAR slot for these stocks.

CHAIRMAN NANCE: I think we could certainly do that. I think that would be a good way out of it, for sure. Doug.

MR. GREGORY: That's good, and I'm glad to hear that people are interested. I have learned that, on the commercial side, there are a couple of young guys who just started targeting these fish, I think probably because there is no IFQ and that's something they can do, and so that has definitely increased the commercial fishery, if that is true. Recreational, I really don't know.

CHAIRMAN NANCE: That would be something to look at for that meeting.

MR. RINDONE: I was just going to say, if you give me a second, I can probably look that up.

CHAIRMAN NANCE: Okay. We'll let Ryan go and look that up.

MR. RINDONE: Well, I'm kind of doing it as I'm talking.

CHAIRMAN NANCE: I know.

MR. RINDONE: For gag, what do you guys want to do?

CHAIRMAN NANCE: I think it would be good to move up cobia and put gag in 2025. That would be my suggestion. Paul.

DR. MICKLE: Jim, thank you. To that point, I have emailed a motion. Listening to the discussion and everything, and really understanding the justifications for moving up one versus the other, and my reservations with cobia and the status and what we're doing with them now, and we're understanding it, and it just -- There is the motion there.

I think I got everything that I heard in the discussions, and I may have just messed up, and I think we had made the strongest discussions toward gag being a benchmark in 2025, and was that it, or I may be -- Was that the research track, but there was that discussion on the other side of having too many research tracks in that year. **This is the motion that I'm putting up for discussion. I** would like to get a second, and then we can tackle the benchmark versus research track after that point. Thank you.

CHAIRMAN NANCE: Okay. Thank you, Paul. Anybody second that?

DR. BARBIERI: I will second it.

CHAIRMAN NANCE: Okay. Luiz seconds. It's up for discussion.

MR. RINDONE: Mr. Chair, just a nomenclature thing, and so FWC is the only one that still uses the benchmark framework for doing assessments, and so, since this would be an assessment done by the Science Center, it would be considered a research track.

CHAIRMAN NANCE: Yes.

MR. RINDONE: So, Jess, if you would just change "benchmark" to "research track".

DR. BARBIERI: If I may, Mr. Chairman, real quickly, a clarification here. Ryan, whether that could be, based on what Julie said, and then what Shannon said as well, and it could actually be considered an operational assessment, depending on how

we define the terms of reference.

CHAIRMAN NANCE: That probably would be an easier push into it. Paul and then Carrie.

DR. MICKLE: I am just joking when I say this, but we could just make FWC do it, because they catch most of them over there. Then you decide what to do, and we don't care.

MR. RINDONE: Is that Dustin's hand that I saw?

CHAIRMAN NANCE: Carrie.

EXECUTIVE DIRECTOR SIMMONS: That's a tough one to follow. The cobia operational assessment being moved up, I think we've talked about this before and I think it's okay, but just keep in mind that the council is taking, hopefully, final action, or councils, in the end of this year, on 2021, and so implemented in 2022, and so you might -- You will get that size change that we didn't get last time, but the new measures would not be realized, I wouldn't think, in this assessment. Whether you want that other year or not would be the only -- Other than that, I'm not sure what other new data we have on cobia, and so we might also consider that as well, and I don't know if there's any other tagging information that would be worthwhile to push it up as well.

CHAIRMAN NANCE: Thank you, Carrie. Julie.

DR. NEER: Two things. Number one, if you guys believe a research track is the way to go for gag, that's what you should request, and the Science Center will look at their overall workload and see if they can accommodate two of them running consecutively, and that's not for us to say, and you should recommend what you want.

With regard to gag, given everything you guys just said and all of the things that were presented, and Lisa presented, I would argue that you shouldn't probably do an operational, because there were quite a few things that Lisa wanted to do in this operational, but were just beyond the scope of what the Science Center was comfortable doing in an operational. Some of them were quite technical, and the recommendation from the Science Center was to do, I believe, a research track next for that species.

I would consider that feedback as well in what you recommend, and then, Ryan, if you could switch real quickly back to your screen, and I just wanted out that now we have a dedicated Caribbean team, and so you no longer have five slots, and you have four of analytic spots, and you don't have five anymore, and so, for 2025, you have

four slots and not five that you currently have listed, and, moving forward, you will have four, because the Caribbean Branch --

The Caribbean Branch used to be Gulf and Caribbean, and now it's a Gulf and a separate Caribbean, and so you have four spots and not five, analytical spots, open for your planning, just so, moving forward, that's how we've adjusted for it, unless Shannon is going to tell me that I'm wrong, perhaps, because she just raised her hand, but that was our understanding at SEDAR.

CHAIRMAN NANCE: Shannon, to that point?

DR. CALAY: Thank you, Chair. That is actually news to me.

CHAIRMAN NANCE: Good. That's good.

DR. CALAY: I mean, we do have, right now, a few bottlenecks that we're struggling with, to be honest, and the bottlenecks are that our data provision group is quite small, but we are backfilling a number of positions, pretty much as we speak, and I think we can discuss this further in the SEDAR Steering Committee, but I don't think that's necessarily a long-term reality. I think we might be able to -- We'll come to a final decision at the SEDAR Steering Committee, but I think we might be able to do the five slots without trouble.

CHAIRMAN NANCE: Okay. Thank you. We'll leave it like this for now then. Jim.

DR. TOLAN: Thank you, Mr. Chairman. My comment has to do with the cobia operational assessment, and I just wanted to make sure, because the last involvement I had with cobia was a joint assessment between us and the South Atlantic, and I wanted to make sure that this was just a Gulf assessment that's on the table right now. Thank you.

MR. RINDONE: Thanks, Jim. Yes, this will be just for the Gulf migratory group. If you really want to go to Charleston, I'm sure we can figure out a way to get you there though.

DR. TOLAN: That would mean flying, and that's not going to happen, but thank you, Ryan, for that clarification.

CHAIRMAN NANCE: It would be a good drive, Jim. Let's see. Will.

DR. PATTERSON: Not to belabor this too much, but I agree with the sentiment that expressed that the next assessment for gag should be a research track. Lisa was unable to do some explorations, as

she indicated, that she would have had more freedom to do under a research track, and we talked quite a bit about sex change and these protogynous groupers, and we often talk quite a bit about sex change and implications and uncertainty there, and so I think that could be explored a bit more in a research track setting.

Then, also, in this meeting, we've talked about integrating the ecosystem modeling to examine red tide effects and some statements about how do we actually do that in the future, and there are MSE implications that we've talked about, like do we project forward assuming that red tide is going to occur periodically, and, if so, what scale, and how do we account for that.

It just seems to me that there's lots of things here that gag -- That it would be quite useful to be able to explore that, and, still, we're talking about four years from now, and so I imagine the issues around gag and other groupers are only going to become more prevalent in the next four years.

CHAIRMAN NANCE: Yes. Okay. Any other discussion? Let's go ahead and put the motion up. **The motion is the SSC recommends to the council that the cobia operational assessment currently scheduled in 2025 be moved to 2024 and a gag research track be added in 2025. Is there any opposition to this motion? With none indicated, this motion carries with no opposition.** Paul.

DR. MICKLE: Ryan, refresh some of the folks that are new to the SSC and that are on the phone about -- Has a research track been completed yet with any species to this point to fruition?

MR. RINDONE: Scamp.

DR. MICKLE: Scamp is in its final throes, or it's done?

MR. RINDONE: The stock assessment report for the research track portion of that assessment has been completed for Gulf scamp and is published, and so it is done.

DR. MICKLE: Okay. Well, that's good, and I think we should all commend SEDAR and the Southeast Fisheries Science Center for taking on this new approach, and I know that Clay pushed it real hard, when it started, and I think it's starting to show its fruits, and, after red snapper, gag in 2024 is going to seem easy, or 2025. All research tracks after red snapper will seem easy.

MR. RINDONE: I mean, this is a new process, and there's going to be a lot of teeth cutting, and especially -- You know, it's good that we got to do scamp first and not doing red snapper first,

obviously, and I don't think anyone has any presumptions that red snapper is going to be easy, obviously, and scamp was made more difficult by COVID, which definitely interrupted some things, but everyone did do a good job trying to adapt and move forward, especially in an almost exclusively virtual environment.

If we can go back to the schedule, we're moving cobia to 2024, and we're putting a gag research track in for 2025, and so, as recommended to the council, and I am going to update this schedule, an then the council can make edits to it as they please, and this is kind of a living document, and then it will allow Dr. Simmons and Mr. Diaz to take this to the SEDAR Steering Committee, and then they know what they want when they're having those discussions and negotiations with SEDAR.

Other things that we had on the schedule for 2025 were the research track continuing, and hopefully wrapping up, for gray triggerfish and the lane snapper operational assessment, and we'll now have the gag research track, and then the greater amberjack operational assessment, which may also have some research-track-ish things to it, depending on what comes out of the Greater Amberjack Count.

At this point, I don't think that there's much to change, as far as the greater amberjack, on this part of the schedule. The thing that definitely requires some discussion from you guys is going to be what to do about tilefish.

As Dr. Simmons and I have both separately said, having three research track slots for three different tilefish species is probably less than palatable, and not just for the council, but for this body, because of the other science needs for other species, and so -- As Dr. Simmons said, council staff can work on trying to figure out the data that are available for tilefish. As Dr. Calay said, for certain species, there may be options for doing things that are -- Even in SS, that are less data intensive.

I think, given the data condition of the tilefish complex, that probably wouldn't be a bad thing to consider, and so, at this point, tilefish is kind of a question-mark, I think, given what we know. Trevor.

MR. MONCRIEF: I mean, I would agree with those sentiments. It's definitely a question-mark. I think, before I would feel comfortable putting anything definite on the schedule, or saying what should or shouldn't be done, I still would like to see what is actually available for the species and what is the quality that it's in.

If that's a separate endeavor with what we're talking about with the evaluation, it is what it is, and I'm not trying to put a large workload on any one person or bog down the system, but I would think that just taking a cursory look at what's available would kind of give us a better idea of how to move forward.

MR. RINDONE: Yes, and one area that we have that we can source for information is the SEDAR 58 assessment of tilefish in the South Atlantic, which had a stock ID portion to it, and so there was some examination of especially tilefish on the West Florida Shelf and trying to discern connectivity between the Gulf and the Atlantic. We can certainly peruse those data, to see if there's anything there that could be contributory to looking at the Gulf stocks, and so it looks like you have Dr. Walter.

CHAIRMAN NANCE: John.

DR. WALTER: Thanks very much I just wanted to take this opportunity to roll out one of our data provision initiatives, and I was about to send this out to the council. As part of our realignment and restructuring and streamlining, we're being able to more readily access a lot of our basic databases, and one of the things that we can do is provide access to council staff to directly query the databases, which then allows them to do some of these analyses a lot more rapidly than to have to ask us to do them, and I think it probably will streamline some of these things, so that it will be immediately obvious whether the species are separated or not.

I hope that that could help here, so that we don't wind up getting caught in thinking that we have to rearrange the schedule, when it turns out that you can't separate two different species of tilefish to the landings, and the same for like deepwater snappers, and so I just wanted to raise that as a positive development that I think that, once we get that program sorted out and online, it should help us here, and so thanks.

REVIEW: INTERIM ANALYSIS SCHEDULE

CHAIRMAN NANCE: Thank you very much, John. Let's go ahead and move on to XXI, which is the interim analysis schedule. Jessica, if you would bring that up, please. Here is the interim analysis schedule that we have for 2021.

MR. RINDONE: 2021 interim analyses are completed. For 2022, we anticipate getting a red grouper interim analysis, and not at the January meeting, but at probably the March meeting, the reason being, in order to include the fall groundfish survey, we need to

have a little bit more time for the data processing to occur for that survey, frankly, it won't be ready in time to be able to complete an interim analysis and have it to you.

I had reached out to Dr. Siegfried, and she talked with the folks at Pascagoula, who are the ones that collect and sort through all of that, and that cruise will be returning in the mid to late fall, and they usually need two or three months to be able to work up all of the data that they collect, and so it's too narrow of a window, and it simply wouldn't be ready in time. That one will just have to be pushed off to that March meeting, and so a little bit of a delay, but not a terrible delay, I don't think.

Then we have greater amberjack, looking at the combined video survey and doing an interim analysis for that, and then king mackerel, and we haven't had interim analyses for greater amberjack or king mackerel, and so it will be interesting to see if those are able to be done in that way.

Then, for 2023, that one is pretty intensive, and we currently have listed red grouper, again with the NMFS bottom longline. Again, if we have the SEAMAP groundfish survey tied into that, again, then that pushes that from January to March, and I will update this to be reflective of all of that.

Gray triggerfish, looking at the combined video, and we don't know what would be used necessarily for gag, but I figure that, during that gap year between now and when we get the research track going, we probably should have at least a heat-check for gag and see how things are going, and 2023 is likely to be the first year of management for anything that follows from SEDAR 72.

CHAIRMAN NANCE: Let me ask you this real quick. Is there anything proposed for gag as an index?

MR. RINDONE: It depends on what data we think would be useful in supporting that, and so, right now, there is the Panama City and Pascagoula Lab surveys that were done for a fishery-independent index, but there aren't an awful lot of samples that were taken from those video surveys for gag specifically, and so I don't -- I dare not say exactly, but I would doubt that those would be super informative for that. Someone from the Science Center might want to chirp up.

There is also the potential to use a CPUE indices, but that would need to be explored, and so, right now, I think a question-mark is kind of where we are, until we get some advice from the Science Center about how we might be able to proceed with gag.

We have used the NMFS bottom longline in the past for red snapper, and our ABC that you guys recently recommended is informed by the NMFS bottom longline survey presently, and then the headboat CPUE index, as an example of the CPUE index being used to inform catches, is what is currently used for lane snapper, and so we're asking for that again, because it's been a little while for lane snapper, and, especially amongst the headboat fleet, that species has gotten more popular, and we're starting to see more private recreational landings of lane snapper, also. They appear to be doing pretty well on the West Florida Shelf and they're fun to catch.

Then, in 2024, again, annual for red grouper and again for gray triggerfish, greater amberjack, and king mackerel. Where we have these asterisks next to these different species, we have them considered as health-checks. However, if the stock is behaving in such a way that the SSC thinks it prudent to recommend revised catch advice, you guys could certainly do that. Is there any questions on our interim analysis schedule? Again, this will be updated to reflect, at least for 2022, and, instead of that being delivered in January of 2022, it will be March of 2022.

OTHER BUSINESS

CHAIRMAN NANCE: Okay. Thank you. Any discussion on this? Okay. I have one item of business. Under Other Business, I had something else to bring up, and so I will bring it up now. Back I guess a year ago, or two years ago, there was the 7th National Meeting of the Science Coordination Sub-Committee, and the abbreviation is SCF7.

It was going to be in Sitka, Alaska, and I think some of the SSC members were going to that, and that was cancelled with COVID, but it is still going on, and it's slated to be in 2022. One of the topics that they're looking for is how to incorporate ecosystem indicators into the stock assessment process.

They're looking for examples that have been used in our region, and I do think we have some great ones, and so what I would propose is -- Ryan, you have the email that has that link on it, and, if not, I can send it to you again, and what I would like is -- Ryan will send that out to the SSC. If you have any stock assessment processes that have ecosystem indicators, like red tide, for example, I do think that we want to put some things in. I would recommend that, we as the SSC and the Southeast Fisheries group, put in some and propose to be able to talk about that at this meeting. Is there any questions on that? Roy.

DR. CRABTREE: When is it, Jim?

CHAIRMAN NANCE: I think it's -- I don't know exactly, but I think it's 2022.

DR. CRABTREE: It's in Alaska?

CHAIRMAN NANCE: I think it's going to still be in Alaska. It's being coordinated by the North Pacific Fishery Management Council.

MR. RINDONE: The meeting is going to be in August of 2022, and I don't think the dates are solidified yet.

CHAIRMAN NANCE: Okay, but it's being -- I think it's still Sitka. Will, I think you were going to this, if I'm not mistaken.

DR. PATTERSON: I think I was on the list, and I think Chagaris was too.

CHAIRMAN NANCE: Yes, Dave, but I think it's things like that, and Dave certainly -- I think the stuff he's doing would be greatly applicable to this, and so, when Ryan sends that out, please take a look at that, and let's submit some information so that they can consider it to be presented at this meeting.

MR. RINDONE: I would just add that submit your ideas to Dr. Nance and myself, so that we can put those ideas together, so that they don't have an additional unplanned twenty-some-odd people putting things on the Google form.

CHAIRMAN NANCE: I think that's good, and I think Ryan is absolutely right, but I am still going to send you the link. Don't use it to submit, but use it to look at what is being needed, so that then you can put that information -- Send that back to Ryan and I, and we can submit on that link to that process. David, please.

DR. GRIFFITH: Thank you. Just to be clear, you want us to send you articles and things like that?

CHAIRMAN NANCE: No, no articles, but it's basically have we done research. Is there ongoing research where, for example, an assessment is using ecosystem-type data in that assessment, and that's what they are interested in seeing. What I will do is, when Ryan sends it out -- It gives an informational about what's going on, and I think that will be better than what I presented here. Then you can use that link just to check and see what

information they are needing, and you can send that information to Ryan and I, and we will be happy to submit that for you. Jim.

DR. TOLAN: Thank you, Mr. Chairman. I believe I was on that original list also, when it got cancelled for COVID.

CHAIRMAN NANCE: Yes, I think so.

DR. TOLAN: I will gladly free up my spot for anybody who wants to, given my travel restrictions, if the situation still exists come August, but I will gladly give up that spot to somebody else on the SSC. Thank you.

CHAIRMAN NANCE: Thank you. I think it's August of 2022.

DR. TOLAN: I don't foresee any of the restrictions easing up between now and then.

CHAIRMAN NANCE: Okay. Well, I hope they do though, Jim. It would be nice to see you face-to-face again.

DR. TOLAN: I would love to see all the rest of the SSC, but, if I have to wear a mask, I ain't flying.

CHAIRMAN NANCE: I know. I understand. Hopefully it will be fixed by then.

DR. TOLAN: Thank you.

CHAIRMAN NANCE: Any other comments or questions? That is all I have for today. Let me remind us of what we have tomorrow. At 8:30, we're going to have Dr. Gordon from the Southeast Fisheries Science Center, and he's going to give a presentation on using field experiments to assess alternative mechanisms for distributing fish to the recreational sector.

Then we have the review of the finalized Great Red Snapper Count report with Dr. Stunz and his team, and we have a presentation by David Dale on the essential fish habitat consultation process, and we have our greater amberjack projections and OFL and ABC recommendations and gag grouper projections and OFL recommendations. Those are the things that we have on the schedule for tomorrow.

I am looking at probably a little after -- I think we're going to have to have lunch tomorrow, Carrie. We'll have regular lunch, and then we'll be able to get through this, hopefully, and Jessica wants to be out of here quick, and so I think that's good. Anyway,

that's the schedule for tomorrow, and so come prepared to be able to do this, and I greatly appreciate each of you, and I think it was a great meeting today, and the input and so forth was very productive.

(Whereupon, the meeting recessed on September 29, 2021.)

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September 30, 2021

THURSDAY MORNING SESSION

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The Meeting of the Gulf of Mexico Fishery Management Council Standing and Special Reef Fish, Special Socioeconomic & Special Ecosystem Scientific and Statistical Committees reconvened on Thursday morning, September 30, 2021, and was called to order by Chairman Jim Nance.

CHAIRMAN NANCE: Good morning, everyone. We're ready to start our SSC meeting this morning, and we are going to start out with Dr. Gordon's presentation. Dr. Gordon, I appreciate your willingness to shift to this later time, and I thank you for doing that. His presentation is using field experiments to assess alternative mechanisms for distributing fish to the recreational sector. Dr. Gordon, you have the time.

PRESENTATION: USING FIELD EXPERIMENTS TO ASSESS ALTERNATIVE MECHANISMS FOR DISTRIBUTING FISH TO THE RECREATIONAL SECTOR

DR. ALEXANDER GORDON: Thank you, and good morning, everyone. I am an economist working with the Social Science Research Group at the Southeast Science Center, and so I'm going to discuss an experiment that we've been designing for the private recreational sector of the Gulf reef fishery.

The goal of this experiment is to assess the feasibility and desirability of alternative management tools intended to provide anglers with more flexibility and fishing opportunity, while continuing to constrain catch within the given allocations.

Before moving on, to provide a sense of the outline for the talk, the first couple of slides are going to briefly cover the motivation for trialing alternative management tools and then proceed immediately into outlining one -- Our typical example of the sort of experiment under consideration, and then I will discuss

the prospect of evaluating and potentially expanding the program, and I will highlight some open issues, and, finally, I will discuss our current status and next steps.

I will start with the motivation for this work, and let me first clarify a term on the first bullet point here. We currently distribute quota to the rec sector with season lengths, bag limits, and size limits, and so the emphasis here is on the first verb in this bullet point, "distributing", and where, by distribute, I mean what rules are in place, how does the management system regulate anglers' interactions with the stock, to keep catch below the catch limit.

The quota, the pounds available to the sector as a result of the kind of technopolitical decisions made about stock parameters and sector allocation ratios, that's all being taken as given, and the issue then is how do you regulate anglers, to maintain that catch limit?

As the slide says, currently, we do that by telling people that, hey, you can take X fish out of the water per day on these particular days out of the year, and, under such a system for distributing fish, when fishing pressure increases, as it has in the Southeast, decade over decade, the only options are to reduce bag limits or shorten seasons, and, particularly, when bag limits are already kind of low, one or two fish, as they are, then the only policy lever you have left is setting the season, and that can be done, and is done, largely successfully, at least if we're defining success in terms of constraining the catch below the catch limit, but it's less clear that that management system is succeeding with regard to delivering the greatest possible benefits to anglers for a given catch limit.

The principal reason for that being you're using a one-size-fits-all approach to regulating what is actually a very heterogeneous group of anglers. In other words, what might be an ideal season for one angler, one person, might be very inconvenient for another, in terms of the value per trip that they are getting from the way you've set the season there.

The question is then can you have a more responsive policy, a policy that responds to input from anglers to deliver greater benefits to them while maintaining that catch limit, and the answer, right now, is we don't know.

There is some reason for optimism, and so that's what I have outlined in the last bullet, that communications, smartphones in particular, should make it possible to implement something like

that, a responsive policy, in a way that wasn't feasible say ten years ago, but there is also formidable challenges, which I'm sure we'll get into, and, hence, we don't know, and, hence, let's try it out on a small scale and find out, through experiments.

There is one more thing, before describing the program, and I would like to mention as well that two National Academy of Science reports recently wrapped up regarding recreational fisheries, one of which is particularly relevant here on the subject of limited access privilege programs, or LAPPs, in mixed-used fisheries. For the Gulf reef fisheries, the commercial sector already has LAPPs, in the form of the various IFQ programs, while the recreational sector has no comparable management tools.

One of the primary recommendations from this report, that I have excerpted here, is that, in such cases, the management system should propose and implement reforms, including harvest tags or day passes, for private anglers that foster accountability, while enhancing fishing experiences and opportunities to heterogeneous groups of anglers, which is very much in line with what I will be sketching out here.

The proposed pilot program is a one-year pilot in which anglers would be allotted day passes, allowing them to target and retain a particular species outside of its regular season, and, as a condition of participation in this program, they would agree to certain data provision requirements that serve both to implement the program and to provide useful data on angler behavior that could inform future policies on it.

To quickly address a couple of important program design considerations, let me mention that, one, the passes are to be allocated to interested anglers using a lottery, which we see as a good compromise between fairness and practicability. Two, that anglers will not be exempted from any other regulations, aside from just these seasonal closures, as part of this program. In particular, they would still be subject to the usual bag limit and gear limitations and anything like that, and, three, we intend that the data to be collected would be like trip log style data submitted via a smartphone app.

Precisely what information will be included on those trip reports is, I think, open to discussion. At a minimum, it would include the number of the target species caught and harvested on a trip, and you might ask questions like bycatch, or you might also ask for information on trips that weren't target trips for the program species, et cetera. Like I said, that's kind of an open question, how important those things are versus the reporting burden that it

adds to anglers.

Then another thing to draw attention to is it should be possible to design and implement this program in such a way that it doesn't have a significant downside for the people who aren't interested in participating or who don't win that lottery, and that primarily means that the fishing mortality attributable to the program doesn't require a compensating reduction, a shortening, of the regular season, and that should be particularly achievable, since it's a pilot program, and, to give you a sense of scope, which I don't think I've mentioned yet, we're imagining this could be fielded with maybe a few hundred participants, and so if that gives you a sense of the scope we're talking about.

So there's the question of how do you like fund, in terms of like quota the program, and one approach that's been discussed is to use some of this quota which is currently like administratively retained by NMFS, and another is that, in the case that the catch limits are set to increase in a particular year, that you set some of that increase towards establishing a program like this, rather than having all of that increase in the catch limit accruing towards expanding the season. With all that said, let me move into just a diagrammatic exposition of how what I'm proposing would work.

You can see here that most of this diagram is grayed-out on this slide, and so I'm going to kind of step through this piece-by-piece over multiple slides, focusing on different parts of the program. If you first direct your attention to the bottom here, you will see that time is running along the horizontal access, and I marked out three periods of the year, and so, on the left, there is the period before the season would open, the pre-season period, followed by the regular season, followed by the remainder of the year after the season is closed, the post-season, and so right above that there is this line segment with four red dots.

That represents the trips being taken by an angler over the course of the season, and so each red dot there is a trip, or a day of fishing, and, above that, you see these dotted lines going down to some, but not all, of the trips, and one of those lines is labeled "MRIP", and they are connecting the trips to a big bubble labeled "NMFS", and so these represent the surveys currently used to monitor the fishery, and, to be clear, I write "NMFS", but I sort of mean the fishery management system as a whole, and some of these surveys are done by the states now, and things like that, and so it's just kind of a shorthand for the whole management system.

To the left, you see this box with this big jumble of adjectives,

and that's representing the anglers who participate in the fishery, and the adjectives here convey that what I am -- What I am covering here applies to everyone, and so the season length is the same for everyone, and the same monitoring provisions apply to everyone, and we're still in like the one-size-fits-all world here.

To recap, this slide essentially represents like the status quo of how the fishery is managed, by setting a season that applies to everyone and capturing information on a subset of the trips, to monitor harvest, and that, in turn, informs how future seasons are set and so on.

On this slide, and over the next couple of slides, I am going to move into describing this pilot program, and the first step is you announce the program and explain to people the deal, which is represented at the top here, and so you tell people that there will be this opportunity to be awarded these day passes for out-of-season harvest of the program species.

Here, I have illustrated two trips in the post-season period, on the top-right there, and you explain to them that the other part of the deal is that they will be expected to directly submit this information to NMFS, and, again, that's both so we can operate the program, since, for those out-of-season trips, we need to report on when they have used up those passes, and, as well, this data will be used for program participation. That's just the first thing, is announcing the program, explaining it to people, and accepting applications from people who want to participate.

Once you have accepted applications to participate in the program, that effectively sorts people into two groups, which you can see, on the slide, I have split our box of anglers into two boxes, one on the top for those who have applied, or who are interested in the program, and another on the bottom for those who are not.

You can see that I have sorted our adjectives between these two boxes, and that kind of represents the hypothesis that the anglers interested in this program would be, on average, more avid, and/or more skilled anglers, and that, by the way, would be a testable hypothesis, once the program is underway, in the sense that you could actually take that trip-log data and look at something like their catch per trip and see if that's higher or lower than the average from MRIP.

Let me mention also that, in the likely scenario that there's more applications for slots, more applications than slots available, it's our intention that membership would be determined through a lottery.

Then, when the season opens, you've got these two systems working in parallel, right, where the people who have been accepted into the program are on the top here, and they're sending this detailed data to NMFS, and they're using these day passes outside of the regular season, contingent on compliance with the requirements for the program. Then, at the bottom there, you see you still have the regular management system, with the season that applies to everyone, working essentially the same as before.

Having covered how this one-year program would work, I want to spend some time thinking about how to move forward after that initial pilot year.

In particular, I had stressed that my kind of model for thinking about this is that you do the program, and you would want to somewhat quickly assess -- I mean quickly meaning say on the scale of months rather than years, and, if it goes well, you would want to do something substantively similar on a larger scale in the following year, perhaps refined somewhat based on feedback, but probably similar. If it goes just okay, then maybe more substantial revisions are in order, and, if something goes like terribly wrong, then it's like back to the drawing board.

All of that, as opposed to say like doing this one year program and that's it and then we go back and see how it went, and we try to publish on it, and then we come back at some like undefined, indeterminate point in the future to revisit it, and so I have sketched out on this slide like the optimistic case, I guess, where things have gone relatively well, and you're asking yourself how could you like think about scaling something up like this.

The notion here is that people who participated in year-one could be afforded the chance to like renew their participation in the program, and so you would use data from the first year of the program to establish like catch histories and use that as the basis for assigning more day passes in the second year, and, in addition, you could also like bring some people on for an initial year at the same time, and so you could proceed on like this, if there was like positive feedback, and this was seeming to work, by some pre-established criteria.

Here, I am returning to just kind of a diagram, to trace out what I explained on the last slide in a little more detail, and so you can see I'm like keeping the convention, and the red circles still represent trips, and we have time on the horizontal axis there, and what's different now is I'm showing like two years of the program, and the top and bottom lines -- They now represent

different types of anglers participating in the program, rather than participants versus non-participants, and I am showing two different types of anglers, and you can see that Angler 1 is the kind of more avid angler.

They take four trips during the regular season in the first year, rather than Angler 2, who only takes two, and I am also showing one out-of-season trip under the day-pass program. In year-one, I was showing two.

Under the kind of mechanism that I am describing for expanding, you have three things that would happen in sequence, and so, number one, the anglers participate in the regular open season during the pilot year, and they fish as much as they're inclined to, and they comply with the reporting programs under the program.

Two, they use the day passes that they're allotted under the program after the regular season ends, and so that's that part in the middle there, and then, three, after the first year is over, and so NMFS examines the data collected under the program, and, here, I'm suggesting that they would use the average number of trips taken by participants, and so, here, there's a total of eight trips taken, and there's two people, and so that's four trips per person, and that's the number of day passes that you allocate people in the second year.

I tried to illustrate this as like as absolute simply as possible, but now I want to like draw issues to a couple of like complications here, and so, first, an important constraint on any kind of mechanism you would want to use to do this is that I can't create an incentive to increase fishing effort in the open season of the pilot year, and otherwise -- Because, if you do create an incentive to increase fishing effort, then you're in violation what I previously mentioned, that the program be made to not shorten the season for non-participants. If the people in this are going to be given some incentive to like fish more than they would, that would require a compensating reduction in the season.

That's the reason for assigning people passes based on the average rather than their own number of trips. If you tell people that, hey, fish as much as you want in the first year, and then you will be able to do the same thing next year, but whenever you want, that creates a --

Second is that people need to opt-in for the second year of the program, and, obviously, you can't like -- I don't think there's any mechanism by which you could like have people make some kind of multiyear commitment, and you probably wouldn't want to, and so

you have to have them opt-in for the second year of the program, and they might not be inclined to.

In other words, suppose you have like an Angler 3, and they take like sixteen trips in the first year. Then your average number of trips would now be three, and so you take the original eight plus sixteen is twenty-four, and so you get eight, and so you would be telling this person that they would have to take half as many trips as they normally would to be part of this program, and they get to do it like at their leisure, right, but that might not be enough of a carrot for them, and so you might like create some very selective attrition from the program, which could be a problem.

To me, there's a couple of like open, important questions about how to do this, how you would scale this program up in the following years, but I think what is sketched out here is a useful starting point.

I want to pivot now to talking about how you could assess the program, and, on this slide, I have organized some important benefits and costs, according to the perspective of -- On the top row here, you have the benefits and costs to individual anglers, and, on the bottom, you have the benefits and costs to the whole kind of management system.

To start with the effects on individual anglers, the obvious benefit, which I have stressed, and how I have framed the program, is the increased flexibility and fishing opportunity that it would afford them, and so, for instance, if people tend to have conflicts, due to work or other commitments during the regular season, that might be a particularly good thing for those people.

On the other hand, you have costs, and I think that principally has to do with the data provision requirements, and so you can think about that in a few different ways. One is just like literally the time spent logging their activity in the app, and maybe also that like having to do that somehow like interrupts the flow of the trip, that it kind of detracts from the experience value, and another consideration would be any like privacy concerns that anglers have about submitting detailed data on their activity.

I guess, hopefully, you would think that like the voluntary nature of it kind of ameliorates that a little bit, since you're selecting for the people who aren't particularly concerned about that, and then, for the system as a whole, one major benefit of the program would be there is more precise measurement of catch and removals from that sector.

There is two ways to think about the value of that, one being that you have more detailed information on how anglers behave over the course of a season, since the kind of data you're gathering here is essentially like panel data, whereas almost all of the recreational data we currently have is this like repeated cross-section data, and, two, if this program reached a point that you have like some meaningful percentage of the catch reported under this program, that reduces the overall uncertainty in the catch estimates, which is valuable to the stock assessment.

Now, the cost, this is something that I have like largely glossed over so far, but there is certainly a lot to consider in terms of monitoring, and, if you have the system where, after the season in the post-season period, people have -- Some people have the right to retain fish, while others don't, and how are enforcement personnel supposed to know who is who, and how does that make things more complicated in that regard, and, also, you might want to validate these reports in some way, like random intercepts, which is currently done in I think Mississippi for the red snapper management, and there is certainly a cost to that, like a direct monetary cost.

Here, I am just going to -- It's the same table, but now focusing on like -- Specifically like metrics, how do you measure the concepts that I was mentioning on the list side. For assessing the importance of increased flexibility to anglers, and so this top-left one, one really simple kind of descriptive statistic you could get, if you were doing a second year of this program, as I described it, would be the percentage of passes used outside the regular season.

The notion there, right, being that, if that number is close to zero, then presumably the program isn't really doing anything for them, and they're not electing to use any of that flexibility, and now, on the other hand, if that number is like 60 or 75 percent or something, then it seems safe to infer that that's really doing something for them and allowing them to just like spread out effort in a way that works better for them.

Now, the cost to anglers, and so on the right, the sort of reporting burden, you could assess in a number of ways, and one really simple one is just like literally the amount of time that people spend in the app filling out these forms, and so that's an easy thing to track, if it's electronic, as well as just like what kind of IT issues people have, just in a more qualitative sense, like how smooth the experience is, and is it getting in their way.

I will add, also, with regard to the costs and benefits for

anglers, I think the ultimate like cost-benefit statistic would be the attrition rate, and so both attrition in terms of within the first year, and so you don't want people saying like, oh, this reporting is like totally ridiculous, and I thought this would be good, but forget this, and I don't even care about these day passes, and I'm not doing this anymore, and so that kind of like within-year attrition, and then also like attrition in terms of second-year like renewal attrition. If they're offered a chance to participate in the program, do they decide that that's something they're interested in continuing to do.

On the bottom, for the benefits to the broader management system, probably a key metric is the reporting accuracy, and that could refer to a few different things, and so whether people are fully reporting everything that they're supposed to, whether they're sending reports for each trip that they go on that's supposed to be covered under the program, and so the exact specifics are going to depend on exactly what the requirements are, which, as I said, I think is kind of like a big, open question for the program design.

The costs to the management system, I wrote misreporting rate, which I guess is really just like the flip side of reporting accuracy, but you could also put here like any direct administrative or staff costs for monitoring the program, depending on what provisions you deem necessary.

Getting like pretty close to wrapping up here, let me just repeat a couple of the things that I see as important, open issues. First, there is the initial allocation, which, so far, I have said lotteries would be used, but there are, of course, other options, and so auctions would be one, but, even just like within the realm of lotteries, there are some questions as to how exactly you would structure that.

For instance, Florida is soon going to be considering a harvest tag program for goliath grouper, with the intention to distribute them through a lottery, like what I am suggesting here, but I believe they have also mentioned a fee to apply for the lottery and then another fee to actually buy the tag if you win the lottery, and so that's a consideration, whether it's worthwhile to like create some barriers to entry, which presumably kind of like enhances that sorting effect towards people who are seriously interested in the program, and so just details like that even within lotteries.

