GULF OF MEXICO FISHERY MANAGEMENT COUNCIL

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

WEBINAR

MARCH 30-APRIL 2, 2021

STANDING SSC VOTING MEMBERS

Joseph Powers.....
Lee Anderson.....
Luiz Barbieri.....
Harry Blanchet.....
David Chagaris.....
Benny Gallaway.....
Bob Gill.....
Douglas Gregory.....
Walter Keithly.....
Robert Leaf.....
Kai Lorenzen.....
Camp Matens.....
James Nance.....
Will Patterson.....
Sean Powers.....
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John Hoenig.....
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Jay Rooker.....
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Matt Smith.....
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Jessica Stephen.....NMFS
Ted Venker.....
Bob Zales.....Panama City, FL

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TABLE OF MOTIONS

PAGE 303: Motion that the review team (external consultants and SSC) considers that the Great Red Snapper Count provides a representative estimate of abundance for the eastern Gulf and a highly uncertain estimate for the Western Gulf. However, the review team also considers that the true uncertainty in both estimates is substantially larger than implied by the 11 percent CV stated in the report, and that the estimate for uncharacterized bottom is particularly uncertain. The motion carried on page 316.

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The Meeting of the Gulf of Mexico Fishery Management Council Standing and Special Reef Fish and Socioeconomic Scientific and Statistical Committees convened via webinar on Tuesday morning, March 30, 2021, and was called to order by Chairman Joe Powers.

INTRODUCTIONS AND ADOPTION OF AGENDA

CHAIRMAN JOE POWERS: Good morning. My name is Joe Powers, and I welcome all of you as 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 Tom Frazer, and council staff in attendance are Ryan Rindone and Jessica Matos.

Notice of this meeting was provided to the Federal Register, sent via email to subscribers of the council's press release email list, and was posted on the council's website.

Today's meeting will include the following topics: Adoption of the Agenda, Review of the Great Red Snapper Count, Approval of the January 5 through 7 Minutes, Scope of Work, Selection of SSC Representative for the Next Council Meeting, Review of Great-Red-Snapper-Count-Informed Catch Analysis, Review of Red Snapper Interim Analysis, Public Comment, and Other Business.

The meeting, 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 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 the raise-your-hand function, and staff will display your name. Please remember to identify yourself before speaking and to also re-mute your line each time you finish speaking. Thank you. We will begin with the attendance.

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: Dave Chagaris.

DR. DAVID CHAGARIS: David Chagaris, here.

MS. MATOS: Benny Gallaway.

DR. BENNY GALLAWAY: Benny Gallaway, here.

MS. MATOS: Bob Gill.

MR. BOB GILL: Bob Gill.

MS. MATOS: Doug Gregory.

MR. DOUGLAS GREGORY: Douglas Gregory.

MS. MATOS: Walter Keithly.

DR. WALTER KEITHLY: Walter Keithly.

MS. MATOS: Robert Leaf.

DR. ROBERT LEAF: Robert here.

MS. MATOS: Kai Lorenzen.

DR. KAI LORENZEN: Kai Lorenzen.

MS. MATOS: Camp Matens.

MR. CAMP MATENS: Camp Matens, here.

MS. MATOS: Jim Nance.

DR. JIM NANCE: Jim Nance, here.

MS. MATOS: Will Patterson.

DR. WILL PATTERSON: Will Patterson.

MS. MATOS: Joe Powers.

CHAIRMAN POWERS: Joe Powers.

MS. MATOS: Sean Powers.

DR. SEAN POWERS: Sean Powers is here.

MS. MATOS: Ken Roberts.

DR. KEN ROBERTS: Ken Roberts is here.

MS. MATOS: Steven Scyphers.

DR. STEVEN SCYPHERS: Steven Scyphers is here.

MS. MATOS: Jim Tolan.

DR. JIM TOLAN: Jim Tolan.

MS. MATOS: Jason Adriance.

MR. JASON ADRIANCE: Jason Adriance.

MS. MATOS: Judd Curtis.

DR. JUDSON CURTIS: Judd Curtis.

MS. MATOS: John Mareska.

MR. JOHN MARESKA: John Mareska.

MS. MATOS: Kari Buck.

DR. KARI MACLAUCHLIN-BUCK: Kari Buck.

MS. MATOS: Jack Isaacs.

DR. JACK ISAACS: Jack Isaacs is here.

MS. MATOS: Andrew Ropicki.

DR. ANDREW ROPICKI: Andrew Ropicki is here.

MS. MATOS: We have Tom Frazer, as our council liaison.

DR. TOM FRAZER: Tom Frazer.

MS. MATOS: Thank you. Also, I will call out for our three consultants that are on the line. We have Steve Cadrin.

DR. STEVE CADRIN: Steve Cadrin.

MS. MATOS: Mary Christman.

DR. MARY CHRISTMAN: Mary Christman.

MS. MATOS: Dave Eggleston.

DR. DAVE EGGLESTON: Dave Eggleston is here.

MS. MATOS: Great. Thank you.

CHAIRMAN POWERS: All right. Thank you. The agenda that I just read out, essentially, and listed there, is there any objections to this agenda?

MR. RYAN RINDONE: Dr. Powers, one modification for timing, to accommodate Dr. Ahrens early wakeup time in Hawaii, would be to move Dr. Murawski's presentation up underneath Greg's summary presentation.

CHAIRMAN POWERS: Okay. That's fine with me. Also, when we get to that point, be sure and remind me about that. All right. Thank you. Are there any other -- Not objections, but are there any other adoptions or adaptations to this agenda? If not, then since what we're talking about doing here, in terms of the agenda, the first two days are, essentially, the review of the Great Red Snapper Count.

The latter part is the council, or the SSC, being asked to address certain issues, and, in particular, what it is we're going to use the Red Snapper Count for, to adjust the catch advice for the 2021 year, and perhaps later, and so, essentially, the -- As I said, the first two days will be focused on the Great Red Snapper Count.

The agenda for those first two days was given on the council website, and we also have terms of reference for that, but, before we get to the terms of reference, let me kind of remind people how we got where we are.

At our last meeting, January 6, we were presented -- We, the SSC, were presented the results of the Great Red Snapper Count, in terms of the broad overview of the results, and we were also being asked to utilize that information, as appropriate, to perhaps modify the catch advice for 2021 and later, and this was different than the previously-scheduled interim analysis that was being provided to us at the SSC.

As we get to the last part of the meeting, the SSC has been provided with a couple of analyses, and one of them is the interim analysis, which does not use the Great Red Snapper Count, which used the procedures agreed to prior to that, and then also some advice about how to utilize the Great Red Snapper Count, if it is appropriate to do that, and so the second part of the meeting will be the point where we develop that catch advice, but the first part of the meeting -- At the January 6 SSC meeting, the SSC recommended that we have a review.

It wasn’t the council that recommended it, and it wasn’t NMFS that recommended it, and it was the SSC, and the first two days of this meeting is essentially that review, and we recommended that we have outside reviewers, and that has been set up, I believe by the council, and these are independent of the SSC, and the SSC was not involved in the selection of the members, although we did give some advice about terms of reference.

That is kind of where we are, and we are utilizing this meeting to take advantage of the independent experts and their review and to further elucidate, from the principal investigators of the study, details of that study, so that we can provide better advice to the council, and so, in terms of the terms of reference, Ryan Rindone will introduce the terms of reference, and that will kind of give some guidance over how we proceed. Ryan.

TERMS OF REFERENCE

MR. RINDONE: Thank you, Dr. Powers. On your screen, you should be able to see the terms of reference, and these were developed in consultation with the members of the Great Red Snapper Count team, with the Southeast Fisheries Science Center, council staff, and NMFS. A great many people definitely helped weigh-in on these and helped hone these down to be as useful and directed as possible for reviewing this large, unprecedented study.

The terms of reference are broken up into four main components: study design and sampling approaches, statistics and data analysis, results, and the deliverable. The independent consultants have been asked to provide a preliminary individual perspective on the terms of reference, as it relates to the Great Red Snapper Count, and those are provided as background.

It's assumed that some of those initial findings by the independent consultants may change, and so those perspectives are all drafts, as considered by the consultants and by us, and

so please view them in that light. We expect lengthy discussions to happen over the next few days surrounding these terms of reference. I am not going to go through and read all of them, but does anyone have any questions? Kai.

DR. LORENZEN: I just wanted to sort of clarify exactly what the objective of that review is, and so we had a deadline for materials, I think, on the 19th of March, and we received that project report in good time, and that's what we've looked at so far and what the consultants have reviewed, and we have not had a chance to see any of the presentations that will be provided today in advance.

I am presuming that those presentations are essentially a mixture of information, additional information, about the report that we have already seen, and possibly responses to the independent reviews that were posted over the last days, and so it's to clarify that -- My understanding is it's the document that really contains the heart of what we're reviewing, and it's that report, and that we would be treating any new information that is provided today as essentially a response to reviewers.

If that is correct, I think it would be good for the presenters today to clarify, in their presentations, what is information that is identical with what was provided in the report and flag any new or different analyses. Thanks.

MR. RINDONE: Thanks, Kai, and, Joe, just to that point, those of you that have been involved in the SEDAR process probably can see that the review of this estimate of absolute abundance differs from how we typically review something that is going to be used to inform abundance through the classical stock assessment process.

Usually, the data are either being published, or are already published, and they're presented at a data workshop, where they are discussed with the data workshop panel, and they go through the assessment process, and then they're reviewed again independently during the review workshop portion of the stock assessment process, and that process typically occurs over anywhere from eight to fifteen months, depending on the amount of work for a particular assessment.

In this case, the Great Red Snapper Count project was completed not too long ago, in 2019, and so there hasn't been an opportunity for a lot of the material that has been generated that has ultimate resulted in this estimate of absolute abundance to be published, and the red snapper research track

assessment, SEDAR 74, has, for all intents and purposes, just got underway, and so we won't expect management advice to come from that effort for quite some time.

This review of this information is, from a time standpoint, being done in a more compact fashion, and most of that has been driven by the SSC's request for the review and the council's acknowledgement of the importance of red snapper to the region.

CHAIRMAN POWERS: I would also say that it's been driven by the council asking for the estimate, Great Red Snapper Count estimate, to be utilized, or evaluated, in terms of the catch advice for 2021, and so there's some being driven by that as well. Kai, did you have anything further?

DR. LORENZEN: No.

CHAIRMAN POWERS: Okay. Sean and then Will Patterson.

DR. POWERS: Thanks. I just wanted to respond to one thing that Kai said. Realize that we, as presenters, just received the review, and so the expectation that this will fully encompass our response to the reviewers might be a little too much right now.

I mean, we've had some time to discuss it, but, ideally, and I think this is Greg's plan, we will write -- Any manuscript or anything like that, we will consider the reviewers' comments and respond to them in a final version of the report, and we'll deal with some of those, but just the expectation that we haven't had much time, and no time as a team, to sit down and look at the review and comment. Some of us will address some points in the presentation, but I wouldn't assume that this is our total response to the reviewers.

CHAIRMAN POWERS: Thank you. Will Patterson.

DR. PATTERSON: I was going to mention similar comments as to what Sean just said. I think we, as presenters, can clearly point out any comments which are in response to the reviews that we've seen, but we haven't spent a lot of time going over those, and I don't know of any new analyses that have been done.

As far as the presentations, the SSC often receives presentations as the speaker is approaching the podium, and so I don't really understand the consternation about the timing of PDF or PowerPoint files arriving for this, and I don't foresee any departures from the report, but we were asked to explain

details and present methods and results, and that's what we aim to do.

CHAIRMAN POWERS: Thank you. Kai.

DR. LORENZEN: I agree with all of those points, and it wasn't consternation, but all I'm asking for that I think if speakers could point out if there are additional analyses, or different analyses, that they are presenting, so that we are aware of where that might be the case, and that's all. Thank you.

To Sean's point, obviously, I wasn't suggesting that the presentations would contain complete responses, but just flagging that it's possible that there may be new or different information, and if speakers could point that out, when that is the case. Thanks.

CHAIRMAN POWERS: Thank you very much. As we get started, I would like the consultants to introduce themselves, a little bit about their background, and, if they wish to say something about their preliminary findings, feel free to do so, but, obviously, these are preliminary, and we're going through a process to -- Those findings may evolve. Let me begin by recognizing Dr. Christman, Mary Christman.

DR. CHRISTMAN: Good morning, everyone. I am Mary Christman from the University of Florida, and I'm a statistician who has worked in fisheries statistics for quite a while. My particular areas include sampling strategies, estimators, and modeling, but I did have a question about the timing of all of this for the rest of you.

What you're being -- What we're being presented with today, I assume, is details, fill in the blanks, for areas, at least for myself, where I did not see the information in order for me to actually review it, and I'm not expecting any new analyses, or at least I don't recall asking for any, although it wouldn't be a bad idea, and sorry, and I'm getting into details now, but I just wanted to remind everyone that we have final reports due by April 10, and so we will be reviewing -- At least I will be reviewing only what has been presented in the report, plus what's presented today, and so I just wanted you to keep in mind that any information that you can provide to me that could update my review would be helpful. Thank you.

CHAIRMAN POWERS: Thank you. We'll keep that in mind. Dr. Cadrin.

DR. CADRIN: Hello, all. As a quick introduction, I'm a stock assessment scientist, and I'm with the University of Massachusetts, but, previously, I was an assessment scientist for the Northeast Fisheries Science Center and the Massachusetts Division of Marine Fisheries.

Most relevant to this week, I have some experience in field-based estimates of absolute abundance and integration into the assessment, and, Mr. Chair, I don't know if you want me to wait until it looks like later in the agenda that we'll be talking about our review.

CHAIRMAN POWERS: Well, I don't want to get into very much detail. If you wish to say something preliminarily, feel free. Otherwise, we'll go on then.

DR. CADRIN: Just, generally, it's an impressive study, but the challenge is going to be how best to use it for assessment and management. Thanks.

CHAIRMAN POWERS: Thank you. All right. Dr. Eggleston.

DR. EGGLESTON: Good morning. My name is Dave Eggleston, and I'm a marine ecologist and a professor at North Carolina State University, and I'm not a statistician, and I'm not a stock assessment scientist.

I have experience with animal behavior and habitat use and restoration and recruitment processes, demographic rates, and metapopulation dynamics, and so I do employ basic and advanced statistics in my research, and some of our results are integrated into stock assessments of exploited species. I guess I would just echo Steve's comments, in terms of the breadth and depth of this Great Red Snapper Count, and I look forward to further discussions. Thank you.

CHAIRMAN POWERS: All right. Thank you. Ryan, do you have a comment?

MR. RINDONE: Yes, Dr. Powers, and I just wanted to clarify something procedurally, now that we've introduced everybody. The independent consultants' involvement with the meeting is focused on this first part, which is the review of the Great Red Snapper Count, and they will be weighing-in equally with members of the SSC, as far as any determinations about the Great Red Snapper Count itself, as it pertains to it being a representative estimate of absolute abundance of age-two-plus red snapper in the U.S. Gulf of Mexico, and also about the

assessment itself and about its bearing on this.

When it comes to the SSC meeting portion of this four-day extravaganza, the independent consultants will not be involved in that portion of the meeting. However, since it's open to the public, they're more than welcome to hang around for it, if they choose to, but that part of the meeting, voting and whatnot, will revert strictly back to the SSC.

In keeping with National Standard 2, we have asked that the SSC members who are PIs on the Great Red Snapper Count project abstain from voting on matters as it relates to the review of the Great Red Snapper Count, since their research was directly involved, and, if the SSC decides to make a determination about which of the -- Which, or either, of the interim analyses, later to be presented, constitutes best science and/or is appropriate for use in management.

However, when it comes to actually determining the OFL and ABC and application of the council's ABC control rule or other proxy method, as the SSC determines appropriate, those co-PIs who are also SSC members are encouraged to participate fully at that point, and they have all been briefed on this. Mr. Chair.

CHAIRMAN POWERS: Thank you. I think we're ready to begin the set of presentations. The actual location of the presentations on the website, Ryan, can you kind of remind people where that is?

MR. RINDONE: Sure, and so, if you're at the council's home page, across the top, you're going to see a blue meeting bar, and, among those -- Or a blue menu bar, and among those is something that says "Meetings", and you mouse over that, and you go to "SSC Meetings" and click on that, and that's going to take you to a landing page for all of our current and pending SSC meetings, and you will see the meeting for March 30 to April 2, and that's this one, and there will be a link that says "Agenda and Materials", and you will click on that, and that will bring you to the landing page for all the materials. Jess is showing you guys where to find all of that right now.

Some of these presentations are rather large, because they contain embedded videos, and so we're working on getting some of these things up there right now, and so some of them aren't quite up yet, and they will be, and please just bear with us. They are all up there. I stand corrected. They're all up there now, and so be patient if you're trying to download them.

Some of them are rather large, and, for the presentations that have embedded video, we'll be going back and forth between us and the presenters, to make sure that that video can be portrayed, especially it relates to things like the ROV work. Showing a still image, because we can't play the video, isn't very informative for you guys, and so just be patient with us as we move presenting controls around and try our best to accommodate everyone. Thank you.

CHAIRMAN POWERS: All right. Thank you. I believe we're ready to start the nuts-and-bolts of this, and we will begin with Dr. Greg Stunz, and he will be making an overview presentation, to get us started. Greg.

REVIEW OF THE GREAT RED SNAPPER COUNT PROJECT
REVIEW OF THE GREAT RED SNAPPER COUNT
PRESENTATIONS
SUMMARY PRESENTATION OF GRSC

DR. GREG STUNZ: Thanks, everyone. It's really great to be here with the team that did this monumental study to talk about where we're at. We were very interested to get the reviews back from the independent reviewers that had some very insightful comments that we feel we can address.

As far as the format of getting through this section of the agenda, the council staff had asked me to give an overall high-level review, or presentation, so everybody is kind of on the same footing, and I believe there's a lot of public joining in, and there's probably that haven't heard it. Many of you have heard it before, or maybe multiple times, but that will bring us all up to speed of where we are, and then I have various key investigators along the way to present certain sections that relate to their area of expertise.

Dr. Powers, thanks for accommodating some of our schedules, and it's monumental, trying to get this many people together at one time to give these presentations, and so there may be just a little bit of moving around and things, to accommodate time zones and that sort of thing, and we really appreciate that.

I thought too that I would give a little history about where we arrived where we are, because that also influences, ultimately, the assessment and that sort of thing, and so I will go ahead and begin with what were the main goals, and, I mean, obviously, anyone tuning into this call realizes the magnitude and the iconic nature of red snapper, and so part of an issue of having the best-informed management was, obviously, having an absolute

abundance estimate, which could lead to more sophisticated modeling as well as just general expansion of our knowledge base about this really important fishery.

Because the management was very controversial, and it's a mixed-use fishery, and a whole variety of things, leading to a lot of issues in managing this fishery, it certainly has the attention of Congress and many other people, and one of the main needs recognized was this absolute abundance estimate that was needed, and so Congress appropriated funds, at a really extraordinary level, in terms of doing a study that was required of this magnitude and broad coverage that the region spans.

The total price tag on the estimate was $12 million. It was an independent assessment, which I will explain a little more later, but that means it was independent investigators and not NOAA scientists directly involved in the study, and it was really nine-and-a-half million in appropriations, and the institutions were required to come up with the remaining match, bringing it to a $12-million total.

There is twenty-one leading scientists, and that numbers comes and goes. Some joined late, and some left for other places and that sort of thing, but roughly twenty-one, representing twelve institutions. As we were putting together this report, we really realized that, wow, there was over eighty scientists involved, when you begin looking at graduate students and post-docs and lead scientists and that sort of thing.

I will introduce that team, because I think that's very critical for understanding where we're coming from and the expertise that we brought to bear on this project, from beginning scientists all the way to well-seasoned scientists.

We'll talk about that in just a minute, and how was it estimated overall? Well, I wish we had a magic bullet, and we will talk about that quite a bit, of how this phased-in approach occurred and that kind of thing, but what it comes down to is there's really not one overall sampling methodology that can cover the vastness and diversity of the Gulf of Mexico in all habitats and all regions, and certainly that creates some challenges that we have to deal with.

The first task at-hand with this project was habitat classification, and you will understand why that is as I begin to explain that, but we are quite limited on the mapping that we have available, and that just doesn't exist, but it was very clear, in the appropriations, that money cannot be spent on

habitat mapping and only using and utilizing what was currently available, and the money was to be directed at actual counting components of this and a few other areas.

We relied heavily on direct visual counts, where we could. As many of you know, snapper occur in low visibility and high abundance, and that creates a problem for visual counts, obviously. At those points, we relied on hydroacoustic surveys, and, in many instances, depletion surveys were required, and I will explain that better, particularly in areas of very high abundance of red snapper, and then, also, while it wasn't in the terms of reference, we were directed to spend a very large portion of that money on a Gulf-wide tagging program, through those congressional appropriations and through Sea Grant.

While we won't really talk about that too much today, although we've asked Dr. Catalano to talk some, because, even though you're not evaluating that, it is very important, in terms of the scope of the whole study, particularly for the SSC members, and as we move even beyond this project.

That was generally the scope of it, and so I will kind of get into this broad overview, and then, just so everyone is aware, the experts in each of these regions and sampling methodologies is here, and we'll hear a lot more detail about how it was done, what our approaches were, and that sort of thing.

I think it's really key to talk about who we have involved in this project, and these folks are really quite a modest bunch, and so I feel it's really my job to talk about them, in the sense that they're the best in the business, and we spent a lot of time putting this together and drawing who is the best person in the Gulf, or really in the world, to address this project, because we knew what the challenges were at-hand, and we needed to have the best teams.

Of course, each one of these individuals have very robust teams behind them of post-docs all the way down to undergraduate and technicians and that sort of thing, but these that you see here were the key lead investigators.

Dr. Rooker is from Texas A&M Galveston, and he, obviously, handled the Texas region, particularly the uncharacterized bottom, which I'm sure we'll spend a lot of time talking about over the next two days. Dr. Cowan with LSU handled the Louisiana components, and Dr. Powers from South Alabama handled the Alabama and Mississippi regions, which were grouped together, and Will Patterson handled the Florida region.

Dr. Murawski really integrated uncharacterized bottom Gulf-wide across the entire region, using his C-BASS, which we'll hear a lot more about this morning. Dr. Catalano from Auburn was in charge of our tagging program, which he will discuss. Marcus Drymon, while he's a red snapper ecologist in his own right, his primary role here was to do the outreach and engagement, but he spent a lot of time making significant contributions to the abundance estimate.

We had an expert analytical team and design team, and that was led by Dr. Ahrens, who at the time was at the University of Florida, and he now has a NOAA position in Hawaii, and that's one reason that we're adjusting the schedule, and so I didn't want to get him out of bed at 3:00 a.m., and we're making him get out of bed at 4:00 a.m., and so, anyway, he was willing to do that, recognizing the importance of this project.

Lynne Stokes is at SMU, and she's a sampling expert, and she will be contributing to this as well, and she was involved in the alternate validation estimate that we did, and then John Hoenig was also a key player in the analytical components, particularly as it related to the initial design, but especially the depletion surveys for the Mississippi/Alabama region.

In addition, and I know this is a lot of people, but I think it's really important that you understand the level of expertise and those minds that are on this project. Kevin Boswell, you'll hear from him in a few minutes, and he's the acoustical expert. He's world-renowned, and particularly in the Gulf of Mexico.

Liese Carlton at VIMS is also with John Hoenig and is key in those design components. Judd Curtis is with our group, and I guess you will probably recognize that part of the issue here is that many of these folks are all SSC members, compounding some of the evaluation problem, but it's good to have these kind of folks in both places, for obvious reasons. Judd was critical in the proposal and design and pulling it off in the report.

Robert Leaf at Southern Miss was important for some modeling components of this, and Vince Lecours was a major backup to Will out in Florida and dealing with those estimates. Will Liu at A&M Galveston is a quantitative fisheries scientist that many of you know. Both David Portnoy and Eric Saillant, while we could not -- We were precluded from doing any type of genetic analysis, but we put our hands on a lot of fish for this project, across the entire region, and so we were able to collect a lot of samples, and we were allowed to archive those

samples for future analyses, although we could not do the current analysis with this, but, because of that opportunistic component, those two led the genetic collections for the eastern and western Gulf.

Matt Streich is a key person that was involved in the design, particularly from the beginning, but the Texas estimates, as well as David Wells from A&M Galveston was key for acoustics and other components in Texas. Tara Topping was the one behind the scenes, and I think the amount of administrative workload and just coordinating this many folks and this variety of sub-projects within the overall projects is just a massive task that I greatly underestimated going into this, and Tara pretty much helped keep this together, and still does, even today.

Steven Scyphers at Northeastern came in a little later in the project, but he was responsible for some of the work in terms of surveys and the major angler engagement piece of this, with commercial and recreational fishermen. While that's not under evaluation either for this component, it was an important component, and we were directed to spend a lot of that money engaging with constituents in the red snapper fishery.

Finally, while it was an independent study from NOAA, we did involve two NOAA -- They were non-compensated collaborators, and, I mean, obviously, it's important that we want to be designing a study that has some utility, and these two folks were key to make sure it gets integrated in what NOAA currently has going on, or providing that advice, although they did not contribute to the actual estimate and that sort of thing, in terms of analytical procedures and that sort, but certainly John Walter knows what is needed for the management process, and he was really key and a really good advisor, as well as Matt Campbell, who is probably one of the best scientists in the Gulf, in terms of sampling with a variety of gear for red snapper, and so they were instrumental components.

I'm sorry that took a little time to get through, but I think it's important that you realize the team and the other eighty, or seventy-ish, scientists that are behind these leaders pulling it off. Of course, we're represented from key institutions around the Gulf.

So how did we arrive where we are? Well, it was led by a well-integrated steering committee that was formed from the appropriations, and they heavily were involved in the process the whole way, initially with many, many meetings and workshops to define what this process would look like.

It was a Phase 2 process, and we're talking about Phase 2 at this point, but we were really fortunate, in that we had a Phase 1, where we were able to compete -- Several groups competed for a design proposal and what the best design would look like. That steering committee took those designs and brought together the best components of those designs, which was somewhat of an unconventional task, but a very good planning component.

They then incorporated that into Phase 2, and then a separate request for proposals, or what we're talking about today, for actually following that design that was loosely proposed by the steering committee, and, I mean, we certainly had some flexibility to adapt with that, but they gave us the general guidance.

What did that look like? It was specifically detailed, in terms of what the scope would be and the goals and objectives and what the general sampling methodology should be, based on those Phase 1 proposals of putting some of the best minds that have been doing this in the Gulf to figure out how we needed to accomplish this.

It specified a general statistical analyses, what our target CV should be, and the geographic scope, in terms of the depth ranges, where around the Gulf of Mexico, what habitat types they would like assessed, and it also specified that tagging component and a comprehensive stakeholder engagement and this concept of no genetic methods, but only archiving of those samples. We, obviously, stuck to those and encouraged those guidance documents.

It was very clear and specified in the RFP. Because of the unique difference of each region, we don't have this magic bullet or this one-size-fits-all approach that is going to handle all of those habitats and all of those differences in the region, but it relied on the wealth and skills of all these regional investigators to really develop the methods that were proven and that were successful in those areas and utilize that for a broader Gulf-wide estimate.

It encouraged the use of multiple sampling methods, but, as we'll hear this week, that complicates things and presents some challenges, but, in a perfect world, and maybe other groups learning from this can come up with methods that can do that, but, currently at-hand, we had to rely on these different methods.

In addition, we were charged specifically with developing advanced technology and gear that would be appropriate for these study needs that could be done for future studies, and, as many of you know, there is a similar study going on in the South Atlantic, and there is another one out for request for proposals now for a greater amberjack study, and so we're hoping that, as being the first study, others can use our lessons learned to adapt and do what they need to do to carry out the goals of those projects.

We were very much hindered by surface bottom habitat mapping, which was clearly well known, going into this, as not sufficient or comprehensive, but to really do what we need, and the better maps that we'll have in the future will improve the estimate, but we knew that going in, but we utilized what we had, and that, obviously, creates some challenges there as well.

Then, for areas in much of the Gulf, it's unmapped, and typically now we call it "unclassified", but the RFP specified it as uncharacterized bottom, was really a catch-all category, and it did -- This is going to become very important in a minute, and that's why I'm spending some time here. Although it's a very high coverage throughout the Gulf, many people think that it's just sand and mud, and that's not the case.

It is a lot of that, but in it is a lot of other stuff that is structure-oriented, and being reef fish, that tend to be utilized by red snapper, and so it's probably unmarked reef, unknown natural habitat, debris, shipwrecks, unknown oil-and-gas infrastructure that's not mapped, and it's really everything that falls within that, and that will become important, for reasons that you will see.

We spent a lot of time defining that sampling frame, and we've got an entire presentation coming up in a little while on exactly how we did that and defined that sampling frame by regions, but it really fed into our overall design, and so I won't spend much more time talking about that, since we'll have a lot more details, and I'm sure questions, from that coming up.

The overall design looks something like this, feeding in from the Phase 1 designs, and ultimately through Phase 2, and we broke the Gulf into four regions that were generally ecoregions, which lined up very nicely with state jurisdictions that we further divided into state jurisdictions, so it would be appropriate for management advice, but the ecology matches that, as well as the geography.

With those regions, we had three depth strata, from shallow to mid to deep, and we had artificial reef as a category that was specified to be assessed, and we broke that, generally, into large and small categories, but that presents a problem, in the sense that the distribution and size of these reefs vary greatly, even within a region, even within some strata, where, generally, small structures are in the east and large, massive, the size of three or four-story building kind of size, in the west, in the oil and gas platforms, and everything in between.

That presents -- You know, how do you group and categorize those, and we'll explain how we did that, the best we could, with the sampling levels that we could allocate to that sort of thing.

Those are well-known habitats that a high abundance of snapper occur, as well as natural reefs, or what typically were called snapper banks, in the fishery, and those are well-known natural reefs, and they're very conspicuous, particularly in the western Gulf of Mexico, where there are very few of them, but they're very well-known, large features with high relief, and then, finally, this uncharacterized, or probably a better term is unclassified, bottom habitat, which is everything else.

That is important, because we suspected, for a long time, that there was red snapper in this area, and, in fact, it was known. I mean, if you knew where to go on a structured piece out there, you could find snapper.

During NOAA bottom longline surveys, they periodically were, obviously, characterized by a lot of zeroes for snapper, but they would also periodically set on unstructured bottom and catch a lot of snapper, but it had never been assessed, and it had never been well developed or incorporated into the stock assessment process, and so we were charged with defining what that bottom more look like, more in terms of absolute abundance, the areal coverage, of course, presents a sampling challenge there as well.

Each region will talk in detail about all the sampling that occurred, but, in general, it was a lot, and a lot of effort, thousands of locations, and probably thousands, if not hundreds, of hours of ROV video to work through, and not only did the team spend days and days at sea, but they spent days and days behind a computer processing ROV video transects and hydroacoustic profiles and other types of output coming back from our collections, as well as vertical longlines and this extensive tagging.

When we could, looking at the fish is probably our preferred method, but that isn't the case in many areas of the Gulf, and you'll hear a lot more about that, but the technology that we have at our hands today, even in the last few years, is quite amazing, what we can put in the water and get instant video feedback, and equipment can be outfitted with all types of sonar and other scientific instrumentation, and you'll hear a lot more about that, but visual was the primary means, where we actually go down and look and see the fish. However, as we all know, these fish move, and they're in areas where we can't see them, and so that creates a problem and a need for alternate gear.

To give you some examples of what habitats might look like, reef pyramids, where you typically can see the entire reef in a single field of view, off of Florida in the top-right, and there's some examples of smaller reefs that are put out intentionally, in Alabama, for example, or in Mississippi, and even in Texas.

Then, as you get out into the western Gulf, the bottom-right is what you might imagine, and it's a large -- This is just a tiny portion of an oil-and-gas platform that tend to have large concentrations of red snapper, and, of course, the different and interesting thing about these habitats is they have such a vertical reef that the snapper are distributed throughout the entire water column, typically, mainly near the bottom, and they're demersal fish, but near the bottom. In general, we have to account for them all throughout the upper water column, which does not occur on the other reefs, where they're much more side attached to the habitat.

To give you an idea of what that would look like, and we'll talk about species composition coming up, but this is an example of characterizing a species composition. As you're descending, what might be eight piles of jacket, is what we call it, of the oil-and-gas platform going down. Many times, these are cut off, or toppled, or converted into artificial reefs, with still substantial relief, and sometimes they are extending completely up out of the surface of the water.

Our equipment is outfitted with lasers, so we can get a size composition, and the way we did the visual surveys, to give you an example, is this challenge we had with turbid and clear water. If you look at the video on the right, typically, Dr. Patterson was blessed with calm seas and great visibility, and it would look generally something like this, although this happens to be out in the western Gulf, in a good time when we

can do species composition. Unfortunately, that's a relatively rare occurrence.

What you see hanging down is a means for us to zoom in and back away to calibrate our visibility that we had, but, often, in the Gulf -- This would be a pretty good day in the Gulf, and the video you see on the left -- I mean, obviously, those are red snapper.

Many times, we can't tell for sure if those are red snapper, or are they gray snapper or vermilion snapper, and they get lumped into a snapper category as unknown and were not counted as part of the estimate, and that's a good example of how, typically, we erred on the side of caution, at pretty much every fork in the road like this with the estimates, and so probably our estimate is underestimate, but we didn't want to call fish snapper that we couldn't 100 percent confirm as red snapper.

As you saw in that video, clearly some of those are red snapper, but that's a pretty good visibility, and so, many times, we've got to develop other methods, when we can't see at all, and that's where acoustics, or really hydroacoustics, comes into play, and, in just a minute, Dr. Boswell will talk about the details of how we performed hydroacoustics.

Dr. Murawski will be giving a presentation, but, to introduce what he's got, oil-and-gas infrastructure is prevalent, particularly throughout the western Gulf and a little bit into the eastern Gulf of Mexico, that you can see here. Some of that is pipeline, and it's exposed and unexposed, and there's about 42,000 miles, in fact, of pipeline, but Dr. Murawski was in charge of the pipeline, but also the associated uncharacterized bottom as well, and using his C-BASS, which he'll talk a lot more about, and these are general examples of flying down a pipeline, and we'll talk about his methodologies with counting fish, either like this over uncharacterized bottom or perhaps natural reefs, and Steve has a lot more to say about his C-BASS.

We used a similar methodology called TARAS, which is another towed gear that's patterned off of the C-BASS, in some of the regions, where we have to cover vast areas of bottom that aren't appropriate for taking two hours to get your gear in the bottom for an ROV survey when you have that vast amount of coverage of bottom that you have to account for.

Kevin will spend a lot more time talking about hydroacoustics and how we generated an abundance estimate of that in areas of low visibility, but why do we need it? Typically, the Gulf has

a persistent nepheloid problem, and this is a toppled oil-and-gas platform that is sticking up probably a hundred feet there, but, as you can look down and see -- Obviously, you see a lot of snapper, but, about ten meters below that, it goes to almost zero visibility, kind of a fog layer, which is actually silty sand and mud that we observed the snapper going in and out, and so we don't know how many snapper are down there, visually.

Alternatively, it might look something like this on the right, where, again, you see the snapper there on a natural bottom, but how many snapper are behind those that we can't see, and that certainly, obviously, creates a problem. Particularly, this detectability issue that we're talking about here leads to somewhat of an underestimate, when we can't count the fish.

Sometimes we have the opposite problem, and Dr. Hoenig and Dr. Carlton, as well as Dr. Powers, will talk about depletion, where, all of a sudden, it's clear enough that you have so many fish in the field of view that you can't count them all without the issue of double-counting, and they will talk a lot more about the use of our depletion methodology and that here in just a little while.

Then the last piece that Dr. Catalano will talk about, and I'm not going to spend a lot of time talking about, was this tagging program that we have that was tremendously successful, and he'll talk a lot about that, in terms of angler buy-in and engagement and a whole piece that's not under evaluation today, but I think it's very important that the SSC understand that component, in terms of this tagging thousands of fish Gulf-wide with an astonishing 30 percent return rate, which has a lot of implications for discard mortality, use of SeaQualizers, or descending devices, and that sort of thing that's outside of our terms of reference. It also allowed us to do genetic samples for future analyses as well as this angler engagement piece that I have been discussing.

While we won't talk about it today as much, or the next few days, the stakeholder engagement was a major piece, a whole series of videos that Dr. Drymon developed, as well as anglers surveys, working with Dr. Scyphers, to get the word out about this study, and that was a major charge coming from NOAA Sea Grant, and well as those appropriations.

There's fact sheets and videos, and everything is housed in one spot at snappercount.org, and this has become an extremely popular way to get the information out, and so I would encourage everyone to go there, if they want to learn more about the

project, and including the final report and executive summaries and all that sort of thing are archived there as well.

Getting to the estimate and analysis, which we'll talk about in much more detail, we had the primary analysis, which was led by Dr. Ahrens, and we thought it was a good idea to have an independent assessment, and they were different, particularly in terms of post-stratification and that sort of thing, which I'm sure we'll talk about.

What was interesting is both came back with relatively low CVs, and I'm sure we'll spend more time talking about that. In general, they converged, with only about a 4 percent difference, which was roughly about five-million fish, and so we were very happy about that, in terms of different ways of looking at the data and partitioning that data and still arriving at a similar estimate.

In terms of what that overall looks like, our estimate has come back at 110 million red snapper in the Gulf, with a low, 11 percent, CV. I'm sure we talk more about that, and did we capture all of the variability, and just certainly not, and we know there's variability inherent that we can't even measure, but, right now, the data is supporting our 11 percent CV.

We'll talk a lot about how it's broken down, in terms of regional and habitat, and that's about thirty-six million, compared to thirty-six million from the last SEDAR assessment, and so, roughly, we're just about tripling that estimate, and there's some very plausible reasons why that's the case.

I won't go through this, because each region is going to go through this in much greater detail, but, in general, we have broken this out by natural and artificial reef and uncharacterized bottom. In our alternate analysis, we even further characterized this into depth strata and others, and, obviously, you sacrifice some of your CV as you begin to parse this out more and more, in terms of losing sample size and that sort of thing, but we'll talk a lot more about that coming up.

I wanted to put this -- This is sort of a challenge, and why this has become so important, and maybe I should go back, and what you see here, and it will become very apparent today, is the lion's share of the fish are uncharacterized bottom, a habitat that hadn't been sampled, and it doesn't get sampled by the fishery, generally, because those fishery-dependent information coming back are coming from artificial and natural reefs.

Typically, it’s somewhat of unknown areas out there that are hard to fish, that are very far from port, and so you typically have a population of fish that probably is not showing up in fishery-dependent data, and, for that matter, probably fishery-independent data, because not a lot of sampling has been occurring out there, short of the bottom longline survey, until we started going out there.

You wonder, well, why in the world are all these fish on the uncharacterized bottom, and, literally, this just came across my news feed this morning, and so I thought it was interesting to put in there, about why haven’t you received your shoes yet, check the ocean floor.

Just this year, there’s about four-times as many containers that have hit the ocean floor, due to shipping issues, like traffic and rough weather and that sort of thing, and so these are, obviously, perfect habitat. We have no way of knowing how much of that is out there, but, doing our surveys, through C-BASS and TARAS, at least we’re able to account for some of that, and so that’s a big challenge, of what’s out there on the unclassified bottom that we simply just don’t know about.

Just to set the stage, what are our biases? What about our assumptions? What are we uncertain about, because certainly no study is perfect, by any means. We certainly have a lot of hindsight, and we would most certainly do things different if we knew then what we now know today, but, obviously, we’re going about this in a massive study with little information, and sometimes no knowledge at all about your sampling frame, and so that was quite the challenge.

When you begin to look at the biases overall of the estimate, it is very conservative, and we knew that going in, and I think that’s clearly picked up on by the independent reviewers, and it’s likely an underestimate of abundance, and we certainly would prefer an underestimate than an overabundance, from a precautionary standpoint, but we tried to take these conservative terms when we didn’t have the information that we needed.

Visual constraints lead to an underestimate, when we can’t positively identify fish that probably were red snapper, but are grouped into an unknown category, and hydroacoustics that we used in low visibility likely underestimates, and I’m sure we’ll talk a lot more about that, just by the nature of that gear, and it excludes a portion of the fish.

We assume that habitat types are known with certainty, and we certainly don't know that. Alabama has done surveys in areas where they have put out reefs in generally unknown locations, but in certain areas. In other areas of the Gulf, we generally know where the artificial reefs are, but it's not certain, and then, obviously, in the uncharacterized bottom, we don't know at all what could be out there, in terms of the known mapping that we have, and so improved mapping will certainly reduce those biases, but that's well beyond the scope of this study.

The pipeline habitat availability and other visibility issues, but, with that habitat, you could be considered somewhat ephemeral. Sometimes it's exposed with currents and scouring, and sometimes it's covered up, and so estimating that availability is a difficult task.

Known populations do occur outside of our defined study area, and there is salt domes and pinnacles that we know, from bottom longline and other longline surveys, that snapper occur there, but they were outside of our specified study area, and so those are not accounted for.

We made some downward adjustments to our estimate for small fish size, and we were required -- Just to be very clear, these are not young-of-the-year fish and small juvenile fish, in terms of red snapper, and these are age-two-plus fish, typically the size that they recruit to the fishery and into the exploitable population, but a fish is 364 days old, and then the day a one-year-old turns into a two-year-old, and is that -- If they join that population, are they really arbitrarily defined in this age class?

That creates a problem, because smaller fish can be age-two, but, also, bigger age-one can be age-two, and so we did some calculations, and this is particularly a problem in Florida, and somewhat in Alabama, where the population is recovering and growing in areas that it traditionally occurred, but it has been overfished, and that's not so much a problem in the western Gulf, where there are much, much larger fish, and anything below age-two on our habitats generally don't occur, and they're extremely rare, if they do, but, nevertheless, we adjusted that down, to make sure we weren't counting any age-one fish, but, in general, these were fish that were large and recruited to the fishery.

Then, just in general, the detectability, in a variety of reasons, leads to underestimation, and just the nature of the

habitats and regions lead to some uncertainty in our estimate, because we just can't detect the fish.

Obviously, we've got a need for a lot more calibrations, and I put in there or perhaps not, and we -- As the study was developing, we recognized that the regional differences are so great, and even differences within a region, and even with strata are so great sometimes, that it makes calibration a really steep hill to climb, in the sense that, just because you calibrate an area with great visibility in a particular type of habitat, that visibility and habitat is so drastically different in other areas that you pretty much have to calibrate in every single area, and, many times, you can't do the calibration, because there is not visibility, and so much of our estimate is very much stand-alone, in a regional area where we had known gear and that sort of thing, but that's definitely something that I'm sure we will discuss more, in terms of how that integrates into our assessment.

What are the key takeaways? Well, certainly science is working, and this was never intended to be some type of competitive-type of process, and it was to supplement our knowledge base and to build on the current assessments and be able to refine and improve those assessments, and I think it's doing that.

Our independent analyses have quite a bit of agreement, that we talked about, and we won't talk about the tagging, but the fishery exploitation pattern is very important, and the exploitation happens to occur on the habitats with the lowest abundance, and that certainly is outside the scope of the actual abundance estimate, but it has clear management implications.

That high abundance on uncharacterized bottom may explain this weak stock-recruit relationship, which has plagued us for many, many years, and so that clearly needs to be explored, later down the line. We're in the middle of an effort recalibration, and the short story is we're seeing a lot more effort where we didn't think there was in the past, and that, obviously, influenced that earlier stock assessment. When those calibrations come into play, those might change the differences between our number and the last SEDAR 52 number.

That tagging return rate, which you'll hear more about, was just very astonishing, and, in fact, it was a budget blower, but it also is showing that, at least under the parameters that we defined the study, the discard mortality -- These fish can be caught and released, to at least some extent, which is good news, in terms of regulatory discards.

Then, finally, tremendous angler buy-in, and this study just became very, very popular, particularly with Dr. Drymon's work, and it's just the tagging component and that sort of thing, and that's always nice to have, just in general, in the sense that we're working on a study that a lot of folks care about, and so we're real proud about that, and we'll continue that engagement plan throughout this process and beyond, as things begin to develop.

I will go ahead and stop there. I mean, I want to say a few closing remarks, but, maybe before I really do that, I just want to talk about that we clearly recognize that there are some issues in the assessment, things we could or could not deal with, variants we could or could not capture, and no study is perfect. I mean, no study is, and we don't pretend that this one is.

We certainly feel as though we produced a very robust estimate, and it's going to greatly advance the stock assessment, and, in general, our knowledge base. We clearly have a lot of lessons learned, a lot of lessons for the next great name-your-species counts coming up that we highly recommend that those folks take into consideration, and, in fact, we've already been involved in that process, to build upon what we know, since this was really the first study of its kind, and for sure in the Gulf of Mexico on red snapper, particularly at this scale and magnitude.

Some of those things certainly don't devalue our estimate by any means, and much of those concerns we tried to clearly capture in the report and point out. Would we do things differently? Certainly. For example, we would spend a lot more time on that uncharacterized bottom, and we would spend a lot more time trying to get any type of mapping, or request additional mapping be performed through other avenues.

I want to point out that it's important to realize that there's a lot more to this study than the terms of reference that we're evaluating just here today, and the reason we are here, and it's outside, and Ryan Rindone did a good job of summarizing this early on, but, in terms of the compressed timeline, and not only were we on a compressed timeline to do this study, but great management needs have arisen recently, as this study was ongoing, and so it became even more relevant, and so those management needs became very important, and so our team has been working closely and accommodating and trying to provide that information, so we can deal with the management side of this, but, typically, obviously, we would have preferred to go through

our normal data collection workshops and assessment workshops and that sort of thing, build this in through peer review, but we also want to be responsive to the management needs in that fishery.

That's how we arrived where we're at today. In terms of acknowledging, and I will do that now, so each group doesn't have to do that, but we're certainly very appreciative for the appropriations from Congress that NOAA facilitated through their Sea Grant Program.

To do a study like this -- I mean, $12 million is a lot of money, but, when you start looking at $10,000-a-day ship time, you can spend the money fairly quickly, but we were appreciative of that, to be able to do, certainly -- We are appreciative that they recognized the value of what was needed.

We recognize NOAA Fisheries, and particularly Clay Porch was instrumental from really day-one, making this happen, as well as his whole leadership team and other scientists, and LaDon Swann, who leads Mississippi-Alabama Sea Grant, was really key in this, and he led the Steering Committee, and he guided us all along the way.

The administrative workload behind the scenes, as any of you that administer grants knows, it was astronomical, and he allowed us to focus on the science, and he really dealt with all of that, and he was really responsive to any of our needs, and particularly his staff, Loretta and Devaney. I guess, as a caution for other studies to take that into account, in terms of there's a lot more just beyond the science, and so LaDon really came through in a big way with helping with that.

Dr. Powers, that's the general overview. To get through the general overview took a while, and it's, obviously, going to take a little more time to go through each piece, but I will go ahead and stop there. Dr. Powers, I don't know if you want to handle questions, because that could get tricky, because we've got a lot to get through, in terms of all the specifics, and so I will kind of defer to your guidance on that.

CHAIRMAN POWERS: Thank you, Greg. You are actually on the next agenda item, the project goals and objectives, and, Greg, you were going to address that as well?

DR. STUNZ: Well, I did that. Sorry, Joe, and I should have pointed that out, and I addressed the goals and objectives within the study that I just gave, broadly, and each individual

will be giving specific goals and objectives for their particular component, and so I just addressed that in a much broader --

CHAIRMAN POWERS: Okay. That's fine. One of the things that strikes me, from this presentation, and I haven't read the report, is that, in a sense, this is not an estimation, and it's a bunch of coordinated little projects, and so, once we get to the point of actually estimating abundance, the actual estimations are being made at individual strata, and so decisions that are made about sampling, the actual methods of sampling and things like that, really vary depending on what region you're in, what depth you're in, and so on and so forth, and so people should keep that in mind, and also the presenters keep that in mind, is that, when they're talking about sampling, which one of the strata are they referring to, artificial reef versus UCB, UCB being uncharacterized bottom, versus natural reef, and also depth zone, things like that, and region.

In my mind, these are independent studies that have been coordinated to provide some overall estimates, and so, as we go through these discussions, I would like both the listeners and the presenters to not gloss over those sorts of differences, so we are clear what we're referring to. Thank you.

DR. STUNZ: Dr. Powers, might I make one comment that I think might affect the agenda, or just the flow, so everyone is aware?

CHAIRMAN POWERS: Yes.

DR. STUNZ: So we've asked Dr. Boswell and Dr. Murawski to go before we get into the regional frameworks, because their methods cross the regional network, and so we don't have to be repetitive within each -- Florida doesn't have to talk about acoustics, and Alabama and so on, and we can sort of address that broadly. That way, I think it will make for a more streamlined process, and it also gives a little bit of time for Dr. Ahrens to join us at a reasonable hour for him, when we can really get into the design framework and that sort of thing.

CHAIRMAN POWERS: All right. Thank you. With that then, I believe, in terms of the agenda that I have, Dr. Boswell, Kevin, will be talking about some of the acoustic methods. Then, after that, this is where I'm unsure about the -- Is Dr. Ahrens following after that, or that being delayed?

DR. STUNZ: It be would be Dr. Murawski after Dr. Boswell and then Dr. Ahrens.

CHAIRMAN POWERS: Okay. Thank you. All right. With that then, Dr. Boswell, Kevin.

GENERAL ACOUSTICS METHODS ACROSS REGIONS

DR. KEVIN BOSWELL: Like Greg had indicated, we were asked to generally provide a broad overview of acoustics and sort of walk through the process that we used to ultimately get to an estimate of density.

In all of the regions, acoustics was used, and I will just throw the caveat out there now that in each region it was used slightly differently, but, for the most part, the methods that I will present this morning are pretty consistent across all the regions.

Just for those unfamiliar, I'll give a quick primer on acoustics and why. Clearly, it's useful, because it can see through waters, where visual, or optical, methods don't really work, and so there's a lot of benefit to having it as a tool, and, across the world, it's used in many assessments, and so it's a tool that we can rely on derive quantitative estimates, but I will point out --

I will make it clear that this requires independent information to help validate backscatter. In other words, we need additional information to identify -- What you see on the bottom-right panel is an echogram, or a fish finder, if you will, and that provides a full water column view, where you can see that hard red reflector on the bottom, and that is the substrate, and you can see fish, as well as some other smaller scatterings, and so this is generally what we're working with and what we're attempting to -- Then, on the left, is another type of acoustical instrument that was used as some level in this project. It's an imaging sonar, and it still uses sound, but it provides a different perspective, which provides additional context, and I think we'll get into more detail on that a little later on.

It's, of course, got large potential when conditions aren't really suitable for optics, and you saw this comparison earlier, in Greg's talk, between the east and west Gulf, and, of course, this is where the utility of acoustics comes into play, and, as we move more westward from the Mississippi River, this becomes more apparent, for the need to rely on another method beyond just optics, and so they can work together in tandem, and that's how we've used it in this project, is the acoustics are

effectively informed by what the video saw for helping to identify proportions.

In the majority of the studies, we relied on some information from optics, as they were available and based on the quality. There's been some new developments in acoustics that provides additional species context, and I will just touch briefly on that at the very end of the talk, and so, absent of that, we need additional information, and understanding the backscatter is really important, particularly in a challenging condition like the Gulf of Mexico, where we have relatively speciose systems, and there is numerous species that occupy similar habitat, and so our challenge, of course, is not only to detect the fish, but then to try to isolate, or separate, them, and then, of course, becomes challenging.

MR. RINDONE: Dr. Boswell, I would like to interrupt you. Your sound quality, for a presentation where sound is key, is actually kind of poor right now, and I don't know if it's the same for everybody else, but we're having some trouble hearing you, and I would like to take a pause here to see if we can get you to connect using your cell phone, and see if that improves your audio quality, and then a second presentation point is we are seeing your staging screen and not your main presentation screen, and so if it's possible to --

CHAIRMAN POWERS: Let's take a ten-minute break now. We've been in for ninety minutes anyway, and so let's take a ten-minute break and see if we can get this resolved. We'll be back in ten minutes.

(Whereupon, a brief recess was taken.)

CHAIRMAN POWERS: Let's reconvene, and, Kevin, if you will continue on then.

DR. BOSWELL: Okay. We were just going through the final details here, and one of the other elements that is provided with additional information or ground-truthing methods and optics is -- The primary one here is understanding distribution of organisms near the substrate, and, of course, acoustics is challenged by its ability to detect targets near substrates or hard reflectors, and so the seafloor, of course, is a challenge that we just always have to deal with.

The primary objective here, of course, was to quantify red snapper across all the regions and then use the video data to inform the composition estimates. The role that we played

specifically at FIU, in my lab, was to data collection across the whole Florida region, participate in a calibration study, which I think Dr. Patterson will go over, and then also serve as a -- Serve to provide guidance for all of the other groups that were using acoustics, and so to develop a consistent and broadly appliable workflow, which I will walk through today, and make sure that everyone was essentially doing the same thing with the acoustic data, so that we had comparable results, and that was our take-home message.

Specifically, in Florida, and I think Will will talk about this a little bit as well, but we had a substantial field effort, which encompassed the use of acoustics across the broad West Florida Shelf, across each year, and so a point that I want to make clear is that all groups used a seventy-kilohertz echosounder, and we were able to incorporate additional frequencies, and so that means that there is one consistent among all of them, and then, in Florida, we added a bit of additional frequencies, which helped us to provide some more refined information, acoustically-derived information.

Then, finally, survey designs are going to vary, and you will see this in the next slide, and these were developed as appropriate to the habitat that was being surveyed, and so you’re going to find that, in just reading the report, there’s different approaches for the ways to conduct the acoustic surveys, and those were done because of the habitat context that is important.

The process for deriving our estimates were, first of all, to perform echosounder calibrations, and so, using the standard sphere method, all regions applied this, and they did this routinely, and so all of the echosounders were continuously calibrated, so that we could ensure that the backscatter received was comparable and quantitative, not only to understand the intensity of the target, or the sphere, but also to ensure that the system was operational.

Like I mentioned, the survey designs were somewhat variable, and this was a regional and habitat-specific context, and so, in some places in the report, you will see that there was this transect design, shown here with the artificial reefs, and, in others, it’s the sort of flower-pattern design, and then there are even still some others, like, for example, some of the unconsolidated habitat and pipelines, and those were straight-line transects.

After that was done, there was the task of post-processing the

acoustic data, and it encompassed a suite of different sub-processes, which are identified here, and then this process to identify targets, delineate them, classify or categorize them, and then perform echo counting and echo integration approaches.

Here's just a quick walk-through of what that looks like. I'm not going to spend a lot of time on it, but this is generally the process that all of the different regions performed, and so, on the bottom-left, you'll see that there's this Echoview workflow, and so Echoview is the software that we use to process the acoustic data, and it allows for not only a very complex process, but you can also scrutinize, at every single step, the effects, or the changes, that are made and the assumptions that underlie that approach, and so the benefit of this is that everyone can share this workflow, and that means that you have the ability to understand how there may be variance in the approach and also understand where things may have gone wrong, and so it helps to troubleshoot as well, which is quite helpful.

I will just walk through this little cascade of echograms, and so, in the upper-left, we see the raw data, and the raw data is, of course, what we measure with the echosounder, and our goal is to get from that fuzzy-looking mess to the final stage that is identified as extraction, where we actually pull out the pieces within the water column that represent the targets that we're interested in, which, in our case, is red snapper.

We go through a filtering process to clean out noise and to try to eliminate scatters that don't represent fish, identify, through the detection process, where the fish exist in the water column, segment that, and then search for surrounding targets, and so, in the report, you will notice there is mention of a Sawada index, and that's used to determine where we can look at individual targets around groups, to help scale what ultimately will derive two important parameters, and those derived as targets strength and SV within each of these cells.

If you look at this image, you will see that there's this grid that's applied to the echogram, and so, in the report, those are described as cells, and those represent a spatial domain, and so, within each of those small domains, we get an estimate of the target strength and the SV, which allows us to derive density.

This is how those are derived, and so the first is target strength, and so, for those unfamiliar, target strength is an acoustic representation of the cross-section we're in, and so it scales in length, which is nice, and, based on some recent work,

which, of course, wasn't published by the time the study was done, but we used it to try to inform what we thought was a reality, and so this represents, on the right side, a target strength to length relationship for red snapper that we derived from some models that my lab has developed.

We understand, generally, the relationship between this acoustic backscatter intensity and the size of the fish, and so that provides us this development called target strength, and then the next one is volume backscatter, and volume backscatter represents a summation of all of the scatters in a volume, and so, basically, all of those little target strengths for each individual within a volume is summed up, and we can scale those two and derive an estimate of density.

What that's going to provide is an estimate of the number of fish per square meter within each of these cells, and that allows us to then go and take all of those cells and put them into the analytical framework to derive an overall estimate of density, including the estimate of variance that will go along with that.

That's the process. That's what happens, and now I'm going to sort of wrap up this piece by identifying some of the challenges that we have and talk about some things that are going forward, and not that it necessarily helps to inform this particular project, but it provides some guidance on where we think things may be improved.

Inherent challenges, as I mentioned, are the dead zone, and that is something that we're not going to get away from, and so we have to just recognize that that's a bias, and we're going to do the best we can, or we have done the best we can, to mitigate that, but, ultimately, we're likely underestimating, with acoustics, the distribution of fish associated, within about a meter or so, of the bottom, and that's why we can, at least in some cases, rely on the use of optics or video data to try to inform that and understand what we might be missing.

Next is species allocation, and so, as I mentioned before, this is a complex system, and it's quite speciose, and so picking out red snapper exclusively from the mix of species is challenging, and so we can make some progress with that, based on understanding the acoustic properties of any type of scatter and organism, and try to narrow that down a bit, but also using the video data to help understand the proportional abundance of our target, red snapper, versus other things helps to scale those estimates as well, which is what we did.

Then, lastly, there was some comments in the report about our direct comparison, at least in Florida, with the ROV and the acoustics and the distinct differences between the estimates abundance, with the acoustics being far less, and so we think that there’s some ways to improve that relationship, and I will we’ll touch on that just a little bit later on, but recognizing there is some differences in the sampling domain of these, and the acoustics -- That estimate of density was diluted a bit by a more expansive survey area, and, like I said, I think he’ll touch on that a little bit later on.

This is a challenge that we really have to deal with when thinking about acoustics, and so ways to improve this, and, of course, there are possibilities of improving or changing survey designs for particular areas, and we’re hoping to have additional ground-truthing information, and then also relying on acoustic modeling, where acoustic modeling informs us on what we should expect an organism to look like acoustically.

This is the one where I think we really have an opportunity to improve on in our region, because, for the most part, millions of scatters, outside of just a very small body of work, are really not well described, and so this offers opportunities, like I stated, to understand how these different organisms, and so, for example, a complex of species, can be identified acoustically, and we recognize that there is morphological and species-specific variability, and we can use those differences to help identify, potentially help identify, how to separate these, and this is where using multiple frequencies comes in, and so you can see here the thirty-eight kilohertz, the seventy, and the 120-kilohertz system.

They’re all looking at the same thing, but they provide slightly different pieces of information, and, if we look at the graph on the left side, we recognize that, for some organisms, those differences are dramatically different across those endpoints, and so this is the thirty-eight kilohertz, seventy kilohertz, and 120 kilohertz, for example, and this is where those endpoints would lapse.

At minimum, it allows us to remove noise, or remove information from scatters that we’re really not interested in, to really allow us to drill down and focus more on the fish, and then there’s some potential for using some of these newer approaches to look specifically at patterns in fish, and so I don’t want to dwell on this, but just recognize that, with these new broadband techniques, there is some potential, and we’re continuing to

explore this, of ways to try to really drill down, if you will, into the species-specific patterns of backscatter that might help us to inform our taxonomic classifications down the road.

We provide that with some additional information that we can get, or derive, from additional methods, like optics, and it might help us to, through a suite of new analytical approaches, really improve our ability to classify and discriminate among a community that's relatively complex, and that's about all I have.

Again, the goal was to provide a broad overview of what was done acoustically, for all the reasons that we're broadly adopting, and then I think there's going to be some slight differences that are described later on in the remainder of the talks, and I'm happy to talk through this, if anyone has additional questions, and so I will leave it at that, Dr. Powers.

CHAIRMAN POWERS: Thank you. Let me then open the floor to questions. Again, this is more an overview, and we'll get into other detail, I'm sure. Dr. Christman.

DR. CHRISTMAN: This question probably shows my ignorance of hydroacoustics, but are hydroacoustics, or at least some of the frequencies that you used, sensitive to flocculant or sediment loads or things like that, that would make it more difficult to interpret the results?

DR. BOSWELL: Generally, the response from those types of materials would be observed at higher frequencies, and so, the lower frequency that we are using, we tend to not have issues with them, and it's not that they can't be detected, necessarily, but getting rid of it is pretty straightforward.

DR. CHRISTMAN: Thank you.

CHAIRMAN POWERS: Thank you. All right. Any other questions at this point? Luiz Barbieri.

DR. BARBIERI: Thank you, Mr. Chairman. Kevin, a quick question that might be actually more appropriate for Will, when we get to his presentation, but I was wondering about the decision, in terms of study design, the decision of sampling allocation of different methods, but the report says that about 6 percent of the total sites sampled in Florida used the acoustics methodology as well, besides the ROV, and so can you explain that and how that decision was made? How was that arrived that, or is this something that I should ask Will later?

DR. BOSWELL: I'm not sure that is accurate, but I think Will would be probably the best one to respond to that question.

DR. BARBIERI: Okay. Got it. Thank you, Kevin.

CHAIRMAN POWERS: Will can respond to that in the context of his presentation, unless he wants to deal with it now. Okay. Any other questions?

DR. EGGLESTON: Dr. Powers, can I ask Kevin a couple of questions?

CHAIRMAN POWERS: Sure. Please do.

DR. EGGLESTON: Kevin, that was a very clear presentation. Thank you. One concern I have, for the discrepancy between the hydroacoustics and the ROV, for the calibration studies in Florida, was the algorithm that was used to convert the target strengths to volume, and so what I got from your presentation was basically it was the exact same software and algorithms that were used across all regions and strata, and is that correct?

DR. BOSWELL: Yes, sir. That's correct.

DR. EGGLESTON: Then, also, what you mentioned is that the discrepancy may be due to the fact that the hydroacoustics is simply sampling a larger area, and so you're just diluting the estimates of fish aggregation, and is that correct?

DR. BOSWELL: Yes, and so we spent substantial time yesterday actually trying to dig a little more into that, and I think -- Like I said, I think Will will touch on this a little bit later on, but, in restricting -- I can't remember which review requested this, but, in restricting the analysis of the acoustic data to the direct overlap of the ROV surveys, we got down to a nearly four-fold difference, versus nine, which is actually much more believable, and, again, I think the dilution effect is important, but, also, we have to keep in mind the limitations of the acoustics itself with respect to the dead zone, or our seafloor interaction, if you will. That's particularly important in complex habitats, because that can be greater.

DR. EGGLESTON: Kevin, can you repeat how you were able to refine it from a four -- Basically nine-times to four-times difference?

DR. BOSWELL: Sure. We, effectively, took the footprint of the

ROV and constrained our analysis and the acoustic data to just that area, and so, effectively, it was eliminating the much more extensive survey area.

DR. EGGLESTON: All right. Great. Thank you very much.

DR. BOSWELL: Yes, sir.

CHAIRMAN POWERS: Thank you. Benny Gallaway.

DR. GALLAWAY: In some of our studies, where we're studying red snapper on platforms, we have run companion mark-recapture studies along with our hydroacoustics, and we found them in very close agreement, and so I'm cognizant of the dead zone and the impacts that there can be, but we've had pretty good success with it, in terms of agreement with mark-recapture studies. Thank you.

CHAIRMAN POWERS: Thank you. All right. If there are no other questions at this point, I believe -- Dave Eggleston.

DR. EGGLESTON: I think that's just a delay, where I keep clicking on the hands-up, and it's finally showing my name, and so Kevin already answered my questions. Thank you.

CHAIRMAN POWERS: Just for future reference, when you click on it, your name doesn't automatically show up, and it's actually transcribed by somebody, and so keep that in mind, for all of us. All right. Thank you then. I believe, at this point, we are going to have Dr. Murawski present information about the uncharacterized bottom and pipeline. Am I correct?

DR. MURAWSKI: I think you are, Joe. Thanks.

CHAIRMAN POWERS: Okay. Great. Steve.

GULF-WIDE UNCHARACTERIZED BOTTOM AND PIPELINES

DR. MURAWSKI: Thanks very much, and I want to give a shoutout to Dr. Stunz. You know, he's been the ringleader of all of this, and I know what a full-time job is, and apparently he has three now, and it gives a new meaning to the whole term "herding cats", and I think Greg has done an outstanding job, and we all owe him a debt of gratitude, at least those of us on the project.