Second, the race for catch history problem, which is the vernacular for what I described earlier, in terms of, if you intend to renew

the program, you want to avoid giving people an incentive to like fish more than their like baseline level of effort in order to secure themselves like a higher catch history.

Then the last one here is I think one of the big concerns, as I was just saying, would be program dropout, program attrition, in terms of within year and potentially across years, and so how you mitigate that -- How do you mitigate that, and how do you ensure that this program is something that people find valuable and want to continue to participate in.

Finally, just to give everyone a sense of where we are, the proposal that I have outlined here also lives in a document that we have kind of drafted in the style of an EFP, an exempted fishing permit application, and, from my impression, that's what we think would be kind of the appropriate legal administrative instrument to get this off the ground, and that's drafted as like sort of an options paper kind of structure.

Like I said at the start, what I have presented here is kind of the typical example, with like a lot of the edges kind of sanded off and some nuances excluded, for the sake of facilitating the exposition.

Then, going forward, in the near future, I think one of our most immediate next steps is to start fielding this around with a wider group of stakeholders, and so, so far, this has just been developed by myself, with like feedback from more senior staff here, and so like a good example of a next step might be to be doing like some focus groups with anglers or clubs and like to tie that back to the last slide.

Even returning to that again, when you like have an app developed, and you have some more of these specifics ironed out, that's like a strong way to filter out the possibility of something that would have a lot of attrition. All right, and so that's it for the presentation.

CHAIRMAN NANCE: Thank you very much. I have just a couple of questions, and maybe I missed it at the very first, but the type of data that you're interested in is catch, locations, discards, cost of the trip, those types of things? Is that correct?

DR. GORDON: Yes, and I think, at the bare minimum, I think you would want numbers of harvest and discard for the target species. As I said, I think there's a lot to work out, in terms of -- Obviously, the more things you add, the more that adds to the reporting burden and potentially turning people off of the program,

and so, yes, at the bare minimum, I think the harvest and discards is what you would want.

CHAIRMAN NANCE: Okay. Then have you considered any mechanism for evaluation to -- Let's see. To validate the data that's being taken in?

DR. GORDON: There is like -- I guess, specifically, with those numbers, the harvest and discards, you could be worried about either like under or overreporting, and so one way for validating that you don't have overreporting of catch would be if you're like using the camera on the phone, and you're asking people to like take a picture that gets submitted, which presumably also would help with the reporting burden, just in the sense that that's easier for people than them like filling out some kind of form, and so that's one thing that is potentially on the menu.

CHAIRMAN NANCE: Okay. Thank you very much. Trevor.

MR. MONCRIEF: Thank you for the presentation, and it seemed like you really had a good grasp of all the pros and cons to it, and you really kind of thought this out a little bit. My -- This is just kind of a comment here, and it's more at a higher level, on the philosophical side on it, and I don't -- I am struggling with I just don't think were on the recreational fisheries side, as far as limited access programs, and I will defend that by at least going back and trying to link to the closest thing we have in the natural resource world, which is quota hunts, tag programs, and everything else on the wildlife side.

In these areas, you're giving out tags for species that are relatively case-selected over a small area, and there is a large probability that, if you open it up to the complete public, that overharvest would occur, and then there's also places where it's limited spatially, where they only allow a certain amount of folks, to be able to limit harvest there.

In my mind, I don't think we have reached this point for the recreational sector, given that we've had a greater amberjack season that has pushed ninety days, and we have gray triggerfish seasons that have been relatively long, and we've had red snapper seasons, within states, that have been relatively long, and we still haven't really gone through the process of optimizing season lengths, or anything like that, to be able to benefit the recreational sector.

Now, I am not saying that -- I think moving forward with this, and doing it on a small scale, just to see its feasibility for a future

application, could be useful, but I am just hesitant on a scale-up of this type of program, given where we are with the recreational fishery at the moment. Thank you.

CHAIRMAN NANCE: Thank you. Rich, please.

DR. WOODWARD: Thanks very much. As you know, this an area that is near and dear to my heart, and I am really pleased to see that you guys are thinking about pursuing these type of data. I think it will be really helpful, if it's done right, and it's a good step in the right direction.

In terms of whether we're there or not for the recreational fisheries, I would say that's a testable hypothesis, and these are the type of data that will help us test that hypothesis. In that vein, this wasn't the presentation to do that, but I think it's going to be really important for you to think very carefully about how you're going to test your hypothesis and how you're going to estimate your models and taking into account selection bias issues that may result.

Given sort of as a question, how are we going to estimate the value of expanding the season? I mean, have you thought through carefully how you're going to do that? I guess, particularly, one of the concerns that I have is that you're going to see higher attrition from -- Or let's say less consistent participation from anglers that are going out fishing a lot and catching a lot of fish, because the burden is actually going to be higher on them, and they're going to say forget it, and I've done this four times, or five times, and I'm done with using the app, whereas the fisherman that goes out two or three times a year, they may be perfectly willing to play along with you for those trips.

When we were originally pursuing our funding to get this, the early proposal sort of had let's track somebody for the entire season, and the anglers that we spoke to said, you're out of your mind, and so we moved rather to a model in which we're just going to ask them to participate for a single day. Anyway, they're sort of questions and comments, but, overall, I am very enthusiastic about this type of work.

CHAIRMAN NANCE: Thank you. David.

DR. GRIFFITH: Thanks, Mr. Chair. First of all, I would like to -- If you could send us the link for that National Academy of Sciences study that you referenced in your talk, that would be nice. The other thing is so you're going to try and select people, it seems like, that are more avid fishermen, and they're less

likely to be the casual fishermen, and they're more likely to be harvest-oriented, you said, I believe.

I was wondering, and are you going to -- Are those people going to be self-identified, or do you identify them, or how are you going to identify those people and to put them in the pool for the lottery? I guess that's one question I have.

Then the other thing is I am a little confused over -- So this essentially expands -- Does it expand the fishing quota for different species, or does it distribute what the quota is, or what's available now, over the season longer, and I'm not clear on that point. Thanks.

DR. GORDON: For the first thing you asked, about the link, I will maybe send that to one of the organizers or something, and they can distribute it. Then the first question about how people would be selected, as we've currently thought about it, I think it's just advertising widely that this is going on, and just an open application for people, and then that lottery system.

Again, the notion that these would be people who have certain characteristics, that's just a hypothesis, and the more skilled, if you just want to interpret that as higher catch per trip, that part is definitely testable using the data. At a minimum, you would be collecting -- Whether they're more avid is maybe a little harder to test, since our data on how many trips people take per year for a particular species is not really entirely there, but part of that would be clearly testable, and part of it would -- We're not like having any mechanism to specifically only get those people, and so it would be open to anyone who would want to.

On the second thing, about what's going on with the quota here, that, again, just kind of is one of the specifics that is not entirely ironed out, and so there is this one option, which I mentioned, which is that, for some of the IFQ species, there are these small amounts of quota that are currently like administratively retained by NMFS, and, honestly, exactly what the detail is there is beyond me, but it's been suggested that that could be used towards this.

I think there is also like an amendment under development that would use them for something else, or a quota bank or something, and I'm not entirely clear on what's going on with that, and then, like I said, another one of the options would be, if the quota was set to increase, you could use part of that to do this, and so that's another option that's been put forward.

Another thing that has been suggested, although I'm not clear on it, would be that there is the -- There is the ACL and the ACT, right, and the catch target is like this uncertainty buffer, and so, since this would reduce the uncertainty in part of the removals could you somehow make the argument that you could do something there, that, since you would be measuring the removals more precisely, that you could have like some compensating reduction in the buffer between the catch target and the catch limit, and so that's been thrown out there, although we certainly haven't like worked through that in full.

CHAIRMAN NANCE: Thank you. Jason.

MR. ADRIANCE: Thank you, Mr. Chair, and thank you for the presentation. I have a few comments and then a question. We have our recreational offshore landing permit here in Louisiana, and, as part of that, there is a voluntary reporting for red snapper that is part of that, and attrition has been mentioned, and we noticed that's a big deal. You get a flurry of folks interested at the beginning of the season, and then that tails off rapidly, and I think we get somewhere -- It's less than 1 percent of trips, and it might even be less than half-a-percent that actually end up -- Now, that's voluntary.

I understand there is not an incentive base, like there would be here, but certainly we have folks here that can definitely discuss our experiences with you, if you want, at some point, and then the question I had was the Chair brought up validation, and I had a question related to that.

Since you're establishing a trip history in this program, how would you validate that trip history, to make sure you're not incentivizing someone to put down more trips than they necessarily have, to be able to take more trips out of season, to skew that average? Thanks.

DR. GORDON: Thank you, first of all, for mentioning the stuff that's already been going on in Louisiana, and so that's interesting, and I would like to hear more about that. With regards to the question about how do you avoid -- Again, the vernacular for this is the race for catch history, and so it's my impression that this was dealt with like some good time ago, when some of the IFQs were first getting off the ground, both in this region and in other regions in like the 1990s and 2000s.

It's like -- In their case, it was how many years back from before you're announcing these IFQs are they going to use catch history, because they at least there have the data going back, and so it's

just a question of like which -- How far back do you have to go in that data to get to the point where like no one even had the idea that this might be an impending policy change and so it couldn't have influenced their decisions, and that's like a much more tractable problem.

Here, there is no -- We have no historical data on anyone in particular. We have historical data on, in each year, some like cross-section of people, and so, yes, that is a pretty difficult problem, and the way I've thought about it is essentially that it's kind of like an incentive design issue, and so I think, with the average rule there that I kind of very fuzzily laid out on one of my slides, I mean, that certainly doesn't create like a direct incentive to do that, to increase the number of trips that they're going to be reporting on, but it does -- To the extent that people kind of like -- Like if they think of it as like a cooperative, that they're in with these other people, if there's kind of like a prosocial incentive there, then that could still be an issue.

Like I said, I consider that one of the kind of big, open issues, but I do think it's important, because, as I said, I think you definitely want to do this in a way where you like plan on coming back to it. Like here's one way to think about it.

If you have the option between doing a program like a one-off, like one year and that's it program, with a certain like sample size and amount of quota to go towards the program, versus doing something with like half of that amount of resources, in like an institutional commitment that you're then going to come back and revisit it in the next year, I would take that second option, because I think it's just inevitable that you're going to like get things wrong the first time around. I think it is important to think about -- Have a plan going into it to come back and change things the second time around.

CHAIRMAN NANCE: Thank you. Lee.

DR. ANDERSON: Thank you. Dr. Gordon, thank you very much for that presentation. I must say that this is the fourth day of this meeting, and all four of the economists have finally broken into the speakers for questions.

CHAIRMAN NANCE: I am grateful.

DR. ANDERSON: You've really got us going. Thank you. I have a couple of questions. I guess, first, I enjoyed your presentation, but I don't really understand all of what's going on, and I wonder if you could send Ryan -- There must be some background documents,

and the PowerPoint had some stuff in it, but I would certainly like to see a paper that describes it.

I would also like to ask, Dr. Gordon, where this idea came from. I mean, I like to have people think outside of the box, but sometimes I'm a little cynic. We get ideas that sound real good, but it may be -- Well, I have to be very careful here. But sometimes people come up with ideas, and I found, when I was on the Mid-Atlantic Council, they would come up with ideas that this is going to do this, and this is going to do that, and it's really great, and it's sliced bread and everything else, but really what it is is a way that people can bend the rules, finagle things around, and so I want to know where this came from, et cetera.

I would also like to know if you have really talked about it with more stakeholders, and I think you said you're still working on that, and the final thing that really worries me about this is enforcement. The Mid-Atlantic Council had an RSA, a research set-aside, program, where there were bad guys in the thing, and you've heard about the Codfather and everybody, and they cheated like heck on that research set-aside.

I have thought a lot about enforcement and the incentives and everything, but it's a big ocean out there, and, if there's some guy that has got a pass that says that I can go fish, I'm not sure how we can separate between those that legitimately have a day pass and those that don't.

I am not trying to say that's a death blow to it, but it's something that really has to be thought about, and so, if you could answer my question, I would appreciate it, and congratulations on thinking outside of the box.

DR. GORDON: All right, and so I'm going to attempt to take those in order. Thank you, Lee. First, with the documentation, like I said, there is -- I have this kind of drafted out as an EFP application, and that's probably the tidiest thing, and so will add that to the list of things to send to the organizers for distribution, along with the National Academy of Sciences report on LAPPs.

As to where the idea came from, I have been working at the Southeast Center for just a little over a year now, and the idea definitely has gone through more than a few phases at this point, and so I think the initial framing -- First of all, I think the initial framing -- We were more optimistic on the idea of using auctions for initial allocation, and then the practicability of that I think just seemed really difficult, but the motivation, as I understood

it, when I first started on designing this program, was that allocation, in terms of like allocation ratios between sectors, have become like really hard to move.

Is there a way that you could either have a program that would like give you a way forward to making it easier to move quota between sectors, and so, for instance, Alaska -- In Alaska, they had this guided angler fish program some years ago, where you could buy -- It was sort of like a harvest tag, but it was like a tag for an extra fish over the usual bag limit, I believe, and that could be like bought out of the commercial quota, and so you could like take someone who has commercial quota and like -- There is a conversion factor between the quota pounds and this extra guided angler fish, and so that was like one kind of thing that I learned about early on that was kind of a model for what you could do here.

Then another motivation was like or could you do something where you would get a better estimate for like the marginal value of quota to the recreational sector, and so I think we've moved more in the direction of like the first thing, of like could you move towards a system where it would be easier, potentially, for quota to like move between, instead of having to set these allocation ratios.

Then your next question was about who have we engaged with, and, for me at least, it's largely just been people internally within NMFS, just getting acquainted with everyone and kind of giving various iterations of like this talk to people and getting help with drafting that document that I mentioned that we're going to send.

The last one, you're right, is the big one, enforcement, and so, yes, I'm aware of the SRA program, and specifically the Mid-Atlantic one, and so my understanding, and, admittedly, it might not be the best understanding, but my understanding is one of the like central program design issues there was that they had a hail-in and hail-out, or hail-out and hail-in, for the charter operators who were going to use some of that RSA quota, but the hail-in didn't have to be submitted until like twenty-four hours after the trip ended, or something like that.

I'm not like 100 percent on that, but my impression is that it was something like that, and that created the conditions to where, if no one comes to look at what you've got, and you don't have to tell people what you got until twenty-four hours after you've already landed, and everyone has taken off, then that makes, I guess, essentially like fraud, or breaking the rules, a lot easier.

I think that's not what was done in the most recent kind of effort in this vein with the headboat pilot project, like five years ago or so, and I believe they also had a hail-out and hail-in, and they were supposed to submit this information before landing, right, and so then, if there's someone there to like validate the catch after you land, then they have some reference point of what it's supposed to be, right, which is what makes sense.

Of course, that's for headboats, and that's much more like economical, to have someone coming out there when there's like a whole bunch of people in a big boat, and it's like a lot less economical to be doing that just for private boats with potentially just a few people on them, or even just one person.

My impression is that, with the state management of red snapper, the state that is closest to having something that looks like this is Mississippi, and I believe, at some point, I was told that Mississippi does actually have some like random intercepts of people who are coming back under the Tails 'n Scales program.

Of course, it's also my impression that it kind of helps, just geographically, for Mississippi, that there's like kind of concentrated access points, and so, at one point, we had been considering, and I think this is in that document, like would you do this Gulf-wide or would you do this like state-specific, or, if you were going to do it in Florida, just like some area-specific, and would you require like a hail-out and hail-in, and so those are kind of like details that we have -- That I kind of glossed over, just to make this presentation fit inside of twenty or thirty minutes, but, yes, those are definitely considerations that we have in how we're going to design this, and, again, thank you, Lee.

CHAIRMAN NANCE: Thank you. Paul.

DR. MICKLE: Thank you, Jim. Real briefly, and I know there's a lot of folks that want to comment on it, I appreciate the presentation, Dr. Gordon, and it's a great thing to look outside the box and come up with these ideas, and there were some new things that we haven't really thought of recently, but one of the things that we looked at specifically, when I was with the State of Mississippi, was that mandatory reporting really helps with the attrition issues.

If you don't give them a choice, then you don't really have that attrition issue, so to speak, but, along with making things mandatory, you have to have enforcement, which I don't think is possible on a Gulf-wide scale for a survey, for a recreational

survey, at least at this time.

One of the things that popped up, when you were presenting, was how this potential design would work with some of the lags that they still have with MRIP, as far as data compilation and computation, and so, just with the lags that you have with the current system, it seems very difficult to be able to fish inside and outside of set seasons, even with a carved-off percentage to do so.

Then, I guess, lately, it would be potential good news, from a regulatory position, of looking at if the states could swallow losing off maybe, I don't know, a small percentage, like 0.5 Gulf-wide, in accordance with state prioritization, or proportions, they could go through an EFP process and try something like this out within a pilot phase, to see, just as was talked about with another survey, but things like that -- It's great to look at these different options, and I think it can be done.

I think it takes a lot of thought and creativity, which is obviously there, but, as far as I can figure out, I think this -- Was this study paid for by NMFS and the Southeast Fisheries Science Center? Who funded this research? That will be my question.

DR. GORDON: Thanks. That's a consideration, and I don't think, if I'm understanding your first thing about the data lags with MRIP, and so, if I'm understanding you, you're saying that like, because we don't know, sort of until well after the regular season ends, if the catch limit might have been exceeded, that that would be a regulatory issue to be telling people that you can be fishing outside the regular season, even though there is like some uncertainty about whether the catch limit was already exceeded, and so, if I'm understanding that correctly, that's a concern that hadn't been brought up.

I think a concern had also been brought up -- Thank you for that. Another kind of related concern, maybe, was that we might be having some like duplication of effort, in the sense that like, if someone in the program happened to be approached by like one of the access point surveys, it would be like a duplication of effort, which I think technically we're not supposed to have, and so, yes, there is definitely some issues like that, and so thank you for pointing that out.

Then, as far as -- Thank you also for pointing out about the states submitting the EFP, and so that's definitely something that has been floated, is doing the state-specific and then doing it in close cooperation with one of the states. Then I am -- I work at

the Southeast Center, and so, yes, I think it is funded by them, or maybe it's Headquarters.

CHAIRMAN NANCE: Paul, a follow-up?

DR. MICKLE: Thank you, Dr. Gordon. Lastly, very quickly, and I want to make sure that everybody can do their thing, but, as far as the duplication issue, very creatively, the State of Mississippi added trip numbers, and so, when you got a trip number and created the trip -- When they are intercepted and surveyed, they tie it to that trip, and so no duplication of data is created. Thank you.

CHAIRMAN NANCE: Jack.

DR. ISAACS: This is a really very interesting project. I mean, it's kind of exciting to see something like this unfolding. Of course, there's many a slip between the cup and the lip, and exactly how to get it right can be kind of difficult to figure out, but you're aware of that.

I'm glad to see that you're going through some public outreach, because this is going to succeed, really, to the extent that it appears legitimate to your stakeholders, and, of course, you're really aware of that. A very interesting thing here -- I wonder though -- I have a concern about angler behavior, in terms of the particular species, and, with fishing, it's going to differ a little bit from hunting.

Now, hunting, I think, has a great precedent for fishing, because they have used tagging systems of various sorts for years and years and years, and you can learn a lot from what they have done to legitimate hunting tags and make them effective tools, but, of course, when it comes to fish, it's different than say hunting for bucks or does.

If you're hunting for a buck or a doe, you're -- It's very, very targeted behavior. If you're going to shoot a doe, you better be aiming for a doe, right. If you shoot a buck, you're aiming for a buck. When it comes to some of these fish, I wonder to what extent the people who catch them are targeting the fish and to what extent the people who catch them are just sort of catching them opportunistically or incidentally, and I wonder how that is going to complicate your allocation, but experience will tell us. Thank you.

CHAIRMAN NANCE: Thank you, Jack. Mandy.

DR. KARNAUSKAS: Thank you, Mr. Chair, and thank you, Zander, for

a really exciting presentation, and it's great to see this work. We have been doing some work with stakeholders, at the Southeast Center, sort of along the lines of a focus group, and we call it the participatory modeling process.

One of the goals of that process is trying to understand the preferences and end goals of anglers, and we've been focusing on the for-hire sector recently, and one of the findings from that process is that there's really a lot of desire to increase flexibility, and people seem to be pretty happy with the management overall, bag limits and things, but there's a lot of desire for flexibility.

It's great to see this, and I think it's important that the SSC discuss the possibility of trying to move some of these things forward, but my question is I'm wondering if there's some sort of intermediary steps that could help us assess the appetite for this sort of program, and I will just throw out one of the things that comes up a lot as we talk to stakeholders, and so, for example, for the for-hire sector, for red snapper, it has a sixty-day season.

A lot of people say just let us pick the sixty days, and they know that this would be facilitated with the new electronic reporting, and folks see that that would kind of reduce the derby fishery effect for that species, and so I understand that it's a different sector, and it's not necessarily applicable to what you're talking about here, but I'm curious if you considered something along those lines, because it seems like that sort of program could help you test the desire for increased flexibility without getting into the allocation issues, and, essentially, you have that default allocation, and every federal permit is equivalent to X days on the water for red snapper, and so you could just look at the appetite for sort of fishing the regular season. Thanks.

DR. GORDON: Yes, that's great. Thank you. We have definitely considered the idea that -- I mean, I don't see it as unrelated at all. I see it as very similar, and so, in your example, if it's sixty days, and they get to pick the sixty days, then I kind of see that as basically you're giving them sixty-day passes, with the constraint that they have to all be consecutive, and that's how I would think about it.

We have thought about that, and, obviously, a lot of the same issues come up, in terms of the enforcement issue, and how do people know who is who, like who set their sixty days when, and you still have this -- You still, in that case, would expect -- At least I would expect an increase in the mortality attributable to

those people, and so, if they pick the sixty days that's best for them, you would think maybe they're picking that because they think they can go on more trips during that time period that they're picking or they can get more catch per trip, or whatever their reasons are.

Something I thought about, when I was considering that idea, is maybe a parameter that you would have to figure out is like some reduction, some like multiplier less than one, if you were going to try and make it so that, by doing this program, you're not, again, increasing the mortality from the people in the program, which would require an off-setting reduction in the season for people not in the program.

Maybe it's, instead of sixty days, they get like fifty days and they can pick when it is, or forty, or whatever it is. Yes, I think that's an interesting idea, and maybe there is some features of that that would made it more practicable, and so thank you for that. That's a very interesting idea.

CHAIRMAN NANCE: Thank you. We're running over a little bit, and so we need to kind of keep our questions and answers a little bit succinct, but Sean.

DR. POWERS: Thanks. Lee and I were both on that NAS LAPPs panel, and I agree with you that there is support for this. I think the support I've seen is largely from academics who would like to pursue some of these ideas, which is fine.

One of the things that was pretty clear, that we heard from the commercial fishermen, and just a warning, is your idea of buying quota from commercial and then turning it into recreational tags or anything would probably serve to erode support from the commercial, and so I would be very cautious about that, and, in general, just -- I agree with Mandy that you need to engage and survey folks, the different stakeholder groups, but make sure you have a pretty good vetted plan with engaging them, because it will be easy to alienate them, and that support will be hard to get back.

I agree with stakeholder engagement, but be very cautious and do your homework before that, and get as much feedback as you can, because Lee and I can attest that the LAPPs discussions in open forum -- There were some very opinionated people.

Then, secondly, and it's not really a question, but it seems like you've already done it, or looked into it, and I would strongly encourage you to not go Gulf-wide and to do a very regional, very

trackable study, and maybe at one of the smaller state levels, like you said in Mississippi, and maybe Alabama would be interested, or a region in Florida, where you can have a very trackable system and where you can survey attitudes before, expectations before, and then survey afterwards, and so those are kind of my two observations, but no real specific questions.

CHAIRMAN NANCE: Thank you, Sean, very much. Steven.

DR. SCYPHERS: Thank you, Mr. Chair, and thank you, Dr. Gordon. This was a really interesting presentation, and, actually, Sean and Mandy raised some of the points that I was going to make, and so I will say, first and foremost, that I really appreciated, in your presentation, all of your comments about barriers and potential social consequences of these types of programs, because I think that's really important, and I am glad that you raised it as part of your presentation, and I think it's something that we need to discuss in this type of venue.

Related to that, my question was if you had considered, as you move forward toward pilot studies, assuming that's kind of the next step, if there are more representative ways to engage participants in that, and I think a volunteer-based -- An advertise and sign-up type of program probably would lead to a quite skewed participant pool, as you already mentioned, and I wondered if you had the capability and the resources to be able to do something more representative, as Sean mentioned, even on a local level, with a survey to recruit across the different levels of avidity, as you showed in your presentation, and so that was my general question there.

Then just kind of a last comment, and connecting to some of the participatory work that Mandy and Matt McPherson and others have been doing, I think that type of approach could be really useful to identify some of the tradeoffs, because, as you mentioned very clearly, a lot of this won't be clear. There will be benefits, possibly, for data quality or that type of thing, but there could be consequences, particularly if it becomes like a mandated-type program, and seeing that a lot of those things interact together just could be a useful way to see this system as it is, and so thank you again for your presentation.

DR. GORDON: Thank you. I will just get to the one question there about if we have more representative ways to be recruiting people, and we definitely do, and so what you mentioned I think definitely has been floated.

At one point, and I am not entirely sure on this, but, at one

point, I had the impression that like, if there was any restrictions on who participates, that that makes it more of an uphill battle, like administratively, and that having it open access just eases things that way, and so that's the way I framed it, for the same reason that I also didn't go much into auctions either, just on the grounds of like practicability, but it's definitely something to consider, and so thank you.

CHAIRMAN NANCE: Thank you. Martha.

MS. GUYAS: Thank you. Good morning. Thanks for the presentation. Just a couple of quick questions. I am wondering if you have particular species in mind for this and if you have considered -- I guess is this a Gulf-only project, potentially, or are you also looking at the South Atlantic? Then, lastly, it sounds like the Science Center would be the EFP applicant in this case, and what's the Science Center's plan for covering administration, in terms of costs for tag distribution and validation, et cetera?

DR. GORDON: Thank you. I think the first one there was about the target species, and I think the three that have been like floated the most were red snapper, red grouper, and gag. I think those are the ones that we've talked about the most. In terms of whether this is like exclusively something we've been thinking about for the Gulf, I would say that has been almost all the discussion. I think, at various times, we have talked about the South Atlantic as well. I am much less current on what is going on on that side.

As far as administratively, who is going to submit the application and who is going to be covering the costs, I think we have mostly thought about the Science Center, although, like someone has mentioned, potentially cooperating with one of the states and they could submit it, and that certainly has been floated as well, and so we're just not really there yet, as far as deciding exactly how we would get this off the ground and who pays for it.

CHAIRMAN NANCE: Thank you very much. Harry.

MR. BLANCHET: When I got to eight different points that I wanted to ask questions on, I decided that maybe the best thing to do is just to reiterate Jason's point that I think it would be useful to review some of this with some of the Louisiana folks that have been through some of these issues with our recreational offshore landing permit.

I will make a couple of points. One of the things, when we considered mandatory reporting, one of the things that got brought up to us was cellular coverage issues offshore, and, even in those

areas where you do have adequate coverage to be able to hail back in prior to landing, the cost of that may be quite high, because it may be something that is not covered under your cellular plan. I have heard of pretty spectacular cell bills from some of these.

The other quick question that I have is anglers versus vessel operators, because that vessel operator is going to have a different investment in the trip than his next-door neighbor that he invited along at the last moment, and so I don't know if he would want to go along on that trap and say, well, I can catch cobia, but you can't, and so how does that kind of within-vessel allocation occur? Thank you.

DR. GORDON: Thank you, and so the question about potentially having multiple people on a trip and some of them are in this program and some of them aren't, I mean, there are certainly questions there. I mean, for instance, you probably couldn't say, with a straight face, that like it has to be the person who is in the program who catches it, in order to retain it.

I don't think you could really say that, and so I think it would probably have to be like, if there's one person on the trip who is in this program, and the bag limit is two, then you can keep two, even if it's not the regular season. If there's two people that are in the program, you could keep four, and I think that would pretty much have to be how you -- How you worked that out.

MR. BLANCHET: But you are considering this on a per-angler basis, because there is a very big difference between the person who owns the vessel and the other anglers, and that doesn't necessarily have to do with avidity, but it has to do with access.

DR. GORDON: We have, so far, framed it in terms of an angler and not tying it to a specific vessel that they operate.

CHAIRMAN NANCE: Thank you. John.

DR. WALTER: Thanks. Good morning, everyone, and thank you, Dr. Gordon, for the presentation. I think this idea is something that's actually been fairly long percolating throughout fisheries management, and, in particular, I think it's been talked about for many years, and I know that some of the states have done something like this for tarpon, I think, with tags and are considering it for some other species.

This is really part -- I will just answer some of the questions about why, and this is part of a larger agency and Center initiative to better address the recreational fishery needs, and,

in particular, to be more responsive to concepts like optimum yield, where the recreational fishery may want better opportunity and not just yield.

One of the ways that you might be able to achieve that is some way of defining what a good recreational trip is, and so this is a larger initiative, and, in particular, here for the tags, this is, I think, a conceptual one that might probably be useful to take to our Reef Fish APs, to have it go through that channel about kind of putting it through its paces and gaming out what the pros and cons are, as well as reaching out to the groups who have been considering this, such as Louisiana.

I think the larger picture is that we've got the most valuable recreational fisheries in the country, and probably the world, and we've also got them growing pretty substantially, if you look at boat registrations and vessel purchases and the latest census numbers that have people moving to our region. This is going to put more and more pressure on our fisheries and motivate us to find creative solutions, and it's probably going to be on some of the stocks that are in particularly rough shape, like we're kind of seeing with gag.

They might be the ones that are the good ones to explore this kind of thing, so that we can maintain the continuity of some information that we might be able to get while getting the fishing mortality under control and being able to get better precision on catch estimates from the recreational fishery.

I think these are the kind of things we have to begin to explore, given the demographic changes that are occurring in our region, and I will thank Zander for that, and I do want to just announce Dr. Matt McPherson, who is the head of our Social Sciences Research Group, is really taking the lead on this, and I see it as a partnership between the SSC and the councils and our stock assessment groups, in trying to find some creative solutions. Matt, if you want to just say hi to the group. Matt is the supervisor for our Social Sciences Research Group.

DR. MATT MCPHERSON: Thanks, John. Some of you know me, and some of you don't, but I'm Matt McPherson, and I lead the Social Science Research Group. Just really quickly, there have been some questions about where did the idea for this project come from, information that Zander probably doesn't know about, but, about three-and-a-half years ago, we organized a regional social science group that included the social scientists from SERO, from the councils, from our group at the Science Center, as well as from Headquarters.

We were talking about what are some of the regional research needs, from a social standpoint, and how really social science could have a larger impact in addressing the need to achieve OY, as well as all of the challenges that were coming up in terms of allocation and things like that, and one of the discussions that we had at that meeting was the need for kind of creative alternatives and incentive-based approaches and perhaps developing some experiments to be able to come up with better ways of addressing some of the challenges, especially in the recreational fisheries.

Based on that, we kind of formed an advisory group that included economists, largely, from our group, from Headquarters, from the councils, and there were even some from some of the other regions, and we were fortunate to receive some funding from Headquarters to support hiring Zander, who has been working really closely with David Carter, who is our recreational economist, on this project, as well as other recreational projects.

This is something that came out of that meeting, and it's something that sort of slowly evolved over the last three-and-a-half years, and I am really glad that it got to this point where Zander was able to present where we are, in terms of our thinking on those ideas, but, yes, it's all internal, and it has all sort of come out of our own research agenda and, as John mentioned, our desire to contribute more to working on some of the recreational fisheries issues in our region.

Then, beyond that, I mean, some of the suggestions that have been brought up, engaging stakeholders and so forth, we've been -- Beyond just our regular economic and social data collections and all the work that we do to support regulatory analysis and best scientific information available, we have also been doing a lot of participatory workshops, I think as Steven mentioned, working with Mandy on EBFM issues.

We've been doing a lot of stakeholder engagement, and I do think that would be -- The next step would be to take this out, and I think Zander had mentioned it, and doing some focus groups or approaching fishermen and doing some outreach and getting some good information from them regarding what their, I guess, attitudes would be towards something like this and what they see as the strengths and pitfalls and so forth. Anyway, thanks, John, for just giving me the opportunity to introduce myself.

CHAIRMAN NANCE: Thank you, John and Matt. It's good to have you on. Ryan.

MR. RINDONE: Thank you, Mr. Chair. I will try and tie this off. To something that Jack had mentioned, Jack Isaacs had mentioned, about the ability to target specific species, recreational fleets definitely can do that, by varying when you pursue different species, like what time of year and spatially, the tackle used and the bait used and different fishing methods, and you can absolutely select for a very narrow suite, if not specific species.

With respect to fish tags in general, I mean, obviously, this idea of trying to use fish tags is not new. Clearly, the SSC's comments have demonstrated this, and the APs for the Gulf Council have talked about this several different times, as have both the Gulf and South Atlantic Councils, and the Gulf and South Atlantic Councils' joint working group of recreational issues has talked about this a couple of times.

There is quite a record established at this point about the southeastern councils' sentiments with respect to using fish tags, and, generally speaking, while it's been -- While the idea has merits on some surfaces, there is an awful lot of detraction, and I think that establishing catch history is something that is going to be a large red flag for both councils. As the staff person that sits on and helps coordinate the committee for the joint council working group, I am pretty confident in being able to say that this is not something that they will receive well.

Also, the idea that a single program is going to be able to address the perceptions of what OY means for different stakeholders that are encompassed within a single fleet, like the private recreational fleet, is probably a little too idealistic, because what may be an ideal fishing experience for me might be different for Luiz or Trevor or anybody else, and so a single program's ability to do that -- I think we've seen in the past that implementation of single programs trying to solve multiple problems is -- Sometimes it misses the mark.

With that, Mr. Chair, we are a bit over on our time for this, and I don't know if you want tie this off, and we have Greg's team on the line.

CHAIRMAN NANCE: Doug, go ahead, and then we need to move into Dr. Stunz's presentation.

MR. GREGORY: Well, okay. I was going to ask -- I had a text message from Captain Bob Zales, who wanted to make a comment or ask a question, and that's easy enough to do if everybody was in the meeting.

CHAIRMAN NANCE: We can do that in public comment, for sure.

MR. GREGORY: Well, that's at the end of the day, right?

CHAIRMAN NANCE: Yes.

MR. GREGORY: Okay, and so we can't do it now. All right. Thank you.

CHAIRMAN NANCE: Thank you. Okay. We'll go ahead and move into our Great Red Snapper Count report with Dr. Stunz and group. We'll turn the time over to you.

REVIEW OF FINALIZED GREAT RED SNAPPER COUNT REPORT

DR. GREG STUNZ: All right. Thank you, Mr. Chairman. Before we get going, Mr. Chairman, I just wanted to make sure -- Dr. Rob Ahrens and Dr. Lynne Stokes are key members of our analytical team, and they're going to present a portion of this, and so I wanted to make sure that Jessica was available to unmute them. There is others, of course, on our team that probably will have questions directed their way or something, and so just hopefully that will go smoothly, and I just wanted to give a heads-up on that before we get started.

CHAIRMAN NANCE: That will, for sure, and we just need to know which ones, when they speak, and then we'll unmute them.

DR. STUNZ: Okay. Good. First, thanks for allowing us to come back. As everyone is probably very well aware, we went through our evaluation process in a live, well, written and live, external review earlier this spring. Then we were asked to come back here to update how we responded to those recommendations that came out of that expert panel review, as well as the SSC review.

In this short period of time, obviously, I don't have time to go through every little mundane thing that we changed, and we're, obviously, going to hit the highlights of the major areas that came out of that review, but we provided an extensive written response to that that you should have in your briefing book, as well as, of course, the final report, which contains the detailed, explicit changes to that.

The other thing is I have some slides in here, and I know there are some new members of the SSC that may not be familiar with how we arrived here, and I don't plan to go through all of those slides, but, for the record, I wanted to make sure that they were in here, so for people that are just coming into the process now,

and they're aware of how we arrived here.