My role in this project, and our team, has been to use a towed video imaging system that we have called C-BASS to specifically

count red snapper over the pipeline network in the Gulf of Mexico and also to do more work on the so-called unconsolidated bottom habitats.

We also did some work on some of the natural habitats, particularly in the western Gulf of Mexico, which we actually have a lot of experience with in the eastern Gulf, and so we have a large team, and I'm mentioning Sara Grasty; Chad Lembke, who is the chief engineer; Matt Hommeyer, who is our acoustics expert; Alex Silverman, who is our software engineer; and others. There were lots of people involved in this particular aspect of it.

In terms of an overview of the presentation this morning, I want to give sort of a description and document the development of the C-BASS towed video imaging system, just because it's probably not familiar to most people. I'll talk a little bit about its operating characteristics and its capabilities, and I will talk a bit about sampling effort and the image counts during the Great Red Snapper Count, in particular the pipeline work that we undertook.

I want to talk about the calculations required to expand our images during the transect surveys to actual density estimates, and ultimately population estimates, and then expanding the density estimates to the sampling universe. Then I want to also make some comments on the use of randomized transect results and their interpretation, and a number of the independent reviewers discussed the issues of things like serial autocorrelation in transect surveys and others, and so kind of an overview not only of this system, but a little bit of the interpretation of the data.

I would say that Dr. Ahrens, when he comes on, he's going to talk in more depth about merging some of the data and some of the statistical methods that were used throughout the series, and so what is C-BASS?

It's an acronym for Camera-Based Assessment Survey System, and it was originally developed by the College of Marine Science and its Center of Ocean Technology from a 2012 grant that we had from the NMFS Advanced Sampling Technology Working Group, and then we also got later funding from the NMFS Untrawlable Habitat Strategic Initiative, the so-called UHSI.

After we developed the engineering on the system, we applied it in several subsequent studies, which included a very extensive set of testing that Dave Somerton, who used to be at NMFS in

Seattle, ran as the UHSI team lead, where we amassed different technologies to try to evaluate not only C-BASS, but other towed imaging systems and ROVs, relative to what we thought was the underlying population of animals in the viewing area.

After that, we used this extensively in a NFWF-funded -- This is a post-Deepwater-Horizon-funded project to map benthic habitats on the West Florida Shelf, and, in particular, do habitat-stratified population estimates of both reef fishes and sea turtles, and so sort of a mini Great Red Snapper Count, and we've got a number of publications that are out and pending on this one.

Then we specifically used this technology to count red snapper over the pipelines, of which we have some extensive experience on the one pipeline on the West Florida Shelf, and, in particular, over mud and sand bottoms and natural hardbottom habitats.

In using towed cameras, there are a number of challenges, as any group that has tried to do this can attest. We have this issue of attraction versus avoidance of fish to the camera system, and it's a difficult one to evaluate, and this is sort of the analog to the catchability problem for most fishing gears that we're familiar with. We've done some analyses internal to the system, and we've also looked at comparisons with acoustics and other things, and I will talk about that in a minute.

Visibility, the so-called detection probability, is not an overarching issue in the eastern Gulf, generally, and I will show you some videos that are generally clear enough to get by, and, around the Mississippi River outfall, it becomes a major issue, and so, to the extent that we've made counts in those areas, we have not adjusted them for detection probability, and so, as Greg said, we have a potential underestimate, due to that.

The calibration of the view to estimate density, clearly we can show you videos, but we have to actually estimate the area that is being swept, and I will talk a little bit about that. Obviously, as Greg said before, if we had a great habitat map, this would be a snap, but trying to infer habitat-stratified abundance, using any of these technologies, is difficult, and this so-called stacking of particularly red snapper, where you've got not only the near-bottom abundance that we can image with a camera, and this particular system flies generally between three and four meters above the bottom, but we have -- With red snapper, you have stacking over the height of the

camera, and so this emphasizes the importance of using the video and camera at the same time.

There are species identification issues, particularly in murky water. Red snapper is somewhat easy to identify, relative to other species identification problems, but, again, it's not perfect, and then our concept of operations, and that is scaling up use of a technology like this to population levels, is one that we spent a lot of time and effort looking at.

In terms of design features, this is a schematic of what the system looks like, and it's configured with six video cameras, four of them facing front and two of them to the side. We have two HD cameras, basically with overlapping fields of view, and so we can actually do stereo, which is really cool when you put the stereo glasses on and look at things, because they really pop out at you.

The system also has an altimeter, which is incredibly important, not only for trying to avoid smacking into the bottom, but also there are a series of measurements that we take in order to determine the field of view from the cameras, and so the altimeter, when we're flying the system, allows us to maintain our height above the bottom that is optimal.

It has a CTD, and so we can get some coincident environmental data, as well as a fluorometer and a turbidity meter. We have played around with a DIDSON forward-looking sonar, and, honestly, it hasn't proved its worth, in terms of, originally, we wanted to see if we were imaging animals that we didn't detect on the cameras, and, in most cases, we see the animals when they're in the field, and there was very few times, when we see the animals avoid the system, that we didn't image them.

It has an onboard computer and a high-capacity solid-state hard drive, so we can basically archive, in HD, all of the images from all six cameras, as well as all of the other sampling instruments that are onboard. It has a lead-detection system, and it tells us if we've got a problem.

One cool thing about this is it has a video relay to the surface, and so it uses the CTD towing cable, which includes basically a paired inside plus the steel jacket, and so we send the power down the CTD cable at 750 volts, and there's a power conversion inside the system, and it also will send up low-resolution images from any of the six cameras that we want, and so literally we can fly the system and see what the camera system is seeing in real time, and that really helps us for

collision avoidance and other things.

It has full-spectrum lights and dual green lasers, which are somewhat superfluous when you have stereo, but they've been helpful in trying to gauge the view window, and we have a disaster recovery system, which, if you look at the float on top, and we've lost it a couple of times, but we've recovered it both times. Then the system development includes a dashboard that integrates all of the video feeds and the environmental data and ship location, and I will show you that in a minute.

This is what the observer sees, and so the image you see here is one of the camera images, and, again, it's a low-resolution camera, but it also gives you all of the data on where the system is, and so, in particular, the depth of the water and the altimeter are really important, but it also gives you other environmental measurements, and then it has a false horizon, and so you get yaw, pitch, and roll, and a compass, so you understand what direction it's going. That Dunkin Donuts coffee is probably the most attribute of the system, particularly when we've got long transects that we're doing.

This is what the system looks like in operation, and so we paired this with mapping sonars. One of the things that we don't want to do is fly this system very close to the bottom in areas where we've never looked for obstructions, and so the concept of operations that we had for this particular study was we randomized the transects over existing pipelines in the western Gulf, and then what we would do is actually image those pipeline segments at night, using the mapping sonar, and so we actually had, in the morning, a multibeam map of the area, and so, if there was like a toppled oil rig, or some other obstruction, that we would know how to avoid it.

We can also see the water column sonar at the same time. In this case, it's an EK-60, and so, a lot of times, in the lower-left-hand corner, we would have to hop an obstruction like this, and so, if we're running a simultaneous map in ArcGIS, we can plot where the ship is and know that, for example, in another quarter-mile, we're going to have to hop over a fairly significant obstruction.

The control in this system is basically wire out, and so, if you're going at a standard speed, which is generally four knots, you have quite a bit of depth control on the thing, but you need to basically -- In order to hop an obstruction like this, you have to reel in enough wire to get over the top of it, and that has taken a little practice.

The two ships that we've used in this study are the Weatherbird II, which is a 115-foot research vessel, and the new William Hogarth, which is a seventy-eight-foot vessel, primarily used for the mapping-related work at this point.

I wanted to show you a little bit of the deployment of this system, and so you can see that -- This is sped up a little bit, but it handles pretty well on deck, particularly if the weather conditions are okay, and this is just some stuff on the West Florida carbonate reef shelf, and you can see these are some low-resolution analog cameras.

One of the things you will note is a lot of the fish really don't care, and, going into this, we weren't really sure whether we would see any fish at all, and most of the fish don't seem to have a strong negative reaction to the presence of the camera, which is important. This is a school of vermilion snapper, and it's probably one of the more reactive species that we actually imaged with the system.

One of the things that we seem to attract a lot of are dolphins, and it's probably because of the pinging of the altimeter, but, nevertheless, we seem to always have some pals, which can be a nuisance at some point, and this is the recovery of the system.

In terms of using the system, one of the issues we have, of course, is where is C-BASS relative to the ship, and that doesn't represent a problem as long as we're just imagining a linear feature and trying to figure out what the density of animals are, but, if we actually want to register both the water column sonar and the images together, then we need to figure the layback and then calculate the time delay, in order to sync up the multibeam sonar image, the water column sonar image, and the actual video image that we have, and the layback can be calculated by this equation that you see at the bottom here, and it's in the report as well.

I wanted to actually show you what it takes to actually link up the water column acoustics and the towed video, and so this video was shot over the -- This is the Gulf Stream pipeline off of Tampa, and so you see us going along. If you see this dotted line, that's where the camera actually is relative to the sonogram that you see, and so you can see that we're approaching something on the bottom that's a big structure and a whole lot of animals, and so, if you do the layback and synchronize the clocks, you can actually do this, and so you will see that, and this particular image is synced-up pretty well.

This is sort of the best of all worlds, because we're looking downward, and, if you look at the scale on the side, it's four meters above the bottom, and so it's at fifty-six meters, and so you can see this, and this is a valve cover along the pipeline, and then, all of a sudden, you see just an enormous number of fish. Most of those fish are actually above the pipeline, and so this emphasizes the importance of synchronizing the technologies.

This is a typical image of what red snapper might actually do, and Kevin has already interpreted some of the sonograms here. In this case, this was a school of barracuda that we were looking at, and they were sufficiently out of the dead zone that they didn't represent a particular problem, but animals that are in the dead zone, and that is the meter closest to the bottom, are going to represent a problem, but, nevertheless, by using the technologies together, we can sort this out.

One of the experiments that we did was with -- This is Matt Campbell of National Marine Fisheries Service and the UHSI group put down some cameras that were imaging on the bottom, basically high-resolution video cameras, along a transect, and we roped the transect together with this yellow poly line, so we could try to follow it with the towed video, and what you got were some really great images of how the orientation of the system is and then what the surrounding fish community was when the camera systems passed by.

One of the things that I wanted to note was the orientation of the C-BASS, and you can see it's a little stern heavy, and that's by design, because, if we're touching the bottom, all the expensive stuff is upfront, and so, if we're touching bottom, it's going to touch stern first, and so that means that the cameras have to be tilted at a greater angle in order to image downward and forward, and so that's sort of the design of the system.

Here's just some pictures of different animals. Again, if you've got good visibility, red snapper are relatively easy to discern, particularly by experienced video readers, but there are, of course, a number of species, and this is a source of uncertainty.

In terms of quantifying how fish react to the system, Sarah Grasty who was my graduate student and now works in this program, in her thesis, took those video images that you saw and characterized the reactions of various species as both strongly

and weakly positive, and that is they seem to be swimming towards the system, or as strongly or weakly negative, and that is there was a strong negative reaction, in terms of their flight, and then sort of neutral, where they seem to be actually transiting across, perpendicular, to the camera system, and so there's -- Of course, there's this issue about the unobserved reactors.

In particular, a lot of the imaging systems have difficulty with species like gag grouper, which seem to be particularly shy. We didn't image a lot of gag grouper, but we've seen them in there, but I would suspect that that's going to be a problem.

In terms of the various species, this is Sarah's work, in terms of her thesis, and the snapper species generally showed like a weak negative reaction, and that is, when they were in the field of image, eventually, if you got closer, they would sort of swim away. Some other species, like for example amberjack, they would have more of a strong positive reaction, and so amberjack are pretty easy to image. Other species, like lionfish, they were totally neutral to our camera systems. This generally supports, I think, what Will Patterson is going to talk about, in terms of his calibration studies with looking at attraction and repulsion.

The visibility spectrum is incredibly important for us, in terms of imaging animals with this system, and these are actual video snips from various places where we've tried to image, and they go from zero. Everything marginal and greater, we can actually count the fish, and you can see -- For example, in that marginal frame, you see the pipeline going through there, and then there's a whole spectrum of good to optimal. This needs to be counted for in terms of the potential biases of the method.

Estimating the area swept, this is why we employ engineers, and our engineers figured this out. I said before that the C-BASS is tilted to the stern, and so you have a number of measurements that you can take, and you know that the camera has an angle to the bottom, and we also used the green lasers to try to calibrate this, you know how many laser widths across you were actually looking at.

The altimeter is also above the bottom, et cetera, et cetera, and so you see a bunch of different angles and vectors. These equations are specified in the report. The bottom line is the Equation 5 gives you the width of the transect, based on all these measurements, and, of course, they assume that the animals are 100 percent detectable and that the range is not

significantly degraded by the turbidity of the water. We have tried to think about how to test that, and nobody wants us to pollute their swimming pool, and so that's kind of an unresolved issue for us.

I wanted to give you a short treatise on actual counting fish on pipelines. This is optimal, and this is a lot of vermilion snapper, and a few grouper, Goliath grouper and other things. This is why there is so much fishing effort on the Gulf Stream pipeline off of Tampa, and there's a lot of vermilion snapper in this particular scene, and so these are all countable. It takes a lot of work to count a particular scene, but one of the nice things you can see is that they are clearly visible.

Now, this is a much more challenging pipeline segment, and you can see the red flashes, and those are red snapper, and so, even in this particular scene, we know we're following a pipeline, and there's some amberjack mixed in with red snapper in this particular scene.

Again, you can see that they're somewhat reactive, but not really, and certainly not repulsed by the system, and this is more like what is on the West Florida Shelf, and then we'll get into a little bit more of what's it like in Mississippi.

You can see that the pipelines in the northern Gulf have these armored jackets on some of them, and, in particular, the armored jackets are there when you have one pipeline crossing another, and I suspect that this is to try to prevent damage to the pipelines when you've got the issue of trawling for shrimp, and you also see that not all pipelines are created equal, and you saw the six-inch pipeline that was there, versus the normal thirty-inch pipelines that we are used to, and pipeline width is an issue.

In terms of the actual work that we did, in terms of the target areas that we stratified, in order to do both the pipeline and the uncharacterized bottom, one of the datasets that we used was the combination of VMS and logbook datasets, which gave us some site-specific CPUE data, and so we had two major strata, depth strata, that we worked in, the thirty to 100-meter strata and the 100 to 200-meter strata.

The areas inside of thirty meters, by regulation from BOEM, all pipelines are supposed to be buried. Now, they aren't necessarily always buried, because of shifting sand or whatnot, but that was our initial assumption, in terms of the stratification that we're using. Also, inside of thirty meters

becomes incredibly murky water in there, and so it’s difficult for us to work, and not always, but certainly, during some times of the year, it becomes very challenging, and we also used some longline data from another project that we had, and so you can see that some of the areas that we were targeting -- The relative abundance of red snapper there, based on using this consistent gear, is very high.

I just note to you the incredible disparity in relative abundance across the U.S. and Mexico border, and, using the same technology, it’s very apparent, which actually is another issue, in terms of the unit stock hypothesis for the Gulf.

This is a subset of the pipelines that were within the thirty-meter to 200-meter strata, and so these are all the pipelines that are there, and there is something like -- Well, I think there’s about 67,000 kilometers of pipeline that are available, and so the idea would be to select a subset of these for random transects and direction. Again, this is sort of the stratification scheme, and it also highlights the reef bank HAPCs, where we have the natural hardbottom reefs there as well.

This also adds the other dimension of the areas that actually have been imaged with multibeam, and you can see they are very scarce on the continental shelf areas, particularly in the western Gulf, and even in the eastern Gulf, and, ironically, the deep water is where we actually have some of the best multibeam data, and that’s because BOEM did a project with the oil companies to utilize their information.

Our towed video survey methods are basically selecting transects and then off transects, and, for the unconsolidated bottom, we generally tried to do them in pairs, and so we would take a randomized start point, particularly for the unconsolidated bottom, and do the directional work there, and so we did a series of three cruises, one in April and May of 2018, and this is mostly in the eastern Gulf and off of Alabama and Mississippi.

We did a cruise in July of 2018, which is more up around the river and then to the western part of Louisiana, and then a third cruise in January of 2020, in order to complete the work.

Just in terms of densities, these are pipeline densities, and you can see these are the numbers of animals per square meter, and so there is quite a few on some of the pipelines, and these are a series of pipeline segments that were imaged off of Alabama, and here is some off of western Louisiana, and then the

last thing I want to emphasize is the C-BASS density estimates in the mud versus pipeline samples off of Texas.

Just as kind of a summary of the effort, we did 635 kilometers of pipeline, and we observed a total of 10,099 red snapper, and the average number of red snapper per square kilometer, and so we can do a square kilometer calculation, was about 3,000 on the pipelines, and, if you just used the number of red snapper per linear meter, it's about 0.017, and, again, the sampling universe for pipelines is about 67,000 kilometers.

If you compare that to the mud habitats that we observed, mostly simultaneously or near the pipelines, we observed about 150 kilometers of mud habitat. The total number of red snapper we imaged over those habitats was 255, and so the average red snapper density in the mud habitats, the unconsolidated habitats, versus the pipeline, is about a factor of ten, and that's carried over in the per-linear-meter calculations.

The sampling universe is what really blows up the total population size, and you've got nearly 160,000 square kilometers of mud habitat, and the way we actually analyzed these data were in fifteen-second bins, and so, basically, you have a count for every fifteen seconds, which is on the order of 100 meters, and so we had nearly 16,000 of those fifteen-second bins that we could resample for estimates of the standard deviation and CVs.

The pipeline analysis methods, this is explained in the report, and I think that Dr. Ahrens will maybe talk about this as well, but we had to stratify the population estimates on the pipeline for the different pipeline width categories, and we did an analysis there, and we used the georeference polyline data from BOEM to get the sampling universe.

We did some randomization of the transect data, in order to try to minimize the effect of spatial autocorrelation in the observed counts, and I know some of the independent reviewers had some ideas about the adequacy of that, and then, of course, the total population is an expansion from the density estimates to the total.

Just in terms of the red snapper abundance estimates for pipelines, obviously, Texas leads the -- Everything is bigger in Texas, I guess, and most of the pipeline fish were there, and the overall CVs, by state, are arranged from 22 to about 5 percent in Florida, and there's not a lot of red snapper on the pipeline in Florida, relative to the other areas, and the counts are very consistent.

In terms of some considerations in using the C-BASS data, we set the siting probability to one, despite some of these visibility challenges in some areas, and I suspect that, in some of those areas, we undercounted the number of red snapper that were there, although you can see that, even in murky water, you get that flash of red when they get close to the camera, but, nevertheless, I suspect we're underestimating there.

You have the issue of stacking above the height of the camera, which acoustics can help us resolve, and, the serial autocorrelation, we've dealt with that in an ad hoc way, and we can talk about that, and then, for red snapper, we set the attraction and repulsion to null, and that basically we're counting what was there and there was no cryptic biomass.

I wanted to just give you some sense of the notion of serial autocorrelation as it relates to these kinds of transect data, and so this is a place called the Southwest Florida Middle Grounds, and so it's a natural-bottom habitat, and so you can see the counts across the fifteen-second intervals, and so they show sort of a random distribution. Sometimes you'll get a few together, and so this is the autocorrelation function as it relates to lags of one, two, three, four fifteen-second intervals.

The critical thing is that it does not exceed statistical bounds of -- All of these are non-significant, right, and so this is a natural-bottom habitat, and this is another natural-bottom habitat, which is the Madison-Swanson area. Here, you can see that you have a series of correlations between things, and the one and two certainly make sense, that we are on a habitat, particularly in a fifteen-second clip, and you're likely to be having the next fifteen-second clip be similar.

With a lot of these kinds of data, you see these sort of harmonics that evolve here, and a lot of them are not significant, but, every once in a while, one of these will pop up, and there's no reason to believe that some of these are actually real, as opposed to just sampling variability, but then you see the pipeline data, right, and so these are the number of -- In this case, it's the number of species that we saw, and this is the sort of species richness along the pipeline, but so this is the total number of animals that we see along the pipeline, and this is the autocorrelation profile.

Basically, it means that, when you get into these long runs of animals, because it's basically a -- It's basically an oil-and-

gas rig on its side, right, and you're going to get these areas where you just have long runs of correlated things, and so I do think that we have some issues to resolve, in terms of understanding the effects of autocorrelation. In natural habitats, I don't think they're a very big problem. With that, I want to just recognize our team and then give back control to you all, and so I appreciate the time.

CHAIRMAN POWERS: Thank you. Let me open it up to questions. Let me start off then, or let me defer to Dr. Christman.

DR. CHRISTMAN: Thank you for the presentation. I just have a question about the bootstrapping. Could you describe in a little more detail how you did the bootstrapping?

DR. MURAWSKI: Tell you what. If Rob is next up, we'll let him discuss that, because he did it.

DR. CHRISTMAN: Okay. Thank you.

CHAIRMAN POWERS: All right. Kai.

DR. LORENZEN: I just had a question about the size selectivity of the C-BASS counting, and it was in the report that the average size of the fish on the uncharacterized bottom is a lot larger than anywhere else, and I was wondering to what extent that reflects sampling or whether your sense is that they actually are, on average, much larger. Thanks.

DR. MURAWSKI: You know, with a mobile system like this, we're somewhat challenged to measure things, and so we need a good imagine of them. Generally speaking, we don't try to image things much below like fifteen centimeters, and my recollection is that it's probably true that the average size of these animals on the unconsolidated sediment seems to be higher than the other areas, and perhaps this is related to the life history of the animals, but I would defer to others, like Jay Rooker, who have done more work on the unconsolidated substrates.

CHAIRMAN POWERS: Thank you. Luiz.

DR. BARBIERI: Thank you. Steve, thank you for the great introductory presentation. As you know, I am very familiar with the C-BASS, and I have been following this, and I think it's -- The development of this piece of equipment and program that you're putting in place is an impressive piece of equipment that can, obviously, accomplish a lot, in terms of improving our survey capabilities, regarding reef fishes, but I have a few

concerns with its application to this project that I would like to ask you a few questions about.

First of all is sample size, how you guys arrived at the sample sizes that were used, in particular like the sampling fraction that was used here, when you consider the area that's covered by the cruises and the total area of unconsolidated or uncharacterized habitat that you have out there and that that entire area is going to be used as an expansion factor for that total absolute abundance estimate.

My concern is that the sampling fraction is very, very low, like the proportion of areas sampled of the Gulf are miniscule compared to the total area that we're going to be expanding that number towards. Can you talk a little bit how you arrived at the sampling sizes and the sampling allocation for each one of the strata sampled?

DR. MURAWSKI: Thanks, Luiz, and I appreciate the comments and that particular question, and it's one, in retrospect, given the counts that we had on the pipelines over there, we probably should have actually flipped the allocation to much higher allocation on the unconsolidated bottom, at least for my piece of this, because of the expansion factor, as you said, and so a lot of this was sort of range finding, in terms of, originally, we thought, given our experience with the pipelines in the eastern Gulf, that, given the network over there, that there must be a lot more fish on pipelines, if you total them up, and so, in retrospect, I would say that we probably would want to reallocate -- If we have a fixed amount of time, that we reallocate to that.

Any time you do a swept area estimate, particularly a habitat-stratified one, the amount of sampling effort you're going to have, or in this case the amount of area swept versus the total area, is going to be a small fraction, and that's true for trawl surveys and anything else.

Again, the average width of the vision area is about ten meters, and so those are long, thin transects that you're going to do, and so what we did was we actually combined the unconsolidated density estimates from this survey with others, in terms of Jay's work as well, to try to inflate that sampling fraction a bit, but you're correct that there's a big leap of faith from the samples that you've got to a large area, and so that needs to be considered as kind of a cautionary adjustment to the data.

DR. BARBIERI: Thank you, Steve, and if I might follow-up with

one more, Mr. Chairman?

CHAIRMAN POWERS: Yes.

DR. BARBIERI: Steve, when you go from there, from the sampling fraction issue, the area coverage of the survey, I'm also concerned about how the estimation, actually, of the numbers was obtained, because you had a slide there, and I don't remember what slide number, but it talks -- It shows that the density of red snapper along the pipelines is about ten-times higher than over muddy, completely uncharacterized habitats, right, but the pipelines were sampled, actually, four-times more often than mud.

Did the estimation procedure take this into account? I'm thinking about the fact that this is really sort of like a cluster sampling, multistage, and then there would be some level of weighting of the estimate.

DR. MURAWSKI: To a certain extent, it was cluster sampling. You've got the transect itself, but, also, we more or less paired the unconsolidated transect with an adjacent pipeline segment, and so a more refined analysis could actually take that into account, but, again, we have sort of limited information, when you put it all together.

One of our limiting factors, of course, is that the cost to do this, on a daily sampling, is very high, and the ship alone costs $10,000 a day, and so we were only -- As I said before, we were only able to do three cruises in order to do this, and, particularly, when we're coming from the eastern part of the Gulf over to the west, I mean, that's a three-and-a-half-day sail just to get there, and so there's a lot of logistic problems that we probably could mitigate if we had a more distributed access to the technologies kinds of things, but that's sort of lessons learned from this project as well.

I suspect that, if in fact these are going to be more routine, that these are the kinds of things that would go into sort of a revised plan of operations and kinds of operations to do this.

DR. BARBIERI: Right. Thank you, Steve. Just to try and explain my concern here, I mean, I understand all the constraints, and, like other folks have mentioned, I applaud the team, and, I mean, this is an unprecedented level of work, and, of course, we are learning a lot about red snapper and about sampling reef fishes in the Gulf, and so all of this is moving the science forward, but, when I think about interpreting this

in the context of using that number, the estimated total number of red snapper, for management, I really have to take into account a lot of these sort of details and how I interpret that big number.

For example, looking at the proportion of the total numbers of red snapper estimated for each one of the four regions, and, if I run a regression, and I did, looking at the proportion of the total number of red snapper against -- As a function of the proportion of uncharacterized bottom in the area, I end up with a significant regression, with a R square of 0.96.

Basically, because those numbers, from those transects, were really expanded to those large areas of the Gulf, regions, like Florida, that had a much larger area of UCB ended up proportional, really, to the total number of red snapper that was estimated by the project, and it's hard for me to really understand how that would be, considering that we don't really have that much information about the amount of structure that we have in all those different areas.

DR. MURAWSKI: Luiz, it's one of the great ironies of all this work, is that there's so much emphasis, particularly with reef fish, on hardbottom habitat, because the work we did in the Elbow, for example, indicated that half the fish were on 4 percent of the area, and that's where the high densities are, but it's just a large fraction of area that is containing low densities, and so it almost argues that we ought to be counting the fish where they mostly aren't, in order to close that gap. I completely empathize with the point you're trying to make, and there is no free lunch here.

DR. BARBIERI: Right. Thank you, Steve.

CHAIRMAN POWERS: Thank you. Ryan, did you have an interjection?

MR. RINDONE: Yes, Dr. Powers, and I just wanted to ask Dr. Murawski to send his presentation.

DR. MURAWSKI: Yes, and my boss has already reminded me of that.

MR. RINDONE: Thank you, Sarah.

CHAIRMAN POWERS: Jason Adriance.

MR. ADRIANCE: Thank you, and thanks for the presentation, Steve, and I'm glad that Luiz segued into pipelines, and I just

had a question on the area, or the amount of pipeline, used in the table, and I apologize if I missed this in the report, but were there any pipelines of a certain size that were dropped from that, or does that include everything you all were aware of?

DR. MURAWSKI: I will have to double-check, but I think we dropped the smallest of the pipelines, the six-inch pipe and that kind of thing, and so we tried to trim that a little bit.

CHAIRMAN POWERS: Okay. Thank you. Kai Lorenzen.

DR. LORENZEN: Thank you. I'm just trying to sort of confirm a number, and I think, in your table, you have a number, of I think 225 red snapper observed on the uncharacterized bottom away from pipelines, and I can't remember whether that was the total or it was per mile, or if you could just clarify that.

DR. MURAWSKI: That was just the uncalibrated total, just to give you some sense of scale. We saw nearly 11,000 on the pipelines, and 255 of these animals on the unconsolidated, which it's an important observation, because, as you all well know better than I, there was this inate, long-term debate about whether there was any cryptic biomass on the unconsolidated sediments, and we were able to document that, and we have photographs of it, and so, from that point of view, the exercise generated some information, but, of course, very low density.

DR. LORENZEN: So is those 225 fish that were then expanded to seventy-million?

CHAIRMAN POWERS: Can you actually show that slide?

DR. MURAWSKI: Sure. I mean, again, this is just the total number of red snapper imaged over this, and so the important numbers are the red snapper per square kilometer over these habitats, and this is assuming the pipeline sample width of about ten meters, because you get animals on the pipeline and associated with it, as opposed to the mud habitats, which are basically the calculations that I explained to you, in terms of the geometry, and so there's a factor of ten difference in both of those metrics.

CHAIRMAN POWERS: Let me interject. I have a question. When you say mud habitats, you're referring to uncharacterized bottom?

DR. MURAWSKI: Correct.

CHAIRMAN POWERS: So you're using those terms equivalently?

DR. MURAWSKI: Yes, and a lot of this work was done over in the western Gulf, and the majority of it that we imaged was mud, but, yes, I mean, there's always other things in the habitat.

CHAIRMAN POWERS: No, and I'm just trying to make sure we're clear what we're talking about, because sometimes --

DR. MURAWSKI: That's the unconsolidated habitat, right.

CHAIRMAN POWERS: Which is the same thing as uncharacterized bottom?

DR. MURAWSKI: Yes.

CHAIRMAN POWERS: Okay. Thank you. Go ahead, Kai.

DR. MURAWSKI: There is some definition things in the report, as Greg said.

DR. LORENZEN: Right, but so it is -- I mean, it's those 255 fish, at very low density, that basically then is expanded to seventy-million in the total habitat, and is that right?

DR. MURAWSKI: Again, the uncharacterized or unconsolidated mud habitats, these data were combined with more extensive data that Jay and others pulled together as well, and so it wasn't just these 255 fish.

DR. LORENZEN: Thank you, Steve, for clarifying that.

CHAIRMAN POWERS: All right. For the uncharacterized bottom estimates, for example for Texas, the data that were used were C-BASS data exclusively, or did it include information from the Texas surveys?

DR. MURAWSKI: It included both.

CHAIRMAN POWERS: Okay. So, for --

DR. STUNZ: Can I jump in, when you get a minute? I'm having trouble, and I lost the icon to raise my hand, but I think I can shed some light on this portion of the discussion.

CHAIRMAN POWERS: Well, please do.

DR. STUNZ: We asked Kevin Boswell and Steve to go first because their method is integrated across the Gulf. In a few minutes, Rob Ahrens is going to talk about the design, and then we're going to get into what each region did, because that would answer some of the questions that are coming up about, well, yes, there is more additional data on uncharacterized bottom than just Steve collected, and it was collected in each region.

I think we're kind of getting ahead of ourselves a little bit. I mean, I'm certainly going to follow your lead of what you want to do, but, if we heard the design, and then the regional sampling, a lot of these questions, I think, would come out as we get through those presentations.

CHAIRMAN POWERS: Thank you, and that was some of my point early on, where I made the comment that there's a lot of individual strata that different things are going on, and we should be made clear what we're talking about. All right. Thank you. Next up is Dave Chagaris.

DR. CHAGARIS: I am curious what proportion of the C-BASS samples had low visibility, and how were they treated in the analysis and estimation?

DR. MURAWSKI: I would have to go back and actually look at that. I mean, each of the bins is scored, all 15,000, but I don't have that number. They were a very high proportion off of Louisiana, a low proportion off of Florida, and a modest proportion off of Texas, and I don't have any statistics. I can go back and get it for you, if you want, Dave.

DR. CHAGARIS: Okay. Well, then, when you did encounter a low-visibility site, how did you treat that sample? Was it tossed, or was it included, or was it imputed?

DR. MURAWSKI: Unless the visibility was zero, we included it, if we could recognize fish in the scene, and so that's one of the reasons why I listed, in one of the final slides, that, all things being equal, the estimates we have for pipelines are probably an underestimate.

DR. CHAGARIS: Thank you.

CHAIRMAN POWERS: Thank you. Clay Porch.

DR. PORCH: Thank you. Dr. Murawski, I am getting chatted by a few folks that are indicating they sent me a snapshot of the MMS regulations, and it says that pipelines installed -- Basically,

eight inch and over pipelines installed in depths less than 200 feet have to be buried, and I think I heard you say --

DR. MURAWSKI: I said thirty meters, which is -- You know, I mean, it's 180 feet.

DR. PORCH: Well, thirty meters is less than 100 feet, and so I'm wondering, if it's actually you have to bury it less than 200 feet, and that may change the expansion.

CHAIRMAN POWERS: I'm unclear about that one. I will have to get back to you on that.

DR. PORCH: All right. Thanks.

CHAIRMAN POWERS: I would remind people basically what Kai said, that the bigger issue is uncharacterized bottom, and that's 70 percent or 80 percent of the total estimate.

DR. EGGLESTON: Chairman Powers, apparently I'm struggling with the hands-up icon, and I've got it on the green arrow this whole time, and I'm not showing up, and so I'm going to jump in, if it's okay.

CHAIRMAN POWERS: If you're having trouble, then interrupt me whenever you feel the need. Go ahead.

DR. EGGLESTON: All right. Steve, thanks. I wish I had a C-BASS system in the Carolinas.

DR. MURAWSKI: We'll sell you one.

DR. EGGLESTON: But I have four questions, and one is just more of an operational question, and that is what is the -- I realize this might be vessel-dependent, but what was the sea state at which you basically had to call off a C-BASS tow operation because of inability to compare across time?

DR. MURAWSKI: Actually, the sea state limitation is really the deployment and recovery phase, and it's not so much actually when it's on the bottom, because you get a long catenary in the wire, and so it will surge a little bit, but, generally speaking, as long as the wave height is six feet and less, you can utilize the system.

DR. EGGLESTON: Okay. The next question is I had a lot of the same questions that Luiz Barbieri asked, and, yes, I mean, it looks like the sample effort in the uncharacterized bottom was

an order of magnitude lower than on the pipeline, and so, obviously, hindsight is 20/20, but it would have been great to flip that, now that we know how important that habitat is, and so that was some of my concerns, in terms of scaling up to absolute abundance, just because of, again, sort of the under-sampling relative to other strata.

DR. MURAWSKI: Certainly, if we knew then what we know now, certainly we would do that, but, remember, you have to wait until Rob's presentation about that area, because we did combine it with other estimates as well.

DR. EGGLESTON: Right. Then I was one of the reviewers, I guess, harping on the serial spatial autocorrelation, and so it was -- I know that you anticipated that, with respect to the pipelines, in terms of some of the post-processing, but I don't recall seeing the serial autocorrelation plots in the report for the hardbottom habitat, and so I think it would be helpful to include that in the revised version of the report.

DR. MURAWSKI: Yes, that's a good idea. We certainly should do that.

DR. EGGLESTON: The last comment is I was really intrigued with the fact that you can couple the multibeam sonar with the C-BASS video feed, and, in particular, when you showed that -- I guess you hit that pipeline that had some kind of valve cover on it, and you showed a lot of fish above that, and can you talk a little bit about how you -- If that was handled at all, with respect to the sort of fish aggregations that you know are there, but not picked up by the camera?

DR. MURAWSKI: So we didn't -- We collected EK-60 data along with the information that we collected on the pipelines from the video data, but, for this project, we didn't actually merge the two results, and it's just because of the logistics of doing all this relative to the project, but we also didn't see a lot of stacking up, particularly on the pipelines, except for places where you had some other structure, and so it was an issue, and it will be an issue, in terms of actually doing that, and I do think that some additional analysis of that would allow us to actually figure that out.

DR. EGGLESTON: Okay. Thank you.

CHAIRMAN POWERS: Thank you. Lee Anderson.

DR. ANDERSON: I hit the button by mistake.

CHAIRMAN POWERS: All right. Thank you. One other comment, and we're about ready to break for lunch, and I think this would be a convenient time, but one other comment/question/thought process, and, at the very beginning, when Greg made the presentation, there were a number of issues that would indicate that the red snapper estimate is an underestimate and that decisions were made to consciously accept that, but, as I looked at that, almost all, if not all, the issues were related to actually estimating the density, the actual sampling density, and I'm not so sure that one can say that in terms of how the actual sampling process goes, whether it's definitely -- Because of how the sampling is allocated and that sort of thing, whether it's definitely underestimated or overestimated or whatever. That is something that I think we should think about as we go along.

Again, this is lunchtime, for those of you in Eastern Daylight Time, and we are scheduled to have an hour's break for lunch, and I think we should do that then, and so, if there's no other comments, then we'll come back at 1:00 p.m. Eastern. Thank you.

(Whereupon, the meeting recessed for lunch on March 30, 2021.)

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March 30, 2021

TUESDAY AFTERNOON SESSION

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The Meeting of the Gulf of Mexico Fishery Management Council Standing and Special Reef Fish and Socioeconomic Scientific and Statistical Committees reconvened via webinar on Tuesday afternoon, March 30, 2021, and was called to order by Chairman Joe Powers.

CHAIRMAN POWERS: Good afternoon. I think we're ready to start again. Good afternoon for some of us anyway. On the agenda, we have Dr. Ahrens, who will be presenting the design framework. When he is ready, we can proceed with it.

OVERALL DESIGN FRAMEWORK

DR. ROB AHRENS: Good morning, Chair. Good morning, council, and good morning, reviewers. I will attempt to share my screen. I was asked to, in this part of the presentation, to talk about

the initial sampling design, and I think this was mainly to answer questions as to the nature of the random forest model and how it was used. I would like to acknowledge Dr. Zack Siders, who was integral in the development of the random forest model and some of this work.

For the initial strata determination, I will be talking about developing the sampling design for the uncharacterized bottom as well as the artificial structures, in terms of the determination of the strata for the random stratified sampling, allocation, as well as the preliminary estimates of the sample sizes required for the known hardbottom, and you will see the individual presentations, and Dr. Murawski informed us as to how the pipelines have been sampled, and so I will not -- I was not involved in the development of those.

For the uncharacterized bottom, as well as the artificial structures, strata were developed on a region, depth, and the probability of red snapper presence used as a surrogate for expected red snapper density, and this was -- The probability of presence was estimated using a random forest model that was trained on fishery-dependent and fishery-independent data sources, as well as some physical structure.

The basic sampling unit was derived from the BOEM bathymetry maps at three arc seconds, and so approximately ninety-meters-by-ninety-meters, and the strata location was determined to be the centroid of those three-arc-second areas. We combined, again, the fishery-dependent data and the physical characteristics into the random forest model, to classify the presence and the absence, and we developed a probability of presence map for the regions, and then that probability of presence was then classified as low, medium, or high, based on thresholds.

Here is the coverage of the data that went in, in terms of the fishery-independent and dependent data, and we had camera surveys, longlines, and vertical lines, and some of these were presence only, the details of which are in the report, and most were presence and absence data, and we utilized all the time periods of the datasets, which is in the table as well in the report.

In addition to that, some work by a former PhD student of mine, Nicholas Duchon-Barth, was utilized, where he developed a finer-scale catch per unit of effort, using VMS data, and that was also used as an information layer going into the analysis. Then we had these physical characteristics, and the distance to

vegetation and distance to hardbottom were derived from the USGS data, and then the mean salinity and the bottom temperatures were pulled from NOAA datasets.

When this information was combined into the random forest, we had a map of the probability of presence, with distance to hardbottom or distance to most structures as the most important variables for determining the probability of presence.

We had a very good model fit on the test and the training data, and then that was chunked up here, and I think, if we look at the specificity and sensitivity plot for the area under the curve, you can see the low, medium, and high, and that’s how things were chunked up, and these weren’t the thresholds that were used, and we used roughly 0.53 to define the low and 0.86 to define the high, and these were in order to maximize sensitivity and specificity in the datasets.

The result was the production of low, medium, and high classified habitat that could then be chunked up into depth strata, as well as by region, and then, to move on to the sample size requirements and allocation to there, we basically assumed we were going to approach this with a random stratified sample by region by depth.

In the classifications, we assumed that 90 percent of the forty-three million estimated red snapper from the stock assessment were going to be located within 10 percent of the habitat, and that gave us an estimated mean density to be expected. From that, we assumed a variance, and that variance was going to -- In the data, we were going to try and achieve it so that two-times the standard error was going to be 30 percent of the mean density for the confidence interval, and so not the percent standard error calculations that was decided on later in the work, but the basic numbers, or total number of samples required, then required estimates of variances from the various strata, and these variances were estimated from scientific research.

That’s some preliminary work that was done, and I will get into a little bit of the details, where we can take a look at that, if people want.

We assumed uniform costs across the areas, and the weights, of course, were dependent upon the actual physical size of each of the stratum, and, once we had the total number of samples to be allocated, those were divided to the various strata based on the expected variances up there.

In order to get estimates of the variances, using the historic scientific data, density estimates from the studies, as well as the associated coefficients of variation, were assumed to have been derived from fairly high quality bottom habitat and that those density estimates were likely in the 95th percentile, and those were assigned to the highest habitat quality from the random forest model. We then assumed 50 percent of those observed mean densities for the medium habitat, and 5 percent of those were assumed in the low-quality habitat, and we used the coefficient of variation of 150 percent on those means to develop the variances.

We will see that, given the samples that were collected, this was an underestimate of the coefficient of variation amongst the samples, and that certainly would come into play if we were to redesign this study.

The mean density estimates that came are region and depth specific, and, if none of that information was available, they were imputed from the nearest region-level estimates, and then, at a minimum, we allocated ten samples to each strata, because, given the size of the number of the strata out there, certainly the allocation routine would indicate that no samples were necessary from some of those areas, and so a minimum of ten samples were placed.

For artificial reefs, we took the unit of a reef as the sampling unit, and those were divided into five different categories, small, medium, large, extra-large, and a few jumbos out there, based on the weight of the material that was recorded in a database or the physical description, and then samples were, again, allocated assuming a stratified random sampling was going to be taken.

We used, again, previous scientific sampling programs to estimate the density of numbers that we would expect per structure and the associated coefficient of variation with that, and we assumed that 10 percent of the stock-assessment-estimated population was going to occur on the artificial structures, and, again, the allocation algorithm that we used was the same for the uncharacterized bottom, and, again, we set a minimum threshold of ten samples on the structures.

The final result that came out of that for each region was a map of sampling locations, and so here we have an example of Texas and the West Florida Shelf, where we had the circles indicating the location of sample sites in the low, medium, and high

habitats stratified by those qualities, as well as the depth for each region, as well as the associated artificial reefs that would need to be sampled in those regions.

Then those, of course, were selected at random from the full list of available ninety-meter-by-ninety-meter habitats, and then each region was provided a list of the centroids of those locations for sampling. That covers the sampling design, and I'm happy to take questions on that at this point.

CHAIRMAN POWERS: Thank you. Are there questions? Dr. Christman.

DR. CHRISTMAN: Thank you. I have a few questions. First, your random forest results are interesting. Did you do any comparison to your previous GAM modeling? I'm wondering if they tended to provide the same results, since you used different results in the GAM modeling.

DR. AHRENS: Thanks, Mary. The GAM modeling produced very different results, and that was -- Not a lot of time was spent on the GAM modeling to deal with some of the spatial issues, but there were some pretty strong biases that showed up in the predicted distributions from that in the southern extremes of both the Florida shelf and in Texas, and we, at the time -- In my lab, we were playing with random forest models, and we decided to give that a go, and it seemed to produce results that were much more in line with what was seen in the fishery-dependent and independent data, and, also, when we ran the random forest, we had that work that Nicholas Duchon-Barth -- That also was available at that time, to help inform that dataset.

DR. CHRISTMAN: My next question was you said that you used previous data for densities and then turned that into probability of presence, and is that correct? Not probability of presence, but presence/absence?

DR. AHRENS: Yes, and so we took -- For all the datasets, whether they were a presence-only dataset or a presence/absence data or an actual count dataset, we turned those into just zeroes and ones.

DR. CHRISTMAN: So I'm just curious. Not being a biologist, does density actually predict a higher probability of occurrence, given how patchy red snapper might be, since there was only -- Like, in the case of Florida, for example, only 6 percent of the observations even had red snapper in them, and so

I'm just curious whether the use of density, in and of itself -- I don't really know, and that's why I'm asking. I need a biologist to answer this.

DR. AHRENS: So my gut in that is that there are more snapper there, and only a certain portion of that snapper population are going to be responsive to any gear that's in the water, particularly hook gear, and a higher number of fish would result in a higher probability of having an encounter, because they are reactive to the gear.

DR. CHRISTMAN: That's for longlines, but not necessarily some of the other gear? That's just a question.

DR. AHRENS: Yes, it is certainly a question-mark.

DR. CHRISTMAN: I am just curious as to whether probability of presence is wrapped up with detectability issues.

DR. AHRENS: Yes, it would be. I can't see how you would tease those out.

DR. CHRISTMAN: Okay, and then, to discuss the sampling design that was proposed, although not actually implemented, there isn't a table anywhere, except in your report, in the appendix, that shows proposed sample sizes, but they don't appear to have been implemented, one, and, two, what I really want to know is, when you came up with a sample size, you had a population, a statistical population, for UCB, which was the grid cells, so that, when you were referring to N, you were referring to, little n, you were referring to how many grid cells should be selected for sampling, correct?

DR. AHRENS: That is correct, yes.

DR. CHRISTMAN: You were assuming, on top of it, that everything was going to be completely at random, which it wasn't. I mean, the different gears drive non-random data collection.

DR. AHRENS: Yes, that is correct, and certainly the realities of implementing the sampling design and the challenges at region levels were taken into consideration after the sampling design was initially proposed.

DR. CHRISTMAN: So what were the constraints then? Like those two figures that you showed for Texas and Florida, basically, and maybe it's just the size of the point, but it looked like the Texas shelf was going to be completely covered, and so I'm

just a little curious as to how many observations were you recommending, say for the UCB off of Texas?

DR. AHRENS: This was the initial kind of allocation for -- This is Texas up here, and -- I think I have them all there.

DR. CHRISTMAN: That’s close enough. They’re very low sample sizes.

DR. AHRENS: Yes. 624 would be the proposed number of samples for the Texas shelf area.

DR. CHRISTMAN: That’s specifically for UCB, or does that include the artificial reef?

DR. AHRENS: No, and that’s just --

DR. CHRISTMAN: You say natural habitat here on your figure, and it says natural habitat, and I thought you were referring to --

DR. AHRENS: Right, and so the language that was used has evolved, and so I -- When I am talking about natural bottom, I am talking about the unclassified, or unconsolidated, bottom, and so this is not the known hardbottom habitats, which were removed and treated differently, and so I am talking about the unclassified bottom.

DR. CHRISTMAN: You allocated -- Just out of curiosity, you said that you used estimated means and variances in your allocation scheme.

DR. AHRENS: Yes.

DR. CHRISTMAN: Did you feel comfortable those values were based on sufficient data that you actually had a good sense of what the average density was for the variance associated? You did say that the CV you proposed was too low.

DR. AHRENS: The CV that I proposed was actually on the high end of the observed CV for the data we had.

DR. CHRISTMAN: Was that based on standard deviation or standard arithmetic mean?

DR. AHRENS: That was standard deviation of the sample.

DR. CHRISTMAN: Of individual samples?

DR. AHRENS: Of individual samples that were taken. Now, the assumption was that these were probably cherry-picked sites with high density, and probably not a great deal of variation among them, which is why we chose the higher CV, but the reality, when you look at the actual data, is that CVs range from 200 to 1,000 percent, in some cases, depending on the strata, and so certainly this allocation and these sample sizes did not reflect the realities of the true variability that would exist in the data.

DR. CHRISTMAN: Okay, and then my final question relates to the artificial reefs. This is just a really basic question. Every single artificial reef was considered a sampling unit? I am thinking, for example, Bill Lindberg's artificial reefs up in the Bend there, and every single one of those set of four structures would be considered -- They were lined up along an isobath, and they would be considered as an individual artificial reef, or would the entire set of twenty be?

DR. AHRENS: No, and each unit would be considered a reef in this, though I will say the sampling frame evolved over time, and so, in the initial design, we really only used, or had access to, I should say, the artificial reefs that existed within a publicly-available NOAA database and some of Bill Lindberg's work and a few others here and there, and we didn't have the lock-and-keyed state-specific artificial reef dataset to use as a sampling frame, in this case.

DR. CHRISTMAN: So, when you provided this list to the different regions, it wasn't necessarily sampling from what they knew as the sampling frame.

DR. AHRENS: That is correct, yes.

DR. CHRISTMAN: They were sampling from a subset of it. Okay. I mean, I don't know that you could have done anything else, but I was just curious. Sorry that I took up too much time.

DR. AHRENS: No. Thank you for the questions. I expect that I will hear more from you.

DR. CHRISTMAN: You probably will.

CHAIRMAN POWERS: All right. Thank you. Kai.

DR. LORENZEN: Just a general question, and I wondered whether the fact that these were sampled with a wide range of different methodologies and whether there was, at the design stage,

consideration of the need to intercalibrate and to also perhaps estimate the variances associated with individual samples for those different methods. Thanks.

DR. AHRENS: Thanks, Kai. There was certainly discussion, and I think, at this stage of the development, the realities of what people were faced I don't think had reared its head quite as much, and I think there was much more optimism at the tractability of achieving this at the time. There was certainly also discussion, at the beginning, and throughout, about how one would quantify the measurement error that would exist on any given observation.

DR. LORENZEN: That's what I meant, and it's sort of interesting, and I think you can see how you would have done the design, and then people go away and do their studies and sort of lose track of those issues in the course of a big project. Thank you.

DR. AHRENS: Thanks.

CHAIRMAN POWERS: Thank you. Luiz Barbieri.

DR. BARBIERI: Thank you, Mr. Chairman. Thank you for this overview presentation, and it was very helpful, and I will be brief, because Mary already covered a lot of the questions that I had, but, just to confirm, I was looking at -- I'm sure that you don't have that in front of you, but there is a table, Table 6, on page 84 of the report, that is basically the bottom-line table, where all the numbers that were estimated -- The standard errors and the CVs are presented for each one of the regions by habitat, and then the bottom line number at the bottom, with a CV of 11 percent.

A couple of questions there, and one is, when I look at that table, I see a lot of the same pattern that I saw with the uncharacterized bottom sampling, where sampling was focused on high-density habitats, where the density of red snapper, in some cases, can be ten or fifteen or twenty-times the density in other habitats, but those habitats of high density can sometimes, when you look at that table, be sampled two, five, or up to twenty-times, more frequently than in the lower-density habitats, but I don't see there, in that table, anything that applies to the weighting factors that you mentioned to compensate for that sampling scheme.

To be aware of costs, you go to the sites where you know you're going to find them, but then, in your estimation procedure, you

downweight those areas, right, those sites, so you can generate a more realistic number for the whole frame that you have sampled. Do you know of any kind of weighting procedure that was applied in the estimation procedures for each one of the regions to generate those number?

DR. AHRENS: I can speak to the initial design, where the unconsolidated bottom, or unclassified bottom, the artificial structures, the sample sizes that were originally allocated -- They were treated as separate, and so, for the unclassified bottom, the goal was to achieve the estimate, plus or minus 30 percent at the time, which evolved to a PSE later, but the design was -- If you can still see my screen that I showed Dr. Christman, this design, in terms of these sample sizes, was intended to achieve that objective.

In terms of the known hardbottom habitats and the pipelines, those surveys weren't actually factored into this original design, in terms of the sample sizes that were required, and you can see, from this initial assignment, that the majority of the samples -- In fact, wherever you see a ten, it suggested that you did not need to take a sample, because of the magnitude, if you would like, of the specific strata in the unclassified bottom habitat. It required the majority of the samples to achieve the variance estimates that were specified, and so, in this case, the majority of the samples were going into that unconsolidated bottom habitat.

The samples that occurred on the known hardbottom habitat evolved as the study went on, and there were stakeholder needs that needed to be met, and then the pipeline habitat also evolved out of the work that Dr. Murawski and his team had been doing as a potentially important habitat for red snapper, and so those sample sizes happened as a result of those programs and weren't part of the original design. I don't know if that answered your question.

DR. BARBIERI: It did. Thank you so much, Rob.

CHAIRMAN POWERS: Dr. Eggleston.

DR. EGGLESTON: Kai asked it, and he asked it more eloquently than I, and I was curious about any design that would incorporate potential gear detectability, and so you've already answered that question, and so I'm just kind of curious then. Given what we know, how would you redesign the survey, and, in particular, I was interested in how you would reevaluate, potentially, the random forest model and the probability of

occurrences.

DR. AHRENS: Those are fantastic questions. Would I revisit the random forest model as a way of classifying the habitat? I think the -- Certainly, given the samples that have been collected, it gives us an opportunity to ground-truth the random forest model and its ability to classify the habitat, and we've done some poking around here and there, but nothing -- Dr. Siders is potentially going to look at that more in detail.

There is a general pattern that the classification of higher to lower probability in the random forest model does line up with the densities that were observed, with variation around it, and so it did a somewhat okay job of helping to define strata when no information, or very little information, was available to do that.

I think there's some work that could be done there to ground-truth and improve the model, but it certainly was a convenient way to come up with that third layer of stratification, to try and reel in the variance of the estimate, and so, given what we know now, how would I redesign the sampling program?

I think we still are uncertain as to the measurement error that exists as a function of the gears used, and more work needs to be done in that area to more fully understand the added variance that needs to exist on the individual samples. We could guess at this point, and certainly we could develop a simulation where those observations were fuzzy, and look at how that affected the overall estimate and how additional samples would be required to rein-in the variance of the final estimate.

Then, given the nature of the gears and how they are utilized, we're not randomly sampling, particularly with the towed gears, and it's probably more appropriate to take a look at a systematic cluster-type approach, where we take a closer look at the potential autocorrelation that exists, although, admittedly, it does not seem to be statistically strong in the unclassified bottom habitats, but to recognize that, in a number of the locations, in the Texas and Louisiana area in particular, that we would be dealing with more of a systematic clustered approach than a random stratified approach, which really was -- The West Florida Shelf and the north Florida area, it could be successfully implemented there.

I would also say that certainly, particularly for Florida, given the variation that existed in the samples, a much larger sample size would be needed for that region, and so I think, given the

realities of the gear that exist, you would have to incorporate those into a redevelopment of a sampling program.

DR. EGGLESTON: That's great. Thank you very much.

CHAIRMAN POWERS: Thank you. Dr. Christman.

DR. CHRISTMAN: Actually, I realized -- When you were talking to Luiz, you were commenting about the sample sizes having to be large in some areas, and what I am curious about is, as we all know, count data, generally higher counts mean higher variances, Poisson or negative binomial or whatever you want to call it, which would imply that you would be oversampling high-density areas at the dearth of low-density areas. Do you think that the habitat and general stratification structure was sufficient to remove that, or is there still some artifact of that showing up in the sample sizes?

The whole idea behind stratification is to put like with like, and so, if you can get everybody who is high density together, then you remove some of that natural variation, and it's the UCB that's such a problem, I think.

DR. AHRENS: Yes, and that was the hope of using the random forest model, to try and assist with that. You know, the medium and low-probability classified random forest habitats are the majority of the area out there, and those certainly would come out in the weights in the allocation.

So there would be an order of magnitude difference in the mean densities that were assigned in the original design, and so that would result in certainly an order of magnitude difference in the variances, and then you would have an order of magnitude difference in the weights, and so my guess is they kind of cancelled each other out, to some degree, but I would have to, certainly, do more digging to look at the actual relative effect of each of those.

DR. CHRISTMAN: Then another question I had related to the document that you provided in the appendix is your very last statement says that you did a validation by checking the Florida boundaries for low, medium, and high, and the boundaries went from 0.33 and 0.77 to like 0.93 and 0.99, or 0.98, which implies, to me, that there's either regional effects, and it shouldn't have been assumed that red snapper respond to these variables in the same way everywhere, or there is something else going on that the random forest model is not catching, and can you address that?

DR. AHRENS: I think the random forest model is certainly not catching the possibility that any given sampling point is going to have high densities on it, and we saw that in some of the Florida data, as you pointed out, where you could, by chance, land on a habitat that had just a lot of red snapper in it, and so you're really probably looking at a negative binomial-type distribution, in terms of the expected counts that you would get.

I don't know how random forest classifications relate well to assumed probability distributions within the data, and I haven't done a lot of work in that area, nor a lot of reading in that area, and so my guess is you could potentially turn it to -- Instead of a classifying presence-absence, you could turn it into a classification of category, and I don't know if that would -- If the variation that we saw occasionally in Florida would show itself in some of the data that's available, if that makes sense, and so I don't quite know the relationship to the random forest and the output and the underlying true distribution of the data.

DR. CHRISTMAN: All right. Thank you.

CHAIRMAN POWERS: Thank you. Harry Blanchet.

MR. BLANCHET: Thank you. I have a very simple question. You have a column in this table that we're looking at of number of samples, and the numbers of samples on the uncharacterized bottom seems to be very big. Can you explain what that number actually represents?

DR. AHRENS: For Texas and Louisiana, that's where those numbers are big, and that is a function of the towed systems and the chunking of that data. Those data come from the C-BASS work that was done, as well as the acoustic systems that Jay Rooker put out, and, the data that I received, at the time, the PIs were comfortable with having it assumed as a -- That what I was getting was a random sample, and so I'm not sure -- In the case of the C-BASS data, it's the raw data.

The other data was subsetted, to some degree, and it's not 100 percent clear to me exactly how it was subsetted, but that is why those counts in those regions are so high. Whether that is a true reflection of the sample size, given the nature, or how that would play out if you took and accounted for across-trawl, or across-tow, variance, as well as within-two variance, in terms of the actual variance estimate, would have to be

reassessed, assuming a different sampling design, like a systematic cluster or just even a basic cluster, where you had some sub-sampling at the two level.

MR. BLANCHET: So for Texas, for instance, the 6,400, that is the number of ten-second, or whatever the duration was, of those video clips that they looked at?

DR. AHRENS: It's about 3,000 C-BASS, and those would be fifteen-second chunks that exist in there, and Dr. Murawski showed that, for the unconsolidated bottom, there certainly wasn't a great deal or a lot of correlation in there, and one would argue there is certainly, very likely, a pseudoreplication issue, and then it's still not 100 percent clear to me exactly how the other Texas samples were sub-sampled under the broader dataset.

CHAIRMAN POWERS: This maybe is a question for Steve Murawski, but, those fifteen-second chunks, are they considered random observations?

DR. AHRENS: They were certainly treated as random observations.

CHAIRMAN POWERS: So, if you used thirty-second chunks, then the sample size would be half of that? That seems like an arbitrary decision, if you're considering it as a weighting, in terms of defining the precision of the standard deviation.

DR. AHRENS: It certainly has a noticeable impact on the 11 percent PSE that is shown at the end, yes.

CHAIRMAN POWERS: Thank you. Dave Eggleston.

DR. EGGLESTON: Thank you. Rob, obviously, a lot of interest in your design, and we're learning a lot from it. I wanted to go back, and I thought Luiz Barbieri made a really good comment, in terms of I also used Table 6 quite a bit in my external review, and Luiz made the comment about potentially weighting those values, and I didn't hear everything you said, because I was writing things down, but I thought it was based on the proportion of an area sampled, and so I've got two questions. Is that -- Would that be an appropriate thing to do, is weight these estimates based on the proportion of the area sampled?

DR. AHRENS: Are you talking just for the unconsolidated bottom, or are you talking across --

DR. EGGLESTON: I'm talking about all. Like, for example, when

I look at Table 6, I could almost envision another, at least maybe three columns, that would be weighted values of number of fish, variance, and coefficient of variation.

DR. AHRENS: So, for the pipelines, the artificial structures, the known hardbottom, and the unconsolidated bottom, they were all treated separately, and so it wasn't one kind of unified sampling program, in some big stratified sense, across all those habitat types, and they were literally -- The estimate that you're getting for pipelines is -- Given the way the data was treated for pipelines, that is the estimate that exists on pipelines.

If you wanted to come across -- I guess, if you wanted to come up with considering all habitat types, and come up with some grand mean density estimate per unit area, then you could weight those by the variance of each of those estimates, if you wanted, and their habitat size, but, the way this was conducted, they were considered separately, and so the estimate was done separately.

DR. EGGLESTON: Rob, one reason I bring that up is, in Steve's presentation, he -- Basically, with the pipelines, they sampled 635 square kilometers, and the pipeline sort of footprint, if you will, was 67,422 square kilometers, and then he indicated that the uncharacterized bottom that was sampled was 151 square kilometers, and, according to Table 6, the uncharacterized bottom area in Texas shows 57,535, but I thought Steve showed 159,915.

When I calculated out the sort of proportional area sampled, it was an order of magnitude lower in the uncharacterized bottom, and so that's why I thought, when you have such a large difference in proportion of an area sampled, that comment by Luiz made a lot of sense to me, in terms of allowing sort of another sort of independent perspective on these values.

DR. AHRENS: Yes, and, again, I think, if you were going to treat those habitats on a per-area basis, you could do that, and then you would get into arguments of, well, how wide should the swath around pipelines be considered the habitat unit, and that is the pipeline, and I'm not sure that I would do that in a sampling program, and I think I would still leave them as separate entities and get estimates separately for them.

By virtue of the magnitude of habitat that exists in the unconsolidated bottom, and what we know now about the variability that exists between the samples, certainly a larger

sample size would be required, and here we have a much larger sample size than what was prescribed, based on the initial design for -- As a result of the towed systems, and there are certainly further discussions that will be needed on how to more appropriately treat that data, in terms of estimating the overall variance in the final estimate, and so it really becomes the variance weighting.

DR. EGGLESTON: Exactly, and it just seems like there is this -- I don't know if it's a typo or what on Table 6 for the Texas uncharacterized bottom, and so it might be a good idea to kind of go back through and maybe double or triple-check the values on that table. Thank you.

DR. AHRENS: Yes. That will be done. Thank you.

CHAIRMAN POWERS: Thank you. Mary.

DR. CHRISTMAN: Where do I start? I totally agree with Rob that, in fact, the number of samples on the uncharacterized bottom is an egregious overestimate, and they should not have been treated as independent observations, but, secondarily to that, I agree with Rob as well that they should not have combined the C-BASS and the hydroacoustics data, but instead it should have been two separate estimates for the same area that could then be combined. In other words, they are two independent observations on the same stratum, and they should not have been combined a priori and treated as though they are the same information, because they are very different systems for sampling. Then there was one more point, which, of course, I can't remember. Okay. That's enough for now.

DR. AHRENS: Mary, in case it pops back into your head, I will say that, for the Texas area, the deeper habitat was sampled mainly by C-BASS, and the shallow and intermediate depths -- There is a bit of overlap there in the intermediate depths, but it was the other, the hydroacoustic system, for those.

DR. CHRISTMAN: But they didn't have the same stratification, right? C-BASS had thirty to 100 and then 100 to 200, I think.

DR. AHRENS: Yes, but those -- Again, all samples were, given their geolocation, where then dropped onto the random forest classification.

DR. CHRISTMAN: Right. There was further stratification. I'm sorry.

DR. AHRENS: Post hoc. We ignored -- They were dropped back onto the original stratification post-hoc.

DR. CHRISTMAN: So is that true for everything or just for C-BASS?

DR. AHRENS: That's true for all unconsolidated bottom habitats.

DR. CHRISTMAN: So are the random forest strata -- Were post hoc applied, and so these are a posteriori post-strata?

DR. AHRENS: Yes.

DR. CHRISTMAN: Okay. They were not applied based on what people were seeing, I hope.

DR. AHRENS: Not at all, no. They were not. Given the habitat rankings that people observed and classified, those were not used, and it was dropped back onto the original design, in terms of the strata.

DR. CHRISTMAN: Okay. All right. Thanks.

CHAIRMAN POWERS: Thank you. Dave Chagaris.

DR. CHAGARIS: Thank you. I have a question about -- My question is about the area estimates for the different habitat types, specifically the natural bottom, and I'm thinking mostly off of Florida, where you used a dbSEABED database and estimated the hardbottom area based off of 1 percent rock classification, and that seemed to result in this big, continuous expanse of hardbottom off of the southwest coast of Florida.

For the purposes of determining the area of natural bottom, was that treated as a continuous expanse, because I'm wondering if, in reality, that natural hardbottom is actually a patchwork of natural hardbottom and uncharacterized bottom, and, if so, it could potentially inflate the area of hardbottom that goes into the expansion of the estimate.

DR. AHRENS: Right. For the dbSEABED information, that was only used to calculate the distance to hardbottom that went into the random forest model. We made the assumption that the high-probability habitat in the random forest classification was, or could be, called hardbottom, if you would, in terms of coming up with an estimate of the numbers of individuals essentially on hardbottom in west Florida, and so we didn't use that hardbottom dbSEABED information to say this is the known hardbottom in

Florida, and we didn't do that calculation for Florida. That was only done for Texas, Louisiana, and Mississippi/Alabama, where they have other programs that have defined the actual physical expanse of known hardbottom.