What you see in your briefing book are the direct responses to the reviewers' expert recommendations. Late this weekend, the Science Center asked us if we would reconsider some reruns, and that was sort of the eleventh hour, which was fine, and we were happy to do that, and it wasn't that heavy of a lift, and I am speaking for Rob and Lynne now, and they may not agree with that, but they were gracious enough to do that rerun.

I provided that, and we just finished that yesterday, and so it essentially just involves the Florida uncharacterized bottom rerun, and the rest remains the same, but we wanted to be responsive to that. How the SSC wants to handle that, that's certainly up to you, and Dr. Ahrens and Will Patterson will address those, since they were part of their changes.

Finally, all of this really boils down -- This giant study boils down to really two tables, Tables 5 and 7, and I cannot efficiently put them in the presentation, where everyone can read them, and so you should have got a PDF with these tables, if you want to pull it up so you can clearly see and not have to squint and strain to catch that, because it's not efficient enough to put on a one-screen presentation. Anyway, that's where we are, Jim, and I will go ahead and get started and bring everyone up-to-date.

Many of you have seen this presentation, or something similar to it, many times, and so I'm going to hit the highlights of where we are and how we got here. Obviously, the goal is to estimate absolute abundance, and it was $12 million in congressional appropriate funds, and we had twenty-one leading scientists from around the Gulf and beyond, twelve institutions, and many of them are sitting on this call or in the room right now, and we did habitat classification, and we enumerated those fish with direct visual counts, where we could.

Visibility issues provided substantial constraints, and we had to use hydroacoustics, as we've been hearing about other similar studies, as well as depletion surveys, and then, also, I want to just mention, and I will do this again, to reiterate, but there's a lot of data here that is outside of just abundance estimates and what most folks really care about, in terms of we were directed to spend almost half of that money on a tagging program, as well as extensive outreach and samples of genetic tissue, and so there is a lot more here, including exploitation rates and a variety of things that I think this committee will be very interested in in the future. I don't plan to present that today, but I just want to keep plugging that from that aspect.

Our team, just for completeness, was led by Senior Principal Investigators and a team of other investigators, and I've been through each one of their qualifications in the past, and so I won't do that again, and their institutional affiliations, but, again, that's just for completeness sake and for the record, to make sure that that's in the briefing book, for those that might just be tuning in.

How did we arrive where we are? Well, this process started with a whole variety of planning meetings and workshops. To be very clear and refresh everyone, it was a two-phase process, unlike the current studies that are going on.

There was a Phase I, where teams were to come up with a design, and so the design then was implemented in Phase II, to generate that abundance estimate, and the best points, aspects, I guess, you could say, of each design were pulled out and coalesced into another RFP that we were responsive to that actually generated the design, and so we're talking about Phase II, obviously, here today, those kind of things in Phase II, those designs, where it laid out the goals and objectives, but what was our general analyses that we should use, the target CVs, the scope, depth, habitats, the tagging study component, and we were also charged with spending quite a bit of that money on a comprehensive stakeholder engagement piece that we're not really going to talk about today, and then these genetics and other components that I was mentioning earlier.

The point being is we have this design component, and then we have this Phase II, which clearly spelled out how we should do this, and then we won that contract to do that, and, obviously, this study was responsive to that Phase II goals and objectives.

Then we brought this in front of this committee, after an extensive external review by some experts in the field, and I have to say that was probably the most intense process I've been through, career-wise, and I don't think anybody could say that they enjoyed that, but, I think, at the end of the day, the team feels really strongly that that substantially improved the report.

To get that level of scrutiny and peer review by both the external team as well as the SSC only could improve what we did, and that's what you're hearing about today, is how did we really respond to that, and so we think that was a very valuable process and substantially improved the report.

I can tell you that our team has diligently worked exceptionally hard to reanalyze and address each and every one of these

recommendations that were coming out of that team and the SSC. While I don't have time to go into each one of those, it's captured in the final report and the response letter that I mentioned that everyone has.

It really revolved around these big topics that we're going to talk about here today, and that was it explicitly wanted us to -- It recommended that we use a stratified random sampling design, which we did, and we analyzed it according to that. You're going to hear a little bit from Rob and Will in a minute about that last-minute change that dealt with that stratified random design for that uncharacterized bottom in Florida, and stay tuned. That will come up.

They had recommended removal of our original random forest designation, which was used in Florida for the uncharacterized bottom, and we did that. Recently, we were asked, by the Southeast Fisheries Science Center, would we consider reincorporating that, and what would it look like in a rerun. That's what was sent around late yesterday, and so we were responsive to that, and, obviously, we can discuss those changes and what does that look like.

The big recommendation we had was to capture as much additional variability as possible. There was some concern that there were small places that we didn't address, some of the small variability that occurred, and that occurred in several places, and could we essentially address that in a way that captured as much as possible, and we certainly have done that, and Lynne Stokes is going to talk about it.

Not only did we go beyond capturing all the variability that we mathematically could, that was possible, but we added an additional what we essentially call a variance buffer, and that is, in areas we know it might be more variable, we have a mathematical tool that she is an expert in that can essentially add in a buffer to account for variability that we can't calculate when we suspect that it's there.

We also improved the estimators, calibrations, modification of post-strata, based on all of those suggestions. Obviously, the concern revolved around the uncharacterized bottom, because that's where we find a lot of those fish, and we clearly address that now in the response and, of course, in the report. Dr. Stokes has also developed some alternate estimators that capture more of that variance and uncertainty that I was referring to, as well as just other estimators that reduce the bias.

You might recall that we did two analyses, which I will talk about in just a second, and they used different estimators, and slightly different methodologies, about how they arrived at the final estimate, and each one of those leads on the analytical team is here today to discuss that.

I just have this slide just as the plug, and, again, remember there is a lot of exploitation rates, which will become very important as we get into this assessment phase for red snapper, I believe, and Matt Catalano is leading that. We had a huge tagging component, which we're not talking about, and there are a lot of genetic samples archived for future analysis, and, of course, this angler engagement piece that many of you have seen, and everything we've done is archived at snappercount.org. Many people have, obviously, seen this, and I just want to make sure, for those that haven't, and this pretty much is sort of our history book of how this project came to be, from start to finish and everything that occurred.

Back to the real meat and potatoes, in terms of what about the abundance estimate. As I mentioned, we had two independent estimates. They both had relatively low CVs. Even after capturing that variance, they continued to still have relatively low CVs. When we did the random stratification that was recommended by the expert review panel, that actually increased the estimate in some areas, particularly Florida, and decreased it in other areas.

However, those two independent estimates converged, and there was about a 6 percent difference, and roughly seven million fish is the difference, and, if you look closely at those tables, which we'll present soon, you can see that.

To put it in a bigger perspective, this kind of captures it in a nutshell. With that reanalysis, actually, we originally were at 110 million fish, with an 11 percent CV. Based on the latest recommendations, that went up to 118 million fish, with a 15 percent CV, and, as we all know, and we've been discussing for the past few days, and certainly in the past, comparing to that SEDAR 52 stock assessment of thirty-six million fish.

This, obviously, spurred some question about Florida, and likely the question that the Science Center had about would we reconsider that random forest modeling for that Florida uncharacterized bottom, and Dr. Ahrens will talk about that here in just a second.

When you break it down, and you start looking at it by regions, and, of course, this is captured in the report, but, you know, very broadly, this is what we're looking at as we begin to break

it down. This is much more refined in Tables 5 and 7, which I will put up here in a minute, and you all have much more -- You have better copies that are much more resolved.

This is the Table 5 that I was telling you about, and I know it's hard to read here, in terms of when we really break down the total area and the number of samples, the area we sampled, the density, or the number of red snapper per structure, and essentially what we get in those associated CVs. I am certainly not going to sit here and talk through each line of that table, and I'm sure this will come out during questioning, and so I will go ahead and move on and save that for the question period.

However, this is the alternative analysis of Lynne's, which is even a little bit harder to see, and I would direct everyone down here to the very bottom. She's at 111 fish, with a 15 percent CV, but I want to draw your attention in these tables, as you're looking through them. We have the CV, but we also have what we call a conservative CV, and Lynne will talk about how we arrived at that CV later.

That is the overview, and I will stop there, to ask if Jessica could unmute Dr. Ahrens to talk about specifically how we addressed the reviewers' concerns in an analytical way, and that's kind of the thirty-thousand-foot snapshot. While she is unmuting Dr. Ahrens, I would draw your attention to this Florida panel on the right side.

What you're seeing, and what I just presented, was the briefing book and the review request of how that analysis was done, and then, right below it, is the Southeast Fisheries Science Center requested reanalysis, and, just to reiterate, that was only for Florida uncharacterized bottom. The rest remained the same, and we were able to run that during the past few days, and you received it late yesterday. Dr. Ahrens, if you're available, and hopefully unmuted, are you able to --

DR. ROB AHRENS: Good morning, everyone. Most of the changes that were requested had to deal with what was assumed as the sampling unit. In particular, over in Texas, we had acoustic trawls that, in the original estimate, each individual chunk of those trawls was taken as a unit, and we dealt with covariance, and we backed off from that, after discussions, and each individual acoustic transect was taken as the sampling unit, and the mean and the standard deviation of fish density from those acoustic transects was then taken as -- Was then used to calculate the appropriate metrics that were expanded up to the overall estimates.

In Texas, there was also some concern that the acoustic signatures, of course, were of all fish, and there was some other further studies where the proportion of red snapper in each of those acoustic signatures was assessed through visual surveys, and, of course, there was uncertainty associated with the proportion of red snapper, and so that occurred in Texas, in the central, north, and south regions. There were separate estimates of the proportion of red snapper, and so the variance associated with that was also combined into the estimate. Then, for the deepest strata in Texas, we still used the individual transects of visual surveys from the C-BASS visual surveys.

In Louisiana, there were some of the survey effort that Jay's group did that fell into Louisiana waters, and, therefore, that was incorporated into the estimate at this point, along with the proportions red snapper inferred from those additional investigations. Again, the transect itself was used as the sample and not the breaking down of the transect into chunks. Again, in Louisiana, the deep transects, the deep estimate, was taken from C-BASS visual counts on the unconsolidated bottom.

For Mississippi and Alabama, we, again, used the individual transects from C-BASS visual surveys to calculate that mean and standard deviation of red snapper density that was expanded out by the area.

For Florida, this was done in two ways. The original request was to eliminate the random forest classification that we had done, due to concerns that it was being overly optimistic on the accuracy of the estimate, and so we dropped that, and, therefore, the visual surveys done by ROVs, as Dr. Patterson's group had done, was at each individual point, and they were stratified by depth and sub-region, with the Big Bend area, kind of the central area, and the southern area as the sub-regions and by the three depth strata, and each count in those was treated as a random point, and the mean and the standard deviation of red snapper density was calculated for those and expanded up.

The re-request to re-look at the random forest model came up given the nature at which the samples were originally allocated, and the sampling effort was allocated across strata based on depth, sub-region, and random forest strata in the original, and that was the sampling protocol that Dr. Patterson's group had followed, and, therefore, excluding the random forest model was really a violation, to some degree, of the probability sampling method that had been used to allocate the samples, and so we brought that random forest back in, and then we used the mean and the variances associated with the twenty-seven new regions, as a result of the

random forest model. In some instances, in two instances, we had to impute information into strata that were not sampled.

For the hardbottom, in Texas, these, again, were done from acoustic transects, and each transect was paired with species composition over areas of known hardbottom, and, again, the sampling unit was the transect at that point and not sub-sections of that transect.

Information was borrowed, or imputed, from the Texas observations for Louisiana, and, for Mississippi and Alabama, there was an area of kind of known hardbottom features that were sub-sampled to get estimates for MaxN count from the fixed-area surveys, and then that was expanded up in an estimate of the number of hardbottom features expected over the entire area. Then, for Florida, it was not -- Hardbottom was not separated out, and, therefore, it falls within the unconsolidated bottom habitat.

For artificial reefs, again, in Texas, we had acoustic surveys on structures. For the larger structures, these were taken as individual units, and with the proportional estimate of red snapper on those, and those were used as the number of individuals on each of those larger units.

For smaller units, it was determined that roughly 3.5 of the smaller units were showing up in the acoustic images that were being created, and, therefore, 3.5 was used as the average number of small structures that were included, and then mean and standard deviations were calculated for each of those and then expanded up appropriately by the number of structures thought to be in the system.

Again, we used a similar approach, borrowing information from Texas for Louisiana. Then, in Mississippi and Alabama, we had red snapper density was calculated in stratified means by areas within kind of a known artificial structure region, as well as outside that known artificial structure region, and mean and variances were calculated on the units within those and expanded up by the appropriate number, and, again, uncertainty was incorporated, given the uncertainty in the number of units that exist in that region. In Florida, visual ROV counts were used on each individual structure, and mean and variances were calculated from those individually randomly-selected structures and expanded up to the total number.

Pipelines, pipelines were -- Densities on pipelines were estimated from C-BASS and visual counts. Again, mean and variances were calculated from individual transects along arc segments, and the arc segments were drawn at random from the BOEM database, and

expansions were made up, given the number and total length of pipe above a certain size category across the entire region, and so the pipelines was done as a Gulf-wide estimate and not broken down initially by region. I think that's it, Greg. Thank you.

DR. STUNZ: Okay. Thanks, Rob. Mr. Chairman, if it's okay with you, I would suggest that we hear from Dr. Stokes, and then I think that would sort of complete the overall update, and then, of course, the team is here for any questions, if that's okay with you.

CHAIRMAN NANCE: That would be perfect. Thank you.

DR. STUNZ: Okay, and I want to point out -- So, in your document that was sent around last night, there is four tables. There are two tables that are similar, other than, obviously, the values are different. There is Table 5 and Table 7. The two Table 5s represent the analysis that was requested by the expert review team, and then the second one represents the Southeast Fisheries Science Center's requested rerun.

That is what this is, which drops the estimate from 118 down to ninety-six, and everything is the same, except the last row of Florida there, where, essentially, there is a reduction of twenty million fish on the natural and uncharacterized bottom in Florida.

There is a similar one for Lynne's estimate, that you will hear about in just a minute, and how we were responsive to the review team, and, just to reiterate, we started with the random forest, and the review team asked us to remove the random forest, and, for the justification and reasons you heard in the request from the Science Center, we removed that random forest as a rerun, and so that's kind of how we are arriving here, and hopefully that's clear, and, if not, I am happy to explain it better.

Jessica, if you don't mind unmuting Dr. Stokes, she's up next to talk about the alternate analysis and some of these alternate ways to capture variance in conservative CVs and her estimation procedures for this project.

DR. LYNNE STOKES: I am going to talk about something that Rob already mentioned, but in a little bit more detail, about how we, about how I, at least, incorporated the extra variance from calibration in Texas, and then by imputation to Louisiana, and those were the main things that I am going to talk about in this presentation.

The Mississippi/Alabama variance was also adjusted to account for

estimation of the artificial reef population size, and so the revision is different -- It has a slightly different number for the variance in Alabama and Mississippi, but that I just -- I didn't personally do that. The team in Alabama and Mississippi incorporated the variance, the extra variance, from the estimated number of artificial reefs, in actually the very same way that I incorporated the extra variance in Texas, and so, in the next slide, you will see the formulas that both of us used.

Anyway, for Texas, the variance estimate, as Rob said, didn't capture -- The initial one we did didn't capture the variance component due to calibration in the uncharacterized bottom stratum, and so we did have was we found the data, or we obtained the data, on which that calibration was based, and it turned out that, for the different regions of uncharacterized bottom, north, south, and central, a different set of data was used to estimate the number of red snapper proportion from the overall number of fish, and then they simply multiplied that estimated fraction times the total fish count to arrive at a red snapper count in each of those regions, north, central, and south.

We post-stratified by north, central, south, because those fractions were different, and so we were applying a different calibration system, based on different independent data in each one, and so each was done independently, and so we were still able to -- Once we post-stratified, we were able to simply add up the variances for those three pieces.

Then Louisiana, since Louisiana was imputed from Texas in several cases, that additional variance was absorbed into the Louisiana estimates as well, and so those standard errors are going to be different, too.

Some of those changes increased -- Well, all of those increased the -- All those measurement error implementations increased the standard error. There were a few other changes that decreased the standard error, and they had to do with the Florida issue that Rob was talking about and that you just heard about, the ultimate changes in the stratification design. Anyway, the total abundance was reduced a bit, but, in the end, the CV was nearly unchanged, and it was increased a little bit, but not significantly.

This is just the method, and there's just a little more detail on how the calibration variance was incorporated into the standard error in Texas, and so the idea was simply that there was additional data that was used to calculate this calibration factor, which is simply a proportion, and so there were measures.

There were actual counts made of total fish and red snapper, and then simply a fraction was imputed, based on several observations, and let's say R observations, and that's from my notation here, and then the calibrations system is they simply averaged those estimated fractions within each region.

Then, to predict, I guess you would say, what the total number of red snapper were from the data collection system, they simply estimated this average proportion. They multiplied that by the observed number of fish and so you get a formula that looks like the one -- That sort of ugly formula that's the next-to-the-last bullet down there for the estimator, but the main point is that you can write the estimator as a product of two estimators, the total number of fish, which I have called $T_{hu,r}$, and the "r" means the region, and the "u" means fish, and not red snapper, and they took that estimate of total fish for a region and multiplied it by this calibration factor for the region, and they did that for each of the north, south, central regions.

If you're wanting to know the variance of that estimator, you have to work out the variance of a product, and, luckily, those two pieces of the product are independent, because they are made from separate data, and so the T-hat is made from the operation of the data collection during the main body of the data collection process. The P-hat was from a separate experiment where they got this calibration factor.

There is long-standing methods for calculating a standard error for this formula, and it's from 1962, and so I simply used that expression for the standard error for all the strata where this was the method used for calibration. That was on the uncharacterized bottom.

On the natural bottom in Texas, there was also a calibration method used, but I did not have access to all the data, and it was a different calibration process, and so what I did to try to guestimate at how much the standard error would be, and this is what Rob was talking about, sort of a conservative way to look at it, is I looked at the increase in variance, the multiplicative increase in variance, or the efficiency, the relative efficiency for the ones on uncharacterized bottom for all the regional strata, and they ranged actually --

The increase in variance for all those estimates was a relative efficiency of 1.01 to 2.77, and so what that means is that, for some of the regions, that extra calibration factor, I mean that calibration process, increased the variance by 2.77 times, and so that's a pretty big increase.

For others, it virtually did not change the variance, and so 1.01 was the smallest, and 2.77 was the largest, and so what I did, to just try to get an idea of what the effect would have been for natural bottom, was to just take that worst-case scenario, the 2.77, and say what if the calibration procedure on the natural bottom was as bad as the worst we saw for the unconsolidated, at 2.77 times the old variance.

Then I used that, and, if you look in Table 7, there is a column that says something like "conservative variance", and so that's all that is. For the ones where I didn't -- For the strata where I didn't actually have data, I simply substituted a multiplier for the variance that we got when I knew that calibration was done in that stratum, and so the increases in the variances are shown in Table 7 of the report. You can't really see it, but that was the result.

DR. STUNZ: Well, thank you, Dr. Stokes, and so this table that you have, as I keep mentioning, and I always tell my students don't present tables like this, but this is a placeholder, and you have the actual tables that you can read. It brought the Florida down to forty-seven million, which you see circled there in red, and you can also see the CV here, as well as the conservative CV that Lynne just explained, where it could be calculated, where possible.

That is the response to the reviewers, as well as the short rerun that we have. To really tie things up, Mr. Chairman, I guess the key takeaways from this is that this report has undergone exceptional rigorous peer review, obviously, and we're happy about that, in the sense that we feel like it's greatly improved and really refined our study and analyses in the end.

We feel strongly that we've met the concerns of the external review team and the SSC, and, clearly, in this short amount of time, we can't present everything, but it's captured there in our responses, and certainly the final report, as I mentioned.

I wanted to reiterate that this estimate is still very conservative and likely an underestimate. We took conservative measures at every step, and we really beat this horse to death during our review, but I just want to reiterate that, for the new folks, we removed some of these really high density points that, as I mentioned last time, we debated and debated over, and there were one or two samples that could really drive the entire estimate, and, for conservative sakes, we removed those.

We know about known fish that were outside of our prescribed depths

that the RFP specified, such as deep pinnacles off of Texas and Alabama and other areas that are not incorporated in here, and, of course, we've had a lot of concern and questions and review over the detectability in our acoustic surveys and visibility issues and that sort of thing, which typically, and especially some of the calibration studies that Dr. Patterson and Dr. Boswell did that showed that acoustics are underestimating that as well. I'm sure we'll have probably some more questions about that, and, typically, that underestimate was the visual or detectability constraints.

Also, our habitat types are not known with certainty. Improved mapping will definitely help that, but that's sort of outside the scope of this study, and we had to deal with what we had, and so, as we map better, and know habitats better, that estimate will certainly begin to improve, and then, as I mentioned, these known populations occurring outside the study area.

To end it here, Mr. Chairman, just some brief acknowledgements, and, of course, we appreciate the congressional appropriations and NOAA Sea Grant for leading this, NOAA Fisheries for developing the steering committee and a lot of guidance on that, and, of course, LaDon Swann's group at Alabama Sea Grant that helped with much of the administrative side of this and pushing it through, and we greatly appreciate the external reviewers, Doctors Christman and Cadrin and Eggleston, which went out of their way, in a way, to provide us with above and beyond, in terms of their constructive criticism.

That is the conclusion, Mr. Chairman, and our team is here to answer any questions. Since we're a little bit behind, I know some of our team may have to go, and particularly Dr. Patterson, I believe, has a class coming up in a little while, and so, if there's probably -- I don't want to tell you what to do, Mr. Chairman, but, if there's Florida questions that Dr. Patterson can answer, we might want to go with those first, while he's still here and available to do that, and certainly Dr. Ahrens can help with that, but Will was in charge of that region. Other than that, our team is here and available to answer any questions or comments.

CHAIRMAN NANCE: Thank you very much, and so we'll go ahead and start taking questions. Luiz.

DR. BARBIERI: Thank you, Mr. Chairman, and thank you, Greg, for that overview presentation, and so I'm going to take your advice, and, since Will will have to leave us, I have a few questions about Florida, some of the things that I am not completely understanding with the results.

Will, I am looking at the Florida numbers in the new table, Table 7, I believe, and I have to admit that it is surprising to me to see those densities in different areas, and so there is the northwest region, the mid region, and the southern region, and I am seeing that the densities in the southern region, which is not completely clear, and maybe you can help me see where the southern region starts over the West Florida Shelf, but it's about the same, or higher, than it is in the northwest region, the Florida Panhandle.

That doesn't seem to gibe with our expectations. I mean, all the survey data, all the fisheries landings data, all the observations that we have about red snapper distribution in the Gulf and densities of red snapper along the West Florida Shelf, the Big Bend, and the Panhandle, that doesn't really gibe with that, and looking at a map, a chart, that was produced by our fisheries-independent monitoring folks, looking at densities of red snapper along that whole area, it confirms my gut feeling expectations that the higher abundances and densities of red snapper in Florida is up in the Panhandle. Can you help me understand this part here, Will?

DR. PATTERSON: Hi, Luiz. It's an interesting observation and question. All I can say is that we followed the design that came from the random forest model, which was a random design, and then the stratification from the random forest model produced the estimates. Now, the table that you see here is not the updated table from Rob more recently, and this is --

CHAIRMAN NANCE: I think the updated table was sent in an email, and is that correct, Will?

DR. PATTERSON: Right, and I think this is actually Lynne's. I think what we're seeing here are Lynne's estimates, but that's fine, but you can see --

CHAIRMAN NANCE: Do you have that, Jessica?

DR. PATTERSON: You can see the differences here.

CHAIRMAN NANCE: This one has four different ones. If you go down, there's one that is red. Will, I think this is -- Is this the one you're talking about?

DR. STUNZ: That's it, Jim.

DR. PATTERSON: This is the sort of synopsis table, but, if you go

down to the one below, I think it has the details that Luiz is talking about.

CHAIRMAN NANCE: Yes. Okay. Perfect. So this is the updated one.

DR. PATTERSON: Yes. Part of the reason for the discrepancy that Luiz is talking about, and Rob can speak to this more completely, is that, in following the recommendations that came from the initial peer review, we stratified simply based on depth. However, through conversations that Rob had with John Walter, and some internal conversations within the Center, they questioned whether the data were randomly distributed by depth strata, and Rob confirmed that in fact the stratification done in the original random forest modeling sample size selection was by depth stratum and habitat type.

When that was recomputed with the original stratification, we got these lower estimates, relative to the ones that you were just pointing out from the report, and so our apologies for the confusion here, and we think this is the appropriate approach, but it changes the perception of what Luiz just mentioned.

DR. BARBIERI: Thank you for that, Will, because I am looking here, if I may, Mr. Chairman, just as a quick follow-up.

CHAIRMAN NANCE: Yes.

DR. BARBIERI: Looking at this table here, Will, I am seeing like the estimate is a little over sixty-six million red snapper for Florida, and more than half of those are in the mid-region, that I would expect to be between Tampa Bay and the Big Bend, and, in the mid-region in the shallow zone, about thirty-eight million fish.

CHAIRMAN NANCE: You're looking at the old table, Luiz.

DR. BARBIERI: I am confused then.

DR. AHRENS: Mr. Chair, can I maybe help?

DR. PATTERSON: Yes, please.

DR. AHRENS: Luiz, you are correct that, with the removal of the random forest component of the stratification, the fact that there is so much real estate in that central region of the West Florida Shelf, the mean density that is calculated in that shallow depth stratum between ten to forty meters gets expanded up to very large

numbers of fish.

This was one of the concerns that was raised about just the visual aspect of, if you plot up those sample locations in that ten to forty-meter depth range, they do not appear random with respect to depth, and it's not a nice, random distribution, and that's simply because those samples were originally allocated using the depth range as well as our random forest classification of low probability of snapper, mid probability of snapper occurrence, and high probability of snapper occurrence.

When we moved back to including those, the stratification with the random forest, which we therefore are random with respect to that stratification process, we break out those density estimates a bit more, and they're not getting expanded as much as they do when you get rid of the random forest classification, and that's where you lose the twenty million fish, basically, simply because that mean density estimate is not being expanded up by such a large number anymore.

DR. BARBIERI: Right. Thank you, Rob. That helps a lot. That helps a lot, but, still, and I am just trying to kind of align the results of this with what we know about red snapper from years and years of survey data over the West Florida Shelf and how the fisheries operate in those areas, right, and the sizes and numbers that you obtained from different areas.

I am finding that one of the issues here, and it's understandable, Rob, and I understand that, for a study of this nature, it's difficult to account for all of these problems, and, when you try to go from site-specific densities, and you do a linear interpolation, and you expand that to areas that have not been sampled, and for which you don't actually know what habitats you have there, you might end up with large margins of error that don't seem to align with our expectations for red snapper abundance in those areas. Does that make sense?

DR. AHRENS: Mr. Chair, can I respond to that?

CHAIRMAN NANCE: Absolutely.

DR. AHRENS: Thank you. Luiz, I think you bring up a really good point, and I think there's two things to say. One, five years ago is when we designed the survey, and I think we know a lot more about the distribution of red snapper now than we did then, and I know one of the concerns, if we look at that central region shallow-water habitat, is that some of the other survey methods are suggesting there are not red snapper in that -- In particularly

the shallower habitat, the ten-meter to twenty-meter band.

It is possible that, knowing what we know now, we would design a different sampling program, and I certainly think we would have liked to have had more samples in Florida, now that we understand what a significant influence it can have over the estimate, and I think, the report and the work that was done by this group aside, I think there are opportunities, given what we know from other sampling programs, to take a closer look at some of the Florida data, if we wanted to, and, using those alternative information sources, perhaps do some additional post hoc stratification of the data, to see how that might influence the results. Again, I think that would be an activity outside of this report that's been produced.

DR. STOKES: Can I say something?

CHAIRMAN NANCE: Yes, please.

DR. STOKES: The fact that the strata were not drawn as one might draw them now would affect the variance, but it would not affect -- It would not make the estimate biased, and it would not make it systematically higher or lower, and so, in fact, it really doesn't -- The purpose of drawing strata is so that the samples that you get have less variability within the stratum, and, that way, when you add up the estimates across all the strata, the variance would be smaller, if you've done a good job of drawing strata.

If you haven't done a good job of drawing strata, it simply means that the variability within the stratum is just like it is overall, and so you haven't really gained anything by stratifying.

If the samples were drawn randomly within the strata, then the estimate may be too big, and it also may be too small. The chance of each is 50/50 if the samples have been drawn randomly, and, furthermore, the standard error that you compute, again if within the strata the units have been -- If the transects have been drawn randomly, what you can say is that the chance that it is too big by a giant amount, that is twice the standard error, is about 25 percent, I mean 2.5 percent. I'm sorry. It's 2.5 percent.

In answer to the question of is it possible that this is way bigger than it should be, the answer is it's possible whenever you're doing sampling, whenever you're making conclusions based on a sample rather than the whole census, but the beauty of random sampling is that that miss is, with a very high chance, controlled to the size of your standard error, twice the size of your standard error if you're in a 95 percent interval, and so the key thing is

whether or not these transects were chosen randomly within whatever stratification scheme was determined at the beginning.

CHAIRMAN NANCE: Luiz, to that?

DR. BARBIERI: Thank you, Lynne. I think this is spot-on, and it helps a lot, but I am also thinking about the fact that, because we are using the areas within the strata, or whatever blocks we are setting up as the areas for the expansion factor, unless we have enough samples from within that box to really capture the variability within the box, and so there is a minimum sample size that's needed, right, to capture that variability. Otherwise, we assume that everything inside that box is somewhat uniform and that I can just expand that.

This is my concern here, and it's not critical of the team. I mean, the team did what they were able to do, given the resources and the time that they were provided to conduct the study, and this is fine, but what I feel is that, now that we know more, and I think Rob brought this up, but, now that we know more about this area, the more we know, the more we can actually set up sampling programs that can start refining the process of how we sample to generate -- Because the issue of generating an absolute abundance, based on these local estimates, can create -- It's necessary to expand, but that expansion increases the probability of error. You don't have enough samples in there, right?

DR. STOKES: Well, actually, that's not true. It does make the margin of error bigger if you don't have a large sample, and that is absolutely correct, but it doesn't depend on uniformity within the stratum. The units within a stratum can be -- I mean, the correctness of the standard error calculation doesn't depend at all on the units within the stratum being uniform. They can -- As long as your sample is selected randomly within the stratum, then, if they are very variable, then your sample should reflect that, and so then what does that do? It makes your standard error bigger.

What you're saying is that the estimate might be too big, because it's quite variable, and that's true, but it should be reflected in the size of the standard error. I guess I would say that it is true that some of these variances within some of the strata are large, and, if you had to redo the design, you could do a better job, because now you know where the variability is, and, frankly, you know where it's very hard to estimate variance when the density is real low, and so, as you see, for some of these strata, you have --

They saw no fish, and so, in those cases, the variance is going to be small, and so, if you were to do it again, you would probably increase the sample size, to get rid of the chance that you get no fish in any of the transects, and so I do believe that you could improve the design if you did it again, because you know a lot more now about how to make it efficient, but there is no guarantee that, if you did it, that you would get a larger number or a smaller number. It should be unbiased, and it's just that you could do a better job of getting an estimate with a smaller variability, with hindsight.

CHAIRMAN NANCE: Thank you. Paul.

DR. MICKLE: Mr. Chair, I will defer to Will Patterson. My question is not for Dr. Patterson, and so I will wait.

CHAIRMAN NANCE: Okay. Harry.

MR. BLANCHET: This is kind of to follow-up on Luiz's point. I was looking more at the density values rather than the final numbers, but, looking at the densities in that mid-region, and I realize that they are what they are, and, in terms of sample numbers, the mid-region shallow, with the middle probability of red snapper, has one of the highest numbers of samples in the whole group, but still those values in the mid-region don't seem that different than the northwest region, and, in some cases, they're higher than the northwest region.

Now, I realize that this is parsing this down to a really fine thing, and so maybe I'm just getting too far into the weeds, and I think what I would end up saying is very much like what we said yesterday and what other folks have said both days, is that this really is a good starting point for really getting into getting a better handle on how to capture an estimate of red snapper, rather than an ending point.

CHAIRMAN NANCE: Okay. Thank you, Harry. Mandy.

DR. KARNAUSKAS: Thanks, Mr. Chair. Thanks to the team for all their work on this and all the explanations and reanalysis, and I appreciate the effort. I don't want to belabor Luiz's and Harry's points on sort of aligning these numbers with our general expectations, but I do think that the point Rob brought up about the real estate is an important one, because the shelf area is something we know with relative certainty, and, in the 2017 paper that we did, we had to calculate the total area that is covered by a depth seven to 140 meters, which was our presumed red snapper habitat area, and we came up with Florida covers approximately 49

percent of the area.

Your default assumption, if you knew nothing about red snapper density, would be that Florida has about half the red snapper in the Gulf, and, if I'm looking at the right final estimates, it looks like we've got forty-seven million in Florida and forty-four million in the other states, and so that's approximately half and half, but all of our available survey data, I think everything from the bottom longline to the plankton surveys, would suggest that red snapper densities in the western Gulf are quite a bit higher, and so, again it just -- I don't see how the numbers, the final numbers, gibe with the expectations and the information that we have, and so thanks for letting me put that out there.

CHAIRMAN NANCE: You bet. Will.

DR. PATTERSON: Mandy raises an interesting point, and, obviously, Luiz had spoken to this earlier, and so we have really three components here. We have the stratification and how that expansion is done statistically, and we have the area for each of the individual strata, and then we have the method for estimating the density of red snapper.

Rob has talked about the design, and part of the design, from the random forest, is the classification of low, medium, and high probability of encountering red snapper, and so that affects the areal coverage of those various strata.

Then the last component then is producing the density estimates for scaling up from individual samples, and Luiz is right that you could look at the sample sizes here and question whether they were adequate, but the samples were randomly taken, and then the CVs reflect the uncertainty in the process error in the distribution of red snapper within these strata.

These were uncharacterized bottoms in Florida, and that's different than unstructured habitat in the western Gulf, and so some of this habitat, like in the northwestern Gulf, large, expansive areas, are sand. They're not soft corals and sponges, or even higher-profile natural reefs that we see in, one, the West Florida Shelf or farther to the south.

Uncharacterized here is catching a lot of things that aren't necessarily the same, and I think what Rob was speaking to about, given the information that we've gained by these 930 samples, or 830 samples, across the Florida Shelf, the stratification could be done differently if this were ever attempted again, but, as far as the estimates of red snapper density, in Florida, these were all

done with ROV.

We presented at the previous, at the peer review, some work that we had done to try to test red snapper behavioral reaction to the ROVs, and our conclusions, from the paper, which now the revisions have been submitted, and our conclusions were that red snapper had a more or less neutral reaction to the ROVs, but the reaction was different near the ROV versus far from the ROV.

We did attempt to examine this, but we had a couple of caveats that we had mentioned at the time, is that our work was done at artificial reefs, and the reason for that was because we could reliably predict that there would be red snapper present on Panhandle artificial reefs. If the artificial reef structure itself affected red snapper behavior, then that would affect the results, or affect our conclusions anyway, from the results.

The second caveat was that we had to deploy the camera rig and the sonar rig that we put on the bottom ahead of any of the survey gear, and so if doing that and having divers in the water to position those gears affected the red snapper distribution, and they weren't able to settle down completely between when that was done and the hour later, when we actually did the sampling, that could have affected our estimates as well.

I just wanted to point out that these are important issues, but this component of density -- We attempted to test for that, and our perceptions, from the data and the analysis, is that red snapper reaction to the mini ROVs that we utilized was more or less neutral, and maybe slightly positive, but, again, we attempted to account for that in the study and the analysis.

CHAIRMAN NANCE: Thank you, Will. John.

DR. JOHN HOENIG: Good morning. I wanted to go back to Luiz's point, and maybe it's beating a dead horse, but what Lynne said was completely correct, and, just to expand on that a little bit, you get unbiasedness if your sample selection is random, and you get representativeness, meaning low variance, if your sample size is large.

In determining how to sample strata, what you want to do -- Say you have a certain number of stations that you can afford, and you want to allocate them between two strata, and you assume that the cost of sampling in one stratum is the same as the other, then you would basically make your sample size in each stratum proportional to the inherent variability, the variance within the stratum, and the size of the stratum.

Luiz is completely correct that the size of the stratum has an important influence, and that's what happened with the unconsolidated bottom, where we all focused on, oh, the variance is going to be low, and there are not many there, and didn't give enough thought to the size of the strata. It's huge, and so your expansion factor is huge, and so, when we wound up with more variability within the unconsolidated bottom, because, among all the zeroes we were expecting, there was also some red snapper there, and that kind of sank us, in terms of having a huge expansion in there for a lot of uncertainty.

The other thing I wanted to point out is that we are talking about the Great Red Snapper Count in very detailed fashion, about, well, did you do optimum allocation, and could you have stratified better, and could you have allocated samples to stratum more efficiently, and I compare that to yesterday, where we had a presentation by LGL, and they compared their results to the Great Red Snapper Count, and we were not allowed to comment on that, except if we were members of the SSC, and, really, the LGL study is flawed.

Apparently they did not use random sampling, and so that renders their results little more than anecdotal, and I went there, because I thought that's where I should go and hear my results, but there is no randomization, and so you have no known properties, and who knows what the results mean, and, also, they presented a statistical model without any reference to the literature, and apparently it's ad hoc, and they gave enough detail to basically tell me that what they did was flawed, because they have two sources of variability, the variability from one station to the next, and then they have variability within a station, where they are fitting a model, and their variance did not calculate the effects of the model being slightly wrong.

They really need to read up about conditional variance, or, equivalently, they could look at two-stage cluster sampling, and they would see that, oh, they're two different components, and you have to estimate both of them, and they didn't, and so their variances don't mean anything.

Between not random sampling and inappropriate analysis, you should not be accepting those results, and you should not be comparing them to the Great Red Snapper Count. It's just not a reasonable thing to do. I would suggest that the SSC get a proper review of the LGL study before they accept them, and they should not do any comparisons with the Great Red Snapper Count until they're sure that what was done was in fact legitimate.

CHAIRMAN NANCE: Thank you, John. We're certainly looking into the randomization of those samples, and we asked for that, and so we'll be able to determine that at a future date.

DR. HOENIG: But you need to do more than that, because, if the methodology is not sound, the results are meaningless, and --

CHAIRMAN NANCE: We will certainly look at that, and I appreciate that, John.

DR. HOENIG: You didn't want to hear us yesterday, but that doesn't mean that there isn't a problem there.

CHAIRMAN NANCE: Okay. Paul.

DR. MICKLE: I don't know if Will is on or not, and it's fine, but the question is kind of to Will and to Greg and the group that executed the sampling. The area per strata that was brought up, that Luiz talked about in those great expanses, and I think it was the mid depths off of Florida, midway down Florida, and is that -- Okay. I am trying to get it correct.

In my opinion, at least to this point, in the beginning of this, and the large expanses you see -- I would think a power analysis, and you're at the point now, and this is for future work, I guess, but now you have variances and uncertainties in a spatial realm, and so now you can actually do a power analysis, to find out how many samples you would need to actually have some sort of grasp on the reality of the variance.

Right now, I feel, in these large expanses, the reality of these variances are hard to grasp. Whether they violate some assumption or something like that, and I've been digging around and seeing, but it seems like these survey statistics, they just -- They don't need -- There is no base minimum amount per area or per universe that you need, and it creates these massive issues in calibration, and I have a few other questions, but they are not -- They are toward, I guess, the process of this review, and I will hold off. Thank you.

CHAIRMAN NANCE: Okay. Thank you.

DR. STUNZ: Mr. Chairman, could I comment to that point of Dr. Mickle's, please?

CHAIRMAN NANCE: Yes, please.

DR. STUNZ: Thank you. I will be brief, and, Paul, that's a great point, and I just wanted to remind everyone, and, I mean, I clearly hear Mandy and Luiz's comments, and, when we went into this, certainly, from a broader perspective -- You know, we were asked to sample in areas that hadn't been sampled, in terms of the uncharacterized bottom, and I know you all have your bottom longline there, but that is very different.

We were guided by areas that we had little information, if any, on the abundance and that variability, and so what Paul is bringing up about the ability to do a power analysis and being able to adequately allocate samples, we just didn't have that information going in, although the RFP, and many of us, for years, have speculated that there's fish on this uncharacterized bottom that are holding on structures, probably, but we didn't have that -- What we have now to guide that.

Really, Paul, I think hindsight is 20/20, and, yes, I agree that now we do have that ability to do some more sophisticated power analysis, with much better data, but we weren't afforded that in the beginning, and we just had to do that, and, had we known we were going to see this many fish, of course, we would have allocated more effort, but, you know, we didn't know that going in, and so I just wanted to make sure that that was clear, in that we were asked to sample in areas that had not, essentially, been sampled, for the most part.

DR. STOKES: May I say something?

CHAIRMAN NANCE: Yes.

DR. STOKES: You don't do a power analysis unless you're doing a hypothesis test, and so this wasn't a situation where a power analysis is needed. What was needed was an estimation of precision and a sample size allocation based on that precision. It's the same idea, and so it's not that a step was skipped. That step -- That analogous thing to -- The analysis procedure for estimation, as opposed to hypothesis testing, is sample size determination and allocation, and that is no less scientific.

I had the feeling that you were saying, oh, samplers don't really do anything scientific, and they don't do a power analysis, and we don't need to do a power analysis unless you're doing a hypothesis test. The problem was that variances, as has been repeatedly said, the variances are not known, were not known before, within the various strata, and so that is what could be improved next time, but it's not that next time we do a power analysis, but it's that next time we would have better variances to do the sample size

determination and allocation. Thanks.

DR. STUNZ: Jim, I would add to that a main issue we have, in general, and still have, and will continue to have, is the level of mapping, and so, kind of to Luiz's point, there may be, especially in Florida -- The way the geography and habitats are laid out over there on the uncharacterized bottom, there's probably a lot of rubbly-type structures that are holding snapper that we just don't even know about, that we can't map or even direct sampling, and so that hinders us a little bit, when we don't have high-resolution mapping of the seafloor.

CHAIRMAN NANCE: Yes.

DR. AHRENS: Mr. Chair, could I just quickly say one thing?

CHAIRMAN NANCE: Yes, please.

DR. AHRENS: In the initial design -- I really appreciate the comments, and I just wanted to let everyone know that, in the initial design, the information that we had on variances had come from previous scientific studies, and so, in the initial sample allocation, we had used CVs on kind of hardbottom habitats of about 95 percent, and then we assumed, on the unconsolidated, it was 150 percent.

In some instances, in some of those habitats, the CVs on the samples are upwards of 300 percent, and so certainly the sample allocation would have been done differently, given what we know now.

DR. PATTERSON: Jim, can I speak to that? I have to take off here in just a few minutes.

CHAIRMAN NANCE: Yes, Will. I was going to call on you. Go ahead.

DR. PATTERSON: Okay. Thank you. Sorry, John. Also, another thing to note is that, in the initial design for this, that we put together the proposal and budgeted for, there were actually about half as many natural bottom, or uncharacterized bottom, sites in Florida. Then, when we did the stratification, to actually decide where to go sample with the ROV, we had some new information that changed our perception of variance, and we nearly doubled the number of sites that we had to sample in Florida.

It really created quite an issue for my sampling teams in Florida, but they got it done for the same money that we had originally budgeted, and then I just want to make one more statement here

about all of these issues.

We have statistical uncertainty, which I think we can account for here, and, obviously, there have been some changes made, based on recommendations that came out of the review, principally from Mary Christman, but also from Doctors Cadrin and Eggleston, and so we have a random design, and we're talking about Florida here, and so we have this random design from the random forest, and we have the variance estimates that were produced from that.

Now, we can say, definitively, that these weren't haphazardly collected samples, and always sample sizes are a concern, but the design predicted a CV less than 30, and we produced a CV of 22 percent, or 0.22, and so we actually -- Our design, it worked.

Now, the issue about the gear and detectability, or attracting fish, those types of biases we attempted to deal with, and we didn't just ignore that, but, yes, those are always going to be concerns with optical gear and with sonar gear, and red snapper may be the ideal species to use those types of gears with, and so, as these programs get pushed forward, if we continue to have congressional funding for these amazing, large-scale comprehensive surveys, we really need to think hard about the types of sampling that's done and whether optical and sonar approaches are appropriate, even for a fish like red snapper. I mean, obviously, concerns have been raised here, even though we attempted to deal with some of those issues in the study.

CHAIRMAN NANCE: Thank you. Any questions now from the SSC for Will, because he's going to be leaving here? Probably your students wouldn't mind though, Will.

DR. STOKES: I would like to say something, again, that's related to what was just said.

CHAIRMAN NANCE: Okay. To that point, Lynne.

DR. STOKES: The requirement was a 30 percent CV on the entire Gulf, and there was no requirement for individual states, and so it's always dangerous when you have a design that's designed to achieve a certain precision at one level and then you start worrying about it at a much smaller level.

There's a lot of -- If the instruction had been that you need a CV of so-and-so on each and every state, well, it would have been a lot more expensive, but then you could have -- Then you could have more, I guess, safely made -- Sort of saying this stratum has a number that's bigger than I expected it to be, but that would be

quite expensive, when you have as many strata as we have here, and so, next time, maybe that's what people want, because they want -- It's sort of hard not to start interpreting at the state level, but just keep in mind that that wasn't the objective of the design.

CHAIRMAN NANCE: Okay. Thank you.

DR. PATTERSON: Jim, can I just respond to that real quick, before I have to take off?

CHAIRMAN NANCE: Yes, Will.

DR. PATTERSON: Rob's design was Gulf-wide, and it was to produce a CV of 30 percent. The design wasn't followed in every region, for local concerns that were talked about in the previous presentation of the results of this study, and so I didn't mean to misspeak and say that we were focused strictly on that number in Florida. However, all of the regions produced estimates that were beneath that threshold in this study.

CHAIRMAN NANCE: Okay. Thank you, Will. John Walter, please.

DR. WALTER: Thank you, Mr. Chair. I think this brings me back to kind of one of the first principles of evaluating your data, and it's to plot your data, and I think we have never seen an actual map of the distribution of where the estimates are putting the fish in space, and I think that -- I have not seen a comprehensive map of where the samples are, from the raw data from the survey are, and I think that's really key to being able to make inference as to whether the sampling is representative and whether the estimates are making sense relative to other things we know.

What I guess I would request is to see those maps of the samples and then a map of where the fish are, and, in particular, I note that the counts put most of the fish in the shallow-depth bin, which we always -- We aren't surprised that there's cryptic biomass out there, but we kind of thought that was more in the deeper areas and not in the shallowest depth bin, and I think that's kind of one of the questions and the real dichotomy between other surveys, particularly like the visual, the reef video surveys we have, that don't put a lot fish in ten to twenty meters. That is -- Can we get those maps? I think we need to see them.

DR. STUNZ: Mr. Chairman, to that point?

CHAIRMAN NANCE: Yes, please.

DR. STUNZ: John, thanks, and that's a good point, and I could

envision a heat map as well that includes the results on that, as well as the sampling, and that is in the report, in the appendices, and it's probably not just readily right there and apparent, but we're more than happy to do that, and, obviously, that's not something I can produce today, but I also want to just caution you, because we fall into this trap a lot, too.

Maybe we shouldn't have used "shallow" as a descriptive term, but it is the shallowest of the strata. Depending upon where you are, especially on the central Gulf coast, that shallow strata extends, and it's not a narrow fringing area like it might be in some places in Florida and other regions, where it's just right along the shoreline. That extends out, in some ways, well into federal waters, in many cases.

I guess I would just be very careful about, when we say "shallow", kind of what comes to mind. Now, I do know, in terms of the depth strata, that ten to forty, which was the prescribed depth strata we used, there is -- That can be a difference, potentially, but we certainly can -- That would be a good exercise, to look at those maps.

DR. AHRENS: Mr. Chair, could I --

CHAIRMAN NANCE: Yes.

DR. AHRENS: At the end of Greg Stunz's presentation, we have a map that maybe he could throw up and people could look at.

DR. STUNZ: Mr. Chairman, it's probably on the one that I have, and I'm probably going to have to share my screen again, and sorry that I keep jumping in, Mr. Chairman, but I don't have a raise-hand feature, for some reason, this time.

CHAIRMAN NANCE: It's because you're a presenter, and so you're welcome to come in, Greg. That's fine.

DR. STUNZ: Okay. Give me a second here. I've got to pull it back up.

DR. AHRENS: Stop at the first of the images of the full Gulf, Greg, and then we'll go to the Florida-specific one. I just thought that it might be helpful, and these are the results of Dr. Zack Sider's at the University of Florida random forest model, and I just want to say that this incorporated all the fishery-dependent and independent data that was available at the time, and so it is, I think, one of the more interesting portrayal of the probability of presence of red snapper on a Gulf-wide scale, and so I will

just leave it at that, but we can go to the next one, Greg.

This is kind of, I think, what Dr. Walter is asking for, in terms of kind of the distribution of sample points and how they relate to the various strata that were sampled, and we certainly, I think, could produce these types of maps, and maybe it would be helpful to put the depth contours, or at least the strata, on there, but maybe we can leave this up for a second, Greg, and see if John has any other additional comments on this.

CHAIRMAN NANCE: John, any comments on this?

DR. WALTER: Mr. Chair, I guess what I would point out is the sparsity of samples in like the ten to twenty-meter depth bin and how we're saying that the density is the same for ten to forty as the ten to twenty, and, as Rob pointed out, that's a lot of real estate that we're putting fish in that other sampling doesn't see fish, and I think that's kind of the potential concern with using this, or interpreting it, is that like all of the really light-shaded ten to twenty-meter depth, there's about like maybe five samples.

At least off of Tampa, I'm counting four, and so that's then -- Basically, we're using the samples from the twenty-five to forty-meter, that depth contour out further, where there appears to be higher densities, and then saying that it applies in the shallower ten to twenty.

I think it gets to whether that is something we would want to say, that, yes, indeed, there is that much cryptic biomass in those shallow areas, and, if you cut that area in half, you would then potentially cut a lot of the overall estimate in half, if you said that the habitat for red snapper was actually deeper. I think that's where we could overlay the FWRI sampling, that does really well cover that area, and determine if indeed they are seeing red snapper in those shallow areas.

CHAIRMAN NANCE: I see what you're saying. Greg, does anyone want to comment?

DR. AHRENS: Mr. Chair, if I could.

CHAIRMAN NANCE: Yes, please.

DR. AHRENS: I am not going to disagree with you, John, but, because the sample size in that shallow-water habitat is low, and, in fact, throughout Florida, in that region, we have twenty-six, I believe, samples in the ten to twenty-meter range, and so

certainly that is something to think about, but, back to the point, again, that Lynne brought up, and, in theory, if those are truly random in there, then the mean for that full depth stratum should be unbiased.

Now, we do -- We do certainly have significantly more samples in the twenty to forty-meter, and so the influence of those shallower samples on the mean, where do see zero fish, and we do get zero fish in the observations that Will Patterson, Dr. Patterson, had produced. If you kind of separated them out, there may be less of an effect on influencing the overall mean for that depth, but, in theory, because they were randomly selected, it is an unbiased estimate of that mean density, but I agree that it's something that we might want to look at a little more carefully.

CHAIRMAN NANCE: Is that area its own strata though?

DR. AHRENS: The strata was ten meters to forty meters, and, in Dr. Patterson's surveys, he sees -- Out of the twenty-six surveys that occurred in the ten to twenty-meter strata, no fish were seen, which aligns with what is being seen in some of the other sampling programs.

CHAIRMAN NANCE: Okay. Thank you.

DR. STUNZ: I will go ahead and stop sharing then, Mr. Chairman, if you're okay with that.

CHAIRMAN NANCE: Yes. Thank you. I appreciate that, Greg.

DR. CRABTREE: Greg, that figure is in the final report somewhere?

DR. STUNZ: Yes, Roy. In fact, a lot of these maps that John is requesting do occur in the final report, but they're just probably not right upfront, and they are in different appendices and that sort of thing, but, yes, Roy, that's the case.

DR. CRABTREE: Okay. Thanks, Greg.

CHAIRMAN NANCE: Thank you, Greg. John Hoenig, did you have a comment for the analysis?

DR. HOENIG: I did, but Lynne Stokes beat me to it, and so there's no point in repeating what she said.

CHAIRMAN NANCE: Okay. Thank you, John. Any other -- I guess Will is gone now, but, from the SSC, any discussion on the analysis itself? Roy.

DR. CRABTREE: I guess my question, Greg, is so you have the revised Florida estimates, and are you going to revise the report, or how do you intend to handle that?

DR. STUNZ: Well, Roy, that's a good question, and I don't have an answer for you right now. That request came this weekend, and we worked diligently to get it done, just a few days ago, and so we have not -- Much of the team, other than the Florida contingent, doesn't even really know about that, and so we need to figure out what the strategy is.

We produced the final report based on the external review and our guidance from Sea Grant and all that, and we thought we were done, and so now this changes things, and so I don't know, Roy, is what it comes down to. We could do that, and it's not some heavy lift, and it's not a bunch of text or something, but then a question, and what if another analysis -- At some point, I guess, from our perspective, and we're not state or federal employees, and we were contracted to do this, and we spent that money a long time ago.

I don't know, and maybe this is a broader question, Roy, for the SSC or the Science Center in general, but we're not -- I mean, we're more than willing to help and contribute and help explain these data in the future, but, at some point, we have to draw the line at, okay, well, where is the report, and then the subsequent analysis is just that.

I mean, we'll be analyzing this stuff, I'm sure, for years to come, including all the things that aren't even the subject of the abundance piece, and so that's a long-winded, Roy, that I don't have a good answer. We would be open to suggestions, and we can get back with everyone on that.

DR. CRABTREE: Yes, and I'm not sure what the best way to do it is, but we do need some way to document that, okay, this is the final report, but there is a revision to Florida numbers done after the final report, and we need some way to document that.

DR. HOENIG: If I may, the report, as it stands now, has been peer reviewed, and we can't undo that by putting in non-peer-reviewed analyses instead. What we can do is add additional analyses, so that the peer-reviewed report stands as it is, and we have some additional thoughts, some additional analyses, that would get either appended to the report or issued as a separate report.

DR. STUNZ: To that point, Mr. Chairman, the review team technically reviewed this, but that's what they didn't like. Then

they asked us to do the random stratified approach, and we discovered that, well, maybe that wasn't the best way to do that, and so, I mean, Roy, I agree with you. I think we just need to -- I mean, we're documenting it now, and this is part of the public record, but maybe in a more formal way or something, and we probably need to document that as other analyses come out it.

I mean, John Walter was talking, just now, about building in some other -- I can't remember if it was FWC or what other data there was to supplement some of that, and that could be another analysis, and so there probably needs to be a way to archive that and historically track those changes through time, because let me tell you that is no small undertaking, and it gets very confusing very fast.

CHAIRMAN NANCE: Certainly, during the SEDAR process, that would occur, I think. John Walter, please.

DR. WALTER: Thanks. I just wanted to bring up two points, one about the unbiasedness, and that's an expectation, and, yes, if we redid this multiple times, it would be unbiased in expectation, but we've only got one realization of the sampling, and that's where we're left with trying to interpret that for what that means, and I think that the distribution of samples is rather sparse in some of the large parts of the real estate.

The other point that I think needs to be made, that wasn't made, and it relates to this recalculation aspect, and it wasn't just a minor request to recalculate, and, in fact, because the sampling design selected samples using depth and habitat type, you have to honor the sampling design when you calculate the overall totals, and that was what was missing prior to this and why the recalculation needed to be made, and I think that's a key point, is you've got to honor the sampling design when you get the total, and so that's where -- I just wanted to point that out. Thank you.

CHAIRMAN NANCE: Absolutely, and I think the key is we've got the final report, and, as has been mentioned, that's been peer reviewed, but, as these other analyses are done, and those numbers are available, we need to make sure that those are captured, so that we have a good documentation of all of the different things that have been done and probably will be done in the future. Rob Ahrens, please.

DR. AHRENS: Thank you, Mr. Chair, and this builds off of what John just said. I think, if we look at the original concerns from the reviewers, it was the use, or the reallocation of samples, to

the random forest design post hoc for areas where the sampling protocol had changed, and that being everywhere but Florida.

I think there was a lack of clarity, likely on my part, when the information was originally presented, in that I would assume that the review team, knowing that the probability sampling was based on depth and random forest in Florida, would not have requested the removal of the random forest for Florida itself, but certainly it was a reasonable request to remove the post hoc use of the random forest from the other regions, given the nature of the probability sampling that occurred in those other regions. Thank you.

CHAIRMAN NANCE: Okay. Any other general discussion on this presentation and the analyses that have been done? Okay. From a standpoint of moving forward with this, do we want to recommend that this is available to be utilized by the Southeast Fisheries Science for consideration in the red snapper SEDAR 74? Roy.

DR. CRABTREE: I mean, I certainly think this will be looked at as part of the SEDAR assessment that's coming up, and hopefully a lot of these issues and questions and things will be worked through and looked at in a great deal of detail, and they will figure out how best to integrate all of this into, ultimately, stock status and catch levels and those kinds of things.

What strikes me now is -- I mean, one, I guess, because we have these new Florida estimates, and there are some questions there, that needs to be -- To make sure that everybody is comfortable that those have been addressed, and then apparently there are issues with respect to the LGL study and some of those things that I think need to be hashed out, or looked at, somehow, and I don't think we're going to resolve any of that here today.

I mean, what I see with all of this, ultimately, is there are more red snapper out there than we previously thought, or that the assessment indicated, and that seems pretty clear to me, and I'm not surprised that there is a biomass of red snapper out on this unconsolidated bottom, and I have had fishermen tell me that for years, and the longline survey has shown that there are fish out in these areas.

I wouldn't have guessed there are as many fish out there as there are, but, you know, who knows? We all have these preconceived notions, but I think they're largely based on what we're used to thinking, but you can't get around the fact that, one, the estimates and things have moved around a little bit, and so we need to get through that, and then we do have these big areas of

bottom that are very sparsely sampled.

I get what the statisticians are all telling us about biases and CVs and all those kinds of things, and I'm sure that's all correct, but, still, it doesn't really give you a warm, fuzzy feeling that we know, with a great deal of comfort, what's going on there, and then the other thing is, even though the CVs are properly calculated and all those things, in my experience with this stuff, there's always a whole host of uncertainties, whether it's -- I think Will brought up how the fish react to the cameras and divers in the area and a whole host of things like that that are going on that really don't get captured in them.

I am quite certain that, if you looked at the true uncertainty of all of this, it's greater than we think, because that's really the case, and so, at some point, you've got to get to, okay, there's more fish out there than we thought, but how do we get to a catch level that properly accounts for all of these uncertainties, and maybe it's a higher level than where we've been, but how much higher, and what level would adequately take that all into account, so that we could be confident that we were moving forward with a solid recommendation.

The path to how you get to that exactly isn't completely -- Well, it's not apparent to me right now, and maybe it's to let this run through the SEDAR process and work it out that way, and maybe there is some way to do something in the meantime that gets us there, but I am not clear exactly what that is at this point.

CHAIRMAN NANCE: Thank you. Anyone else? What is the -- For right now, we have that November meeting, and I think it's November 16.

MR. RINDONE: It's the 18th.

CHAIRMAN NANCE: The 18th. Thank you. The intent of that meeting right now, Ryan?

MR. RINDONE: It was to evaluate catch limits for red snapper based on the Great Red Snapper Count and the LGL study off of Louisiana, with you guys also having, at your disposal, everything that was discussed at the March/April meeting, which is the NMFS bottom longline survey through 2020, and then also the combined SEAMAP video index that's submitted, and it's, I think, Working Document Number 4 for SEDAR 74, with the latter being -- Not being used as part of the catch analysis, but more being used as just another index to look at when thinking about what's being picked up by the bottom longline survey, considerate of the fact that, obviously, those surveys have different selectivities.

Then the council has asked that you guys evaluate those four things in consideration of making recommendations about red snapper catch limits, and so, given the current status of the LGL study and them needing to do some more investigation, I don't know that you're ready to consider that as part of that, and so, per the council's request, you would either have to move forward without considering that, and justifying that, or make some other decision.

I think you guys should just be clear about however it is that you're recommending things move forward at this point, so that the council's expectations are clearly -- So that your expectations for what should happen are clearly defined and the council understands why you are or are not doing whatever you are or are not doing.

CHAIRMAN NANCE: To that point, discussion from the SSC? Luiz.

DR. BARBIERI: Thank you, Mr. Chairman, and I don't disagree at all with the points that Roy made and then the perspective that Ryan brought up, and it's just difficult to comment on what we would do in November and how this information would be considered without knowing the analysis that's being proposed by the Center, I believe, right, for us to see, because --

MR. RINDONE: That analysis was supposed to be informed largely by what transpired here, and so, if everything had proceeded as had been expected and laid out in the scope of work, you guys would have been making a decision about, essentially, like which estimate of absolute abundance to use for Louisiana.

You can't make that decision right now, and so the Center won't have the information necessary to move forward with the analysis it had planned, because, at this point, the analysis that they did before would stand, unless you guys say that it should be modified say by the current version of the Great Red Snapper Count, and you have a couple of different analyses that have been presented to you, based on whether you're using the random forest or the stratified random approach with the random forest used in that, or what you want to do.

As far as that goes, I think you just need to be clear about what direction you're giving the Center for any analysis they should do for November, if they should do an analysis for November, what your expectations are for the LGL study, and like you guys have -- You just have some things that you need to map out, I think.

DR. BARBIERI: Just in response to that point, real quickly, Mr.

Chairman, that's why I don't think that we are ready to make a decision on using this for management advice at this point. We just don't have all the different pieces right in front of us.

I don't know how the Center -- What numbers the Center would use, without guidance from us, to bring back an analysis in November, and so I think -- I don't know, and perhaps in November this could be discussed, but perhaps in November we discuss -- We kind of review some of the information that LGL promised, like the document that we asked them to put together, and that will provide more details about the design and all the other components of their study, but, until that issue is resolved, I don't think we are ready to move forward with that, and I would not encourage the Center to proceed with an analysis at this point.

CHAIRMAN NANCE: Paul.

DR. MICKLE: I will try to put some -- I guess I'll try to give some direction on my opinion here. From what I see, there's a lot going on, and we've got to do our best to do what we can with both the analysis and the estimates, right, and then there's the designs that we've seen, I guess before a few of us were on the SSC, but you all have seen the Great Red Snapper Count full presentation report. Some of us have not, but that's fine.

At this point, I want to piecemeal it. Working for the General all those years in Mississippi has got me piecemealing things, and it works. It works well, and so trying to identify, and trying to offer something to the group that is palatable of a dose to take, at least with our time here left today, and that's it, right, until November, but to try to bring it in.

From what I'm going to say, what all I've seen this week, we need more information on LGL's presentation both design data and estimates. Now, moving on, and let's compartmentalize that there. That's an audible of let's just wait, and that's fine. We may be able to give it our peer stamp in November, and I hope so.

Now, Great Red Snapper Count, and we have already given the blessing of peer review on the original report. There was recommendations that came out of a peer review done by Sea Grant, with the consultants, and those recommendations were addressed, and there was an amendment to the estimate, spatially done, and so, in that sense, I am just sharing what I am going to vote on, trying to get everyone to follow and trying to understand my way of thought. The analysis is blessed, and the data and the design is blessed, but the estimate is not blessed, for me, at this point. I am looking at each survey and --

CHAIRMAN NANCE: Would you say that again, Paul?

DR. MICKLE: Sure, and help me with it. I may be off here, but so both LGL and the Great Red Snapper Count -- There are over a hundred people involved, safely saying, and so trying to understand levels of it, and there's design. If the design is flawed in the beginning, we save ourselves a lot of time and work.

Then there is analysis, right, and there is the type of analysis done, and then there's the execution of the analysis, if they did it right, right, randomized versus stratified and all those things. When you start to think about that, I see three levels of peer-reviewed blessing, right? There's design, analysis, and catch advice. I'm sorry. Estimate.

That's how I look at these, and I am trying to be a good SSC member of what can I do to compartmentalize and get to the point where the Southeast Science Center can take what we give and run with it, right, and that's what they want. They bring that up constantly, and I love when they do that, because that's how things get done in an efficient manner and the best way possible.

Our job is to stamp these, and I'm trying to break them up into levels, but I have other comments about the process that I will save, and I have been holding onto those, but that's what I will share at this point. I may be wrong, but that's how my brain is working, and I am hoping that it can help the group, and, if it doesn't, that's fine. It's okay, and I'm open to thinking other ways.

CHAIRMAN NANCE: I appreciate that. Thank you. Steven.

DR. SAUL: Thank you, Mr. Chair, and thanks to Greg and the research team for the presentation for the presentation and sharing the results and the information, and that's super helpful. A couple of things, thoughts, that I have regarding kind of moving forward.

One being that I feel fairly strongly that this should be integrated into the SEDAR process, and that's the most appropriate way to figure out how to utilize this information. However, stock assessments are integrated assessments, and integrated numerical models, which means that they pull together these various disparate different data streams, and then the model, obviously, works to find the parameter space that best fits or matches those data streams.

I think that one of the -- When speaking about trying to set the

ACL and an ABC and such, in my thinking, the way that that should be done would be to somehow integrate the Great Red Snapper Count and/or the LGL work, again providing that they pass the peer review, and liked Paul's comment about these sort of levels of peer review, and I think that's probably the --

Sort of breaking it down to bite-sized pieces is really useful, but, provided they pass those, perhaps there's a way to integrate these values into the stock assessment, such that, at the end of the day, when the assessment is run, and the projections are run, we have an integrated picture of -- An integrated estimate of fishing mortality that accounts for everything, including the surveys, these surveys, and, again, I'm not sure -- From a logistical, pragmatic perspective, I am not sure how that would work in Stock Synthesis.

From a theoretical perspective, I can think about how it might work, where perhaps it's conditioned on the biomass in the Great Red Snapper Count, let the model condition on that, but, irrespective of how it's sort of figured out, I think it's important that it be wrapped into the SEDAR process, so that these sort of things can be figured out in the best possible way.

The other thing to keep in mind is, if we do want to use -- Again, just another layer to that same point of assessments being integrated, if we do use any of the Great Red Snapper Count information to set ACLs or ABCs or any of the sort of targets that we have, then we need to be really, really cautious about mixing and matching, right, and so pulling some information from a stock assessment model and some information from the Great Red Snapper Count.

The two are not equivalent, and the two are statistically different, and they carry different assumptions, and, because the assessment models are integrated, the biomass estimates that you get, the abundance estimates, everything is interconnected, and so all of the estimates and all of the values are all interconnected with one another, within the assessment model framework, and so, by combining survey information with information coming out of the assessment, that could be, one statistically inappropriate, and also inappropriate in terms of perhaps mismanagement of the resource. Thank you.

CHAIRMAN NANCE: Thank you. Trevor.

MR. MONCRIEF: Just real quick, I share Paul's views on at least trying to compartmentalize and making tangible goals that fit within a specific timeline, and, in my mind, the most pragmatic

way to go about this is we've got a one-day webinar set up in November, and we've still got outstanding questions on the LGL side, and we still need to, I think, have a little bit of time to go through the Great Red Snapper Count and the different analyses that were sent to us this morning, just to make sure everybody is comfortable with them, but, to me, that date in November is a pretty good time to be able to try to come up with some consensus as to the numbers to use, the estimates that we're going to choose, whether we do LGL or the other side, that kind of thing.

I really think, given the scope of questions that arose yesterday, if the document can be provided prior to that meeting, and those individuals who have questions compile those questions, we'll be at a lot better spot in November than having to run through everybody's questions in the morning and leading to a long discussion, and so that's my way to view it kind of pragmatically.

CHAIRMAN NANCE: Yes. Roy.

DR. CRABTREE: One thing that came up with respect to the Florida issues, and I think John Walter raised this, and Mandy and Luiz, had to do with where the fish are, basically, on the West Florida Shelf and then how the strata -- Where we're applying density estimates from here to there, because there were concerns that we're showing fish in areas where people don't believe there are fish.

I wonder if there's some way we could look at that, and I don't know, Luiz, if you have access to all this, but is there a way we could take a look at that a little bit in November, to get some comfort, because, I mean, are we taking densities from forty meters and then applying it to twenty meters and ten meters and that sort of thing, and is that a problem here, and I don't know if it is or isn't, but there seemed to be concerns with that, and I would like to look at that in a little more detail, but I'm not sure who has what and who can do what.

CHAIRMAN NANCE: Trevor, to that point?

MR. MONCRIEF: Just to get a sense of what you're talking about, it's almost like you're talking about almost constraining that depth zone, and, rather than be ten to forty, maybe it would be twenty to forty, that kind of idea?

DR. CRABTREE: I don't know. I just want to look and see if we're really estimating that there are fish in areas where all the other data we have, and information, says there are not fish there, and, if that is happening, then how do we account for that?

This is part of what -- We have these CVs, but then there are these concerns that are being raised, and it's just not clear to me how well all of this is captured in the uncertainties of it. I look at where it's showing the fish are, and I'm a little like Luiz and John, and it doesn't match up to where I expected they would be, but I'm old enough now to realize that most of what I thought turns out not to be quite right, and maybe that's true here, but it would just be nice if there was some way we could look at that a little bit, and maybe then we would feel great about it, but --

CHAIRMAN NANCE: John.

DR. FROESCHKE: To that point, one idea -- On a random forest model, when you fit those models, you can generate what is essentially a partial effects plot, which attributes the impact of the various explanatory variables on the probability of occurrence, and you can map that across the range, for example, of depth, and so you should be able to see if the fit is linear between the ten and forty depth meters or not.

If it's not, you probably could use that to parse it out, or at least know if you need to look further into it. It probably is not set up by strata, and so it may not be the right geographic space, in which case you would need to look for the interaction by strata with the depth fit first and then probably dig into it, but I suspect that all that information could be pulled out from the model, and that would probably be a really good place to start.

CHAIRMAN NANCE: Thank you, John. Harry.

MR. BLANCHET: Thank you, Mr. Chairman. This is more in terms of the logistics. I know that, yesterday, Benny made a comment that he was not going to be available in November, and so, just in terms of if you want Benny to be available, I don't know if November is the best time to do it.

CHAIRMAN NANCE: Okay. I will talk to Benny and see if -- He's got a very good team, and I am -- I will talk to Benny, just to make sure, from that standpoint for sure, Harry, but that's a good point. Josh.

DR. KILBORN: Thank you, Mr. Chair. I guess I would like to, first, echo some of what Steven Saul was saying and point out that I am -- I guess I should also back up and say that I am relatively new to this whole process, and I was not present at the in-depth meetings where we went over the Great Red Snapper Count.

Given the disparity between the Great Red Snapper Count and this LGL study that we looked at yesterday, and just that one regional difference, I think we have a lot to consider between those two things, but I don't know that what we're trying to do here is the appropriate way to get this done, and I do think that we need to fold a lot of this into the SEDAR process and really take a good look at what's going on here before we offer any concrete management advice, as far as catch limits and appropriate levels.

The estimate from the Great Red Snapper Count, just in Florida alone, is more than what the SEDAR process estimated for the entire Gulf of Mexico, and so, again, I really think this needs to be folded into the SEDAR process before we drastically change any of the catch advice that we're offering for the council, and so I just want to, again, reiterate some of the things that Steven was saying and just point out that there is a lot of unknowns here, and I think that we might be putting the cart before the horse if we're giving any concrete advice on this. Thank you.

CHAIRMAN NANCE: Thank you. Doug. After Jim Tolan, we'll cut it off, and then we'll come back after lunch, but go ahead, Doug.

MR. GREGORY: I will be brief. I agree with the previous speakers. The Florida estimates need to be evaluated further. I don't think there will be very many people who will believe that the Florida continental shelf has as many red snapper as the entire rest of the Gulf of Mexico, U.S. Gulf of Mexico.