DR. CHAGARIS: Okay, and so that area of 22,858 is --

DR. AHRENS: It's based on random forest classification of high probability, and it may have not -- That validation hasn't been done.

DR. CHAGARIS: Okay. All right. Thank you.

CHAIRMAN POWERS: Thank you. Jay Rooker.

DR. JAY ROOKER: Just quickly, to clarify, and I see Steve on here as well, and so he may attempt to chime in as well, but the 6,435 for the uncharacterized bottom in Texas -- Just to let you know, ours was done a little bit different than Steve's. Steve's may have been fifteen-second pieces, and we had approximately 3,500 sampling units, or cells, in Texas, and all of those were from the ten to 100 meters, and Steve's group, in Texas, I think primarily did the deeper zone, beyond 100 meters. What we did is we provided those 3,500 cells to Rob and Lynne, again, with that heads-up on spatial autocorrelation, but we gave them all 3,500 cells to work from. I will turn it back over to you, Rob.

DR. AHRENS: Thanks, Jay. I appreciate that.

CHAIRMAN POWERS: Thank you. Bob Gill.

MR. GILL: Thank you, Mr. Chairman. In terms of the number of samples discussion we're having, and clearly Texas has the bulk, but, if you look down at the pipeline section, there is only twenty-seven samples, and yet, if my memory is correct, the Steve Murawski presentation showed a lot of pipeline samples, for example in Texas, but, looking at the fifteen-second sampling rate, why just twenty-seven samples, and are the Texas ones, for example, incorporated in the UCB section of the 6,435 number?

DR. AHRENS: Great, and so, for the pipelines, that is -- The unit there, in terms of the sample size, is the pipeline and not the individual video count. The densities for those were calculated by subsetting the data with the random -- The first 100 samples, a random starting point was taken, and then every fortieth sample was used to calculate the mean, and then that

was bootstrapped to get an estimate of the standard error, or the variance, of the mean, and the sample size that was chosen for the degrees of freedom was the pipeline.

MR. GILL: So the 6,435 does not include any pipeline sampling at all?

DR. AHRENS: No. Those were the 3,000 C-BASS samples, the roughly 3,000 C-BASS samples, that occurred in Texas, were the mud bottom, as designated, and not pipeline.

MR. GILL: Thank you.

CHAIRMAN POWERS: Thank you. Steve Murawski.

DR. MURAWSKI: Thanks. So a lot of back-and-forth there, and I think mostly we got it cleared up. We did supply the 15,618 fifteen-second bins, and I think Rob has described the calculations based on the bootstrapping method that was used to basically calculate the means and the densities from that.

A couple of other small items, and there was a back-and-forth about the margin of the sampling, and it’s true, actually, that the MMS regulations, early on, specified a 200-foot and shallower burying of the pipelines, and it’s not universal. The reason we cut our sampling regime off at thirty meters was primarily because other parts of the sampling procedures were doing more of the inshore work, as Jay just said, and so there was no reason to overlap those, and so it was a combination of where is the pipeline buried and also minimizing the overlap between sampling procedures that we’re doing, and so it’s a combination of both.

There was also a little bit of confusion, I think, about that short table that I put together, in terms of the sampling universes. The pipeline universe, I mean, you can do it two ways, and the universe of 67,000 is linear kilometers, right, and, if you multiply that by ten meters, and then you’ve got a square kilometer metric, whereas the mud habitats are based on the sampling universe of 160,000 square kilometers, and so there’s a unit difference there to take account of. Thanks.

CHAIRMAN POWERS: Thank you.

DR. CHRISTMAN: Rob, one last question. You were provided with the pipeline data to do the bootstrapping?

DR. AHRENS: Yes.

DR. CHRISTMAN: Is what Steve just said? So you bootstrapped the pipeline samples and not individual observations within the pipeline transects.

DR. AHRENS: I subsetted each transect every fortieth and then calculated the mean from that and then randomly assigned a start point, again subsetted every fortieth, and calculated a mean from that.

DR. MURAWSKI: So he used the entire 15,000 fifteen-second bins and subsetted from that.

DR. CHRISTMAN: But what I'm asking is this is a two-stage cluster sample, and so you got the second-stage variance, and what about the first-stage variance, which is the selection of pipelines, the sample size of twenty-seven pipelines? Did you randomly select those and then randomly sub-select?

DR. AHRENS: Yes, and that first-stage clustering across-pipeline variance was not factored in, and it was not treated as a two-stage cluster.

DR. CHRISTMAN: Okay. Thank you.

CHAIRMAN POWERS: Thank you. I think we'll probably have -- As the day and tomorrow goes on, we may be revisiting some of these things, but, at this stage, I think it would be good to move on and get into some of the details about the regional information, and next on the agenda is Dr. Patterson, who will be talking about Florida and the Florida methods and so on, and so let me transfer the floor to Will.

REGIONAL SAMPLING FRAMEWORK
FLORIDA

DR. PATTERSON: Thanks, Joe. I'm going to talk about the Florida sampling and attempts to assess various biases, potential biases, in our approach, and then I will talk briefly about some results, saving the bulk of that for Rob's wrap-up, as far as the estimation.

Our group in Florida included myself; Rob, who just spoke; Steve Garner, who was a post-doc; and Joe Tarnecki, who is a research biologist working with my group. Steve and Joe led the sampling teams in the field and spent lots of time on the back of boats in the summer and fall of 2018 and then the late spring and early summer of 2019.

Miaya Glabach is my lab manager, and she coordinated a lot of trips, as well as sampling supplies, et cetera. Vince Lecours is a geospatial scientist who is in our department, and he was instrumental in some of the early mapping exercises to do with the sampling, and then Rob introduced Zach Siders at the beginning of his talk. Zach was a post-doc working with Rob on spatial analysis and sample design.

My talk is divided into three sections, and we can either take breaks between each of these and have questions or make it all the way to the end, and, Joe, I will, obviously, leave that up to you. I can pause after I finish the first section, and, if you want me to continue, I can, or I can stop there, and we can have some Q&A before moving on.

These three sections involve, first, the mobile gears utilized in the study and some commentary about that, and then talking about some behavioral experiments we did to estimate red snapper reaction to our various gears, and a real focus on the ROV gear that we used in the eastern Gulf. Next, I'm going to talk about ROV and hydroacoustic sampling on the Florida Gulf shelf, and Kevin Boswell talked a bit about the hydroacoustics methodology, but I will talk about the spatial distribution samples and the degree of overlap between ROV and the sonar work.

Lastly, a bit of other information about how we attempted to examine other potential sources of bias in the red snapper counts, and so everything I'm going to cover today is actually included in the report, either as text or as an appendix.

I do have a couple of additional figures that I will point out, things that Kevin Boswell and I have been kicking back and forth the last couple of days, things that we had considered before, but the early reports that we got from Dr. Cadrin and Dr. Christman sort of sparked some ideas, and so I'll talk about some of that, with respect to ROV and hydroacoustic comparison.

Rob just showed the initial design from the random forest model, and you can see, in Florida, quite a distribution of samples. There were 749 stations selected for what's been referred to as uncharacterized bottom, and our sampling team has referred to this simply as just natural bottom.

We don't have great maps in Florida, and we know where some natural hardbottom exist, in certain particularly high-relief areas, and FWC has done a nice job in recent years of adding to the universe of mapping there, and so has Steve Murawski's

group, with their NFWF funding, but, because we couldn't stratify based on habitat, we just considered this to be all unclassified bottom.

In Florida, we followed this design explicitly, as far as the natural reefs, and the yellow stars that you see here, relative to the size of the structures, these were selected in the random forest model, and you can see there's only a handful of those in Florida waters. With other work we were doing in the Panhandle, in particular, but also into the Big Bend, we subsidized this quite a bit with some more artificial reef samples, and so we ended up having sixty-five artificial reef samples in our work.

Among the various groups in the study, we have ROVS that were used for different approaches to sampling, and that's in the top-left, and this is the small video array ROV that we utilize in my laboratory, and you can see, on the front of it, two GoPro camera cases, and this is a stereo camera rig that we developed for this project, and I will talk about that toward the end.

These ROVs are quite small, and there are some larger ROVs used in different regions, and these ROVs are basically twenty centimeters high and thirty centimeters wide and forty centimeters long, and they weigh about six kilograms, and so they're quite nimble, and there's a tether that goes to the surface to control them.

On the bottom-left, this is the sonar rig from Kevin Boswell's group that we had on many of our cruises, and then, on the right-hand side, we have the two towed camera vehicles that were used by members of the team, and so, in Texas, the TERAS sled, and then, on the bottom-right, the C-BASS that Steve spent some time talking about this morning.

With all of these gears, there have been questions and discussion about different levels of bias, whether it's detectability or avoidance or attraction or selectivity issues, and, in Florida, having done quite a bit of sampling there through the years, we perceived the visibility was going to be pretty high in this study, and we didn't foresee there was going to be much of an issue with visibility, and, also, we knew that red snapper that would be on the habitat sampled were going to be age-two-plus fish, and they start recruiting to a hardbottom habitat and away from more low-profile structures, like oyster reefs and sponge and soft coral habitats, when they're about 250 millimeters.

We thought there would be a low incidence of younger fish where

we were, and the bigger fish are quite conspicuous, and they don't tend to hide under ledges, like some species do, and they're not skittish, like Steve Murawski was describing of gag earlier, nor do they have a reputation of chasing the camera, like an amberjack, or perhaps a gray triggerfish, does.

We weren't too concerned, at least in Florida, about detectability, and, in fact, we assumed it was 100 percent, and I will show you some images to form sort of a basis of that assumption. However, we had never really looked at the issue of attraction or avoidance.

Steve presented some information from Sarah Grasty's work with the C-BASS, where, obviously, they had paid attention to those issues and tried to quantify it, but we took the opportunity here, because we knew in this study it was going to be particularly important, since we weren't after a relative measure of abundance, but a true measure of abundance, to actually examine that.

Because we were working with the ROV, in that respect, we also used the acoustic sled from Kevin Boswell's group, and we also borrowed a TERAS from Jim Cowan's group at LSU, to tow that through our system as well.

The objective of this work was to test the behavioral reaction of red snapper to mobile sampling gears utilized in the Great Red Snapper Count and to assess the potential, we should say biases -- To test the potential for biased fish counts in density estimates, and so, on the bottom-right, you can see two stainless-steel stands, or aluminum stands. On the left, this is a sonar that Kevin Boswell's group worked with, and you can see a computer and battery pack beneath the sonar, and the sonar we shot sideways at artificial reefs.

You can see the sediment here is open, sandy substrate, but both of these gears are aimed toward a concrete module artificial reef. The rig on the right is a stereo camera system, actually a paired stereo camera system, one aimed slightly upward and one aimed parallel to the plain of the reef, and the distance between the cameras here is 750 millimeters.

We worked quite a bit with Matt Campbell and other folks at the NMFS Laboratory in Pascagoula, who had quite a bit of work done with the stereo camera rigs and the large study, multiregional study, that Steve Murawski mentioned earlier, to examine how fish react to these mobile fishing gears. Matt was a key component of those studies, and they published a paper, Somerton

et al., for vermilion snapper in the Gulf, and so we worked with Matt and his team on this research, and we definitely took advantage of their expertise.

On the left-hand side of this figure, you can see, at the top, there’s a map that shows the northeastern Gulf of Mexico, and then the inset at the bottom is -- This was an artificial reef permit zone, and the numbered circles that you see -- These were seventy Vemco VR2Tx receivers that we deployed in the system, and so this basically encapsulates about a twenty-square-kilometer area where we could track fish three-dimensionally with acoustic tags, and these were Vemco VAP, and so accelerometer and pressure. There were thirteen tags that we attached to fish externally.

In the fall of 2019, late summer of 2019, we went out, and we knew -- We had done some work in this area before, and so we knew where the reefs existed, both the reported and unreported reefs in the system, and the red triangles that you can see there are reefs where we tagged fish, and the black triangles were other reefs that we knew existed in the system, and the reason we worked on artificial reefs is because, at least in the northeastern Gulf of Mexico, the probability of occurrence of red snapper on artificial reefs is basically -- It’s about 97 or 98 percent of the reefs have red snapper, and they usually occur in some abundance, and so we could reliably capture and tag, and our goal was to get ten fish tagged with our fish tags on each of these five reefs, for a total of fifty, and we were able to do that.

The reefs themselves were small-scale modules, or paired modules, typically about three cubic meters apiece, and so quite small, prefabricated concrete modules, and I’ll show you some images of those in later slides, so that you can see them.

The tags themselves were programmed, for the first three weeks, to ping every thirty seconds, and we realized that that would cause some tag collisions, because of the density of tags in the system, but we wanted to have a high ping rate, and so these were randomized, to try to avoid tag collisions. We wanted to have a high ping rate, because our goal was to tag fish and then come back ten days later, after they had a chance to acclimate to their external tags, and then do the series of experiments that we had in mind.

This worked out quite well. We weren’t able to get back out for about ten days, and then, two days later, we finished this work, and so, the high-tag frequency period, we were well within that

to examine fish movement and reaction to the gears.

On a given site, when we went back to do the research, the first thing we did was the boat circled up on the site, and they got over the top of the reef, and the captain found is referred to as the hold-up, what the current and wind are doing and how the captain can live float it, so we don't have to set an anchor, which is usually a lot more trouble than it's worth.

Once the charter boat captain, in this case Johnny Greene out of Orange Beach, Alabama, found the hold-up, then we dropped the gear in the water, and you can see, on the left-hand side, we have sonar, as I mentioned, and then a camera rig. After the gear was dropped in the water, we had a second vessel, with a crew out of Destin, Florida, Josh Livingston and his brother and some other folks, that they would send a diver down and then position the rig so it faced the reef.

We gave it an hour after the gear was initially deployed for the fish to kind of settle down. When the gear hits the bottom, it stirs up sediment, and the fish get excited, and they come check it out, and so we gave it an hour for fish to acclimate, and then we sent divers down to position the gear, and the divers spent fifteen minutes around the reef structure, and we wanted to estimate the red snapper reaction to the divers.

Once the diver left the water, we waited fifteen minutes, and then we randomized which of the three gears we sent down next, and so, if it was the ROV, then we did standard ROV surveys around the reef. It was the towed vehicle, or the towed acoustics sled, we just tried to maintain fairly tight turns and tow the gear back and forth across structure. Again, each time we took a gear out, we allowed fifteen minutes for the fish to reacclimate to having gear not be present.

The figure on the left here, you can see, on the Y-axis, we have red snapper counts, and on the X-axis is minute, and so this is the, more or less, sixty minutes of time, and the count that you see here, and this is after we put the camera and/or acoustic platform into the water, and we allowed this sixty minutes, and so what you're looking at here is the acclimation of fish over time, and, by about minute-forty, you can see a leveling off, and so we pretty good that our sixty minutes allowed fish to sort of settle down after the initial deployment of the two stands, the acoustic stand and the camera stand.

The snapper count here is just from the stereo camera rig, and it's important to remember that this wasn't yet aimed at the

structure, and so there’s a lot of variance in the data, because sometimes it was aimed right at the reef, and sometimes it was aimed away from the reef, but these are the red snapper that came to check it out and moved around.

On the right-hand side, you can see three panels, and the top is distance to reef, the middle is height off the bottom, and the bottom is acceleration, in meters per second, and so these tags had accelerometers in them, and so they sent accelerometer data to the receivers on the seabed, and so not only could we estimate the position of fish three-dimensionally, because the receivers were set close enough to triangulate position, on the X and Y, and then the tags had pressure sensors that allowed us to estimate the Z.

We also had these acceleration sensors in the tags, so we could measure that as well, and so what you see here are means, plus or minus standard errors, and all the measures of dispersion here in this component of my talk, these behavioral experiments, the measures of dispersion here are always standard error, and so quite a bit of variability in the data.

The vertical line in each of these plots is when the camera rig was deployed, and so, because we had these acoustic tags already in the system for ten days before we did this experiment, we were able to look at the one-hour pre-deployment period and the one-hour post-deployment period, and so, consistent with the higher counts of fish early on after deployment, you can see a bit of reaction, where the distance to reef maybe gets a little bit smaller, and clearly the height off the bottom gets a little lower, and the acceleration in particular, and so fish are kind of closer to the reef, or not so much closer to the reef, but they’re closer to the bottom, and they’re not moving quite as fast. They’re kind of checking things out.

By about fifteen or twenty minutes after deployment, you can see they have settled down, and we have a return to activity patterns that existed before we put the two stands in the water.

The data you’re looking at here are compilations of the stereo camera data, and so, with the stereo camera, we were able to track individual fish, and it took Steve Garner about six months of pretty intensive work to track all the fish that are on these figures, and a considerable amount of effort.

The arrows that you see are color-coded from the first minute up to the fifteenth minute, and so this is during deployment. Diver in the water, and the top-right is the ROV, and the

bottom-left is the towed camera vehicle, and the bottom-right is the towed acoustic sled. This color scheme that you see here is one that will be used for all of the panels of the four different gears.

The numbers that occur on the arrows, and so these arrows are vectors, which is the mean movement that was observed in direction and distance of fish during the course of that minute of observation, and so, among the five different artificial reefs and the different gears, we have lots of different fish that are being observed, and the number that you see at each arrow is actually the sample size, and so the color of the arrow, the shading, tells you the minute, and then the number tells you the number of fish that are represented by that mean vector.

What you can see here is that most of the fish, in all of the systems, were pretty close to the reef, and perhaps this isn't surprising, because the stereo camera rig only had a certain field of view, and it was a few meters left and right of the reef, and we positioned the stereo camera and the acoustic stand five meters from the reef, and the acoustics spread out in a cone, as they do, and so there is not a whole lot of difference that we see here in the stereo camera data among the various components.

We do see, perhaps, a little bit more distance from reef for the towed camera vehicle, and then even a little bit higher in the water column for the towed acoustic sled, but it's important to recognize the scale that we see here, especially on the Z-axis, and height off bottom is a maximum of only two-and-a-half meters, and so the height off bottom ranged from about half a meter to about a meter-and-a-half, in most cases, and so not a whole lot of difference that you see there. Again, we could probably measure, effectively, about three meters into the water column above the reefs with this gear.

Next, the telemetry data, and so, on the left-hand side -- Actually, these are the raw counts from the stereo camera, and the right-hand side are the telemetry data, but so, the first column, we have raw counts of number of red snapper observed, and this is once the stereo camera system was pointed at the reef, and then, on the right-hand side, we have the scaled counts, and so the scaled counts are the individual counts, the mean counts per minute, minus the mean count from the fifteen minutes before, that acclimation period before we put gear in the water, and so before the diver went in, before we put over the ROV, et cetera.

Again, the error bars for all of these are standard errors, and so you can see that the diver has the most pronounced effect, and there is an increase, a significant increase, in the count of fish that were observed when the diver was milling about the artificial reef, and the fish came in closer, and they were quite curious, maintaining a distance of a meter or two from the diver, but they would circle around the diver.

For the ROV and the towed camera vehicle, there are some positive deviations here. However, with the standard errors, you can see the error bars are all capturing zero, in the case of the ROV and except for a couple of cases for the towed camera vehicle.

As far as the counts of fish on the reefs, the towed acoustic sled had the least effect, as far as what was observed by the cameras, but you have to remember that the towed camera sled was up in the water column, at a depth of about a few meters, three or four meters, and I failed to mention, earlier on, that these reefs are between about thirty-five and forty meters depth, and so that’s an important consideration that I apologize for leaving out.

On the right-hand side, you can see we have three panels that are the movement data estimated from our three-dimensional acoustic telemetry and accelerometer tags, and so at the top is the distance to the survey reef, in meters, and, again, this is just in the X and the Y dimensions, and the Panel B is height off the bottom, measured in Z, and then Panel C is acceleration is meters per second squared.

For each of these, and so, again, the diver on the left, and the sort of mustard-yellow is the ROV, and the blue is the towed camera vehicle, and the orange color is the towed acoustic sled, and so you can see the control for each of these is that fifteen-minute acclimation period.

If you look at the distance to the survey reef, you will note that the means are pretty similar for these acclimation periods for whether the diver is in the water or the ROV, and, for the towed camera or the towed acoustic sled, the distance to the reef actually drops a bit, although this is only significant here for the towed acoustic sled, and the sample sizes that you see above each of these are the number of pings that we got for fish.

The model that was fit here was a generalized linear mixed

model, and so we had a random effect on the fish, to control for autocorrelation there, or lack of independence for those individuals, and so, in some cases, we were able to track one fish for some period of time, but each of these represents multiple fish from different reefs, but, if we had multiple measurements for a given fish, we controlled for that by putting a random effect on the fish.

Panel B, the height off the bottom, you can see there's a real significant drop, or there was a real significant drop, from a little over three meters to a little less than two meters, when the diver was in the water, but, for the other gears, there is not much of an effect whatsoever. Again, for acceleration, the only one here that showed much of an effect is the towed camera vehicle, and there's a slight drop in acceleration for the ROV, but there is too much variance, in the case of the towed acoustic sled, to draw much inference there.

Overall, among these various data, what we observed is that the fish, these tagged red snapper, and otherwise observed red snapper in the systems, had the greatest reaction to divers, with decreased height off the bottom, decreased distance from reef, and increased acceleration.

The behavioral reaction to mobile fishing gears were more or less neutral, and we did see some slight effects, but, in Sarah Grasty's analysis, we would consider this to be perhaps mildly positive, or slightly positive, but, in many cases, we had no significant statistical effects, and so the ROV is slightly more than the towed camera vehicle, which is slightly more than the towed acoustic sled.

In examining the data and understanding the scale of the sampling, as far as the width of the transects, which were about fifteen meters for the ROV, we felt that we didn't have a significant issue here with either attraction or repulsion of red snapper during the ROV sampling.

Now, there is an important caveat here to mention, in that experiments were conducted around artificial reefs, and, again, this was due to the probability of observing and being able to tag and track multiple red snapper, but the artificial reefs themselves may affect red snapper behavior that is different than at least low-profile natural bottom, and I have a couple of images shown here of what these reefs look like.

You can see that there's a pretty expansive sandy habitat around these reefs, and they very much function like patch reefs, and

so, if the artificial reefs themselves somehow affect the behavior of the fish and mask any attraction or avoidance of the gear, then, obviously, we wouldn't be able to test for those effects, and they would be confounded with the artificial reef.

We would very much like to repeat this study on natural bottom, and it's just very difficult to reliably find red snapper in high enough concentrations to be able to do this kind of work, and you need a very large sample size, and it would take probably a few hundred thousand dollars to be able to put this together to test this in natural bottom, even with all the gear that we already own, with the acoustic receivers and the ROVs, et cetera, and so it's just a really daunting task, and we just didn't feel like we had the budget to do it here, other than a place where we could go and reliably tag and observe quite a few fish.

That is the end of this early section on the behavioral experimentation. Joe, I don't know if you -- What is your prerogative here, as far as fielding questions or just moving on to the next sections and fielding them all at the end?

CHAIRMAN POWERS: Let's see if there's any overriding questions now, and then, if not, we can move on, but, first off, are there any questions of Will? John Mareska.

MR. MARESKA: Will, I was looking at Slide 8, and one thing I noticed is, on the right-hand side, for the towed slide, and, for your deployed, it looks like your number of pings -- That that number systematically goes down, and so it's like the fish are leaving the system, but the ones that remain don't seem to have much difference with their reactions, but, with that sled, it looks like the number of pings is going down, and is there something different about that towed sled that may be driving fish away?

DR. PATTERSON: If you look at the height off bottom, the height off the bottom is higher for the towed sled, in Panel B in this figure, and, if you go back to this figure, you can see, from the stereo camera estimates, the fish were higher in the water column, and you can see, from the direction of the movement, the ones that were observed were moving up into the water column.

It's possible that some fish went to check out what was going on with the acoustic sled up in the water column. With the stereo camera, we didn't have the ability to track them more than a few meters above the reef.

In the case of the telemetry data, we would be able to track them into the water column. After a certain distance, depending on where they were in the array, we would start to lose resolution, as far as triangulating their position for distance from reef, and we would also -- We might lose the ability to capture accelerometer data, and so I don't think the accelerometer data issue would be as high as the potential to lose the ability to track their distance from the reef, but, yes, that's an important consideration that we probably need to think some more about.

MR. MARESKA: All right. Thank you.

DR. PATTERSON: If you look across the sample sizes for the other gear deployments, you can see some other numbers that are in the teens, and so it's just kind of luck of the draw of what got randomly selected on that site, and there are plenty of other sample sizes in the twenties, and even the distance from reef estimate for the control period for the towed acoustic sled is forty-five, and so I would have to think about your question some more, John, but it doesn't look like the sample sizes are that much lower than what we see across some of the other treatments.

MR. MARESKA: But your pings -- So if you look at the control and the deployed for the distance from the reefs, the height from the bottom, or the acceleration, the number of pings tends to go up for the other gears, the diver, the ROV, and the towed camera vehicle, but, for the sled, regardless of that parameter, the number of pings is going down for the deployed.

DR. PATTERSON: Yes, I see that. We hadn't paid much attention to that, and we need to think about that some more. Thanks, John.

CHAIRMAN POWERS: Go ahead, David.

DR. EGGLESTON: Thanks, Will. Just seeing your pictures underwater got me thinking about -- I know you know this, but you're diving during the day, and those fish are hanging around the reef, and then, at night, you go out, and they're spread out over the surrounding area, and I was just curious about any of the towed hydroacoustic gear. Was it always during the day, or was any of that done at night? Again, I'm just wrestling a little bit with the, I guess, lower detections with the hydroacoustics.

DR. PATTERSON: It was always done during the day for this work,

and, for the sampling, that was done on the West Florida Shelf. I don't know the answer to that question, and I think it was during the day in the west with Jay, but he could answer that better.

DR. EGGLESTON: All right. Thank you.

CHAIRMAN POWERS: Go ahead, Mary.

DR. CHRISTMAN: Thank you. I'm also looking at Slide 8, and this is more for your publication that's in review. I assume that, when you did the generalized linear mixed models, you used a lognormal or a non-normal distribution with a long tail?

DR. PATTERSON: Yes, and they were different distributions based on the data, but yes.

DR. CHRISTMAN: Okay, because one of the things I'm thinking here is that you're putting these standard errors on as though the data are symmetric, when they're not, and so I would recommend you rethink how you describe them, because one's first reaction is, looking at the raw counts, for example, on the left -- I'm looking at that and going, is that mean correct? Five, when I see something like eleven of those minutes are above five, and so I think it's more of a matter of just you have to keep in mind the actual distribution of the data, and I think it would be reinforced if you didn't use symmetric standard error marks like that.

DR. PATTERSON: Okay.

DR. CHRISTMAN: I did have one more question. You were commenting about your study being done on high-density artificial reefs, and what about -- I would expect that fish that are in a conglomeration would respond differently to something that an individual or just a few fish would respond to something, because an individual smaller fish might be less likely to want to go explore and see what's happening, as opposed to when there is twenty-five fish surrounding them. Can you address the assumption that you're making that fish behave the same everywhere and in every density?

DR. PATTERSON: I don't think fish behave everywhere in the same density, but we just don't have any data to suggest how that might differ. Therefore, the caveat there at the end. If they do, if their behavior does change demonstrably, then that would affect the inferences that we're able to draw from this.

Red snapper aren't schooling fish, and they don't form tight aggregations, and so they're not -- As they swim, they're not teeing-off their neighbor, like a herring or a menhaden would do, and they don't form tight aggregations like an amberjack might, and so, in that respect, they move more or less independently, but, you know, in any case, when fish sense feeding opportunities, they tend to all kind of go check out a feeding opportunity, and we think that's what we see when the camera rig and the sonar rig hits the bottom and fish kind of show up on the video.

They are responding to that cloud of sediment that's being resuspended, and there are, obviously, fishing techniques where you can bounce jigs off the bottom, to try to cause the sediment to get stirred up, to get fish to bite your gear, but, as far as whether they're more likely to respond to an ROV or to a towed camera vehicle, if it's a single fish or multiple fish, in the case of red snapper, I don't have a good feel for that.

CHAIRMAN POWERS: Carrie.

EXECUTIVE DIRECTOR CARRIE SIMMONS: Thank you, Mr. Chair. Very interesting study, Dr. Patterson. I have a question about the diverse mean abundance in the counts there, and it looks like you were getting higher counts, mean counts, with divers than the other three methods. Was that significantly different for this study, and do you have any idea why?

DR. PATTERSON: The divers were significant, and one drawback of the way we constructed this, and something we would do differently in the future, is that we had a finite amount of time to do the work, and we weren't able -- Ideally, in hindsight, we should have put the gear in the water, put divers down, aimed it, left it a few days or a week, allowed the fish to totally acclimate to it, and then come back and do our experiment, but we didn't have the flexibility then, and we didn't really consider that issue as we should have on the frontend.

One of the issues here, Carrie, with the diver deal is that this is following -- Always the diver is following the deployment of the rig, the rigs on the bottom, and so you have -- It's sort of a confounded effect here. The diver issue couldn't be randomized like the ROV, the towed camera vehicle, or the towed acoustic sled, and it always followed right after we put down the gear, and so we can't separate whether this is an acclimation effect.

However, what we did see in our data beforehand, and this is discussed in the paper, is that the fish tended to settle down after about forty minutes, and so that hour acclimation period on the frontend we presumed was enough time for things to kind of go back to normal, but, yes, the divers did have, in many different metrics, the most substantial effect on red snapper movement behavior.

EXECUTIVE DIRECTOR SIMMONS: But they were counting more fish when they put the gear down first and then they went to the reef and counted the fish, versus counting the fish and then going back down and putting the gear down, and is that what you're saying?

DR. PATTERSON: No, and I'm sorry. The counts on the two columns on the left, those are counts from stereo cameras, and so the divers were just in the water to be in the water and affect fish movement, and they weren't actually doing anything productive. They were just kind of hanging out.

EXECUTIVE DIRECTOR SIMMONS: Okay. Thank you.

DR. PATTERSON: Sorry about that.

CHAIRMAN POWERS: Thank you. If there are no other questions at this point in time, perhaps now would be a good time to take a quick break, let's say for ten minutes, and then Will can go on and rapidly go through the other parts of his presentation, and so let's take a ten-minute break.

(Whereupon, a brief recess was taken.)

CHAIRMAN POWERS: All right. Welcome back, everybody, for this afternoon's session. Dr. Patterson, will you carry on?

DR. PATTERSON: Yes. On the left-hand side here, you see a map of the West Florida Shelf and the Panhandle, and so the five regions, and these were just regions that we had internally to map out our sampling, and these don't have anything to do with the study design, but you can see, across the shelf here, the circles are natural bottom sites or unclassified bottom, and the triangles are artificial reefs.

The filled shapes are ones where we had sonar as well as ROV data, and so you can see, toward the southern end of the range, most of these we have overlap, and in the Panhandle, we have overlap, and, for part of the central region, Region 2 and Region 3, we have overlap between the sampling gears.

When we get up to the northern end of Region 2, we had less overlap, and that was due to the fact that we were working on some smaller vessels, and Kevin's group had some other sampling that they needed to do at the time, and it was difficult to coordinate the overlap there, and so we didn't have the acoustic data for much of those.

Throughout the sampling that we did, we took multiday trips on participating charter boats, and so you can see an example here in the top-right, and this is the Intimidator out of Orange Beach, and it's sixty-five feet, and there's plenty of room for a crew of three or four, between myself and Kevin's lab.

On the bottom, you can see the R/V Hogarth, which is owned and operated by the Florida Institute of Oceanography in St. Pete, and this vessel had been christened just prior to when we started our work in 2018, and I think we were actually the first substantial multiday trips on the vessel.

The vessel was meant to be equipped with a dynamic positioning system, but they didn't have that working quite yet, and so it took us a couple of days to get rolling with that. One advantage was that the transducers for Kevin's work were mounted to the hull, and so we didn't have to deploy any gear or retrieve the sled there, and so we did ten-day trips on the Hogarth, mostly on the greater than fifty-meter sites, and we did typically four-day trips on the various charter boats that we utilized from the Panhandle of Florida down into south Florida.

I just want to reiterate that we followed the design that Rob produced with the random forest modeling explicitly, except for the fact that we did add some additional artificial reef samples in the Panhandle from other research that we were doing in that region.

The gear, again, that we utilized predominantly to estimate red snapper abundance on reefs were these small video array ROVs, and my group actually has three of these systems, and so we were able to send two on the Hogarth, in case we had any kind of issues, and we didn't want to return to shore from that far out, and then one on the charter boat, and so we often had two field crews, one led by Steve Garner and one led by Joe Tarnecki, operating a time.

You can see a typical natural reef here, and this is off of Destin, Florida. There's lots of diversity and lots of small

demersal reef fishes. Artificial reefs tend to be much more focused biomass around the reef, and you can see lots of different snappers, and you can see vermilion, red, gray snapper, triggerfish, and some grouper, but the two habitats, because of their different spatial dimensions, required different types of sampling.

On the bottom-left, this is a point count sampling approach that we took for these smaller single or paired-module artificial reefs, and the right-hand side depicts the transect method that we utilized for larger-scale natural bottom. For those sites, we got to the coordinates of the structure, and we dropped the ROV in the water, and, with a downrigger weight ball, a twelve-pound weight ball attached to the tether, and we flew four twenty-five meter orthogonal transects away from that weight ball, and so in opposite directions to and from, and then changing north, south, east, west.

In the document, there is quite a bit of algebra shown, and trigonometry, for how the C-BASS estimates the path, the width, of the transect, and so we take a similar approach here with the ROV, and theta-one is the angle of the camera relative to the seabed, and, if we know the height off the bottom, and we know this angle, then we can estimate the distance, A, and then, because we know the field of view of the camera, and different cameras have different fields of view, and they have different issues with parabolic shifts, and then we also have different settings on different cameras, and so it’s important for us to know what camera we have and what the settings are.

We mount a GoPro actually to the top of the ROV, and the image that you see here on the top-right, this is actually from the camera of the ROV, and we have a GoPro mounted above it that’s angled at forty-five degrees toward the substrate, and so this allows us to see in front of and then down and map those transects, and so, again, two different approaches, and there are several different papers that we’ve utilized these approaches that are cited in the report, and I can provide those to anybody who would like to see them, but these methods have been utilized extensively over about the past fifteen years in the region.

There’s quite a diversity of habitats, and so grazed-down seagrass beds in the Big Bend area. In the top-right, we can have these broad sandy patches with lots of benthic microalgae on the sediment surface.

The bottom-left is some patchy reef habitat, and you see lots of

gray triggerfish here, and some snapper in the background, and then there's some other small-scale patch reef habitat with red snapper, and we find that it often doesn't take much to hold snapper and triggerfish, but a lot of these areas were away from known habitats that had been well mapped and studied, like the Middle Grounds and Madison-Swanson and The Edges, et cetera, where we would find snapper.

The randomization from the design that Rob put together I think was really key in Florida, to be able to estimate the numbers and density of fish in the system there.

Next, we had some higher-profile and more continuous reef systems, and so from one to two meters in vertical relief, and sometimes we would find lots of species, but not red snapper, and you can see scamp here in the background, and this is small damselfishes here in the foreground in the top-left image. On the right, and so this is midway across the mid-Florida Shelf, and you can see a couple of red snapper, three or four in that image. The bottom-left is another habitat in that same system, and the bottom-right is some significant sponge habitat.

There's quite a bit of this natural live-bottom habitat on the West Florida Shelf, and sometimes we would find red snapper and sometimes we wouldn't. Often, they would be in pretty low densities, one or two or three fish, spread out over this expansive low-relief natural bottom.

Then, as we move farther offshore, we tend to get higher-profile reefs, and so this is -- These are mesophotic reefs in the top-left off of Pensacola and Destin. On the right-hand side over here, this is along the outer Middle Grounds area on the West Florida Shelf, and, again, we would encounter quite a diversity of fishes on these deeper mesophotic reefs, and sometimes we would see snapper present, and sometimes not.

Then, at the bottom, we have a couple of examples of artificial reefs in the system, and so, on the bottom-left, it's just a concrete module, and you can see several red snapper around this module.

Ten years ago, we might see forty or fifty red snapper on this site, but, since 2010 in the Panhandle, where this reef is, we've seen about a 70 percent decline in the red snapper densities in that system, and this is more indicative of what we would encounter today, just a handful of fish, and there's several lionfish around the reef as well, but this is pretty indicative of what we find there. In the bottom-right, here we

have a more substantial artificial reef, and, obviously, there's lots of snappers and triggerfish around there.

All told, we sampled 749 of these natural bottom or unclassified sites and sixty-five artificial reef sites. Among all of these samples, we counted 148,644 individual fish. Of those, 3,850 were red snapper, and there were 179 different taxa, 155 to species, ten to genus, and fourteen to family, and so there's a wealth of information that we're just starting to tap into here, and, obviously, that wasn't the objective of the funding agency, Sea Grant, for us to develop fish community structure and habitat associations across the West Florida Shelf, but it's actually one of the more interesting components of the work, from my perspective, and so I'm glad to have had the opportunity to collect these data, and I think it's going to be very useful for lots of applications, both fisheries-specific or independent.

On the bottom here, you can see the distribution of estimates of number of fish per hundred square meters among these 749 natural bottom sites, and so you can see we have this distribution with lots of zeroes and then this long tail from that, which is not atypical.

We did throw out one sample, which is mentioned in the report, and I'm drawing a blank as to what its density was, but it was an order of magnitude higher than the other samples, and there were ten other samples within that stratum, and it had an enormous effect on the overall estimate, because of how large the stratum was, and so this kind of speaks to some of the issues raised earlier about numbers of samples and number of unsampled cells in a stratum.

An argument could be made that, if your samples are randomly distributed, and your variance is what you expected it to be, then that's a perfectly appropriate way to conduct the sampling. In this case, that one sample, which we replaced with a mean among the other samples, had a twenty-million fish impact on the estimate, and we had quite a bit of discussion about that sample.

My initial perspective was that the data are what they are, but, after talking to Jay and Greg weighing-in, and a big group discussion we had among the team, and how they were handling a couple of the outliers they encountered in Texas, the consensus was developed to remove that sample and present their rationale for that in the report, and so that's what we did, and I ended up changing my mind about that, that sample. Joe, this is kind

of the end of the second section of the talk, and I can keep going. There are only a handful more slides in this last section, or we can pause here, whichever you would like.

CHAIRMAN POWERS: Why don't you go ahead and keep going?

DR. PATTERSON: Okay. In the report, and, obviously, in Doctors Christman's, Cadrin's, and Eggleston's draft critiques of the report, this issue of the correlation between the sonar-estimated red snapper and the ROV estimates, and, again, scaled to an area of a hundred square meters. That shouldn't be meters to the minus two, and that should just be square meters on both axes, and sorry about that.

What you see here is there is a relationship. There's a correlation here, and it's significant, but, overall, the count from the ROV is, on average, nine-times higher than the count from estimates from the sonar, and so we've thought quite a bit about what could be driving this difference, and so one thing to consider, and this figure on the right is a new figure that doesn't show up in the report, and I'm sure we'll include it in the final report to Sea Grant, which is yet to be submitted.

Here, we have the transects, and these are sonar transects that Kevin Boswell's group would have run, and there were at least three one-kilometer-long transects centered over the top of the reef coordinates from Rob's model, and there were as many as six, but, after the first few days, or the first couple of trips of sampling, we settled on three, to get as much done in a given day as we could with the sonar gear, and so, after the initial sampling, this became sort of the path forward.

You can see that the dimensions of this -- The area in which this sort of asterisk shape is, it's a square kilometer. Now, all of that square kilometer is not being sampled, but that's the other dimensions, and so that's a million square meters, and the path that you see here is the center black line, and this is the shift path, and then the gray is the estimated width of the sonar cone, about a meter off the bottom, where we can reliably measure, or estimate, fish targets.

Then the path, you can see the arrows kind of indicate, and the dotted lines are the turns, and then this purple box that you see here, this is the sample unit, and so these are the three units, basically ninety-meters-by-ninety-meters, from Rob's design, and so they're not perfect squares, but I show it here as a square, and, inside that, this is the dimension of the ROV transects.

The dimensions of the sonar transects doesn't match at all the ROV, right, and it's an order of magnitude greater. The ROV transects are about 1,500 square kilometers, and the sonar transects, on average, are 100,000 square -- Not square kilometers, but square meters. So 1,500 square meters and 100,000, and so two orders of magnitude, actually, difference between the two.

The ROV transects actually match pretty well the dimensions of the sample unit, but the sonar is sampling over a much broader area than what we see in the ROV transects, and so, if we think about this in the context of the habitat, of these sample units, like the purple squares that you see here, with the one with the ROV transects highlighted in a little thicker outline, the area over which the sonar sampling is occurring is about a square kilometer, which is approximately 122 of these sample units that we have from Rob's design.

We're averaging across a much broader area, and, if we had random placement of our sample units that were selected, such as the highlighted purple square, then we have a random sample of the shelf distributed according to the random forest model.

However, with the sonar, we're actually averaging across multiple of these units, and so, in the top-right, this image, where we have this more expansive habitat, sort of the central West Florida Shelf habitat, then that probably isn't that big of a deal, because the habitat is more or less continuous, and it's similar throughout that one-square-kilometer area over which the sonar is really sampling.

The bottom two habitats -- So, in the center one, this is sort of that patch reef habitat that can occur throughout the region, where we just have random carbonate or sponge habitat, and a handful of red snapper may or may not be there, and so, if this is the case, and, through our random draw, we end up with a sample that's right there, like we did in this case, and we count a handful of red snapper, most of the habitat around this is sandy bottom, and so that's what Kevin's sonar is going to be seeing in that particular -- Away from those central coordinates.

Then, at the bottom here, artificial reefs in the system, especially where they exist in highest concentration in the Panhandle, where the sediments are mostly the MAFLA sand sheet, and we have these quartz silica sediments, and this sort of flat plain that we have on the shelf, and these are true -- The

patchiest reef habitat in the entire Gulf, and it’s artificial, but it’s reef habitat, and it’s incredibly patchy.

We actually had nine stations where we had sonar data and we had the ROV data where we saw red snapper, and so, at these sites, we went back -- We had excluded those from the initial analysis, and we went back and looked at these, and there’s actually a thousand-fold greater red snapper estimates from the ROV than from the sonar at these sites, because there is no other reef habitat in that one-square kilometer, unless there happens to be another artificial reef in that area.

I really think it leads some credence to this issue that the patchiness of the habitat, the scale of the sampling, is driving some of this difference. How that applies to the western Gulf, where the sonar is being used over really truly unconsolidated habitats, I don’t know exactly how to put that together yet, but I think the patchiness here is driving some of this, and it’s confounded.

We can’t say how much of the snapper we’re missing because of the reflectance along the seabed and having to subtract out some of that, but I feel pretty confident that some of the difference that we’re seeing here is just driven by the patchiness and the scale of the habitats, and so spatial distribution of habitat versus transect length is probably an issue.

Acoustic dead zones could be contributed to this, and then, lastly, if we have attraction of red snapper to the ROVs, this would, obviously, bias the ROV counts high. Across the data, watching the fish in the water, the experiments that we did, I don’t perceive this to be a large issue, and, again, I won’t go back and repeat the caveats of our experiments, but I think it’s probably these first two that are playing a larger role.

In the course of doing this work, we wanted to develop a system where we could get really good length composition data, thinking about integration with the assessment and reconciling our estimates with the assessment and understanding the size distribution of fish from inshore to offshore, et cetera, and, traditionally, we’ve used a small laser scaler, shown here in the top-left, with this Distance B.

This is a paper that Steve Garner recently published, and so we developed the stereo camera system to be used with our small video array ROVs. The pictures in the bottom-left, these are some pool experiments, where we adjusted our calibration for this gear, and so the calibration involved this checkerboard

pattern and moving it up and down and all around, to get different fixes on the data points, to calibrate the R package, on the backend.

The figures on the top-right, the panels here, the top row is for a fish that was 280 millimeters, and then, in the middle, we had a fish that was about 550, and the bottom is a fish that was about 890, and you can see the different rows, and these are from the laser scaling. The middle is the stereo camera distance of 409 millimeters, then 610 millimeters, and then 762 millimeters, and so we wanted to see -- The farther you set your cameras apart, and they're inset ten degrees, but, the farther you set them apart, the higher the accuracy, and, typically, the better the precision of your size estimates.

I will actually point out too that, going from the lightest shading to the darkest shading for each of these distances, one meter to five meters, from the ROV to the target, the difference shadings are angles from perpendicular that we're viewing the fish, and so, the greater you depart from perpendicular, the more bias you have, and you can see that, as long as we keep this less than twenty degrees, then we meet our threshold of about five degrees of bias.

Amazingly, with the stereo camera system, even set at 409 millimeters, most of the samples were well within our five degrees of bias tolerance, except at five meters, and we start to get some departure at the 409 millimeter distance, but the cameras that we deployed in situ, to take measurements, we had the setting at about 500 millimeters, because we felt like that would capture the bulk of the issue here with the potential bias, but, also, it would enable us to get the cameras in close enough that it wouldn't affect the stability of the ROV, and, at 500 millimeters, we found sort of our sweet spot.

We also then scaled these PVC pipes in situ, and so these are multiple measurements taken at different distances away from the pipe on deployments off the Panhandle for our sampling, and so you can see that all of these are within our five degrees of bias, and mostly a slight positive bias, and we used these data then to produce a distribution to then correct for the estimates that we derived of fish that we actually observed.

This is an image from Steve's paper that shows a school of vermilion snapper, and this fish right here in the center, Number 4, is one where the laser scaler actually hit it broadside, so we could estimate the length of that fish, and, among the other fish on the screen, we had nine fish that we

could estimate using the stereo cameras, and the two stereo cameras are shown in Panels B and C. Panel A is actually this top camera on the ROV.

We're able to greatly enhance the number of length measurements, estimates, that we can take using this stereo camera system with the small ROV, and the result of that is this distribution, which we were able to scale 637, or 16.5 percent of all of the red snapper that we observed.

We have this pattern of quite a drop-off after about 400 millimeters, and then lower numbers of fish and sizes above 500 millimeters, and so could this be an issue of avoidance, a potential selectivity issue of the gear, or is this related to the fact that we have a rebuilding snapper population in the West Florida Shelf and fish in the size range from 700 to 1,000 millimeters, that could be between ten and fifty years old, just don't occur in that system, to the extent that they will be as the age composition recovers over time?

We don't really have the information to tease that apart, and I do want to highlight a comment that Dr. Cadrin had in his review of the report, when he went back to the stock assessment and talked about the biomass distribution estimates in the east and the west and how the biomass distribution in the west was estimated -- The biomass amount in the west was estimated to be so much higher than in the east.

The metric for that is eggs in the assessment, and so it's an SPR measurement that's being utilized as a measure of spawning stock biomass, and so it's kind of a disproportionate issue, because most of these are small, young fish here that we're showing in the east, again sampling bias potential notwithstanding, and so these fish aren't going to represent the same egg distribution, or egg amounts, as what we see in the west, where we have a much more filled-out age composition, age distribution, in that system.

We did see some of these really small fish, and, as I mentioned earlier, these fish really start recruiting to reefs when they're about 250 millimeters, and so this would argue that they probably don't fully recruit to the reefs until they're about 300 or 350 millimeters, but we know they start showing up later, as they're entering their second year of life, but we had these really small fish that were likely age-zeroes, and so, because we had all of this age composition, or size composition, data from Florida, we thought we really needed to address the potential for these fish affecting our estimate, and that was

meant to be an age-two-plus estimate.

We computed a growth function, based on some older data and some more recent data for red snapper in Florida waters, and the different distributions that you see here are for fishes estimated to be one-year-old, 1.25, 1.5, 1.75, 2.0, and 2.5 years.

Now, a red snapper, on average, is going to be 1.5 years old January 1 of its second year of life, because they have -- Most of the spawning happens in mid-summer, and so we settled on a cutoff here at 250 millimeters, and some of the reviewers pointed out that this might have been a conservative cutoff, given the fact that 40 percent of the fish that are 1.5 years, estimated to be 1.5 years old, are actually less than this threshold.

However, if we drop the threshold down to 225 millimeters, then we have a lot of fish that are less than 1.5, again, which is that age-two cohort at January 1, and that wouldn’t meet our criterion of age-two-plus, and so we had a lot of discussion about this, and we thought this was probably the most pragmatic solution, was to set our threshold at 250 millimeters, and so that was the cutoff, and then we went back and recalibrated, re-estimated, what our density estimates were, and Rob produced the population abundance by stratum, and then for the Florida Shelf, and then Sean also used this approach in Alabama.

Obviously, thanks to the funding agency, Mississippi-Alabama Sea Grant, for allowing us to do this work, funding this work. Then the captains and crews of lots of participating vessels up and down the west coast of Florida, and they were really instrumental in performing the work.

Jess Van Vaerenbergh and Jordan Bajema were the two folks that accompanied Joe and Steve on most of the trips, and we had quite a few different NOAA Fisheries collaborators, talking to Matt Lauretta and John Walter about some of the assumptions and how to go about some of our bias checks, and then Matt Campbell and his crew, Adam Pollack and others, and Pascagoula, kicking around ideas about how we could actually test some of this.

Then the Grants Administration folks at the UF School of Forest, Fisheries, and Geomatic Sciences were really instrumental in helping us move money around and get invoices paid. There were lots of volunteer anglers for different components of this. Peer reviewers and editors for the couple of journal submissions that we’ve had so far, and then, obviously, the external and SSC

reviewers, whose comments have already and will continue to improve our work, and so that's a wrap.

CHAIRMAN POWERS: Thank you. Let me open the floor to questions for the issues that Will has brought up in the presentation. Steve Cadrin.

DR. CADRIN: Thank you, Mr. Chair. Will, thanks to you and your team and the broader group for a great presentation and an impressive report. I know the SSC is going to be focused on catch recommendations in the near-term, or this week, but I completely agree with what you said in your presentation, and I expect this is going to be a goldmine of information to use for a lot of different applications for years, to study the biology, but one clarification on the comparisons of the SEDAR 52 estimates to the Great Red Snapper Count.

I was looking through the SEDAR 52, and I was a little challenged to find a direct comparison of age-two abundance, and so I bothered Matt Smith to send me the Stock Synthesis output, and so my comparisons were based on the age-two-plus abundance from both, and so I didn't really get into the egg fecundity currency on that, but my question to you is based on the comparisons of the ROV and sonar. From your perspective, can those results, and those comparisons, be used to estimate a detection for sonar?

DR. PATTERSON: First, my apologies, Steve, for misappropriating a comment to you that was inaccurate, and I actually didn't catch that, and so that was my mistake. Sorry about that. The answer to your question is I don't think so. I don't think so, because we don't have perfect knowledge of the habitat, right, and so we can't -- There are some confounding effects here that we can't pull apart with the information we have.

If we went back out to these systems and we felt confident that we were right on top of the numbers that we sampled before, and we could do some broader surveys, and collect new information, then perhaps we could examine the detectability of the sonar.

My group is actually leading the new study that just was announced in the Atlantic to estimate the population abundance of red snapper in that system, from the Carolinas down through the Keys.

There are a couple of different methods that are being used there, one of which is close-kin mark-recapture, and that's being led by Dave Portenoy at Texas A&M Corpus Christi, but

there's another integrated modeling approach that involves the camera trap survey that MARMAP and SEFIS does combined into SERFS, and then we're going to be doing some ROV sampling over there, where we're going to do longer transects, and we're also -- We didn't do the longer transects or worry about detectability in Florida, because the water is so clear, and we just didn't think it was going to be an issue, but it can be an issue on the east coast, in different areas at different times of the year.

There, we're going to do some distance sampling, to try to estimate detectability, flying the ROV at different heights off the bottom and different water clarity conditions, and I think, if you combine that approach with the sonar, especially if you had them running at the same time, then you could perhaps look at detectability of -- Because you could estimate the detectability of the ROV based on the distance sampling, and, if you then assume that that's an unbiased estimate, detectability, then you could back-out what the detectability then would be with sonar, but I think you would have to do that type of approach to really get at that question.

DR. CADRIN: Great. Thanks. Well, hopefully we can have that South Atlantic red snapper meeting live.

DR PATTERSON: Yes.

CHAIRMAN POWERS: Thank you. Mary.

DR. CHRISTMAN: Thank you. Thank you, Will. That was very interesting, but now I have a bunch of questions about sample sizes and things of that nature. You said sixty-five artificial reefs were selected by Rob that you sampled, and is that correct?

DR. PATTERSON: Actually, it was nine, or maybe fourteen it was, that Rob selected, and we were able to add some data from other work we were doing in the Panhandle, and so we randomly selected from a broader number, a larger number, of reefs to be included into this estimate.

DR. CHRISTMAN: Okay, and so that -- How do you explain that -- In the text, you say it was sixty-five, but Table 6 shows eighty-four.

DR. PATTERSON: I don't know. I didn't put together the table, and so I would have to go back and look. I'm sure there are going to be other situations where we transposed something

incorrectly, but I can't answer that. I don't know.

DR. CHRISTMAN: Okay. The next question I have is, when you talk about natural bottom, what you mean is the uncharacterized bottom, the UCB?

DR. PATTERSON: When I talk about natural bottom, it's showing my bias, and that is there are no artificial reefs there, and so it can be sand, or it can be reef, or it can be high-profile reef, or ledge, but simply -- I should use the vernacular of the study and just call it uncharacterized. I have a hard time doing that in Florida, because some of it has been characterized and some of it hasn't, but sorry to add confusion to this, but, really, I'm talking about this uncharacterized bottom.

In Florida, we had artificial reefs and we had uncharacterized bottom, which, for people listening, or people that fish, or do research in Florida, they may think that's kind of silly, because there's so much area that's been mapped and is known, but the problem is, from a stratification point of view, all of it hasn't been mapped, and so we knew where some stuff was, but not everything, and so we couldn't use a habitat map to stratify bottom.

DR. CHRISTMAN: Okay, but that leads to lots of confusion, for me at least, with respect to where the sample sizes wound up. If you look at Table 6, it shows uncharacterized bottom, and there were 530 observations, and, for natural bottom, there were 505, and so is one a subset of the other, or did you take the uncharacterized bottom, the 749, and remove some that were post-hoc stratified to natural hardbottom?

DR. PATTERSON: We provided Rob the habitat characterizations after we did the ROV sampling, and so we didn't know, heading into it, what the habitat was in those areas, and we had some reasonable guesses, given, geographically, where samples were placed, but we provided the information to Rob on the backend, as to what we actually encountered when we went to the coordinates of those 749 sites.

DR. CHRISTMAN: Okay. So, somewhere along the line, either some sites were double-counted or something, because 505 and 530 don't add up to 749, and so we need to figure out what's going on there.

CHAIRMAN POWERS: Along those lines, if I may interject, I'm unclear -- I mean, in the original design, you had three basic strata of the artificial reef, unclassified bottom, and natural,

and so where does the natural come in, in the case of Florida? Are you saying that all of what you called -- Well, what you just reported on, you say it was basically uncharacterized bottom, and so where is the natural stratum? Where does that information come from?

DR. PATTERSON: I am going to defer to Rob for that question, because I don't want to misinterpret, or misstate, that design aspect, and so if Rob is on and he could field that.

DR. CHRISTMAN: Probably not.

MS. MATOS: He's not on right now.

DR. CHRISTMAN: So that's something that, Ryan, maybe you could make a note that we need to get Rob to clarify. So where did the 22,800 square kilometers of natural, presumably hardbottom, come from then? That was left over from the prior work that was done in Phase 1?

DR. PATTERSON: I thought I just heard Rob's voice.

DR. AHRENS: Yes, I'm on.

DR. CHRISTMAN: Rob, can you explain why Table 6 shows 505 natural bottom and 530 uncharacterized bottom samples?

DR. AHRENS: I cannot.

DR. CHRISTMAN: Okay, and so we need to find out why those numbers aren't right.

DR. AHRENS: For Florida, all bottom was uncharacterized to allow -- For some of the tables being created, we took the high-probability random forest category habitat as the natural bottom.

DR. CHRISTMAN: But still that adds up to more than 749 samples.

DR. AHRENS: What happened in the sample sizing here, I'm not 100 percent sure, and that would have to be looked into.

DR. CHRISTMAN: So let me make sure that I understand you correctly. You did not post-stratify based on what was observed in the videos?

DR. AHRENS: I did not.

DR. CHRISTMAS: You stratified hardbottom as the high-probability random forest class, if it was uncharacterized bottom?

DR. AHRENS: Yes, and the decision was made, to allow for comparisons where natural hardbottom was known in regions, that we would use the category of high-probability as a natural bottom comparison.

DR. CHRISTMAN: I'm sorry, but could you say that again, because that doesn't sound like what I am trying to -- If 749 sites were selected on what was called internally in Florida natural bottom, some of which is -- All of which is basically uncharacterized bottom, how did you wind up with any hardbottom, for estimation purposes? That's what I am trying to figure out.

DR. AHRENS: We selected the high-probability habitat as the hardbottom.

DR. CHRISTMAN: So any intersection of uncharacterized bottom with random forest high-probability bottom, that then got labeled as natural bottom?

DR. AHRENS: Yes.

DR. CHRISTMAN: It was then removed from the remaining uncharacterized bottom?

DR. AHRENS: That was done by others, and so I'm not exactly 100 percent sure how these exact numbers were derived, and Greg, or someone else, could answer that question.

DR. CHRISTMAN: Okay. Then one other question related to this high-probability random forest class. Which cutoff did you use for high-probability? Was it 0.77 or 0.989?

DR. AHRENS: For the random forest?

DR. CHRISTMAN: In Florida, specifically.

DR. AHRENS: Well, in Florida, I believe it was 0.86.

DR. CHRISTMAN: Okay. It's listed in the report as 0.989 is the cutoff between mid and high-probability.

DR. AHRENS: Let me -- It's 0.86, and so zero to 0.56 was low, 0.56 to 0.86 was mid, and 0.86 to 1.0 was high.

DR. CHRISTMAN: All right, and so the one thing we have to find out is why these two numbers don't add up to 749.

DR. AHRENS: Yes, and I will start looking into that and hopefully have an answer for you shortly.

DR. CHRISTMAN: Okay, and, just to clarify something with you, since you're on, the document says that Florida's shelf was divided into three regions, and I'm not sure if was pre or post-stratum, but the graphic, Figure 5, shows five regions, and so, when you were doing your random selections, were you using three regions, five regions, or no regions?

DR. AHRENS: For Florida?

DR. CHRISTMAN: Yes.

DR. AHRENS: Florida initially was two regions, the Bend area and then the West Florida Shelf. For the analysis that was later, the West Florida Shelf was split, roughly at Tampa, into a north and a south, to try and further control variance.

DR. CHRISTMAN: So do you know why Figure 5 shows five regions then?

DR. PATTERSON: That's my fault, Mary. I didn't coordinate that with the post-stratification, and the five regions was simply how we handled sampling in my group, how we divided stuff up.

DR. CHRISTMAN: That's what I thought happened, and I just wanted to clarify how the actual allocation of observations was done, and it was done not based on five or three, but on two.

DR. AHRENS: Yes.

DR. CHRISTMAN: Okay. Thank you. Let me see. I think those were just the sample size issues that I had. Thank you. I will get off now.

CHAIRMAN POWERS: Okay. Thank you. Doug Gregory.

MR. GREGORY: Thank you. Will, I understand your discussion of the comparison between the sonar and the ROV, and I may have misunderstood something that I read in the overall report, but was that nine-times difference used to adjust or upgrade the sonar estimates in the western Gulf, where there was little or no visibility? Maybe somebody else would be better to answer that, but I thought that I read somewhere where the data from

your study was taken to adjust actual sonar readings in the western Gulf.

DR. PATTERSON: Not to my knowledge, Doug, but Greg or Jay could probably speak to that better.

DR. STUNZ: Doug, that nine-fold difference was not -- We made no adjustments in the western Gulf for that.

MR. GREGORY: Okay. Thank you.

CHAIRMAN POWERS: Thank you. Kai.

DR. LORENZEN: Thanks, Will. I was equally intrigued by that comparison between the sonar and the ROV, and particularly because you really looked very carefully at what's involved and the influence of habitat, for instance, and my question is sort of a generalization of Steve Cadrin's, I guess, and I am wondering -- From your perspective, and that of the other PIs, would it be possible to actually develop reasonably robust sort of intercalibrations between these different methods, and not just hydroacoustics versus ROV, but ROV versus the C-BASS and so on, because, if that could be done, obviously it would be huge, because it would then directly compare abundance estimates between strata, and it would also, arguably, provide a more meaningful overall abundance estimate, but I am really not sure whether -- I am assuming there would be additional funding for that, and if that's something that could realistically be done, or is it essentially not possible to do that? Thanks.

DR. PATTERSON: I think it is possible. It's a challenge, because, any time you enter the system, you change it, and so, when you tow one gear past an aggregation of fish, you're going to affect them, and so there are issues there to sort out, but that could be handled, perhaps, through randomization.

I think, for gears that are up in the water column, the comparisons between sonar and either a towed camera or an ROV flying long transects, I think are probably the easiest to make.

We thought quite a bit about these calibrations ahead of time, but I had tunnel vision about what was happening in Florida and how we tried to -- I was mostly concerned about the ROV in that system, but, taking a step back, I think there are some approaches that could be done on large scales, and it would have to be done in multiple systems, and it would have to be done in systems, perhaps, where the visibility was pretty high, or at least high enough over a range of visibilities where you could

estimate detection probabilities.

That would be key, but I do think there are some approaches here, and they wouldn't be cheap. It would take a significant effort, and it would have to be done in different regions and to develop distributions that could perhaps be used as priors in a type of calibration, and one value is not going to pop out of this, I don't think.

DR. LORENZEN: Thank you. That makes sense, but I do think -- I think maybe funders are listening, and it would be something that could add a lot of value to the information that's already been collected. Thanks.

DR. PATTERSON: Thanks, Kai. I agree with that, and, also, as National Marine Fisheries, or NOAA Fisheries, is examining the surveys that exist in the Gulf, and we're going through this process in the Atlantic, with the camera trap versus ROV and some intercalibrations there, and what's the effective sampling area, and Nate Bacheler and Kyle Shertzer and Jeff Buckel and others have been working on those questions for a little while now, but I think there's a lot of potential there. Again, trying to optimize surveys and get as much information as possible, those intercalibrations would be really important.

DR. LORENZEN: Thank you.

CHAIRMAN POWERS: Thank you. Luiz.

DR. BARBIERI: Thank you. Will, thank you for that very thorough presentation. It was really helpful. Most of my questions really have been already asked, and so my points are really, I think, some statements that I want to make while we have this table there and we're in the middle of this discussion.

I see this issue that you brought up, the sonar versus ROV estimates, and I think that brings up the point that the scale and the resolution of these different sampling gears that are being used, and that we're trying to integrate those data into something that is an absolute abundance, generate an estimate of absolute abundance, becomes extremely difficult, because you're dealing with different levels of detectability, and that scale and resolution of the different gears. It's something, I think, for the committee to think about as we look at that estimate of absolute abundance.

Then another point, Will, regarding that Table 6 that's there on

the board, is, again, I have to bring up, again, the issue of sampling fractions, and so you -- Looking at that map that you showed for the distribution of your samples, I mean, you covered a vast area, and you sampled both the northern, middle, and southern portions of the West Florida Shelf, but still your sampling fraction is just about 1 percent, and so add to this the fact that we have this very imperfect knowledge of the habitats that we're expanding these numbers to, and not just the quality, but the quantity of those habitats that are being used as expansion factors for the absolute abundance numbers, and it's hard for me to reconcile that into my brain and see how that would work. I will just stop there, and I just wanted to make those points while you were on this specific topic. Thank you, Mr. Chairman.

CHAIRMAN POWERS: Thank you. We have Steve Murawski and then Greg Stunz, and then I would like to stop the questioning there, after they get through. We have about an hour left, and I would like to spend that hour to Sean Powers and the Alabama estimates, their part of the study, and so that's my goal, is to finish after Alabama, and so let's go on then with the questions or comments from Steve Murawski and then Greg Stunz.

DR. MURAWSKI: I am just reacting to Kai's question about intercalibration of the gears, and I do think that that's a very productive way to go. You've got these long-term studies. For example, FWRI, and also NMFS, have the still cameras, basically 360 cameras, now, but a lot of samples over a lot of different habitats, and a lot of experience with that.

Of course, the important thing, in terms of going from relative indices to absolute, is understanding the zone of influence of the baited traps, et cetera, but that's possible to do. I mean, as Will's work points out, you have to be careful, in terms of actually understanding all the factors that are influencing the biomass trends and the numbers that you see, but I do think it's possible.

The NMFS study looking at the untrawlable habitat did a lot of work, in terms of the logistics of setting up field samples, and I do think it's worth a go. In fact, Ted Switzer and Sarah Grasty have an experiment going on later this year, where we're going to try to intercalibrate the fixed cameras and the towed video as well, to see if we can try to sort out some of these issues, and so I think it's a very good investment in the already-paid-for infrastructure of surveys, to try to understand how to scale them up, and I will leave it at that.