One thing I will note, and it's just a coincidence, is I think the continental shelf off of Florida is probably equal to the rest of the U.S. continental shelf in the Gulf, for what it means, but I think, in November, unless we can do something productive in this direction, we really can't set ACLs, catch limits, without resolving some of these issues that we've been discussing this week. Thank you.

CHAIRMAN NANCE: Thank you, Doug. John.

DR. HOENIG: I was wondering about your plans for dealing with the LGL report. Are you going to get an independent review, and when you have a meeting in November with Benny, will you allow public input, because there are scientists who actually have some expertise on this subject that could provide you with feedback on the quality of it.

I really think the LGL report -- I think LGL needs to clarify exactly what its design and analysis procedures were, and I think it needs to be reviewed. Otherwise, the controversies that you

face in trying to make decisions are only going to be compounded by the fact that you have a report that hasn't been reviewed, and you will be criticized for that. It's like how can you make decisions if you're not getting it reviewed?

CHAIRMAN NANCE: Ryan.

MR. RINDONE: Thank you, Mr. Chair. The review that was done for the Great Red Snapper Count was done largely because it was a Gulf-wide study that was congressionally funded, and it impacted the stock throughout its managed range for the Gulf Council. The LGL study was commissioned by the Louisiana Department of Wildlife and Fisheries, and the task of putting together a peer review befell that agency.

As we've been told at this meeting, the study design was something that was left out of that review, and so the team, that research team, is going to get all of that information together and provide that. If the council so directs us, we can go about the process of setting up a similar review, but we would certainly wait on the council to direct us to do that, since this study was specifically commissioned by LDWF, and so the responsibility of that peer review really does befall that agency before it would befall the council. That's why we had waited until now for reviewing it, to allow that process to happen.

DR. HOENIG: To the extent that the LGL study gives different results for Louisiana than the Great Red Snapper Count, it basically calls into question whether you can trust the Great Red Snapper Count, and so it's rather critical, and, if you think that people won't point that out, I think that's just not realistic, and so I think you're going to need to deal with this, and it's not just the design. It's also the analysis. From what I saw yesterday, there are serious problems with the analysis.

MR. RINDONE: At this point, Dr. Hoenig, we're waiting on this additional information to be provided by the LGL team and by the Louisiana Department of Wildlife and Fisheries, and so, insofar as it matters for the SSC making any decisions based on picking which one to use for the State of Louisiana, that decision is clearly on hold at this point, and, based on the conversations that have been had around the table, insofar as it matters to the Great Red Snapper Count, the SSC does seem to be recommending that that move through the SEDAR process, through SEDAR 74's research track. At this point, until some of these other questions can be answered by the LGL team, moving forward beyond that appears to be on hold.

CHAIRMAN NANCE: Thank you. Paul.

DR. MICKLE: To that point, my opinion is that we treat them completely independent from each other and vote on them, if they're appropriate for management. They can both be appropriate for management, and they can not be dependent on each other. That is a massive issue.

CHAIRMAN NANCE: Thank you. Jim.

DR. TOLAN: Thank you, Mr. Chairman. Having been one of the SSC members that was privy to the original Great Red Snapper Count presentation --

MR. RINDONE: Jim, you're shouting in a train tunnel. We can't understand anything you're saying.

DR. TOLAN: I will let us break for lunch.

CHAIRMAN NANCE: Okay. Thank you, Jim. We'll go ahead and break for lunch, and we'll be back at 1:00 p.m.

(Whereupon, the meeting recessed for lunch on September 30, 2021.)

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September 30, 2021

THURSDAY AFTERNOON SESSION

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The Meeting of the Gulf of Mexico Fishery Management Council Standing and Special Reef Fish, Special Socioeconomic & Special Ecosystem Scientific and Statistical Committees reconvened on Thursday afternoon, September 30, 2021, and was called to order by Chairman Jim Nance.

CHAIRMAN NANCE: We're going to go ahead and reconvene now, and I appreciate everybody -- Greg, when we left for lunch, I forgot to say what a great presentation you guys gave, and I appreciate your whole group being on and being able to respond to those questions, and so thank you for that, and I apologize for not saying that before we broke.

Let's go ahead and -- So we've had some discussions over lunch, obviously, and any SSC members that would like to continue on with our discussions? Jim Tolan.

DR. TOLAN: Thank you, Mr. Chairman. The only comment that I was going to make, from right before we broke for lunch, was more in context to the new members, to let them know that I was one of the previous members that got to sit through the full presentation of the Great Red Snapper Count, and I was a little dismayed at the eleventh-hour change in methodology and numbers, and so, at this point right now, I don't think -- Given the deficiencies that we identified yesterday with the LGL study, and this sort of new information to evaluate on the fly, I don't think we're at the point right now to give it the grand thumbs-up on either study to pass on to the Science Center. That's just my opinion.

CHAIRMAN NANCE: Okay. I appreciate that opinion. Thank you. John.

MR. MARESKA: Jim, I kind of agree with your sentiments. Particularly for all the new SSC members, I felt like they would probably abstain from voting on any motion related to the Great Red Snapper Count, or even the LGL study, but, in the process of just trying to move things along, I did draft a motion, and if Jessica could put that up.

I felt like what we've seen so far, I think most of us, and the CIE reviewers, agreed with the design and the data that had been captured, but I think what we're all struggling with is the estimates that have come out and those estimation procedures, and so I think the data itself could probably move forward in the SEDAR process, and we can let the Science Center, in further evaluations, determine the best way to parse it out, so that we do get the most accurate estimate of abundance that we can.

We know it's not exactly right, but it's better than we had before, and so, with that, I will read my motion, just in case anybody can't see it. **The SSC recommends the design and data from the Great Red Snapper Count are suitable for consideration of the SEDAR process. The SSC also recommends further evaluation of the estimates of abundance and the methods used for estimation.**

CHAIRMAN NANCE: Thank you. Do we have a second for that motion?

DR. GRIFFITH: I will second that.

CHAIRMAN NANCE: Second by David Griffith. Thank you. We have a second. It's up for discussion. David.

DR. GRIFFITH: I am just wondering if we should put the methods and analysis used for estimation.

MR. MARESKA: I am fine with changing the wording.

CHAIRMAN NANCE: Okay. With that change, let's continue on with discussion. I don't see any hands up from outside. Anybody in here? David.

DR. CHAGARIS: Thank you. By SEDAR process, do you mean specifically SEDAR 74, and should we be specific about that in the motion?

MR. MARESKA: Dave, it could be an additional SEDAR workshop, if it was deemed necessary, and I don't know how far down in the weeds all this is going to go, but, in general, yes, it was for the SEDAR 74 process.

CHAIRMAN NANCE: I think, John, if you're not opposed to that, could we put the SEDAR 74 process?

MR. MARESKA: Absolutely.

CHAIRMAN NANCE: David, would you be okay with that change? Dave, I think that's appropriate, so we know which SEDAR we're talking about, and I think that's a good suggestion.

DR. CHAGARIS: Yes, I'm fine with that change. Thank you.

CHAIRMAN NANCE: Jim Tolan, please.

DR. TOLAN: Thank you, Mr. Chairman. If John will allow a friendly amendment, I would only like to add, in the third sentence, if we could make a chance of "estimates of absolute abundance", because that really was the point of both of these counts.

CHAIRMAN NANCE: He is shaking his head. Josh.

DR. KILBORN: Thank you. I would just like to clarify, and is the SEDAR 74 process the research track that is ongoing, or is that for the operational assessment?

CHAIRMAN NANCE: Yes, it is. It's for the SEDAR 74 process that's going on right now, the research track.

DR. KILBORN: Thank you.

CHAIRMAN NANCE: Mandy.

DR. KARNAUSKAS: Thanks, Mr. Chair. I have -- I don't know if I would say a concern with this motion, but just a little bit of

reservation, in terms of what exactly the SSC is recommending in the SEDAR process, and so, at the end of the day, we're going to have this estimate of absolute abundance, and the number is going to be what it's going to be.

It's one thing to say that there is this biomass of fish in the shallow areas that are accessible to fishery that perhaps the stock assessment hasn't been estimating, for whatever reason, but then it's altogether another thing to say that there is this biomass that is offshore and largely inaccessible to fishery, and then there's questions about how much that biomass actually contributes to the fishable proportion of the stock.

I am wondering if we could include some wording, and I realize that this isn't a part of the original study, and that the study was funded to get an absolute estimate of abundance, but I do think it's critical that the SSC recommends some sort of consideration of how that biomass is distributed in space, because that's really kind of what is going to drive how the numbers can be used in the assessment and management process, and so I will put that out there for some discussion.

CHAIRMAN NANCE: Okay. Thank you. Ryan, to that point?

MR. RINDONE: Thank you, Mr. Chair. When I'm thinking about how the SSC normally drafts its terms of reference, it uses language that is very specific, and not to necessarily prescribe things to happen, if it's endeavoring to add flexibility, and so, here, where it says, "are suitable for consideration", I think "of" should be "in the SEDAR 74 process".

"Consideration in" doesn't mandate that it be used exactly as it's currently drafted, and it allows for the SEDAR process to do what it does and investigate the data and look for ways to make improvements and whatnot as it needs to happen, in order to incorporate it into the larger modeling framework.

Then this gets stated often, about the different fishery's ability to access different parts of the uncharacterized bottom, and I would just like to remind everybody that, given the current technology available to fishermen these days, whether they be commercial or recreational, there is not really a part of the fishable area of the Gulf, in terms of the EEZ, that is truly inaccessible, and it's really just a matter of how much fuel you have with you and what sort of boat you're running, but it is all, in fact, accessible.

CHAIRMAN NANCE: Okay. Thank you. Mandy, any comment on that?

DR. KARNAUSKAS: To your last point, Ryan, I'm in agreement with you there. There is very little of the Gulf that is not accessible anymore, but I guess largely exploited versus very lightly exploited would be a better way to put that.

MR. RINDONE: I agree with that.

DR. KARNAUSKAS: Yes, and so I just wanted to make it clear that, if I would support this motion, that it would be with that sort of caveat, hoping that the wording doesn't tie us into having to use the absolute abundance estimate in the assessment in a certain specified way.

CHAIRMAN NANCE: I think that's suitable for consideration. I think that's the key phase phrase there, that we have, during that process, that ability to choose and so forth. Go ahead, Roy, to that point.

DR. CRABTREE: Mandy raises -- It's an interesting question, and it's one we've had a lot of discussions about. I mean, clearly, if only a fraction of the population is really exploited by the fishery, you have the potential to really fish it down in certain areas, and, how you deal with that, I'm not quite sure. That's a question to be worked out, and a lot of that has really management to figure out.

Some of the rest of this, still, I scratch my head. I mean, we know we fished this stock down, and essentially collapsed it, on the west coast of Florida, and I am struggling to understand, and how did we collapse it if there are all these fish out there that aren't being fished anyway? Wouldn't they have just moved back in? I still have a lot of issues with that, but I appreciate Mandy's point, and I am not sure how you would -- I don't think we're tying in, through this motion, any particular course of how it's used or how it affects the ABC or how management should proceed at this point, and I think that's a lot to work out there.

CHAIRMAN NANCE: That is. Harry, please.

MR. BLANCHET: This was to Mandy's point, and Roy addressed a lot of my issues, and so I will just pass at this point.

CHAIRMAN NANCE: Okay. Thank you, Harry. Jason.

MR. ADRIANCE: Thank you, Mr. Chair. Most of the points have been covered, but I was just curious, and I understand this stock is a

little different, because we now have this work on absolute abundance, but do we consider an unexploited portion of any other stock in the SEDAR process?

I mean, I understand this is unique, but I just -- I had some concerns about that. I understand the points about how this is going to be something for management to figure out, but, I mean, we currently don't look at it that way, as far as I can see for other stocks.

CHAIRMAN NANCE: You're right. Josh.

DR. KILBORN: Thank you. Mandy's comment got me thinking, and what does "further evaluation" actually mean, and should we be more explicit about that? Recommends further evaluation of the validity of the assessments and methods, or, I mean, that could be interpreted either broadly or very narrowly, and so I just wonder, and I will let the rest of the group make the decision, because I don't know the answer, but do we need to be more explicit in that respect?

CHAIRMAN NANCE: I am going to -- Go ahead, Ryan.

MR. RINDONE: Thank you, Mr. Chair. Just going back to how we typically draft the terms of reference, how the SSC drafts those, there is a general avoidance in being overly prescriptive, because you don't want to constrain the latitude of the process to be able to explore different things and pigeonhole yourselves into a corner.

I would just encourage you guys just to think critically about how precise you really want to be. The SEDAR process has a great advantage in its flexibility to be able to explore different things, depending on the time and resources and data available to do so, and so you certainly wouldn't want to, I don't think, limit that, if you had the choice.

CHAIRMAN NANCE: Personally, I would --

DR. KILBORN: Thank you for that clarification.

CHAIRMAN NANCE: Personally, I would rather leave it in this, only because then it doesn't constrain anyone from being able to do what they see fit in that process. Any other comments? Lee Anderson, please.

DR. ANDERSON: I just -- This is pretty rare, an economist jumping into the middle of this thing.

CHAIRMAN NANCE: I was going to say -- Twice in one day, and you're doing good.

DR. ANDERSON: Well, that was -- The other one had some economics in it, and I do know a little bit about population dynamics and fisheries management, and I would say that, despite what goes on here, I would like to have something in this motion that does say not only absolute abundance, but -- I don't even know the exact words, but is this abundance available to the gears, and I heard what was said and everything, but I would like them to look at that question.

I have heard in a lot of discussions with this, and I think we should stop dancing around it, excuse me, and just say, all right, and let's look, and is this extra fish really subject to fishing gear? Not only technically, but practically, and so I think mentioning that would make sense.

CHAIRMAN NANCE: Okay. I think, in my opinion, I'm not sure that it does, but I will let others weigh-in on that. Roy.

DR. CRABTREE: I mean, on the West Florida Shelf, you've got a big grouper longline fishery that is all over everywhere out there, and they catch red snapper, and so I'm not sure where there is that someone isn't fishing for something, and I keep thinking of what other fishery can I think of where you have this large, unexploited fraction of it, and the one that always comes to my mind is red drum.

You've got pretty much the adult population in the EEZ that is not fished, and then you've got this little stretch of state waters where we pound them, and so it's not unprecedented, but I have a hard time thinking of places, at least on the west coast of Florida, that aren't being fished at some level.

CHAIRMAN NANCE: John, did you have a comment? Okay. Harry.

MR. BLANCHET: I agree that there is some fishing pressure, and there is also movement, but I think both of those are way beyond what the scope of the Great Red Snapper Count was, and I think we're going several steps beyond what these studies were originally intended to do.

In terms of -- I think we're kind of putting apples and oranges together and getting potato salad, and it's not really clear, to me, how you would use the information from this in order to derive a susceptibility of a fraction of a stock to a particular fishery,

and that seems to be well beyond what either study was designed to do. I think that, to go beyond where we are right now, it's really kind of getting past where they were. Thank you.

CHAIRMAN NANCE: Thank you, Harry. Will.

DR. PATTERSON: Sorry for missing a little bit here, and I think maybe most of what I missed you were at lunch, hopefully, but, anyway, I am looking at this motion, and I'm trying to figure out what that last bit means about the methods and analysis used for estimation. Is that for stock status determination or for projecting OFL and ABC, or like what does that actually mean?

The second thing is I just heard Roy's comment about red drum, and that is an interesting analogy. Red drum is managed for I believe it's a 30 percent escapement, and so how would we quantify that for red snapper, that sort of dynamics about what's being exploited and what's not being exploited, and do we think of the stock as the proportion of the population that is recruited to the fishery, and then I think the spatial analysis that John Walter was advocating for earlier would be quite germane, to try to lay that out in the context of the various fisheries here.

CHAIRMAN NANCE: John, to that point?

MR. MARESKA: To answer your question, that last sentence for further evaluation is, in general, in regard to the comments by John Walter and yourself and Luiz, and so I wanted it wide open, so there's a lot of flexibility, so that it can be evaluated by the different depth contours, and even to the point that, if you wanted to change region, and so you felt like the current regions that are set up aren't adequate, and that it needs to be changed along that aspect as well, and so it's very general in its purpose.

DR. PATTERSON: I've got you, and so what you mean here is used for red snapper population estimation, and is that --

MR. MARESKA: Yes, and so the absolute abundance that potentially would be incorporated into the SEDAR 74 research track.

DR. PATTERSON: Okay. Maybe we want to put --

CHAIRMAN NANCE: Should that be more -- Will, do you think it should be "estimation for the abundance of red snapper"?

DR. PATTERSON: I was just confused whether you were talking about to reconsider the methods and approach for the red snapper population estimation or if you were talking about using the red

snapper population estimates in the stock assessment, and so, I think, if you just included something here to signify that that last "estimation" was actually for computing red snapper population abundance, then it might clarify it.

CHAIRMAN NANCE: Okay. **Jessica, why don't you go ahead and put that in? Put it in, and John and David can see if they like that, and so I think it's "used for estimation of the red snapper population".** John.

MR. MARESKA: I am okay with that. Is that kind of Will wanted?

DR. GRIFFITH: Yes, I'm okay with that.

DR. PATTERSON: Yes, I think that clarifies it for me. Thanks.

CHAIRMAN NANCE: Okay. Thank you. Mandy.

DR. KARNAUSKAS: Thanks, Mr. Chair. I apologize if I'm going too far down this rabbit hole of distribution and what's accessible and not accessible, but Jason made a really good point, and it got me thinking a little bit more. I just want to say that I think one of the things that makes red snapper unique is kind of how we have re-engineered the system to target it with the artificial structures and so on, and how the habitats are being used differs across space.

In places like Alabama, you have most of the recreational, and a lot of the commercial, fleet targeting artificial structures, whereas, in Florida, you have a lot of the pressure on the natural reefs, and so the absolute number has really different implications, in terms of the different sub-regions, and so I just wanted to put that out there for other thoughts.

CHAIRMAN NANCE: Thank you. If there are no further comments, let's go ahead and vote on this motion. Jessica, if you would put the --

MR. MARESKA: Is there any objection?

CHAIRMAN NANCE: I guess I will ask that first. Cindy wants to talk, and then we can move on with that. Thank you though.

DR. GRACE-MCCASKEY: Thank you. I just have a, I guess, more of a procedural-type question, and I apologize that I don't know who mentioned this just a few comments ago, but I thought I heard someone suggest that those of us who are new to the SSC, and perhaps aren't as familiar, obviously, with all the background,

that we would abstain from voting, and so I just was asking for some clarification on that, please.

CHAIRMAN NANCE: No, you would not ever be -- Unless you're in direct conflict with something we're voting on, you are welcome to vote on any of the motions.

DR. GRACE-MCCASKEY: Okay. Thank you.

CHAIRMAN NANCE: You're very welcome. You can also abstain. You can vote no or yes, but you're free to make that vote. Luiz.

DR. BARBIERI: Thank you, Mr. Chairman. Just for my level of comfort, I think -- I just want to put this on the record, that I agree with John and Dave that this idea of considering this data as suitable for evaluation in a SEDAR process is the extent of our explicit language in this motion, and we are not saying that all of this data are suitable for direct integration into -- Because I have still questions that we haven't resolved, for example the Louisiana numbers, right, and so we don't have a full deck in front of us, and I imagine that, by that point, that the SEDAR 74 process, the data workshop, or whatever process is looking into this, that some of these issues will be -- Thank you.

CHAIRMAN NANCE: Thank you. Let me go ahead and read the motion. **The SSC recommends the design and data for the Great Red Snapper Count are suitable for consideration in the SEDAR 74 process. The SSC also recommends further evaluation of the estimates of absolute abundance and the methods and analysis used for estimation of the red snapper population. Any opposition to that motion? It appears there is no opposition to that motion, and the motion carries. Thank you.**

Obviously, with the LGL project that we looked at yesterday, we have asked for specific things from them that will be presented at a later date to us, so we can more fully evaluate and critique the estimates that came out of that review. Okay. I guess we're ready to move on to greater amberjack, I guess.

MR. RINDONE: We haven't received any updated presentations yet from the Science Center.

CHAIRMAN NANCE: Okay. Katie, are you ready?

DR. SIEGFRIED: Thanks, Mr. Chair. I did just send that to Ryan.

CHAIRMAN NANCE: Okay. Perfect.

DR. SIEGFRIED: We're working fast and furious over here.

CHAIRMAN NANCE: Katie, I just wanted to let you know that we really appreciate your response to this and all the extra work, and I thank you very much, for your whole staff.

DR. SIEGFRIED: Thank you very much, and I've very proud of them, too. I do have a question, while Ryan is pulling that up. I just wanted to make sure that we're clear that there is no additional interim reports that are needed before the meeting -- I mean, I may be beating a dead horse again, but I just want to make sure that we're not missing some interim analysis request.

CHAIRMAN NANCE: For which one?

DR. SIEGFRIED: To update the OFL advice based on the Great Red Snapper Count, and it sounds like we need to wait until we can just move through the SEDAR process, and is that correct?

MR. RINDONE: That is correct, Katie, at this point. The information necessary for the SSC to make that recommendation -- They don't feel that they have that, and so, once that information is better defined, then that request can be approached at that time.

DR. SIEGFRIED: Thanks, Ryan. I realize that you all just voted on that, and I just wanted to make sure. It's never a bad idea to be extra sure.

CHAIRMAN NANCE: That's right. You don't want to be caught the week before and we're asking for it.

DR. SIEGFRIED: Right. I don't want Matt Smith or Latrice Densen to be caught either.

CHAIRMAN NANCE: That's exactly right. Thank you for asking though.

GREATER AMBERJACK PROJECTIONS

DR. SIEGFRIED: So the presentation is displayed there. I do have all of the runs in Excel format, and I was just making small tweaks to it, and I don't want to put that in the presentation until we go through all of this, but I have all of the fifteen runs that were -- What is it? Yes, fifteen runs that were requested in Excel format too that I will put into a document after the SSC meeting.

CHAIRMAN NANCE: Okay. Perfect. Thank you.

DR. SIEGFRIED: Okay. I would like questions along the way, if you have them.

CHAIRMAN NANCE: Okay, and so, if anybody has questions, just let me know, by raise of hand, and I will go ahead and call on you.

DR. SIEGFRIED: Thanks. I put what Jessica sent me, and I appreciate that, Jessica. She sent me the spreadsheet that we went over on Monday that specified the projection configurations. We used the -- It really was the settings that we had initially said we were doing in SEDAR 70, with the corrections that we displayed on the first day, and so, if there's anything that is incorrect there, please let me know, but this is what I worked with.

I provided projections for what I am calling the base run, which is with the original allocation ratio of 27/73 recreational to commercial. The relative Fs for the fleets forward in time is 2016 to 2108, and we decided on our F current with the geometric mean. Selectivity and retention had the recent three years, terminal three years of the model, and our recruitment -- This is for setting -- For calculating benchmarks, as well as what the expected recruitment will be, and the projected time period was the average of 2009 to 2018, which is the recent ten years of estimated recruitment.

The ABC was to be 75 percent SPR 30, and I still provided rebuilding, because I provided that before, and there is that rebuilding plan on the books, and so I just have it there for you as well, and then I didn't deviate from landings, because we couldn't pull any more recent landings.

This is just what I showed before, but just so that you have it in this presentation as well, and the recent average is substantially lower than the long-term average, and it's here in millions of fish.

MR. RINDONE: Katie, can you go back a couple of slides for just a second? I just wanted to verify something, real quick. Just for the allocation ratio, it's 27 percent commercial and 73 percent recreational, correct?

DR. SIEGFRIED: Yes, and I must have said that backwards. Did I say that backwards?

MR. RINDONE: Yes, and so just verifying was all.

DR. SIEGFRIED: Sorry. I must have just said it backwards.

MR. RINDONE: Okay. Thank you.

DR. SIEGFRIED: There are so many different ways that I could have presented it, and I am happy to replot these and show them right after gag, if needed, but this what I thought was most valuable to show you. All of the allocations with respect to the base, which is the previous set of allocations, I listed the allocations in order that they were given to us in the memo, and so the first one says 73/27, and that's the dark-blue line, and that's the SSC base, is what I'm calling it, OFL.

The first allocation is the one that increases the recreational component to 80 percent, and that's the orange line, and I didn't say it wrong again, did I, Ryan?

CHAIRMAN NANCE: No, and you're saying it right. Now the numbers are flipped, but as long as we've got that first number is recreational now and all the way down.

DR. SIEGFRIED: Yes. Except for 4, where there is no recreational allocation, and it's just commercial fixed, and then we calculate the recreational allocation. The orange line is the 80/20. The gray line is -- I am sorry. I am getting myself confused by looking at my own presentation instead of the screen.

The first allocation is 84/16, and that is the orange one. The second allocation is 78/22, and that is the gray, and the third allocation is 80/20. That is the yellow, and the fourth allocation request is the commercial fixed, and I think it's the most recent take, and I have that number in whole weight in another slide, or previous slide, and that's Number 4, and that's in light blue. This is just meant to show you the relative effects of allocation, the change in the recreational component, and the effect on the long-term yield.

This is the ABC projections. I think that the color scheme is the same. I hope it is the same. It's the same set of allocations, and so you can follow the dark-blue line for the base all the way down to Allocation 4 for the light blue, and you can see there is not as big of a difference here for the ABC as there was for the OFL.

Then the next slide is the rebuild projections, and, of course, that's switched with the colors, and I apologize. This is what happens when you're doing so many at a time, but they're labeled there for you. In this instance, the base rebuild is in orange,

and the allocation one is dark blue. Allocation 2 is in gray, and Allocation 3 is in light blue, and the Allocation 4 is in yellow. I will adjust that color scheme to be consistent with the other two for the report.

CHAIRMAN NANCE: That's not a problem, but thank you for pointing that out.

DR. SIEGFRIED: Okay, and then are there any questions about the allocation scenarios, because I think that fulfilled the actual request from Monday, where I used those settings and carried out the requested allocations and reran the base projections with SPR recent recruitment, as it should have been in the first place.

CHAIRMAN NANCE: Okay. Doug, did you have a question? I just see your hand now.

MR. GREGORY: Yes. Katie, the difference in these trajectories, is it due to the recreational fulfilling their quota and the commercial not, and so we're giving more fish to them and assuming they fulfill their quota, or is it due to increased discard mortality on the recreational side, because, the more we shift the recreational, it seems like the higher the mortality is.

DR. SIEGFRIED: It's not going to be the first one. It's going to be more about the selectivity changes, and so, if we have more of one fleet versus the other, based on the allocations, that's going to affect the trajectories, and the discarding is kept constant, or kept to -- It's part of the retention function that is kept to the last three years, and so that's not necessarily going to change across these, except for just a larger proportion of the recreational fleet in all of these scenarios, and so it's not the first. It's not that they are exceeding any quota, and I don't take any implementation error into effect, or into account.

MR. GREGORY: Okay. Do both sectors have the same size limit, or is that still different? I think it's still different.

DR. SIEGFRIED: Let me look.

MR. GREGORY: Ryan would know that off the top of his head.

CHAIRMAN NANCE: He just went to look, I think.

MR. GREGORY: It's not important.

DR. SIEGFRIED: Well, I can find it. I just need to pull it out of this other larger presentation.

MR. GREGORY: Don't worry about it. I will look it up myself. Thank you.

DR. SIEGFRIED: Okay.

CHAIRMAN NANCE: Okay, Katie. Go ahead.

DR. SIEGFRIED: Okay. The previous slides sort of provided you what you asked for, and they were basically the homework that I had. Consistent with the decision tree and the idea that we want this to be a much more open conversation and a more explicit set of projection configurations, I wanted to show you the impact, the relative impact. I did just get a chat that commercial is thirty-six inches and the recreational is thirty-four inches. I don't know what unit that is, whether it's --

MR. RINDONE: It's fork length.

DR. SIEGFRIED: Fork length. Okay. Thanks. What I have shown you here is, on the plot by year, in millions of pounds, and so this is the OFL projections for the SSC base, is what I am calling the one you all configured for me, and that's the blue. Then the base with the stock-recruit curve, and that's the green line, and then the base with the stock-recruit curve and using an SPR target.

We did look back and sort of check the FMP on the books, and we checked with legal and all that, to see if everything that we were doing was okay, and we found that the rebuilding plan was reset from 2012 to 2027, using a projected yield, which did pull from the stock-recruit curve. The steepness of that curve was the 0.85 that the SSC had decided on, after seeing multiple runs that Nancie had done years ago. I just wanted to bring this to your attention.

To deviate from that, I mean, it's possible, and it's something the SSC and do, but it does mean reducing that expected long-term yield and the recovery target, and it's something that we would encourage a lot of -- Hopefully a lot of basis for changing that goalpost, as Dave said the other day.

I show here that the long-term yield for the blue line is quite a bit lower, just this ten-year timeline, than it is for the green or the brown line, but then you will see, in the report, that, yes, of course it does recover faster, because the recovery target is so much lower. This is just for your consideration.

Then, because I'm a little bit of a glutton for punishment, I had done a few relative effect of allocation runs before, and so I

wanted to show you those, just so that you had these also for your deliberations, and I will walk you through this.

The bottom cluster of lines is the SSC base. The brown line at the very top is the base with the stock-recruit curve recruits pulled and the SPR target, and these are all OFL runs. The second cluster at the top, below that brown line, are all of the allocation scenarios using the stock-recruit curve and assuming MSY, and so this is just for you all to see that the allocation scenarios have relatively the same effect, regardless of what your target is, or what recruits you choose, but the long-term yield is greatly affected.

CHAIRMAN NANCE: Luiz.

DR. BARBIERI: Katie, if you don't mind, since you're asking for questions -- This is not really a question, but just a comment, Katie. Thank you, by the way. I mean, I think this helps a lot to see this, because now you're putting it in a picture, right, where you were trying to explain it to us the other day, and it made sense the other day, but, here, it's just easier to see.

It gives us an appreciation for the consequences of those decisions, but I am thinking that, in terms of -- Since we don't know, at this point -- We may not have enough information, enough data, in this assessment, right, and the analysis to provide us really what the true productivity of the stock is, and, because it looks like recruitment has been relatively low for so long, and since we have failed to rebuild before, and I feel more comfortable going with the lower-recruitment value inputs right now, but with the thinking --

This is my point, Katie, is that with the thinking that, if we can reevaluate this rebuilding plan, I mean the trajectory of where we are here, in three to five years, because, if the stock responds to management, and starts to really produce higher levels of recruitment, that should be detectable, and, that way, we could perhaps adjust what is here.

DR. SIEGFRIED: Yes.

DR. BARBIERI: Right?

DR. SIEGFRIED: Yes, and I didn't want to interrupt you if you were continuing, and it sounded like you were still talking.

DR. BARBIERI: Thank you, but I just finished. Thank you, Katie.

DR. SIEGFRIED: Okay.

CHAIRMAN NANCE: Roy.

DR. CRABTREE: Katie, given that we don't have any kind of index -- I don't think we have any real fishery-independent index or recruitment index, and how would higher recruitments be picked up in the assessment? I mean, I guess we could see higher CPUEs.

DR. SIEGFRIED: We have the combined video index that we felt was a successful addition to this assessment that does capture --

DR. CRABTREE: So what you show up there?

DR. SIEGFRIED: Yes. The youngest individuals, and so we think that that would be a way to determine -- But, you know, as you're saying, Roy, we can use that -- Even if we have a good index, we can use that index to determine whether there is potentially recruits coming in and do interim analyses using that, but you're starting with that low goalpost, right, and so that's just something that I wanted to show, but, yes, I think that the combined video index, based on the composition data in that, we would be able to pick it up there.

DR. CRABTREE: Right, and, by going with it, this route, we're in fact keeping the Fs lower than they would be under any of these scenarios, because, under these other scenarios, we would be catching -- We would be pulling more fish out, and, if we see evidence that the stock is doing better than we had anticipated, then we can make adjustments, but I have -- You know, all those high recruitments, as I recall, were all back in the 1980s and earlier.

If this was like gag, where we had seen high recruitments in the more recent, more data-rich time period, but we really -- We haven't, and those numbers in the 1980s were really variable, and I would bet you, if you look back at the history of red snapper, that we have reset this thing repeatedly with projections showing the stock is going to recover rapidly, and it just never happens, even though we keep adjusting it, and it just makes me think that those higher recruitments just aren't doable. Thank you.

CHAIRMAN NANCE: Dave Chagaris.

DR. CHAGARIS: I think this is an incomplete picture here, because, if we look at these lower yield streams with the lower average recruitment, it does give the appearance, just by looking at this plot alone, that that is a more conservative measure, but keep in

mind that it's only going to rebuild the stock to a lower level.

Now, if we still have the targets based off of the historical benchmarks, and not the current recruitment, then that would call for an even more aggressive rebuilding plan, with the lower recruitment, and so I think what we're missing here are those recruits, or are the projections with the stock-recruit curve that also incorporate the low recruitment into the projection years.

DR. SIEGFRIED: Dave, that's a good point, and it's something that we would like to do, but we just weren't able to do it in the past couple of days during this meeting, and it's something that we would want to implement and test and make sure that we're doing it correctly before we showed you results in that way, but you're absolutely correct.

If we used the benchmarks that were derived from applying the stock-recruit curve recruits, and the lower recruitment into the future, we would have a much more pessimistic view of the recovery time, for sure. I don't think the long-term yield expectation would change, because the benchmarks are still set to that stock-recruit curve, and it would just show that the rebuilding just wasn't happening as it was supposed to, but we would like to do that, but we just weren't able to do that on the fly in the past couple of days.

DR. CHAGARIS: Okay, but you do have that capability, and I understand that two days' time is not enough turnaround, but that is possible to do?

DR. SIEGFRIED: Yes, and it's something -- We have a forecasting working group at the Center now that is working -- There is even national efforts, and then, like Nathan told you, we have that RESTORE project, where we will certainly look at all of these different ways to improve on our projections, and that is just a one-year project, and so we should be, at this time next year, coming to the SSC with a much better suite of information and a suite of possibilities to provide when we give you management advice through projections.

CHAIRMAN NANCE: Thank you, Katie. Dave, anything else?

DR. CHAGARIS: No, and I think I've said all that I need to on that, and Katie has clarified things, I think.

CHAIRMAN NANCE: Okay. Katie, go ahead.

DR. SIEGFRIED: If Dave or anybody else that has experience with

SS wants to talk with us about that, as we carry out our efforts, and we really would like to make this as useful as possible.

This is just giving a little bit more information about what bar should we be reaching, and I just wanted to provide all of this in this report for you all to have complete deliberations. The SSB time series for the model is on the left, just in a simple plot for the whole time series. On the right is just the commercial landings, with the largest at the beginning of the time period, and so I just wanted to show you when they came in, and that was where the SSB dropped, just to show you that that's quite a large mortality, sudden mortality, on the fishery all at one time, and so that kind of explains that drop.

If you go to the next slide, I wanted to illustrate sort of what that SSB is projected to be at the end of the ten years for each of those runs that I have shown, and so the SSB from the model is plotted here, and I just showed a more relatively recent time period, from 1979 to the terminal year, and that's in blue, and the base ABC, which is the SSC's configuration for the projections, is in orange there, and so that's just -- In ten years' time, that's where the projections say the stock can recover to, or can get to, and then the yellow line is the stock-recruit curve with the assumption of an SPR target, and then the gray line is the stock-recruit curve applying MSY, and so I just wanted to show you.

These projections, in the short term, are not trying to recover to anything like SSB virgin, but it's just -- This is just to show you the relative difference between what we're assuming it can get back to in ten years across the projections.

CHAIRMAN NANCE: That's a very -- I really like that slide. Thank you for doing that. That's a very telling one, to me.

MR. RINDONE: You know what that means? Now you have to make it all the time.

DR. SIEGFRIED: I probably won't be doing your projections myself much anymore, but it wasn't hard to make, for sure, and I just think it's interesting, when we're talking about regime shifts, or when we're talking about what target we're trying to recover to, and it's just -- For me, it's a way to understand what we're trying to --

CHAIRMAN NANCE: It is, because, when we were talking about it on -- Whenever we were talking about it, Monday, I guess, and this visual, for me -- I can see it better than seeing it with numbers,

and so I do appreciate seeing this.

DR. SIEGFRIED: It's a tricky thing to be doing all this work in the background and then listen in, and I don't think I've been very useful today, but you have a pretty plot.

The next slide is just sort of reiterating what I showed to you yesterday, and we did consult with Mara, just to make sure that we weren't providing you anything that was going to lead you to violate anything with the rebuilding plan, and so we just wanted to make sure. If we are going to infer a regime shift, by saying, okay -- I acknowledge Dave's hesitancy to call it a regime shift, and so we can call it a change in assumed productivity, but it's just important that it be documented by -- That we give some justification for it.

I'm sorry, and not "we", but that the SSC gives some justification for it, and the next slide is just a paper that we've been discussing internally that has a really nice sort of matrix of values in there and how much evidence that you will need to accept a productivity regime shift in our stock assessments. I mean, this is a 2015 paper, but I don't think that we've really taken a look at it in our region, and it certainly is something we need to start paying more attention to, because we're talking about all this climate work, and we're talking about species moving, and we're talking about recruitment crashes and things like that in other species, and so I think it's important for us to start coming at this from an objective point of view, and it's nice to see that there's some literature out there that might be able to help us.

Now, I wasn't able to provide this to you in time for this meeting, but I'm hoping that it can be something that we can walk through together at the next open slot in the SSC's agenda. I can send that paper to Ryan or whoever to provide to the SSC.

CHAIRMAN NANCE: Go ahead and send it to Ryan, and he will distribute it to us. Thank you.

DR. SIEGFRIED: That's the end of my presentation. What I assumed would happen now is I can provide any other answers to questions, as you all are talking about this, and then I can write up the report with all of the tables of values for each of the model runs that I have shown, so that you have those as well.

CHAIRMAN NANCE: Okay. Doug.

MR. GREGORY: Thank you. This is good, and I appreciate all the work you all have done with this. Now, I have a question and then

a comment. Have you done projections with the long-term recruitment average? I am curious how different that would be from the ten-year recruitment average, because I'm still concerned about calling it a regime shift, when I think the population has been in bad shape for twenty or thirty years.

If you did the long-term recreational, that would kind of tell us that, and I don't know how difficult that would be, and so that's a request, if we can do it, and now my comment -- Can we go to the slide where you have the horizonal bars and the spawning stock biomass trend? Are these horizontal bars -- Are these ABCs? You have one is base ABC.

DR. SIEGFRIED: Yes, and they're all ABC runs. Sorry, and I didn't mention that, but these are all ABC runs, and I didn't show the OFL. I showed what you probably would manage, at least actively, but I can show the OFL ones as well.

MR. GREGORY: No, and I'm just slow at picking things up, a lot of times, and so the 4,000 ABC run for the base is the lower projection, but what is 4,000? That wouldn't be the ABC that we set, is it?

DR. SIEGFRIED: No, and that's where we would assume SSB would get to.

MR. GREGORY: So that would be SSB for MSY.

DR. SIEGFRIED: Well, for the base, it's SSB assuming F SPR 30. For the SR, it's assuming MSY, and, for the SR with the SPR ratio, it's also assuming F SPR 30, but it's assuming higher recruitment levels.

MR. GREGORY: Okay, and so those are the proxies for MSY, and the things that strikes me with this graph is spawning stock biomass apparently collapsed in the early 1980s, a decade before the fishery started, and the amberjack fishery really started after red drum was closed. The restaurants needed some meat for their dishes, and so that's why the fishery just took off in the 1980s, and so it doesn't appear to me that the fishery had this effect, and this was going on before the fishery started, and so that confuses me a little bit. That's all. Thank you very much.

CHAIRMAN NANCE: Roy.

DR. CRABTREE: Katie, the Fs that these are figured off are the same for all of the scenarios?

DR. SIEGFRIED: The gray stock-recruit curve is actually based on FMSY, and then the orange and yellow are based on F SPR 30.

DR. CRABTREE: Okay, and the base is based on -- The blue is --

CHAIRMAN NANCE: The blue is just SSB.

DR. SIEGFRIED: The blue line is just the SSB trajectory from the base run of the model.

CHAIRMAN NANCE: Any other questions for Katie on this presentation? Carrie, did you have any?

EXECUTIVE DIRECTOR SIMMONS: Thanks, Katie. Do you have like a MSRA table for the status quo allocation scenario, or do we still have to make a decision on something else first, before we do that? I can't -- Maybe I am getting confused with gag on that. I'm not sure.

DR. SIEGFRIED: I can produce a MSRA table. I just hadn't done it with all of the plotting and runs, but I can do it for the one that you choose. Do you mean in order to determine the status?

EXECUTIVE DIRECTOR SIMMONS: Right, and so should we be using the rebuild or the other, the 75 percent, I guess, right?

DR. SIEGFRIED: Let me -- I might have produced them. Just one sec.

CHAIRMAN NANCE: Carrie, isn't it 27/73 is the base?

EXECUTIVE DIRECTOR SIMMONS: Correct. Do we have the overfishing and overfishing criteria for --

CHAIRMAN NANCE: I think that's what Katie is going to bring up now.

EXECUTIVE DIRECTOR SIMMONS: Okay. Thank you.

CHAIRMAN NANCE: You're welcome.

MR. RINDONE: You guys used F rebuild last time, in January, to rebuild the stock by 2027.

CHAIRMAN NANCE: Okay. Thank you, Ryan.

MR. GREGORY: Let me jump in to finish my line of thought. If you look at this SSB trend, that does not indicate a recent regime

shift. If there was a regime shift, it happened back in the 1970s.

CHAIRMAN NANCE: You know, I think, when we throw out the term "regime shift", we're kind of -- In my mind, we're kind of throwing out a term that we don't really know whether it is or not, and so we certainly have a lower stock right now than we have had.

MR. GREGORY: Well, this says the stock is what it's been for forty years. It's been up and down, but it hasn't really changed since the 1980s, right?

CHAIRMAN NANCE: Yes, that's right.

MR. GREGORY: Thank you for letting me jump in. I'm sorry.

CHAIRMAN NANCE: Doug, absolutely. Will.

DR. PATTERSON: I also am kind of leery of the term "regime shift". I mean, I know it's in the literature now, and people have been talking about how to deal with this, and climate change is real, and we see shifts in productivity in systems like the North Pacific that are very apparent, but, in this case, there is a correspondence here with the data-rich period, and I think Roy was talking about this yesterday or the day before.

The productivity estimates are different across this time period, but we don't actually know whether that's a data issue or whether there's actually variance in the system due to a shift in the ecosystem, and we don't know what's driving that.

CHAIRMAN NANCE: I agree with that, Will. Roy.

DR. CRABTREE: But I think what we do know is that, if we put in place these new ABCs, and bear in mind too that these are FES numbers, and so that's what we're going to use to monitor the fishery, and so the restrictions on the fishery are going to get more restrictive, and we should have lower Fs than we've had in the past and be managing in a more conservative stance than we have in the past.

The chances of seeing some response out of the stock, it seems to me, go up, and then we can gauge, based on what that is, when we get the next assessment, and I think, potentially, when we get around to another full assessment and pull in -- There is going to be a lot of new information on amberjack, and so it could be quite a different look at things.

DR. SIEGFRIED: Okay, and so have the MSRA table, and I can just

email it to Jessica or Ryan, and is that appropriate?

CHAIRMAN NANCE: Yes. Who wants it? Jessica?

MR. RINDONE: I will take it, and I will funnel it, and just kind of a comment on the history of the assessment of the stock and its response to management, and it might certainly behoove the SSC and the council to consider keeping an eye on greater amberjack, to the extent practical, as we move through this, because the stock has shown a historic reticence to responding to changes in management.

DR. SIEGFRIED: I did just send that to -- Well, it's taking a second to send, but I just added a slide to the end of the presentation, so it would all be in one place. I mean, I appreciate Will's point, and I think Dave has brought it up before too, about being careful to call it a regime shift, and we're only implying that -- We're saying that the SSC would only be implying a regime shift if we change that rebuilding target, or if you recommended changing that rebuilding target.

CHAIRMAN NANCE: Yes.

DR. SIEGFRIED: If we're doing the opposite -- If we're doing not the opposite, but a slightly different thing, which is what Dave has asked for that we will work on -- It would be completely different if we assumed low recruitment as we're trying to achieve the same benchmarks that were set with the stock-recruit curve. Then we wouldn't be discussing whether this was a regime shift or not, and we would just be talking about poor recruitment because of low stock size, and so I appreciate the conversation about the language used, and I hope we can agree on something, going into the future, so that I don't confuse you all and so that I don't get confused.

This should look very much like what you all saw at the last SSC meeting that Nancie and Nathan presented with the SSB over MSST at 1.53. It does indicate overfishing, and it has not rebuilt, but it is over the MSST. Is this what you needed, Carrie?

MR. RINDONE: Yes, this is it, and so this is essentially telling us that, though the stock is no longer overfished, it is still rebuilding.

DR. CRABTREE: When I look at this, the MFMT, which is F 30 percent, is 0.242. Then, below that, where it says F current, it's 0.75 times F 30 percent is 0.302.

DR. SIEGFRIED: Roy, I just pulled this up from the document and moved text to columns, and so I think --

MR. RINDONE: I think some of the columns are not -- The definitions are not necessarily lined up with the variables. F current is supposed to be the geometric mean, basically the next row down. F current is supposed to be the geometric mean of the 2016 to 2018 fishing years, and that is 0.302. The math still works out for the F current over the FMSY proxy.

DR. PATTERSON: It looks like the FOY just needs to shift down one cell in the first column, and the F rebuild is missing.

MR. RINDONE: Yes, that looks correct, Will.

DR. SIEGFRIED: The FMSY proxy, the F SPR 30 percent, is 0.242. The F current is 0.302, and the fishing status is 1.25, using that F current. I think I might have cut you off a little bit there, Roy, when I was trying to explain the copy-and-paste thing. Were you going to say something else?

DR. CRABTREE: No, and I just couldn't -- I was having a hard time reconciling the F numbers with the -- That's all.

DR. SIEGFRIED: Yes. Sorry about that.

DR. FROESCHKE: Katie, can you just clarify, or confirm, that what you have on the screen is assuming that low-recruitment regime, and, if you were to go with a different assumption on that, it's likely that the stock status would be different?

DR. SIEGFRIED: Yes, and so I can -- I don't know if the stock status in all instances -- By that, you mean it's not over MSST yet, right? I mean, none of them are rebuilt. For instance, if I used the stock-recruit curve and MSY, the SSB MSST gets over one in 2024, but it's not that way at the end of the -- At the terminal year of the assessment, and so it does make a difference. This is with the recent recruitment, and it does change that status.

DR. FROESCHKE: So, operationally, it almost seems that the SSC needs to make a recommendation about the recruitment they want to assume, such that we could then figure out whether we need F rebuild or something else for the projections.

DR. SIEGFRIED: I mean, I have all of the F rebuild scenarios prepared, and I plotted those, so you get an idea of yield, but they all recover in 2027, but the ABCs that you asked for, which are 75 percent of F SPR 30 -- I mean, they're there as well, and

they're in the ABC plot.

CHAIRMAN NANCE: Carrie.

EXECUTIVE DIRECTOR SIMMONS: Thank you, Mr. Chair. I think my understanding was the SSC had made a decision on recruitment, both for the model and for the projections, and maybe that was my misunderstanding, but that they were going to go with the low-recruitment, and I think it's a ten-year scenario, 2009 to 2018, I believe, and could somebody -- Ryan, is that right?

MR. RINDONE: That was that lower period, yes, 2009 to 2018.

CHAIRMAN NANCE: Yes. That's correct, yes.

MR. RINDONE: It's the recent period that reflected the lower threshold, and then there was the data-rich period that was 1984 to 2018, and then there was the entirety of it, which is 1970 to 2018.

CHAIRMAN NANCE: We had a long discussion on that, but I think we did come up, at the end, with that, yes. Carrie.

EXECUTIVE DIRECTOR SIMMONS: Thank you, Mr. Chair. The follow-up on that would be, for the projections that you showed, Katie, that had the spawner-recruit carried forward, that was for greater amberjack, but just for illustrative purposes, if we had gone a different way with recruitment?

DR. SIEGFRIED: Yes, and it's to go along with the decision tree that I presented on Monday to show you the impacts, and so that was more a thought experiment before, and, this time, it just shows you, in practice, if a different assumption was made, what the impact would be.

CHAIRMAN NANCE: Thank you. Doug.

MR. GREGORY: Thank you. I don't recall if this was presented the other day, and, again, I have a hard time sometimes visualizing stuff. If you've got the graphs of what OFL would be, the OFL projections, for the different recruitment approaches, that would be helpful to me.

Now, from what others have said, my understanding is the approach that we're leaning towards now will give us lower OFLs and ABCs, but might recover more quickly, whereas the other approaches would take longer to recover, or you said they all recover in 2027, and that's what is throwing me off a little bit.

MR. RINDONE: Well, they're fixed to recover by 2027, Doug, because that's the end of the rebuilding plan, and so, if we don't rebuild by 2027, then we'll have to wipe the board and rethink what we're going to do, but, until we get to that point, the projections are supposed to target that rebuilding date.

MR. GREGORY: So we're going with lower OFL projection.

CHAIRMAN NANCE: Yes.

MR. GREGORY: We would be recovered with less yield coming out of the fishery, but we're recovering to --

CHAIRMAN NANCE: A lower bar.

MR. GREGORY: A smaller status determination criteria.

CHAIRMAN NANCE: Yes.

MR. GREGORY: Or lower. All right. I think this is the first time we've addressed anything like this, and so I apologize for being slow on this.

CHAIRMAN NANCE: No, it is, and that's why I think -- On Monday, we had a two-hour discussion on this, and so it's not -- You're not slow on the take-up on this, Doug. Will.

DR. PATTERSON: Back to the MSRA table, when Roy was talking about that 0.302, as far as it being F rebuild, if that is actually the FOY, then 0.302 is still -- It seems to be a mistake. Whether it's F rebuild or FOY, it should be less than the FMSY proxy.

DR. SIEGFRIED: That's F current.

DR. PATTERSON: Okay. Thanks.

CHAIRMAN NANCE: So the 0.302 is F current?

DR. SIEGFRIED: Yes.

MR. RINDONE: Right, and you divide F current, the 0.302, by the 0.242, and that gives you the 1.25, which is the F current over MFMT, and that tells us that the stock is currently undergoing overfishing, because that value is greater than one.

CHAIRMAN NANCE: Yes, but we're not --

MR. RINDONE: The other side of it is SSB at 2018, which is the terminal year in the model, divided by the minimum stock size threshold, and that's the second-to-last row there at the bottom, and that's 1.53, and so, because that's greater than one, it's not overfished. However, because that -- So the third line from the bottom, SSB at 2018 over the MSY proxy, because that's less than one, it means that the stock is not rebuilt, but, under this -- As Dr. Siegfried noted, under this recruitment scenario, you are rebuilding to a lower target.

CHAIRMAN NANCE: Doug.

MR. GREGORY: I apologize, and this will be my last time, but --

CHAIRMAN NANCE: I know it won't be, but you're okay.

MR. GREGORY: It will be a challenge, at least on amberjack, and so I'm having a hard time understanding that, if we adopt a more stringent rebuilding target, that it can be fixed at 2027 and be rebuilt, and we allow for greater landings, between now and then, than what we're doing under our proposed approach. To me, that seems counterintuitive.

MR. RINDONE: Doing so, Doug, would assume greater recruitment, because, if you're assuming that you can reach that higher rebuilding target, there has to be something to put back into the population, and so the discussion has been around how realistic it is that amberjack can achieve a level of recruitment consistently enough to be able to rebuild the stock to where we were previously trying to rebuild it to.

MR. GREGORY: Okay. I missed that. I forgot that part, but, still, if we're wrong, we're going to say we have a rebuilt fishery when it hasn't changed at all, or much at all. Okay. Thank you.

MR. RINDONE: Well, you're not actually saying it's rebuilt, because the SSB at the terminal year, at 2018, is still less than the SSB at the MSY proxy, and so it's not actually rebuilt yet. You're saying it's not undergoing -- It's not overfished anymore, but it's not yet rebuilt, and so --

MR. GREGORY: Right, but it would be rebuilt in six years.

MR. RINDONE: Yes, it could be rebuilt in six years, is generally what it's saying, but we're not starting the projections for this actually until like 2022 or 2023, Katie?

DR. SIEGFRIED: 2022.

MR. RINDONE: 2022, and so it's actually a more narrow time than that.

MR. GREGORY: But we're going to be at a much, much smaller stock size than otherwise.

MR. RINDONE: That's correct, and so what's the penalty for this decision, right, and that's what you guys are having to mull. If you're assuming that recruitment is going to be at this depressed state compared to what we have thought it could be in the past, and you're wrong, and you could be wrong in either direction, but, if you're comparing it to what we've looked at in the past, and recruitment is greater, then the stock will rebuild more quickly.

If you're wrong, and recruitment is even less, than this will suffer the same fate as the previous assessments that may have been overestimating what was possible with respect to recruitment, given how we had set up the projections following those assessments.

CHAIRMAN NANCE: Okay, and so we need a motion here.

DR. SIEGFRIED: Mr. Chair, can I just comment on that, please?

CHAIRMAN NANCE: Yes, you may.

DR. SIEGFRIED: Okay, and so I guess what's needed, it sounds like, Doug, certainly with this too, is that using that recent recruitment, which is lower than the long-term average, it's important to be clear that you think that the long-term recruitment potential has changed to this lower value.

By acknowledging that fact, you're changing the benchmarks, and an impact of that is a longer long-term yield. This is in FES units, and it quite a bit lower than the current take on the books. I guess what I would ask is just does the SSB believe that that long-term recruitment potential has changed, or do they think it's just a short-term issue?

If it's not something where you all think that the long-term recruitment has changed, then you can change the benchmarks with that, and we would need to do that work, where we provide projections with just recent recruitments projected forward, but still maintaining the old benchmarks calculated on the stock-recruit curve, and it just depends on whether the SSC really believes that the long-term recruitment potential has been reduced. Thanks.

CHAIRMAN NANCE: Luiz.

DR. BARBIERI: Right, Katie, and then I think what's missing here, right, for us to consider is a third scenario that Dave Chagaris brought up, right, that would be that you actually develop the target that needs to be reached, a new benchmark based on the stock-recruitment relationship, but then do the short-term projections using the lower-level recruitment, because that's what represents the current state of the population, and probably the level of recruits that can produce at this stage, but, until we see a change in that, that we actually see materialize, it's difficult for us to assume that higher recruitments are going to be forthcoming. I mean, it would be nice to have that part, so we could compare with this, right?

CHAIRMAN NANCE: Yes. Dave.

DR. CHAGARIS: Thank you. The question that Katie posed I think maybe, in my mind, is probably not the right question. I think the right question is has recruitment been -- Is recruitment lower due to recruitment overfishing, or is it some environmental regime shift?

Depending on how we go about those, we don't really have any evidence to suggest -- I mean, I think we have more evidence to suggest that it's been recruitment overfished than we do a regime shift that would cause them to be less productive, and so, if it's recruitment overfishing, then we should be taking more conservative measures to rebuild the stock.

The other thing that I kind of wanted to maybe point out, and perhaps this will help you, Doug, is, if you look at that figure that has the horizontal lines going across it, and you could think about -- Those are basically the different targets of where we would be rebuilding to, and so, if you think about that in terms of the proportion to the unfished spawning stock biomass, the gray and yellow lines are sitting at about roughly 25 percent of the virgin spawning stock biomass, and so going down to roughly half of that.

It puts you at 10 to 15 percent of unfished spawning stock biomass, if that gives you some perspective, and that's what I have been talking about, as far as moving the goalpost when we assume this recent recruitment in the projections, or at least the way it's done, but I completely agree with Luiz, and I think we're missing that third figure that would show us how do we rebuild to those existing benchmarks of 6,000 or 7,000, given that we're in a period

of low recruitment, and we don't have that information yet.

CHAIRMAN NANCE: Thank you. Roy. Katie, go ahead, to that point.

DR. SIEGFRIED: We haven't provided that to this SSC ever, and so I do think it's important to go down that line of research to be able to provide that to you, but this has been a decision that the SSC has made without any projections of that sort, up to this point, and so I think we're really -- If you want to talk about recruitment overfishing, is just general SSB low?

I mean, that's another way to put the question about recruits, is say do we think it's due to recruitment overfishing, a regime shift due to recruits, and, I mean, I don't think that we're actually saying that many different things, but we are missing that other piece, but the SSC has never actually seen that sort of comparison before.

CHAIRMAN NANCE: Yes, and we really haven't -- I mean, is that available?

DR. SIEGFRIED: We don't have that set up and ready to show you now, but it's something that we're currently investigating. It's definitely something that we want to be able to provide, because we don't think it's always appropriate to infer changes in productivity with our projections the way we have in the past, because of instances like amberjack. Sometimes it doesn't matter, but it matters here.

CHAIRMAN NANCE: Thank you, Katie. Roy.

DR. CRABTREE: I am sensing some discomfort about assuming the recruitments can't go back up in these things, and so, Katie, if we stuck with the current way the projections have been done, it would be 30 percent SPR, using recruitments coming out of the spawner-recruit relationship, right? Then my question to you would, given the low recruits we have now, what would be the difference in the ABC, or I guess the first year of this, which is -- What are we looking at, 2023? What would be the difference with the ABC under that scenario versus the ABC we're setting now?

DR. SIEGFRIED: I can speak to the directionality, and I don't have the actual numbers, because we haven't been able to complete those, but, if we -- Your first comment was true that, yes, we would be able to produce the benchmarks using recruits assumed off the stock-recruitment curve, with the SPR target, but we would assume, just in the short-term projection period, that we had lower recruits, which means the yield would probably drop in order to be

able to rebuild by 2027, but I don't know the exact comparison between that and this low recruitment assumption, but that would be more consistent with the rules on the books now, the first example, the thing that you asked for, Roy, than to change --

DR. CRABTREE: I would be okay if that relieves people's discomfort about making assumptions about how long-term productivity has changed. I am fine with doing that, if that's the path we want to go on, but I just have a feeling that what we're going to find out is we're going to come back in years or so, and recruitments are not going to have gone up, and we're going to be back in this same boat again.

If people aren't comfortable that they have seen enough, and, to me, the whole regime change is more to do with skepticism about what we think was going on with the fishery back in the mid-1980s and before that, as much as anything else, but, if people would be more comfortable with going with what's on the books now and then setting the catch levels under the low-recruitment scenario, I'm okay with that. To me, what's important is where we set the ABCs now, until we see some evidence that some of these improvements are occurring.

CHAIRMAN NANCE: Harry.

MR. BLANCHET: Thank you, Mr. Chair. I guess my -- I have always struggled with amberjack and assessments, and the Slide Number 9 that you have really kind of highlights the challenge that I have with it. The first fishery that we really had on this was a small, vertical line fishery that started in 1970, give or take. If you look at the spawning stock biomass estimates, it looks like, in 1970, the spawning stock was about one-half of what it was twenty years earlier, and that's not yet back to virgin.

Even before we had anything like a fishery, we had a much smaller spawning stock biomass than our target of virgin biomass, or our metric for virgin biomass, and I am just having a hard time making those two sides of that picture make sense to me, and so how did we go to --

I understand all of the low data side of the graph and all the rest of that, but it seems that we're talking about almost two different things, and whether we're talking about regime shift or whether we're talking about getting a better set of estimates, once we've gotten better information and what that actually pertains to, or whether we talk about what fraction of the stock is available to the fishery, it's a struggle to me how we get there.

DR. SIEGFRIED: To that point, Mr. Chair?

CHAIRMAN NANCE: Yes.

DR. SIEGFRIED: Sorry, Harry, to confuse things, and I should have shown all of the landings. There were substantial recreational landings assumed in the historical period, and this isn't meant to re-litigate the historical time periods of landings, and, I mean, I understand the consternation there. The reason that I was putting this out is just because of the landings that come in, and I apologize again, and I should have shown all of the different landings, rather than just commercial.

We also are assuming -- We have to fit the composition data and the patterns in the composition data, particularly the cohorts, in SS, and it does have to be explained somehow back in time, and so that's part of where this SSB trajectory back in time comes from. I didn't mean to confuse people with just showing commercial.

All I wanted to show was just what the model assumed SSB to be historically and then in the more recent time period, to give you all an idea what the targets were, but, in order to really address that point from Harry, or Doug, for that matter, we have to look at all of the landings time series, so that you could better understand that trajectory. We did that pretty extensively in the panel, and initially, when we showed all of the results to the SSC in January, but I do apologize for causing confusion.

MR. BLANCHET: My confusion is long-standing and well-known with this group.

CHAIRMAN NANCE: Thank you, Harry. Roy.

DR. CRABTREE: Let me suggest, just to get us out of this -- In listening to Katie, and concerns from Dave and others, my suggestion would be let's just stay with the current reference points and do the projections out with the spawner-recruit, and I think that's what we've done in the past, and then we set the ABC based on this last ten years of recruitment, something that, in theory, could recover if we get all these high recruitments.

Then we're not making these changes about productivity and regime change, which are getting us so wrapped around, and we move on, and I guess, Katie, does that seem to be the Center's perspective of maybe the better course of action at this point?

DR. SIEGFRIED: Yes, I would say so. In our discussions, we're

just -- Like I said, we talked to Mara and everything, making sure we weren't deviating from how the rebuilding plan was set up originally, and it may be pretty difficult to justify a regime shift according to that article that I showed you, and I know I keep using that term, but we would have to do --

DR. CRABTREE: So what do we need for a motion on that, Jim, that we recommend that we continue with the 30 percent SPR reference point, rebuilding projections using the spawner-recruit curve recruitments, and ABC set based on the low-recruitment scenario of 2009 to 2018, and I think the F was 75 percent. Then the question, Katie, is, is that 75 percent of F 30 percent sufficient to rebuild it under that scenario, or do we have to go to an even lower F?

DR. SIEGFRIED: We haven't done that exact run yet, because we don't have that configured to use just the recent recruitment, and so it would have to be something that we provided after the meeting.

MR. RINDONE: Well, so that's two different things, because, if it's 75 percent of F at MSY, or its proxy, it may rebuild at a different pace than F rebuild, which would be designed to rebuild by 2027 on the target. The goal is to rebuild by 2027 or sooner, because, at Tmin, it would still be less than ten years, and so you're required to rebuild within ten years.

DR. CRABTREE: I guess my question is, does that motion then, Katie, cover us and give you what you need, even though I think, right now, we don't know what that poundage for the ABC would be?

CHAIRMAN NANCE: Carrie.

EXECUTIVE DIRECTOR SIMMONS: Thank you, Mr. Chair. Katie, could you check, or Nancie perhaps, and have we historically done the projections for amberjack using the spawner-recruit curve, or have we just used that longer time series, like back to the 1980s, to do the projections for recruits?

DR. SIEGFRIED: I looked into it pretty carefully, Carrie, and it was pulled from the spawner-recruit curve for 33, and I think Steve Atran was the one who helped change the 2012 rebuilding year to 2027, and then I did check with Nancie, and, before this current version of SS, we couldn't actually pull from the estimated recruits, and we would have had to assume the stock-recruit curve, and so that's what I have found during this week, that it was pulled from the stock-recruit curve.

EXECUTIVE DIRECTOR SIMMONS: Thank you.

DR. CRABTREE: That was my purpose for putting it in there, was consistency, because I thought that's what you had said.

DR. SIEGFRIED: Yes, and so I think that -- I know the rebuilding year is 2027, and I would be able to run a projection with the information on the screen, once we got that methodology, or just the toggles in SS figured out, and then provide that, yes.

CHAIRMAN NANCE: Let's get -- We have a motion, and I'm going to read it. We need a second though.

DR. BARBIERI: I will second the motion to open it for discussion.

CHAIRMAN NANCE: Okay. Let me just read it, so we're all -- **It's to continue with the 30 percent SPR reference point rebuilding projections using the spawner-recruit recruitments and ABC based on the low-recruitment scenario (2009 through 2018).** Luiz.

DR. BARBIERI: Well, and my question, Katie, for you, or for Roy, is like -- I mean, if we're still in a rebuilding plan, right, how can you actually get an F rebuild, which should be a constant, and I guess you just inform that for the next three years, for the F rebuild with the low recruitment? You're going to have an F rebuild that comes out of the higher --

DR. CRABTREE: Well, there's going to be an F rebuild that will rebuild us up, assuming the recruitments are going to go up, but we're going to set the ABC based on that F, with the current level of recruitment, and just maintain that over the next several years, until we see some evidence that we actually are getting higher recruitments and good things happening.

DR. BARBIERI: Gotcha. Okay.

CHAIRMAN NANCE: Okay. Doug.

MR. GREGORY: Thank you. Originally, I thought this was similar to what Dave Chagaris asked earlier this week, but maybe not, because, if we were to base F rebuild on the low-recruitment scenario, with F rebuild gives you ABC, we would never get to the 30 percent SPR reference point, because there's not enough recruitment, but we don't know what recruitment is going to be, and I think that's what is in this motion. Are you saying that we'll set an ABC based on the low-recruitment scenario and just keep it constant until 2027?

DR. CRABTREE: Well, it will stay constant until we give the

council another ABC and they change it, and so, to me, what that means is it will remain in place for the next few years, until we get an updated assessment, or an interim assessment, and having it start going up is dependent on actually realizing higher recruitments, which may or may not happen.

MR. GREGORY: But I'm concerned that the lawyers would kill this, because your projected -- You won't be projecting recovery by 2027. You're just hoping you will get it, and my experience is --

DR. CRABTREE: You will be projecting it, because you're going to -- The projections are going to have recruitment going up and fishing at F rebuild. Now, whether that will happen or not is a whole different thing, but the projections will show recovery. What I don't want us to do, what I wouldn't do, is put in place ABCs that go up every year based on the projected increasing recruitment, because then, if the recruitment doesn't increase, and it hasn't recently, you're going to be overfishing. Do you follow me, Doug?

MR. GREGORY: Yes.

CHAIRMAN NANCE: Trevor.

MR. MONCRIEF: I just had a quick question just related to what we're getting ready to vote on. Ryan, if we go through with this on the ABC side, what kind of prognosis would that look for in the fishery? What kind of management changes will we be looking at, given that we can't predict those at the moment, but --

MR. RINDONE: Well, it's going to be a pretty steep cut, because we're going to FES, and the majority of this species is allocated to the recreational sector, and so the harvest rates and the fishing effort is projected to go up considerably under FES, and we have about the same amount of fish, at least numerically, and the currency is different, but, numerically, as we have now. Given that difference, I mean, it could be as much as half, give or take.

MR. MONCRIEF: All right. I will say, and I think this is the time for this, is this is a stock that's been -- I mean, everyone has been looking at it for a long time, and, given, this is my first swing on the SSC, and so I'm lot of the assessment for the first time in detail, and having it presented to me, but I think there's been a --

We haven't realized a drop in mortality every time we make these changes, and I think, for this one, if we are talking about making this decision on this fishery, we should probably give it a fair

amount of thought, and everybody, fishery managers too, give it a fair amount of thought, as to what's the best way to move forward with this fishery to not completely shoot ourselves in the foot on the management regime side of things, but try to do it in a way that kind of makes up for it.

My thought process, right now, is we probably need to look at the distribution of landings and the timing of them and see what we can do on that end first, before we go to another length limit change or bag limit change, and I know this is a little bit off-topic for what we're getting into, but I think it's just -- It's worth -- If we're going to go down this road with this fishery, we're going to have to really think about the best way to go about management.

CHAIRMAN NANCE: Roy.

DR. CRABTREE: I think you're looking at some really painful reductions, whether you choose this scenario reflected in the motion or if you just went with the reduced productivity and rebuild to a lower state. Either way, I mean, you're talking some pretty painful reductions in the quotas.

MR. MONCRIEF: Yes, absolutely, and the only point I was making is that I think those -- As we all know, those reductions can come in multiple forms, and this is one of those where we really probably need to have a lot of thought process to how to get to those.

CHAIRMAN NANCE: I am going to read the motion. **To continue with the 30 percent SPR reference point rebuilding projections using the spawner curve recruitments and the ABC based on the low-recruitment scenarios (2009 through 2018) for greater amberjack. Any opposition to this motion?**

DR. PATTERSON: I vote no.

CHAIRMAN NANCE: Okay. Let's go ahead and bring up the table, and we'll go ahead and vote. Jim, did you have a question, or you were going to vote no?

DR. TOLAN: Thank you, Mr. Chairman. I was just going to ask a clarification before we vote.

CHAIRMAN NANCE: Absolutely. Please do.

DR. TOLAN: The way it's worded, "to continue with", if the motion fails, what's the default position for greater amberjack?

CHAIRMAN NANCE: We have to make another motion to come up with a different scenario.

DR. TOLAN: Okay. Thank you.

CHAIRMAN NANCE: You're welcome.

MS. MATOS: Steven Scyphers.

DR. SCYPHERS: No.

MS. MATOS: Jim Nance.

CHAIRMAN NANCE: Yes.

MS. MATOS: David Griffith. Roy Crabtree.

DR. CRABTREE: Yes.

MS. MATOS: Luiz Barbieri.

DR. BARBIERI: Yes.

MS. MATOS: Michael Allen.

DR. ALLEN: No.

MS. MATOS: Cindy Grace-McCaskey.

DR. GRACE-MCCASKEY: Yes.

MS. MATOS: Josh Kilborn.

DR. KILBORN: No.

MS. MATOS: Steven Saul.

DR. SAUL: Yes.

MS. MATOS: Jack Isaacs.

DR. ISAACS: Yes.

MS. MATOS: John Mareska.

DR. MARESKA: Yes.

MS. MATOS: Lee Anderson.

DR. ANDERSON: Yes.

MS. MATOS: Dave Chagaris.

DR. CHAGARIS: No.

MS. MATOS: Doug Gregory.

MR. GREGORY: Abstain.

MS. MATOS: Trevor Moncrief.

MR. MONCRIEF: Yes.

MS. MATOS: Sean Powers.

DR. POWERS: No.

MS. MATOS: Jim Tolan.

DR. TOLAN: No.

MS. MATOS: Rich Woodward.

DR. WOODWARD: Abstain.

MS. MATOS: Will Patterson.

DR. PATTERSON: No.

MS. MATOS: Paul Mickle.

DR. MICKLE: Yes.

MS. MATOS: Benny Gallaway.

DR. GALLAWAY: Abstain.

MS. MATOS: Harry Blanchet.

MR. BLANCHET: Yes.

MS. MATOS: Jason Adriance.

MR. ADRIANCE: No.

MS. MATOS: Luke Fairbanks.

DR. FAIRBANKS: Yes.

MS. MATOS: Mandy Karnauskas.

DR. KARNAUSKAS: Yes.

CHAIRMAN NANCE: It looks like the motion carried fourteen to eight with three abstentions. Thank you, everyone. We still need to, before we get off the topic of greater amberjack, we need to still do stock status.

The fortunate thing now, when we blessed the model, and we don't have the status determination until we run these projections, and so we don't know what the status is, and so now we need to go back and say the model in here is what the status of the stock is, and so we need a motion on that.

DR. BARBIERI: Do we need a motion?

MR. RINDONE: You need that information in front of you, and then, yes, you guys need to make a recommendation as far as what you have observed to be the stock status, given the projections.

DR. BARBIERI: Well, I have a motion to that effect, if you --

CHAIRMAN NANCE: Okay. Please do, Luiz.

DR. BARBIERI: Motion that the SSC determined that the SEDAR 70 operational assessment of Gulf of Mexico greater amberjack represents the best scientific information available and, based on assessment results, the stock is undergoing overfishing and not overfished. Then, in between parentheses, Jessica, but still rebuilding.

CHAIRMAN NANCE: Is there a second on that motion?

MR. MARESKA: I will second it.

CHAIRMAN NANCE: John will second. Is there discussion?

DR. CRABTREE: A question for Katie. Katie, is it accurate that, under this set of reference points, that we are no longer overfished? I don't know that that's correct, because the bar on the MSST are higher than under the low-recruitment scenario.