CHAIRMAN POWERS: Thank you. Greg.

DR. STUNZ: To Luiz's point, but also regarding this calibration, and, Luiz, we hear you, I think, the team, loud and clear, and we spent a lot of time talking about your issues, but avoiding trying -- You know, there isn't simply just one gear that works in all these regions, and that's the big problem, and so that leads to calibration and Kai's points.

Of course, yes, it's critically important for calibration, and it's very expensive and very time consuming, but, of course, it has high value, well beyond we discovered the scope of this study, but, with all the other studies going on, maybe we can chip away at it piece-by-piece, or it looks like Steve is on it with some of his other studies, but it's clearly something that needs to get done, and it's just our hands are a little bit tied with what we are up against with this study to further contribute time, effort, and resources to calibrations, which we couldn't do at this point.

CHAIRMAN POWERS: Okay. Thank you, and thank you, Will, for the presentation, and so let's move on then to Sean Powers, who is going to be talking about Alabama/Mississippi, and we'll turn the floor over to him. Sean.

ALABAMA/MISSISSIPPI

DR. POWERS: Thank you. I think Liese is going to drive this presentation. We're going to do this in several parts, and I am going to lead off with some background, over the overall design and some of the habitat mapping we have and why it's different from the other regions.

John and Liese are going to follow with some of the specifics of the depletion methodology and then what we did in the natural hardbottom, and then both Liese and I are going to try to control how much Hoenig talks. The overall team you see here, and Marcus Drymon participated as well, and all the peer work was led my lab manager, Crystal Hightower.

These are our three strata, and so we have a lot of artificial reef in Alabama, and also, to some degree, in Mississippi, and we'll talk about the depletion methodology, but, essentially, it's a combination of video and depletion. All of our video was done by ROV, using the same ROV, and it's slightly larger than Will's, and we also looked at natural hardbottom, and we did that by the same ROV and the same similar approach that we used in artificial reefs.

We originally had planned to do depletion on natural hardbottom, but we weren't confident in the results there, and the snapper were in a lot lower density. We did some pilot work, and John might explain that in further detail, but we'll go over -- Essentially, we used our counts in just the area of bottom to extrapolate our number.

The uncharacterized bottom, we originally planned and attempted to actually do some innovative stuff with bottom longlines and to see if we could figure out the area fished of bottom longlines, and that didn't work out for red snapper, and so Steve Murawski helped us out, and we were able to provide some additional funding for Steve to cover the uncharacterized bottom in Mississippi and Alabama.

Natural hardbottom for us is hard reefs. In our case, most of those reefs have significant elevation to them, and uncharacterized bottom is sand and mud and whatever unmapped features are there.

We have been doing this in Alabama for a while, trying to get at an absolute abundance. Our system lends itself a little easier to that, because it's a little more tractable, and we've had a fair amount of investment from the Department of Conservation and Natural Resources, the Marine Resource Division, and we've been doing this since about 2011, and, obviously, with improvements over time, but we also have it where we do a before and after snapper season, and we're trying to estimate catch from our absolute abundance estimate, and so this project, for us, is an extension of what we've been doing for a while.

Let's focus on the map on the right-hand side first, and, here, you see kind of the overview. The blue areas you see are the Alabama reef permit areas, and so that is a unique feature, I think, of Alabama, and that is -- That is an area that's approved by the Corps of Engineers for planting artificial reefs, for lack of a better word, and both the state and private parties are allowed to put artificial reefs in there.

Of the reefs, and there is thousands of reefs, and our estimates are about 20 percent are put there by the state in Alabama, and so those locations are known and published. 80 percent are put in by private individuals, and they are usually prefab, and now they're prefab approved pyramids, or cages, or a variety of things.

Technically, you need a permit. You need to go to the Alabama

Marine Resource Division and get a permit, a $25.00 permit, to put something out there, but you do not have to give the location of your reef. You can plant it wherever you want in that pre-permitted area.

That permit enforcement has been very good in recent years, but, historically, it wasn't, and there was a lot of illegal placement of artificial reefs, and so the upshot of that is we don't know how many reefs were there. We know how many permits were issued and how many the state put in, but we know there's a lot more structure, artificial structures, out there than that.

What we did is designed a side-scan survey where we divided the whole permit area into two-kilometer-by-two-kilometer grids, and then we selected a subset of those, proportionally allocated by depth, and so 50 percent of our grid selection was in shallow, 33 percent mid, and 17 percent deep, and we defined those strata in the report.

Then we side-scan it, and we find the structures in it, and, for some of our surveys, we randomly choose structures within those grids, because there is multiple structures, particularly in the shallow and mid-depth areas, where there is a lot of artificial structure.

We also find natural reefs, and there is not a whole lot of natural reef in the shallow and mid-depths in Alabama, and, really, what you see there is that the colorful multibeam imagery is from the USGS, and they mapped the Alabama Alps and Pinnacle regions, which essentially are around that seventy-meter isobath. They extend somewhat into Mississippi, or at least have been mapped somewhat into Mississippi, and mapped through most of Alabama, and so that's where most of our natural reef is.

We do get scattered natural reef in the permit zone, but the majority of it is in that area that the USGS mapped, and I will show you some video of it in a second, but, as the name indicated, Alps and Pinnacles, these are more or less structures with decent vertical relief.

The purple area is the Mississippi artificial reef areas, and the one with the red box we included, and we didn't include any of the real shallow-water ones, the inshore reefs, because they don't usually have snapper on them, and the Alabama area has about 300 known reefs, and so we will talk about the selection in a second and how we chose those grids.

That leaves a large area of Mississippi and Alabama, and so, if you go west of those blue polygons, you get into an area that is not permitted for reefs. Well, we discovered, through talking to fishermen, and also through the SEAMAP trawl survey, where they reported snags, there is a lot of structure out there as well, a lot of artificial structure that has been either illegally dumped out there, or has fell off of ships, or who knows how it got there, but there is a lot of structure out there as well.

In 2015, we gridded that whole area in western Alabama and started to include that in our side-scan survey. Again, the black-filled areas are areas that we have mapped in those two-kilometer-by-two-kilometer grids. Some of those areas, we have revisited a couple of times, to see the fate of the materials that were put in there, but, from that large pool of sample sites, we gathered all of the contacts from the side-scan, and, to be a contact in the side-scan, you have to have at least a half-a-meter of vertical relief and cover at least four meters squared of bottom.

Most structures are larger than that. The typical artificial reef covers seventeen to twenty meters squared, and it has about one to 1.5 meters relief, and so sizeable enough to be detected on the side-scan without a problem, but definitely not huge, compared to some of the natural reef features that we have seen.

I think that’s, essentially, our survey, and so our knowledge of the habitat is very different from the other states. There is no database that contains all of these artificial reef numbers. When we started talking a lot about how to do this, we also had a little bit of a problem, in that some of these data are confidential, because somebody put out that reef, and they wouldn’t like the state reporting their location, and so we had to keep some of those numbers internal, for site-selection purposes.

This is a typical artificial reef in Alabama, and you see all the way back, and I think that’s either a chicken coop or a chicken transport device, and that’s popular, or an abandoned, rusted-out cargo container, and I can’t tell from this, but this shows why we decided to use the depletion approach. This is all snapper, for the most part, and then you see the scaling lasers that were used.

If we were to do some type of perimeter survey, or even you can see that there’s fish up in the water column, we were worried about double-counting fish in the system, where we get these

large, large densities, these hundreds of red snapper around a small structure.

We have, like I said, depending on the estimates, 10,000 to 13,000 of these structures out there, and this is why we adopted the depletion methodology, coupled with the ROV index.

The next video shows you what happens with most of our natural reef, and, here, you can see that there are prominent features, and it’s not low-relief banks, and it’s prominent features coming up, and we do get scattered boulder fields, and we get a variety of things, but most are these prominent features, and you can see this one has an extraordinary diversity, compared to the artificial reef, but also a fair number of red snapper as well.

Just to drill down more into how we got the sites, what you see is that same map, but, on the left, you see a typical side-scan sonar that reports the contact, and so each randomly-selected grid can have randomly-selected contacts in it, and I will explain that.

To get more comparable to the other studies, we didn’t use that two-stage design, and we pulled all of the contacts that we had and then randomly selected the contacts from the entire pool. The contacts are usually, like I said, reef pyramids or small groups, and we do occasionally get large structures, like this bridge span, this sunken bridge span, but we don’t have many platforms or sunken structures. Most of it are small, nowadays, prefabricated structure.

We do know the type of structures we have. Like I said, most are prefab pyramids, and sometimes we can’t classify them on the ROV, maybe because they’re buried a little bit, or maybe because they’re just not recognizable, but chicken coops and pyramids are the biggest ones, and we have a series of other concrete pipes and bridge rubble, tanks, wrecks, old cars. In the 1980s and 1990s, it was pretty much a free-for-all, and, now, it’s very much controlled, the structures that are there, and the state is trying to do -- To put in some spatial planning procedures to spread out effort and to make sure there is enough artificial reefs, looking at the fate and replacement schedule for many of those artificial reefs.

That’s the background into why our sampling universe needed to be different. Like Greg said, every region has its own nuances, and ours was this, that we had lots of artificial reefs, and probably the greatest concentration of them, but their locations

are not publicly known, and so we relied on our previous surveys.

The natural bottom, we do have, from the USGS, and, like I said, it is naturally occurring more higher-relief hardbottom, and so I think, at this point, John is -- With that introduction, John is going to take over and talk about the technique that we used on artificial reefs and then natural reefs.

DR. JOHN HOENIG: Good afternoon. Sean wanted me to keep this as short as possible, and, to tell you the truth, I would be happy to just sit here and make noises like a turnip, but he also wanted me to cover some material, and so I guess I will have to speak a bit.

The basic idea is that the abundance is the amount of habitat times the amount of fish per unit habitat, and, from 2011 to 2019, we've had an ongoing survey, where they have been going to randomly-selected grid cells and mapping them, and, to date, 432 out of 1,399 cells have been mapped. In the report, at one place, it says that 22 percent have been mapped, and that's an error. It's 432 divided by 1,399, and that is 31 percent.

For each of the depth strata in Alabama, we have calculated the mean number of artificial reefs per grid cell, and so, basically, those 432 cells that have been mapped, each one has a number associated with it of how many reefs were seen there, so we can get the average of that, and we can take the variance of that, as for a simple random sample, and that's formula on the right, and note that there is a finite population correction, because, if we had mapped all 1,399 cells, then we would know how many reefs are out there, but we only did 432 of them, or 31 percent, and so you have this finite population correction, which basically reduces our variance by 31 percent.

Then how many reefs are there total in Alabama, or in a stratum, and you take the average number of reefs per grid cell, and you multiply it by the number of grid cells, and you get your total number of reefs in the stratum, with a variance of just N squared, the number of grid cells squared, times the estimated variance of the mean.

Here is the results, and we were working in waters ten to 150 meters depth, and we actually did it separately for six regions, three of them in the Alabama Artificial Reef Zone and three of them outside the Alabama Artificial Reef Zone. For purposes of estimating the abundance of red snapper, we collapse that down to just three strata, shallow, medium, and deep, but what I

wanted to call to your attention was observations outside the reef zone are to the west of the artificial reef zone, and so we had 691, 1,142, and 536 reefs, which sums up to 2,369, and we used that in the next slide, and so, if you're wondering where that 2,369 comes from, it's from our survey of the western part of Alabama waters.

There, you have the number of reefs estimated, and also the standard errors, and, for most cases, the standard errors are actually quite small relative to the estimate, and so the number of artificial reefs is known quite well.

For Mississippi, there is a list of registered artificial reefs, but we now believe it to be very incomplete, and, nonetheless, we thought, well, if we know about artificial reefs that are there, if we go there, we'll be sure to sample some artificial reefs, and the state is gridded into permit zones, and five of those permit zones were not too shallow, and so it was believed that, yes, there would be red snapper there, and so that comprises 229 sites that are likely to have red snapper, and we took a random sample of those 229.

As we got into it and realized that, actually, we don't know the number of artificial reefs very well, we decided to do some alternative calculations, where we assumed that what has been estimated to be in western Alabama is probably also what's in Mississippi, because it's approximately the same area and the same characteristics of the population of anglers, and also a similar habitat and so on. We did the calculations, assuming that there were 229 artificial reefs, and also assuming that there might be 2,369 artificial reefs.

Here, I have to say something rather embarrassing, and I wrote the section of the report on the statistical methodology that we used, and I've been working with Sean and the others on a manuscript about the previous work in the Alabama Artificial Reef Zone, where we used two-stage cluster sampling, and I copied and pasted and put in some of that language, and then, subsequently, I got confused and thought we used two-stage cluster sampling for this project, but actually we didn't.

We did sample a random number of -- Sorry. We randomly sampled a number of grid cells, but then we took the list of all the artificial reefs that we had found and put that into one master list and randomly sampled that, and so we did not use the two-stage cluster sampling equations.

I don't think that that really matters, for a couple of reasons.

One is that, as the number of cells that you have mapped approaches the total number of cells out there, the procedure we used reverts to a simple random sample, and, since we did 30 percent of all cells, that's approaching now a simple random sample.

The other thing is that we went to 117 sites, and there are 432 cells, and so that's about one site per cell, and so that's assuming that the number of artificial reefs per cell does not vary hugely. If it were the case that most of the artificial reefs are in just a very small number of cells, then we would have a problem, but, to handle cluster sampling methodology, you calculate the within-cluster and between-cluster components of variation, from which you have to have at least two observations per cell, for at least some of the cells, to get the within-cell component of variance.

I suspect that, in almost all cases, we have one observation per cell, and, if we had two observations or more in a few cells, we would get a very poor estimate of the within-cell component of variability. It turns out, if you ignore the within-cell, within-cluster, component of variability, you get the best variance, and so I don't think this is really a problem, but we'll do a few more calculations to demonstrate that.

We took a simple random sample of all known reefs in our inventory, and, at each reef site, we collected the type of data you use for index removals, and so, in the next slide, I will explain index-removal.

Basically, we were using a remote-operated vehicle, an ROV, with a camera to count fish, and we were also using vertical longlines to deplete the population, and we had two kinds of survey sites. We had calibration sites, where we did the full monte, and we did the whole procedure, and then we had production sites, where we used just a subset of the data collection.

At the calibration sites, we had an ROV count, which we're treating as an index of abundance and not as an absolute number of fish, and then we used a vertical longline three times, and so three sets, to deplete the population, and then we used the ROV again to get another index of abundance, and so, supposing the ROV MaxN count goes from forty before to twenty after you fished and removed seventy-five fish, forty going to twenty is a 50 percent decline, and so we're inferring that half the population was removed when we took out seventy-five fish, which would imply that there had been, initially, 150 fish. Since we

saw forty out of an estimated 150, that would be our calibration factor. It's the estimated proportion of fish seen in a MaxN count, on average.

Then we also had production sites, because the deployment of the ROV takes an hour, and so to go to a site and have to spend two hours just to get the two ROV counts, plus the time for the vertical longline, that means you can only go to a very small number of sites per day, and so, to collect additional information, we had what we call production sites, where we did the ROV count, and then we did the three vertical longline sets, and those can be done rather quickly.

All we did was use the ROV counts, and I'll explain why in a minute, but, basically, if you have an ROV count, and you have that calibration factor that you got from the calibration sites, then, essentially, the production sites, the single ROV count, the MaxN count, is providing information on the abundance.

We didn't use the vertical longline, for two reasons. One is that the trend over the three sets was very weak. You could see a depletion, but you really couldn't judge the slope of the regression line of catch per set versus set number. Despite that, in total, the three sets were enough to deplete the site that the second ROV count was lower than the first, in almost all cases, and so it was kind of disappointing that the vertical longlines weren't more useful.

The other thing is that the vertical longlines see a subset of the population, and they don't see the small ones well, and, since we were trying to estimate the population of two-plus, which seemed to be what we were seeing, based on sizes of fish measured in the ROV counts, we thought we will rely on the ROV data as our primary gear for estimating the abundance.

Now, you might wonder, is this legitimate if the vertical longline is size selective, and the answer is it is legitimate, and you get an unbiased population estimate for the population seen by the ROV if the ROV is not size selective, and so we assume that the ROV will see all fish age-two and above equally well, and then it doesn't matter that the vertical longline is not unbiased, and it doesn't matter that it's selective for larger fish.

The table in the lower-right is basically a summary of what did we have for sample sizes, and so you can see that, in total, we had 117 sites in Alabama and thirteen in Mississippi, for a total of 130, but we were really quite concerned that our

calibration factor should be precise, because that's what we're using to convert the ROV counts into absolute abundance, and we did have data on thirty-four sites that were visited in 2014 and 2015, and so we analyzed those data along with the data collected for the Great Red Snapper Count, by assuming that the catchability coefficient in 2014 and 2015 was the same as it is today, but that the abundance was different, and so we estimated one catchability coefficient, or calibration factor, and two population sizes, or, actually, four population sizes, because we had four strata, three depths in Alabama and one in Mississippi, and so we had eighty-eight stations where we did the full calibration study. I think that's the last of my slides.

DR. LIESE CARLETON: Thanks, John. I guess Sean and I didn't do a great job at keeping at John quiet, because John actually stole a lot of the things that I was going to say, but that's okay.

The rest of this talk, I'm going to be discussing in more detail how we formulated our estimation model. As it's part of a general class of depletion methods, I thought I would introduce some of the general notation, to give you an idea of the reasons for how we would up with our final model.

The theory here, usually, in practice, your index-removal method assumes some distribution for your indices, and, as John said, when I say "index", I mean the ROV MaxN counts, and so the expectation for the counts at a given site, i, is proportional to the initial abundance at that site by that calibration factor, and, similarly, your expectation for your second index is proportional to the remaining population, the initial minus what you removed.

In theory, this could be used to obtain estimates of abundance at each site that you visit, but it's not practical, and this is because it's infeasible if the change in your ROV is less than or equal to zero. I believe, if you're looking at the PDF, there's a typo. What you're seeing on the screen right now is correct, and it should be less than or equal to zero, and my apologies for that, and that should make sense, because, if your index goes up, meaning that the change is less than zero, then you're going to get a result that's a negative abundance estimate, and, if there isn't a change, or if it's equal to zero, then your population is going to be infinite.

That means that that's a large amount of data that is not as meaningful as it could be, and this could occur for any reason,

like just random observation error. To get around this, if we assume that the calibration factor is constant over sites, then we can work with the aggregate data, using the means of the two ROV indices and the mean removals, and so that's the next set of equations that follows from above.

For sites within each stratum, the expectation of the mean of the first index is proportional to the mean initial abundance by that calibration factor. The same for the second, and the only thing I would like to note here is, in this, we were treating the removals as fixed, and John touched partially on why that is, and I will explain more in the next slide.

This brings us to our model formulation, and it took many iterations to come up with this, and so, in many traditional count-based modeling approaches, you might usually assume that your index has a Poisson or negative binomial distribution, and both of those have limitations on the variance component, but, as we're now working with means of two random variables, we can consider that the mean MaxN indices have a bivariate normal distribution, which is advantageous, because the variance can be estimated based on the data, rather than relying on theoretical properties of distributions.

In addition, because the two indices are taken from the same site, there is a correlation between the two, which will translate to the covariance that will then reduce the overall variance, and so that's why we're using a bivariate normal.

In our model, our means of variance are calculated as for stratified random sampling, and the next two equations -- The first one here is the expectation of your index, and it's your stratified index for either pre or post-removal, and, essentially, it's a weighted average. It takes your mean index within Stratum h and is weighted, and the stratum-specific weight is the proportion of reefs in that stratum compared to the total reefs in all strata.

Next, we have the variance/covariance component for the stratified mean ROV index, and there is only a couple of things that I want to point out here. The N_h over N squared, that's just the weight squared, and the remainder of that is either the variance or covariance, and you will note that the denominators of those are different in some places, and that has to do with the calibration versus production stations that John mentioned, and so some of our sites had only a single ROV index, whereas some had two. I will leave it at that, but I can explain more about our variance formulation if asked.

We're assuming that our removals are fixed and not random variables, and so we explored the idea of integrating a removal-type method with the index removal that's shown here, but, as John said, we saw a very weak trend, if you looked at the mean of the three consecutive vertical line sets, and so it would be very imprecise, and we would also have to make some -- We would have to resolve some assumptions about the size selectivity, because we have seen, time and time again, that the vertical line catches larger fish than -- Or it tends toward larger fish than the ROV is seeing.

The last assumption to mention here is that we're assuming the calibration factor is constant over time strata and at sites within strata, and we have done evaluations, and so, for example, the over time piece, we looked at if we analyzed our data separately, our pilot data versus the 2018/2019 data, and the changes were minimal, and we tried to look at constant over strata, but, for some of our strata, the sample sizes were too low to really tell, and so we're assuming that it's constant.

Here are the results from our depletion study over artificial reefs, and we estimated a calibration factor for the ROV of 0.122 and associated standard error, and the table here shows the mean density and its associated standard error for our four strata, and so that's mean fish per reef. We also show the weights we used when stratifying and the number of reef samples.

Finally, here at the bottom, we present two estimates for the total population on artificial reefs in this region, and the difference between these two estimates is the first assumes that Mississippi has only the published 229 reefs in the permit zone, which is surely an underestimate of the number of reefs.

The second assumes that Mississippi has the same number of reefs as western Alabama, 2,369, and it also assumes that the sites that we sampled in Mississippi are representative of the reefs in Mississippi.

That was what we did for artificial reefs, and I only have a few things to say for the rest of this, and I will be going over the natural hardbottom habitat, but, really, this was all Sean's work and Sean's idea, his team. As Sean said, natural bottom is very rare in the shallow and mid-depths, but it's more extensive in deeper waters, and so there's no stratification going on here.

The first task was to estimate the area, and that was done with

the most comprehensive data available, which was the USGS multibeam study, and they estimated that that red shape there has a total area of 1,625 kilometers squared, but, within that area, it's known that there is a combination of hardbottom and sand habitats, and sand we would consider as the uncharacterized bottom, and so we did not want to include that in our estimate of the natural hardbottom.

To get to a finer scale, this is paired with the side-scan sonar data that Sean mentioned previously, and they looked at areas that they had scanned within where USGS had surveyed, and, from that, they came up with a 13 percent hardbottom estimate, and so, from that, you can figure out that the total area of natural hardbottom is 211 kilometers squared.

The next task was to estimate density, and so, initially, we wanted to use depletion methods here, to be consistent with the artificial reefs, but, during our pilot studies, we realized that it wasn't going to be feasible. The counts were too low to observe any sign of depletion, and it's possible that this could be due to individuals immigrating from nearby structures during the course of the depletion experiment, and we're not sure why it is exactly, but, in any case, we needed an alternate method to estimate density.

The team took the MaxN from the ROV and standardized it to account for the average area surveyed, and, when I say that, it's not taking the MaxN and then multiplying it by the area, but it's actually dividing by the average area, and so, in other words, the MaxN, which is considered conservative, because it's the minimum number of fish that could be at that site, is divided by the average area surveyed, which is a little over 400 meters squared, and that's how they obtained number of fish per meter squared.

The mean over thirty-two sites that they sampled was 8.42 fish, and that's after taking into account the 250-millimeter cutoff, and so, dividing by the average survey area, that gives a density of 2.02 fish per hundred meters squared. The last step is to calculate population, which we estimate to be 4.27 million on natural hardbottom habitat.

This is our final slide, and it just shows a summary of the population estimate at the three habitat types. The first line is for artificial reefs, and, again, there is two numbers there, depending on the assumption you make about number of reefs in Mississippi, and so somewhere around 1.5 or 1.8 million, and we just talked about natural hardbottom, and, from the

uncharacterized bottom group, we pulled that there were 4.1 million in that habitat type. At the bottom, you can see that, again, depending on your assumptions, there's about ten million fish in this region. This is our last slide, and so I think, at this point, I'm ready to hand-off for questions.

CHAIRMAN POWERS: Thank you very much. I will open the floor to questions then, or, John, did you want to make a comment?

DR. HOENIG: If I might, one thing that it occurs to me that we forgot to say is that there seems to be a lot of concern about whether or not all the sources of uncertainty were captured in the calculations. In what we did, we included uncertainty in the number of reefs, in the calibration factor, Q, and in the sampling error for the mean counts, before and after the removals, and so, undoubtedly, like all studies, we have underestimated the true variability, but, in this case, I think we tried, conscientiously, to include as many sources as we could, and that's all I wanted to say.

DR. POWERS: Joe, I will add to John's comments, and so that is the reason why, if you look at Rob's CV, and Lynne's, I think they are different than ours for the artificial reef. What I provided to Rob was the list of locations, along with the final estimate, and so the error he's reporting is probably just among sites, whereas the error we show here, which is higher, includes these multiple sources.

CHAIRMAN POWERS: Thank you. Hands up, we have Benny Gallaway. Let me recognize Mary Christman, and then we'll go back to Benny.

DR. CHRISTMAN: Thank you. That was very detailed, and I appreciated it. I do have a few questions, though. In the overlap area, and, in other words, that area where you have the orange box for natural hardbottom overlapping with the artificial reef, the AARZ, I think you called it, how were sites within that were selected for natural hardbottom handled? In other words, how did you decide what to do when you got out to the site? It could have had a reef, or it could have been natural hardbottom.

DR. POWERS: We knew that ahead of time on the contact report, and so that grid cell would have been characterized, and we can tell, for the most part, the difference between the artificial reef and a natural reef. In reality, those deeper depths have almost no artificial reefs, and it's very rare that we find artificial reef that deep, and so that contact would have been

it, and so our natural reef sites were a random draw, but only -- Not through the complete USGS area, but only through those areas that we had side-scan.

DR. CHRISTMAN: Okay. That helps a lot. My next question is just a simple one, and I'm not sure. Did the vertical longlines ever saturate when you were doing the removal studies?

DR. POWERS: We kept track of the number of baits returned, and so, when we deployed the ROV, we show the vertical longlines, and we have GoPros on the top of those, looking down, and we also count the number of hooks that return with bait, and, often -- We had a few instances, probably I would say 10 or 15 percent, where all baits were either gone or the hook had a fish on them. The deployments were only five minutes, but, usually, we turned with bait.

DR. CHRISTMAN: Okay, because I was wondering, and, if you had situations where they saturated, your estimate of the actual removals would have been bounded from above, and so it doesn't sound like you hit that very often.

DR. POWERS: I will let John answer that, because I don't think that matters, as long as you know the number of fish that you pulled up.

DR. HOENIG: It doesn't matter for the index-removal, and I think Mary is asking why the attempt to do a removal estimator from the longline data itself not work, and my impression, and I can go back and check, but my impression is that we did not have a gear saturation problem.

To tell you the truth, I looked at the number of red snapper caught, and it was never anywhere near the number of hooks set, but I didn't go back and check, but did you have say a shark that went down the line and just removed one bait after another after another after another, which would essentially be like gear saturation, and it's like, oh, you're not catching more because the number of hooks is limited.

DR. CHRISTMAN: Or if other species are more attracted to the longlines.

DR. HOENIG: Yes, and it's mostly -- You do get sharks and red drum and rays and other things, but it's mostly red snapper, and, from what I saw, and, Liese, correct me if I'm getting this all fouled up, but, from what I saw, it didn't appear that it was a simple matter of gear saturation, and I think maybe it was

just not enough hooks to actually see the trend well.

DR. POWERS: I think it has to do with something that Will was relating from his region, in that a lot of our fish were small, and they may not have been fully vulnerable to the vertical longline gear, and they were to the ROV.

DR. CHRISTMAN: That was discussed in the document, I believe. Then my final question is to the natural hardbottom habitat. You took MaxN from the ROV, but you standardized it using average area surveyed, as opposed to the area actually surveyed by that particular ROV sample, and I would expect the area surveyed to be pretty variable, depending on turbidity, and so I was just curious as to why the decision to use average area.

DR. POWERS: Originally, and we conducted the survey just like it was an artificial reef, and so we did not do a point survey. We go to the one side of the structure and park the ROV five meters away, and we count the number of fish, the MaxN in the area, and we go around to the other side of the structure, and then we repeat that again, and so we're not counting all the way around, and that's how we get our MaxN. How we calculate the area is that the average, and you're absolute right, of that donut that we cover.

DR. CHRISTMAN: Okay, but that was the artificial reefs, or was that -- The donut was actually used on the natural hardbottom as well?

DR. POWERS: Yes, and so, because our systems, like I said, aren't so much like Will's or Texas, where there is vast expanses of low-relief banks, and it's pretty much pinnacles or alps or some features, and we do get scattered boulder fields, but usually it's some prominent feature.

DR. CHRISTMAN: So you essentially sampled it similar to the artificial reef. Okay. Thank you. That helps a lot. I'm done.

CHAIRMAN POWERS: Thank you. Benny, are you on yet?

DR. GALLAWAY: My question, and I have several questions, but one is I was curious as to why the 150 to 160 or so offshore oil-and-gas platforms weren't included in the census. That's question one, and then, on the hardbottom habitat for the pinnacle reef, I think of the pinnacles as being tall, steep-sided structures, and the picture I saw in the slide that I saw at the beginning looked more like a large boulder field or

something, and I am wondering about the 2.02 density estimate, which just seems phenomenal, especially for that area, in light of the existing information about the pinnacle systems, and so I was just curious as to what proportion of that 211 square kilometers consists of the tall, steep-sided peaks, and would you care to comment, or elaborate more, on the density of 2.02 fish per hundred square meters? Thanks.

DR. POWERS: On the first question, we did not exclude oil-and-gas platforms. They would have been treated just like any artificial reef, but it's just that, probabilistically, they just didn't get drawn, because we don't have that many off of Alabama, and so we did not have a separate strata for oil and gas platforms.

On the habitat, the natural habitat, that is definitely one of our limitations, where we didn't have better high-resolution maps and more side-scan. In our area, that we overlapped our side-scan and the USGS survey, those types of what you saw on the video was the dominant form, and you can see that they function essentially like artificial reefs in our system. As far as the density of 2.02, that is higher than the other regions in our survey, compared to other regions, but that is what we saw.

DR. GALLAWAY: So that density estimate would relate also to the pinnacles and that it's not a function of a small fraction of habitat being of that nature?

DR. POWERS: Again, if we had better habitat maps, we could make -- We wouldn't have to make that assumption, but that's the assumption we have to make now.

CHAIRMAN POWERS: Okay. Thank you. We've got two more on the list here, seeing as how we're getting close to the end, I think, for today, and we'll keep it at those two, and, if somebody wants to bring us some more things tomorrow, feel free. Let's begin with Dave Eggleston, and then Luiz Barbieri will finish up.

DR. EGGLESTON: Thank you. I guess, to follow-up on what Benny was focused on, in terms of looking at Table 7 and the Alabama/Mississippi natural and artificial reefs, and I followed the logic, and just looking at the values, and so I see where you came up with the 2.02 and then the average of 170 for mean density per structure, but what was stunning to me is, when I move up that table, for Louisiana artificial reefs, deep, mid, and shallow, especially the mid and shallow, where you have one

to two orders of magnitude higher densities than any of the other values in the table, and I was just wondering if you could elaborate on why those values are so high.

DR. POWERS: Can somebody pull up that table, so I can make sure that --

DR. EGGLESTON: It would be page 88. If you can go down to Louisiana, you see 1,399 for a mean density, and 2,733.

DR. POWERS: I see that. Then your question is?

DR. EGGLESTON: I mean, it's just really -- Those numbers are eyepopping, compared to the other numbers in that column, and so I just wanted to try to understand why those numbers are so high.

DR. POWERS: Are you questioning the Louisiana number?

DR. EGGLESTON: The Louisiana number, that's correct. The Louisiana number.

DR. POWERS: That is a question for Greg Stunz. I can speak to the Alabama/Mississippi numbers.

DR. HOENIG: I think it has to do with the size of the artificial reefs. In Alabama, a typical artificial reef is a chicken coop, and it's not an oil platform, and there's a huge difference in size.

DR. STUNZ: To chime in, that's exactly what it is, John. It's the size of the artificial reefs, which are predominantly oil-and-gas platforms in Louisiana, and that's why that number is so high.

DR. EGGLESTON: Okay, and so that's real.

DR. STUNZ: Dave, that number is also pretty substantiated with other reports in literature, and that's not an unusual finding, and, in fact, it may even be a little on the low side.

DR. GALLAWAY: Is that not a total count at those reefs and not a density per hundred square meters?

DR. STUNZ: Benny, I'm sorry, but could you ask me that again? It cut out for me.

DR. GALLAWAY: I think the Louisiana -- Is that a total count, a

total number of fish, at an artificial reef in Louisiana at those depths, at the oil platform, whereas, for Alabama and the other, I think they're showing density per hundred square meters.

DR. STUNZ: That's correct, Benny. If you scroll up, I need to look at the title of that table, real quick. We're trying to simplify this into one table, so there's not a million rows or columns, but it's mean density of a hundred meters squared or by structure, and so, if it's an artificial reef, it's the number per structure. If it's others, it's per hundred meters squared.

CHAIRMAN POWERS: Thank you.

DR. STUNZ: That's 1,400, or 1,399, per structure and not -- It's per structure and not per hundred meters squared.

CHAIRMAN POWERS: Thank you. Back to the list, I see David Chagaris is on there, and so I guess he gets the last word, but first Luiz.

DR. BARBIERI: Thank you, Mr. Chairman, and thank you, Alabama team, for the great detailed presentation. It was very, very helpful, and I have a few questions. One is about how you estimated your depletion factor for these habitats, using the depletion method, and I know that Liese went into quite a bit of detail to that, but I was wondering about averaging sites where you had negative second counts, versus positive second counts, and how that may have impacted the final results. I don't know who on the team would like to take that one.

DR. POWERS: I think Liese or John can do it, and I don't think it occurred very often, and I know, Luiz, when I gave a presentation to the SSC about this method earlier, before John and Liese joined me, I think it was the case in about 20 percent of the cases, where you actually didn't see a depletion, but I'm not sure, and Liese would be much more familiar with the numbers right now.

DR. CARLETON: Yes, I can take this one. The nice thing about the means is that it takes out that potential negative change, and another thing is we never had negative counts, or a negative index, and so, no matter what, your mean is always going to be positive, but using the means better helped show the decline for the mean abundance, and so the calibration factor -- It's not a mean, and it's just the estimate of the calibration factor, assuming that it's the same for all sites.

DR. BARBIERI: Right, but what I'm wondering is, especially if you didn't have that many happen, where the second number turned out to be larger than the first one, why didn't you just remove those observations? I mean, it looks like that, in those examples, the method that you're trying to use didn't actually work.

DR. HOENIG: It doesn't work for estimating the abundance at a particular site, a particular chicken coop or whatever, but it works for the aggregate. Basically, imagine that you go out and you see, well, what's the relative abundance, what's an index of abundance, before the fishery, and you get forty, and then the fishermen go wherever they want, and they deplete, and then you go out to another set of stations to see, well, what's the abundance now, and you're seeing, well, in aggregate, the catch rate has gone down, and so, okay, that harvest has caused the abundance to go down, and you can calculate so what was the initial abundance.

That's basically what we're doing, but what we're saying is you can't do it by one station and say the initial catch rate that we got at this station was forty, but then, at the end of the season, we went back to it, and, gee, we caught forty-two fish, and that means that the abundance went up, and it may have gone up at that one site, but you're interested in the whole population and what's going on, and so it works for a collection of sites, and it doesn't work very well for an individual site.

DR. POWERS: John, if you did what Luiz was suggesting, that would eliminate the sites where you -- It would only include the sites where you saw a decline, and that would decrease the Q, and, thus, increase the estimate of number of red snapper.

DR. HOENIG: That would be a biased estimate, and I wouldn't recommend that.

DR. POWERS: It would be biased high though.

DR. HOENIG: Yes, because you're basically saying that I'm only going to accept the data if it gives me a higher number.

DR. BARBIERI: Right, but there is a level of sampling error, right, John, that is taking place at that resolution level of the individual reef, and how is that being taken into account?

DR. HOENIG: Sorry, but what's the question?

DR. BARBIERI: If you, at the individual chicken coop, or

whatever little reef you're looking at, and you do a count, and then you do the depletion, and the second count is higher, and so, when you average over the entire area, or time, whatever you're doing, to come up with that estimate of the depletion, there is a level of observation error, right, for those sites where the estimate ended up being higher in the second count, right?

DR. HOENIG: Right.

DR. BARBIERI: I am just trying to find out how was that accounted for in the actual error estimates.

DR. HOENIG: Okay. Basically, what we were doing is we were working with means over all sites combined, and so we would basically say, if you go out to forty sites and you lower your camera, you're going to see, on average, forty, sometimes more or sometimes less, depending upon both variability among sites, which is okay, or measurement error, that you just happen not to see so many or you happen to see a lot, but you still get a mean count before, and you get a mean count after, and if, at all those sites, you did some removals, and they don't have to be the same number removed, it works out mathematically that you just have to remove enough from enough sites that the second count is lower, and you get an unbiased estimate, and you have the right variance, because you have, basically, observed the variability among the sites, and so you have a variance of a mean. It's the variance of the individual observations divided by N.

DR. BARBIERI: Okay. Got you.

DR. CARLETON: I was just going to add that I think John and I have actually looked into, and we have a small proof that doing it this way provides an unbiased estimate, and I don't have it right in front of me, and so I can't give it to you, but I can send it to you, if you would like.

DR. BARBIERI: That sounds good. Thank you. Then one other question. Back to that Table 6 on page 84, and, again, it's the way that sampling allocation was structured and the influence of that sampling allocation on the total number, and this is not really within the artificial stratum, but, if you look down there, between natural and artificial, the density, the mean density, of the artificial reefs is a lot higher than it is for the natural reefs, right? So the artificial reefs were sampled at a rate that was six-times higher than the natural ones, right?

DR. HOENIG: Yes.

DR. BARBIERI: There were just thirty-two samples of the natural bottom and 198, but then, when you go and multiply across there, all you have is basically multiplication of the area of the number of samples versus the mean density, and you end up with those numbers, without accounting for the fact that reefs where you had much higher density were actually sampled more intensively than the ones with lower density. Was that taken into account by the sampling design?

DR. HOENIG: I think what you're asking is, with 20/20 hindsight, would we have done it the same way, and the answer is we didn't really have great information on the amount of habitat, and we were not -- We just had to rely on what had been collected in the past, and so I don't think that our allocation was the best, but it was the best that we could think of at the time that we designed the study. It's easier now to look back and say, why didn't you sample that stratum more heavily than this one, and, yes, I wish I had more information back then to design.

DR. POWERS: That is the case, that, if we had to do it over again, we would reallocate that, although realize that, in our system, the natural reef is very deep, and, hence, very costly to get to, and so it isn't just a one-for-one exchange.

I mean, we would lose ten artificial reef sites for every natural reef, and that doesn't mean that we wouldn't do it, but that also speaks to Benny's question too, and we would definitely have gone more towards including more reefs, natural reefs, in Mississippi, but ship time is expensive, and we only had a limited budget, and so, yes, we would do it differently.

Like always, I see your point, although those samples, those thirty-two, were random samples, and we had a lot of -- We had, whatever, 11,000 possible natural reef contacts in there, and so that thirty-two is a random sample of that, and it has a variance, and, now, that speaks to the problem, or the issue, in all regions, that, yes, we're only sampling a very minute fraction of the habitat, but it's random, and it has a variance with it that is reasonable.

DR. BARBIERI: Right, Sean, and my point is that, again, I think that, as far as estimating indices out of this work -- I think that, when you limit your indices to a specific habitat there, I mean, they are indexing abundance at those habitats, but I think

trying to integrate these estimates that were unweighted, integrate the different habitats, is generating numbers that, unfortunately, the way I see it, do not really represent a good estimate of the total absolute abundance.

DR. POWERS: I guess we'll have a lot more discussion on that, but I go back to my point that we randomly chose these, and were there enough? Would it get more precise with more samples? Sure, but the question is would the mean change?

DR. BARBIERI: Okay. Thank you.

DR. HOENIG: I think a point is that things were weighted appropriately, and so I don't think we're talking about a big bias, and it's only a question of whether we could have been more precise if we had allocated our sampling differently, and that's always the case, that, yes, with 20/20 hindsight, you can design something better.

DR. BARBIERI: John, I don't want to prolong the discussion, but this bound to create a directional change that indicates bias in the total absolute count. I mean, it's almost like I want to estimate the proportion of people in St. Petersburg who go to the Latino market, I mean to go to supermarkets or actually go to the Latino market, and I'm going to find a lot of Latinos there, because that's where they go, but they may not be representative of the total number of supermarkets in the city. I can go there for efficiency, but I have to know --

DR. HOENIG: I would argue, Luiz, that what you're saying is not correct, that what we did was unbiased, because we did a probability sampling design that causes the results to be unbiased, and so I stand by the work, and I think that you're mistaken.

DR. BARBIERI: Sure, and I will drop it here, but I will send you some information on this and how the weighting of those estimates can be applied in a way that they generate unbiased estimates separately, but --

CHAIRMAN POWERS: I think we've gone through this enough now. We're trying to finish up here. Dave Chagaris was going to get the last word, and so I'm going to give the floor to him then. Thank you.

DR. CHAGARIS: Thank you, and I will be brief, because I think a lot of my question was covered by the previous conversation, but I think maybe there's a couple of points of clarification that

need to be made regarding sample sizes. In Table 6, it's listed for artificial reefs, and it's listed at 198, but, in the presentation, it listed 130, and, also, in the report, it was described that what you're calling the calibration sites, or the index and removal method, it says that that was conducted at every tenth site, but, here, it appears that it was done more frequently, at forty-four out of 117 sites, and so I think it would help to clarify those sample sizes and as well as the methodology.

DR. HOENIG: You are correct that we originally thought to do one out of ten as a calibration site, and that was the plan, and, in fact, we wound up doing more, because we kind of rethought it and said, you know what, if we don't get a really precise calibration factor, then the whole results are going to suffer, and so we increased that, and the report is not clear about that, and that should be fixed, and I will look into the 198 and see why that's there.

DR. POWERS: The sample size, I can answer that, John. The sample size included -- Like Will, I used the opportunity to conduct a parallel study, and so I sent Rob both what we call the Texas A&M, the Great Red Snapper Count, plus our regular annual runs, and John and I, after Rob had those numbers, discussed it, and we wanted to stay with just the ones that were designed for this study, and so that will change Rob's numbers slightly, as you can see the overall estimate, but we'll straighten that out, but, in our case, we know where those additional sites are from.

DR. CHAGARIS: Okay, and then I have another question. I'm still concerned about appropriately capturing the uncertainty in the variance if you're using a constant calibration factor across all the sites, but I'm also curious, and what's the difference in the calibration factors from the Great Red Snapper Count versus the pilot study?

DR. CARLETON: I can answer that one. The difference between the pilot study and this one, if I analyze those data separately, they were only different from each other by -- I think one was 0.11 and the other was 0.14, something like that, and, about the variability, we do have a calculation. We have an estimate of the variability, and how it's applied could be concerning, because it doesn't seem to be applied, and so I will leave it at that.

DR. CHAGARIS: Okay. Then, also, with the sample sizes in the variance estimate, you used both the calibration and production

sites as the number of samples, and, I mean, it seems like it might be more appropriate to only use the calibration sites, sample sizes from the calibration sites, where you actually had the calibration factor estimated, and so I'm wondering if the sample sizes might be inflated there for the variance estimation.

DR. HOENIG: No, they're not, because we did this in a likelihood framework, and so, for each bit of data, you are feeding in either the raw data or sufficient statistics, and so it is taking the sample sizes for each part into consideration in the appropriate way, and so it is appropriate.

DR. CHAGARIS: Okay. That was all I have for now, and I'm sure we'll come back to this.

CHAIRMAN POWERS: Okay. Thank you. Thank you to everybody and their contributions today. We will go back and go at it again at 9:00 tomorrow morning, and, if there are updates to things, questions, that we've already brought up today that people -- Some of the estimation team, if they have that information tomorrow, we would certainly accept that, in terms of questions, but, at this point, I think we've reached our limit, and so I'm going to adjourn, and we'll meet again tomorrow morning at 9:00 a.m. Eastern Daylight time. Thank you very much.

(Whereupon, the meeting recessed on March 30, 2021.)

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March 31, 2021

WEDNESDAY MORNING SESSION

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The Meeting of the Gulf of Mexico Fishery Management Council Standing and Special Reef Fish and Socioeconomic Scientific and Statistical Committees reconvened via webinar on Wednesday morning, March 31, 2021, and was called to order by Chairman Joe Powers.

MR. RINDONE: I've got a little thing that I need to read, as it regards the peer review process versus the SSC meeting, and so the council would like to clarify the nature of the peer review being conducted at this meeting.

At its January 2021 meeting, the SSC made a motion to have

independent reviewers assist with their peer review of the Great Red Snapper Count, as the relevant results of this study would be incorporated into a red snapper interim analysis.

Thus, this meeting was set up with two distinct components. The first is the peer review of the Great Red Snapper Count, and the second is the formal SSC meeting to evaluate the interim analysis completed by the Southeast Fisheries Science Center.

A number of SSC members have participated in the development of the Great Red Snapper Count. Under National Standard 2, which addresses formal peer review, these SSC members have been asked to abstain from any motions and voting regarding whether the absolute abundance estimate from the Great Red Snapper Count, and its variance, is reliable and consistent with input data and population and biological characteristics for red snapper in the U.S. Gulf of Mexico.

The peer review evaluation of the Great Red Snapper Count will thus be limited to the SSC members not directly involved in the Great Red Snapper Count and the three independent reviewers contracted by the council. Once this evaluation is complete, the second part of the meeting will serve to discuss the analyses completed by the Southeast Fisheries Science Center for the red snapper interim analysis, and this is where the formal SSC meeting begins.

During this portion of the meeting, the SSC members who participated in the Great Red Snapper Count are again asked to abstain from any motions and voting on whether the analyses represent the best scientific information available and/or are suitable for use in generating management advice, in keeping with historical SSC practices regarding the evaluation of one’s own work.

After the deliberation of the best scientific information available, these SSC members that served as investigators are asked to participate thereafter in any motions regarding recommending catch levels to the council. Does anyone have any questions about this before moving forward?

DR. POWERS: Ryan, I do. It sounds like this is just what you told us yesterday, right?

MR. RINDONE: Yes, and it is almost exactly what I told you guys yesterday, and I’m just trying to make it clear for some who apparently did not understand.

CHAIRMAN POWERS: I would -- You should probably post it someplace, so that we can actually see the written version of this.

MR. RINDONE: I can do that.

CHAIRMAN POWERS: Again, my feeling is we are going through a review process that has been scientifically driven these few days, and, come tomorrow, when you get to the point of actually integrating it into the management, or the scientific advice for management, then those sorts of issues kick in, but, in terms of people's participation now, I think we have been entirely open, and there has been no problems about bias and things like that, and so I would encourage people to participate today, and probably early tomorrow, as they see fit. Thank you.

MR. RINDONE: Thank you, everyone, for your continued professionalism. You guys are great.

CHAIRMAN POWERS: I take it that we're ready to start then, and there were some other items that came up. First off, I got some email traffic between John Walter and Carrie Simmons about a document that might affect how we the SSC might deal with things tomorrow and Friday, in terms of the catch advice, and, either Carrie or John Walter, can you kind of bring me up-to-speed? There's a document that may or may not be available now, that we may or may not want to read?

EXECUTIVE DIRECTOR SIMMONS: Dr. Powers, we asked for the Gardner et al. paper that was cited in the Red Snapper Count interim analysis catch advice, and I'm not sure if that's available yet. I have not received it, but I believe there was going to be some type of summary, at least of the methods, and potentially the key results, that the SSC may be interested in.

CHAIRMAN POWERS: Okay. John maybe can answer this, John Walter.

DR. JOHN WALTER: Thank you, Chair. The document should be available soon, today, and we're just going through a couple of reviews from a number of people, and that document will help us to determine where the fishery is fishing right now, and then we'll have another presentation that will have some analyses on possible scenarios for where the recreational -- The first document will focus on the commercial fishery, and it uses VMS data, and so that's pretty precise, and we have a good handle on where that fishery is currently operating in space.

The recreational fishery, we have to make a few assumptions, and that's going to be very pertinent to considering the advice framework that the Center will present tomorrow, and so these are additional analyses that have really been ongoing to try to characterize how the fishery might be able to fish on the large biomass that's out in the uncharacterized bottom. Thanks.

CHAIRMAN POWERS: Thank you. Kai.

DR. LORENZEN: This is just a question, and I was looking for Steve Murawski's presentation, and I think he sent it, but I can't see it.

CHAIRMAN POWERS: I would also note that John Froeschke sent out some -- There's some difficulty with the website, about security, and he promises that, if you just keep clicking and going ahead, that nothing bad will happen, and so far, with me, that that's the case.

DR. LORENZEN: I had the same, but it doesn't seem to be on the website, and so this is not a security problem, and so I was just wondering whether it was available, and, given the importance of the uncharacterized bottom, it would be great to have that available. Thanks.

CHAIRMAN POWERS: We would like to see that. Another thing is, yesterday afternoon, there was a fair amount of debate about the -- In the Alabama/Mississippi, the longline depletion data, and Dave Chagaris asked for some details about that data. Liese Carleton agreed to provide that information, but Dave will not be available until 10:00 a.m., and so, Liese, I don't really want you to present the information now, and I want to wait until he comes back, but, anyway, we'll keep that on the docket. Liese, is it available, or, John Hoenig, and maybe you could answer real quick?

DR. CARLETON: I am making it as we speak.

CHAIRMAN POWERS: You're what? Excuse me.

DR. CARLETON: I am making it as we speak.

CHAIRMAN POWERS: Okay. Thank you. Steve Murawski, relative to his presentation, I presume?

DR. MURAWSKI: Just to Kai, Ryan has that presentation, and I think they're just trying to figure out how to upload it.

MR. RINDONE: Dr. Murawski is correct, and we've received several iterations of several presentations, and so most got lost in the mix at some point yesterday, but it will be posted here in mere moments, and so the website security issue has also been resolved, and so, if you guys just refresh your page here in a moment, you will be able to see all the freshly-posted things, hot off the press.

DR. LORENZEN: Wonderful. Thank you.

CHAIRMAN POWERS: Thank you. Then, finally, Greg Stunz had indicated that he wanted to cover some quick issues, and I'm not sure how quick they will be, but, before we get into Greg, there are several other people that want to speak, and so let me go for that. Mary Christman.

DR. CHRISTMAN: This is related to what I was asking. The other presentation that's missing is the primary analysis by Rob Ahrens, and so I assume that either he will be sending it when it's time, or maybe Ryan already has it.

MR. RINDONE: Dr. Christman, I do not have that presentation yet.

DR. CHRISTMAN: Okay. Thanks.

CHAIRMAN POWERS: Thank you. John Hoenig.

DR. HOENIG: I was just responding to something that Greg wanted me to cover, and so maybe I will let Greg introduce the topic, and then I will make a couple of quick comments.

CHAIRMAN POWERS: All right. Greg, as I mentioned, in your email, you mentioned that there were some items that you could go over briefly. If the debate goes much longer than that, then we'll have to adjust the schedule accordingly, but go ahead, Greg, and proceed. Thank you.

DR. STUNZ: Thank you, Chairman Powers. I just wanted to tie up a few of those loose ends from yesterday, and so I think they will add some clarity for the discussion today. Also, I will help you, Ryan, with Rob Ahrens presentation, and I'm not sure -- Obviously, he will be joining us later, based on his time zone, but we'll get that to you, if he hasn't already.

One is there was some questions about the sample sizes in Florida, and Rob was looking into that, and he'll explain that to us a little bit later, when we get to that analysis section

in the agenda, and, essentially, it was some clerical errors. He had done some imputations which weren't accounted for in that N, but he solved that, and so he can explain the details on that.

Another minor detail that might help folks with a little clarity is on the Table 6 and 7, which we, obviously, keep referring to, and, just to be clear, and Dr. Eggleston had brought this up yesterday as well, and that has to do with that mean density column, or mean density or structure column, and, just to be clear on that, if it was a habitat such as natural bottom, or uncharacterized bottom, those numbers are reported as mean density per hundred square meters, and that's captured in the table legend, but that doesn't always come up with the table we're looking at on the screen, and so it can be a little confusing.

For artificial reefs, that is a mean number for the entire structure, and it's not based on area, like meters squared or a hundred meters squared or something like that, and I just wanted to clarify that, because some were concerned about why those numbers were higher, for example, on artificial reefs, and that's because that's a total number of red snapper for those structure types.

The other thing, Dr. Powers, is that we kind of have been going through the agenda, and the intent of the agenda, as it was structured, was we would go through our methods and our sampling framework, in terms of our methodologies, which we've been doing, and, of course, the discussion naturally flowed into the analysis, which is really an agenda item that we haven't made it to yet, and so we're kind of jumping ahead, which is perfectly fine and appropriate, while we're discussing these individual regional estimates, but the meat of the analysis will come later.

The reason that I'm bringing that up is our statistical experts have a lot to contribute here, particularly Dr. Ahrens and Dr. Stokes, but they haven't really -- Other than the original design, they haven't had the floor to contribute that, and so I just wanted to remind people that that's coming up later, and we, obviously, want to make sure that they have ample time to clarify a lot of these discussion points that are coming up.

Then the last thing, Dr. Powers, was what Dr. Hoenig had brought up at the last minute yesterday, before it was getting late and we had to end the meeting, but the question came up about the bias, and Dr. Barbieri brought it up, or maybe what I think our

team would refer to as maybe perceived bias, and I don't feel that our team had a chance to respond to that, because we were getting at the last minute, and so Dr. Hoenig has prepared a short slide to better answer that question, or maybe better interpret what the question actually was, but, if we could give him a minute to present that slide, that would be great, before we move on to a new region.

CHAIRMAN POWERS: Actually, I would prefer not to do it until 10:00-ish, because of the questions that Dave Chagaris asked, and I think, if we get into that discussion, it would put John's comments into a little more context, and I would prefer to go ahead and then wait until 10:00-ish to revisit this, and, again, look at it in the context of the other questions that Dave Chagaris asked as well, and is that okay with you?

DR. STUNZ: Yes, that's fine, and, I mean, we just need a point, at some point this morning, to have an opportunity to respond to that question.

CHAIRMAN POWERS: Sure. I will make sure of that. Okay. Then we're on to new business, I guess. According to the agenda, Dr. Jay Rooker has his presentation, and his presentation was on the website, and he will be telling us about the UCB characterization. Jay.

TEXAS
UCB CHARACTERIZATION

DR. ROOKER: Thank you, Dr. Powers, and good morning, everyone. I need Dr. Stunz to --

DR. STUNZ: Dr. Powers, we're doing this presentation together, and so they're sharing my screen. If you just give me just a minute here, I will pull it up.

CHAIRMAN POWERS: Okay. Thank you.

DR. ROOKER: All right, and so we're going to pivot over to the western Gulf for an overview of regional sampling on the Texas shelf, which involves the Galveston crew, and that also included Dr. David Wells and Dr. Liu, and I should mention Jason Mestoe, and he was a graduate student, and he did a lot of heavy lifting on the project, and he was our analyst and field engineer. Then, for Greg's group in Corpus Christi, that included Dr. Judd Curtis and Dr. Matt Streich.

In Texas, what we did is we essentially partitioned the shelf by

habitat type, with our team handling the uncharacterized bottom, and, basically, that means enumerating red snapper on, or in, the suspended mud layer over UCB, and Greg's group, I guess, getting the improved visibility on artificial reefs and natural banks.

We are going to be tag-teaming it here today with the Texas overview, and Greg, again, wanted me to mention that our overview, for the most part, is going to center on the methods, and then with Lynne and Rob following up with the analysis portion of the density data that was provided by each group.

In terms of uncharacterized bottom in blue here, as I think most everyone knows from talks yesterday, it's the primary bottom type that we're dealing, particularly in Texas, accounting for a large fraction, over 95 percent, of the seabed in this region, and, again, I should mention that the uncharacterized bottom represents unconsolidated sediment mostly, but the uncharacterized bottom also includes unclassified, or unmapped, bottom, and so there are -- I'm going to mention that there are some relief anomalies that we do see in what's referred to as uncharacterized bottom.

It's generally assumed, or I guess we all did walking into this, that fish density and red snapper density and biomass would be low and patchy in the uncharacterized bottom, and we know its value for nursery habitat, for those of us that do early life history work, but its value as habitat for larger reef fish and snapper was uncertain before we walked into this.

Surveying red snapper, or I guess I could say any reef fish, for that matter, on uncharacterized bottom in the western Gulf, particularly in water depths less than a hundred meters, is challenging, more than probably any of us had anticipated.

I guess we've all spent too much time working on the natural banks in the western Gulf, on the outer shelf, that typically have really good visibility, and, until this, we have avoided surveying anything in the nepheloid layer. I guess, what I'm trying to say, for the plot, is what I signed up for was probably Image A, and what we ended up getting on the uncharacterized bottom was a lot of blackout, no-vis areas throughout the survey.

Because of that, habitat considerations that shaped our survey for the uncharacterized bottom were the large spatial extent of this habitat type, coupled with a persistent nepheloid layer that, unfortunately, never seemed to go away for many of our

locations, and, as we all know, all gear types impose tradeoffs, and we ended up going with an integrated survey method, and the same applies to Dr. Stunz as well, that compensated for individual gear limitations and conditions, ambient conditions, that we experienced, and so our integrated acoustic-plus-camera approach involved the use of an EK-80 echosounder and towed cameras.

I am going to throw in one slide too, and Kevin mentioned this yesterday, in terms of we threw in some paired deployments with the imaging sonar to provide some clarity, but it also provided more data for my graduate student to analyze.

Because of the large area to cover, and the expectations of low density, and we assumed there would be lots of zeroes on this uncharacterized bottom, we opted for towed gears, to increase our spatial coverage and the number of sampling units, and so Kevin has already covered the EK-80, with our unit, shown here in the top-left plot, integrated into the yellow.

Just a quick note is we went into this assuming that we could use a towed-camera platform, and you saw that yesterday, and the TERAS is on the bottom-left, and it’s very similar to -- It’s not quite as sophisticated as Steve’s C-BASS, but it’s similar, for most of our surveys, but the lack of adequate visibility -- The vast majority of our transects, or stations, were poor or no-vis. That forced us to kind of change gears and move to acoustics for counts and then only using cameras for species composition, for backing out the red snapper densities from those total counts.

We used two types of camera platforms. On the top-right, that’s our blue Batmobile, and it’s equipped with forward and downward-facing GoPros, and this was basically designed, during the study, for surveys on the inner and mid-shelf on small boats, because the second one, the bottom-left, the deep-ocean engineering sled, or TERAS -- Well, for the most part, it required us to be using the Pelican, our research vessel, to pull it off, and we needed an A-frame and everything.

The deep-ocean engineering sled was essentially, as I mentioned, a scaled-down version of C-BASS with an altimeter, high-definition cameras, scaling lasers, and onboard viewing, but, again, it only worked on a large research vessel.

On the bottom-right is another tow-fish, and we had lots of tow-fish, a red tow-fish, and this one includes our ARIS imaging sonar, or multibeam sonar, which we used to essentially try to

verify fish counts from the EK-80 and to make us feel better about some of our estimates.

Our general approach was to estimate total fish counts, or densities, with the EK-80, and an echogram from uncharacterized bottom is shown on the left, and, in this example, and this is a really good example for uncharacterized bottom, in terms of fish biomass, and it shows both isolated targets, in the top-right of the echogram, and then a school target, and Kevin talked about how we actually separated individuals out of schools yesterday, or these school targets.

Just to mention that the Echoview, the processing, and this applies to both Greg's lab and our lab, was done by our graduate students under guidance from Kevin's team, and, for stations with adequate visibility, we adopted a paired sampling approach, where we also had one of our towed camera platforms going, and this was for species composition. The image that I put up there is one-of-a-kind, and that's from our best-of album, is we just went above the nepheloid layer, and so this type of quality counts on red snapper, or species composition, were uncommon for most of what we were surveying on the uncharacterized bottom.

Again, from here, we had our counts from EK-80, and we had our species comp from our towed camera gears, and we applied the proportion of red snapper from the assemblage composition to the total counts, and that's how we generated our estimate for red snapper for each sampling unit, or cell.

We also ran the paired EK-80 with our ARIS imaging sonar, and it looks like that image is working quite well, on about 25 percent of our transects, and this was to verify that the EK-80 was at least picking up similar numbers of fishes, and we were kind of focusing on areas where we had very, very low visibility, and sometimes we would have large school targets, and, when it's going through the processing in Kevin's group and extracting these fish targets, we wanted to align that with what we were getting with the ARIS imaging sonar, where you can clearly see individual fish on there.

In terms of what we were referring to as medium and large-sized counts, our echosounder counts and our ARIS imaging counts, there was a strong positive relationship, and we did see that the gear types with the EK-80 are generally picking up more fish than the ARIS, but, as Kevin mentioned, these were side-by-side pulls, but they may be due to slight differences in sampling domain.

The plot on the left shows the -- I should mention that starting points of our transects and gear deployed at each station on the Texas shelf, and the bubbles, which have some green, or all green, and that's where they were EK-80 transects, and then some of the points that have either red for the camera gear or yellow for the ARIS, and that's where we had paired locations, or we may have had all three gears being deployed, in certain areas.

Now, in terms of the spatial coverage of the different gear types shown here on the continental shelf, for the EK-80, we had 150 transects, and so, again, I'm showing points, but we're running transects, with most of the transects being approximately three kilometers, and so we had well over 300-plus linear kilometers of surveys.

Other shelf locations, which you can see, and, oftentimes, we have all three gear types, and they were conducted on the Pelican cruise, where we were able to randomly select stations along our survey tracks, or our proposed survey tracks, which we go off a bit to hit specific stations, and this came up in the review, and it's a completely valid point.

For some of our inner and mid-shelf surveys, several of these were often day trips out of a port, and some of the points -- Again, they're points that are shown here, and they look non-random, which makes perfect sense compared to Rob's random distribution across the entire shelf, but it is important to keep in mind that actual starting points and headings, directional headings, were randomly selected.

Basically, random distance within say a predetermined window was selected for a cross-shelf transect, and we were trying to cover area on each day trip, and that was typically about four -- We could pull off about four surveys, going from inner to outer locations.

The goal was to try to cover as much potential distance on the shelf as possible. If I were to show a plot here, and we have one, but I couldn't locate it before the talk here, to show transect lines with directionality, instead of points, it would be easier to convey that the starting points and headings were more random than some of these appear, and it looked like kind of uniform plots on here, and it's funny, and this was raised in the review, and I had the same discussion about random starting points with my graduate student, who was doing a lot of the surveys, and I asked for a map with transect lines and headings, to just show that -- To convince me of the random nature of some of the cross-shelf transects that we ran.

It’s also important to note, from this plot, that points presented by our colleagues in other regions, say for instance Dr. Patterson in Florida, often represent one cell, and that’s exactly what Dr. Ahrens presented as well, in terms of the sampling design, and so one cell is kind of our ninety-by-ninety-meter sampling unit, whereas each one of our points, at least shown on this map, represents a transect that may have been three kilometers, and so what we’re showing here as one point likely represents thirty potential sampling units from that, and that’s how we ended up generating approximately 3,500, and I guess we’ll call them useable cell estimates, that could be used for uncharacterized bottom.

Again, our group focused on the two depth zones, from ten to forty and forty to 100, and Dr. Murawski did a Gulf-wide survey and handled some of those outer, deeper zones.

What we did is all cells that were provided to the analysis team, which would be Rob and Lynne, we provided details on transects and sequence of the cells within that transect, with our general advice to omit consecutive cells, potentially due to spatial autocorrelation, although we looked at it, and it didn’t appear to be a big deal, due to the patchy nature of where we were finding fish biomass in these areas, and I believe that Lynne is going to talk to that during her presentation as well.

Occurrence, frequency of occurrence, and density, and these are for total fish, and so the plot on the left is the proportion of cells in a transect where fish were detected, and so you can assume that each one of these points potentially represents thirty cells, and how many of those cells did we actually detect fish on the EK-80, and then the plot on the right is the density, total fish across all the transects, and so the cells within those transects may be completely different, and this is a mean per transect, each point on the right.

Our results are very similar to Dr. Patterson’s in Florida, in terms of the uncharacterized bottom, and we had lots of zeroes. Over 70 percent of our sampling units were zeroes with no fish detected.

We wanted to comment on what we think may be extremely important, in terms of getting a handle in terms of what’s going on on the uncharacterized bottom, at least in Texas, but the presence of these relief anomalies -- The image here is actually showing relief anomalies from the ARIS, and this is from an ARIS plot, with examples of what we would typically see in terms of

relief, from no relief -- So the plot on the left, A, is essentially -- That's typically what we were seeing, no relief whatsoever, but you can see, going from low relief to moderate relief to high relief, and, actually, there's a fish shown on that far-right plot, and we had a lot of relief on the uncharacterized bottom.

I shouldn't say a lot, and we actually had -- I said 10 percent on the slide, and it actually turned out to be 12 percent of the cells had some degree of seabed relief, and so, whatever these bathymetric highs were for this relief anomaly was due to, we couldn't determine that, but we did quantify the cells that had relief.