DR. SIEGFRIED: Right, Roy, and so I was pulling that MSRA table from that run, and it is still below MSST if the stock-recruit

curve is used, and so I can give you the MSRA table, but I can't give projected yields until we run it with the lower recruitment, and so I wanted to pull that up here, real quick.

CHAIRMAN NANCE: Please do, Katie, so we can get this correct.

DR. BARBIERI: Can you confirm that then, that the numbers using this -- The stock is undergoing overfishing and overfished?

DR. SIEGFRIED: Yes, and so I'm pulling up the MSRA table, but just in 2022, in the run that I did, it was still below MSST in 2022, and so I'm getting the actual numbers now.

DR. BARBIERI: Thank you, Katie. Good point, Roy.

CHAIRMAN NANCE: John, are you still okay with that? Okay. Thank you. Okay. Any discussion on this motion? Let me read the motion. **The SSC determined that the SEDAR 70 operational assessment for Gulf of Mexico greater amberjack represents the best scientific information available, and, based on assessment results, the stock is undergoing overfishing and is overfished. Any opposition to this motion?**

MR. RINDONE: Mr. Chair, you might specify in your motion, or not yours, but the motion maker, that, as of a certain point in time, the terminal year of the assessment being 2018, and this has been a recent practice of the SSC, to include the terminal year of the assessment, to make the motion more precise.

CHAIRMAN NANCE: Okay. We have an addition here. Where would we put that?

MR. RINDONE: Based on the assessment results, as of 2018.

CHAIRMAN NANCE: Okay.

MR. RINDONE: That would just bring this in line with some recent motions that the SSC has made to the same effect.

CHAIRMAN NANCE: Thank you. Okay. Thank you. I appreciate that. Okay. I am going to read the motion. It looks like we have some -- Doug and Steven, are those opposition to this or questions?

MR. GREGORY: I am in opposition, because of the inclusion of the 1960s recreational data that I think is flawed.

CHAIRMAN NANCE: Okay. I just wanted to make sure that you were in opposition. Steven, the same for you?

DR. SCYPHERS: I was raising my hand to ask a question, if we were going to have discussion after the second.

CHAIRMAN NANCE: Go ahead, and we'll have a discussion. Steven, please.

DR. SCYPHERS: Okay. Thank you very much, Mr. Chair. I was just simply going to ask for a little bit of explanation on the change that just happened. I followed, when we talked through the table earlier, the explanation of how we got the not-overfished scenario, and then I see the change just happened to overfished, and I just wanted to make sure that I understood how that happened. Thank you.

CHAIRMAN NANCE: When we produce a new projection, then the status can change, and so, under the one scenario that we were looking at first, the stock was undergoing overfishing, but it was not overfished. With our recent motion to use what we had used in the past, the stock now becomes undergoing overfishing and is overfished, and so, unlike how we used to do -- The assessments were run, and then we would see the status of the stock, but, as we change a projection, now the status of the stock changes with that projection, and so we have to wait until the projection is determined for what we want to see, and then we can come back and look at the status of the stock. Does that make sense?

DR. SCYPHERS: It does make sense. Thank you very much. Is this something where it would be possible to see that table that shows us -- If I remember correctly, the value is like 1.25 prior to the change that would adjust it.

CHAIRMAN NANCE: I think Katie is working on that right now.

DR. SCYPHERS: Okay. Thank you.

CHAIRMAN NANCE: You're welcome. Harry.

MR. BLANCHET: Mr. Chairman, I thought that -- The first part of this motion is saying that this is the best scientific information available, and I thought that we had already done -- That we had already accepted that, and that was what my vote on the prior motion was based upon, was that we had already accepted this as the best scientific information available, and so am I incorrect in that?

CHAIRMAN NANCE: No, and you are correct. On Monday, we voted that the assessment, the SEDAR 70 operational assessment for

greater amberjack, represents the best scientific information available, and we then, on Monday, put a period there, because we hadn't seen any projections yet, and so we didn't know the status, and all we're doing, I think, on that motion, while it's redundant to say that again, but we did.

While it's redundant to say it again, I think it just adds, I think, clarity to our motion that -- I stand corrected. I am sorry. Harry, you're right. Okay. so we did it for gag on Monday, but not for greater amberjack, and so I stand corrected here. We need to have it here for consistency.

MR. BLANCHET: Okay, because that was the reason that I voted for the prior motion, was because I had understood that we had already accepted this.

CHAIRMAN NANCE: We had accepted, back in January, when we originally voted on it, but there have been some changes that have occurred since that January assessment run.

MR. BLANCHET: Okay.

CHAIRMAN NANCE: So I think we needed to do it here so that this model that was shown to us on Monday and the projections run today we consider as the best scientific information available.

MR. BLANCHET: Okay.

CHAIRMAN NANCE: Let's see. Jason.

MR. ADRIANCE: Thank you, Mr. Chair. I have some of the same reservations that Doug has, and I hope that some of these discussions today, looking at that historical SSB and that historical recruitment -- Were those truly what they are, and it highlights our need for a research track on this species, and I know we have the amberjack project coming up, but I just wanted to get that out there, that I think this one definitely needs a research track.

CHAIRMAN NANCE: Thank you. Yes, I think that's appropriate. Any other questions or comments on this? Let's go ahead and bring up the table again. We're going to vote on this one. I know there is some opposition to it, and so we'll do the table. I'm not sure how you start, but that's good. Random selection, that's good. Okay.

Before we start, let me read the motion, just so we all understand what we're voting on here. **The SSC determined that the SEDAR 70**

operational assessment of Gulf of Mexico greater amberjack represents the best scientific information available, and, based on assessment results, as of 2018, the stock is undergoing overfishing and is overfished. That's the motion we're voting on.

MS. MATOS: Jim Tolan.

DR. TOLAN: Yes.

MS. MATOS: Sean Powers.

DR. POWERS: No.

MS. MATOS: Trevor Moncrief.

MR. MONCRIEF: Yes.

MS. MATOS: Doug Gregory.

MR. GREGORY: No.

MS. MATOS: Dave Chagaris.

DR. CHAGARIS: Yes.

MS. MATOS: Lee Anderson.

DR. ANDERSON: Abstain.

MS. MATOS: John Mareska.

MR. MARESKA: Yes.

MS. MATOS: Jack Isaacs.

DR. ISAACS: Yes.

MS. MATOS: Steven Saul.

DR. SAUL: Yes.

MS. MATOS: Josh Kilborn.

DR. KILBORN: Yes.

MS. MATOS: Cindy Grace-McCaskey.

DR. GRACE-MCCASKEY: Yes.

MS. MATOS: Michael Allen.

DR. ALLEN: Yes.

MS. MATOS: Luiz Barbieri.

DR. BARBIERI: Yes.

MS. MATOS: Roy Crabtree.

DR. CRABTREE: Yes.

MS. MATOS: David Griffith. Jim Nance.

CHAIRMAN NANCE: Yes.

MS. MATOS: Steven Scyphers.

DR. SCYPHERS: No.

MS. MATOS: Mandy Karnauskas.

DR. KARNAUSKAS: Yes.

MS. MATOS: Luke Fairbanks.

DR. FAIRBANKS: Yes.

MS. MATOS: Jason Adriance.

MR. ADRIANCE: No.

MS. MATOS: Harry Blanchet.

MR. BLANCHET: No.

MS. MATOS: Benny Gallaway.

DR. GALLAWAY: Abstain.

MS. MATOS: Paul Mickle.

DR. MICKLE: Yes.

MS. MATOS: Will Patterson.

DR. PATTERSON: Yes.

MS. MATOS: Rich Woodward.

DR. WOODWARD: Abstain.

CHAIRMAN NANCE: It looks like carried with seventeen to five and three abstentions. Thank you. I think that takes care of greater amberjack, correct?

MR. RINDONE: It takes care of it insofar as you guys have recognized the assessment as BSIA, and you have made a decision about stock status, given what you have requested for projection settings, but you guys still have not recommended an OFL and an ABC, and so we'll have to revisit that when those data are available. The soonest that we could do that would be November, I would think. Katie, is that palatable?

CHAIRMAN NANCE: It looks like Katie has her hand up.

DR. SIEGFRIED: I did want to check on the first year of management.

DR. RINDONE: Dr. Simmons is perpetually glass-half-full and thinks 2022.

DR. SIEGFRIED: Okay. Then that shouldn't be a problem.

MR. RINDONE: But she is sauntering over to the microphone, and so maybe she's a little glass-half-empty today.

EXECUTIVE DIRECTOR SIMMONS: I don't know. It's taken us a year to get through this, and we still don't have catch advice, and so I don't want to make you guys redo everything, because that seems to be what happens, and so let's stay with 2022.

DR. SIEGFRIED: Thanks, Carrie.

CHAIRMAN NANCE: Okay. Thank you, Carrie. Okay. That finishes greater amberjack. We need to do gag grouper. Do you guys need a few-minute break? Let's go ahead and have a ten-minute break, and then we'll get into gag grouper.

(Whereupon, a brief recess was taken.)

GAG PROJECTIONS

CHAIRMAN NANCE: Okay. It looks like we're ready to start here, and so we're going to have a discussion on gag grouper projections. Any comments or issues before we start that process? Luiz.

DR. BARBIERI: Thank you, Mr. Chairman, and I am just trying to get my ducks in a row here. My understanding is that -- I see the presentation being queued up there, and I don't know if it's going to be Katie or Lisa who will present the revised projections, but an issue that I wanted to bring up for SSC discussion, and discussion with the Center as well, is the possibility of us having the full set of diagnostics that were associated with the run, the run for the gag assessment, that used the Florida State Reef Fish Survey.

At that time, the survey was called the Gulf Reef Fish Survey, and so it shows up in the report and in the presentation as GRFS, and the reason I am bringing this up is because, over the last nine or ten years, we've been working, through the MRIP implementation team, our original MRIP implementation team, the MRIP folks in S&T, and all the different states and interested partners, and we have been developing these surveys that are supposed to be more specialized surveys to address some of the issues of reef fisheries and other council-managed fisheries, some of them that may not be properly sampled by a general survey like MRIP.

The idea of SRFS was to develop something that is integrated, actually, and there is a component, the APAIS component of the survey, that is conducted jointly with MRIP, and it's to be fully integrated into that part, but it's targeted for those reef fish species that are council managed.

With that, we have obtained estimates that we believe are superior and that have lower uncertainty, and they can be produced on a monthly time scale, and so they are more timely as well than the regular MRIP survey.

It's something that, of course, has been discussed extensively in the region, and I understand that, and it's a contentious and controversial issue, for sure, but, for a stock like gag, because ninety-nine-plus percent of the landings are actually in Florida, we feel that this switch from the more general survey to a more specialized survey would actually improve landings statistics without us having to deal with that Gulf-wide calibration for this, because it's really a Florida fishery.

I wanted to bring this up for SSC discussion, because conversations that I have been having with the folks -- The S&T folks and the MRIP folks, discussions that we've had through the Gulf implementation team that has been looking into this, is their guidance is that we would be looking into the SEDAR process, the stock assessment process, and take those stock assessment panel -

- Recommendations from the stock assessment panels and from the SSC as the decision point for what data sources are to be integrated into stock assessments.

Here we have, and I understand it's an operational assessment, and so there are limitations to what can be accomplished, but, with us not be able to cross this bridge again and have another assessment before, at the earliest, 2025, is an opportunity to have this process moved forward.

I will leave it there for discussion, and then, if at all possible, and those projections can be provided with the associated model runs and diagnostics, that the SSC potentially can, for the January meeting, have a more in-depth discussion of the pros and cons of this and then proceed accordingly. Thank you, Mr. Chairman.

CHAIRMAN NANCE: Thank you, Luiz, and I think this is what Leann brought up on Monday, during our discussion.

DR. BARBIERI: Correct.

CHAIRMAN NANCE: Any SSC discussion on this topic? Trevor, please.

MR. MONCRIEF: I have a quick question of Luiz. To your knowledge, are those landings for SRFS directly proportional to the MRIP landings over time, for the last ten years at least?

DR. BARBIERI: Yes, directly proportional, according to some constant proportionalities, and so we have a calibration process in place that was developed, and that calibration paper, which was provided as part of the documentation for the SEDAR 72 assessment, was developed in collaboration with the MRIP program, and it was reviewed, peer reviewed, by the MRIP statistical consultants, and so, as far as we are concerned, that has been accepted by the MRIP program as an official calibration for SRFS and MRIP.

MR. MONCRIEF: Yes, and I was a little bit unclear on my question, and I'm sorry there, and I know you went through the calibration process, to go back in time and everything else, but I was thinking the comparison year-by-year, and are they pretty well proportional? Pre-calibration, if you just line up like just 2017, 2018, and 2019, of you all's 2017, 2018, and 2019, with MRIP, are they --

DR. BARBIERI: Well, again, it depends on -- There are outputs -- We need a motion? Well, okay. I can make a motion. Let me send a motion to -- Sorry about that, Trevor. Trevor, your question is whether the relationship between MRIP and SRFS is constant over

time?

MR. MONCRIEF: Yes, and it's probably just something we could talk about on the side, because, obviously, it's going to be calibrated back in time, and they're going to be proportional to -- That's just how it is, and I was just wondering from the point estimate perspective, year by year, just to see how they compared, but we can go over that later on.

CHAIRMAN NANCE: Okay, and so here is the motion. **The SSC requests that the Southeast Fisheries Science Center provide model diagnostics for the gag SEDAR 72 assessment run that uses the Florida SRFS for the private recreational sector. These model results are to be reviewed by the SSC at its January 2022 meeting.** Do we have a second for this motion?

DR. TOLAN: Second, Mr. Chair.

CHAIRMAN NANCE: Jim, thank you. Katie.

DR. SIEGFRIED: Thank you, Mr. Chair. I just have some questions for Luiz about the motion and sort of the background. I think I said this on Monday too, but it was my understanding, being part of this process all the way through, even to the point of where we had a meeting to discuss what the GRFS data were and what would be included, and that composition data from the program were already used in our model, and it's just a matter of using the time series and how we could use the time series, and that it would be treated as a sensitivity run.

Because it was put in that box of, okay, we're treating it as a sensitivity run, we don't have the historical data, and we didn't really fully discuss, as a panel, the uncertainty, or issues, around this program, or the four years of data that have been collected, and that calibration has been approved basically by S&T, but we haven't discussed the calibration back in time, and we certainly -- Back to 1981, and we certainly haven't discussed the calibration or how we would calculate anything in the historic time period.

If it would have been put forward as, you know, potentially use this for the base run, we would have discussed it very differently as a panel, and so we really didn't discuss it in a way that's complete, and I am pretty certain that the diagnostics would show issue with a lot of what we didn't follow through with on the sensitivity run.

We don't carry the sensitivity runs to completion, and Lisa can

talk a little bit more, and she talked a little bit in her presentation about how much those recreational data affect the runs, and so that already is an issue, and then, if we got the diagnostics, I'm not really sure that it would be able to still support its use and then eventual projections and management advice using the GRFS data just for the private recreational sector, because there is -- We would be ignoring the interrelatedness of all of the different fleets, and particularly the modes of the recreational sector.

We just haven't given it the full attention to be able to confidently put this forward as using it within the model, potentially in a base run scenario, and so I am concerned about that, and we would have treated it differently if it was put forward for that in the first place, which is what we're doing for the SEDAR 74 research track.

We're trying to start off with a time series that we can actually compared, to take a look at all of the calibrations, to discuss how we can combine the data from the state programs together, if we can, and then what does it really mean to consider in the model, and how can we do that.

I wish that this would have been forward that way in the first place, so we could have treated the way it's being intended, it sounds like, now, and I think what we can do, potentially, is a workaround, and it would be to provide the calibration, or just basically a scalar, at the end of the process, and that would only really work because it's mostly a Florida fishery, but that would still have its own issues.

It really wouldn't be the way we would normally like to do it, and, I mean, we have all these modes in our recreational fleet that we have modeled separately, and it's just Florida, and it's just private shore, and so this model is just not well equipped to address this concern at this point. Let me see in my notes what else I meant to say.

DR. BARBIERI: If you want to pause there for a second, Katie, I will try to address at least some of your points.

DR. SIEGFRIED: Sure.

DR. BARBIERI: Those are great points, Katie, and let me start by saying -- I mean, my idea was not to come in at the thirteenth hour and throw a bomb like this into the room and be disruptive to the process that we have been engaged in throughout this whole process for the SEDAR 72 assessment.

As you know, I was a member of the assessment panel, and so we discussed a lot of this, but what happens is that, because the SEDAR process functions according to whatever is in the terms of reference, or additions to the terms of reference, which, in this case, is a letter from the council that went to the Science Center.

MR. RINDONE: It went out like a year in advance.

DR. BARBIERI: Right, but requesting for this sensitivity. We discussed a little bit how the SRFS, or GRFS, data would be used, but I realize that, at that point, your hands were pretty much tied, by the process, to sort of depart from what had been requested originally and what was included in terms of reference.

Unfortunately, yes, this is coming in at this point, because, as the SSC reviews the assessment, I thought you would have the ability to evaluate all the different data inputs and make recommendations and request additional runs, or additional settings, right, configurations, for the model.

I acknowledge all of this, and, Katie, I am just putting this out there, because it is an opportunity for us to address an issue, considering our SEDAR assessment schedule and how often we conduct assessments, and it takes a long time, and this is something that we have been developing, working with not just NOAA Fisheries, but our regional partners, through the GulfFIN and the MRIP implementation plan, for a long time, with the idea that, at some point, this would be ready to be used for assessment and management.

It came across as a little complicated, and, I mean, I apologize for that, Katie, but I didn't really see any other opportunity earlier to bring this up, but I understand your points, and I don't necessarily disagree with them.

CHAIRMAN NANCE: Thank you. Ryan.

MR. RINDONE: Thank you, Mr. Chair. When I say that it was sent out a year in advance, I was referring to the terms of reference, and so the progression is typically that we try to have the scope of work finalized at least a couple of years in advance, and then the terms of reference a year in advance, to allow everyone the proper time for preparations.

With respect to -- I think that you had indicated earlier, Dr. Barbieri, that the certification of your program's calibration had occurred in between that time, and so, given how long sometimes

that the SEDAR process takes, and it's a slow bake, and we don't microwave it if we don't have to, and things can -- Circumstances can certainly change along the way.

CHAIRMAN NANCE: Thank you, Ryan. Roy.

DR. CRABTREE: Would it not be possible to take the ABC that comes out of the gag assessment, and then the council will allocate that into commercial and recreational, and then take the recreational portion, which would be in FES currency, but then convert the appropriate fraction of that into GRFS currency, and then use GRFS to track the fishery?

My understanding is, since we have this calibration, that we can convert back and forth, and then that would seem to make it unnecessary to go through all these assessment steps, and it seems, to me, that's something that the council could just effectively say they want to do.

I guess you would have to somehow figure what fraction of the recreational quota is going to be caught by headboats and charter boats, because they are kind of distinct, but I'm sure that people could figure that out, but would that be an acceptable alternative to going through all of this?

CHAIRMAN NANCE: Katie.

DR. SIEGFRIED: That is exactly what I was saying we think is a good option here, Roy, and so you said it as well as I could have said it, and, obviously, that is what we would propose, for the reasons that I stated and then the reasons you stated, that there is the units for monitoring, but we haven't actually addressed all of the units for the assessment, and so we would like to proceed with that, and we would more than happy to participate in that exercise.

DR. CRABTREE: I think that's just a matter of the council specifying that in the plan, which we would see, ultimately, and review at some point.

CHAIRMAN NANCE: Luiz, to that point, please?

DR. BARBIERI: Thank you for that, Roy and Katie. I mean, obviously, this will be a plus, right, because it will allow us to use the survey to monitor the fishery, but I still felt it was important, because of the lower uncertainty that you have with SRFS, just because it's a more targeted survey, and that would be important to make this request and have this issue discussed,

because, if it's possible to have this integrated and having the yield streams, stock status and yield streams, that come out of the assessment be based on that, I would certainly prefer to have those, but that's why the discussion came up, yes.

CHAIRMAN NANCE: Thank you. Doug.

MR. GREGORY: Thank you. Is it feasible for the Center to give the State of Florida the data and the code and, Luiz, your assessment scientists can run the model themselves? I mean, I have been to two review workshops, and I have seen these international scientists come in and demand the data and the model and run their own versions of it and play with it, and so it seems like that's doable, and that's one way to try to tackle this.

If your results come out to be significantly different, and not in a statistical sense, but then we can come back to the SSC and review it, and, of course, NMFS will be reviewing it, and we can go forward from there.

DR. SIEGFRIED: Mr. Chair, to that point?

CHAIRMAN NANCE: Yes, please, Katie.

DR. SIEGFRIED: The models that are put forward for management advice have been going through the SEDAR process, and I thought that was the agreed-upon process in the Southeast, and to circumvent the process is sort of out-of-bounds, I would have thought, and it's certainly not a matter that we're incapable of producing the runs, and it's a matter of the process that I described before of considering those data for a base run was not gone through, and we did not make this transparent to the panel, and we didn't go through individual --

You know, sort of that bridging analysis that Lisa showed, including different data series, and so that is what we think it should go through, is a process like that, and not just a run that Lisa does at her desk and then produces at the next SSC meeting. It really should have gone through the whole process. Thanks.

CHAIRMAN NANCE: Thank you. Clay, please.

DR. CLAY PORCH: Great. Thank you, Chair. I just wanted to reiterate some of the points that Katie has made. While the calibration for recent data has been reviewed, that's really just only a few recent years. The calibration for the whole time series has not been reviewed, and there's a lot of subtleties in there that caused us to calibrate FES back in time, and remember that

it's an attenuated calibration, because it takes into account things like increasing cellphone usage and decreasing landline usage and all those sorts of stuff.

That hasn't been really discussed or analyzed or reviewed, as far as the Florida calibration back in time, and that's why it was only regarded as a sensitivity run.

For that to be used in a stock assessment, it would need some more scrutiny and a higher level of review, at least as part of a topical working group, but that never happened, and so I would say, at this point, the way the motion is written, it's a little bit of a process foul and something that we've been trying to avoid.

You conduct an assessment, and you come to an endpoint, and then, after the fact, people want you to look at a bunch of other things, and it ends up really messing with the schedules and reducing throughput and all these things that we're trying to improve, and so what I would suggest is that something like this should be taken up as part of the SEDAR Steering Committee.

It could be part of a subsequent operational or research track assessment, and then we could take a hard look at those numbers, but, as it stands now, I don't think it would be appropriate just to ask the Center to do another set of calculations looking at thorough diagnostics, with the point of view that it could be used for management advice. We really need to have that time series that is calibrated back in time in the SRFS series fully reviewed.

CHAIRMAN NANCE: Thank you, Clay. Any further discussion? Luiz.

DR. BARBIERI: Thank you, Mr. Chairman. **Clay and Katie, I understand the concerns, and so, in this case, I would say, based on what you are -- The points that you are making here, I think the best course of action at this point would be for me to withdraw the motion and request that you proceed with producing the outputs, the catch level recommendations, converted to the GRFS currency, and is this a possibility still, for the purposes of monitoring?**

DR. SIEGFRIED: I'm not sure if Clay is still on.

DR. PORCH: There's Katie, and we were both waiting for the other to answer, but I think Katie is right, and I think that's the, and Roy, and that's the most expedient solution, and it will get at exactly what you're asking for.

DR. SIEGFRIED: Also, to that point, Luiz, if we could take a

little bit more about whether we need sort of what Roy put out and any other fine-scale separations of that catch series, or if that's really something that can be handled by the SSC or at the council level, and I just wanted to make sure that we provided what you needed.

DR. BARBIERI: Mr. Chairman, to that point?

CHAIRMAN NANCE: Yes, please.

DR. BARBIERI: Katie, thank you, and I think, like you said, the best course of action here is for us to stay in touch. We're going to be working with you and your staff, and, if there is information there that we can provide to make this happen, that would be great.

CHAIRMAN NANCE: Thank you, and I appreciate that discussion. I think that moves us forward on that topic. Let's go ahead, and, Lisa, do you have the presentation?

DR. AILLOUD: Yes. It's up here. Perfect.

CHAIRMAN NANCE: Thank you. I'm glad you're back. Why don't you go ahead and take us through this, and then we can make some decisions as a body.

DR. AILLOUD: Okay. Great. I did a couple of slides, because, after the discussions, I realized that we should probably look at the diagnostics for the SSB combined run. Just as background, if you look at the point estimates of all the parameters in the model between the SSB female-only and SSB combined, they're all extremely close. There is a bit of a worse fit to the age data, but, overall, everything is really, really close.

I did run some diagnostics, and so I did add on the retrospectives on there, which are very, very similar to the SSB female, as you can see. I will try to go a little quickly, but I am happy to take questions. This is the exploitation rates, and, again, there is no pattern here.

The likelihood is a little bit different, but you're not probably used to seeing it so zoomed-in, but, if you look on the bottom here, the slight difference with the SSB female is that there's a bit more of a disagreement with the age data, which is the light-green, which is seeking a slightly-higher R0, but, overall, the likelihood is well defined, and the MLE is at 9.57 here. Because it was last minute, my jitters are still running, but I was able to glance at them, and they pretty much exactly like the SSB female, and so is something that I can certainly provide right

after this meeting, for the record.

For the projections, just as a reminder, these were the agreements. Relative F from the last three years, excluding the red tide, and selectivity comes from the last year, because that's where there is a time block change with the commercial minimum size limit coming into play. The recruitment for the projection is pulled from the Beverton-Holt stock-recruitment relationship.

The interim landings have been provided up to 2020, and then, for 2021 and 2022, we're using the three-year average of landings from 2018 to 2020, and then the allocation ratio is unchanged here, and it's 39 to 61.

I ran two different benchmarks, the F SPR 30 percent and the F max, and on top of it, I have considered three different red tide scenarios, and so I just wanted to outline what we did, and so Dave provided an overall estimate of the red tide mortality over all the ages, and so that gives us an M.

Because we are in the middle of 2021, he proposed several assumptions on the length of the bloom, and so, in the numbers you're going to see, the assumption is that the bloom will end in mid-November, which was selected because it's typically when the red tide dissipates, and, if you use that assumption, those are the three different natural mortalities from red tide numbers, for the low red tide, which would be the lowest end of the 95 percent confidence interval, the medium, at 0.32, which is the point estimate, and so it's a medium red tide, and then high would be the upper bound of that 95 percent confidence interval on that point estimate from the ecosystem model.

In order to translate this into SS, what it results in, in terms of the magnitude of this red tide, is that a low-red-tide scenario corresponds to a red tide of about 10 percent the strength of 2005. That medium scenario is about 30 percent of the strength of the 2005 red tide, and then the high scenario is about 72 percent of the strength.

Here are each of the scenarios, and you have your three F SPR 30 on the left-hand side, and your three Fmax on the right-hand side, with each column showing the different red tide scenarios, and so low, high, or medium. What I want to point out is, if we look at the status, F current over MFMT, in all scenarios, we are in a situation where the stock is undergoing overfishing. Then, if we look at our SSB current over MSST, in all cases, it is below one, and we are -- The stock is overfished.

The difference, the main difference, between these runs is going to be if you look at the line that says, "Year rebuilt at F equals zero", and it's going to change with plus or minus one year, depending on the strength of that assumption on that red tide, and so that's really where the red tide comes into play here.

Finally, and you will see it on the next slide, but just, if you look at the percent SPR equivalent of that FMSY proxy, and we discussed this a little bit on Tuesday, I believe it was, but your Fmax corresponds at about 16 percent SPR, and so we're going to see quite a different history of overfished and overfishing status for these two scenarios.

Here is -- If some of you were involved in the benchmark assessment, and I think that was in 2012, it's what they observed. If we use an F SPR 30 percent FMSY proxy, and we combine it with a scenario where the spawning biomass is defined as SSB combined, what you end up seeing is that the stock has been overfished since about the mid-1970s, under each one of these red tide scenarios, and then that overfishing has been occurring also since 1970, and so each -- The dotted line in gray in the background gives you a ten-year line marking.

In comparison, and we looked at Fmax the other day, and this is the picture that you've been observing, which is very different, which is that the stock is considered overfished starting in around 2005, with overfishing occurring most years since 2000, with some years close to FMSY, or, in this case, Fmax. I think, before I move on, and I have all the F rebuild and the yield streams, but perhaps now is a good time to -- I can take a break and see if there is any questions.

CHAIRMAN NANCE: Jessica, can you put back the table? I think that's the more -- So we can look at it while we're discussing. Let's go ahead, and we have that table up. Are there discussions from the SSC on appropriate next steps? Ryan.

MR. RINDONE: Looking at the intensity of the 2021 red tide, obviously, is going to have some implications that you guys are going to have to weigh, and I had talked with Luiz a little bit about this earlier, just thinking about the general intensity of this red tide in the central West Florida Shelf compared to the one following Hurricane Irma.

It seems as if this one isn't quite as severe, but we are seeing -- We have heard of some effects, especially like off of Pasco County and a little bit north, about fish kills offshore, and so I don't know if we -- What sort of information is available to

help better frame that estimate of intensity?

CHAIRMAN NANCE: Luiz.

DR. BARBIERI: Oh my gosh. I don't know if Dave Chagaris is still on, because I know that Dave has been talking to the red tide folks, and maybe Mandy as well has been in touch with them. I mean, I get the same impression that this year's red tide events have not been as intense, not as large, in terms of spatial coverage, and, sure, some areas of inside Tampa Bay became very visible, and people responded, immediately responded, but that doesn't necessarily mean the areas offshore were really as severely impacted.

CHAIRMAN NANCE: It seems like, from what I saw in some of the presentations, it was that it was nearer shore, and the offshore wasn't as affected. Mandy, please.

DR. KARNAUSKAS: We've been actively monitoring the red tide, in partnership with the state, and also with our surveys, and we've been trying to do a much better job of using our regular fisheries surveys and taking a look at the environmental data that comes off of those surveys in real time, and also working with the fishermen.

They have been monitoring offshore areas as well, and, Lisa, I don't know if you have any of the plots of the most recent conditions out there, and I think we had some real-time data from last week, but my sense, from the monitoring, is that it's not as bad as the 2018 event, and I was actually speaking with some fishermen, over the lunchtime, who had some observations of fish kills offshore, in the Big Bend region. They noted a few grouper, but I haven't heard a lot about grouper offshore and massive fish kills.

The other sort of working hypothesis we have at the Center is that a lot of these really severe events are associated with hypoxia, and they seem to be, in that analysis that Brendan has been doing, and, so far, we don't see any offshore hypoxia in this event. We saw, about a month ago, a small area of hypoxia developing off of Boca Grande, but the fishery surveys, FWRI surveys, that went over that area, and the bottom longline survey, showed that it didn't extend offshore, and so that's my anecdotal and best scientific guess as to where things currently stand.

CHAIRMAN NANCE: Thank you. From my perspective, I would think, for the projections, we would want to use low. Trevor.

MR. MONCRIEF: I will just comment, real quick, and I don't know

about the other folks that are on the phone or at the table, but that is not something that -- I know we had a presentation on it, but I'm not directly familiar with the ongoings of red tide here and everything else, and I would really, honestly, probably rely a lot on what Mandy and Dave would say on that, as far as the ranking goes for projections.

CHAIRMAN NANCE: Okay. I don't see any other hands. Roy.

DR. CRABTREE: So we discussed F 30 percent versus Fmax, and I am not entirely sure where we left that, but it seems to me that somebody raised an issue, and I think it was Shannon, that, if Fmax was higher than F 30 percent, that would be a problem, and that appears to be the case.

CHAIRMAN NANCE: I think it was Will.

DR. CRABTREE: That appears to be the case in the table.

CHAIRMAN NANCE: Where is it at? Let's see. What are we looking at, Roy?

DR. CRABTREE: MFMT, FMSY proxy.

CHAIRMAN NANCE: It looks like they're identical. Let's see.

DR. CRABTREE: Well, I'm looking at MFMT, and it's 0.285 under Fmax and 0.148. I don't know if I'm reading that right.

CHAIRMAN NANCE: Okay. Let's see.

DR. AILLOUD: Yes, that's correct.

DR. CRABTREE: I guess, Lisa, or maybe Shannon, if she's on, is that --

DR. AILLOUD: I think there was a little bit of confusion around those numbers, but, essentially, the F SPR 30, in this case, is giving a more let's say conservative F than Fmax, and I think what Shannon was referring is that, when we were in the case of SSB female, we have seen the opposite happen, and so we've seen the case where sometimes your Fmax is actually more conservative than F SPR 30, which is counterintuitive, and I think it happens because we're dealing with a hermaphroditic species, and we're looking at SSB female only.

In this case, we're looking at SSB combined, and what we're seeing is what we would expect. I do have a follow-up point on that,

because I know that -- I believe that Mandy brought it up the other day, and Katie, but the guidance on SPR 30 is really -- It was defined for SSB female, and specifically for non-hermaphrodites, and there is some guidance that, if one is to use SSB female, then one should use something higher than 30 percent if we're using an F SPR.

However, there is no guidance on what to use when we're using SSB combined, and so I think this is why we're seeing such a drastic picture, and somebody also, I think, brought up that we don't really know what the F SPR should be for a hermaphrodite, when using SSB combined, and so not to add more confusion, but I just wanted to kind of bring all those points together.

DR. CRABTREE: Well, I guess what's on the books right now is Fmax is the reference point.

CHAIRMAN NANCE: Yes.

DR. AILLOUD: Correct.

DR. CRABTREE: My question is whether we should change that, and I don't know if Clay or Shannon or somebody is still on, but I'm wondering if the Center has any guidance on that, or maybe Dr. Barbieri does.

DR. BARBIERI: Well, I don't know if I have any guidance on that, but this is something that we have considered in the past, right, in terms of -- Every time that we have a stock assessment and we are looking at reference points, and what is appropriate or whether to use an MSY estimate versus a proxy, this is something that we make the decision here based on the science and then provide recommendations to the council.

DR. CRABTREE: Right, and I'm fine with a proxy here, and I think it's probably appropriate, but we almost never use Fmax. I can't think of another species that we use Fmax on.

CHAIRMAN NANCE: Okay. Shannon or Katie, and both of you are on, and so whoever would like to, or both.

DR. CALAY: Thank you very much. Yes, we consider Fmax to be an upper limit for a proxy, the choice of proxy selection, but the Center supports the use of F SPR 30 in this case.

CHAIRMAN NANCE: Okay. Thank you. Dave.

DR. CHAGARIS: I just had a comment about the red tide discussion,

but we can come back to it, if you want to.

CHAIRMAN NANCE: Go ahead and say it now, Dave.

DR. CHAGARIS: Okay. Just a little bit of context around the estimates that Lisa showed on Slide 7. What I did, when I provided those estimates -- So the estimate of red tide mortality, through the period of available data, which was about mid-August, was 0.083, which is about half of 2018, and, when I provided those values to her, I made some assumptions about, well, what it might be, and so I basically just prorated it forward another month-and-a-half, through mid-November.

These would be -- Those estimates of 0.09 and 0.32 and 0.72, the low, medium, and high, would be on the high end of the mortality prorated through basically the red tide season, but I just wanted to provide that context.

CHAIRMAN NANCE: Thank you. Doug.

MR. GREGORY: It seems to me that we should do what we did for red grouper, if the sexes were combined for red grouper, and I don't recall. I was just looking back at it, and it looks like -- What I just found is -- Is it possible that red grouper spawning stock biomass was based on number of eggs? That sounds strange to me, but that's what I am reading. It seems to me, since they're both grouper, that they should be treated similarly, for consistency in logic.

CHAIRMAN NANCE: What did you say we use in there, Doug?

MR. GREGORY: Well, I'm not sure. On page 13 of the assessment report for SEDAR 61, it says the fecundity at-age vector was fixed within the assessment model and spawning stock biomass was defined as the number of eggs in the assessment model.

CHAIRMAN NANCE: So was that female then?

MR. GREGORY: It would have to be.

DR. AILLOUD: Mr. Chair, I can clarify. He's correct that they had an estimate of batch fecundity, which we don't have for gag grouper, which is why we have to do a direct equivalency between SSB and fecundity, but it did use female only, and it used SPR 30 percent.