We looked at -- We did develop fish habitat models, and we looked at the influence of these relief anomalies in our GAMs, and, as you would expect, there was a positive relationship with the occurrence of these relief anomalies and total fish density.

We had counts from our EK-80, total fish counts, but we needed assemblage composition, and that is where the camera gear came in, to determine the density of red snapper by cell, or by sampling unit. Unfortunately -- Yes, you walk into this, and you would like to have a paired camera, so you have species composition at each cell, or each transect, and that was not possible.

At least in 80 percent of our transects, and there was no vis, and so, essentially, we took all of our species comp information from the different transects and we put it into regional estimates, and these regional estimates were used to back-out red snapper density from our density on total fish counts from the EK-80, and you can see here that we did have some slight differences, in terms of north Texas, the assemblage, and the assemblage was dominated, dominated, by red snapper in all regions.

We did have, as you would expect, fair numbers of carangids and scombrids, and some sharks as well, but the red snapper were consistently, in terms of the assemblage, numerically dominant throughout, and so 28 percent in the north, and then we had nearly 50 percent in both our central and south region, with an overall species composition for red snapper, in the entire assemblage, being 37 percent of that assemblage.

We had to use this to -- It isn't ideal, but we had to use this to estimate red snapper on uncharacterized bottom on the Texas shelf, and, as mentioned, we applied the proportions of red

snapper from our total fish count densities to arrive at these estimates.

From cell estimates, and we used a few cell estimates from others as well, but ours were basically 3,500, and they were generated for 3,500 cells, from our uncharacterized bottom, and this is what Lynne and Rob are going to talk about, and the estimate was close to sixteen million red snapper, with a CV of 16 percent.

Now, certainly, there are limitations to our study, and we had a limited number of transects on uncharacterized bottom that had sufficient water clarity to quantify species composition the way that we would want to.

I mentioned that at least 80 percent were poor to blackout conditions, and we had approximately 10 to 12 percent of the transects that we ran where you could call it marginal to good for estimating species composition, and we were -- Dr. Murawski, and Greg’s group as well, when they did hit some uncharacterized bottom, if they did get species comp information, they did provide that to us as well. We factored that in, but it was very similar to what we had from our surveys.

In terms of the regional species composition estimate, this was brought up in the reviews as well, talking about underestimating natural variability. When you’re applying regional species composition estimates to total counts from the echosounder for individual cells or sampling units that are based on regional species composition data, you would expect to downweight your natural variability estimate for red snapper, because you’re using the same proportion for all of those cells within that region, and so I think that’s important to note.

I did want to mention one thing, before I pass it along to Greg to start talking about the artificial reefs, and it’s just a point about a gear comparison. Yesterday, there was discussion in Florida, by Will and others, about the EK-80 and some mismatches between the EK-80 and the visual data, but I think it’s important to note, and Lynne may be talking about this, but keep in mind that we had independent estimates on uncharacterized bottom in Texas from C-BASS.

Now, these were mostly outer-shelf locations from Dr. Murawski, where visibility is better than we see on the vast majority of the rest of the shelf, and so we have a camera-based approach, and our approach, which was fish counts generated with the echosounder and then species composition applied to those fish

counts to back out red snapper, and it, from my understanding, but, again, maybe Lynne or Rob can talk about that, that they produced very similar overall estimates of red snapper on the Texas shelf, and I think, with that, I'm going to pass it along to Greg to keep going.

CHAIRMAN POWERS: Let me interject here, and I would like to open it up for a few questions before we get to Greg, if I may.

DR. ROOKER: Certainly.

CHAIRMAN POWERS: First off, I would like to ask a question, and this is something that I'm unsure about. For the Texas estimates of UCB, what you've been discussing here was for the two shallower-depth strata, and the deepwater depth strata was taken solely from the C-BASS data?

DR. ROOKER: That is correct.

CHAIRMAN POWERS: Okay. Thank you. So, in terms of -- We got into discussion yesterday, a little bit, about that there were independent estimates of some of the strata, or not independent, but two different methodologies, and so, in this case though, you're really talking about different methodologies for different strata completely.

DR. ROOKER: That's a good point, but Steve did, occasionally, venture into shallower zones, and he was focusing on the deep zone, the hundred-plus, but he did occasionally go into the shallower zones, and it was my understanding, but the analysis team can comment on this, that, when we were in similar zones, the numbers matched up quite well.

DR. STUNZ: Jay, there was some overlap between you and Steve's depth sampling.

CHAIRMAN POWERS: That gets carried over into the estimate? Well, I will wait for Rob to deal with that. Are there any other questions about this? Dave.

DR. EGGLESTON: Jay, thank you. Just two simple questions. One, is that nepheloid layer typically found on the shelf break?

DR. ROOKER: Surprisingly -- We assumed that, once we made it out into eighty meters of water, that we wouldn't see it as much, and that's where, obviously, we had good camera work, was on the outer shelf, but, surprisingly, the vast majority of our stations had that, and, if we had any type of relief anomaly

that went three or four meters above the seabed, we could get good counts with camera gear, but, for most of the shelf, inside of seventy or eighty meters, the vis was near-blackout, and, if there was no relief, oftentimes, it would extend three, four, five meters above the seabed. It was very difficult.

DR. EGGLESTON: Then the second question is I'm impressed with the fact that the species assemblage, regionally, averages about 50 percent red snapper, and so, if that's for real, then what is it about red snapper that allow them to be so dominant in that species assemblage compared to other fish?

DR. ROOKER: That was significantly higher than we probably assumed, going into it, and Greg can likely comment on that as well, in terms of what they're seeing on the artificial reefs, but I think their species composition was, similarly, very high. Is that correct, Greg?

DR. STUNZ: That's correct, Jay, and, Dr. Eggleston, that is interesting. As you get into the western Gulf, you really get a dominance of snapper, compared to some of the videos that Will, and even Sean, showed yesterday. Now, on the artificial reefs and natural bottom, there is diversity, especially on the natural bottom, but, yes it's dominated by red snapper.

DR. EGGLESTON: But any idea why?

DR. STUNZ: No. No real understanding as to why. We just don't have all the grouper species and the variety of snapper that they have in Florida, and maybe more on the coral-based-type sediments or something, but, in general, it's much less diverse as you move west from the Florida region.

DR. EGGLESTON: Okay. Thank you.

CHAIRMAN POWERS: Thank you. I have one other question about the sampling. You showed the slide of the beginning points of transects, and how do those relate to the original random locations that were generated by Rob?

DR. ROOKER: That's a good question, and, basically, if you -- I don't know if you remember from Rob's scatter plot, but Rob had 380 sampling units for uncharacterized bottom in Texas, and they were essentially found across the entire shelf with -- I think the one thing you probably noticed from Rob's plot is there was kind of a greater concentration of points kind in the north region, with the broader shelf, and that is where we had more of the transects going as well.

CHAIRMAN POWERS: It might be good to visualize that, and I'm not sure, but, anyway, there's a number of people that want to ask questions, and so let me go on. Mary Christman.

DR. CHRISTMAN: Thank you. That was interesting. About the composition of the species, I assume that, when you say it was 50 percent red snapper, that's 50 percent red snapper that were, what, 250 millimeters and up?

DR. ROOKER: That's correct.

DR. CHRISTMAN: Okay, because you didn't really mention the size aspect of it, but I'm sure that's what it was, and, secondly, when you show that graphic where the transects are located, it would appear that almost all of your camera work was offshore, because it was clearer, and is that correct?

DR. ROOKER: That is correct. We basically -- Anything on the mid or inner shelf, we had near-blackout conditions the whole time, and it's interesting, because we do have some artificial reef areas that we survey here, and we have dropped cameras on them before, and, occasionally, occasionally, you have good visibility, but it may only be a handful of days out of the summer.

DR. CHRISTMAN: So, related to this distribution of where the cameras were, one, is that also where a lot of the natural features were that you found, because, like, for example, in Alabama, all of the natural stuff was offshore, the natural hardbottom, and so I was just curious whether there was any overlap of that, which would therefore influence your estimates, and then the second question is, if you have mostly offshore species composition, is it reasonable to assume -- I just want you to discuss this a little bit. Is it reasonable to assume that the more shallow regions would be the same composition?

DR. ROOKER: No, and that's a good question, and one that we've -- When we initially came up with our species composition estimates, we had it in the two depths by region, but then we found ourselves essentially using data from two transects to apply species composition for across that entire inner region, and we didn't feel, necessarily, comfortable with having one or two stations with decent visibility influence everything, and so we opted for a more conservative approach.

The numbers were generally similar, but we opted to be a little bit more conservative. The inner, the ten to forty and forty to

100, were similar, but, again, our sample size was so small, in terms of species composition on the inner shelf.

DR. CHRISTMAN: So how about the distribution of the relief features, the reef anomalies, as you called them?

DR. ROOKER: That's actually a really good point, and I was talking to my graduate student, who has defended and gone, and we were talking about that last week, in terms of that we should plot them, and we haven't yet, but we will plot those, and we could include that, if anyone is interested in seeing it, because there was a strong relationship. Most of those relief anomalies were relatively small, and so, if you were to do maybe thirty cells in a three-kilometer transect, you would probably notice a couple, and, oftentimes, those relief anomalies, regardless of how they small they were, would hold fish.

DR. CHRISTMAN: Then the final question I have was you said the individual, the 147 individual EK-80, transects were randomly -- The starting point and direction were randomly chosen, but I would call the ship transects, which are the larger tracks of the points that you're displaying in your graphic here, were they randomly chosen, because they don't look random.

DR. ROOKER: That's what I mentioned too, and, in terms of -- They were on the outer shelf, where we had research cruises and we were going along a depth gradient, and it was easier to -- The issue for the inner to mid-shelf locations is just it's a logistics issue, in terms of how do we sample that area in one day on a small boat, and we're trying to run as far as we can, which may be sixty nautical miles, and we can usually get three or four transects done in a day, and we're trying to cover regions, different depth regions, across that entire transect, and so, basically, there would be a window, a random window, once we got to a specific point, within say five kilometers, where you would pick a starting point in there randomly, and then you would pick a random heading, and that's the direction that you would pull the transect. It's not ideal.

DR. CHRISTMAN: But the cluster of transects that would run along the line might -- I guess that's Galveston there, where it looks like you ran out one, two, three, four, on like seven or eight days, to run a long a particular line, running transects in random directions along that line. Those ship lines, and let's call them that, weren't randomly selected, or were they?

DR. ROOKER: Basically, what we were trying to do is we were trying to fill the area the best that we possibly could.

DR. CHRISTMAN: Yes, and I completely understand. I mean, it's expensive to do this.

DR. ROOKER: Yes, and then the thought was, Mary, when we into this -- So we thought that -- We tried to be random, in terms of where the actual starting point was, and then then heading, and then our assumption was, if we created thirty sampling units, or cells, from that transect, that the team could randomly select three or four of those units from the transect, because, again, we generated about 3,500, if I'm not mistaken.

This gets back to Luiz's question, and maybe that's what he is going to ask me about, earlier, just in terms of do we have enough sampling effort, and so, from Rob's design, remember that he had about 380 ninety-by-ninety-meter cells, and we actually did an order of -- Or at least provided an order of magnitude more cells to actually pull from.

DR. CHRISTMAN: So, just to make sure that I completely understand, in an individual transect, and say you had twenty cells, each individual cell was ninety-by-ninety meters, and so what you did was you took the image and converted that into a total count within that ninety-by-ninety.

DR. ROOKER: That's correct.

DR. CHRISTMAN: Then you converted that to red snapper, using the composition.

DR. ROOKER: That's correct, Mary.

DR. CHRISTMAN: Okay. Thanks. I'm done.

CHAIRMAN POWERS: Thank you. Kai.

DR. LORENZEN: I think Mary has asked most of my questions, and so I will be quick. Coming back to the location of the camera versus the acoustics, and I get the point of doing a regional thing, but it still seems to be that your species compositions come from the edge of your region, and then you sort of extrapolate them into the rest of the region, and I understand why, with the visibility, but it still makes me not as confident in the validity of those distributions and the more inshore areas.

That was one, and I guess it's more a comment than a question, but the other is I'm still -- About, basically, the deviation

from the original design, and I get those practical points, but I'm also wondering to what extent, maybe, is it better to stick with the design and do fewer samples, if you have to, and that's kind of more of a question, I guess, for our statisticians, but I'm a little puzzled by the extent of the deviations from the original design that seem to be more common in Texas than in the other parts of the study. Thanks.

DR. ROOKER: I think both of those are valid points, and it was one where we -- We were actually concerned by the number of zeroes going into this with the points, and there were logistical issues too, in terms of how you sample this broad shelf properly with towed gear, but there were a lot of zeroes, and I think this gets back a bit to Luiz's point yesterday about this expansion factor.

Actually, I think it provides justification for the use of transects, which is going to generate more sampling units, and, again, our assumption going into this was there are going to be lots of zeroes on uncharacterized bottom, and it's going to be extremely high, and we worried about having sufficient samples, not knowing how many relief anomalies were out there, holding biomass to adequately capture the natural variability on this uncharacterized bottom.

It wouldn't be a big deal if it was zero, zero, zero, zero, one, but, if you get twelve zeroes, and then you get 120, we were worried that, with only having 380 sampling units, would we be able to capture that properly, and so we opted for increasing the sampling area, and I think that's one too -- Will maybe -- He commented on this yesterday, but, where they had sampling units, and he had -- If I remember, correctly, and, Will, please chime in if I'm wrong, but it was one or two samples that had a huge impact on the overall estimate once you extrapolated, once we applied this expansion factor.

Hopefully, by having more sampling units, and knowing that relief anomalies are -- Their distribution is scattered, and somewhat random, that we captured that, and hopefully smoothed out some of the variability, without having one or two sites on uncharacterized bottom that held a bunch of fish having a big influence on our overall estimate.

CHAIRMAN POWERS: Thank you. Jim Tolan.

DR. TOLAN: Thank you, Mr. Chairman. Thank you, Jay. That was a really good presentation, and I feel for you when you start working with the nepheloid layer off of Texas. The question

that I have is, really, it kind of struck me when you had -- The species composition that were not red snapper, I was surprised not to see sciaenids up there, because, in most of the longline data from this persistent nepheloid area from the shrimp grounds, red drum are really much more prevalent than red snapper, and so, from an acoustic point of view, this -- Do you think the number of red drum out there might be inflating these red snapper numbers in places where you don't have the camera view? Thanks.

DR. ROOKER: Jim, that's a good point, and I think we were -- I think our effort in that twenty-meter and shallow depth was limited, because we couldn't see much, and I would imagine that the sciaenids would be -- I know some of the nearshore platforms here hold lots of sciaenids, red drum and other species, and I know that places like Freeport Rocks, some of these bathymetric highs that are only -- They are relatively shallow, less than twenty meters, but they hold a lot of sciaenids as well, and, again, I don't know if there's a possibility that those individual sciaenids are farther offshore, and maybe their proximity to the seabed biased our ability to pick them up with the camera here.

CHAIRMAN POWERS: All right. Thank you. Steve Murawski.

DR. MURAWSKI: Thanks, Mr. Chairman. I wanted to generally support what Rob said about the species composition in this particular region, in the northwest region of the Gulf, and, as I said yesterday, we had done a longline survey around the Gulf of Mexico, and it included a number of inshore to offshore transects, and, by inshore, I mean starting at forty meters and going out to 300.

We did some species accumulation and rarefaction curves for these different sections of the coast, the northwest area, the north central, the West Florida Shelf, and then off of Mexico and Cuba, and, if you look at the rarefaction curves, and particularly the species accumulation curves, they top out at a much lower level, which means that the species diversity in that particular region on the bottom, subject to a longline survey gear -- It's a very depauperate fauna, relative to the other places in the Gulf, and I suspect some of the issues of -- Over on the West Florida Shelf, for example, it's much more grouper dominated.

You never see red grouper on the western side, as opposed to it being a dominant grouper on the eastern side, et cetera, and so I'm happy to make that -- We published those results in a paper

in 2018, and I am happy to send that along, but, generally speaking, when we looked at the longline survey data, large red snapper, and I mean very large red snapper, were the dominant species in the composition from forty meters on. Thanks.

CHAIRMAN POWERS: Thank you. Luiz.

DR. BARBIERI: Actually, I will pass, Mr. Chairman. Thank you, Jay, for the presentation. It was a great presentation, but, between Mary and Kai and Jim, they have already asked the questions that I was thinking about, and so I will pass. Thanks.

DR. ROOKER: Thanks, Luiz.

CHAIRMAN POWERS: Benny Gallaway.

DR. GALLAWAY: That was a very nice presentation, and I concur. All my experience tends to concur with your proportions of red snapper dominating in most of these habitats. I would like to also point out that I -- It's my belief that the shrimp trawl data, the point data, the every-ten-minute intervals while towing, provides a very good index of things and these small habitats and the distribution of habitats utilized by red snapper, and so the shrimpers avoid those areas, because they lose their nets.

DR. ROOKER: Good to hear. Thanks, Benny.

CHAIRMAN POWERS: All right. Thank you. Jim Nance.

DR. NANCE: Thank you, Dr. Powers. Jay, that was a great presentation. I've got a quick question, and most of my questions have been answered, but, on the composition of those relief anomalies, are they hard substrate, or are they just large areas of mud that can move around during storms and things?

DR. ROOKER: That's an excellent question that we do not have an answer for. We could pick them up, but, without having something like side-scan, we don't know what the composition is of those relief anomalies, and so it could be exposed pipeline, or it could be somebody dumped a canister overboard, or it could be that there's some type of uplift in the bottom, and it's really difficult to say, Jim, but it's clearly something that we need to know a little bit more about.

DR. NANCE: Yes, because I know that they do attract fish. In

those areas, you see a pile of fish around them, but there could be some movable, that sometimes they're not there, and sometimes they're there in large quantities, and so that would add a lot of variability to the composition data, also.

DR. ROOKER: Yes, I agree.

DR. NANCE: Okay. Thank you.

CHAIRMAN POWERS: Thank you, and thank you for the presentation. Then I believe that now we're going to move on to Greg's portion of the presentation for the other bottom types.

ARTIFICIAL REEF/NATURAL BANKS

DR. STUNZ: That's correct, Joe. Before I get going, those were some great questions, and, just to Benny's and Jim's points that they just brought up, because it will be relevant to some of the work that I'm going to talk about here, a main thing that I think we need do, post-this-study, meaning someone, scientists, is better characterize the uncharacterized bottom.

Benny has a good idea of looking at shrimp trawl tracks, to try to get a better idea of how much of that is really out there, as well as we just didn't simply have the ability, with the towed gear, to go back and characterize, well, what was it, what was the structure, but that's something that is much needed, and, Jim, your point about movement back and forth among these habitats, even from artificial and natural back to uncharacterized bottom, is important, but it's just something, obviously, that was beyond the scope, but it's very important, in terms of related to the management of the species

Anyway, back to the Texas estimate, and, obviously, most of the fish were on the uncharacterized bottom, but, specifically, to be true to the RFP, and they explicitly asked us to look at artificial reefs and natural banks, which we did, and, based on Rob's design, we were able to -- His original design was asking us for sixty sites, split evenly between pretty much being artificial reef and natural bottom, and, of course, distributed within our depth strata that we've been talking about since the beginning of these presentations.

Before I get into that, I want to just talk about what do these habitats really look like, so that it can inform some of our sampling, and I think, Mary, some of your questions about the distribution of natural reef habitat -- I have some maps that I think might answer your question that could be informative.

Artificial reefs are very diverse. In fact, the artificial reef in Florida is very, very different than artificial reefs in Texas, and we've talked about that, including natural banks, and even within those structures, even in Texas, they're different.

Sometimes they're standing and up out of the water column, and sometimes they're toppled. Sometimes they're cut off, and sometimes they're tiny, as small as a car, with hundreds and almost thousands in a field, and sometimes they are in between, and, at the sampling resolution we have, that makes it very difficult, and it really forced the post-strata analysis into really large and small-type structures, just because of the sampling resolution we could dedicate to that particular habitat.

To give you an example of what a rig might look like, and you've seen a lot of video now, but this is the example of the snapper, and, yes, it's dominated by snapper, and they are typically large snapper, compared to the sizes that you see out east. Certainly, there's more size composition, especially on the habitat that Jay just reviewed.

The natural banks, what's different about the natural banks in Texas is there is relatively very few of them, but they're very high in relief, at least the ones we know about, and, in fact, we find ones that we don't know about all the time, and it's fairly routine, and likely that's what those fish are holding on in our unclassified bottom. That's typically what it looks like in an ROV, but what I want to draw your attention to is the predominance of snapper that you see there.

Mary, I put this in, because you had the question yesterday, and this is actually a map that John Froeschke with the Gulf Council did, and so, John, thank you. I just pulled this out of a white paper that you did, last night, when I thought about that this might be informative to our discussion.

Now, keep in mind this is somewhat cursory, and, in fact, it could be more resolution, but I'm trying to fit this on one slide. What you see here is -- This is hard bottom, and it's rock dominant and rock subdominant, and you could group these into various categories, but clearly what you see, as you go to the west, especially in the northwest Gulf, is there's just not much.

You kind of see the Flower Gardens out here, over in this region, but there's not a lot of bottom, and so they're very

discrete, and so that also gets to your point about are we running over them with a transect, like you would in Florida, and probably not.

It also lends to the sort of Florida-centric design that we had, where you have a lot more of the sort of ones, twos, and threes that Rob talked about, but, in reality, our habitats are very much more like threes, high-feature and high-relief habitat that do hold an abundance of snapper, and, in fact, they're called snapper banks, because, traditionally, that's where the commercial fishery operated, is on those banks.

I am happy to pull that back up later, through our discussions, and I think that's important, to get a feel for how different -- It's a good example of why this regional approach became so essential, because it wasn't a one-size-fits-all sort of thing.

Here is the universe of our known natural banks that we have, and I am going to overlay this in a minute with the artificial reefs, but I want to bring up another point that you had, Mary. Here, these are to scale, and that's the actual size relative to the uncharacterized bottom, but I want to draw a point that I think will help clarify some other discussions.

We have three depth strata that you see here, and, even though it's a shallow depth strata, that can be a little bit misleading, because the shallow depth strata ends right here off of Galveston, and that's a long way. You're talking sixty-plus kilometers out.

Of course, it's narrower as you get into south Texas, and then the deep band, obviously, is very limited as well, and so that also limits our stratification design, because not everything is distributed within the strata that we necessarily would like them to be, but I think those are some important points, that some might be thinking this shallow is up nearshore, in state waters or something like that, and that's not quite the case, especially as you move east, and even more in Louisiana.

What's not to scale is this slide. These are our known artificial reef universe, and, Mary, you asked the question about, well, it looks up they take up the whole shelf, and, yes, they are scattered about the shelf, but, in reality, they take up just a very tiny portion, in terms of their actual areal coverage.

If I put the true scale on here of the reefs, you probably couldn't see them, and they would be tiny, and so they're not to

scale in these images, and they're just so we can have them for discussion purposes.

Back to the design, and so, for our layers, we were able to randomly select by strata, of course, our natural reefs to sample, as well as the artificial reefs.

The gear, and it's very similar to what Jay did on natural banks, and so I won't go into that, and, of course, Dr. Boswell gave a great presentation yesterday on all the acoustic details, so we could efficiently get through these presentations and not have to be repetitive, and so I'm just going to add what's different from what Jay did to the uncharacterized bottom.

We have our ROVs, which essentially allow us to get a species composition, and we are not -- Unlike Will, we're not counting the fish from the ROV for the estimate, simply because we don't have the visibility, and the nepheloid layer is just as persistent on these two habitats as they are on uncharacterized bottom, although we get days where we can generate really good species composition, but that's very unpredictable.

The top surface waters are crystal clear, generally, but, once you get down to the bottom, it turns to zero visibility, and there's no way to predict that, even from the surface, and so we simply just have to go out and collect what we can, and we're fortunate to do enough sampling that we come across days where we can generate good species composition.

I will give an example, because we have some challenges with species composition when you have high-relief habitats that I will talk about, but, essentially, you generate a MaxN, a maximum number of fish in any particular field of view, and we can do that for forward and rear-facing cameras, to avoid duplication, and I will talk in more detail about that in just a second.

However, we pair that with the acoustic data, which is actually getting our total fish density, which then we can extract the composition, just like Jay did, and then generate a species-specific density, either per unit area on the natural reefs or per structure for the artificial reefs, which we considered as discrete units.

How we might do a transect, while there is relief on the natural banks, it's not relief like you would have with an oil-and-gas platform, and so we have several papers that are cited that you can read much more details, and we have worked very hard, in the

past decade, to develop ways to adequately describe these structures and characterize them, using this methodology, but, for the species comp here, we essentially descend the ROV.

Then we have a randomly-selected location on one of these banks. When we get down to the location, the ROV is georeferenced, so we know how far it goes, and we do three forty-meter transects, and then we ascend back to the surface, making interval stops along the way and calculating that species composition.

Typically, red snapper on natural banks being demersal, they are at the bottom, or similar to what you saw in Jay's video, or near the bottom, and so we capture that from our surveys and ROV video, and that's not the case for artificial reefs, and so the difference with artificial reefs is you have a huge amount of relief coming up off of the bottom.

Typically, the snapper are at the bottom, but, in fact, they do distribute throughout the water column, generally at lower composition as you get higher in the water column, and so that requires us to do essentially a depth interval composition that can be applied across those habitats.

To give you an idea of what this would look like, clearly the depth varies, and so we start at the bottom and then go up every ten increments to calculate various layers, and then, based upon those layers, we assign back a species composition, and so we'll go down to the bottom and do a one-minute count, and then we'll move up to the next ten meters and do another one-minute count and so on, until you get to the top, depending upon the layer depth that you have.

Essentially, from that, we can apply species composition, but, in this case, we apply that species composition by site, by depth, and then, of course, we did the species composition for depth strata, because the composition differs from shallow out to deep, and so we do it by depth strata and, of course, by the habitat type, whether natural or artificial, and so that composition, from when we get our acoustic profiles, is applied back to those depth bins, and so we don't just apply one species composition, but, to give you an idea -- So each layer would have a different composition.

To give you an idea of what it is though, compared to like a natural bank or a natural bottom or unconsolidated bottom that Jay was looking at, where you can have a pretty much uniform composition that you can apply back in a more uniform way, here, it's roughly about 11 to 15 percent red snapper, but the

difference is that our acoustic profile captures that entire water column, and so we have to apply those lower-abundance estimates higher up in the water column, where snapper aren't as dense.

Then that was for the species composition, to actually generate the echogram from our hydroacoustics, and, again, we would go to random point selected on a natural bank, and, based upon the guidance from Dr. Boswell, there's really two patterns that we've been talking about, and so I won't get into that in much detail, other than we typically fly the flower pattern, with a centroid point that we go out a fixed distance, and then, of course, post-process all that back in the lab. That gear is towed, and it's either fixed on the bottom of the boat or towed behind the boat, to create these patterns.

Many times, we don't know what we're up against, and we know we're in an artificial reef field, but we don't know sometimes even how many are there, and we know one is there, and there might be a center point, and so we have to do a little bit of ground-truthing in those areas, because we can never know, until sometimes we get back in the lab, for sure what's there.

We will do -- I should point out that, in general, from our tagging studies in the literature and a lot of work that many other folks have done, there is a zone of influence around these natural banks, and especially artificial reefs, of roughly, depending on what study you read, fifty to a hundred meters, and so we considered that, but, typically, the fish are very tight and very close to the structure.

We would do these more mow-the-lawn transects, just to make sure we covered the area, or we may not know exactly how bit it is, and remember these oil-and-gas platforms are very, very large, the size of three-story buildings on their side and that sort of thing, and so we want to make sure that we capture the whole structure, but, at the same time, we've gone beyond that, but, in the lab, that would be back and truncated to that zone of influence.

I won't get into the processing, because Kevin has already been into that in a lot of detail, but you can kind of see here, in this instance, where a rig was cut off, and it was set next to it, and you see the fish above that, which we would apply the composition to, and the green line, and maybe you can barely make it out, is what we would extract just to get to the fish.

We know there is fish inside the structure, and there is

probably fish next to the structure, and, of course, right on the bottom, that we're having detectability issues, leading to an underestimate.

We will talk about the exact analyses for this in Rob and Lynne's portion coming up here in just a few minutes, but, to give you the overall take-home message from natural and artificial reefs, this is the estimate on artificial reefs and our variability of just about a million fish, with a 40 percent CV.

We share the same uncertainties that we've been talking about for the past few days, about the issue with the detectability of these fish, primarily as it relates to visibility concerns, in the sense of the ROV, but also the echogram as well, in terms of what it is or is not picking up.

For the natural reefs, it's about six million fish, with a 38 percent CV, and the same kind of issues we might have, and there's somewhat of a dead zone about a half-meter off of the bottom, which leads us to exclude fish that might be really attached to the bottom, and I should point out that snapper are generally very site-attached. They're very close to the structure, and they do forage out, especially at night, as Dr. Eggleston had mentioned yesterday.

All of our surveys were done during the day, but they're still typically within that zone of influence, and we know that from our electronic tagging with our acoustic tags and that sort of equipment, but, many times, our tags would have let's say a three-year lifespan on the battery, and those tags are going out on the same structure we tagged them in, and so we tend to think that they're very site-attached.

Putting this all into perspective, and this is just a copy-and-paste out of the Texas region for the table, if we have some discussion on that, in terms of our sample size, the density per structure, of course, and our total estimate, bringing the Texas estimate to twenty-two million, when you add in Jay's fifteen million uncharacterized bottom component. Dr. Powers, that's the Texas component of this, and I will go ahead, I guess, and stop there for questions, or what do you prefer?

CHAIRMAN POWERS: Yes, let's stop for questions here and give people a chance to -- First up is Mary, Dr. Christman.

DR. CHRISTMAN: I seem to get my hand up a lot. Thanks a lot, Greg, for the maps. That really made a big difference. I

appreciated that.

DR. STUNZ: Sure.

DR. CHRISTMAN: I just have one quick question, or, actually, I have two. One was did you do the average composition by artificial reef type, or did you apply the composition, the depth-stratified composition, that was observed at each site for the artificial reefs?

DR. STUNZ: It was combined for the depth strata, and so, for example --

DR. CHRISTMAN: So it was the averages.

DR. STUNZ: Yes, and so we did -- I'm trying to -- Let me think here. We did sixty-six species composition, and, of those -- I don't remember, Mary, and I could get you how it came out, but roughly about half of those on artificial reef and half on natural bottom, and those were by depth strata, and that generated an overall species composition for that depth strata for that habitat that we applied, and, of course, it was binned by depth.

DR. CHRISTMAN: Yes. Exactly. I wanted to confirm that that was done similar to the others. The other question I have, related to your sample size, is you said that Rob had provided you with thirty and thirty, and your final table shows thirty-six and forty-nine, of which four were on small, and I'm wondering -- You did not pre-stratify the artificial by size, and that was post-stratified, correct?

DR. STUNZ: Yes.

DR. CHRISTMAN: So that's why the small sample size for the smalls. Okay. Thanks. I'm done.

CHAIRMAN POWERS: Thank you. Dave Eggleston.

DR. EGGLESTON: Hi, Greg. Thanks. The maps were also very helpful for me, and I'm still struggling with the species composition issue a little bit, in terms of coming up with a reasonable ecological explanation for the dominance of red snapper, and I appreciated Steve Murawski's comments about their published longline data, and I think that would be really helpful to reference, and then Benny made another comment, kind of confirming, and so I just wanted to probe this a little bit, and two things.

One, in terms of the ROV, we've spent a lot of times, in terms of the visual methods, looking at red snapper behavioral response to that gear, but we really haven't talked about the other species that are in those assemblages that might be responding to that gear, and so amberjack are pretty curious, and so I can see where they might be attracted to it, but, other things, and you showed cobia in your schematic, and I'm guessing that there are species that might see that gear and flee.

I just wanted to talk about that a little bit, and then the other part of my question relates to reminding me of what target strengths -- What other species have a similar target strength to red snapper? If you could just talk a little bit about those two questions.

DR. STUNZ: To your first question about the sort of species or the dominance of snapper, in Texas, in the western Gulf in general, if there's a relief feature at almost any depth, and now even we're starting to see this inshore, with our recent freezes that we didn't know about, it's got snapper on it, almost guaranteed, and it's just they are very, very prevalent, and there's a lot of reasons why, and maybe it's great juvenile habitat, a lot of juvenile habitat, that they can easily recruit right back to these preferred habitats or something, and we just don't have a good answer for that, Dave.

On the behavioral component, Will did the studies out in Florida that generally showed not a lot of attraction or repulsion, and I think we feel that's the case here, and we don't have data, and we didn't do behavioral studies to the ROV or anything like that.

Just from observation, there's really not a response similar to what Steve saw, where we would have captured them on the video before we -- We don't see them on the video and then they flee, for example.

Now, of course, they could have fled before the video was in the water, and it does take us time to get the gear, and it can take hours to get all of your gear set up and in the water and all that sort of thing, but the general behavior of almost all the fish are unresponsive, except when they get hit by the lasers. They don't like that, and so that's kind of strange, but, in general, and so I don't have a quantitative answer for you, as far as what behavioral response would look like. If anything, I would say that our composition is probably biased towards the low side.

Target strength, we've got other species, such as gray snapper and vermilion snapper, large cobia, and there are some grouper, but they are very rare, but those all those species are -- We have, of course, all the species compositions calculated, and they're relatively rare, collectively, things like amberjack, compared to the abundance that we see of red snapper, and so, sorry, and I don't know what else you were looking for regarding the target strength, but those are a few species that would be captured on the echogram.

DR. EGGLESTON: Yes, and that's what I was asking, and so, basically, the gray snapper and the vermilion snapper would have a similar target strength to red?

DR. STUNZ: Yes.

DR. EGGLESTON: Okay.

DR. STUNZ: Fortunately, they do partition some that we see, especially the gray snapper and vermilion snapper, on how they are utilizing the reef, especially further up in the water column, which is nice for our -- The snapper tend to utilize areas closer to the bottom. Now, of course, we didn't -- We have not analyzed the distribution around these structures like that, which would be something interesting to do.

DR. EGGLESTON: Just sort of an ecological question, and who is preying on those two-year-old red snapper, besides humans?

DR. STUNZ: Well, given their expansion lately, not a lot in the western Gulf, and so there is amberjack that will eat them, for sure, and, of course, there's a lot of sharks around the area, and there are some large grouper, and we get goliath, but nothing like in Florida or anything. We have bigger warsaw grouper, and those fish

DR. EGGLESTON: Thank you.

CHAIRMAN POWERS: Thank you. Then we have Benny Gallaway, and I think that will close out this section. Then Luiz wants to talk, but go ahead, Benny.

DR. GALLAWAY: Greg, nice job. Would you comment some on the relative density on natural banks from going onshore or offshore? What are the typical densities that you see on those shelf-edge banks, and what is the distribution of red snapper on those shelf-edge banks? Are they on the top of reefs or the

slope? Would you comment on that? Then, also, the proportion of red snapper on platforms, on your artificial reef category, is that a large component, an average, or multiple? Thanks.

DR. STUNZ: Benny, those are some great questions, and that's something that some of my graduate students are looking at. As you start to discover things, as you look at -- Not only are artificial reefs very different in Florida than they are in Texas, but even the natural banks and such are different, even how they spatially use some of these natural banks, versus the crest and the edge and that sort of thing, and we're just beginning to sort of look at that, Benny, but we don't -- We think we can tease that out from the information we have, but it's definitely not part of the study.

Now, we see, on average, about 0.4 or so per hundred meter squared snapper density, and that's pretty consistent across the depth strata, in terms of even with that shallow shelf coming out so far, even in our shallow regions, and a lot of our regions closer to shore, even within state waters, have large populations of red snapper as well.

I don't know if I would say that it was even, and I would need to go back and look at the data, to really see what that distribution change would look like, but, as far as the overall number per artificial reef compared to natural bank, it's kind of hard to make that comparison, because the artificial reef, as you well know, is going straight up out of the water, and so the footprint is relatively small, where the footprint of a natural bank is much greater, but they are generally less dense.

We see very high concentrations of densities of red snapper on artificial reefs, much greater than the natural bank, but then, when you start expanding that out to a footprint, of course, that's where it changes, in terms of a little lower densities, but a lot more area, and so they also, Benny, probably partition from size as well, and Dr. Streich, who is the lead for these components with our group, has done a lot of work looking at size distributions, and that's an important component as well. There are still large age-two-plus, but we tend to get larger fish on the natural banks.

DR. GALLAWAY: I'm looking at Table 7, and I saw the deep natural reefs had a density per hundred meters squared of like 0.1, whereas it was about 0.4 in shallow and mid-depth, and is that characteristic?

DR. STUNZ: Yes, with keeping in mind that shallow could mean

far from shore, depending on what area you're at, and so it's kind of this misleading -- Shallow doesn't necessarily mean very nearshore.

DR. GALLAWAY: Got it.

CHAIRMAN POWERS: Okay. Luiz, and then I want to close out of this section.

DR. BARBIERI: Thank you, Mr. Chairman. Greg, thank you for the summary presentation. That was very helpful. I am back looking at that Table 6 on page 84 of the report, and I am just wondering if you could comment on that main density in uncharacterized bottom.

When you look across-the-board, for Texas, Louisiana, and Florida, and they're not very different for even Alabama and Mississippi, but the mean density is very similar, if not the same, and I'm trying to reconcile this with what you just said regarding just the dominance of red snapper over vast portions of the bottom there, of the species composition being so tilted towards red snapper in most of the sites.

DR. STUNZ: Luiz, I don't have -- I can speculate for you. In terms of do I think those numbers are real, yes. I think, when you look at that map that I showed, and that was one reason that I put it up there, that Dr. Froeschke created, it's that they have a lot -- I am suspecting there is a lot of relief anomalies out in the uncharacterized bottom that we just don't know about.

I think we're just scratching the surface of it with this study, and it, obviously, needs to look a lot better at that, including -- Now that we have this hindsight, Luiz, we should have spent a lot more time on uncharacterized bottom, but we didn't know, going in, that we would discover what we did.

Definitely future studies need to go back out and better characterize that bottom, not only what it is, but are they ephemeral things that come and go, or are they containers, or what does it really look like, but I think, in Texas at least, that I can comment, and the western Gulf in general, there is just a lot of area for them to spread out, and so that's why you see, even though the composition is high, they have places to go, essentially.

DR. BARBIERI: Okay. Got it. Thank you, Greg.

CHAIRMAN POWERS: Thank you, and thank you for the

presentations. We're going to take a break now for about ten minutes, and I noticed that Dave Chagaris is back on, and so, when we come back, we will be going to revisiting some of the issues for Alabama/Mississippi, and, essentially, that's going to start with the information from Liese and John Hoenig that we talked about before and to respond to some of Dave Chagaris' questions and debate we had yesterday, particularly with Luiz, and so let's take a ten-minute break, and we'll be back.

(Whereupon, a brief recess was taken.)

CHAIRMAN POWERS: Welcome back, and thank you. The way we left it before, in terms of the agenda, we were going to revisit the issues relative to Alabama and Mississippi, in particularly some requests for information that Dave Chagaris had made, and Liese Carleton had agreed to provide some information, as well as John Hoenig was going to address some of those issues as well.

A number of people have to drop in and out, and I'm not announcing each one of them, but, if there are -- If you're not going to be available for something, drop Ryan or I a line, and we'll try to -- If it comes to critical things, we'll keep that in note, and so, at this point, let me turn the podium over to Liese or John Hoenig, or let's start with the information that Dave Chagaris asked for. Can you kind of start off with sort of reiterating what Dave asked for and that type of thing?

DR. CARLETON: Yes.

CHAIRMAN POWERS: Okay. Thank you.

FURTHER DISCUSSION OF MISSISSIPPI/ALABAMA

DR. CARLETON: Dr. Chagaris asked a couple of very specific questions, and the first one was he wanted to see sort of summary box plots of the vertical line data from just the calibration sites for the depletion study that we did in the Mississippi/Alabama region.

The first plot on the left there, those are the calibration sites from the Great Red Snapper Count, and there are fifty-four of those, and it's just showing the data summarized in box plot form, with three vertical line sets. On the right, we have the pilot study, because we did use pilot data for calibration. For the pilot study, we only did two vertical lines, which is why there's not a third there, but, again, a similar trend. I'm not sure if you would like to ask more questions about that or if you just want me to move on to the next question.

CHAIRMAN POWERS: Go ahead to the next question.

DR. CARLETON: Okay. Dr. Chagaris' second question was how often is the first ROV index greater than the second, and, again, this would only apply to the calibration sites, and so I put together a little table there, and the first column is when it's greater, and the second is when there was no change, and third is when the first was less than the second, and I'm happy to answer any questions.

CHAIRMAN POWERS: Let me first give the floor to Dave, to see his thoughts and so on.

DR. CHAGARIS: Thank you, Liese, for generating these plots, and so the reason that I asked this question is because I am concerned that each individual site cannot be considered a closed population, and that would be a big assumption that would have to be met for the depletion study.

For example, if you have an artificial reef that's surveyed by the ROV, and it's measuring fish right on the reef, and then there's fish swimming around the reef, outside the field of view, but then you put down your longlines, and is that going to actually start bringing in fish from outside the survey area, and, if so, if you're allowed to keep fishing and still maintain high catch rates, and it looks like they could be potentially the same as the first longline set, then you aren't really removing enough fish to substantially deplete the population, the local population, and it might also suggest that fish are moving in.

Because the density estimates are ultimately scaled to this total catch that you get, it could -- If fish are moving into the area, it could bias those densities higher. The second question this is, because you have acknowledged that the density of red snapper on artificial reefs is so high that you can't really count the number of fish, and then you go to the MaxN method -- I mean, I'm wondering if, just by chance alone, if you were to put the ROV down and get a MaxN count, and then put it down again and get another MaxN count, there is probably an equal probability that that second count could be lower or higher, and so, here, it looks like there's potentially a lot of noise just within the ROV data, with almost half of them, or over 40 percent, of the second ROV site taken being higher than the first.

It just gives me some concern about what is actually being

measured here and the scaling of that change in -- What's being measured by the index and then how that's scaled up based on the catch, if the population is not closed.

DR. HOENIG: Liese, do you want to answer that?

DR. POWERS: To your first question, we have good spatial information on distribution of habitat, and so, in general, the reefs are not on top of each other, and so there is at least 500 meters that is spread out on different contacts on artificial reef, and we have done experiments, and I think Jim Cowan's group has done similar, to look at the effect of distance that the fish travel to bait, and it's about 150 meters.

I don't -- I am not as concerned about the migration of pulling from other structures. Now, if there is fish off the structure, which generally there's not too many of, they're generally associated with that structure, and I'm not worried, and I think the assumption that we're dealing with a closed population that is centered around that reef is a justifiable assumption, and I will let John speak, or Liese, to the second issue.

DR. HOENIG: I would like to say something, and sorry, Liese, and I'm kind of elbowing you aside.

DR. CARLETON: It's okay.

DR. HOENIG: But I've been thinking about this quite a bit. The first thing is, with removals, the whole timeframe that we're talking about is very short. The ROV is down for an hour, and then the vertical longlines are fifteen minutes apiece, and so we're talking about a very short period of time, and then the follow-up ROV, and so, when you're talking about, well, maybe the longline is attracting, it's not a ten-hour soak, and it's not a four-hour soak, and it's a very short soak, and so it's not going to be pulling things in from far, and certainly not from other reefs.

Now, if there's some fish that are kind of swimming around the reef, but not on the reef, that might get pulled in, that depends -- What that does depends upon the behavior, and so, if they circle the reef, but they never come into the reef, then it's like they're a different population, and they are missed. We're estimating what is close to the reef and not what circles the reef, but never comes to it.

On the other hand, if they randomly go on the reef and off the reef and around the reef, then it doesn't matter. What we're

basically saying is you remove some fish, and the fish are redistributing anyway, and so, if there's always 25 percent that is away from the reef, and that remains the case even after the removal, then you get an unbiased estimate, and so the removal does not have to be a random sample of the fish that are there if the fish move around randomly, and so I don't really think that we have a problem with that aspect of the study.

The other aspect is about whether or not the second ROV count is just random noise around the first ROV count, and, yes, when you look at it and say, gee, 57 percent of the time, ROV1 is greater than ROV2, yes, but, when you take the mean, the means are significantly different, and so there's definitely the case, and you can test the hypotheses.

Is the mean count after equal to the mean count before, and the answer is no, and it's -- The test says it's lower, and so, in that sense, it's working for the aggregate, but it doesn't work for the individual site, and that's not uncommon with small sample sizes. You can't say what's going on in each component of the population, each reef, but, overall, you can say what's happening over all reefs, and so I think that we're also on safe ground there. Liese, did you want to add anything?

DR. CARLETON: I think you just about covered everything, John.

DR. POWERS: The other thing, Dave, is we do -- We chose to use this approach, but we also did total counts on it, and, as I suspected, total counts were often much, much higher than our estimates derived from this method.

CHAIRMAN POWERS: Thank you. Are there other questions about this, or anything about the Alabama/Mississippi section?

DR. HOENIG: I think the question is whether Dave is satisfied with our explanation.

DR. CHAGARIS: Definitely thank you for the explanation, and it's good to know that the distance between structures and the amount of time -- I still think some other -- Did you do the t-test on the ROV1 and ROV2? It would be interested to see how correlated the cumulative catch is with the delta ROV estimate, and I guess I feel like there's a lot of noise here that might be being attributed to a depletion effect, and so was total catch, total removals, correlated with the change in ROV?

DR. HOENIG: We did do a correlation matrix, and we did do the t-test, and I don't happen to have that in front of me, but we

were satisfied that the ROV count after is definitely lower than the ROV count -- The average ROV count after is less than the average ROV before.

DR. CHAGARIS: I think part of the reason I'm struggling here is that, with every other region, we have site-specific density estimates and variances, whereas, here, we're taking an average over multiple sites, and I feel like we might be losing some of the variability at each site. Those site-specific densities, from the ROV or the hydroacoustics, are averaged together to get a stratum density that's expanded, but, here, it's done differently.

CHAIRMAN POWERS: There is a variance estimate.

DR. HOENIG: We have a site-specific index of abundance, and that site-specific index of abundance can be divided by the calibration factor to give you a site-specific estimate of abundance, but that's basically --

DR. CHAGARIS: But the calibration factor is constant?

DR. HOENIG: It is. This calibration factor, in theory, could be subjected to various hypothesis tests, for example depth of the reef, size of the reef, type of reef, and so on, and we tried to do a little bit of that, but what we found was, as soon as you started subsetting your data and trying to do tests, you really didn't have much ability to distinguish whether the medium depth is different from the shallow depth and whether the medium depth is different from the deep depth, and so we basically would have been very happy to estimate more Qs, but the data didn't support it.

That being said, the approach of estimating a Q, or appropriate Q, if you take other factors into consideration, and then applying that to a population, is not an unreasonable thing. When National Marine Fisheries Service does a trawl survey, if they want to get an absolute abundance, they divide by a catchability coefficient, and, even though there is a great deal of noise at any one site, in terms of you could repeatedly trawl the same spot, and you wouldn't get the same answer every tow, that doesn't mean that you can't get an average abundance.

DR. CHAGARIS: Okay. Well, thank you for following-up.

DR. POWERS: Dave, that is the case. When I gave the data to Rob Ahrens, we used the coefficient, the Q, to turn all the MaxN numbers into a site estimate, and so that is the source, but

there is not one overall site estimate that Rob does have the variability in there, but the Q is based on the average.

CHAIRMAN POWERS: Thank you. Are there any other questions?

DR. HOENIG: If I could make one more comment.

CHAIRMAN POWERS: Please do.

DR. HOENIG: We are not assuming that the abundance is the same everywhere, and we are only assuming that the Q is the same everywhere.

CHAIRMAN POWERS: Okay. Mary, do you have a question?

DR. CHRISTMAN: I just want to go back to Dave's first question about the variability and your t-test for the table that you showed there. Well, the table doesn't have the results of your t-test, but I think maybe some of the confusion here is you looked at the delta for the calibration sites and then tested whether that delta was less than zero or more than zero, whatever direction it was supposed to go, but how does that compare to running an experiment where you didn't have any depletion efforts, but you ran the ROVs twice, once and then do it again?

In other words, how variable is that compared to the variability of the ROV's MaxN count each time? The question then becomes is some of what we're seeing just because MaxN varies between repetitions of the same site, or is it varying because of the depletion?

DR. HOENIG: We did not do repeat ROVs without vertical longline between them, and so I can't answer your question.

DR. CHRISTMAN: It's just a general question of MaxN has some noise associated with it, and I'm just curious as to how much of the total noise was due to that, and that's all.

DR. CARLETON: That's a good question. As John said, we didn't explicitly test this, and there were some sites where the ROVs saw fish, but there was no vertical line catch, and so I could look at those sites, if that would give a bit more of an answer.

DR. CHRISTMAN: That would be interesting. I am not asking you to do it, but I'm just -- It's a more generic, conceptual question.

CHAIRMAN POWERS: All right. Thank you then. Thank you for the explanations and the response to the question. With that then, we have the Louisiana portion, and Dr. Stunz will be presenting that information. Greg.

DR. STUNZ: I'm ready, and I'm just sharing my screen.

DR. HOENIG: Excuse me, and sorry to barge in, but I was also going to address Luiz Barbieri's comment.

DR. STUNZ: Okay. I will stand by.

CHAIRMAN POWERS: All right. Please do.

DR. HOENIG: I can do it later if you prefer, but --

CHAIRMAN POWERS: No, and I am giving you all the opportunity you need. Please do it.

DR. HOENIG: Okay. Yesterday, it got kind of late, and I got kind of tired at the end, and, in my defense, it was the second day in a row that I was up before the crack of noon, and so it was a long, hard day.

Luiz made some comments, and so I'm not entirely positive that I understood what his issue was, but I believe it had to do with combining strata and the weights and what should happen and whether there was a bias, and so I put together a very simple example, to try to show what the weights do and what happens with variances.

On the left is a diagram where I'm saying we have a sampling problem that consists of two strata, and you could think of it as one region has 80 percent of all of the artificial reefs, and another region has 20 percent, or you could think of it as going into two different fish houses, one of which has 80 percent of the red snapper landed in that port on that day and the other has 20 percent, and we decide to get either the mean ROV per artificial reef in the two strata or mean length of the red snapper in the two fish houses, and we do that based on simple random samples.

In the big stratum, 80 percent of the occurrences, we decide to sample ten reefs, or ten fish, and, in the other one, we sample a thousand, and you might say why would you do that, and, yes, that doesn't seem very sensible, but, supposing we did that, what happens?

On the top-right, what we're doing is calculating an overall mean, based on the sampling weight in the first stratum times the mean in the first stratum, plus the weight in the second stratum times the mean in the second stratum, and so the weights are 0.8 and 0.2, and they have to add up to 1.0, the whole thing, and so 0.8 times 140 plus 0.2 times 180 gives you a number that is very close to 180, rather than 140, because -- Sorry. That's very close to 140, rather than 180, because most of the weight is going to the bigger stratum, and so, even though we didn't sample it very intensively, we're giving a lot of weight, because most of the sampling units are in that big stratum.

The Ns don't enter into the weighting, and they only enter into calculating the means, because you sum up all of your observations and divide by N. When you look at the variance then of this overall mean, this stratified mean, it's the weights squared times the variances of the means, and so that would be the weight for the first stratum is 0.8 squared, and you take the sampling variance, the variability among the observations in the stratum, and divide by the sample size, N, which is ten, and, for the second stratum, you're dividing by 1,000.

Essentially, that second term, the variance for the second stratum, starts to approach zero, because you're dividing by 1,000, and what we're saying, is, yes, all your variability is due to the fact that you didn't sample Stratum 1 very much, and that gets magnified by the fact that the weight is very large.

The bottom line is we sampled the big stratum with very low intensity, which means we have a very imprecise estimate, but it's unbiased, in the sense that we can't say that our estimate is too high or too low, and it all depends upon whether our mean for that first stratum, 140, is too low or too high.

In this example, I'm assuming that the sampling variability within the units in the two strata is the same or similar. If we knew that, oh, Stratum 2 had huge variability relative to Stratum 1, then it might make sense to increase the sample size for Stratum 2, but, if the variances are similar, then making the sample size for Stratum 2 1,000 was not a good idea, and it basically said that we'll take a minor component of the population and estimate it very precisely, and the whole action is in the big stratum, and that we're not going to estimate very precisely.

In terms of what we did with unconsolidated bottom and

hardbottom in Mississippi and Alabama, we did not realize the importance of unconsolidated bottom and hardbottom, the huge amounts that are out there, and so we did not sample it as intensively as we should have, given great 20/20 hindsight, and so our estimate is not as precise as we would have liked and as we could have done with better prior information, but it's not unbiased.

I hope that that gets at what Luiz was asking. If not, then I apologize for misunderstanding you, Luiz, and I can try again, if you can help me to understand the issue more.

This second slide just says, for the Great Red Snapper Count, we had thirty-two sites selected, and the MaxN count was 8.48, with a standard error of 1.8, and Sean put this slide together and said, well, if we look at all the sites randomly selected since 2018, and so that's Great Red Snapper Count plus some other work he's doing, it's a bigger sample size, and it's a similar population, and so the mean isn't really changing, even though the sample size is going up, but the standard error of the mean is coming down.

That's kind of what we were trying to say, is, yes, more sampling error would have given us more precision, but the lack of sampling -- The lack of a huge sampling effort did not cause a bias, but just in precision, and that's all I wanted to say about that.

CHAIRMAN POWERS: Thank you. I will give Luiz a chance to respond, if he so desires.

DR. BARBIERI: Thank you, Mr. Chairman. Thank you, John. I mean, I think this helps me understand what you guys did and how this figures into that Table 6. What you're telling me is that those mean densities that show up in Table 6, page 84 of the report, those are actually weighted means that have already gone through the same process that you explained here, because, here, you're presenting weighted means that I agree with. Is that what it shows in Table 6?

DR. HOENIG: I'm afraid that I don't have Table 6 in front of me. Liese or Sean, can you help me out?

DR. STUNZ: I think Sean had to leave for another meeting, and he will be back, and maybe we can address this later or something, and I'm not sure, but he's not on the call right now.

CHAIRMAN POWERS: I am seeing Table 6 on the screen.

DR. CHRISTMAN: The means that are in Table 6 here are for natural, and that should be the weighted average over say the depth strata, or, in the case of Florida, it would be depth strata and the north, central, and southern regions, and so they should be weighted means.

DR. HOENIG: Yes, that's correct.

DR. BARBIERI: Okay, and the same, because, obviously, we're trying to achieve this in a way that each one of these parts of the different habitats becomes additive, so we can come up with a total number.

DR. HOENIG: Yes.

DR. BARBIERI: Do we have weighted means for the artificial habitats as well?

DR. HOENIG: Yes.

DR. BARBIERI: Then that takes care of it, John. That's exactly what I was trying to find out. Thank you for bringing this up, and I was wondering about that, and you clarified it. Thank you.

DR. HOENIG: Okay. Thank you.

CHAIRMAN POWERS: Thank you. Benny, do you have a quick question?

DR. GALLAWAY: Yes, and I asked a question yesterday about the 2.02 density per hundred square meters and the total area of 211 square kilometers, and the response I got was that the survey, the density, was for features on the average size of sixteen and 193 meters squared, but most of that area, the 211 square kilometers, is represented by nine features, each of which is considerably larger than the sample size, and, given that -- Every other natural bank in that depth zone, I have seen no densities like that anywhere, and I think we have a mismatch of the sample extrapolation beyond where it should be used to extrapolate.

CHAIRMAN POWERS: But isn't that 2.02 density per structure?

DR. CARLETON: It's per hundred square meters.

CHAIRMAN POWERS: I can't tell which is italics and which isn't.

Okay. All right.

DR. GALLAWAY: The 2.02 is density per hundred square meters. The survey area, which -- I just -- I can't accept this, and I'm sorry.

DR. HOENIG: I think Sean will have to address this, and I'm not positive that I have this right, but I think what Sean was saying is the big structures were not uniformly hardbottom and that there is sand mixed in, in places, and so I believe he was looking at these large features and saying, yes, but how much of that is actually hard, and how much of it is sand in between, and so it's kind of --

DR. GALLAWAY: I would refer you to the Corps maps.

DR. HOENIG: Yes, but he had some additional information, more fine-scale observations, to show that the Corps maps, where it indicates hardbottom, it's not uniformly hardbottom, and it's mostly hardbottom, but there's lot of sand in there, and so he had some observations on how much sand is there mixed in there, how much of the area is actually hardbottom and how much of it is sand.

CHAIRMAN POWERS: I am going to cut this off now, because we're going to have to revisit this, particularly in the context of the overall estimates, as we get into that, as so I would like to move on then to the Louisiana portion of it and Dr. Stunz, if we may. Dr. Stunz.

LOUISIANA

DR. STUNZ: I am presenting the Louisiana section for Dr. Cowan, and some of the work that you're going to hear about today was from me and a post-doc working with me, Dr. Coffey.

We had some serious sampling challenges, unfortunately, in Louisiana, due to some personnel issues, and so that led to a severe reduction in the amount of samples that we had that required imputation using Texas data nearby, across that Louisiana shelf, essentially, to generate those abundance estimates.

We had a lot of discussions with our analytical team, who, in just a minute, because they will be up shortly after this, to discuss how those imputation methods were actually applied, and we did have some rationale for why that should occur, but, before I get into that, we did have some sampling there, and

particularly Steve Murawski and the uncharacterized bottom, and I will show some maps in a minute, for some of those natural banks and uncharacterized bottom, and so it wasn't that there wasn't any sampling, but, particularly for artificial reefs, we definitely could have used more.

One rationale is just the sense that we have the western and eastern sub-stock management, in terms of how that's done, and so that's grouping Texas and Louisiana, and so there's some justification there for applying data that might be similar across that area, from some of our original GAM work we did in the Phase 1 component of this proposal, where we really looked at ecoregions throughout the Gulf, and based on habitat type primarily, but also a whole variety of other parameters.

It's very similar ecoregions and geologic features of that Louisiana/Texas shelf in that region, which, of course, I know that's not all the way, as you move further east, but there is some justification there, and then, also, Texas and Louisiana have very similar artificial reef programs, particularly as they relate to the predominant structures being oil-and-gas platforms.

The primary difference, from Texas, is there's only a few of them left in Texas that are still active and functioning, and that's not the case in Louisiana, and there's still a lot of them standing, and a lot of removals as well, of course, but there's a lot more artificial reef available habitat, in terms of oil-and-gas platforms that are still there.

To give you an idea of sort of similar maps that I showed earlier for Texas, in terms of what the natural bottom features look like, probably, and I'm certainly not a geologist, but, as that river is flowing from east to west, you don't get the predominance of shallower natural-feature bottom, and it's much more out on the shelf edge, where it's probably not covered and that sort of thing, with the sediment load coming out of the river, but, for whatever reason, the natural banks are restricted to our depth zone, largely, or close to it. In fact, there are some outside of our depth zone for natural bottoms.

Similar to Texas, and, again, not to scale, as we've been discussing, is the predominance of artificial reefs in this region. The differences that we see in our estimate here probably can be explained, in the sense that the snapper have a lot of artificial reef habitat and very little natural bank habitat in general, and so that's why you see the elevated abundance on artificial reefs in Louisiana.

The methods were similar, in the sense of the data collection methods, and so I don't want to go through that again, but, in terms of the imputation, and we can talk about that more, when you look at the ecoregions, we divided -- Essentially, what it was is the upper Texas coast down to the central Texas coast, roughly off of Matagorda Bay, was a similar ecoregion that we applied back to the Louisiana shelf, and so we applied both in situ sampling that we did on uncharacterized bottom, as well as some natural banks, as well as the sampling from natural banks in Texas and artificial reefs that we imputed that data back across to the Louisiana shelf.

The approach, again, was the same, and, obviously, we don't need to go through that, in terms of how we arrived at our species composition, but the one key difference, and this is why I want to bring this slide up, is that we did have very good species composition information from Louisiana, and it was a plot, obviously, exactly the same, because the data is being imputed across that, but, from a species composition standpoint, we had that information, and the species composition for Louisiana is in fact a little bit higher, more like -- Well, depending on what area you're talking about, what habitat, but, in general, it's about 15 percent on natural and artificial reefs, compared to further out west, but we had good information on that, from work that had been done.

Where we are with that estimate, and we can talk about the detailed imputation methods and how we broke that down in the next analytical section, and we're looking at a natural reef estimate of about 4.5 million, with a CV of 43 percent, and, of course, it would have the same exact caveats as we had with the methodology in Texas.

On the artificial reefs, it's about 6.7 million, with 31 percent, and, again, we feel that that's the reason, because of the prevalence of those structures in that region, and also lack of natural habitat, relatively speaking, compared to the other areas.

Again, this pattern holds the same, as far as uncharacterized bottom, with a lot of fish on that, and so the estimate there is about seventeen million fish, with a 27 percent CV, and the same sort of uncertainty caveats that we would have with the Texas estimate. Mr. Chairman, I know that's brief, and much of the discussion, I think, will occur around the imputation methodology that we utilized, that our analytical team is much more capable of discussing.

In that appropriate section I think is the best place to discuss that, and so I will be happy to answer any questions that folks may have, but here is the general same table that we provided for Texas, and it's a copy-and-paste out of the Table 6 that we've been discussing for so long, and so I will stop there.

CHAIRMAN POWERS: Thank you. Let me open the floor for questions or comments. We'll start with Jim Nance.

DR. NANCE: Thank you, Dr. Powers. Greg, I've got one question. For the artificial reefs, for the number of oil platforms, from what year did you get the number of platforms?

DR. STUNZ: This would have been when the assessment occurred for 2019, I believe, Jim, and I would need to go back and look and see, and keep in mind that those are not just oil-and-gas platforms. They're other things, in addition to oil-and-gas platforms, that we mined datasets, but I know your question is probably related around the rapid removal of those structures, and certainly that's a moving target.

It's a moving target for all of the reasons, because, literally, as we speak, stuff is being put in the water right now in Texas, and probably other places, as well as being removed, and that's a difficult universe to define, because it's so rapidly changing, but I would have to go look back in our report, Jim, and I don't remember offhand exactly which databases we used to generate that.

DR. NANCE: Okay, because it seemed a little high, because I know they have taken a lot of platforms out of Louisiana now, and they haven't added as much back, but I thank you, Greg, for that. Thank you.

DR. STUNZ: Yes, and something that I want to bring up, Jim, related to that, but also work that LDWF is doing, contracted through Benny Gallaway, and they had appropriated funds, and I don't know the details, and certainly maybe -- I don't want to put Dr. Gallaway on the spot, but, if he's willing to comment or not, and that's up to him, but roughly in the $2 million, I think, range, and they appropriated funds for him and his team to do a Louisiana-specific Great Red Snapper Count, which I know is underway, which I'm confident I think is going to provide a lot of the missing gaps that we have here.

As far as the timeline and that sort of thing, Benny has mentioned that I think they've pretty much finished with the

actual in-the-field data collection and crunching the numbers, and they may be further along, and I don't want to comment on that, since that's Benny's realm, but, to make up for I guess what we had to do here, and the imputation methodologies and any issues associated with those, Benny hopefully will be coming in shortly with a much more detailed sampling design and accomplishing that design, as well as more than, in fact, I think we would have done for this study, and so, anyway, I will go ahead and stop there.

DR. NANCE: Okay, and maybe I missed it in the report, but is there a place where you specify number of oil platforms against the number of other artificial structures?

DR. STUNZ: No, but we could get you that information, Jim, and, in fact, I've had some discussions with Benny, to really get to the bottom, and this is another good example of where mapping is critical, and having the most current, up-to-date maps is really informative.

I've had some discussions with Benny about really pinning down exactly the best, most accurate habitat availability at that particular time, which I mentioned is somewhat of a moving target, based on when BOEM databases are updated and whether they got removed or they didn't get removed, and you've got a whole other issue, Jim, as you well know, from trawling and other things, and did they really get removed, or is there still something that will hold snapper, and so that's really getting down into the weeds a little bit, but those are some of the challenges.

DR. NANCE: Okay, because I know that an oil platform itself, a large oil platform, is going to hold a lot more than a single pipe, for example.

DR. STUNZ: Yes.

DR. NANCE: Okay. Thank you, Greg.

CHAIRMAN POWERS: Thank you. Benny, did you wish to talk about this?

DR. GALLAWAY: Artificial reefs, in the Louisiana study, included wrecks and obstructions and the formal artificial reefs, as part of the Louisiana state program, and I have matched those numbers up very well, and you end up with the Louisiana study showing something like nearly 3,600 offshore oil-and-gas platforms of all types, and that's -- That includes

everything, and there is only -- There are less than 1,000 total structures, oil-and-gas, still remaining, and that 3,500 is an error, in my opinion, and, also, the application of the more Texas density, I have questions there too, having to do with water quality overall.

We are nearing completion, and our data are preliminary, but I can tell you that the number of structures, the number of artificial reefs, is badly overestimated in Louisiana.

DR. STUNZ: If I could respond to that, certainly we need to reconcile that, Benny, with basically the data that we have, in terms of artificial reef. As far as applying that to -- Those Texas data back to Louisiana, I mean, certainly, that imputation across there has its limitations, and water quality is just one example of that, but the issue is that's the best data we have to use. I think it's the most appropriate data we have to use at this point, and certainly your study coming in is going to shed a different light on that.

DR. GALLAWAY: As I may have mentioned before, I would urge you to -- It's not a small difference, and we're talking a big difference, a large difference.

CHAIRMAN POWERS: Benny, can you repeat that, what you just said, because you were cutting out.

DR. GALLAWAY: Yes, and I was saying the number of offshore oil-and-gas structures that are being used in this report is badly incorrect, and there's less than 1,000, and they claim to be over 3,500. Historically, there were. If you count all the installations, there have been that many installations, but they're not all still standing, and they haven't been for a long time.

CHAIRMAN POWERS: All right. Thank you. Any other comments or questions for Greg or others? If not, then next up on the agenda was Dr. Catalano, talking about the Gulf-wide tagging initiative, and he is not going to be available until after lunch, and so I think this would be a convenient time to break for lunch. It's a little early for the Central Time Zone people, but, at this point, we will --

DR. RINDONE: Dr. Powers, since we had previously spoken to Matt, before the last break, and he's in class right now, and we had told him that we would come back from lunch at 1:00 p.m., 12:00 p.m. his time, the lunch break should probably go until that time, so that, when he's getting out of class, he's

prepared and ready to jump back on.

CHAIRMAN POWERS: Yes, that was what I was going to continue on saying. All right.

MR. RINDONE: Good talk.

CHAIRMAN POWERS: So we'll get a little bit longer for lunch, and we will reconvene at 1:00 p.m. Eastern Daylight time. Thank you very much.

(Whereupon, the meeting recessed for lunch on March 31, 2021.)

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March 31, 2021

WEDNESDAY AFTERNOON SESSION

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The Meeting of the Gulf of Mexico Fishery Management Council Standing and Special Reef Fish and Socioeconomic Scientific and Statistical Committees reconvened via webinar on Wednesday afternoon, March 31, 2021, and was called to order by Chairman Joe Powers.

CHAIRMAN POWERS: Good afternoon. This is your Chair again, and we will resume. We're picking up the agenda where Dr. Catalano is going to be talking about the tagging information, and I haven't checked. Dr. Catalano, you're on?

DR. MATT CATALANO: Yes, I'm here and ready to go.

CHAIRMAN POWERS: Great, and so let us begin then, and I will turn over the screen to Dr. Catalano. Thank you.

GULF-WIDE TAGGING INITIATIVE

DR. CATALANO: Good afternoon, everybody. I'm going to talk about the high-reward tagging that we did, and this presentation -- The title slide here, with me being the only person on here, is just a reflection on who is talking, and, obviously, it's not representative of this huge team of people that worked on this in all the different regions and the Texas A&M crew of Danielle and Cara and all those folks there that were getting tag returns back, and they had a call center set up, and so, again, a huge amount of people that were working on this and contributed to

this.

What I'm going to talk about is, first of all, just our objectives, and so what we want to do with this tagging is to estimate some parameters of interest for the recreational red snapper fishery for the Gulf of Mexico, and the first thing is to look at -- Try to estimate regional and sector-specific exploitation rates, sector being private and charter, and so just within the recreational fishery, but splitting it into the private and charter sectors.

Before we go any further, we need to back up and just make sure it's clear that, as you'll see here in a little bit, we're not talking about exploitation rates on the entire population. We have some very realistic constraints on which fish we can get our hands on to tag, first of all, and so these estimates are going to pertain to -- As you will see, these are shallow sites, artificial reef sites, and so these are exploitation estimates on fish that probably experience some of the highest levels of exploitation of the entire population.

As we've seen from the overall Red Snapper Count part of this, there's a lot of fish, and these fish at these shallow sites, on these artificial reefs, represent probably a relatively small fraction of the total population, and so keep that in mind as we're going through, but, nevertheless, these are some interesting parameters to get at.

We would like to see if there are effects of distance from port on capture rates of these tagged fish, and we would like to estimate vulnerability to capture, and we can also get at tag shedding rates and discard rates. Along the way, we can look at movement or differences between tagging and recapture locations, and then we can assess angler awareness of the tagging program.

As far as the methods, and I'll just bounce through here, and it's kind of a long list, but we were tagging January to June of 2019, and we were trying to get out to fish, to tag fish, before the recreational season opens, but we put them out as close as possible to the opening of the season. We're using a relatively simple model, where it's just a within-season recapture model, and we're not looking at year-to-year -- We're not modeling survival from year-to-year, and so it's just put the fish out and more or less look at what proportion we get back in that first year.

We have these four spatial strata, these regions. Texas is split into east and west, and then we have Alabama, and then we

have the Florida Panhandle. We didn't tag down the west side of Florida. We thought that having high enough catch rates to get enough fish tagged would be in the Panhandle, and so we focused there.

We were trying to get 300 fish tagged per region, at at least thirty sites per region. We tried to limit the number of fish per site, just because of issues of the relatedness of the fate of fish tagged at the same site, trying not to select too many fish at each site, but also trying to balance efficiency with -- It costs money to go to more sites too, and so trying to balance there.

As far as the site selection, as you've seen, it depends on the region, and so we had a -- At the Panhandle region, we had different sets of waypoints, and so of these were in-house sites, known, high-relief structure, based on publicly-available sites, depending on the region, and that was true in Florida and Texas. In Alabama, as you saw, we have several years of randomly-selected grids that Sean Powers' crew has been doing side-scan sonar at and looking for bottom structure there, artificial reefs, mainly, and so that's a really nice program dataset that we were able to draw from, and so randomly selecting sites from that dataset of waypoints.

One other wrinkle to this is that we were trying to look at this effect of distance to port on the return rates, and that just really wasn't going to work out. In Florida, we didn't have -- These are mostly relatively close-to-shore sites, and not a lot of -- There's a lot of access points and not a lot of variability there.

In Alabama, we have a lot of sites to draw from, and we know, from experience, that we can get good contrast and distance to port, at least within that small realm, because we have so many sites spread across the shelf there. In Texas, we have relatively few access points, and there are some pretty big gaps there without access, but there are some sites there. We wanted to not end up with all of our sites stacked up right outside of the ports, and so we did stratify, with respect to distance from port, in order to get more contrast in our data, in terms of the distance to port, and that was done in the Texas region, so we would have sites scattered along the coast.

These are shallow sites, less than forty meters deep, and we're trying to minimize barotrauma, as much as possible, and so we decided to stick with these shallow sites, and we're tagging legal-sized red snapper, and we used the Hallprint dart tag.

We double-tagged every third fish, to get at tag loss, and these were done with hook-and-line, and we used a few different hook sizes, to try to get as wide of a range as possible of sizes of fish that we were tagging, and then fish were released with a descender device.

The tag returns, Texas A&M ran the tag return program, and so the phone line they had set up, and the phone number was written on the tag, and these are high-reward tags. Anglers would receive a $250 reward for returning the tag, and that was written on the tag, what the reward amount was, and the phone number.

The information we collected from the anglers were, obviously, tag number, when it was captured, where it was captured, which sector they were in, what they did with the fish, whether it was harvested or released, and then we had -- I mean, this is sort of a subset, and this isn't every single thing we asked, but these are questions that pertain to what I am going to show you today. We asked how they became aware of the tagging program and whether that awareness developed before or after they caught the tagged red snapper, and so did they know about it ahead of time or not?

As far as the modeling goes, we used a probabilistic tag return model using a Bayesian-type of analysis. It was fitted to the tag returns from June to October, and so we have returns beyond October, and there were really not many coming in during the winter, but, in 2020, we did have some more returns, and we're not analyzing those. The rewards ended at the end of the year there, and so we didn't analyze the 2020 data, because we weren't sure what to make of the returns.

With the response variables, we're looking at the capture fate, and so either the fish was not returned by anglers or it was returned, either in the private or charter sector, and, if it's a double-tagged fish, either with one of the two tags shed or not, and so there's five fates there, and so we could model the probability of fish coming back with one of those five fates. If it's a single-tag fish, there's only three fates, because the shed tag is not observable.

Then we had another likelihood, another Bernoulli distributed variable here of having to do with whether the fish was -- The post-capture fate, and whether it was harvested or discarded.

We're estimating regional and sector-specific capture rates for

fully-vulnerable fish, and then regional or length-based vulnerability parameters, using this exponential logistic curve, and it's a flexible function with three parameters that can accommodate a dome-shaped type of vulnerability curve.

We modeled a loglinear distance effect on the capture rates, distance to the nearest port, and regional tag shedding rates, discard rates, and then we added a term for site-specific mortality anomalies, or really capture rate anomalies, that we allowed -- Fish released at the same site would share some -- They would have some shared variability in the capture rate, and so that's a random effect in the analysis.

Then the tagging mortality, or the discard rates, are also included here, but there's no, really, information in the data on those, and so those are really totally dependent on informative priors that we specified, based on the literature values, and so we have the Campbell et al. analysis, which is a really nice meta-analysis of tagging mortality, or, really, post-release mortality studies that have been done, and so we had -- So we developed informative priors based on that analysis, and then there's some more recent work here, a couple more recent studies.

This is a weakness of the study, is that we don't have estimates of tagging and post-release mortality conducted with this study, and so we're having to rely on literature values, and so there is probably some room for refinement or sensitivity analysis, but we wanted to allow for uncertainty in these quantities to be carried forward into the estimates of all these other parameters that we're trying to estimate.

How does the model work? I have a visual here of kind of how we step through the fate of a fish that is released, and so in the blue boxes are things that we can observe, and the black boxes are things that we can't, and the gray are kind of like model quantities.

A released fish is either dead or alive, based on a tagging mortality rate. The tag is either retained or lost, and so the tag retention in this model is immediate, and so the tag is either retained or lost right away. Within that relatively short season, outside of the model, we looked at time at large and whether or not that was related to the proportion of double-tagged fish that came back with a shed tag, and there was no relationship, and so we just applied an initial tag loss, a tag shedding rate.

Fish were either captured or not, based on an exploitation rate, and I'm oversimplifying a bit here, because we have -- It's more complicated than this, because we have two different sectors, and it can be caught by either the private or the charter sector, and we have some fish that have two tags and some fish that have only one, and so, if you have a double-tag, it could be caught with one of the two tags, and so I'm oversimplifying a bit, but I just wanted to give you a sense and a flavor for how this is working.

The captured fish, they are either discarded or harvested, based on D there, the discard rate, and then they're either reported or not reported, based on a reporting rate, lambda, and then the fish that are discarded either survive or don't, based on a discard mortality rate. I will get to assumptions in a minute, but that's, more or less, how it's working, and so, for each fish, we have the observed data coming back on the captured fish that were either harvested or released.

There's a bunch of assumptions with any of these sorts of things. A big one is we're assuming a 100 percent reporting rate, and so that lambda parameter. We're assuming that every angler that catches a tagged fish calls it in, and we don't know whether or not that's true. We're going to try to get at that here in a little bit, and I'm going to talk about angler awareness and how they learned of the program. Again, that's not going to answer the question for us, but it can at least provide some information to interpret these data.

We have a $250 reward, which it's hard to know what sort of reporting rate that might induce, and we felt like it was high enough that we could reasonably assume 100 percent reporting, but we can't rule out that there are some anglers that aren't going to return those tags, or didn't even notice the tag on the fish, that notwithstanding.

Literature-based priors, and so we had those literature-based priors on tagging and discard mortality, and we're not estimating those parameters from our data. We're assuming negligible movement among the regions, and we're going to look at movement, and so we'll come back to that.

Vulnerability to charter and private anglers is identical, and we only have so many degrees of freedom here, and so we did have to make some decisions on shared variability and vulnerability. Distance to nearest port effects are constant across regions and sectors.

These site-specific capture rate anomalies that I talked about, and, at some sites, the fish are returned at higher rates than others, and those applied both to the charter and the private, and so, in other words, we're assuming that the charter and private are operating in the same space, that they're overlapping in the distribution of fishing effort, which we all know that there's probably some violation of that.

Immediate tag loss and discard rates are identical for the two sectors. We didn't deal with the commercial returns, and we didn't have a lot of them, but we didn't model them, and we included those fish that were eventually returned in the commercial fishery as releases, but we didn't model the commercial returns, and it's hard to know what to do with those, because we don't know the reporting rate on those, and it's probably a lot lower than the charter and private sector, but the exploitation, or the proportional rates of return, from the charter and private sector should still be valid to look at.

Then, again, the sphere of inference, we're looking at this relatively restricted part of the population of red snapper, and these are on shallow, high-relief artificial reef sites.

Here's our tagging sites in Texas. You can see the blue line is separating the east and west, and so we had a pretty good distribution of sites down the coast, and you can see our tagging numbers there, and we were able to hit our targets. In some cases, we did end up with more than ten fish per site, just depending on how the day was going and what catch rates were looking like and where the next nearest site was, and so there were some times where we tagged some more fish.

Here is Alabama and Florida, and the red dots are our ports, and so I measured the -- Those are the axis points, and so the distance to port was the distance between those red dots and each of the sites. In Florida, we had some sub-legal fish tagged in the Florida, and so we don't have our 300 fish there.

Here's the distribution of tagged red snapper, the size distribution in each of the regions, and a couple of things are worth noting. There's a lot of those 400 and 500-millimeter fish, and I think the Florida data roughly line up with what Will was showing yesterday, that, really, beyond about 600 millimeters, we're not seeing a lot of fish there, and so I think these are consistent with that.

Over time, these are the tag returns in each of the sectors, commercial, private, and charter. Again, charter and private

are the only ones going into our model, but I wanted to show the commercial returns, and then our model is fit to the data within those vertical dashed lines, but I wanted to show you the returns that we got outside of that too, and so you can see that we did get some fish back in 2020, but the numbers were a lot lower than 2019.

The dashed lines, that roughly corresponds with the open recreational fishing season, and it, obviously, varies based on the different regions, but we just wanted to standardize that, as much as we could, across the regions.

Here is just the overall tag return numbers and what they look like for each sector. The private returns, the largest number of returns were from private anglers, followed by charter, and then we did get some commercial, and, for a few people that called in, we didn't have that recorded, which sector they were in, and that was mainly -- That was outside of our model period.

Overall, the return rate was 32 percent across all regions, and so it was a very high return rate. It was 43 percent in Florida, and that includes -- That is all the fish, and so that includes even some of the sub-legal fish.

I mentioned this tag return model that we fit, and we actually fit eight different models that represented different combinations of regionally or spatially-invariant vulnerability, tag shedding, and discard rates. We estimated regional exploitation rates, and we didn't test whether or not -- Whether the exploitation rates differed between regions, because we were interested in just what those were, and so we just looked at -- We let the model estimate those, but we did explore whether or not we could model a common tag retention rate for all the regions or needed regional tag retention rates, for example, and so that's why we did this model selection.

The best model here was the one with regional vulnerability, but regionally, spatially-variant, tag retention, and release rates, but, really, none of the models -- Those top four models, there wasn't a lot of clear separation, is probably the best way to say it, and so there was some weak evidence for regional effects on some of these parameters.

What were the exploitation rates? What did those look like? This figure shows -- We have region on the X-axis and the exploitation rate on the Y, for private on top and charter on the bottom. These bars shows the median, posterior, and the 95 percent credible intervals for the exploitation rates, and these

are fully vulnerable exploitation rates, and so these are going to be higher than the exploitation rates on the overall tagged population.

We just wanted to show you the fully-vulnerable rates and the west-to-east gradient and the return rates and the exploitation rates, and we're seeing higher estimates coming in for the charter -- I mean, for the private sector, and then the charter, and we're seeing that. When we looked at the data, that's what we were seeing. In some cases, it's up to 60 percent of fully vulnerable fish were returned, and that would be for Florida, and so we saw really high return rates in Florida.

What about just the tagged population? If we just forget about fully vulnerable, and not all the fish are fully vulnerable, and so what does our tagged population look like, and so, here, we're seeing anywhere from 20 to 40 percent exploitation rates on the tagged population. Again, it's highest in Florida.

The Alabama rates are coming in at 25 percent, 0.25, if we add up the private and charter. That's a little bit higher than what we saw when we did some work over the last -- From 2016 to 2018, we did some tagging in Alabama, and the rates in this depth stratum were similar, although these were a little bit higher than what we found in our study.

Then you can see the estimates from Texas there, and so there seems to be a spatial gradient west to east, although keeping in mind that there was a little bit of different site selection in the different regions, depending on just the realities of the available sites that we had, to waypoints that we could go to to tag, and then, in Texas, we imposed a bit of stratification on the distance to port, and so that could have driven down the exploitation rates in that region a little bit, and so maybe a better way to look at this is to look at the distance from port and how that affects these rates.

This is looking at, on the X-axis, distance to the nearest port, and these are the capture rates, and so the finite capture rates, and so the proportion of fish that the model estimates would be captured by anglers, and the different colors are the different regions.

You can see there that there is a modest decline in capture rates with increasing distance to port, although it's pretty variable, and so the capture rate is not very well predictable from distance to port, although there is a relationship there.

What do the vulnerability curves look like? These are -- The vulnerability is a function of fish total length, and this is the length at release, and we didn't worry about -- Again, a model doesn't deal with movement, and it doesn't deal with fish growth, and so these are all based on the release length, which makes sense for a relatively short period of a tag-return period here.

Generally, they are dome-shaped relationships, and all of these estimates that I am showing are model-averaged estimates, and so we're taking the model weights from the WAIC model selection analysis and then applying those weights to the posteriors, to get a weighted average posterior estimate, and so dome-shaped estimates, and a little bit smaller modal length in Texas than in the east, and a fair amount of uncertainly. For example, in Texas, we had larger fish, in the 700 to 800 millimeter class, come in at a lot higher return rates than the model would predict, and so I'm not sure what to make of that, but that's what we got.

Then, in Florida, there's a lot of uncertainty in that dome-shaped relationship. Because we didn't tag a lot of fish over 600 millimeters in Florida, you can see that that whole dome-shaped relationship is riding on one datapoint, which is -- You never want to see that, but that's what we've got, and that's the data, and so there's a lot of uncertainty in that relationship.

I did explore some different approaches to sharing information, sharing vulnerability, between regions, but, for this analysis, I just kind of wanted to let the data speak for that they were, and so rather than trying to kind of rein-in the analysis by sharing vulnerability among like an east versus west type of approach, which would probably have helped with that Florida vulnerability estimate. The vulnerability in Alabama was very consistent with patterns that we got from our 2016 to 2018 tagging study.

Tag shedding rates, so, the data here, this is the proportion of double-tagged fish that were returned with a shed tag. Let's look at the tag shedding rate, because there's two different ways you can lose a tag, because you have two tags, but, anyway, the data that we looked at would be the proportion of those fish that come back with the shed tags, and so those are the dark bars, and that's the observed proportion, and then the gray bars are with the 95 percent credible interval, and so those are the model average estimates.

Then, from that, you can back out the tag shedding rate, and that was 6 to 13 percent, depending on the region, but that's the observed tagging shedding rate, and, again, similar estimates in Alabama, similar estimates to that recent study that we did in Alabama.

Discards, this is the proportion of red snapper discarded, ranging from 11 to 22 percent observed discard rates, and we didn't see a relationship with length. Some of there were out-of-season discards that happened within that timeframe, and we fit the model to the tag returns from the same timeframe across all the different regions, but there were some periods when the fishery was closed within that timeframe in some of the regions, but there were plenty of discards from during the open season too, and so some of this was just -- I don't know if it was anglers high-grading or what they were doing, but there was some discarding going on, and so those are the estimates there.

This is movement, and we didn't see much movement, at least in terms of the larger regions, and so there were -- All of the fish were recaptured in the region in which they were tagged, except for two, and two fish were tagged in Alabama and were recaptured in Florida, although they were not recaptured all that far from where they were tagged, and the Alabama region is a very small section of the coastline there, and we had plenty of sites in Florida just across the border, and so those two fish didn't really move very far.

Here are the distances, and so the absolute distance on top between tagging and angler reported recapture locations, and these are for fish -- This is the subset of the fish that had a lat/long coordinate that was reported by the angler, and so you can see that the distances are five to fifteen kilometers, and the average distance, in terms of the vector, distance vector, is going to be less than that, because it's taking into account all the fish and where they're going, and then the bearing, or the directional movement there.

Zero would be due north, and so Texas, west Texas, and Alabama have sort of a northern movement, and that makes sense in west Texas, but maybe not so much in Alabama, and then I know the roughly 80 to 90 degrees there in east Texas and Florida, and 90 degrees would be kind of a right, and so like an eastward movement along the coast there.

How did the anglers hear about the tagging study? On the top here is the answer to the question. When the anglers called in the fish, and said they had recaptured a tagged fish, we would

say, did you become aware of the tagging program before or after you caught the tagged fish, and so 40 percent of them said before, and 60 percent of them said after, and so, in other words, 60 percent of the people learned about the tagging program by noticing the tag in the fish and reading on the tag to call the number and then reading that there's a reward and then finding out more about it.

Thinking about reporting rate, you would like to see more awareness ahead of catching a fish, a tagged fish, because that would mean that it's less likely that people would just not notice the tag, or see the tag and think, well, I don't know what that is, and I'm not going to mess with it and just throw the fish back, and so I'm not sure what to make of these estimates, in terms of the reporting rate.

There on the bottom is how did you become aware of the tagging program, and so, for the 40 percent that knew about it ahead of time, word-of-mouth was important, and that's 20 percent, and then there's an other group, and social media and website are both less than 10 percent, and so we did have a great program, outreach program, but it still seems like a lot of people are getting this information by word-of-mouth, if they're getting it at all.

I'm about to wrap up, and here are the summary and conclusions. The exploitation rates were high, keeping in mind that these are shallow sites, artificial reef sites, and these are sites that we would expect to have the highest exploitation rates for this population. We don't know what fraction of the population these fish represent, but, again, these are significantly -- I mean, these are high exploitation rates, looking at 20 to 40 percent of these tagged fish getting captured, but not unlike what we saw in Alabama in that recent study.

There was a modest of the distance to the nearest port, with fish further offshore being less likely to be captured, and so that's interesting, and it's not unexpected, but it does provide a quantification of the potential for this -- If there's a protective distance effect and knowing now, with the spatial distribution, or having estimates of the spatial distribution of red snapper, from this Red Snapper Count.

Ultimately, it would be nice if we could take this information and overlay it with the spatial distribution of the population, because that's how you would get at an overall population level exploitation rate, but, anyway, this gets us down that road of trying to quantitatively deal with this potential for a

protective distance from port effect on these fish.

That's not to mention the fish that are not even on the structures, or at least in this unconsolidated bottom, which we can't even get at that point here, because we just tagged fish on these high-relief structures.

Dome-shaped vulnerability, that's consistent with previous studies and consistent qualitatively with the federal assessment model, although those estimates are in terms of age, and we estimated in terms of length here, but it's still a qualitatively-similar idea.

We didn't see much in the way of movement, as we would expect from looking at previous studies, looking at the literature. I believe that's my last slide, and so I would be happy to entertain questions, if we have time.

CHAIRMAN POWERS: Thank you. Let me open the floor then for questions and comments. Greg Stunz is up first.

DR. STUNZ: Thanks, Mr. Chairman. I just wanted to comment on Matt's role and presentation. While, obviously, as we discussed earlier, and I appreciate you letting him talk, since this tagging piece was not in the terms of reference, and even though it was high value to the study, for a variety of other reasons, exploitation rates and other things that will be very important for the management component, but our team felt that it was important, as I mentioned yesterday, for the SSC to hear what Matt has done, because there is some important information there that I think they can use in the future, even though what he was generating was not directly used in calculating the actual abundance estimate.

CHAIRMAN POWERS: Thank you. Doug Gregory.

MR. GREGORY: Thank you. I haven't been close to this project at all, and, when I originally heard about it, I though tagging was going to be the main component for estimating population size, and I see it's not.

One bit of information that I would like to see is days of freedom and how that affects some of the other attributes, like return rates, and I think looking at distance moved within the regions could be interesting, since it's probably seasonal. Clearly, you couldn't put all the tags out at once, and so days of freedom -- It seems that would be quite variable, and looking at 2019 versus 2020 tag returns might be interesting as well.

In the document, Table 11, and the graphs you showed, include recaptures through 2020, but it also has about 150, or 140, additional tagged fish than what is reported in the verbiage of the document, and did tagging continue past the opening season in 2019?

DR. CATALANO: I think the discrepancy there is probably some of these sub-legal fish that were tagged, and so the analysis -- The tags that we analyzed were just the legal fish, but there were some sub-legal fish that were tagged, particularly in Florida, and so those would be -- Depending on which -- If it's the model type of -- If this table has to do with just the tags that we put out, I included all the tags, even the sub-legal fish, but the model is just the legal fish, and so there's a little bit of a discrepancy there.

MR. GREGORY: Did you look at the effect of days of freedom on recapture rates? Clearly, if some of these sites were from the recreational sector, and they're aware of the sites, and I think 40 percent were aware of the study ahead of time, and they would be hitting these sites pretty hard, any sites they know about, because that's just the opening-season phenomena.

DR. CATALANO: I didn't look at the effects of days-at-large, and so you're interested in is there a decline in -- Within the season, is there a decline in the capture rates, or the exploitation rates, as you go through the season, and I didn't look at that. I did look at the effects of days-at-large on tag loss, but not on those return rates.

MR. GREGORY: You said you tried to tag fish as close to the beginning of the season as you could, which wouldn't give them much time to move about, given the open-season phenomenon, and so I was just curious if that could be observed, and the exploitation rates might be somewhat enhanced, or biased, upward, but I am not asking for anything specifically.

DR. CATALANO: Certainly, if we were interested in a more representative estimate of exploitation, ideally, you would tag and let those fish mix back into the broader population, so you could interpret your exploitation estimates as pertaining to the entire population.

We don't really know what those mixing rates would look like, and so we had to make some decisions on what part of the population can we even get at, and so I think the decision was that we would live with knowing that our estimates are really

not -- We can't really extrapolate them to the broader population, and they would be conditional, in that these are fish are the shallow, higher exploitation sites, and so that's part of the interpretation of our estimates, is that we can't really extrapolate to the broader population.

In that sense, it makes sense to get the tags out closer to the season beginning, rather than trying to get them out more ahead of time and letting them mix in, and so that's just kind of the decision that we made and a way to approach it, but certainly -- We can't assume that these fish are -- Just looking at the movement estimates, they probably did not mix a whole lot back into the rest of the population, fish off of structures and things like that, and so that's part of it.

MR. GREGORY: Thank you, and I thought it was kind of comical, the presumption that commercial fishermen would not want to turn in a $250 tag. I don't think that would be true, but thank you very much. I appreciate it.

DR. CATALANO: You might be right, and the problem is we don't know what the rate would be. If there are some commercial anglers in the room, that would be interesting, to get some feedback from them on that, but, presumably, they are very busy out there, and they have maybe not as much time to inspect each fish and things like that, and so I guess that's the thinking behind it.

MR. GREGORY: Okay. Thank you.

CHAIRMAN POWERS: Thank you. Dave Eggleston.

DR. EGGLESTON: Matt, thanks for your presentation. My question has to do with thoughts on sort of getting the biggest bang for the buck with respect to awareness, and so I wanted to get your thoughts, both on just awareness of the tag recapture study, but also just on the Great Red Snapper Count project in general, if you could comment on that. If you were able to redo the study, are there certain things you would do to kind of enhance that awareness, because it was a fairly expensive tag recapture, your study.

DR. CATALANO: Well, I don't want to get ahead of Marcus Drymon, and that was his -- That was a big part of the stuff that he was doing, the outreach and awareness stuff, and so I don't have specific thoughts on that. From my standpoint, I'm looking at how are these tagging studies typically done, and how do we get at the reporting rate, and one of the things that you can do is

vary the tag reward amount, and so potentially put out some fraction of the fish with even higher rewards than $250 and see if -- Do we get the same return rates on those higher-dollar tags, or is there a saturation point, beyond which, as the reward goes up, there is really diminishing returns, in terms of the return rate from anglers?

That's the way I would look at it, is could we afford to do some sort of a variable reward, and, I mean, in this case, it's expensive enough just to pull it off at this scale, with one reward amount, let along trying to have say $500 reward tags also out there, and so we kind of had to stick with -- We had to pick a reward amount and go with it, but, if it was me, and we had funding to do more, I would probably do some sort of a variable reward, and that might be a better read on the reporting rate than angler surveys and trying to translate what anglers' answers might mean in terms of a reporting rate.

DR. EGGLESTON: Were you working with the different state Sea Grant communications groups or commercial and recreational fishing associations at all?

DR. CATALANO: I personally wasn't, but, as a group, we definitely were, and Marcus could comment more fully on that.

DR. EGGLESTON: Okay. Great. Thank you.

CHAIRMAN POWERS: Thank you. We have Bob Gill and then Greg Stunz.

MR. GILL: Thank you, Mr. Catt, for your presentation. By way of background, I had a fish house on the west coast of Florida for thirty-three years, and we did deal in red snapper and grouper, et cetera, and I absolutely agree with Doug Gregory that every commercial reef fish fisherman would return that tag. $250 to them is a lot, and you would have had virtually a 100 percent return rate.

DR. CATALANO: Okay. Thank you. That's good information to have. Under that assumption then, we could try to interpret those commercial tag returns, and we didn't get a lot of them back, but it could be that they're fishing in other areas, maybe fishing in different areas than where we tagged, maybe a little deeper.

MR. GILL: I think using that assumption would be valid for drawing any conclusions and doing your analysis.

DR. CATALANO: Okay. Thank you.

CHAIRMAN POWERS: Thank you. Greg.

DR. STUNZ: Thank you, Mr. Chairman. I am trying to enable, o recognize, Marcus Drymon. I see he's coming in now, but he was having trouble unmuting, and so he's the one to -- I was going to talk for him, but I would rather that Marcus jump in here.

CHAIRMAN POWERS: Okay. Go ahead.

DR. MARCUS DRYMON: I'm with Mississippi-Alabama Sea Grant, Mississippi State University, and I just wanted to answer the question about angler awareness, and this is something that we actually looked at, through a series of surveys, and one of our primary findings was that roughly 60 percent of the people that we surveyed were aware of the Great Red Snapper Count, which we were pretty pleased with, and, for more details, we have a paper in press right now with *North American Journal* that's included as an appendix in the back of the Great Red Snapper Count, and so a lot of the details about angler awareness and satisfaction and things of that nature are included in those galley proofs, if you would like more information, but just a brief answer to Dr. Eggleston's question.

CHAIRMAN POWERS: Thank you. I want to close out this discussion. John Hoenig.

DR. HOENIG: I think this is really a very interesting study, and it's one of the only ones, or maybe the only one, that has ever bothered to ask people how they knew about the high-reward tagging program, because, clearly, if people say, huh, what high-reward tagging program, then there's the possibility that people overlook tags, and they just throw them back, because they think that I don't want another baseball cap.

There is one thing that you can do to interpret your results, and that is to say that, supposing -- Well, for simplicity, we'll say that you got back 40 percent of your tags, implying that the exploitation rate was at least 40 percent. If your tagging rate was 100 percent, that's -- If your tag reporting rate was 100 percent, then that's, in fact, what your estimate would be, that the exploitation rate was 40 percent.

On the other hand, if your tag reporting rate was only 40 percent, you would be saying that the exploitation rate was 100 percent, and that implies that your reporting rate couldn't possibly have been less than 40 percent. Otherwise, you would

be catching more than you released, which is impossible, and so, just putting in a few numbers, if your reporting rate is 50 percent, that implies that your exploitation rate is 80 percent. If your reporting rate is 80 percent, it implies that your exploitation rate is 50 percent, and so you can put together a curve like that, and you can kind of get some idea of where your reporting rate is and where your exploitation rate is, in the sense that the two together have to make sense.

It's probably not the case that your exploitation rate was actually 90 percent, or 80 percent, because your reporting rate is only 50 percent. If you believe that the exploitation rates could have been less than 80 percent, then that implies that your reporting rate was above 50 percent, and so you get kind of a range of possibilities that says, yes, that reporting rate makes sense, and that exploitation rate makes sense, and that is a way to get at how well did you actually do, what was your reporting rate, and I think that might be worth looking at.

DR. CATALANO: Thanks, John, and any estimates coming out of that, under lower reporting rates, would only be higher than the estimates that we reported here, and so that’s just to kind of give folks an idea of the estimates would only go up as you move that reporting rate down, and so a similar take-home message that these are pretty high exploitation rates.

CHAIRMAN POWERS: Okay. Thank you. All right. Moving on then, let me talk a little bit about the agenda. What we have now is Items IV, V, VI, VII, whatever, which is basically the abundance estimation analysis, and we have allocated, according to the agenda, two-and-a-half hours, and so that’s basically the end of today.

When we go through these, I want to give the authors the opportunity to kind of go through in the order that they wish to do that, and so we will certainly entertain questions, but I would really like to get that phase of it done today. Then, tomorrow, essentially, we come to some conclusions, collectively, and, at that point, I think, tomorrow, we’ll end up asking many more detailed questions, and we’ll have to have a very structured discussion, structured in the sense of I don’t want to bounce around from subject to subject.

My feeling was, because of the magnitude of the unconsolidated bottom and a lot of the questions thus far, we might begin with kind of focusing on that, the unconsolidated bottom estimate for each one of the states, regions, depths, whatever is most important, and so that’s kind of how I was seeing structuring

the discussion tomorrow.

Also, the consultants -- I'm not sure exactly -- Well, I am sure that they have a report that they're giving outside of this meeting, but I would like to give them a fair amount of time and the opportunity to report to us tomorrow about their conclusions at that point in time as well, and so that's how I'm seeing the rest of this meeting today and tomorrow morning going, unless there is some objection to that.

According to the agenda then, we have -- We are starting under II(d)(4), which is the discussion of the primary analysis, the validation analysis, the final estimate discussion with Greg, the discussion of sampling biases, and some conclusions and key takeaways, and that's the order that we're going to proceed, and, according to the agenda, that might take two-and-a-half hours. Of course, if we can do it more quickly, I would be appreciative, but I have my doubts. Anyway, are there any questions about the way I envision this, and, of course, there are. Mary Christman.

DR. CHRISTMAN: Unfortunately, this is, to me, the most important part of all of this, and I know there's all that technology, but it's the analysis of the information that was gathered that they spent extraordinary effort to obtain, and I have to ensure that it's been done correctly, and I cannot do that if I cannot ask Rob Ahrens, who won't be available tomorrow morning, any questions. I need to be able to ask questions, and I can't hold them off.

CHAIRMAN POWERS: Good point. I wasn't aware that he wasn't going to be able to be available.

DR. CHRISTMAN: Well, he's in Hawaii, and so I assume he's not going to be available.

CHAIRMAN POWERS: Well, I don't care where he is, but I thought he was going to be available. Greg, do you have a comment?

DR. STUNZ: Yes, Joe, and that was to -- For your agenda management point, I've got -- There's a lot for me on the end there, but I wanted to tell you that we've really covered a bunch of that.

Obviously, there are some conclusions and take-aways that I want to wrap up the piece, once we're done with all of this, but I think we could get right into those questions with the analytical team, primarily Dr. Ahrens and Dr. Stokes, who have a

lot to contribute right away, if that helps you speed things up. I don't think the other parts are going to take that long, once we get into the -- Obviously, we're going to have to discuss that, but I don't have a long presentation, in other words.

CHAIRMAN POWERS: All right. I have noted Mary's comments, and I agree with her, and I'm glad that she reminded me of that. This is the crux of the review, essentially, is the estimates, and, in fact, it's not going to bother me if we end up going longer than tomorrow morning too, and so it's entirely open, and we're going to proceed with questions to the point that is needed, and we will not curtail that, and my apologies for indicating otherwise. At this point, we are -- We want the presentations item for their abundance estimate and supporting analysis by Dr. Ahrens.

ABUNDANCE ESTIMATE AND SUPPORTING ANALYSES
PRIMARY ANALYSIS

DR. AHRENS: Aloha, Chair, and thank you. Aloha to the SSC and our reviewers. I will say that I can be available tomorrow, and I'm happy to join. I'm not happy to join at 3:00 a.m., but I can try and join as early as possible to answer further questions on this.

I have structured this to basically just step through, and I know we've had some discussion around these points, but I think it will be helpful to really clarify what the main issues or concerns are, moving through, and so this is a fairly quick presentation to lay out basically what was done, and really to facilitate the discussion that needs to happen around this.

I will begin with the unclassified bottom, since this is the one that sets really the magnitude for everything, and so, at the strata level, which was region, depth, and the random forest classification of low, medium, and high probability, the samples that were given for the analysis were assumed to be random samples with no measurement error at the strata level, and the means and the variances were calculated accordingly.

There were, at times, samples that had to be assigned to depth and random-forest-determined categories based on the geolocation of those samples post hoc, and the regional-level and the Gulf-wide estimates of abundance were calculated as expanded means of variance derived from the appropriate calculations for stratified mean and variances, and then for confidence intervals, using Satterthwaite approximations for degrees of freedom.

The majority of the Texas data came from acoustic paired with camera composition surveys, as well as camera sled tows, and the Louisiana data for this came from camera sled tows. Mississippi and Alabama came from camera sled tows, and the Florida data came from ROV surveys.

For some of the regions, depth and random forest categories data, due to logistical constraints, was not collected. This occurred in two strata in Florida, and the mean and variance associated with the most similar strata next to it was used, and, for Mississippi and Alabama, there were four missing strata, and it was determined that those should be assigned a mean and variance from the deepest most probable RF category strata.

This is a smattering of what the data at the strata level look like, and so the main headings on these -- 1 is Texas, and 2 is the second depth strata of forty to 100 meters, and this one is the random forest classification, and so 1, 2, and 3 are the lowest probability, mid-probability, and highest probability. Then we move across to Louisiana, Mississippi/Alabama, and then a smattering of the Florida data in here. The data is, obviously, zero inflated.

Interestingly, about 88 percent of the observations were zeroes, and then, depending on the region and the habitat type, the density estimates certainly varied, and there were some quite long tails from some of the data.

I put this in here so we have it as a reference to go back to, if needed in discussions, but, at the region, depth, and RF classification, we have a mean density and a variance and the sample size associated with that, the associated stratified, and the weight used in the stratified estimates, and then the strata-level population estimates.

Of note, here are some of the strata, and they appeared to have zeroes. Some of those were associated with small sample sizes, and some with moderate sample sizes. In the case of Mississippi/Alabama, we can see some similar numbers here, and that is the imputed values for habitats that were not sampled.

Then here’s the Florida estimates, and we, once again, can see occasions where we have zeroes, and some are associated with very few samples, and some zeroes are certainly associated with slightly more samples, and, again, we can see a couple of instances here -- Like, here, with the seventy-seven and the 210

sample estimates, those were used to impute to strata that were not sampled at the time, and so that's the unclassified bottom.

For artificial structure population estimates, we had Texas data coming from acoustics and species composition. The Louisiana data was imputed from Texas data, and the Mississippi/Alabama data came from corrected MaxN count from video, and the Florida data came from ROV surveys.

Again, at the region and depth level, the data were assumed to come from simple random samples with no measurement error. In Florida, the stratification provided no benefit across the structured categories, and, therefore, they were not used. In Texas, estimates were made for small structures and extra-large categories. Data imputed was just for extra-large structures. The Mississippi/Alabama data came from mainly small structures, and that had a correction factor applied to the observed counts, and that was a correction factor of 0.06, in this case.

The total population values, again, were estimated from expanded means and variances, and the total estimates for the number of sampling units in each category were obtained from state and federal records. Here's just a look at the summary data, and here we have Texas, Mississippi/Alabama, and Florida. Here's the overall estimates that we have by the size categories, the associated reef, the mean density, and the variance associated with it.

Again, for the known hardbottom population estimates, the samples were assumed to be random samples with no measurement error, and mean and variances were calculated accordingly. Texas were acoustic paired with species composition. Louisiana were imputed from Texas data. Mississippi/Alabama were correct MaxN count numbers per habitat unit, and, for Florida, we did not have a defined known hardbottom habitat, and it was inferred off of the random forest Category 3 habitat.

At this point, this comes back to a question that came up yesterday about discrepancies in sample sizes, and, while it's in my mind, if we go back here, there was a discrepancy in the numbers used in Florida, and this was just a numbers error. There were 749 samples that were taken in Florida, and then I believe there was 1,035 that appeared in a table, and that was just an error of including the imputed 210 and seventy-seven values in those total numbers, and so that can easily be corrected by removing those.

Here are the known hardbottom estimates, given the size of the

habitat, the mean densities that were calculated, the associated variances, sample sizes, and, ultimately, the population estimates that came from them.

For the pipeline estimates, it was classified into three categories. The total pipeline distance was calculated from the BOEM georeferenced datasets of active pipeline, pipeline that had not been removed, I should say, and some of it may not actually be active, and then the data was sub-sampled to account for potential biases due to spatial covariation.

To do that, I ran some Moran's I and some basic spatial autocorrelation tests, and that suggested that every fortieth segment would result in a lack of autocorrelation, and those data were then sub-sampled at every fortieth for each of the segments that were done, randomizing the initial starting point of where that fortieth sample was to start, and then, for each pipeline, the mean density per linear distance was calculated for each category, and then the total population values for each pipeline category were expanded from the means and the variances, based on the total linear length of that category.

Here we have the mean densities and the associated variances for each of those categories, and, here, the sample size was taken to be the number of pipelines sampled and the total distance of pipeline and the population estimate that we saw, and I will end there, and I will open it up for questions. Thank you.

CHAIRMAN POWERS: Thank you. First off, let's take, for example, the unclassified for Texas, or not unclassified, but uncharacterized bottom, or you called it unclassified, but uncharacterized. I had asked earlier today, and, I mean, that is coming solely from the C-BASS surveys, and I was told yes, at that time. Is that correct?

DR. AHRENS: That is not correct, no. The majority of the C-BASS surveys occurred at the deeper depths, and so this Depth Region 3, and there is some of those surveys that occurred in Depth Region 2, and the shallower regions came from the acoustic surveys, paired with the species composition, and so it was roughly about 3,000 of each.

CHAIRMAN POWERS: All right. Then I am sort of wondering -- To some extent, what is being randomized is the transect, and I am trying to understand all the individual little segments, and, in cases, they are -- I'm not sure what method is used where, and, in some cases, you're using fifteen-second intervals, and are all those -- I mean, all those are being treated as random

samples, but, in some sense, it's really the transect that's random, and these are replicates of that transect. I guess this goes to the discussion yesterday about cluster sampling. It would seem, to me, because of this, that the variance is -- It could be very underestimated.

DR. AHRENS: Yes, and I would agree with you.

CHAIRMAN POWERS: All right. Do you have any inclinations about how that might affect the point estimate?

DR. AHRENS: It's difficult to say, without getting back into the data, whether or not there would be a bias as a result of that, and my general thought is that it would not bias the point estimate, although, last night, I did do a two-stage cluster analysis on the pipeline data, and there was a slight difference in the mean estimate that was produced, but that was more of a function of the sub-sampling, and certainly the variance was much larger on that estimate.

CHAIRMAN POWERS: Thank you. I will pass it on to some of the consultants then. Mary.

DR. CHRISTMAN: Thank you. Okay. Let's continue with that cluster sampling. Clearly, these are not clusters. These are not independent observations, no matter how you look at it. When we talk about independence, in a design-based setting, we are referring to the observations that are selected independent of each other, and it's irrelevant whether they tend to be similarly valued or not.

What's important here is that, one, it was -- The cluster sampling effort was totally ignored, and it's actually a three-stage cluster, at least in Texas, because of the clustering of transects along a ship transect, those lines, and so that has to be addressed.

Secondly, I am very concerned about -- As one person pointed out yesterday, the sample size that is being currently used is totally artificial, because it's a function of the choice of fifteen-second grabs, and so you use a different grab, and you get a totally different number.

One way to get the variance down would be to look at -- To consider each transect in toto. In other words, look at all of the fifteen-second ones and come up with a single number that totally characterizes that transect, and now your only source of variation are the transects and how the transects are aligned

along the ship tracks, and so that would be one way to reduce variance.

The second thing is could you go to your slide on unclassified bottom population estimates, and I think it's Slide 5, and what are these strata sizes here?

DR. AHRENS: These are the number of ninety-meter-by-ninety-meter sampling units.

DR. CHRISTMAN: So these Ns represent -- The sample size and strata size refer to the number of ninety-by-ninety-meter cells that a fifteen-second video fell in, at least like in Texas, for example?

DR. AHRENS: Yes. It would be, given that observation, which of the ninety-meter-by-ninety-meter did it fall into.

DR. CHRISTMAN: Okay. The next question I have for you is how come some strata have no strata sizes? Did that strata simply not exist?

DR. AHRENS: That strata may not have existed.

DR. CHRISTMAN: Like let's take Texas 133, and there's eighteen observations in it though, and so were they eighteen that fell offshore or something, I mean further out than 160, or what?

DR. AHRENS: It could have been. Those could have been deeper than the 160-meter cutoff.

DR. CHRISTMAN: Then the next question is why are there no population estimates when you do have strata sizes, but you have a zero? Were they excluded from the summaries or what?

DR. AHRENS: Those were all observations that were zero.

DR. CHRISTMAN: Right. I understand that, but that means that the population estimate in that stratum is zero, that there are no fish in that stratum, but you ignored that in the summing. That's what I'm asking. You had to average over your sub-strata, correct?

DR. AHRENS: Yes.

DR. CHRISTMAN: So that would have been RF 2, I guess. Yes, it was RF 2. No, I'm looking in Mississippi, the Mississippi/Alabama, and so I'm looking at that line that is RF

2, which had forty observations out of whatever that huge number is, and so that zero actually has -- That zero estimate for that stratum actually has a weight.

DR. AHRENS: Yes.

DR. CHRISTMAN: So what did you do when you were summing across the three RF strata for that depth?

DR. AHRENS: Well, that would have been for -- At least at the state level, that zero would have included -- It would have been included in the calculation of the mean, stratified mean, density for that.

DR. CHRISTMAN: Okay. That's what I -- I was concerned that, for some reason, you just threw out all the strata that were zero.

DR. AHRENS: No, those were -- No, no, no.

DR. CHRISTMAN: Okay. That was where my confusion was. All right, and so thank you. Could you -- Why, on the -- You have another table where the number of reefs was not an integer, page 8. Why is that a non-integer, the number of reefs?

DR. AHRENS: That's for Texas?

DR. CHRISTMAN: Yes.

DR. AHRENS: That is the -- Given the small reefs that exist in Texas, and maybe Greg can comment on this better than I can, and it is average expected number of small clusters of reef that would make up a reef unit that they believe they had sampled, and so, in some instances, that reef cluster they sampled would have been five, or four, or three, of those little associated reefs, and so, to expand the -- Since you weren't dealing with a single unit there, but you were dealing with an aggregate of three, four, or five reefs that were considered the sampling unit, the total number of small units needed to be divided by the kind of average number of those small units that made up what was considered the reef that was sampled.

DR. CHRISTMAN: So this would be a case where, like if we were to go back, if they had sampled the whole row of structures and called that a reef?

DR. AHRENS: Yes.

DR. CHRISTMAN: Okay, and so they sampled, and then they had to adjust accordingly.

DR. AHRENS: The expansion. Yes, the expansion had to be.

DR. CHRISTMAN: Okay. What did you mean about the fortieth segment and the random start was within the first hundred?

DR. AHRENS: Yes.

DR. CHRISTMAN: If your systematic sample was every fortieth, you have forty systematic samples.

DR. AHRENS: Yes, you do.

DR. CHRISTMAN: I mean, since you actually have all the data, you could have just -- You didn't need to sub-sample, and certainly, by doing the first hundred, you were lopping off some of the observations.

DR. AHRENS: Yes, and it could have been randomized at the first forty. You are correct, yes.

DR. CHRISTMAN: Could you go back through the imputations? I'm not 100 percent sure that I completely understand the imputation numbers that you were talking about for Florida, and could you clarify that, please?

DR. AHRENS: For Florida?

DR. CHRISTMAN: 749 are what were sampled with, what, two missing strata, was it?

DR. AHRENS: With two missing strata, and so --

DR. CHRISTMAN: So it was 749 plus the imputed sample sizes for the missing strata.

DR. AHRENS: Yes, and so the -- As close as possible, the mean and the variance from kind of the nearest strata was used, and so, for Strata 513, we used the values from Strata 533, and then I think that's the right way it happened. Then, for Strata 521, we used the values from Strata 511.

DR. CHRISTMAN: Okay, and so that does represent sample sizes, unless you want to -- So it's not a big deal, but it explains why we have that difference, certainly.

DR. AHRENS: Right. It explains the sample sizes that ultimately went into the calculation of the means, the overall mean and variance, but it's not a measure of the samples that were actually taken, which is what was, in theory, reported in that table.

DR. CHRISTMAN: Okay. I think my other major question for you, Rob, is the column that is the area or number of structures that is in Table 6, for example. Those are all estimates in and of themselves, correct, and, in the case of at least Alabama, we actually have a variance associated with the estimated number of artificial reefs.

DR. AHRENS: Yes.

DR. CHRISTMAN: Or they could get a variance.

DR. AHRENS: Yes.

DR. CHRISTMAN: So that variance could easily be included in the estimated standard error for the number of fish that are -- The estimated number of fish that are out there, at least off of Alabama.

DR. AHRENS: Yes, absolutely.

DR. CHRISTMAN: My concern is that that would be an example of how inflated, or deflated, these standard errors are, if you were to compare the standard error with the variance associated with the number of artificial reefs into the standard error that you currently have listed, or I don't know that you have it listed, but it's listed there, and so I think it would be worthwhile, in the discussions of uncertainty, to at least address that that's estimable, the actual variance of the product of two random values.

Then, associated with that, another instance is the cases of where they used ROV to get estimates of species composition, and they used the average species composition for the region, or the sub-strata, and those averages have variances associated with them as well, and so the values that you're treating as fixed, with no measurement error, could actually be addressed. Some measurement error could be addressed for them.

DR. AHRENS: I agree with you 100 percent.

DR. CHRISTMAN: Yes, and it just feels like that there's a lot of variances missing.

DR. AHRENS: Yes, there are.

DR. CHRISTMAN: So I think I have gotten my big concerns. I know. The last one was the overlap of C-BASS and Texas sonar work, the just throwing them together. I just don't think that's a real good idea, and I have to think about that some more, but that concerns me a lot. Okay. I am not going to bother you anymore, in the interest of trying to move on. If I have more questions though, maybe I can send them through Ryan to you, and you could answer them via email or something.

DR. AHRENS: I will try my best, and I appreciate your comments, Mary. Thank you.

DR. CHRISTMAN: Talk to you later.

CHAIRMAN POWERS: Thank you. I think one of the key points that is being made here is there is -- Some of this extra variance could be estimated with the existing data. I think that's one of the key points. Steve Cadrin.

DR. CADRIN: Thank you, Chair. Thank you, Dr. Ahrens, on this presentation, on the analysis. Going back to Slides 5 and 6, which Mary was asking about, the stratum statistics, I see that seventeen of the fifty-four strata, and so 31 percent of them, had a mean density of zero and a variance of zero, and two of those had zero stratum size, and so they don't really impact the estimates much, but the other strata with a mean of zero and a variance of zero don't contribute to the stock-wide abundance estimate, as I understand it, and, I mean, they're in the calculations, but they don't contribute any fish to that abundance estimate, but they have considerable influence on the stratified variance.

If those unclassified bottom strata have red snapper habitat, then the true density is greater than zero, and it's small, and the true variance is greater than zero, and so I'm bit concerned that the mean equals zero and variance equals zero results -- Those are from densities that are below the detection limits, and so they're not true zeroes, either in the mean or the variance, and, for that and other reasons, I'm concerned that the stratified variance is biased low from those zero-variance strata, and have you given that any thought, or how much those zero strata contribute to the stratified variance estimate?

DR. AHRENS: I mean, they will contribute based on the strata weight, and I think you raise a really important point about

kind of the sample sizes and what we've learned about how variable the observations actually are. The numbers that we have here, we're assuming a CV of 150 percent, and we see ranges 200 to 1,000 percent, and so are the zeroes really zeroes?

I agree with you that they may not be zeroes at all. For the West Florida Shelf, of course, if they aren't zeroes, given the kind of magnitude of the area, they would, obviously, have this full effect on not only the population estimate, but the estimate of the variance, and there's a reality of the data that we had and kind of the information we had going into this, and I would love to be able to go out and do a couple thousand more samples on the West Florida Shelf, to make sure that those zeroes are really zeroes and that we have much more confidence in those means and the variances that we're using.

DR. CADRIN: Great. Thanks. That answered my question.

CHAIRMAN POWERS: Thank you. Dave Eggleston.

DR. EGGLESTON: Rob, thanks for the presentation, and, actually, I thought that you did a nice job of encapsulating the different data sources and imputations by strata, and, in fact, it was such a good job that I don't know if there's a way to like take the essence of your presentation and put it upfront, because I thought it provided a nice roadmap to the details that are in the remainder of the report, and that's just a comment.

Also, I knew that there was weighting by stratification, after reading the report, but then I got a little confused yesterday in some of the discussions, which sounded like maybe there wasn't weighting, and so this Slide Number 5 -- I think these sort of Excel spreadsheets would really be helpful in the appendix of the report, because it's nice to be able to actually look at how you get from these spreadsheets to Table 6, which I think is the table that keeps getting a lot of scrutiny, as well as Table 7. Then the last comment is just can you remind me how the values were generated for the weights?

DR. AHRENS: The weights is simply the strata size divided by the sum of the strata sizes.

DR. EGGLESTON: Okay. Great. So I think, maybe if there's a way to have these spreadsheets in the appendix, with a table legend that describes the weighting, et cetera, it would also help the reader. I guess, lastly, I just wish that all my grad students could have Mary on their committee.

DR. AHRENS: She is an incredible resource.

DR. EGGLESTON: Thanks, Rob. That's it.

DR. CHRISTMAN: I'm blushing. I'm blushing.

CHAIRMAN POWERS: Thank you. Dave Chagaris.

DR. CHAGARIS: Thank you. Rob, I noticed, on Slide 7, I think it was, where you were reporting the artificial structure population estimates, it says that the Mississippi/Alabama data had a correction factor of 0.06, but I recall, from yesterday's presentations, and what I was looking at last night, that that calibration factor was reported as 0.12, and so there's a big difference there, which would -- I am just wondering what's the explanation there, and am I interpreting those two numbers as being different numbers?

DR. AHRENS: I'm not 100 percent sure, Dave, and I would have to defer to Dr. Powers or Dr. Hoenig to that question.

CHAIRMAN POWERS: The other Dr. Powers.

DR. AHRENS: The other Dr. Powers, yes, and not our illustrious Chair.

DR. HOENIG: If I may, I'm not positive, and we'll have to check with Sean Powers, but I think that there was just basically just a communications screwup that we sent Rob results when he asked for it, and they were preliminary, and the first estimate was 0.06, but then, when we checked everything and improved the model, it changed to 1.2, and it appears that perhaps someone screwed up, and I don't know if it was on our side or Rob's side, in not correcting that factor, and so I think that's something that will need to be addressed, and it won't be hard to address, other than having to redo the calculations, and I'm not sure how much of a pain that will be for Rob, but I think that's a good point that was caught, and, yes, we need to fix that, and I don't think it will change the overall results by a whole lot, but, nonetheless, it should be fixed.

DR. CHAGARIS: Well, if you're dividing the ROV count by that correction factor, if it's twice as large, then your divisor is double, and so your population estimate would be halved. I think we'll see how the calculations turn out.

DR. POWERS: That is not Rob's fault, and that is my fault. When I conveyed the data to him, what I did is I did all the

conversions and then supplied him with the reef-specific densities, but I just gave him the wrong one, and the 0.122 is the correct one, and the densities reported in Liese's table are the correct densities per structure, but, yes, we'll have to loop back with Rob, to make sure how that changes his overall estimation in the procedure.

Again, for some of us, we hope that we have time to take all the review and the panel comments back and incorporate them before our numbers and everything are final, and so we will definitely do that.

DR. AHRENS: Can I make comment, Chair?

CHAIRMAN POWERS: Sure.

DR. AHRENS: I think the best route to go, since Dr. Powers and Dr. Hoenig also incorporated some additional uncertainties in their estimate, would be to use the estimates that they have come up with, as opposed to the one that I did, since, at the artificial reef structure, each area was treated separately and not combined into a random-stratified-type sampling approach. If it is 0.12, it would halve the number that I had come up with, but I know they have done a few other different things, and so maybe they can get together with Dr. Stunz and figure out what they would like to do.

CHAIRMAN POWERS: Thank you. Ken Roberts.

DR. ROBERTS: Thank you, Mr. Chairman. It was said, previously, that the artificial reef projections for Louisiana were imputed from Texas, and am I correct in that?

DR. AHRENS: That is my understanding, yes.

DR. ROBERTS: Okay, and that's resulting in about six million fish for Louisiana, and the rest of the Gulf is about three-and-a-half million. I am wondering how operational the word "imputed" is. It was said, earlier, that the reef number for Louisiana might be called into question, because it's too large, and so my bottom-line question is how was it imputed, and if the number of reefs, artificial reefs, changes in Louisiana, how would the number be affected? Thank you.

DR. AHRENS: I will attempt to answer this, and certainly Dr. Stunz can jump in and correct anything that I say that is misleading. I believe a subset of the structures, the large structures, from Texas were used as a representative sample of

what might be found in Louisiana. I can pull that data up and show it, if needed, and so, depending on, out of the Texas data, which ones were selected, that would affect the mean density estimate here.

The more important thing that was brought up was the discussion earlier today about the actual number of physical reefs that exist, and I believe that it was around -- The indications were given that it was a thousand or so reefs, in which case that would be roughly a quarter of the number of reefs that was used here, in which case, given the expansion, we would have to take a quarter of the total population estimate, if we were to believe the number of physical structures is much smaller than that which was used.

DR. ROBERTS: Among the team members, whose responsibility is it to figure that and make the correction, if necessary?

DR. STUNZ: Mr. Chairman, I can jump in.

CHAIRMAN POWERS: Greg.

DR. STUNZ: That would be our responsibility, as the team that is now handling Louisiana. Just to answer that question, I have my team on, and I don’t know if that’s something that we can produce, how quickly, but, for sure, we can get to 2,000 just in simple databases that are quickly available, and so that number would be, worst-case scenario, half of that, but I don’t want to comment on that number until I can have my team fully look at that, and that’s going to take a little bit more time, but that would be our group getting to the bottom of any discrepancy that might be there.

DR. ROBERTS: Good. Thank you. That’s all my point was, and I wanted to make sure that it wasn’t lost in all of the discussion. Thank you so much.

CHAIRMAN POWERS: Thank you. Mary Christman.

DR. CHRISTMAN: Thank you. Rob, I assume you did not throw out any observations that you received, because there was some discussion yesterday about outliers and throwing them out.

DR. AHRENS: There was one observation from Florida that was not used.

DR. CHRISTMAN: That was that 2,200 or something or other?

DR. AHRENS: It was Observation Number 350. Row 350 is the dataset that I am describing.

DR. CHRISTMAN: That's okay, since I don't have the dataset.

DR. AHRENS: Dr. Patterson brought this point up during his presentation yesterday, that that single datapoint, and I can't remember if it was one out of eleven datapoints, had a very significant impact on the estimate of total population in Florida, simply because of the expansion factor on the strata in which it occurred, to the tune of twenty million fish.

DR. CHRISTMAN: That was using your estimation procedures?

DR. AHRENS: That was using my estimation procedures, yes.

DR. CHRISTMAN: The means of density, and so the means of ratios, as opposed to the ratio of mean.

DR. AHRENS: Yes.

DR. CHRISTMAN: The next question I had was who assigned the region and depth RF classes? You mentioned, in your presentation, that, based on geolocation, you moved a few, but, otherwise, how were they assigned? Was that given to you as part of the dataset, or --

DR. AHRENS: I was given the geolocation of the samples, and then I dropped those onto the stratum that were used.

DR. CHRISTMAN: The final fifty-four strata versus the strata that you used in your initial estimation?

DR. AHRENS: Yes.

DR. CHRISTMAN: So most all of this was basically -- An awful lot of the data were post-stratified, essentially, because of geolocation.

DR. AHRENS: Yes.

DR. CHRISTMAN: Which means they are random sample sizes. Under normal circumstances, if things were allocated proportionally, that's not a big deal, but that certainly has an influence on the final variances, and those random sample sizes themselves have an influence, and it's not just in the earlier discussion that Dr. Hoenig showed, where the -- No, I'm sorry, and it wasn't Hoenig, but where the sample -- Like, for example, the

table that you're showing right now. The small sample sizes tend to have really high variances associated with them, because of the way that standard errors are calculated, and so that influences the overall variance estimates.

Finally, this is a question, maybe, for Greg Stunz. The imputation for Louisiana for reefs was to use the large reef estimate from east Texas, basically, and so my question then is, for the reefs in Louisiana, are those numbers, that show up in like Table 6 for example, all large reefs, or what proportion of reefs in Louisiana are large reefs, I guess is my question, and then I think I'm done.

DR. STUNZ: Mr. Chairman, may I jump in on that?

CHAIRMAN POWERS: Yes, please.

DR. STUNZ: Thanks, Mary. We're going through that exercise as we speak, and so at least approximately 1,000 of those are large reefs, and they also have an active artificial reef program that the rigs have been converted to reefs, and so, since it's dominated by the oil-and-gas platforms, that's why we chose the oil-and-gas platforms to impute -- To provide to Rob to impute those numbers across back to Louisiana.

However, as far as the percentage of that, we're working on getting to the bottom of that number right now, to make sure that -- We want to, obviously, get that right, and so, just to follow-up on that, we, obviously, want to get it right, but, even if there were no reefs, and we didn't calculate that, that would be six million fish, and so it's not a lot, but we want to make sure, certainly, that we have the correct number to use there.

Now, if you're suggesting, Mary, if we find some of those are smaller reefs, if we should use a subset of those to assign smaller reefs back to that, we certainly can do that as well, once we determine the composition of that number.

DR. CHRISTMAN: The only reason I'm concerned is, as I recall, the estimated number per structure was hugely different between the two reef types, and so I just wanted to get assurance that, in Louisiana, the majority are large.

DR. STUNZ: Yes, that's correct, but we're verifying that.

DR. CHRISTMAN: That's mainly what I just want to -- I am not worried about if 5 percent are small, and I'm not going to make

somebody redo the numbers because of that, but I just was curious. Okay. Thank you.

DR. STUNZ: That's definitely high on my to-do list.

DR. CHRISTMAN: Thank you.

CHAIRMAN POWERS: Thank you. Dave Eggleston.

DR. EGGLESTON: Rob, I just want to make sure that I'm making some correct assumptions about my initial read of the report, and so, obviously, we want to -- If you had great maps, then you would want to allocate your sample size to the footprint of the different habitats, and so, reading the report, it became apparent that the uncharacterized bottom was really important, and, therefore, that footprint was relatively undersampled, and so is that a correct assumption about sort of the initial workflow?

DR. AHRENS: I would say that, given what we know about how variable the samples are, given what was done, then, yes, the sample sizes that were allocated in the unstructured, or unclassified, bottom were low.

DR. EGGLESTON: Okay, and so what I'm wondering -- I'm looking at the weights again, and so the weights were scaled by the footprint of the strata, which you just said a little while ago, and so is there a way to scale those weights to sort of the proportion of samples in that footprint of habitat? I realize it's complicated by the fact that you've got individual structures, like pipelines and reefs, but I was just wondering if there's any way to scale the weights.

DR. AHRENS: Well, the weights are just from the size of the unclassified bottom, and so no other structures factor into that weight.

DR. EGGLESTON: But for -- I guess is there a way to do that across the strata, and so across like unclassified bottom versus hardbottom versus your low and mid-RF strata?

DR. AHRENS: The strata unit here is a region, a depth, an RF class, ninety-meter-by-ninety-meter, approximately, square area, and so that is a strata, and so, when you look at the strata sizes, that is the number of those ninety-meter-by-ninety-meter units that are within the region, depth, and random forest classification. I feel that we might be cross-talking here on something.

DR. CHRISTMAN: David, the strata sizes, although they're numbers that are number of cells, each cell has an area, and so they actually are just proportional to the area that's associated with that region, depth, and RF class, and so what is happening is they take the density in that area, and then density per unit area, and then multiply it by the area. That's what they're doing there. That's what those weights are doing.

DR. EGGLESTON: Great. That helps. Thank you very much.

CHAIRMAN POWERS: Thank you. Any other questions at this stage? If not, then we would move on to the alternative estimation analysis validation, the ratio of the sums.