CHAIRMAN NANCE: Okay. Thank you. In this one, we're using combined.

DR. AILLOUD: Here, we used combined. Correct.

CHAIRMAN NANCE: Okay. Thank you.

MR. GREGORY: I mean, it would be nice if we could get to a point -- The old-timers will remember me, twenty years ago, complaining that the two species were handled so differently in the assessments that you really couldn't tell if the results were the result of a different assessment model or the result of the different behavior of the species. Now, we're using the same assessment models, but, again, we need to try to be as consistent as possible with how we treat the two groupers, or all the groupers. Thank you.

CHAIRMAN NANCE: Thank you, Doug. Lisa, go ahead and move on. I don't think there's any more questions.

DR. AILLOUD: Okay. I am going to try to walk through -- There is a lot of numbers, but, essentially, those were the guidelines that Ryan provided, and so, for each of those scenarios, I ran the projections to obtain the benchmarks, and then I ran an F zero, and so no fishing, and found the minimum number of years required to rebuild, and then, as we see down here, there is three different options.

If it rebuilds within ten years of the first year of management, which is 2023, then F rebuild is set to either that amount of time, ten years, or halfway in between, and then, if it requires the stock more than ten years, then there are three other scenarios, which is to give the stock the minimum number of years required plus one generation time, which is eight years, in this case, or to double that minimum amount of time required or to fix F at 75 percent of FMSY.

This is where I have all these tables laid out for you, and it's a lot of slides, and I'm not sure how you would like to approach it, but it gives you every single combination possible for these rebuilding scenarios.

CHAIRMAN NANCE: Let's go ahead, I think, slowly through, and just -- This is -- It looks like F SPR 30 low red tide.

DR. AILLOUD: Correct, and so, for this one, and so ten years beyond management is 2033. In this case, it rebuilds by the ten-year mark.

CHAIRMAN NANCE: Okay. Let's slowly go through them and look, and then some of them probably don't rebuild, and is that correct?

DR. AILLOUD: They all -- Well, some of them don't rebuild within ten years. Correct.

CHAIRMAN NANCE: Okay.

MR. GREGORY: Mr. Chair?

CHAIRMAN NANCE: Yes, please, Doug.

MR. GREGORY: Before we look at the results, it seems to me that it would be easier if we choose a low-tide scenario, and that reduces what we have to look at by two-thirds.

CHAIRMAN NANCE: Perfect. I think that's a good suggestion. I don't want to be the pusher, but, Trevor.

MR. MONCRIEF: I think Dave hinted at it, and correct me if I'm wrong, but I think he indicated that it was a low scenario.

CHAIRMAN NANCE: I think that's how we're all leaning. Is there any opposition to choosing the low? Okay. So, Lisa, just show us the low ones, and then you can skip quickly through the mediums and highs.

DR. AILLOUD: Okay. This one, because it takes exactly ten years, that is the extent of the calculation. Ryan can correct me if I did something wrong, but there is no really midway point, and so I think that would be the only projection.

MR. RINDONE: In this case, Tmin is equivalent to ten years at F equals zero.

DR. AILLOUD: Correct. The next scenario would be the Fmax, which is if you go to Slide 20, I believe. In this case, it's Fmax, and so it's also going to rebuild even faster with an F of zero, and so in 2029. If we give the stock the full ten years to rebuild, it would bring the F to about 65 percent of FMSY for the years, and it would be no longer in an overfished state in 2029, and it would be rebuilt in 2034.

If we pick a year between 2028 and 2033, it would be rebuilt by 2032, and, in this case, it would be no longer overfished by 2028, and it would be an F of 46 percent of FMSY. I think that's it, and so, in the first scenario, Tmin would be essentially the F equals zero, which was the first of those slides. That is it for the low red tide.

CHAIRMAN NANCE: Okay. Let's put up that first -- Right there. Let's look at that one, and so this is F --

DR. AILLOUD: This is F SPR 30.

CHAIRMAN NANCE: SPR 30 with low red tide, F equals zero. Luiz has a question.

DR. BARBIERI: Thank you, Mr. Chairman, and thank you, Lisa, for walking us through this, but, since this came in just today, we haven't really had a chance to look through it, and so do you have -- Can you put up there the scenario for F SPR 30, low red tide, but projections at F rebuild? That is zero?

DR. AILLOUD: Yes, because it's the tenth year.

DR. BARBIERI: I see.

CHAIRMAN NANCE: Let's go ahead and just put that one up, just so we can see it, and so it would be -- Which one would that one be, Lisa?

DR. AILLOUD: I misheard, and is it F SPR, Luiz, that you were asking for?

CHAIRMAN NANCE: SPR 30, F rebuild.

DR. AILLOUD: That would be this, because it hits exactly the ten-year mark, and so there is no alternative.

DR. BARBIERI: Right, and that's what Ryan was saying, that, in this case, considering the condition of the stock, that there is no way to have any yield before the stock is rebuilt. The only way to rebuild in ten years is to actually have F equals zero.

CHAIRMAN NANCE: Roy.

DR. CRABTREE: Realistically, F equals zero is not attainable. I mean, there's going to be some discards, and some dead discards.

CHAIRMAN NANCE: Harry.

MR. BLANCHET: Roy made my point.

CHAIRMAN NANCE: Okay. Thank you, Harry. I wish you were here, so I could see you. Paul.

DR. MICKLE: This is just a question. When the SSC sees something

of this caliber, what type of motion -- What options do we have to inform the council on our scientific opinion on the assessment, the path forward, things like that? What are our options today?

CHAIRMAN NANCE: We have already talked about the assessment and given it our -- Then we would have to make a motion on stock status, and we haven't done that yet. We would have to make a status of the stock determination, overfishing and overfished, and we would have to say that, and then we would have to then come up with the scenario for rebuilding. I think what we would recommend is we would recommend, I guess, F zero. Paul.

DR. MICKLE: But F zero, I don't think, is going to get us there either, and that's my question.

CHAIRMAN NANCE: Well, it looks like, in ten years, it does. Luiz.

DR. BARBIERI: Just a question or clarification here, to Roy's point. I think this F equals zero actually means landings, right, and not -- This is what I am asking, whether discards are already accounted for.

DR. CRABTREE: I will have to -- The Center will have to answer that, but I'm assuming that --

MR. RINDONE: Discards are supposed to be accounted for, because we can't manage the number of fish that are thrown back, and so this is harvest only.

CHAIRMAN NANCE: Okay. Lisa, go ahead.

DR. AILLOUD: Yes, the projections take into account total catches, and catches is landings plus discards.

DR. CRABTREE: So this is based on no discards?

DR. AILLOUD: Correct.

DR. CRABTREE: Yes, and that's how I understood it.

CHAIRMAN NANCE: Okay. Thank you.

DR. CRABTREE: But where it pins us is I don't know how you avoid an ABC of zero here, but, I mean, the council is going to have to grapple with this, because I believe that Fmax is in the FMP as the reference point, and so they will have to change that.

If you went with the Fmax scenario, it appeared to me that you

might still be at an ABC of zero even there, because I think, Lisa, with Fmax, it still took almost the ten years to get to recovered, didn't it, to rebuild?

DR. AILLOUD: Yes, and so, at Fmax, it takes eight years, I believe, and so there is a little bit of flexibility, obviously, to put a low F and rebuild in ten years, instead of in the minimum timeframe.

CHAIRMAN NANCE: Can you bring up those?

DR. AILLOUD: That would be Slide 20.

CHAIRMAN NANCE: Slide 20, please.

DR. AILLOUD: Then 21 and 22 give the two options.

CHAIRMAN NANCE: Okay. Roy.

DR. CRABTREE: Lisa, can you tell me, easily, what are the Fs under the current discarding? Can you tell me how much F is occurring as a result of discards, fishery-wide, commercial and recreational and everybody?

DR. AILLOUD: I can dig it out, but it will take me a few minutes.

DR. CRABTREE: Okay.

CHAIRMAN NANCE: So which table? This is 20, and let's see 21. There is Fmax, Roy. It looks like, with Fmax, low red tide, we have an F of 0.185 and those types of things. Ryan.

MR. RINDONE: The projected catch is not supposed to include discards, because we can't manage the fish that people are throwing back, and so the projected catch is only supposed to be retained catch, because that's the only thing that we can count.

SSC MEMBER: (The comment is not audible on the recording.)

MR. RINDONE: Yes, I understand that, but we still can't manage for the fish that are being thrown back, because, even under F zero -- Under F whatever, people are catching and throwing fish back, for a variety of reasons. I am saying, as far as the catch recommendations that you guys would put forward, it needs to be considerate of that it's just retained catch, because that's the only thing that the surveys can count.

CHAIRMAN NANCE: Doug, please. Doug, let me have Shannon first

and then you, please.

DR. CALAY: Thank you. I did raise my hand, and, therefore, it's logical to call on me.

CHAIRMAN NANCE: Well, I know that, when you raise your hand, you want to say something about what we're discussing, and so that's why I always get you.

DR. CALAY: We have struggled with this concept, both in the Gulf and the South Atlantic, for quite some time, and the national guidance, as we have been, as we have confirmed, is that these projections for rebuild -- That the first step is to assume that the removals are in fact zero, no discards and no removals. If the stock can rebuild in that ten-year timeframe, that we're constrained to attempt to rebuild the stock in ten years.

We have asked that very question, about whether there is latitude to recommend a retained -- To recommend a catch that's discards, that represents the discards, and what we've been told is that we do not have that latitude, and I know that Clay is back, and, if Clay would like to weigh-in, he may, but I just want to point out that, in our understanding, when you can rebuild the stock within ten years, with a total removals equal to zero, that is our obligation to do so.

CHAIRMAN NANCE: Okay. Thank you. Doug.

MR. GREGORY: I think everything has been answered. It seems like we're stuck with the rebuild in ten years with Fmax and the low red tide. I mean, that gives us some catches, and I would certainly be reluctant to recommend the scenario of zero catches for ten years.

CHAIRMAN NANCE: Yes, and that one seems almost, because with discards and everything else, unattainable. Roy.

MR. GREGORY: Not only is it unattainable, but it's poor management. We'll have no data, and no CPUE from the fishery, and we won't know when to reopen it, and we've had that problem before with red drum and goliath grouper.

CHAIRMAN NANCE: Fmax with the low red tide, and we are rebuilt within ten years.

MR. RINDONE: I guess that would be a concern, Mr. Chair, is that, if you eliminate commercial and recreational landings, you eliminate the opportunity to collect age and length composition

data, and you eliminate the opportunity to collect really any measurable data, besides discards, perhaps, from like the reef fish observer programs and the headboat program.

Then, I guess, eventually, when SEFHIER gets underway, you might be able to collect some discard information there, but that would really be all that you would be collecting, and, as you guys have stated, and as we've seen with other stocks, the absence of those data does make things difficult.

CHAIRMAN NANCE: It does. Any other discussion on this topic?

MR. GREGORY: If I may.

CHAIRMAN NANCE: Yes, please.

MR. GREGORY: Maybe not us, but the council, or maybe us, should discuss that, well, if we factor in the discard mortality, we know it's going to take more than ten years, and so let's jump to the next level, which is Tmin plus a generation time or something else, but something not extraordinary.

I am not thinking of expanding the rebuilding time to some outrageous length of time, so that we don't do anything in the short-term, but it might give us some more leeway, and I don't know if we could argue about discard mortality really causing it to last more than ten years in a rebuilding schedule. I heard what Shannon said. Thank you.

CHAIRMAN NANCE: Is there a scenario with a little more than ten years on here?

MR. GREGORY: I think Lisa dropped it, because --

MR. RINDONE: I think you guys have made the decision, at this point, that you think that the low-red-tide scenario is most appropriate.

CHAIRMAN NANCE: Absolutely.

MR. RINDONE: Based on the observations that we've discussed in the room, and Mandy's feedback, and Dave's feedback, it seems that there is definitely support for the low-red-tide situation. Insofar as a rebuilding period that goes beyond ten years, I think that you guys -- At least given the justification that you have established, you probably need to keep that decision in mind, about the red tide scenario.

CHAIRMAN NANCE: Right. Clay.

DR. PORCH: Thank you, Chair. I just wanted to address Doug's question, and we've actually had a number of conversations with General Counsel at the National Marine Fisheries Service, and, unfortunately, F equals zero means F equals zero, and so we cannot compute rebuilding times that allow some level of discard mortality. It really is the minimum time to rebuild, which means if there was no mortality whatsoever.

CHAIRMAN NANCE: Clay, let me ask you a quick question. On Fmax, low red tide, it does give us some F and still allows us to rebuild in the ten-year period. Is that correct?

DR. PORCH: I would have to defer that to Lisa, because I stepped out, and I didn't see what the Fmax was, but the essential question is whether Fmax is an appropriate proxy for MSY, and it is the upper limit of what the FMSY would be, and so that means that FMSY should be something less than that.

CHAIRMAN NANCE: Okay. Thank you.

MR. RINDONE: Mr. Chair, under the Fmax scenario, with F rebuilding in ten years, like you see here, you do get some yields, and so you would still have the continual, albeit not quite as much data, but you would still be collecting age and length composition data, et cetera, from the directed fleets.

CHAIRMAN NANCE: Roy.

DR. CRABTREE: I mean, but you're going to have to look at what are the expected Fs that the discards are going to cause, and then how much can you retain, and I suspect it's not much.

CHAIRMAN NANCE: Harry, please.

MR. BLANCHET: For clarity, what is the rule right now, in terms of what our goal is? Is it Fmax or F 30 percent?

DR. BARBIERI: Can you say that again, Harry?

MR. BLANCHET: What is the current benchmark for the stock? Is it F 30 percent or Fmax?

CHAIRMAN NANCE: It's Fmax.

MR. BLANCHET: Okay, and so it would require a plan amendment to go to F 30 percent?

MR. RINDONE: That is correct.

MR. BLANCHET: So we would have to make a recommendation to change the benchmark in order to go to 30 percent, and I don't know if that's -- I don't know if we have really developed a rationale for that at this point. I can see where people would have an issue with the current estimate for Fmax.

When we were discussing this earlier in the week, the discussions around Fmax was that it was equivalent to something much higher than what we're seeing in the current assessment, and I asked the question that, if Fmax came out below F SPR 30, what would be the response, and so I think we're at that point now.

CHAIRMAN NANCE: Just a minute. We're looking at tables. Katie.

DR. SIEGFRIED: Thank you, Mr. Chair. Perhaps it's because I am doing other things at the same time that I am listening that I may not have caught all of the conversation about why we're using low, and I think, especially now, because it has such a huge impact on setting up this rebuilding plan, and I just wanted to make sure that the SSC had all the justification that they needed to go with low or if we needed to perhaps hear from Dave again, and I know that Mandy had to jump off, but it's a very influential choice, and we just sort of picked a low, medium, high, and I just want to make sure that that's covered in the report and that there's enough here for you all to justify that.

CHAIRMAN NANCE: Luiz.

DR. BARBIERI: To that point, I think that's a good point, Katie. I think that conversations, before we actually saw the tables and started discussing what yields are going to come out of each one of these scenarios, and I think the discussion based on the levels of red tide that we've seen this year, and the decision was to go with low, of course, as you know.

All of this, for red tide, is like trying to predict the weather, and so we don't know what will happen next year, or even later this year, but what we've seen now suggests that red tide this year is not being as impactful, as strong and intense, as it's been over the last several years.

CHAIRMAN NANCE: We had, I think, a very good discussion on red tide, for sure.

DR. SIEGFRIED: Okay. Thank you.

CHAIRMAN NANCE: You're welcome. Will.

DR. PATTERSON: As far as the F proxy stuff goes, it seems to me that that conversation is kind of moot, based on the points that Harry was just making. I mean, if what's on the books is Fmax, do we really have any flexibility not to estimate OFL based on Fmax and then set ABC accordingly?

As far as the red tide, we're starting to see some fish up this way, in Levy County, that I am not sure that low red tide is the best approach here, and I don't -- Luiz has more access, obviously, to the folks that are out collecting water samples and reports of fish kills, and so I will defer to him on what he thinks as far as low versus medium, but it seems to me that it would precautionary to pick medium, as far as this year. We don't really know how long this is going to last, and I'm not sure we have a full understanding of what the spatial coverage is.

CHAIRMAN NANCE: Thank you, Will. Doug.

MR. GREGORY: With Fmax, it doesn't matter, as far as the amount of time it takes to rebuild, which level of red tide you have. They all kind of do it in ten years. Where it makes a difference is if the proxy is 30 percent SPR, and then you're expanding some of your recovery times out to 2035, or 2033, and so, with Fmax, it doesn't matter. Take a look at the tables. They all seem to recover within about ten years, unless I am reading it wrong.

DR. CRABTREE: We ought to look at that, because --

CHAIRMAN NANCE: Let's go ahead and look at I think it's probably the next one. That's Fmax high red tide.

DR. AILLOUD: That still is within ten years. Where it's changing is in the SPR 30.

CHAIRMAN NANCE: Right.

MR. RINDONE: It's Slide 8.

DR. AILLOUD: It would be --

MR. RINDONE: You're looking at the third row from the bottom, and it's year rebuilt at F equals zero, which would be your Tmin, minimum time to rebuild, and so, at an SPR proxy -- At an FMSY proxy of F SPR 30 percent, your minimum time to rebuild would be eleven years, or 2034, under the medium red tide severity scenario.

CHAIRMAN NANCE: Roy.

DR. CRABTREE: Then, if that's the scenario, then you're looking at rebuilding in ten years plus one generation, I believe, and a generation was, what, eight years?

DR. AILLOUD: Correct.

CHAIRMAN NANCE: Okay. Doug.

MR. GREGORY: My hand was down. Thank you though.

CHAIRMAN NANCE: Dave.

DR. CHAGARIS: Thank you. Just circling back to the red tide discussion, and to maybe clarify a comment that I made earlier, the three values, the low, medium, and high, that are on the board, those represent the uncertainty associated with the response functions in the model.

Most of the runs that had the best fits to the data, most of them fall within the medium response functions, and so not too sensitive and not too robust to red tides, and so, if we look at that medium value, which is 32 percent of 2005 red tide, the actual estimate, if we prorated it through November, was 0.166, and that's the 32 percent. If we only prorated it through September, then it's around 20 percent of the 2005 event, and, if we assumed that the red tide stopped in August and used the medium value, then that would be 15 percent of 2005.

I wouldn't say that the low setting here is not very precautionary at all, especially given what we know about the red tide in the Big Bend region that is present now, and also some reports that it's flaring back up again in southwest Florida, and so I do think that a medium value of red tide would probably be more appropriately precautionary, but not overly precautionary. Now, if you do want to prorate it through November, then something like 20 percent would be more appropriate.

CHAIRMAN NANCE: Okay. Thank you. I guess I misinterpreted what you were saying at first, and so I am appreciative that you have come back on and clarified that, David. Thank you.

DR. CHAGARIS: No problem.

CHAIRMAN NANCE: Katie.

DR. SIEGFRIED: My hand was still up from before, but thank you, Dave, for clarifying.

CHAIRMAN NANCE: Yes. Harry.

MR. BLANCHET: Dave, this is with regard to any of these, and what kind of frequency of red tide are we talking about in these projection years?

DR. CHAGARIS: The projections were done with data, red tide data and satellite imagery, through mid-August, and that estimate came out to 0.08, which is 15 percent of 2005. What I did was prorated a couple of months ahead, just assuming that the same average monthly mortality rate occurred and just added that for a couple of months, and so we only had to basically -- The only assumptions I am making, as to any data, are what happens with red tide from mid-August through the red tide season, which could be as long as November.

MR. BLANCHET: Okay, but I am talking about 2022 to 2028, or whatever we're dealing with here. I mean, what we're seeing is a pretty significant change, depending upon one year's intensity values, but we've got several more to get through before we get to wherever we're going, and so how is that -- Is that an uncertainty, or is that just something that we're not considering at this point, and we're just assuming that we're good? I don't mean to facetious, but I'm just trying to figure out where are we at.

DR. CHAGARIS: Well, we're not making any future predictions about red tide beyond 2021. Now, I'm sure that the projection model, the assessment model, could be configured with red tide in years further out, if that was what people wanted to do.

MR. BLANCHET: It seems, to me, that we have seen red tide in a lot of years. In some years, it has some impact, and, in some years, it has a lot of impact, but this goes back to what is a -- When can we expect this stock to rebuild, and, if it's not only that we're going to do F equals zero, but we're also going to do red tide equals zero, that seems like a different set of expectations.

DR. CHAGARIS: I mean, I agree with you, but we just don't have any information to make that decision on, and I think this kind of feeds into a lot of the management strategy evaluation work that I know that Skyler and Lisa and other folks at the Center are anxious to do with respect to red tides and harvest policies and what are the most robust strategies for that.

CHAIRMAN NANCE: Thank you, Harry. Let's go to Slide 27. I think this the scenario that we're talking about, which is Fmax, medium red tide, rebuild in ten years. Is this the one that it seems like we're kind of coming to consensus on? Roy.

DR. CRABTREE: Not necessarily, because I would be interested in seeing a 30 percent SPR and medium red tide scenario with an F rebuild shown running out, I believe, for an eighteen-year rebuilding period. If the stock can't recover at F zero in ten years, then I think you can take ten years plus one generation, which I think is eighteen years here. I would like to see the Fs that are allowable in that. One of the strange things about the Magnuson Act is, if things are worse, then sometimes you don't have to be as restrictive. That F is not a whole lot different from the F in the Fmax projection, was it?

MR. RINDONE: Under this scenario, you have a slower build back up to that rebuilt status, and so you have more years of lower yields for a while.

CHAIRMAN NANCE: So Fmax, medium red tide, rebuild in ten years, gives us an F of 1.61. This one gives us an F of 1.22 over most years, for a longer timeframe.

DR. BARBIERI: If I may, Mr. Chairman.

CHAIRMAN NANCE: Yes, please, Luiz.

DR. BARBIERI: In this case, I like this, Roy, because -- This one that is right there on the screen, because I would feel more comfortable going with an SPR 30, considering all the issues about gag reproduction that we heard about over the last couple of days and that those issues need to be addressed at some point. Otherwise, we're not going to be successful rebuilding either, and so, to me, this one would be the --

CHAIRMAN NANCE: Okay. Will.

DR. PATTERSON: Among the options, F 30 percent, with medium response, seems to me is the most appropriate here.

CHAIRMAN NANCE: Okay.

DR. PATTERSON: Of what we have in front of us. We do need to have, I think, a serious discussion, if not here, then in the near future, about appropriate F proxies for protogynous hermaphrodites. I know we've discussed that in the past, but clearly we're in a position here where we should talk about that.

However, my confusion is are we at liberty to select anything other than setting OFL based on Fmax, given that's what the proxy is on the books, and I guess that's really a question for Ryan, or maybe Carrie, and I don't know the answer to that, but it seems to me that we're kind of hamstrung here in what we can actually do.

CHAIRMAN NANCE: Okay. Ryan.

MR. RINDONE: Fmax is on the books right now, using female-only biomass, and, essentially, what you guys are recommending is F 30 percent SPR, using sexes combined, and so, in order to facilitate this change, we would need a full plan amendment, which is, obviously, going to need to include a rebuilding plan, catch recommendations, et cetera, et cetera, and the council would have to adopt that new proxy for F at MSY.

DR. PATTERSON: So, because we're switching from female only to combined sex, can we just go ahead and do this then, or is there some other process that we're missing here?

MR. RINDONE: I mean, you guys can make this recommendation, right, and then the council will either agree to adopt it or it won't, and so, if it doesn't, then that puts them back with the Fmax and picking a scenario that is plausible under Fmax.

CHAIRMAN NANCE: Clay, you had a comment, please?

DR. PORCH: Thank you. It was related to that issue, and Ryan, of course, is right about that. It's an unfortunate reality that the way the FMP and amendment was written specified a specific value, and it really should have specified just MSY proxy, based on the best available scientific information, because, as we saw here, that information can change from one assessment to the next, which implies the proxy should change, but that's not the way the FMP is currently written. That's something that needs to happen in the future, and it's not now.

Having said that, if the SSC can establish a good record for why it should be changed, and, I mean, that would be required anyway, and then, as Ryan said, that can be submitted, and the council can consider it.

The only other thing that I wanted to add was I think someone mentioned being conservative on some of the assumptions, and I just wanted to remind the SSC that, at least as far as the OFL is concerned, then you should actually apply that fishing mortality rate, and, so in this case, it's either Fmax or F 30 percent, to

your best estimate of what the future population biomass will be.

In that case, you would look for the most plausible levels of recruitment and red tide, and then you would buffer that down. Now, in this case, you have a rebuilding plan, and so the ABC is based on F rebuild. Arguably, then you would use the same logic and apply F rebuild to your best estimate of future abundance and then buffer that down for scientific uncertainty.

CHAIRMAN NANCE: Thank you, Clay. Roy, please.

DR. CRABTREE: We have a base M that we're using here, but we know we have these periodic red tides, and so, over a period of a decade or so, that base M is underestimating natural mortality, because, assuming there are some more red tides down the road, there is going to be some more natural mortality that takes place.

Other than picking years or stuff, which I am not at all -- Is there a way to take that into account, and do you think there would be -- That it would make much difference in any of this if the base M in the future years was adjusted upwards to account for that? Do you follow what I'm saying?

DR. PORCH: I know exactly what you're saying. In fact, some of us have talked about that internally, and I am going to defer in a second, but I will say, in general, you raise a very valid point, and the way you would get at that is if you could model -- Look at the past and how M has changed because of red tide, that additional source of mortality, and then you would project that out over the long term, and so it does raise M, on average, which could affect your perception of productivity.

This is where I need to defer to Lisa and those who have done the projections. Two questions. One, how much more, on average, because red tide is not every year, does it increase natural mortality, and I don't think it's a huge amount, on average, but I would defer that to them, because I don't remember how much, each year, the red tide additional mortality rate was. Then I don't know if they actually incorporated that in the projections, and so I would defer those questions, in the long-term calculations, I mean.

CHAIRMAN NANCE: We're kind of running out of time here, unfortunately. I think what we need to do is defer this discussion to our November meeting, and I think we've got the tables, and I think we're coming closer and closer to a consensus, but, in order to have public comment and things, which we still need to do, we need to kind of table this until our November meeting.

Let me ask you this, real quick, but is there anything between now and November, from an SPR standpoint or something, that we would like to see? I think we've got the various scenarios, but is there anything else that would help us inform it? Anyway, think about that. Mandy, I am going to let you go ahead and say something, and then we're going to turn it over for public comment.

DR. KARNAUSKAS: Thanks. I will make this very brief, and it's relevant to the last point. There is a Harper 2018 paper where we actually did a management strategy evaluation of how much of a buffer you would want to put on the catch limit to account for unknown red tide mortality, and that was done for red grouper, but it's probably applicable and worth looking at for this discussion.

CHAIRMAN NANCE: Okay. Thank you very much, Mandy. Okay. We have ended that discussion, and we will go ahead and -- I'm sorry that it has taken so long, but we'll go ahead and have our public comment now. If you have public comment, go ahead and send your name in, and then we will recognize you. Michael, you're first. Go ahead.

PUBLIC COMMENT

MR. MICHAEL DREXLER: Thank you, Mr. Chair. Thank you. First, on gag rebuilding, I just wanted to comment, on the discussion of rebuilding, that, obviously, it's concerning for gag, and, in the establishment of the rebuilding plans, I think it's typical to estimate the Tmax, but note that the guidance suggests that you don't always have to pick Tmax as your rebuilding target, and it's actually meant to -- Tmin and Tmax are meant to provide a bounds, and a T target can be chosen that doesn't necessarily correspond with a Tmax, and so I just wanted to make that point on rebuilding, because I don't think it's something that we typically discuss in this region.

Second, zooming out, gag is now potentially at 2 percent of historical levels, noting that the stock was just rebuilt and declared rebuilt in 2015, and amberjack hasn't made rebuilding progress in over twenty years, and landings of red grouper in both sectors are down to almost 50 percent of their allotted quota. Red snapper was declared overfishing, for the first time in decades, two years ago, or three years ago, and triggerfish also doesn't look very promising on a Gulf-wide basis.

I think it's clear to me that this council's aggressive management is catching up to themselves, and I think the council's risk policy is failing in its goal to achieve optimal yield.

The whole point of these projections, rebuilding or otherwise, is to achieve optimum yield, trying to get as much as we can out of the stock, and I support that. However, in a lot of these cases, these protections aren't being realized. A lot of these stocks are going in the wrong direction, and, at this point, I don't think we can afford to chase rebuilding projections that aren't being met, and they certainly aren't helping to achieve optimum yield in the long term at this point in time.

We need to hit the pause button. We need to hold catch at precautionary levels until we see signs of progress, and we need to revisit our risk policies and take a red-before-green approach, if you will.

A large part of the council's risk policy is embedded in the ABC Control Rule, and that is in need of a major overhaul. We don't often use it, and it's for good reasons, but it's come out of favor, and it needs some work, and that concludes my comments. Thank you.

CHAIRMAN NANCE: Thank you. Any comment by the SSC? Michael, thank you for those comments. Kellie Ralston, please.

MS. KELLIE RALSTON: Thank you, Mr. Chair. Kellie Ralston, representing the American Sportfishing Association. I appreciate the time to give some comments to the SSC. First, I wanted to thank the SSC for moving forward with using the Great Red Snapper Count for SEDAR 74. We support its incorporation into the research track assessment, along with the Gulf states data for recreational anglers, and we also support the inclusion of the Florida State Reef Fish Survey in the gag grouper assessment, and I look forward to that conversation at the council level.

There has been lots of conversation at the council about using this particular survey in reef fish assessments that predominantly occur off of the state coast, and it's particularly appropriate, given the State Reef Fish Survey focus on offshore anglers.

I would also like to comment on the proposed EFP that was discussed this morning to distribute fish to recreational anglers. I guess it's still not clear, from the presentation, exactly what the problem is that this EFP would address. Is it to improve recreational data? Is it to address inadequate recreational seasons?

There was lots of conversation amongst the SSC regarding charter operations, but private anglers are incredibly diverse, and so

it's a little bit different situation, and it may not be appropriate to come at this from a charter or commercial perspective, with a limited-access perspective.

Angler motivation for participating in a fishery really depends on the individual and the species that they're targeting. For some, optimal yield for folks is harvest, and for some it's encounters, and, for others, it's just a chance to get out on the water.

We also find that using flexibility in the fishing year as an incentive to improve recreational reporting, which was alluded to in the presentation, may actually not be the best approach here, as there are likely unintended consequences when you attempt to scale up such a program. At some points, the permits, or day passes -- The demand would actually exceed the available quota, and then what happens?

In addition, every fishing day is not equal on the water. Harvest and effort depends on location, weather, water temperature, and even school and holiday schedules, and so there's a lot of variables that would need to be incorporated there.

We would point the Science Center to the Gulf angler focus group report, which was initiated and conducted by the recreational sector and can be found on the council's website from the January 2017 council meeting. This report explores challenges and recommendations from the recreational sector for the recreational sector regarding the red snapper fishery.

Finally, there is a couple of other items that we would like to point to, from a data perspective, especially looking at angler engagement and retention. The Marine Fisheries Advisory Committee has an electronic reporting taskforce that will really look at some of these issues that will be applicable to some of the goals that were stated in the EFP.

Finally, while we applaud the agency's expeditious response to the National Academy LAPP report that resulted from the Modern Fish Act, the agency's desire to find additional opportunities to improve recreational data collection would be better achieved by taking immediate action on the recommendations from the concurrently released National Academy report on MRIP, as well as the previous National Academy MRIP report from 2017. Thank you.

CHAIRMAN NANCE: Thank you very much. Bob.

MR. BOB ZALES, II: Thank you. Bob Zales, II, and I'm representing the National Association of Charter Boat Operators, and also the

Southern Offshore Fishing Association, the commercial longline guys.

A couple of things. Number one, on the Great Red Snapper Count, I understand how we continue to kick this can down the road, just like we have with red snapper management for the last thirty years, and my suggestion would be -- Because, when this report came out, almost a year ago now, it was all over the world how great this thing was and how many more red snapper was out there, and so you got everybody in a tizzy of we're going to get all these extra fish, and that's what they've been expecting now for the past year, and I would suggest that some outreach, public notice, go out or something to explain to people that don't get a big hurry, and you may get some, or you may not get any, and we don't know what's going on, but, right now, they're expecting great things. Hopefully that will happen.

On this EFP that was in there, I got on that this morning for a little bit, and, with all due respect to the guy doing it, I am not 100 percent that he fully understands commercial fishing and charter fishing and private recreational fishing.

Commercial fishing in the Gulf of Mexico is not commercial fishing in Alaska, and IFQs are not IFQs, and recreational fishing is not the same, and charter fishing is not the same. When he mentioned the catch share plan that they have in Alaska, that's only for two areas in Alaska. It's a complicated plan. If you're going to try to design something like that, they need to get in there and research it, and I can provide several names of people who are actively involved in that plan to people to do that.

On top of that, trying to do something like this, depending on where you're going to come up with the extra fish to provide in the offseason, it's going to be an issue, because, if you take them from the commercial guys, they're going to be upset. If you take them from the charter guys, they're going to be upset. If you reduce the recreational season on the private recreational side, they're going to be upset. There's a lot of complications in this plan, and right now, from what I've seen, I'm not sure we could support it.

One last thing is Roy is not the RA anymore, but I want to thank Andy Strelcheck and the Fisheries Service. In case you all don't know, we just got notified that the for-hire red snapper season in the Gulf reopens on the 15th of October, and it will close on the 5th of November, and so we're getting an extra two or three weeks. Thank you all very much.

CHAIRMAN NANCE: Bob, thanks. It's always good to hear from you. Leann.

MS. BOSARGE: Thank you, Mr. Chairman. I actually had a question when you were going through some of the red snapper discussion, and I was trying to be quiet and hold it until the end, which I think probably was a mistake, because I don't think that Greg and company are on the call anymore, but I will ask it anyway, and maybe you all can address it at a future meeting.

Most of what Greg went over was the more statistical issues that the reviewers asked him to address, but there were a couple of, I guess, more specific quantitative things, and one had to do with pipelines and adjusting the total miles of pipeline to reflect the fact that pipelines in less than 200 foot of water in the Gulf are buried, so long as the pipeline is greater than eight-and-five-eighths diameter, which that's essentially the cutoff for what they even examined in the study.

That would have changed the numbers, especially for Texas and Louisiana, just slightly, and the rig count. It differed a little bit. Well, it differed significantly, actually, from what BOEM had on their website for federal waters, and so we were going to get some more explanation on where that rig count came from, and maybe what it should be and how it was stratified by depth zone, as a lot of those rigs are in state waters, and even inshore, and so not in red snapper territory.

I didn't hear Greg address those, and I am hoping, if you happen to address that study again at a future meeting, we'll actually get some resolution on some of those more direct questions that were brought up during that meeting.

Then my final note was I thought -- I think it was Dr. Mickle that made a good point that, more than likely, you're probably not going to go in and actually choose one study over the other, when you start looking at Louisiana.

If I could take the liberty here, when you go into the LGL study again in November or January, or whenever it is, I think it really would be beneficial to have the Texas portion of the Great Red Snapper Count presented again to the SSC at the same time that you go into further detail with the LGL study, because I agree with whoever it was that made those comments, and I don't think it's going to be a one or the other, and I really think the discussion is going to be more how the two inform each other and how we can look at both of them together and get a better picture of the uncertainties in the estimates and how to interpret these. That

would just be my suggestion, moving forward, and I think that might be beneficial, prove beneficial, for the SEDAR process. Thanks.

CHAIRMAN NANCE: Thank you, Leann. I wanted to just say, for those SSC members and the public and others who are on, I appreciate your attendance at this meeting. It was four days of good discussion, and so we appreciate each of you, and we'll go ahead and end now.

DR. BARBIERI: By the way, Mr. Chairman, thank you also for herding the cats this week. I mean, it was a lot of work, and a lot of good discussions, and we sure appreciate you at the helm.

CHAIRMAN NANCE: Thank you, Luiz. We'll go ahead, and Jessica is ready to turn it off.

(Whereupon, the meeting adjourned on September 30, 2021.)

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