MR. RINDONE: Harry has his hand up.

CHAIRMAN POWERS: I'm sorry. Harry.

MR. BLANCHET: I had a couple of questions regarding the stratification itself, but I think I will just send those by email.

MR. RINDONE: No, those need to be asked.

MR. BLANCHET: Okay. Going back to the initial point of the development of that random forest model, it seems, for -- I was trying to understand how those classifications ended up, in terms of how important that classification was to the final estimate of the number of fish, and it seems like it's fairly significant, and, when we look in the eastern Gulf of Mexico, it seems like there was a fair amount of information to help train, or estimate, information to guide that model development, but, in the western Gulf, there wasn't nearly as much, and so I was wondering your thoughts -- I heard something that I might have taken out of context, but I was wondering about your thoughts in terms of the amount of information that was available in the western Gulf to help decide, especially for this unclassified bottom, of what bin you were going to put them in.

DR. AHRENS: Those are great questions, and let me address one of them first, which is the notion of the categorization from the random forest model and the choice of the thresholds to divide it into the three categories and the influence of the shifting of those thresholds and the influence that may have on the overall estimate.

That we didn't explore, and it certainly could be explored, and

it would take some time, because everything would have to be post-hoc assigned to the new strata derived from the random forest model, but we do -- For regions and areas that have higher density estimates, if we were to re-jig that random forest classification, it would change the weighting of those strata sizes.

Now, how it would affect it is hard to say, because some samples would be put in and some samples would be put out, and so exactly the effect on the mean relative to the strata weight is -- I can’t intuit it at this point, but it would be potentially an interesting exercise, to look at the sensitivity to that classification.

The other question is the availability of data to help inform the model, and certainly, within the western Gulf, we were relying more heavily on vertical line, or bottom longline, data, but we also did have the underlying red snapper relative abundance from the work that Nicholas Duchon-Barth did that helps to inform that western part of the Gulf reasonably well.

One could start to play the game of adding and removing datasets from the overall random forest and look at how that influences, again, the classification of the habitats, but we did not, again, do that.

MR. BLANCHET: So the vertical line is essentially something that’s going to give you information on structure, because that’s where those vertical line samples were coming from, is from structure. The longline samples is something that would be informing of the unclassified bottom, and one of the issues that I had -- I had looked at the Table 1 in the random forest presentation that’s in the appendices, and there is a SEAMAP longline dataset that has a number of samples of 198.

I inquired of Gulf States Marine Fisheries, and, as best we can come up, we’ve got about 345 stations that should have been -- Some subset of that should have been included, but, more significantly, the dataset that you have has eighty red snapper. The dataset that Gulf States has has 576 red snapper, and so that’s some higher proportion positive. That might influence where some of those geographic areas drop out, in terms of one, two, or three, and I don’t know how influential that might be.

DR. AHRENS: Let me just clarify one thing that we did with the SEAMAP data, is that we -- There was an overlap in datapoints in the SEAMAP from the CSP program, the Congressional Supplemental Program, and so we pulled the Congressional Supplemental Program

datapoints out of the SEAMAP dataset before we used it.

Now, I'm not certain that that explains the difference that we're seeing between the eighty and the 500 fish, and I would have to go back and re-look at those datasets, but my guess is that it's accounting for some of the difference that you're seeing in that table.

MR. BLANCHET: Okay. The CSP was primarily 2011, and is that correct?

DR. AHRENS: You're stretching my little gray cells at this point.

MR. BLANCHET: You and me both. Most of the positive samples that I am looking at, for at least Louisiana, where I actually have the data, are coming from 2012, 2013, and 2014.

DR. AHRENS: Okay. This is -- We can go back and kind of check, and we can look into that, in terms of the data that went into the model.

MR. BLANCHET: Thank you. The correction positive, what I've got for 2012 in Louisiana, is 0.44, and 0.38 in 2013, for the fraction of the samples that actually have positive fish.

DR. AHRENS: Right. Okay. I would have to go back and do some digging, to fully understand what was going on there.

MR. BLANCHET: Thank you.

CHAIRMAN POWERS: Thank you. Any other questions of Dr. Ahrens at this time?

DR. BARBIERI: Mr. Chairman?

CHAIRMAN POWERS: Go ahead, Luiz.

DR. BARBIERI: Thank you. Rob, a general question, real quickly. I'm trying to understand the big picture here of what was done, and, in terms of the sampling design and then implementation of the field components and how sample sizes and sampling and allocation sampling meshed into all of this.

I mean, I would expect that, for a study of this magnitude, and considering all those different strata and the subdivision of the overall area, that you guys would have followed something similar to like a probability proportional to a size sampling

allocation scheme, where some of these vast areas that are very large strata, like the uncharacterized bottom, that we know very little about, and, therefore, we could expect a high degree of variability in the distribution, I mean existence, in the distribution of red snapper there, and, I mean, you reported that I think 88 percent of those samples turned out to be zeroes.

Those areas would have been allocated higher sample sizes, and so it could maintain -- The constraint would be to keep your variance, or CV, within desired levels, and so you would estimate what the sample size needs would be for those different strata, keeping that constraint in mind, and so departures from applying that approach are likely then to really tremendously increase the precision of the estimates, and can you try and explain to me if my thinking there is correct and how it fits into what was actually accomplished here?

DR. AHRENS: I will try, Luiz. There is two aspects to this, and one is the total number of samples that you need to take to meet your overall precision objectives, or, if you have cost objective, you can do it that way, or some other objective, but, here, we chose to meet our level of precision, and I showed that equation yesterday, and that has it at, of course, your assumed variance that might exist in that final estimate, and the total number of strata kind of factor in there, and then, at the individual strata level, you have the weight of that strata, the variance of that strata, and then the cost of sampling that strata, and then, here, we just ignored the cost, and so we're really talking about strata weights and the variance that exists.

Given those variances and the weights, those all go in to influence the overall sample size, and certainly the basic assumption we made was that the coefficient of variation would be about 150 percent, and that, of course, is not what we observed it to be, and it would be higher, and, therefore, you would have allocated a much larger number of samples. In fact, in terms of a stratified random sampling design, if it had truly been implemented in a random way, it would probably have been cost prohibitive, given the extremely generous budget for this program.

Once you have that total number, then those get allocated between the strata, and that allocation is a function of the strata size, and the large strata will get more samples, and the strata that have larger variances will get more samples, and then, of course, strata that cost more, if you're considering

cost, would actually get less samples, and so your intuition is correct that the larger sized strata that have higher variances would receive more samples.

In the case of some of the Texas observations, where we saw coefficients of variation pushing 500 to 1,000 percent, you would have allocated a lot more samples to those regions, and we did have, certainly, more samples from those regions than we initially allocated in the preliminary design, and there are some challenges, given the nature and the structure of those samples, as it would relate to a stratified random sample design.

CHAIRMAN POWERS: Luiz, do you have any follow-up?

DR. BARBIERI: No, I've got it. Rob, thank you.

CHAIRMAN POWERS: All right. Thank you. Dave Eggleston.

DR. EGGLESTON: Luiz just asked the question that I've been trying to get at, but much more eloquently, and so I was just curious, and I have two questions. One, is it possible, Rob, just to share this Excel spreadsheet with me? I was just kind of curious about it, and I wanted to make sure that I'm looking at the formulas correctly. Then, secondly, any idea how sensitive the population estimates are to these weights?

DR. AHRENS: In terms of the strata weights?

DR. EGGLESTON: Yes, and I was just kind of curious. I mean, how -- I just feel like there's -- At least in my mind, there is some uncertainty in how those weights were -- How they are truly reflecting what's out in the field, and I was just kind of curious about, if you do sort of the classic sensitivity analysis on those weights, and I'm just wondering how important those weights are with respect to those population estimates.

DR. AHRENS: The only way the strata weights would change is if the random forest designation changed, and so we could explore it that way and explore the sensitivity, and I haven't done that, but, because the weight of the strata is basically the physical size of that strata, the region and depth effect on that weight would not change, and it's only the random forest classification, and, again, for areas that have -- For areas that would have less than an order of magnitude differences in the estimated mean densities, my guess is they would have an effect, but not a large effect.

As the order of magnitude of difference of those mean estimates changes across those strata, and you change that strata weight, it would have a more noticeable effect on the overall estimate, but, yes, we would have to go back and fundamentally change the random forest classification, either the data that went into it or the thresholds that we ultimately decided upon, to change that kind of low, medium, high probability classification, and certainly that sensitivity could be explored.

DR. EGGLESTON: Okay. Thank you.

CHAIRMAN POWERS: Thank you. Kai Lorenzen.

DR. LORENZEN: Rob, I was wondering -- I mean, given that we've established that the CVs that came out of this design-based analysis are likely to be a massive underestimate, I was wondering what -- Would it be possible to at least get a somewhat realistic estimate of the true CV without too much effort, so that we could -- So that we can have a more informed discussion about sort of the actual magnitude of uncertainty?

I was thinking, and Dr. Christman pointed out a number of things, in terms of the video sampling, and I guess one could bin some of those fifteen-second samples, and one could -- Then I'm talking specifically about the uncharacterized bottom here, which is such a big part of this, and accounting for the three-stage cluster sampling, and doing some model-based variance on things like the species composition.

It would seem, to me, that maybe some of that could be accomplished relatively quickly, and it would give us at least some idea of what actual uncertainty we're looking at, and I was just wondering how realistic that would be. Thanks.

DR. AHRENS: I will say something, and then Mary will correct me at this point, but I would say one of the quickest -- A quick-and-dirty way would be to take the sample sizes from some of the towed acoustic stuff and take the sample size not to be the individual cell, but the transect. That would be kind of a really quick-and-dirty way to look at how that variance would expand.

I can't comment on how variable some of the composition is and stuff, where I actually haven't seen that data, and so I would have to defer to Greg or Jay or someone to comment on that, but certainly that -- If you assumed those were independent, they could just be added onto the variance estimate that resulted.

DR. CHRISTMAN: I'm just going to jump in quickly. Your idea about the reconfiguring to make it a transect piece of information instead, what you would do there is your strata sizes would be functions of area, rather than cell numbers, because you would then associated each transect with an area covered, and you would just have an estimate for each of those areas, and so, yes, it's doable.

DR. LORENZEN: Thank you. I mean, I do think -- I don't want to encourage a lot of sort of corner-cutting work, but I do think that it would be useful for us to have some idea of what a more realistic estimate of variance might be. Thank you.

CHAIRMAN POWERS: I think it would be very useful, but, when you say relatively easy, in terms of the SSC's timeline right now, we're talking forty-eight hours, or less, actually, now, and so we have to think of it in that context as well. John Hoenig, you asked for the floor.

DR. HOENIG: Rob, I think you've given a very interesting presentation, and it's quite clear. I imagine that you're probably getting punch-drunk after all this time, and I did have something that I wanted to ask you, and I apologize if you've covered it and I just missed it, but I'm getting kind of overwhelmed with all of the information that's being put out.

I am wondering about post-stratification, the idea being that, if you go over an area of bottom that's just sand, you might not see much at all, but, if you happen to go over a sunken container that was previously unknown, then that's a structural feature that didn't go into any kind of planning, and so you might find that, oh, 12 percent of our transects go over unknown features, and, if you were to create a new stratum, by dividing a bigger stratum into two small strata, one with unknown features and the other the remaining sites, then you might find that you're apportioning the variance quite well, because, when you find unknown structures, you're likely to find fish.

You can't define a stratum in terms of whether there are fish there or not, because fish is what you're trying to measure, but you could define a stratum in terms of whether or not it has a structural feature or not.

The problem is that you also have to estimate the amount, or the size, of the stratum, and so, basically, you would be estimating how many transects, of all possible transects, will you find previously unknown structure, and, if you do that, then you could have a stratum with previously unknown structure and the

rest. Did that enter into what you were doing, because it wasn't quite clear to me if that's what you were doing, and, if so, how did that improve the variance?

DR. AHRENS: We thought about that, and then we slapped our hands for thinking about it, and it's my understanding that it would not be a wise choice, given that you then have to know what the total size of that stratum was, and, basically, the only inference you can make about that is it has to be in proportion to what you've observed it to be.

DR. CHRISTMAN: Can I jump in and ask a quick question related to John's question? That is, basically, does every observation in the UCB actually have bottom, observed bottom, available, because I expect that, with some of that nepheloid layer, that you probably -- Even with the sonar, you may not be able to specify it explicitly.

DR. AHRENS: You would have to take the backscatter, and the strength of the backscatter, and then ground-truth that with habitat. In theory, it could be done.

DR. CHRISTMAN: I'm just saying that I expect that would be an enormous amount of work, to post-stratify the UCB based on what you actually saw out there, in terms of bottom type.

DR. AHRENS: Certainly the visual surveys all have it.

DR. HOENIG: I would have thought that sonar would be a very easy way to see elevations on the bottom, and you could define a new stratum with previously unknown elevations off the bottom, and the fact that you have to estimate the size of that stratum as the proportion of your transects that had surprises shouldn't be a problem, and it's part of the statistical sampling literature. Anyway, it was just a thought for writing things up for publication, and, obviously, you're not going to do that in the next forty-eight hours.

CHAIRMAN POWERS: Indeed. Luiz.

DR. BARBIERI: Thank you. Rob, just one more. Sorry for bringing up that Table 6, the page 84 Table 6, once again, but, as I look through that table, and I try to estimate -- I did some quick calculations, looking at what the sampling fractions were that used for each one of those strata there, and, mostly, they fluctuate around 1 percent as a sampling fraction, right, and I think that --

CHAIRMAN POWERS: The area or the fraction of fish?

DR. BARBIERI: Fraction of the area or number of structures, and so the sample size collected, the number of samples collected, from the frame that was defined and so it can be number of structures or area. For example, forty-five divided by 1,775.

DR. AHRENS: I'm with you.

DR. BARBIERI: Right? I can see how, for a study like this, where we're trying to actually develop estimates of absolute abundance, that we could get away with a low sampling fraction like this, if we have, somehow, a way to do a sampling -- Optimize our sampling allocation, but, in reading the report, and listening to all the presentations yesterday and today, I get the impression that most people -- That this couldn't use the random forest model, and that whatever sampling frame and sampling allocation scheme was developed, based on that, for a variety of reasons, that couldn't be followed. Is that your assessment as well, that that sampling fraction may not be sufficient to really meet our precision requirements for this project?

DR. AHRENS: I will answer a couple of things that came up there. Given the logistical constraints, it was cost prohibitive to follow the initial random stratified design, and it just logistically couldn't happen in some of the locations. We were able to do it in Florida, and, just given the equipment that had to be used in the west, it just was not going to be obtainable and feasible.

In terms of the kind of the fraction of habitat that was sampled and was it sufficient, I think, if we are honest about all sources of variance and kind of the final goal of achieving a 30 percent PSE, then no, it was not likely that it was sufficient, if we're going to be brutally honest about the variability that probably exists in that final estimate.

DR. BARBIERI: Great. Thank you so much, Rob.

CHAIRMAN POWERS: Thank you. Doug Gregory.

MR. GREGORY: Thank you. Rob, I will give you a break. My comment is more for the Chair and for the SSC than a statistical one, and we've had some analyses overnight, like we do with stock assessments, that are explanatory, and I think that's great, but I think we also should identify other suggested modifications that may not be achievable this week, but are

still considered to be warranted.

I would hate for us to be constrained by only asking for things that can be done overnight, and this is a major, major undertaking, and the questions and stuff that I've been hearing are reasonable for something like this, and not to take away from what's been accomplished, but there's more that needs to be done, and so, if we can identify short-term versus long-term re-analysis, or considerations, then suggest it to the PIs, and we identify it as such and just move on. Thank you very much.

CHAIRMAN POWERS: Thank you. I have no -- I think that would be a reasonable idea, and it doesn't have to be done within the next day, and presumably we can deal with it as the SSC, or even perhaps after this meeting, as long as it isn't forgotten, I think. Along those lines, Harry.

MR. BLANCHET: Yes, Mr. Chair. To that point exactly, one of the comments that I've heard a couple of times is the thought that, rather than sampling every fortieth point along the transect, that all transect points be sampled, for the pipelines specifically, but I guess that could also be true of some of the uncharacterized bottom. My hunch is, and I might be wrong on this, but my hunch is that one-fortieth of the video has been counted at this point, and so I don't think that that is an overnight request.

CHAIRMAN POWERS: No, I didn't believe it was either, but let's hear the answer to the question about -- I mean, not all of that video has been analyzed, and is that correct?

DR. AHRENS: I believe all the C-BASS video has been analyzed, and I will defer to Dr. Stunz.

DR. STUNZ: Joe, I would have to get back with you on that exactly, and it should have all been analyzed, and then the subsets provided, but I don't know that. I mean, obviously, if it requires any type of video analysis at this point, that's a major, major undertaking, but I don't have a good answer for you for that towed -- What of that was analyzed and what wasn't.

CHAIRMAN POWERS: All right. Thank you. I think now would be a good time for a fifteen-minute break, and we haven't broken. Steve Murawski, can this subject wait? Can it wait until we get back, Steve Murawski, or is your question short?

DR. MURAWSKI: My answer is very short. All the video from the C-BASS has been counted, all 15,000 fifteen-second bins.

CHAIRMAN POWERS: Okay. Thank you. All right. Let's take a fifteen-minute break, and we'll return. Thank you.

(Whereupon, a brief recess was taken.)

CHAIRMAN POWERS: Okay. It's 3:40 p.m., Eastern Daylight time. Next on the agenda, as I understand it, is the discussion of the alternative methodology of estimation, which was using the ratio estimator.

MR. RINDONE: Joe, we have a request from the council to have an opportunity for the public to provide some comment at the end of the day today, and so not to interrupt this portion of the agenda, and I think this should definitely go now, but, after Dr. Stokes is done, if you want to consider a point for allowing that today.

CHAIRMAN POWERS: Why did they want it done today?

MR. RINDONE: It's a four-day meeting, and it's kind of a long process and a lot to take in, and just to provide the opportunity for some members of the public to --

CHAIRMAN POWERS: It was on the agenda, and so that's fine. If they want to have it today, that's fine.

MR. RINDONE: So it would be to have an opportunity, a brief opportunity, at the end of the day today for it, and then also again after the SSC meeting, at the end of the day on Friday.

CHAIRMAN POWERS: All right. Thank you then. So we are going to do the -- Dr. Stokes will be making a presentation about the alternative methodology as a form of validation.

VALIDATION ANALYSIS

DR. LYNNE STOKES: Thank you very much, Mr. Chairman. This analysis was done a lot by my graduate student, Shalima Zalsha, who has now graduated and is working a real job.

I overheard Mary, during the break, saying I wish we had talked about a planned analysis and then what they actually did, and, by golly, that's the way I've constructed these slides, and so I didn't do them in the five minutes since she talked, and so let me start with this little bit.

The planned sample design was a stratified random design, as

we've talked about here, with three stratification variables having to do with the region, the habitat, and the depth, although not all strata were present in all regions, and so the number of strata varied by region.

I think they were not present -- My understanding is that they were not present in the world and not just that there wasn't any sampling there, and so, anyway, that was the plan design. The planned analysis, based on that design, for me, was the standard one, which is you can estimate a total, in this case total abundance of fish, with just the sum of the estimates of total over the strata, and so that was the plan.

T_h-hat represents the total abundance in Stratum h for these various strata, but T_h-hat varied, the way I did it. If the sample units were items, like artificial reefs, for example, things that you could count, then T_h-hat was just the mean per-unit estimator, the usual one, which I have denoted here with that kind of messy notation as t-hat hy, mpu, for mean per unit, and so it's just the regular old unbiased estimator of total, where N_h represents the number of units in the frame and Y_h-bar is the average per unit in the sample.

I am going to take a little timeout here to say something that's been -- It seems to have been kind of a source of confusion over the last couple of days, and that's about weighting. Didn't you use weights? Well, the weights are actually part of -- The way I have represented it here, and I believe the way that Rob did too, is it's part of the estimator, and so, if you think of that Y_h-bar as the sum of all the Ys over n_h, then the weight is just N_h over n_h, and that weight attaches to every observation that you measure in that stratum that has N_h in the frame and n_h in the sample, and so you don't have to go back and add extra weights to it, because, the way we present the estimators, the weights are already included in the mean per unit estimator, and I hope that makes sense.

Anyway, that was the first estimator that I used for countable objects, things where the units, the sampling units, were actually individually listed on the frame, but most of the data wasn't that way, and it was actually area, and so it will be transects, and there was one other exception, and that was in Texas, where there was -- They ended up estimating the small artificial reefs with a design that -- They didn't have a list of all the little pyramids, and so that also was a ratio estimator.

The way you commonly estimate the total, when you have units

that vary in size, is with a ratio estimator, and the ratio estimator is simply the total in the universe for some variable that is related to the thing you're observing. In this case, the thing that is related about the sampling units are their size, and so t_{hx} denotes the total area of the universe, the stratum, from which that sample came.

Then you multiply that by an estimate of density, and the estimate of density is the sum of all the observed values in that stratum divided by the sum of all the sizes of the observed units, which are transects, in that area, and so you could also represent that as t_{hx} times this density of Stratum h.

These two estimators are appropriate when the observations have been independently selected, and that is when the observations that you're referring to are a simple random sample. My observations are transects and not pieces of transects, but transects, because that's what was randomly selected, and so they aren't clusters. I mean, they are clusters, in a sense, if you want to think about them being grids, but nothing was done with those individual sub-units in the sample unit, because this was a cluster sample and not a two-stage sample.

Again, that was the plan. The variance of the stratified estimator was estimated by the sum of the estimated variances, because the sampling was independent from one stratum to another, and so these are just your textbook versions of the variance estimate for simple random samples for either the mean per unit estimator on the top there and the ratio estimator on the bottom there, and the only thing that's a little bit -- Maybe the notation is a little non-standard for you, but the s^2_{hd}, which is different than the variance estimate in the first equation.

The first equation has the plain old sample variance, the s^2_{hy}, and that's just the observed values, the sample variance of the observed values for all the units in the stratum. S^2_{hd} is a little bit different, because this is a ratio estimator, and this is -- Again the usual estimator of variance from a ratio estimator is based on a delta method or Taylor Series approximation of the variance, and so S^2_{hd} is the variance of the residuals, which I have put a formula down there, which is kind of neither here nor there, but it's just based on a linearization idea of a ratio, and it's a standard way to estimate variances for simple random samples when you estimate it with a ratio estimator.

In some of the ratio estimators, the number of units in the

universe is not actually known, the N_h is not actually known, and so you have to estimate it, and that N_h does not enter into the estimate itself, and so I'm not estimating anything for the ratio estimator itself, and it just comes in in this finite population correction factor for a ratio, when we can't observe the number of units, and so you may say, well, why can't you observe the number of units, and remember that a unit is a transect, and, in reality, the transect only exists after it happens, because you don't know what it is.

I mean, you could plan how big that transect would be, and then you could take the total area in the stratum and divide by the number of transects, and that would give you -- I mean, excuse me. Divide by the area of the transects, the average area, and it would give you the total number, but, in reality, the transects vary a little bit from one to another, and so you can't really observe that exactly, and it's not like we just looked in grid cells or something, which, if we did that, we could tell, if we knew the area of the stratum.

Anyway, if you do what I just said, which is to estimate the total number of units, where unit is a transect, in the whole stratum, and you plug it into that little expression that I have there, and, actually, the correction factor is just going to be one minus not the ratio of sample number to population number, but rather sample area covered divided by the area and the population, which we do assume we know for each stratum.

Anyway, that's what I did, or that's what I planned, and I'm sorry, and the estimates and the standard errors were computed in SAS PROC SURVEYMEANS, which includes both ratio estimators and mean per unit estimators and a variance, and we did it stratum-by-stratum, instead of dumping all the data in at once and weighting it, just because it helped us to keep things straight, because there were so many different strata.

Now, I am sort of acting like the sample design that was planned was the one in the sample analysis was the one that was done, and that's not quite true. There were some changes.

First of all, the sample sizes changed from what was planned in the beginning, due to some complexities of data collection, and I just assumed that the sample was missing completely at random. In other words, I did not make any adjustments for so-called non-response, missing data, except that I reduced the sample sizes to the ones that we actually had, rather than the ones that we had planned.

The one other change that happened from the plan to the execution was the pyramids, the small artificial reefs in Texas which are referred to as pyramids, and they found -- I am a little bit fuzzy on why this happened, and I just got the data, and I really didn't know exactly why this revealed itself as it did in the middle of sampling, but, anyway, maybe Greg can talk about that later, but, instead of sampling individual units, like having a pyramid be a sampling unit, since they didn't really have any way to --

They didn't have a frame for them, and so you couldn't just pick a random number of them, a random sample of them, and what they did was they gridded the area over which these occurred and sampled grids, grid cells, and then again used a ratio estimator, where the Y, the observed value, is number of fish, but now the X, rather than area, is number of pyramids in a gridded cell. In other words, it's a ratio estimator with the auxiliary data being number of pyramids in a grid cell. That was the exception to using mean per unit estimators wherever the observations were countable things.

One other exception to the original plan was, as I understand it, post-strata were added beyond the original plan in Florida. Now, I did not use any of the random forest information for post-strata, and I post-stratified -- I think this is correct, but I was given these post-strata, and they were named northwest, mid, and south, and so it's regions in Florida, which I don't think were in the original stratification design, and so that meant they were post-strata, and, as Mary suggested, when you post-stratify, rather than stratify, it does add a little bit of variance.

That is actually not -- You don't add very much variance, if the sample sizes are reasonable in the post-strata, which they were in this case, and so it really only matters sort of in two conditions that adds variance, and one is when you don't have very much data in every one of them, and the other is if you're mostly concerned with a marginal variance, and, in other words, before you start the sample -- If you're trying to predict what the variance will be, the fact that you have a random number in each post-stratum -- You will end up with a random number in each post-stratum, and that really does affect the variance of the estimator.

That's not what we're doing here, and so what I calculated, by ignoring the fact that these were post-strata and treating them like they were strata, is, basically, I calculated what's known as a conditional variance for them, and, once I saw the sample

sizes in the post-strata, I used those sample sizes, and so, effectively, that's a -- Given the sample size in the post-stratum, this is the variance, which I can observe after they are allocated.

Those were changes to the plan, and there was another change to the plan, and I think you've heard about this. Data from Louisiana were mostly unavailable, and so we did a substitution, and I don't like to call this an imputation, exactly, because we didn't do anything fancy to impute.

In other words, we did not -- We didn't generate new data based on the Texas data or anything like that, but we just simply used the Texas data averages in Louisiana, and we just substituted, and then we expanded by the ratio estimator, and so, in other words, we got the d-hat from like observations in Texas, for virtually every stratum, and we thought we were observing either the t_{xh} or the n_h for those strata in Louisiana, although we heard today that maybe we've got to go back and look at that and see if we got the numbers right for either the area or the -- I guess it was really the discussion was about the artificial reefs.

Anyway, for the artificial reefs, we used a mean per unit estimator, and we just used the n_h-hat, the universe sizes for those strata, in Louisiana applied to the substituted Texas data.

Actually, because of that switcheroo there, I didn't feel like it was appropriate to combine the Louisiana variance with the variances of the rest of the Gulf of Mexico, and so I just made a variance estimate for everything but Louisiana, because we were reusing data, and so it wasn't -- You can't count that as extra sample size, and so, anyway, that's why, in the Table 7, there is no estimate of variance, or CV, for the Gulf-wide, because the additive variance formula would not be correct, and you would need to look at a covariance, and you could probably figure out what that is, but I didn't. Okay. That was Louisiana.

Now I'm going to talk a little bit about variance estimation, and there's been a lot of discussion about the fact that the variance estimates may be biased low, due to two things that have been brought up, and that is ignoring measurement error and autocorrelation between the observations.

I'm going to take both of these and talk about why I don't think, in some cases, they're as big of a problem as they might

have been represented, and so the first one deals with measurement error.

Under certain simple measurement error models, the measurement error actually is included in the variance from noisy observations, and so our observations themselves are noisy, and so, when you look at the variance of those noisy observations, it actually, under certain models for measurement error, correctly incorporates that additional source of variance in the sample variance of the noisy measurements, and so I'll talk about that in a little bit more detail in a minute.

The other one about autocorrelation is this thing that we discussed a lot at the end of Rob's discussion, and they said, well, one thing you could try is to not cut up the transects, and that's what I did. I didn't cut up any transects, and so that's already been done, and so that's not going to take -- If my estimates are okay, then that's actually the variance that was computed in that way that was suggested, and so, anyway, that's that.

Let me go into a little bit more detail about the measurement error and see if you guys buy this. The reason that measurement error variance is captured in the measurement-error afflicted, as I have put it, observed values, the noisy observed values, is similar to the reason, for you samplers out there, that variance for multistage sample design can actually be estimated almost unbiasly with only looking at the between-PSU variance.

In other words, if you have -- Let's just be concrete and say we have a two-stage design, and we know that the real variance of an estimator from a two-stage design, where you first -- Like if I had -- Well, if I'd had a transect, and I had just picked a few observations from the transect, that would have been a two-stage design, and I would have had a source of variability from the transect-to-transect variability, and I would have had a second source of variability, because I can only estimate the total for the transect, whatever it is I'm measuring, and so that has noise in it too, and so the real variance -- If you write down the variance, and not the estimated variance, but the real variance, of the estimator of a two-stage design, it has two pieces.

One piece reflects that variability of the PSUs, or primary sampling units, and the second piece captures the variance within the PSUs. However, you actually can't estimate those two pieces separately, because you never get to see the truth. If you have sub-sampled a transect, if I had 100 grids in it and I

picked ten of them, and I made an estimate of the total from those ten, that's not the truth.

That's not the true total for the transect, and so that is an estimate, and so, when you actually compare those estimates with each other, if you have two PSUs and you're looking at the variability between them, but you have only estimated the totals in each, you actually incorporate some of that second-stage variability into the first stage, what you think is the first stage, variance of the observed estimators. For those of you who have a copy of Cochran on your desk, you can look at Section 10.4 and see him discuss that at length.

That is true if the first stage of a population correction factor is small, and that's true for virtually everything, because the Gulf of Mexico is big, and we don't sample a very big fraction of it, and we don't have transects that cover a very big fraction of it, and so that's why every sampling software, every survey sampling software, from SAS to R, everything, never -- There is a multistage design, and all it does is to calculate the variance from the PSU to PSU variable, and, in other words, it calculates basically sort of an S^2-hat, which is the sample variance, but not based on a real thing, because you didn't see them, the real measurements, but rather on the estimated measurements from the various PSUs.

Anyway, that sort of veered off into something that's going to come up about a correlation again, but measurement error is really kind of the same thing, because, when you observe a value of abundance in a transect, but you've had to do some kind of fast-and-loose stuff with gear and all this kind of stuff, and it's not really the number, well, that observation has variability in it.

I said that it doesn't matter at all under a simple measurement model, and here is just the very simplest measurement error model that exists, and that is, if you suppose that the observation that you see is really a sum of two pieces, the truth, which I'm calling Y_i here, and some sort of measurement error that is zero mean, and, in other words, this model, I will readily admit, doesn't incorporate bias into there, because I have said that the mean of the epsilons are zero, which, if there's bias in it, that's not going to be zero, but, anyway, this is kind of a white-noise measurement error model.

Then the variance of Y_i-hat -- One further assumption is that the truth and the error are uncorrelated with each other. If that's true, then the variance of this thing that you see is sigma-y^2

plus sigma-e^2, and so here's a picture trying to represent why it is that, if you take the variance of the observed values, which are noisy, you get something that is more variable than if you were able to take the sample variance of the truth.

The first line up there, with those blue arrows, let's just suppose those arrows point to the truth somehow, and maybe along some number line or something, but, when I add measurement error, these things wiggle. The blue lines, the arrows, wiggle, and I don't really get to see that point right there, and, instead, I see some point near it, and so forth, on all of them, and so I went here and kind of wiggled and then just dropped an arrow.

If you calculate the variance -- If these things are number lines, and you calculate the variance of the red arrows, it's bigger than the variance of the blue arrows, on average, at least, and I have orchestrated it so that it's also true in this picture, but, on average, if you just calculate s^2, very luckily, it inflates the variance that you get above sigma-y^2, and, in fact, it inflates it exactly the right amount.

This is just the math version of that picture, which says, if I average those observed values -- This is just a plain old sample mean that you're introduced to in your first stat class, and the variance is the variance of the observed units over N, but these observed units have the variance of sigma-y^2 plus sigma-e^2, and so, if I calculate this thing that I'm calling S^2-hat over N, it's actually an unbiased estimator of the actual variance, or it estimates that inflated variance. All those estimates that come straight out of SAS SURVEYMEANS are like this.

Now, here is, of course, the catch, is that not all of the measurement error has this really simple model, and some of it does, actually, because it's -- I mean, except that epsilon -- I am guessing that all the epsilons have some sort of bias associated with them, and so maybe the epsilon-I wouldn't have an average of zero, but it would have an average of some B, and that actually doesn't affect this variance.

If this is centered at someplace other than zero, you still get a valid estimate out of your S^2-hat of the variance, and what you don't get is any sense of what the bias is though, and that -- But that's not variance. That is bias, and so, even if this is off-center, even if the measurement error is off-center, you still are okay on variance.

Some of the measurement error though probably isn't additive,

and so, if it's multiplicative, for example, it won't be exactly this, but, actually, it will be not as bad as people think, and it won't be exactly right if the -- Let's say Y_i-hat is Y_i times some quantity, and that quantity has some randomness in it, then this won't actually be true, but it still will produce a variance estimate that is bigger than the one that I would have gotten had I been able to see the truth, because, luckily, I couldn't see the truth. I say luckily only in the sense that it captures some of the variability.

One exception to this that occurred to me when Mary was talking to the Alabama people is that the measurement error certainly isn't of this form in Alabama for the reefs, the artificial reefs, because, instead, where the measurement error comes in, or at least one of the places the measurement error comes in, is not here, but it's in the n_i, and so, in my mean per unit estimator -- In my mean per unit estimator here, this n_h, I will have to stick a hat on it, and now I do have -- So that's the form of the measurement error for that, I mean the variance for that, and it's not going to be like a sum of variance, and, instead, it's going to be a little more complicated than that, because that's a product of two random variables, but we can use -- As she suggested, we can use the delta method and get a variance for a product, and it's especially easy, because, as I understand it, the two estimates there, the two random variables, are independent of each other, and so that makes it, I think, kind of easy to do that, and that's something that I did not do, and it didn't occur to me until she said that, and so thank you, but I can do it.

The other question, and now maybe this is clear now to everyone, but I had made this slide before some of the later discussions, and so, yesterday, there was a lot of discussion about neglected autocorrelation in variance estimates from transects, and so here's the question. Is there neglected autocorrelation when estimates have been done the way that I suggested, and the answer is no, there isn't any.

That's because the randomness comes from the selection of the units and not from any modeling or anything like that, and the transects are independently selected. By definition, they are independent, and so, if you just look at the transect total to transect total variability, you don't have to get into any of that measurement error, and suppose that's not there in your case, and so here's just a little example.

Let's say this some stratum in the Gulf of Mexico, and we've got fish swimming around in there, and that piece of the stratum --

That stratum has 126 units in it, and so I don't know how many fish are there, and I've forgotten, but, anyway, what I will do is select some transects, and so there is my transects, and suppose I plan on a sample of size three, and so the way the transects were selected, as I understand it, was a random point was selected that belongs to a grid, which means that every grid has an equal chance of being selected, which makes it a simple random sample, and then you just run your transect off that grid, and you try to make them the same size, but maybe you didn't, and so I have drawn them here like they're not, just to show what happens.

I got five fish in one and zero in my other two. Now, clearly, there is -- From the way this word was used yesterday, there is autocorrelation in the transect. Well, there would be if I was treating each one of these little squares as a sample, but I'm not, and so that doesn't matter.

Anyway, this way this would work is, using the ratio estimator, it would be the total size and area, 126 units, times this density, which is simply the number of fish in every sampled unit added up, over the number of squares, or I guess they're rectangles, in every sample unit added up, and that gives me an estimate of 20.3 fish in this stratum.

Actually, I do remember, and I think I made eighteen fish in there, and so that wasn't too bad. However, when you calculate the standard error, it's pretty bad. It's 175 with my sample size of three, and it is because of this large variability from zero to five that's reflected in the standard error, but, anyway, the fact that the data are autocorrelated within transects is not relevant, in this case, and so that's one that you can quit worrying about.

Now, when would it matter? Well, if you split the transect in half, or into the individual units, then you would have to worry about it, because they weren't selected independently. Also, if the size of the transect was corelated with the number of fish in the transect, then the estimator itself would be biased, actually, and so that means, if you go out to your transect point and start running the transect, and somebody says, you know what, we're not seeing any fish, and let's quit, well, that would invalidate all that I just said, because you would -- The two would be correlated.

The size of the transect and the number of fish would be correlated, and so that wouldn't -- I mean, it's not really the correlation that matters, but it's the fact that it's not pre-

designed, that you include less units than you were supposed to have, on purpose, if the numbers or low, or high, whichever way you did it. If you let the number of fish influence the size of the transect, then that's a problem.

If there is a two-stage sample in which the transect grids are sub-sampled, then the standard error of the final estimate would be larger. Like, if I didn't look at all these grid points right here, but just every other one or something, then I would have that second source of variability, but, like I talked about earlier, and I'm boringly long, I guess, you can actually estimate -- If you just calculate the PSU-to-PSU variability, that actually captures that second-stage variability almost perfectly, if the finite population correction for the first-stage units is very small.

Comments on other effects on estimated variance, this is about pipelines, and I think there may be a few things that might be correctable about the pipelines, and I think I maybe learned something about the way the pipelines were sampled in the last two days that might make me think about changing the variance of this a little bit.

The thing that is discussed here is sort of a niggling issue that I noticed when it was too late to fix it on my analysis, but, as I understand it, the pipelines were selected with random starting points, and then they go in both directions a certain number of -- Not random starting points, but random selections along the pipeline, and then you spread out in both directions a certain prespecified length, unless you were within a half-length of the planned transect length of the end, in which case, obviously, you can't go all the way on one.

The way I read it, what they did was they went the regular transect length, but in the other direction. What that means is that the probability of selection for the places near the end of the pipelines are small, smaller, than the probability of selection anywhere else, and so there should be a different weighting, but, in the file that I got, I couldn't tell which, if any, were near the end of the pipeline.

I assume that there must have been some, or else there wouldn't have been that careful description of how it was done if it was near the end of the pipeline, but I couldn't identify which ones, and I am guessing that it couldn't have been very many, because these pipelines aren't very short, and so it would be unlikely that you would get a whole bunch of them near the end, and so there's only two ends to every pipe, right, and so it

would only be two, but, anyway, that's one issue that would affect it.

The fact that you don't weight by the reciprocal of the probability selection makes it a tiny bit biased, if that happens very often, and it also affects the variance, but I don't think this is a -- Unless someone corrects me, I don't think this is a big problem.

The other problem that isn't written down here that might be a problem, and maybe I need to hear from some of the people who actually did the sampling a little more carefully, so I understand it, is that, if the pipeline was chosen in a way that was paired with some other sampling from some other stratum, then that could induce a correlation, because that would make it sort of a cluster, and so that might cause a bit of a problem, and maybe I can ask a question when this is over with, to make sure that I understand how to account for that. I think that's all.

This, I also wrote before the nice discussion that -- Oh gosh, now I've forgotten who did it, but someone gave about why messing up your sample allocations, or sampling at a lower sampling rate in one area than another, or maybe a sampling rate that's so low that you don't get good estimates, and it doesn't bias anything, but it just makes it have a bigger variance, but I think we've covered that, and so that's it. I believe, with that, I think I'm done.

CHAIRMAN POWERS: Thank you. Are there questions of Dr. Stokes? Mary, Dr. Christman.

DR. CHRISTMAN: You knew that I was going to raise my hand, right? I would like to discuss, Dr. Stokes, Table 7.

DR. STOKES: Okay.

DR. CHRISTMAN: It's page 89. I think it's 89 or 90. You said that pyramids were actually sampled differently in Texas than was indicated in the text, and so I'm assuming that means that those number of structures there that you have up, the reason that they're hugely different than Table 6 is because of that? This has 12,800 structures, and the other table has 5,200.

DR. STOKES: What this is, it's an estimate of how many pyramids are there that are made by selecting some grid squares and counting the pyramids in those grid squares and inflating it the number of grid squares in the area, and so this really is an N-

hat that is based on just a mean per unit estimator.

DR. CHRISTMAN: So what is this the sample size for?

DR. STOKES: That is the sample size of grid squares.

DR. CHRISTMAN: Okay. That's not clear, because it's based on -- It's not based on structure or area.

DR. STOKES: Well, it's area, but it's grid squares. They're all the same area.

DR. CHRISTMAN: Right. I think it needs to be clarified in your table then, that the actual number of structures sampled is larger than four.

DR. STOKES: Yes, and the number of structures is -- I guess it's not in the table there.

DR. CHRISTMAN: The reason why I picked on poor Rob Ahrens so much about clusters was because both of you have sample sizes that indicated that you treated every observation, every fifteen-second video, as an independent simple random sample. Your sample sizes there should be the number of transects and not -- For uncharacterized bottom, I'm referring to now, and not the number of videos.

DR. STOKES: If that is correct -- I mean, I don't know. I actually didn't get any -- I just got a list of numbers that said these are the transects, and that's what came to me.

DR. CHRISTMAN: Right, but you know how many transects you used in your estimate, your ratio of means estimate, and so you could put that number there.

DR. STOKES: Right.

DR. CHRISTMAN: It would make me feel a lot better, because, otherwise, if you don't discuss transects in your description of the writeup, it's unclear that you used the actual full transects, which explains why your estimates of standard error tend to be slightly lower, because you did actually use the full data for the whole transect, which helps.

DR. STOKES: Yes.

DR. CHRISTMAN: The next question I have was the sum with the pipelines, and you have 15,618 as your sample size, which you

sure as heck didn't use, I hope.

DR. STOKES: Let's see. Let me look at my pipeline data.

DR. CHRISTMAN: It's just the bottom of Table 7, and that's all that I'm referring to.

DR. STOKES: Let me look at Table 7 again. This is the column for sample size?

DR. CHRISTMAN: Yes, the 15,618.

DR. STOKES: That's actually not sample size, and it's area.

DR. CHRISTMAN: No, it's number of individual fifteen-second videos, I believe.

DR. STOKES: I don't know how many fifteen-second videos -- I mean, I had a list of numbers, and I thought they were the observed numbers for that transect, because I did question that.

DR. CHRISTMAN: The other table indicates there were twenty-seven pipelines sampled.

DR. STOKES: What other table? Rob's table?

DR. CHRISTMAN: Yes. Table 6.

DR. STOKES: I actually never saw Rob's table until the thing was --

DR. CHRISTMAN: You captured some of the problems that I had, which was, namely, that, when data were collected in a cluster sample design, it should be treated as a cluster sample design.

DR. STOKES: Right. I mean, I am virtually certain we did that, and I believe that N represents the smaller units, but we treated them as the transects.

DR. CHRISTMAN: Yes, and you must have used the entire transect, because your standard error is much lower. I mean, it's one-fourth the standard error that Rob got, because Rob treated them as a simple random sample, and he did not use all 15,600 videos.

DR. STOKES: He should have got a smaller standard error if he used the --

DR. CHRISTMAN: No, because remember he only used a subset. He

only used every fortieth, and you used all.

DR. STOKES: So the sample was only one-fortieth as big, but --

DR. CHRISTMAN: Right. Exactly.

DR. STOKES: Okay. Got it.

DR. CHRISTMAN: So that’s the difference there. The other thing that I was a big confused about is, for the artificial reefs in Louisiana, could you cover that a little bit again, because Rob’s estimate was 6,700,000, and your estimate is almost eleven million, and you had said something about Louisiana being treated differently because there was a substitution.

DR. STOKES: We just basically had no information about artificial reefs in Louisiana, and so all we did was just take -- Greg and Tara identified for us artificial reefs in Texas that were similar, supposed to be similar, based on habitat, and we simply took the average of those and then multiplied it against how many artificial reefs in the various categories are believed to be -- What I was told to exist in Louisiana.

It's just a simple -- Thinking that we knew how many were in the frame for the three types of reefs in Louisiana times data that didn’t come from Louisiana. It was times means from a subset of Texas.

DR. CHRISTMAN: So then you didn’t use the average over all the depths, and you used the average within each depth zone.

DR. STOKES: Yes.

DR. CHRISTMAN: That’s why it’s a different value. Okay. The last thing was that I wasn’t referring to measurement error when I was referring to some of the issues that I had. I was referring to exactly what you mentioned towards the end, which is that you’re using the product of two random values, in at least two instances, and I cited two instances, the number of artificial reefs and when you have an estimate of species composition that is the average composition, and you also have a variance associated with that average from your sampling.

You used Leo Goodman’s 1960 paper, who lays out exactly how to estimate variance of products of independent -- Also, he discusses dependent products, and so it would be quite possible to do that, just to get a sense -- There are so many sources of uncertainty here, and my feeling is this is like drops in a

bucket.

That doesn't make big difference, but it's one drop in a bucket, and, at some point, the bucket fills up, and so my concern is that, where we can address variance, we should at least determine whether it has an impact or not.

DR. STOKES: Yes, that's a good idea.

DR. CHRISTMAN: I wanted to keep in mind that, while I agree with you that, yes, in and of itself, this one is not a big deal, and this one is not a big deal, in the aggregate, they could be serious. I totally agree with everything you said, otherwise.

DR. STOKES: Well, that's good.

DR. CHRISTMAN: Obviously, and it's statistics. I should agree with you, but I just wanted to point out that my concern is the constant drip, drip, drip that's adding up, and we can't even measure it. Where we can measure it, we ought to at least get a sense of the impact, and you using the clustering design aspect shows exactly the sort of effects that it has, and so that's --

DR. STOKES: My CV was quite a bit bigger, I think, than Rob's.

DR. CHRISTMAN: Well, in some cases, and, in other cases, it wasn't.

DR. STOKES: I mean the aggregate. The aggregate I think was a difference between --

DR. CHRISTMAN: The aggregate is bigger, then you didn't include Louisiana, and so -- They did the pseudo-replication of samples. Well, not pseudo, but the replication of samples and then treating them as though they were actual measurements. Thank you. I appreciate everything.

CHAIRMAN POWERS: Thank you. Are there other questions at this point? Dave Eggleston.

DR. EGGLESTON: Hi, Lynne. Thanks for a very clear presentation. I was the one that was kind of harping on some of the spatial autocorrelations, and I wasn't necessarily -- I realize the samples within the transect are not independent, and that wasn't so much the issue, but, when I look at these sort of density maps, these sort of bubble maps, and we've seen some in the presentations, and there's on Figure 18 for Texas for the

uncharacterized bottom, and you see these clusters of density, and so that's what I was thinking in terms of, and I thought that -- To me, it looks like there's definitely spatial coherence.

I mean, you see a lot of big density dots right outside of Corpus Christi, it looks like, and some of the other areas, and so I was just concerned about not capturing that spatial autocorrelation at the scale of that uncharacterized bottom strata, and I thought it was interesting to try to characterize that sort of spatial coherence and see how that potentially matched up with potential bottom features.

DR. STOKES: Well, all I can say is that this patchiness of the spatial distribution still doesn't affect -- As long as you pick them at random, and you don't alter the length of the transect, it still doesn't affect the variance, the variance estimate, and it doesn't make it biased in any way, and so I don't know what else to say about that, but, if you're talking about some different source of autocorrelation than that, you need to explain it to me better, because I don't get it.

DR. CHRISTMAN: Do you want me to jump in?

DR. EGGLESTON: Yes, please.

DR. CHRISTMAN: You're referring to the fact that, if you look at the spatial surface, if you look at the variation over space, there's clearly some sort of trend, like inshore is denser than offshore type of thing, or more dense up north and less dense as you move south.

DR. EGGLESTON: Right.

DR. CHRISTMAN: From a design-based perspective, it doesn't matter. What really matters is whether your random sample, or even a non-random sample, in the case of the UCB, actually grabs the variation sufficiently. As long as it does -- In their case, they did a pretty good spatial coverage, except for between the two bays there, that one area that didn't have much sampling.

DR. STOKES: But, you know, randomness -- Sometimes things that -- Randomness looks kind of non-random.

DR. CHRISTMAN: You don't want it to look --

DR. STOKES: That's right, and so, just because there's some

missing, that probably means it's really random, if it kind of looks odd to you, because, if it was too sort of spread out evenly, odds are it's not random.

CHAIRMAN POWERS: We need to kind of hone-in on this discussion, and I think we're kind of losing -- Dave, has your question been answered.

DR. EGGLESTON: Yes. I'm fine. Thanks.

CHAIRMAN POWERS: Okay. Luiz.

DR. BARBIERI: Thank you. I just wanted to echo what Dave Eggleston said, and thank you for this very, very clear and helpful presentation. If you could go to I guess the previous page of the report there, and I guess it's -- I am trying to get to that Table 6, and it's page 84 of the report, and I just wanted to understand, in terms of the sample sizes, based on what you just explained, if you could walk us through.

DR. STOKES: This isn't my table, and so I don't know.

DR. BARBIERI: I see. What I am trying to understand, just so you get an idea of my question, is, for example, for uncharacterized bottom, that big number there of samples, 6,435, this, I believe, came out of seven transects, I believe, or maybe more than that, but it was definitely not even close to 6,435, and so what is really the sampling unit here that we could and should be using for this sampling of uncharacterized bottom, and how do we get to that sample size there?

DR. STOKES: Well, again, that's not my sample size, and so I don't know.

DR. STUNZ: Mr. Chairman, I can answer that question, if you would like.

CHAIRMAN POWERS: Please do.

DR. STUNZ: Luiz, I think what you're asking about is why is it 6,400 there, and, if you go down to Table 7, and, sorry, but I don't remember what the difference is, but --

DR. CHRISTMAN: It's the same value in Table 7.

DR. STUNZ: It is the same value in Table 7?

DR. CHRISTMAN: It was 140 transects of UCB plus the C-BASS

transects, and I don't know how many that is.

DR. STUNZ: If you go down to Table 7, I can explain this, I think, and I can't remember what's in Table -- Lynne is 3,538, right, and the difference there is, in discussions early on with Lynne, and Lynne may not even remember this, because it was early in the process, but Steve Murawski and Jay, obviously, worked in Texas, and this discussion we had about multiple gear within the same strata came up, and, because Jay's overlapped Steve's, Lynne recommended, or maybe we discussed or we thought it was appropriate, only to use Jay's data for her analysis and not to complicate it with another gear in that strata, because that was sort of the only place that that occurred. That is why you see a sample difference there.

CHAIRMAN POWERS: Thank you.

DR. BARBIERI: Okay. That explains it, Greg. Thank you, and thank you, Lynne.

DR. STUNZ: There may be other things like that, because, obviously, as Lynne mentioned, it wasn't like we precluded them from talking, but these were independent estimates of their own, of us talking, and so they looked at things very differently, and so it's not like one was right or one was wrong or something, but it was just that's how they perceived that was the best way to approach this problem.

CHAIRMAN POWERS: Okay. John Hoenig.

DR. HOENIG: I just wanted to make sure that Dave Eggleston's question was answered to his satisfaction about spatial autocorrelation, and, basically, what I wanted to say is, if you have a map, and there are broad trends across that map, and you have spatial autocorrelation, if you were to take a transect and say this transect happens to be high abundance, what does that tell me about the next transect?

If the next transect is say a fixed distance away, then you would have autocorrelation, but, if it's selected randomly from all possible locations, then the fact that the I transect was high doesn't tell you anything about at all about the I-plus-one transect, whether that's going to be high or low, and so you basically, by randomization, you break up the spatial structure, and then you don't have to worry about it. I don't know if that helps you, Dave, or not, but that's kind of why the random sampling of transects makes the transects independent.

DR. EGGLESTON: Thanks, John. I got that, and I guess I was thinking more -- I was kind of excited about learning what the spatial scale of coherence is in some of those random samples, to then couple that with what the bottom features might be that are explaining those patterns.

DR. CHRISTMAN: That's certainly possible, yes.

CHAIRMAN POWERS: Excuse me. Several people are talking, and can we -- The way I left it, John Hoenig was talking. Are you finished, John?

DR. HOENIG: I was just saying it's very interesting to learn about the spatial structure, and you can do that by looking within a transect, about how things vary along the transect, but that's a separate issue from looking at a collection of transects selected randomly.

CHAIRMAN POWERS: Okay. Thank you. Dave, did you want to respond?

DR. EGGLESTON: No, other than great explanation, and I think the short version was it's a random sampling approach that has identified spatial structure in the data.

CHAIRMAN POWERS: All right. Thank you. All right. There is no other questions now, and Ryan had indicated that we were being directed to have a session of public comment. How one operates that, I will leave it to Ryan to lead us through it.

MR. RINDONE: We've given them all your cellphone number to text you when they're ready to talk.

CHAIRMAN POWERS: Perfect. I will answer it forthwith.

MR. RINDONE: Thank you all very much, and so instructions are being put on the screen right now for participants to be able to raise their hand, if they have something they would like to ask of the review panel here, and please feel free to do so, and we'll limit folks to about three minutes for asking questions or making any comments. Kellie Ralston.

PUBLIC COMMENT

MS. KELLIE RALSTON: I am Kellie Ralston, representing the American Sportfishing Association, and I wanted to thank the committee for allowing us a few minutes to address you all, and I know you've had a long couple of days, and more to come, and

so I certainly appreciate this opportunity.

The last two days have really been, I think, a great review of a truly groundbreaking study, and I don't think we've ever seen a fisheries assessment of this scale and scope that has had this kind of funding, and that could potentially serve as a template for other studies, like the one that's coming forth in the South Atlantic, as well as potential modification to the way NOAA Fisheries conducts all of its surveys in the future, and I know that Clay has made that comment at council meetings before.

Certainly I'm not a statistician, and I don't fully understand all of the details that you all are addressing to offer comments on, but I do kind of have some broad take-aways that I would appreciate you all considering.

One is good science is critical to good management in fisheries, and we certainly appreciate Senator Shelby recognizing the need for more funding and better data to be able to move forward with the Great Red Snapper Count. Our fisheries are worth billions of dollars and, therefore, worth significant money on the science side of things.

As far as this specific study, science is iterative, and we learn from what we do to inform and improve our future efforts, and, given the scale and scope of the Great Red Snapper Count, you would expect there to be lots of lessons learned and lots of opportunities to address other issues as you move forward, but that doesn't mean that it isn't good science, and that's why we have the best available science category and not just best science in the Magnuson-Stevens Act.

With that, I would encourage the committee to review this project with your scientific expertise, as you have been, but also kind of in the scope of this broader context, and, with that, I will close, and I appreciate the time. Thank you.

MR. RINDONE: Thank you, Kellie. Jeff Angers.

MR. JEFF ANGERS: Hi, everyone. My name is Jeff Angers, and I'm the President of the Center for Sportfishing Policy. I want to thank all of you for devoting these first two of four days that you all are devoting this week on this committee. I think Kellie's comments were really quite on point, and I want to join all the points that she made.

This really is an unprecedented look at this fishery, and it explains so much of what stakeholders have seen on the water,

and the recreational fishing community in the Gulf region has confidence in the Harte Research Institute and the leadership of the Great Red Snapper Count. It is impressive by any measure, and I know there will be a lot of deliberations.

I would encourage this committee to don't let the perfect be the enemy of the good. There are always questions with new science, and we want you to honestly evaluate, but we hope this committee will find that this massive project that's been undertaken by most of the brightest minds in marine sciences represents a tremendous step forward in our understanding of this stock and that it truly represents the best science that we have on the true range and population of red snapper in the Gulf of Mexico. Thank you very much.

MR. RINDONE: Thank you, Mr. Angers. Is there anyone else who would like to provide any comment? Michael Drexler.

MR. MICHAEL DREXLER: Thank you, Ryan and the Chair. My name is Michael Drexler, and I'm with the Ocean Conservancy. I too want to applaud everyone on this enormous effort, and I think the science being developed here is unprecedented, and it will undoubtedly give us a lot of insight into the red snapper stock assessment.

I think, given the issues raised here, we would urge the SSC to give this review sort of the weight of a full stock assessment, and we have a very well-defined stock assessment process to include information like this that fundamentally changes our understanding of the stocks, which is a research track assessment. The magnitude of the changes being considered, and the scope of the uncertainties discussed here, I think warrant that, and so we would encourage the SSC to move this into a SEDAR process assessment, so we can understand how this integrates with the stock.

On a technical note, I wanted to ask a question, or provide a comment. On the counting of artificial reefs in Alabama, I think it's probably not going to sway the results a huge amount, but, in talking to some -- In conversations, I understand that the materials of the artificial reefs in Alabama may degrade over time, and, looking at the dates of the surveys to count these artificial reefs, it dates back quite a while, and so I don't know how to quantify that or if it's a valid point, but it may be worth considering how some of these chicken coops or other materials may degrade over time, in the span of these surveys, which are then used to essentially extrapolate by the number of artificial reefs, and so thank you for the opportunity

to comment, and thank you to the SSC and researchers here for this great body of work.

MR. RINDONE: Thank you, Michael. Ted Venker.

MR. TED VENKER: My name is Ted Venker, and I'm the Conservation Director for the Coastal Conservation Association, and I really just wanted to echo earlier comments. I really appreciate the tremendous time and effort that Greg and his team have put into this, and also to everyone who has invested the time, the reviewers on this effort, and, really, it's a tremendous amount of work for you guys, and it's largely going to go unrecognized, but it's incredibly vital, and so thank you, all, for your time.

I am wildly unqualified to comment on the technicalities of what you all are discussing, but I did just want to bring it back to the theme that I remember from my grade school science classes, which was don't ever let the perfect be the enemy of the good, and so, if this is progressing to further our knowledge of the red snapper population, I would encourage, just as general advice from a layperson, to recognize that none of the minutia that we're discussing today, that has been brought up, is likely to change the fact that we're talking about orders of magnitude difference in what we thought was in the Gulf of Mexico with what Greg and his team are showing.

That was all that I would like to bring up, and I know that's probably very rudimentary, but I did want to share that it's an important study, and it's impressive to think of all the time and effort that everybody has put into it, and I just wanted to make sure that we're looking at it in the big picture, and so I appreciate it, Ryan, and thank you, all.

MR. RINDONE: Thank you, Ted. I think Dr. Hoenig has a comment for Michael Drexler, or a response to a comment.

DR. HOENIG: Thank you, Michael, for your thoughtful comments. You're absolutely right that, over time, chicken coops degrade, and also, over time, people throw more artificial reefs overboard, so that the inventory that we have of those cells that we surveyed gets out-of-date.

We can get at the rate of disintegration of chicken coops and things by looking at how many did we go to visit and found they were no longer there, and I don't have that information at my fingertips, but that's something that we can look for. The part about new ones coming in that we weren't aware of is harder, and the only way to get at that would be to revisit some cells and

re-map them, and it's not as high a priority for us as mapping additional cells.

All in all, it's very fortunate that Sean Powers, a few years ago, decided to start mapping those cells. Otherwise, we would have been without a sampling frame to choose from, and so I just wanted to say that you're absolutely right that this is more work, and it's just a matter of finding the funding to continue it.

MR. RINDONE: Ms. Bosarge.

MS. LEANN BOSARGE: Thank you, Ryan. I just wanted to commend everybody, Greg and all your group, and I guess -- I mean, I knew it was a huge undertaking, but I don't think I realized exactly how huge of an undertaking it was. It's amazing work, and I commend staff for setting this up logistically, so that everybody could collaborate at the same time and give comments and answer questions, and so just a kudos to everybody, everybody involved.

The only comment that I would make is that I'm super excited about some of the new technologies that were used, really innovative ways of getting at what's out there, and I hope, in the future -- I know we don't have a lot of time here, and so some of it's kind of high-level, but even like using the acoustics in the western Gulf for the uncharacterized bottom, and that's pretty innovative.

We received a high-level presentation on it that showed a few screenshots of what we call bottom machines in my world, but, anyway, and how you tried to take that and filter it, and I would love to see more info on that, maybe the quantitative side of it. How you take that and come out with that number of fish is really amazing, and so we would love to learn more about that in the future, way down the line, when you have the chance, and so thanks to everybody, and I can't wait to hear more.

MR. RINDONE: Thank you, Ms. Bosarge. Are there any other people out there that would like to make a comment? Seeing none, thank you, Dr. Powers.

CHAIRMAN POWERS: Thank you. I would make one general comment in response to several of the comments that were made in the public comment, and that is I think it's clear to all of us that this endeavor to estimate the abundance of the Gulf of Mexico -- It's going to be an integral part of the stock assessments for some years to come, and it provides an awful lot of opportunity

to estimate things that you don't normally think about, like perhaps even natural mortality and issues like that.

I don't think any of us are negative about the usefulness of this. Obviously, when we get talking about details, it sounds very critical, but, in the same respect, I think we will end up with a better product as we go forward, and I would hope that the public realizes that that's essentially our goal. Thank you.

Now, we have reached pretty much our time for this evening, and, without objection then, I would cut off the meeting, and we would return again tomorrow at 9:00 a.m. Eastern Daylight time. Thank you very much, and I think it's been a very productive day.

MR. GILL: Mr. Chairman, is it your intent to start the agenda tomorrow morning with Greg Stunz's portion of the agenda and the follow-on discussions that were intended for today's participation?

CHAIRMAN POWERS: Yes.

MR. GILL: Thank you.

CHAIRMAN POWERS: A large part of it will be details of the discussions like we've had today, and I would imagine, in some cases, some things will be relitigated, so to speak, as we go forward, but, toward the end, I will want some summing up, in terms of the consultants, and then also some direction, in terms of the SSC, how we might go.

Ryan had made the comment about individuals voting on things, and I'm not sure, in this section of the meeting, we really have to vote on anything, and it's sort of irrelevant when we do that, but, basically, we're going to have to decide, either at the beginning of the next meeting or the end of this one, how we want to proceed with this estimate, in terms of carrying it forward to catch advice. Ryan.

MR. RINDONE: Thank you, sir. According to the terms of reference, one of the responsibilities of the review panel, which includes the non-co-PI members of the SSC and the independent reviewers, is to determine whether the Great Red Snapper Count is a representative estimate of absolute abundance and that that estimate is reasonable, as is its variance, as it pertains to red snapper in the Gulf, and so that determination is something that is being asked of the review panel, to be made

in this first-half of the meeting, the part that's actually the peer review portion of the meeting.

Whether or not to use the data from the Great Red Snapper Count in an interim analysis and beyond that for informing management, it would happen under the SSC at the latter part of the meeting.

CHAIRMAN POWERS: Yes, I agree, but, when we typically go through this best available information kind of debate, we're usually looking at it as a whole, for a whole stock assessment. In this particular case, there may be certain things we want to use out of the estimate and certain things we don't want to use, and, obviously, I think we need to get some guidance from that from the consultants, and then also in terms of the SSC's own opinion about that, as we go forward. Thank you. With that, may we retire for the evening? Yes, we may, and so I will see you in the morning then.

(Whereupon, the meeting recessed on March 31, 2021.)

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April 1, 2021

THURSDAY MORNING SESSION

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The Meeting of the Gulf of Mexico Fishery Management Council Standing and Special Reef Fish and Socioeconomic Scientific and Statistical Committees reconvened via webinar on Thursday morning, April 1, 2021, and was called to order by Chairman Joe Powers.

CHAIRMAN POWERS: Good morning, everybody. This is Joe Powers, again, your illustrious Chairman, and we're about to embark on Round 3. Basically, this morning is we want to finish this stage of the meeting, the basic review.

On the agenda is a series of presentations that's being led by Dr. Stunz, Greg, and I would be giving the floor to him, and I will allow him to kind of decide when we would open up for questions, and, obviously, if anybody wants to interject, feel free to do so.

At the close of those three series of discussions, we'll have the overall discussions and further questions, if needed, from the SSC and the consultants, and then we'll have some -- We'll

be asking for some conclusions, at this stage of the game, from the consultants, and then the SSC has to decide how we want to proceed with this information, and so that's the game plan for this morning, and so, with that, I would welcome everybody.

For the Central Time Zone people, it's eight in the morning. For wherever else somebody may be, I'm not sure, and so let me give the floor then to Greg Stunz, and we'll proceed according to the agenda. Thank you.

REVIEW OF GREAT RED SNAPPER COUNT PROJECT (CONTINUED)
DISCUSSION, CONCLUSIONS, AND KEY TAKEAWAYS

DR. STUNZ: Thank you, Mr. Chairman. You should be looking at the same red snapper slide that we've been looking at for quite a while, and just to -- As the agenda goes together and things develop, I think I can -- Because, obviously, we want to get to the meat of the matter, which is the deliberations, and all of these sort of what have appeared as separate presentations I think I can do in a very short presentation this morning that kind of ties it all together.

Of course, we'll be here to answer questions and contribute, but I don't think we need another kind of death-by-PowerPoint kind of thing, and so I think I can just summarize a lot of this, in terms of where we are in our team and then turn it back over to the crux of the matter. With that, I will go ahead and get started, if it's okay, Mr. Chairman.

First, I just want to say thanks for the review panel, particularly the independent reviewers here, that allow us improve our work. Clearly, as a lot of the public is attending here, this is really how the peer review process works, albeit not quite as public, but we're used to that, and that's how we ensure the best science available, is going through this peer review process.

As I look at the screen, many times throughout this meeting, and there's 140 people involved, and, of course, the general public, as well as just all of us that are deep involved in the process, and we would never see that, probably, at a SEDAR workshop, where most of this would normally occur kind of just -- While it's open to the public, very little interaction like that occurs, and that's probably because we're dealing with such an iconic species with such a whole profile and management surrounding that.

Anyway, this process really gets at the core of what we all are

governed under, the broader scientific method and that process of expanding -- As we get new scientific information, we expand our knowledge base, always leaving opportunity to continue to expand and improve on that, and that's really where I think we're at.

I can't help but put my councilman hat on just a little bit during this whole process, to just remind folks why we're even really here and what is that reasoning, and that's really because of some very crucial, pressing management needs that are really driving the pace of this process.

Certainly our team is trying to be very responsive that, and, believe it or not, we actually volunteered for this process, as generally a courtesy, and, believe me, I'm smiling right now as I'm saying that, but, seriously though, we feel, as our team, that we have a professional obligation as scientists to facilitate management, in terms of understanding what this project really means and how it could best fit into this management structure that we work under.

Specifically, in this case, we're evaluating this for incorporation into this short-term interim assessment, which could curb some of these pressing management needs that I was referring to, of course ahead of a formal stock assessment and SEDAR process, but that's years down the line, and we don't have time for that for some of these needs.

I want to emphasize that all of this is occurring well ahead of our sponsor review date who sponsored this program. This isn't a council-driven process, and it's not a NOAA-driven process, and it's driven independently, and so that process is June 1, and so our team has worked -- We have really put our professional and, in many cases, private lives on hold, temporarily, to get this to the review team as fast as we could, because we knew the dire straits that some of the management is in.

With that, I think I'll really kind of start the real presentation component of that, but at least maybe that sets the stage some.

I want to start off that no study is perfect, not one that's ever been done, and certainly one of this magnitude that especially you can capture all the uncertainty and variance, and, in many cases, for a study like this, it's just not possible, obviously, to do that.

Clearly, there were a variety of opinions on how much of that variance was captured, and I think Dr. Stokes did an outstanding job yesterday of really summing that up, the real pro, her, and especially Mary Christman and others on the review team that can really help us understand that better.

Certainly, is that perfect? Well, absolutely not, and no study is without some level of uncertainty and bias, and I think everybody certainly recognizes that. In fact, ironically, one of the alternate interim analyses that we'll review later today, and not dealing with this study, and dealing with the bottom longline study, just has the whole same suite of uncertainty and bias issues, and so I would like folks to kind of keep that in mind as well, although we're -- Because this study is really unprecedented and relatively groundbreaking, we're, obviously receiving a high level of scrutiny, and the project should, and we should pay attention that, as the scientific team, and respond to that, which I think you're already hearing that that's happening, or you will hear that that's happening, based on a lot of the things that were brought up and small changes.

Many of those changes are easily, easily done, and it will greatly improve what we're doing here, and so I want to just talk about what we did here.

We assembled just the leadership team here, but the team in general, the best minds in the business. I mean, this is as good as it gets, to work on a project that many said just simply could not be done, and so their teams, as well as all the investigators, were able to pull off what we consider quite a monumental undertaking.

We held true to the RFP, and that's important to us. It may not be important to the process here, but it's important to meeting our sponsor's goals and what their objectives are, and so I feel that we met those objectives and goals clearly.

We held generally true to our design backbone. Certainly, we had to make modifications to that that were necessary to accommodate regional concerns, and we had to rely on years of expert sampling and experience in those regions by those investigators to adequately carry that out, and so we modified that, obviously, where necessary, and we've been spending a lot of time talking about that.

Really, to I guess pose the discussion terms, and, of course, this is spelled out in the report as well, towards the end, as well as key takeaways, and what are the big points here, and

that is, you know, what is the direction of the estimate, in terms of any types of biases, and it's conservative, and likely underestimates that abundance, for a variety of reasons that we've been talking about.

Principally, detectability leads to underestimation, whether you're talking about species ID counts, acoustics, et cetera. This mapping is, obviously, a challenge, and we knew that. We were well aware of that going in, but certainly future work can improve that, by improving the mapping, when you don't know your habitat types with certainty.

We, obviously, talked some about known populations occur outside of the defined study area, and future studies need to go assess that, and what does that look like, and what does that mean to management, but it also leads to some underestimate in our population, but not necessarily within the zones that we were talking about, but those that occurred outside of our specified depth ranges.

Then, obviously, this discussion we had, and Kai really led a lot of this, I think, in terms of the need for rigorous calibrations and what we could and couldn't do in other studies, and, given the diverse habitats and regional differences, that's almost going to require separate major studies of their own, to really identify what are the best ways to go about that, because I really feel, and I think our team does too, that it's going to be difficult to get around gear challenges, and, like we started out, there probably is never going to be a magic bullet that just does it across all the regions, all the areas, and all the habitats, given that variability.

What are the key takeaways from the team? Well, we feel strongly that it provides a valuable and useful estimate of red snapper, particularly for this interim assessment. We certainly have work that we could do to improve that, and we appreciate the team pointing that out, so we can address those, and, just like we would do in pretty much any study, we're happy to work through that.

It really shows that that is how the sausage is made in the science-making process, and, at the end of the day though, it expands our knowledge based on red snapper, and the perfect example is some of the challenges that we had in Louisiana. Dr. Gallaway is coming in right behind us with his Louisiana snapper count, which will further improve upon that region, which we would like to address a little bit better, but that's why we lead through the scientific method, to leave room for some

doubt, so we can come in behind that and improve what we've got.

Obviously, the large number of fish that occur over this uncharacterized bottom and that high abundance leads us to really warrant additional study in that area, and hindsight is 20/20, as many of us have discussed, and so we need a better understanding, not only of age structure on that habitat and how the fishery exploits that outside of this process, but we need to revisit all those relief anomalies that we found and better characterize what those are and what the distribution looks like.

We need to look at fine-scale movement, because of major management concerns, how much exchange as well as fishing effort is going to occur over that area, as well as just movement among artificial and natural-reef habitats, and then, of course, not only mapping of that uncharacterized bottom, but, of course, mapping in general.

Then I won't get into all of this now, but the report provides a lot of it, and some I've already talked about, and many others, in terms of, like any good study, we would have a lot more questions at the end than you probably started out with, or needs that would be addressed, and so I guess my portion I would just sort of end here.

Many of said, including some of the management leadership, that the absolute abundance is really a holy grail sort of piece that allows management to do all sorts of a variety of management options that may not be available, necessarily, with indices of abundance and that sort of thing. Where our team feels clear is we show that this study advances our knowledge base and will improve management for red snapper.

I want to open it up, and there are several of our team members here that may or may not want to say a few words, or follow-up on anything that I may have missed or that they would like to say, and so I would do that now, Dr. Powers, and then I would really -- I think, at that point, we can turn it back over to your team, after anyone else would choose to comment.

CHAIRMAN POWERS: Thank you. We will open it to the floor, or to the PIs that wish to make comments at this point, if any. We will begin with Will, Dr. Patterson.

DR. PATTERSON: I wanted to thank the review panel and the SSC for the amount of time and effort they've put into reviewing this work. It's clear that they took their charge seriously,

and we are -- We as investigators on this report and this project will benefit immensely from their insights, and, as the final draft of this gets submitted to Sea Grant and publications come from this, it will be greatly improved by their participation, and it's very unusual to have this many sets of eyes looking over things that you've done, and so I think it can only benefit the final product.

Obviously, this was a huge commitment of resources and taxpayer money, and we took our charge seriously, and so I'm happy to see the amount of review that went into this, has gone into it at this point, and I'm glad to have the opportunity, before the SSC members who were part of the team need to remain quiet here at the end. I'm just really appreciative for that level of review.

The second thing that I wanted to say has to do with this idea of uncertainty, and I wanted to have a chance to say it at the beginning here, before, again, this sort of silent period for to answer questions for the SSC members, and there have been components of what's been discussed here that have to do with precision and other components that have to do with bias.

I would just ask the SSC members, when they are considering these issues of precision and bias, not to lump them sort of generically into a category of uncertainty, which we sometimes do, but, instead, if we're talking about uncertainty, if something seems uncertain, then we talk about it as bias or precision issues, and so it's easier for us to track and try to capture the sentiment of what's being expressed.

The last thing is, if I was sitting in the room, and not a member of the report, and just as a member of the SSC that was reviewing this and potentially commenting on things today, my reaction might be that management is management, and this is science, and so, obviously, this isn't going to be reviewed in a vacuum.

There are serious management implications in the near-term and the long-term, and I think the most important thing, as a scientist, is that we get it right, and as right as possible, and so I would ignore, as much as you can, any pressure or potential to think about what the management implications of this might be, but instead focus on the science, review the science, and that should be your main focus here. Thanks.

CHAIRMAN POWERS: Thank you. Steve Murawski.

DR. MURAWSKI: Thanks, Dr. Powers. I appreciate the opportunity

to make a few short comments. I really want to support the comments of both Greg and Will. This is a very complex set of analyses, and each individually could be the subject of a peer review, but what we've done is tried to combine, basically, a bunch of mini-surveys into a larger population assessment, and, therefore, it's highly complex, and each of these is subject to sort of different considerations.

Will made the point about the notion of bias versus sampling uncertainty, and I would sort of expand that to say, look, we have this whole issue of sampling uncertainty, which is the underlying variability in the data we collect, but we have an enormous issue of process uncertainty, and I think therein lies, I think, the challenge of integrating any set of indices, and I would say this is an index of population abundance.

One of the things that came out clear to me, in doing this work and contributing to the analyses, is that there's some really complex things going on in the series of analyses that constitute this report, and I would not discount the importance of the tagging data. The tagging data show a very high exploitation rate of red snapper in the fishable population, the population that is close to population centers and fishing on structures where the animals are abundant.

Even those estimates are inconsistent with the current stock assessment, which has an exploitation rate substantially below 30 percent for the population as a whole, and I think one of the things that I think this study points out is that there's a well of cryptic biomass that is not part of what we call the fished population, right, and it may be larger than we thought, but, nevertheless, of the fishable population, there is an issue here.

I think it kind of begs a larger issue, and that is, looking at the population assessment model for red snapper, you all might want to contemplate some type of meta-population analysis that incorporates spatially-explicit fishing, and so that's a little down the road, but I think the analyses that this study have provided give you at least part of the picture that you can start to contribute to this larger assessment model.

My point of view, the data that were collected as part of the uncharacterized or unclassified or mud bottoms or whatever, it's interesting, because that area is subject to a lot of the current relative population indices that go into the current assessment, right, the non-full-weighted population estimates coming from fishery-independent surveys.

For example, the groundfish survey goes through all of those areas, and it's really impressive how few of the large adult red snapper are actually caught in those surveys versus what we were able to actually put our eyeballs on, and so, clearly, the Q for those large fish is very low for the groundfish survey, and so it kind of begs the issue of, in the longer term, what is going to be the survey capability that would index that subset of the population, and so that's something for the agency to deal with, but, overall, I agree with Greg.

Many of these things revealed in this particular study are a starting point, and this is not an endpoint, and we all appreciate the dialogue, with not only the independent consultants, but also the SSC.

Being a veteran of many contentious population reviews for stock assessment, I can say that, actually, this has been quite collegial, and none of us take offense about the back-and-forth of this enterprise, this scientific process, and so all I can say is it's all much appreciated, and all the comments that you have are feeding back into the science programs that all of us on the academic side are conducting, and so, with that, thank you very much, and I will turn it back.

CHAIRMAN POWERS: Thank you, Steve. Sean Powers.

DR. POWERS: Thanks, Joe. I just, again, want to echo the thanks to the reviewers and to the SSC members. It is a high level of scrutiny that the study receives, and it should receive. As Will pointed out, it's a huge investment of taxpayer money, and we really appreciate all the comments, and there is no doubt that our report and estimates will be improved by incorporating and exploring many of the suggestions that the review team has given us.

To one of Steve's points, I agree that the task of combining a bunch of regional or mini-studies is not easy, but I do want to point out that that is particularly true for the artificial reefs, and, to some degree, the natural reefs, where investigators chose very different approaches, in many areas.

Obviously, we used depletion and the ROV surveys, and others used hydroacoustics and ROV surveys and a variety of methods, but realize that where most of the number of snappers is coming from is on that uncharacterized bottom, and that is largely the same methodology.

Steve did most of that, or it had similar towed sleds, and so I agree that the complexity of putting some of those mini-studies together is difficult, but realize that's largely for the artificial reef component, which is a small fraction of our total estimate.

The issue of variance came up, and I agree with the comments of separating bias and precision, but there is variance in our calculations, and that's totally understandable, but the population, from site-to-site, is very variable, but realize that most of our data and assessments that we deal with greatly underestimate that variance.

I think we are coming closer and closer to actually truly capturing those variances, and the stock assessments often cap the CVs, because the models don't tend to converge unless you do that, and they do that for the catch data as well as some of the survey data, and so keep that in mind when you consider the variance that we have come up with, and I think it's much improved. It's not perfect, but it is getting closer to actually capturing a real variance that we can use.

Finally, in a perfect world, we would be able to proceed with a revision of this project and report as we have, and then that would be included and reconciled, to some degree, with the stock assessment, and clearly we can do that in the research track assessment.

However, there is the reality that, because the public knows the results, and we share information much more freely and timely than any other time, that I do have sympathy for the managers, who will have to manage the stock with the public having the knowledge of this study, and I think, obviously, that's why we have the further analysis that the council has asked for. I think, while science should proceed, as science does proceed, realize that the management body also has a responsibility. That's it, Joe.

CHAIRMAN POWERS: Thank you. I think you were speaking as much to the public about the management responsibility, because I'm sure the SSC is well aware of our role in this, and one thing that I would mention as well, as we go forward, kind of tying in with what Will Patterson said, is this -- He discussed it in terms of precision versus bias and those sort of things.

I too believe that we have to kind of parse our advice, and not necessarily have an overall yea or nay, but kind of what are the things we should go forward with now with these estimates and

then what are the things that we should allow to kind of evolve over the next six months or year or so on. One other question that I have is Greg Stunz mentioned that the final report for your contract, I guess, would be June 1, and is that true?

DR. STUNZ: That's correct, Joe.

CHAIRMAN POWERS: Presumably, in that process, you will be using some of our advice, collectively our advice, to improve upon those estimates, and I guess, just in terms of my own indication, to me, that would mean kind of revisiting some of the strata-by-strata sorts of issues that came up at this meeting.

DR. STUNZ: Joe, that's actually something -- I mean, I guess we sort of anticipated it, but not really, and, in a normal process, we would have never had this opportunity, and, especially for a project of this magnitude, it is great that we have an ability to do that after this.

As one of our team mentioned, we have so many eyes looking at this, and we really have a very good advantage, I guess if you want to call it that, to really refine what is needed sort of thing, outside of this interim process that we're in, to be available for when we get down to the research track assessment.

Dr. Powers, also, and I don't know -- Dr. Stokes, Lynne Stokes, is trying to raise her hand, and I don't see her on there, and she just texted me that she's -- If we could please recognize her.

CHAIRMAN POWERS: Yes. Go ahead, Dr. Stokes.

DR. STOKES: I actually have a document that I could share, and I don't know if it would be helpful, and I will just talk about it until I can figure out if there's a way for me to share it, but I took the -- This relates to the changes that we can make on the variance estimation.

MR. RINDONE: Dr. Stokes, I am sorry to interrupt. If you have a document that you would like to share, and you want to send it to us, we can get it put up on the screen, by making you the presenter, but we would also need a copy of it, just for the administrative record. Sorry for the interruption.

DR. STOKES: That's okay. Does somebody else want to -- Maybe, if there's someone else to speak now, I can do it that way. Maybe I will just talk, because I think that's going to take too

long for me to try to share it, but, anyway, I tried to figure out -- Yesterday, there were some really good suggestions about how to incorporate the uncertainty of some of the estimators into the variance expression.

One of the suggestions from Mary that would work for Alabama and Mississippi is to use the variance, a standard method that is the variance of a product of random variables, instead of just ignoring the variability in the number of reefs that were estimated, and so I actually did that.

I worked out the formula last night, and so that shows it's not very hard, because I could do it quickly, but I didn't have the data to estimate it. However, the calculation shows that the order of magnitude of the error is smaller than the variance itself.

In other words, the change in the variance will be a lower order of magnitude than the variance, and so I don't believe that this is going to be a big adjustment, at least in Alabama and Mississippi, and that's the one I did. I presume the one in Texas -- I haven't worked that out yet, and it depends on the structure of the estimated characteristic. Of course, in Alabama, it was the N, the number of reefs, and so it's very clear how that enters into the estimation of the total.

I don't know enough about the way the estimation of -- I forget what the word was, but the array of the -- Of the percent of red snapper or something like that in the videos, and I don't know exactly how that entered into the calculations, and so I would have to see the structure of the estimate that was made, but my assessment is that it would be also a lower order of magnitude.

The reason for that is that the estimator -- The variance of the thing that you observe is quite large, because it's multiplied by the number of, in the case of Alabama, artificial reefs, but the variance of that estimator is made from a sample of many observations, and so that's where the reduction in the order of error for the change in the variance comes from. I have formulas that I did that I can send if anyone is interested, and so I will do that after I sign-off. Thanks.

CHAIRMAN POWERS: Thank you. All right.

DR. HOENIG: Can I respond to that for a sec?

CHAIRMAN POWERS: You may respond.

DR. HOENIG: In Mississippi and Alabama, we did use the exact formula for the variance of a product, and that's Goodman's formula from I think 1960, to get the variance of the amount of reefs, and that -- So, for each stratum, when we report the number of reefs, the variance of that does include that formula. When we combined across strata, we had to weight the strata by the abundance of reefs, the number of reefs, and, there, I believe we treated the fraction that was in each stratum as a known constant, rather than putting in the variance. We can modify that and put in that variance.

CONSULTANT/SSC DELIBERATIONS

CHAIRMAN POWERS: Okay. Thank you. All right. Where we are, we're kind of at the point here. One of the things that strikes me is that there's lots of debate about details, but the real crux of how this gets translated into scientific advice for catches for 2021 is essentially how to handle the uncharacterized bottom.

My questions, as we go forward, is we've -- Much of the conclusions that I have felt that we've come to, in terms of the estimation of the variance of the estimates for the unclassified bottom are probably underestimated, but I haven't heard any discussion about whether the estimates of those are biased low or high for that strata, the uncharacterized bottom.

I think we need to think of it in terms of that. A lot of the other issues about Alabama and Mississippi are important in terms of, quote, unquote, getting it right as we go forward, but, in the larger scheme of things, in terms of what we might do today and tomorrow, in terms of the catch advice, they are relatively small.

I think it would be -- As we go forward, we need to think of it in those sorts of terms. Steve Cadrin wants the floor, and I think we're also at the point where we will be asking for the consultants to kind of sum up, and I'm not sure if they're prepared for that yet, but let me recognize Steve Cadrin, and then we'll go forward.

DR. CADRIN: Thank you, Chair. That's exactly along the lines, is the wrap-up, and I think I agree with your main points, as far as going forward, and so I do have a quick summary of my review, if you think now is the time to do that.

Again, compliments to the investigators, and thanks to our Chair, Dr. Powers, for herding more than a hundred cats for what

I think has been a collegial, productive meeting, but I also recognize that the most challenging decisions are yet to come.

My draft review is online and available, and that addresses each term of reference, and it gets into a lot of the details that we've been talking about, but, for the sake of developing consensus conclusions, I think there are three main aspects of my review for the group's consideration.

The first is the underestimation of abundance, from assuming 100 percent detection, and I have remaining concerns about the underestimation of variance, and the third main point, I think, is the application in an integrated assessment. Chair, I can quickly summarize each of those together or separately, depending on how you want the discussion to go.

CHAIRMAN POWERS: I think, at this stage, if people are prepared, then I will go consultant-reviewer-by-consultant-reviewer, starting with you, and you can proceed however you want to do that, and so those three items are fine with me. One other question though is you referred to the review being online, and have you modified it since the primary review before the meeting?

DR. CADRIN: I have not.

CHAIRMAN POWERS: Okay. Thank you.

DR. CADRIN: I will take a step back and, once again, give credit to the investigators. I really appreciate the logistics and the problem-solving that were needed for such a large-scale field study, applying advanced technologies to derive an absolute estimate of stock size, literally how many fish are in the ocean.

I am really glad to hear about continuing studies to address some of the concerns that we've discussed this week, and I really don't think the full value of this study will be determined by this peer review this week. I suspect, years from now, this will be viewed as a major contribution to fisheries science.

Toward the conclusions and the recommendations for this week, the main points, I think, as we've discussed in Phase 1 and Phase 2, the stratified design is well justified, and I understand the need for these posterior analyses that deviate from the original design. I understand that they were necessary, because of the challenges faced in implementing, but

we also need to recognize that they may impose some bias, and I think underestimate the variance.

The first major concern I have is assuming 100 percent detectability. The paired observations that we have seen from the experiments off of Florida show that the -- When they compare the optic and acoustic sampling, that optics detected more red snapper, and the initial estimate was nine-times more, and I think, if they changed the spatial scope, that comes down to four-times more, but it shows that the two major sampling methodologies have different detectability, and assuming 100 percent detectability, for one of the gears that has lower detectability, is going to underestimate the abundance.

That has different effects on different strata in different regions. As I understand it, the eastern estimates were more based on optics, and the western estimates were more based on acoustics, and so, when we go forward to apply these, and we use the Gulf-wide estimate of abundance, I don’t think that each of those regional estimates are additive, because I think they have different biases.

Fortunately, the SEDAR 52 assessment does have some broad-scale spatial structure, so that we could use the eastern estimates separately than the western estimates, and so, again, I’m a bit concerned about the detectability.

While I’m on a roll, I will just go forward with my other two major points. The next is the estimate of variance, and I will give full disclosure, first of all, that I’m not a statistician, but, second of all, that my first impression of reading the report was that an 11 percent CV for the estimate of an absolute abundance, across such a large area, a heterogenous area, with all the challenges we’ve talked about, just doesn’t seem realistic.

From the perspective of an integrated stock assessment, where the CV is really important to determine the statistical weight of the information relative to the fishery catch series, size and age composition, other indices -- From my experience, even the best, well-designed and implemented small-scale surveys don’t have such a low CV, and so I realize that that’s my implicit bias in reviewing this, is that I’m having trouble buying 11 percent, just on face value.

In my review, I was looking for reasons why that might be so low, why the variance might be underestimated. For the stratified random calculations, I am concerned about the zero

strata, those seventeen and fifty-four strata that had a mean density of zero and a variance of zero. They don't contribute to the stock-wide abundance estimate, but they have considerable influence on the stratified variance, and they bring it down.

The strata are considered to be red snapper habitat, and so the true density is greater than zero. The true variance is greater than zero, and so those mean equals zero and variance equals zero stratum results are really resulting from densities that are below detection limits, rather than it being true zeroes, and so I am concerned that those are biasing the stratified variance low, because of the detection limits of the sampling gear.

For the ratio estimator, I understand Dr. Stokes' explanation of how observation error actually increases the variance estimates, but, as I understand them, those proofs assume that the measurement error is white noise, and a mean of zero, and I'm really concerned that several measurement errors were not white noise.

One example would be assuming 100 percent detection when that implicitly assumes that there is no red snapper in the acoustic dead zone, and so any red snapper in that zone, and presumably a variable number of red snapper in that zone, make the estimate of variance less than the true variance, and I think there are several other important aspects of the estimation that are assumed to deterministic that are not white noise, that constant detection efficiency, the area selected, the imputed density for unsampled strata, species composition, age at length, acoustic signal processing, number of artificial reefs. There are a number of deterministic decisions that were made that I am concerned are contributing to an underestimate of the variance.

Then, finally, and I've already touched on it, is considering the results in an integrated assessment. From my perspective, with the information that I have reviewed, I don't think the estimates of stock-wide abundance can be used directly for stock assessment, because of these difference in detection between optic and acoustic.

I really think the most appropriate integration would be to consider abundance in the eastern regions, hopefully with a more credible variance, and I think a larger CV would give it more appropriate weight in the model, and then, perhaps, in the western regions, considering those abundance estimates to be lower biomass constraints. Chair, those were the remaining three issues that I have, and I would be happy to discuss them

at whatever length you would like.

CHAIRMAN POWERS: Thank you. I think the way we'll proceed is ask the SSC if they have any comments or questions relative to your comments, and then, after that, then we'll proceed to the next consultant. Mary, I would put you up next, but, first, let me sort of paraphrase a little about what you said, so that other people understand.

In particular, what you're saying is -- You made the comment that you shouldn't use the absolute estimate, and, basically, what you're saying is the Gulf-wide absolute estimate, in a stock assessment. Correct me if I'm wrong.

DR. CADRIN: That's correct. It's the stock-wide.

CHAIRMAN POWERS: It needs to be parsed up into a framework that more fits what is known about the stock, and it will involve spatial sorts of strata, and my guess is that the whole east/west may be revisited as well, as you go forward, and how best to spatially stratify it, given this set of information, and so I just wanted to make that clear. You're not denigrating the estimation process, but rather using it as an aggregate.

DR. CADRIN: For clarification, that's absolutely correct, and, in my report -- Thanks to the Southeast Fisheries Science Center for sending me the Stock Synthesis results, but there's a difference in perception of the east/west balance of the stock, where, in the Great Red Snapper Count, they're relatively evenly distributed. In the Stock Synthesis model, there's a big difference between them, and I think we've got to try to reconcile that difference.

I guess it could be because there is different natural mortality scaling for the different regions, and there might be different catch bias or other things, but we really need to wonder why the assessment is giving us a much bigger biomass in the west than in the east, where the Great Red Snapper Count is giving us more balance. I suspect that's because of the underestimation of abundance from acoustic methods that are primarily affecting the west.

CHAIRMAN POWERS: Thank you. John Hoenig, did you have a question or a comment to Steve?

DR. HOENIG: I don't really disagree with what Steve said, other than a perspective on the conclusion. You can poke all kinds of holes in this project results, talking about bias and

variability, but I think it's kind of important to try to keep a perspective as to how are other survey results treated in stock assessment forums, and so the standard by which Steve seems to be holding us is that it's not perfect and it can't be used, but, if you look around the world, at hydroacoustic surveys and so on, where they are getting biomass and treating it as absolutely abundance, I think then you kind of have to think, well, wait a second, is this somehow inferior to what's being used around the world, and I think it's not.

There is no mandate that you have a level playing field and you compare our results to results all around the world, but I think that, in terms of concluding whether this is useful as an estimate of absolute abundance, I think that it actually is credible and is comparable to what is used for abundance estimates elsewhere, and that was my comment.

CHAIRMAN POWERS: Again, I believe these are the kinds of things that would be debated within the stock assessment format, about how best to integrate these sorts of information. The whole idea of the assessment models is to take the data as they are collected and use that within the model, and so, in my mind, the model will be restructured because of these sets of data, and, this whole debate about whether you can estimate absolute abundance, those are things that you might be able to test through the modeling, as we go forward.

DR. CADRIN: Maybe I should check if my audio isn't working or if John's isn't working, but I did not say that these results are not perfect and so they cannot be used, and I don't know where John got that from what I said or what's in my report, but that is almost opposite of what I'm saying.

What I said was I would be -- Rather than using the stock-wide abundance estimates, I would use them by area in the assessment, and I would try to account for the different detection of the regional estimates separately. Some we might be able to use as estimates, and some might have constraints, but just to clarify, and I think it's important, I am not recommending that these results cannot be used in an assessment, and I'm not really sure how that came through.

CHAIRMAN POWERS: Thank you. Kai Lorenzen.

DR. LORENZEN: I was going to say the same thing, actually, and I think what Dr. Cadrin is saying is that the estimates in the western Gulf and the eastern Gulf may not be entirely comparable, and that has to be taken into account. That doesn't

mean that -- So they shouldn't be used as one single estimate in an overall assessment, but that doesn't mean that they can't be used in some way.

CHAIRMAN POWERS: Thank you. Any other questions for Dr. Cadrin? All right. Dr. Christman.

DR. CHRISTMAN: Good morning, everybody. I want to echo Steve's comments. Part of the problem with my review, to-date -- Let me get back up. I want to echo Steve's comments, and, also, say that I am just so impressed with the amount of work that's been done, and I'm not the least bit surprised that we're sitting here arguing over it, which is good. I mean, that's actually a good thing.

My issues, of course, relate to the statistical analyses, and I have some details. For example, maybe someone can clarify for me, but I feel as though there was some post-stratification, such as taking the pipeline data and converting it into three sub-strata, and the same with the Florida regions, that were done post hoc, based on the belief that fish abundance varied by pipeline size or region off of Florida, and, if that's the case, I would be inclined to not do that sub-stratification, because the sampling design did not use that stratification, and, as a consequence, you are artificially reducing variance.

I am not sure if there is somebody here who can explain to me whether those were planned, although not part of the original design, or whether they sort of occurred after the fact. Can anybody address that?

DR. MURAWSKI: One of the things that we found was that, in actually doing the work, the different sizes of pipelines became more apparent to us, and so the original scheme did not stratify by pipeline size, but, since there did seem to be some differences, that was another factor in that, and so we did consider that in the analysis.

DR. CHRISTMAN: Steve, are you saying that, once you got out there, you noticed that more fish were on the bigger pipelines?

DR. MURAWSKI: No, not necessarily, but that there was this obvious difference in the diameters of the pipelines.

DR. CHRISTMAN: Right, but the reason for stratification would be that you believe that the counts varied by pipeline size as well.

DR. MURAWSKI: I think we did some analysis to show that that was true.

DR. CHRISTMAN: See. There's the problem with using that post hoc then.

DR. MURAWSKI: Right.

DR. CHRISTMAN: So that sort of overstratification would lead to an underestimate of variance, for sure, and also possibly bias in the estimates, because now you are basing the strata on the value, the observations, and not directly. I mean, you're not sitting there saying, oh, look at this abundance, and I'm going to put it in this category, but you're using a surrogate for that, which concerns me a bit, and I couldn't tell you how it affects the results without knowing more details about the data.

DR. MURAWSKI: Neither could I, but I understand the point.

DR. CHRISTMAN: I also wanted to point out that, at the end of the day, two sets of analyses were performed on the same data, and not exactly the same data, because, as I understand it, Dr. Stokes removed the C-BASS from the Texas UCB, and the change in the artificial reefs in Texas also, and so, as a result, you would expect that there would be some differences in the estimates, but my problem is that these are not independent studies that would produce weight of evidence.

In the case of Ahrens' analysis, he did not follow the sampling design for the transects, and, in the case of Dr. Stokes, she did follow it, but not completely, for at least the Texas UCB hydroacoustics analysis, and so the one that would be the more appropriate result would be Dr. Stokes at this point, mainly because of the determination that there were cluster samples and they had to be addressed, and so that's just a minor point.

The other point, and this relates more to dissemination of the information, but the table that's at the beginning of the document, and I believe it's Table 1, which shows the estimates for the different main strata, habitat by region, I believe -- As we know, this table is already being used out there by different entities, because this document is available.

What I would recommend is that this table be amended to include the confidence interval endpoints for each of these estimates, because, otherwise, people believe that, yes, there is actually six million fish out there, as opposed to saying, no, it's somewhere between four and eight million, and so, at a minimum,

I would recommend that the variance be acknowledged anytime you're disseminating this information.

I agree that the variance is underestimated. I don't want to get into the gory details here, but there are still some small issues that I will address in my report that you will have in a few days that are just -- They are statistical issues that I don't think we need to address now, because Dr. Cadrin very clearly characterized the fact that we do underestimate variance and that that needs to be addressed, and so I would certainly do that.

On a separate note, part of the problem that I had with this report was just total confusion as to what was actually done, because so much information was missing that was provided during these two days, and so I would highly recommend that the report be modified to, one, address errors that are in the document.

If you look at my review, I put a table in there that showed how different the sample sizes were depending on where you looked, and so, at a minimum, I think there needs to be clarity as to what was actually done, and that's not very clear in the document, but, overall -- I know it sounds like I'm nitpicking, but, overall, this is an extraordinary project, and I'm very impressed with it. I just wish we could have more sources of variance included, and so I will shut up and let Rob explain why his is good.

CHAIRMAN POWERS: Rob, or Dr. Ahrens.

DR. AHRENS: Thank you, Chair, and thank you, council and the review team. I just wanted to provide some input, for clarification for Mary, and the stratification of the C-BASS data was done prior, or at least, for me, done prior to looking at the data, and so it was a priori.

I assumed that, given the size of the pipelines, there would be differences in the abundance, just given the size and then the physical structure that they would create, but that's certainly -- The removal of that stratification could be done, although it might be worth noting that the one pipeline, the big pipeline, that runs across Florida is probably quite different from the ones that exist in the other regions of the Gulf.

DR. CHRISTMAN: Rob, I just want to confirm. You're saying that you did not data dredge when you were choosing the pipeline sizes?

DR. AHRENS: No, I did not data dredge. Now, the -- After seeing particularly the zeroes that existed in the southern Florida, I made the decision to put in the line at Tampa Bay and to add the extra strata to Florida, and that, of course, could be removed, and that would be an easy change to the analysis there.

DR. CHRISTMAN: I think it should. Speaking of zeroes, one of the points that Steve Cadrin made was that it was problematic that you had strata that were zero -- That all the observations were zero, and, hence, the variance was zero.

One of the points I wanted to make is, if you look at those strata, for the sample sizes that you had for those strata, they're often extremely small, and, when you have really rare events, like one out of a thousand locations would actually have red snapper, those sample sizes are never going to capture that, and I am not sure what my recommendation would be for addressing that, except maybe collapsing strata, but I'm not sure that would be appropriate, and I would need to think about that some, but I just wanted to mention that one of the problems with those zero strata is they are often small sample sizes, which compounds the problem.

DR. AHRENS: Yes, and I agree with you 100 percent on that.

DR. CHRISTMAN: I didn't mean to interrupt you.

DR. AHRENS: No, I'm done.

CHAIRMAN POWERS: Okay. Thank you. Jack Isaacs.

DR. ISAACS: Good morning, everybody. I really wanted to thank Mary for bringing up something about this table that I found a little disturbing, or maybe disturbing is overdoing it. Unsettling maybe, and that is the lack of confidence intervals.

This table would suggest a level of precision that is absent really in any type of science, and I think it would benefit greatly from exactly the things that she talked about of the sample size and some estimate of a variance and inclusion of that to include the confidence intervals. This is an amazing project, and I have greatly enjoyed reading about it. Thank you.

CHAIRMAN POWERS: Thank you. Mary, have you finished your --

DR. CHRISTMAN: Yes, I have.

CHAIRMAN POWERS: Okay. Thank you. I have a question, or one of interpretation, and, the way I am interpreting this, it's that we are estimating, on the unclassified bottom, a substantial abundance, substantially larger than what we thought, and so my question is -- I mean, right now, that estimate, the 110 million versus forty-one million, three-times whatever that was, and so my question is, is there some reason that, from a -- As we go forward in terms of interpreting this information, how -- Can we say that the unclassified bottom, the point estimate, is a reasonable point estimate?

We recognize all the issues that we've brought up, and there are certainly issues about the variance, but I'm talking about the point estimate, and one of the things that struck me was some of the concerns about bias, in terms of a point estimate, I believe, and correct me if I'm wrong, may be alleviated by using the ratio estimate. From an interpretation standpoint, the ratio estimate isn't all that much different than the other, and so I would like some comment on that, if you've thought of it in that context, Mary, or anybody.

DR. CHRISTMAN: Well, I just wanted to say that the ratio estimator should give you similar results to the -- In terms of the point estimate itself for abundance, and I think our issue here is that the uncharacterized bottom represents so much of the area of the study, and that is why these estimates are so high.

That reminds me of a comment that I did want to make that I forgot to make, which was one of things is, when you compare the 110 million to what the last stock assessment came out with, which was like thirty-six million or something, are they the same universe?

In other words, did this study actually sample over the same region that the stock assessment data inputs were being collected? For example, I am just familiar with like the Florida stuff, and so that would be mostly natural, I believe, and I don't think they did it -- At least not the last time that I was involved, that they did an artificial reefs or uncharacterized bottom, except coincidentally, and so I would be a little careful when doing those kind of comparisons.

Back to the point estimates, I don't expect that the point estimates would be very different. The variances are very different though, largely because of the way the data were collected for uncharacterized bottom in at least some of these

locations.

CHAIRMAN POWERS: Thank you. Some of the issues about where the assessment data are collected, versus where -- Typically where the assessment data are collected versus where the survey data were collected, that's discussed in a bit more detail in the NMFS documents that we're going over this afternoon and tomorrow, I think, but, basically, most of the information that goes traditionally into the assessment comes from fisheries-related sorts of information, which is focused more on the natural bottom and artificial reefs. Mary, did you want to comment further?

DR. CHRISTMAN: No, I did not.

CHAIRMAN POWERS: Kai Lorenzen.

DR. LORENZEN: Dr. Powers, you invited general comment, and, from what I understand so far, it would seem, to me, that the point estimate is probably -- For the uncharacterized bottom, it's probably unbiased, or slightly biased low, and the variance is probably a lot larger than is currently expressed, and that would be my summary.

I wanted to point out one other thing though as we start looking at the different habitat types and so on, and so it seems that we often seem to settle into this thinking that, well, the stock assessment did a good job at estimating the fish on structure, and then there's this whole biomass that is somewhat disconnected from that on the uncharacterized bottom, and that's a possible hypothesis, but I wouldn't -- I am not completely buying it yet, and the reason is the fact that what we have on the structure is very similar to what the stock assessment has estimated.

It may be a lucky coincidence, and think about the fact that, since we haven't had those intercalibrations for the different methods -- Probably, once that is done, we will end up with a new and somewhat different estimate of abundance on structure, and, at the same time, the biomass on the uncharacterized bottom, we know from the NMFS bottom longline survey, actually behaved very similarly to the biomass on structure, in terms of changes over time, and so it may not be that disconnected, and, instead, what we might be looking at, for example, is the natural mortality rate, for example, that we're assuming in the stock assessment might be a little low.

If we change that, we end up with a different abundance estimate

as part of the stock assessment, of course, and so I would be cautious about jumping to a conclusion as to what this difference means. Thanks.

CHAIRMAN POWERS: Thank you. Any other questions or comments to Mary or from Mary? John Hoenig.

DR. HOENIG: I just wanted to make a point about post-stratification. There is two kinds of post-stratification, legitimate and illegitimate. Legitimate post-stratification is if, a priori, before doing the study, you know you want to stratify by something, but you can't, because you don't have a sampling frame that identifies the identity of the units.

For example, you might be saying I have a list of voters, but it doesn't say if they are male or female, but I want to know the answer by gender, and so I will take a simple random sample, and then I will divide them into males and females and ask my question, and that does not cause bias, and it can give you more precision, if in fact gender explains some of the variability.

Mary was talking about post-stratifying after the fact by looking at the data, and she is right that, if you look at your data and see what explains the variability and then decide that that's really what you meant to do all along, you will underestimate the variance, and that gets particularly bad if you look at a whole bunch of possible explanatory variables and then you find one that seems to account for the variability, and so you pretend that that's what you wanted all along.

You might say that looking at the results and dividing Florida into different strata is an example of that, and Rob said, yes, I can undo that, and that might be worth looking at, to see how much that changes things, but, looking at the pipelines and saying they're small, medium, and large, and maybe I will stratify by that, that's not going on a fishing expedition, and, if you didn't have the size of the pipeline beforehand, it's kind of logical that that's what you would do.

If you did have it beforehand, and you didn't think to stratify, then that could be a problem, especially if you're looking at the results and seeing that, oh, okay, pipelines matter, size of the pipelines, and so I will stratify by that.

I suspect that the variance reduction that you see is not artificial, and I think it's probably real, mainly because I think size of the pipeline probably matters, but there is a danger, as Mary points out, and I just don't think it's quite as

bad as she indicated.

CHAIRMAN POWERS: All right. Thank you. Mary, to respond?

DR. CHRISTMAN: I just wanted to mention, John, that Rob did in fact confirm that they had a priori belief, and it was the case that you described of the voters with gender, and so I was not implying that he was causing problems, but I just wanted to confirm that he in fact did that a priori.

CHAIRMAN POWERS: Thank you. All right. Dave Eggleston.

DR. EGGLESTON: Good morning, everybody. I guess, first off, I just want to thank Ryan Rindone and Carrie Simmons for the opportunity to participate. As I mentioned in my report, I'm a marine ecologist, and so I'm not a statistician or a stock assessment scientist, but I do work on spatial dynamics of exploited species.

I've really been impressed with the overall attention to detail by the scientists and the monumental scale of the study and the expertise of the team, and, actually, the coordination, in terms of the need to be flexible and adaptive as new knowledge was gained.

I want to make some comments on the process, just for folks that are listening, and so, basically, Greg Stunz provided a brief overview to the three of us external reviewers, and then we dove into the reports and wrote up sort of draft reports, and then we've had the past now going on three days of presentations.

The presentations were excellent, and they were well organized and clear, and that was important for me, because -- I am going to get to some of the details on how to, I think, modify the report to make it more readable and more understandable, but the presentations over the past few days, and the resulting discussions, have been really important for my interpretation.

For example, in reading the report, I tended to get off the rails a little bit when the random forest model occupancy stratification was stated as low and high, but then, later in the report, it was low, medium, and high, and then high in Florida was then reclassified as hardbottom, and so I think I got off the rails a little bit, in terms of concerns that I had about random sampling and weighting of the strata and spatial autocorrelation, and so those are areas that I raised in my draft report.

The discussions that we've had over the past two days have addressed those issues, and I now have a much clearer understanding of the sampling, and so, last night, I revised my report and eliminated those concerns, but I raised some other issues.

Anyway, I think the important thing is that this process, I think, has been appropriate for getting it right, as best as we can, and just to go back to Steve Murawski's comment earlier, and, I mean, this is a very complex set of studies and study design, and so I think that the getting it right piece is very important, but also in terms of modifications to the report that are going to help other reviewers.

What I am going to do though is actually take a step back and talk big-picture, and then I will get down to more details, as I make further comments, and I think it was interesting, because I had written up some of these general comments before the public feedback yesterday from a lot of the stakeholders, and their feedback really resonated with me, just thinking about the investment by taxpayers and the return on that investment.

I think that my experience has been, in fisheries when the stakeholders don't feel like they're part of the process, it leads to problems, and so we see, for example, illegal fishing, when you can get away with it, and so I think it's hard to put a price tag on the, I guess, public outreach aspects of this project, in terms of making stakeholders feel like they're part of the fisheries management process, and so I think that's something that should be highlighted, and it is highlighted in the report, but I think it's -- From a taxpayer perspective, I think it's really important to have the stakeholders feel like they're part of the process.

The other is just sort of the lessons learned with respect to methods and sampling design. So, basically, we've got this increased expertise, in terms of not only calibration of these different methods, but also training of new scientists, and so this is going to save future studies a significant amount of money. Again, it's hard to put a price tag on it, but, when we talk about doing a similar great red snapper count, or amberjack count, in the South Atlantic, we're going to save a significant amount of money, based on the lessons learned, I think, from this particular study.

The last area, in terms of sort of value added, and that's been alluded to in many of the presentations, is just the biological information that's been generated and the new areas of research

that are going to be spawned.

For example, just the -- Again, the uncharacterized bottom and these benthic anomalies that may be holding fish, and, again, I'm a spatial ecologist, and so that's why I kept focusing in on what are the potential spatial dynamics of this population, and it's a really important thing to get a handle on, because my concern is that, if these uncharacterized bottom areas are serving as sort of de facto reserves for the brood stock, it would be good to know something more about the spatial dynamics. As we know, location matters, in terms of fisheries.

Lastly, I would encourage the scientific team to think about a special issue in something like a journal like *Transactions of the American Fisheries Society*, because I think it's not only going to be an important resource for others, but I think it's going to be an important baseline. I mean, we know we're seeing mangroves taking foothold in the northern Gulf of Mexico, and so what is that mangrove community going to look like in fifty or a hundred years, and are we going to see a transition from red snapper to gray snapper, because of that?

I think, again, big-picture, I think there's a tremendous amount of positive information that came out of this study, and so now I want to kind of focus in on the report itself, and, again, I really appreciated the team's identification of the sampling biases and the potential for integration with management, and I agreed with the future research recommendations, and I appreciated the overall collaborative tone with respect to informing the stock assessment models, but not superseding them.

As I mentioned in my initial draft report, I think it would help the reader to place the age-two-plus red snapper in the context of the broader life history or broader life cycle of red snapper, and I think there needs to be some type of life history diagram, this sort of typical circular life history schematic in the report that would depict things like spawning locations and times and settlement habitats and ontogenetic habitat shifts.

That's important, because, again, as Mary and others have mentioned, the public has gotten ahold of this report, and so it's important, I think, as an educational tool, to educate folks on these complex life history strategies and the importance of some of these spawning areas as well as nursery habitats.

The other thing that really helped crystalize my understanding of this, I think, complex sampling design were the presentations

yesterday by Rob Ahrens and Lynne Stokes. For example, Rob basically, in his presentation, had a series of sort of block sets of text which basically highlighted the approaches that were taken in each type of strata in each sort of region or state.

For example, I felt like, in some respects, while there was a general description with text at the beginning of the report that talked about the sampling design, I felt like I almost parachuted into the trees without having a map of the forest, and so I really got into the details of the methods and the calibrations, but I'm still not 100 percent clear on the sampling design.

What I would suggest, in terms of a revision to the report, would be to have some type of sampling design roadmap, in a simple form, that describes the initial sampling design, in terms of where the population estimates were derived from, and, for example, in UCB or artificial structures or known higher bottom or pipeline, and, again, I think Rob really nailed it yesterday, in terms of his presentation.

Then I think another roadmap on the changes to the sampling design and the analysis plan, and I think, again, what crystalized it for me was Lynne Stokes' presentation, where I understood the estimation process much more clearly, and I understood how the estimation process was handling a lot of the potential variance. Those are, I think, two areas that would help the report.

The other thing that I think was causing some trouble, which I know Luiz Barbieri and I kept going back to Tables 6 and 7, is that, again, I think it would help, and I mentioned this yesterday, but it would help to have an appendix table that shows the different strata, the different footprints of the strata, but, also, it shows the weighting, so that somebody can go back and actually, with a simple calculator, kind of estimate some of those population estimates on their own, just to understand the process.

For example, it also threw me off the rails in looking at Table 7, where this was this sort of mix of either mean density, fish per hundred square meters, or the fish by structure, and so that needs to be much more clearly delineated.

I have gone through the report, and I've taken a lot of notes during this meeting, and so I'm going to add -- Like I said, I've basically already revised my draft report, and I guess,

just to wrap-up, Ryan Rindone, yesterday, put it to us that, basically, we're basing our decision on the best available science, and so, in my mind, this is the best available science that suggests that that absolute estimate of abundance is, I think, in the realm of what's realistic, but probably definitely an underestimate, based on the lowered efficiency of hydroacoustics certainly in the western part of the Gulf of Mexico, as well as the point that Steve Cadrin made earlier about the fact that strata that has zeroes probably had fish and just were undersampled.

Then, yes, I mean, obviously, I'm going to defer to the statisticians and stock assessment modelers on how to improve the variance, but, obviously, it seems like there's some opportunity to refine it, and so I think, with that, I will probably just stop and see if there are any questions, and so thank you, Chairman.

CHAIRMAN POWERS: Thank you, Dr. Eggleston. Lynne Stokes, did you have a question of Dave?

DR. STOKES: No, and this is just -- Thank you for recognizing me, but this is just a clarification, or maybe a misunderstanding, of something that I heard in Mary's summary, which is that she seemed to indicate that she thought that I had treated transects differently in Florida and Texas, and I did not. They were taken -- The sampling units were taken as transects in both places. That's all.

CHAIRMAN POWERS: Thank you. Greg.

DR. STUNZ: Thank you, Mr. Chairman. I just wanted to thank the review team for all the thoughtful comments and to assure you that our team will take these very seriously and build that into the report.

Steve, a few things on that number that you commented on, and something that we noticed as the team about the differences in eastern and western, as it related to the prior stock assessment, and with typically -- So everyone is aware, thinking a lot more fish in the west than the east, and, numbers-wise, that does appear to be the pattern, but, if you looked at biomass, which we have not calculated, and that wasn't a task under consideration here, but it certainly needs to be done, I think that west-east relationship would still hold up, in terms of biomass, because we simply have much bigger fish out in the west, as those snappers are recolonizing the east from overfishing. Yes, that's an interesting side outcome, but it's

something important, obviously, for management purposes, for those folks to consider.

Mary, I hear you very loud and clear on the report modifications, and so we will definitely take those, and I did want to comment that this report -- It's for a variety of user groups, from legislators all the way down to scientists at our level of detail, and that's quite the challenge, to build a report at that many levels, and so all of your suggestions will be greatly taken as we try to do this one-size-fits-all sort of thing.

Then, Dave, Dr. Eggleston, I really appreciate you bringing up the public outreach component. While that was not part of our terms of reference here, unfortunately, that was a major component in this project, led by Dr. Drymon, and we're extremely proud of the outcome of that.

In terms of the public, I don't think -- I may have mentioned this, but I don't believe there was a national newspaper anywhere in the U.S. that did not cover this project at some level, particularly as it related from the tagging component of this, and it just took off like wildfire, and our social media would be reaching a quarter-of-a-million people a day on certain posts and that sort of thing, and so was important, not because of what we're discussing here today, but it's important broadly, for many of the reasons that you pointed out, and others, and so thank you for bringing that up.

I think your life history figure is an excellent idea, and we'll definitely do that, as well as, now that we've had all of this discussion back and forth with Tables 6 and 7 and all the strata and footprints and weightings and so on, we'll make sure the team is clearly accounting for that as well in the report, and so, largely though, I really appreciate all your comments, and we'll take those and make those as we finalize this process. Thank you, Mr. Chairman.

CHAIRMAN POWERS: Thank you. Mary, you had another question or comment relative to Dave?

DR. CHRISTMAN: I just wanted to respond to Greg. Believe me, I completely appreciate how difficult it is to put all of this information together and try to do it in a way that you can capture every different kind of reader who is likely to look at this, but I think what I would recommend then would be that all the gory details that people like me love go into appendices, and maybe just do it -- Like, for statistical methods, build it

like *Science Magazine* does.

For the statistical methods, you would describe it in general, and then give me the gory details in an appendix, and so that might be one way around this issue of trying to be all things to all readers, but, at any rate, I really appreciate all the work. Maybe a little bit finer reading of the details, so that the sample sizes are the same throughout and things of that nature.

DR. STUNZ: Yes. Thank you, Mary, and we'll make sure that we address that explicitly.

CHAIRMAN POWERS: All right. Thank you. I mean, essentially, what we've tried to -- The SSC has tried to mimic a SEDAR review process by collapsing it into two days, and what we were looking for is the same kind of detail that would typically go into a SEDAR review. Ultimately, I think we've gotten there, or close to it, and this will evolve as time goes on, and so I think, even though this isn't the normal process by which we deal with reviews, I think this has been constructive in many ways.

At this point, we're going to be transitioning to the real bottom line, in terms of how the SSC wants to proceed. Basically, what is our advice to ourselves, as we go ahead, in terms of making catch advice? I am going to make a couple of comments, and then we're going to take a fifteen-minute break.

One of the things is that, typically, we go through and we have motions, and people vote on the motions, and I would really like if we can come to a consensus on how we can proceed, and, obviously, that's up to individuals, but I think perhaps we're close to it, and it would make much more sense, in terms of providing advice.

Again, the issues are under the rubric of best available science, but, beginning with Will's comments at the beginning of today, we're really talking about what to use going forward for this ad hoc analysis for catch advice for this year and what should we use, what we can't use, and so on, and so let's think of it in those sorts of terms. With that, and I am not taking any questions at this point, and let's take a fifteen-minute break and come back and proceed accordingly. Thank you.

(Whereupon, a brief recess was taken.)

CHAIRMAN POWERS: As I mentioned before, we are at the stage of making a recommendation for how we want to -- How the SSC is going to proceed this afternoon. Basically, this has been

couched in the introduction by Ryan Rindone as determining best available data.

I would prefer to be a little more specific than that about what we want to carry forward, and one of the things that we might consider is basically not worry too much about the legalese of what best available science is, but rather here is an estimate of X, Y, and Z, and we would want to take that forward in evaluating catch, catch advice, for 2021, later on, and so that's the kind of approach that I was thinking about, but I will certainly hear other opinions. Kai Lorenzen.

DR. LORENZEN: I agree, and I think what we should do is we should go through the terms of reference, at least briefly, and sort of characterize our review of the Great Red Snapper Count, but I think we neither can, nor have to, make a determination of best available science on this, and the reason is that, of course, an abundance estimate by itself is not sufficient for catch advice, and so we need to combine this information with information that will allow us to judge what the sustainable take is.

To do that, we need to combine information from the Great Red Snapper Count, if we want to take that forward, with information that is essentially derived from the stock assessment, and we should then judge best available science for the combination of information from those two sources that will give us catch advice.

In fact, I think we might be putting ourselves into a difficult position for later on if we decide on best available science for the Great Red Snapper Count by itself, because we, obviously, still need to use information from the stock assessment. Thanks.

CHAIRMAN POWERS: Thank you. Greg, a point of clarification?

DR. STUNZ: Yes, a point of clarification. Some of my team is asking me, and some are obviously on the SSC, and I assume you're fine with us sticking around, those that aren't, but are we, I guess, released, or are you going to need things from other members? I'm trying to just get some guidance on what you're going to need from us.

CHAIRMAN POWERS: At this stage, this is a true SSC meeting, I guess, and, of course, it's a public meeting, and anybody can listen in, and I can't tell people to stick around, but, obviously, if somebody has a question or something relative to

one of the PIs, I would encourage them to ask that, but, beyond that --

DR. STUNZ: Okay. Well, we should have expertise on the SSC to field probably any questions that come up, and I will just ask my crew to be on standby, should anyone have specific questions that we could readily address or something.

CHAIRMAN POWERS: Okay. Thank you. Ryan.

MR. RINDONE: Thank you, Dr. Powers. As a function of the review, the reviewers, which is the independent consultants and the non-PI SSC members, do need to make a determination about the study itself, as is specified in the background there, that the primary objective of this review is to determine whether the absolute abundance estimate and its variance is reliable and consistent with input data and population characteristics for red snapper in the Gulf.

The three independent consultants have weighed-in on this, and you had talked about coming to a consensus, what I thought was about this, prior to the break, and coming to a consensus statement, or something like that, to offer to the council, with respect to the Great Red Snapper Count and its appropriateness for its estimate of absolute abundance and its variance would be very informative, not only to the council, but to the SSC in its deliberations moving forward, as it considers catch advice.

Before the SSC meeting formally begins, where we start talking about the interim analysis and everything after that, I think that this does need to be completed, in order to complete the review of the project.

CHAIRMAN POWERS: I thought that's what I had just outlined, but apparently you did it much better than I. Harry. Let's go to Doug while Harry gets fixed up. Doug Gregory.

MR. GREGORY: Thank you. I am reluctant to make a decision this week, and this is a lot of information, and it's complex, and it's very important. We got the document three weeks ago, and we got the underlying data presentations just this week, and it's been overwhelming for me. I've got a statistics background, but that was many years ago, and so I would be reluctant to draw any definitive conclusions about this study at this time. Thank you.

CHAIRMAN POWERS: Harry. We're still waiting for Harry. Given Doug's comment, it's unlikely that we will have a consensus

then, and so we will proceed as Ryan indicates, and, Ryan, do you have a comment relative to that?

MR. RINDONE: Yes, sir, and, I mean, understanding that there may be disagreement amongst SSC members and reviewers about the different nascent points of the report, that's totally fine, and to be expected. I mean, when any manuscript is reviewed, there are obvious differences between the different editors of a manuscript prior to it being accepted or not for publishing, and so that's all fine, and we'll do our best to characterize those differences as you guys discuss this.

We have gone ahead and done that for the comments provided by the three independent consultants up to this point, and their comments will be best captured in their reports, which will be delivered to the council by April 10, and we will, obviously, post those for everyone to be able to review at that time.

For the purposes of this review, while we have everybody here in one virtual spot, to the extent to which these discussions can take place, that would certainly benefit the review, to coming to some determination, and then, as you guys have made clear, it doesn't have to be an all-or-nothing, and I think the consultants all led with that approach, that it doesn't have to be an all-or-nothing, and that there's an awful lot that is going to do a great deal to advance our understanding of the species, but it's up to you guys, as a complete review body, to make that decision.

CHAIRMAN POWERS: All right. Harry, are you viable yet?

MR. BLANCHET: I am now. Thank you. Sorry about that. Something got hung up, but I had one or two questions about the project itself that I did not ask during the program. The first was on the pipelines, and I was hoping that the PIs would still be available to respond to it, and one of the things is why was there no pipelines less than eight inches in diameter considered as part of the study?

CHAIRMAN POWERS: Why don't you say your other question too, while we get somebody together to answer it?

MR. BLANCHET: The other was more in terms of a comment, and several people have made or alluded to it, especially Kai, and the outstanding question at this point really seems to be what is that interaction between that biomass in the uncharacterized bottom and the biomass on structure.

Certainly, in the western Gulf, for years and years, we have seen what I have informally called a hurricane effect, in that you can go to that oil rig that everybody in the world goes to fish on, and it's pretty well plumb-out of snapper, and you can go back after a hurricane, and it fills back up, and so there are some mechanisms, in terms of exchange rates between those, but, at least to me, they seem to be at least highly variable, and maybe driven by oceanographic or tropical systems, oceanographic currents, other variables that might be difficult to characterize, in a projective sense.

That seems like that -- I was really hoping, because this is as good of a set of minds as we're going to have on the subject, but how do we approach that type of a subject, to get a good handle on that exchange rate?

CHAIRMAN POWERS: At this stage, I'll take that as a comment, and I'm not sure that we are prepared to answer that, and, in terms of the size of the pipelines, if somebody wants to interject, feel free, but, in the meantime, Kai, I think you have the floor.

DR. LORENZEN: Thank you. I wanted to sort of support Ryan's point, and also to Doug's point that we're not looking at a yes-or-no answer at this review, but we're looking at, essentially, summarizing our discussion, and the actual decision point will then come in the next stage, and, there, we'll have to consider basically the combination of information from this survey and the stock assessment that we have to make in order to arrive at management advice, and I was wondering whether that is -- If not making a yes-or-no decision about this point, but literally characterizing our discussion, would allay your concern. Thank you.

CHAIRMAN POWERS: Thank you. If that's the case, then we need to just -- Those terms of reference that need to be addressed, we need to just go down the list and see how we respond to them, and so if we could do that. I have a couple of people, and I'm going to stick with the SSC members at this point. Doug Gregory.

MR. GREGORY: I thought the joint SSC/technical review team discussion of the study was concluded, and we were going to start talking about the assessment stuff, and, since the review team has not even completed their recommendations, I don't see how we can make conclusions or recommendations on the terms of reference, until we have the full information. All of that seems premature, but I'm eager to go forward with the SSC

agenda. Thank you.

CHAIRMAN POWERS: All right. My understanding is that we are, we as an SSC, are asked to provide some comments and advice about the Great Red Snapper Count. Ultimately, what we want to know is what information we can take from the Red Snapper Count to take forward for this afternoon for the implementation of management advice.

There is a list of things there, but it basically comes down to the abundance estimates and the variance estimates and so on, and how we might want to parse that up. That's my understanding, and so my objective then was can we make either a consensus or a motion or whatever, but a recommendation saying something that the Red Snapper Count, the estimates of X, Y, and Z are useful and should be evaluated in determining catch advice levels for 2021, and, essentially, that's the bottom line, as I see it. How, procedurally, we get there, I will defer to Ryan.

MR. RINDONE: That sounded a lot like a motion, Dr. Powers. I was going to clarify something for Mr. Gregory and for anyone else that may have similar uncertainty. Please be certain that, as I previously stated, the combination of the non-PI SSC members and the independent consultants constitutes the review team for this project at this meeting.

It's not, well, let's wait and see what the consultants come up with, and they have told you guys their thoughts at this point, and they will detail those out in their final reports, but they have definitely made it clear where they stand on this, and they are a component, but not the only component, of the review team. The review team consists of all of you, to make this determination, based on the terms of reference.

CHAIRMAN POWERS: All right. Benny Gallaway.

DR. GALLAWAY: I am going to ramble a bit, because I'm having a wide range of thoughts, and the first is this study has received tremendous publicity and is out there in the public community, and I heard the let not the perfect be the enemy of good several times yesterday, and I kind of feel like it's premature that this information is out, but I think several things about the report need to be cleaned up and addressed before I can render an opinion.

I see, I think, the recognition of the significant biomass over uncharacterized bottom is significant, but it's not new. That habitat, and that population, has been protected since Reef Fish

Amendment 1 in 1990, and it's made a big difference, I think, in the ultimately rebuilding success of the population.

I'm concerned about several things that need to be cleaned up that we've discussed, that I won't go into, but Greg, or Dr. Stunz, is well aware of those, and they're working on it, and they'll get those cleaned up, and I would like to see that.

I remain concerned about the samples, the size of the samples, that are being used to extrapolate to large areas. When I see some of the samples, like a thousandth-of-one-percent, extrapolated to huge areas, I have problems with that, and I think I'm not ready to make a decision at this point.

I think this report has really made a contribution, and don't get me wrong, and I think it's very valuable in a lot of ways, but I think it needs more careful review. It hasn't received, for example, the extent of review that typically goes into a review workshop for a stock assessment, and so I'm still struggling with addressing issues in my own mind, before I'm ready to speak to how this report should be used. Thank you.

CHAIRMAN POWERS: Thank you, Benny. So your position then, ultimately, and this is how I'm interpreting the ramifications of it, is that, at this stage, we should not be making -- Utilizing this information to adjust catches for the 2021 year, and, if that's the case, then we would fall back to what was referred to as the traditional interim assessment approach that the Center put together, has put together, and, I mean, that's how I am interpreting the ramifications.

DR. GALLAWAY: I think that's pretty close to what I'm thinking.

CHAIRMAN POWERS: Okay. Thank you. Luiz.

DR. BARBIERI: Thank you, Mr. Chairman. I don't disagree with the main points that Benny just made regarding applicability of the results to management at this point, where we are, the results of this study, but I also wanted to make some points about the review process itself, and Ryan Rindone already covered some of those.

I mean, I think that this review process was put together to somewhat mirror what is done through the SEDAR original review process, and it's trying to abide by what are the criteria that need to be there from National Standard 2.

If you think about the fact that, whenever we have CIE reviews

of stock assessments, that review workshop is chaired by an SSC member, and there are SSC representatives at that workshop that will serve as reviewers, together with the CIE reviewers.

Now, here, the only difference is that, instead of having just a few SSC members, we have the entire Standing, Reef Fish, and Socioeconomic SSC. Now, we received the report, and we received, during the last couple of days, all of these presentations, and we have the preliminary, pretty much accomplished, review reports from the consultants, the reviewers, and so I don't really see why we should not complete a review here, now, because I think it's going to be confusing for the council, going forward at its next meeting and trying to deal with this, without really understanding what the SSC's decision here was.

There is always more that we can learn about this project or the analysis needed or how additional review could be made, but, at this point, I feel that this process was put in place, and it has been planned and noticed as a review of this project, in the way that it would provide scientific advice to management, and I think we are there, and so I just wanted to put that out there, Mr. Chairman.

CHAIRMAN POWERS: Thank you. Kai Lorenzen.

DR. LORENZEN: I am thinking -- I mean, the crunch is what, Joe, you mentioned, and it's really the last part of the TOR, where we have to say, well, is this is a representative estimate of abundance, and is the variance representative, or realistic, and that's really, I think, the main decision we have to make.

There are a lot of detailed points here in the TORs that essentially can be populated from the consultants' reviews, and perhaps discussed, but I think the crunch is that. I think, if we can see can come to a consensus on that point, then it would more or less determine how we move forward, and maybe we should see if we can do that upfront. Thanks.

CHAIRMAN POWERS: Thank you. Clay Porch wants to interject, hopefully a clarification.

DR. CLAY PORCH: Thank you, Chair. I appreciate you letting me speak, even though I'm not a member of the SSC. I did just want to ask the SSC to be careful not to set the bar too high for best scientific information available. As I think you're aware, the NS 2 Guidelines give you several criteria for determining whether something is best scientific information available, and

you consider the relevance and inclusiveness and objectivity and transparency and timeliness and peer review.

It doesn't say that it has to be anywhere near perfect. Ultimately, you need to make a determination for catch advice based on either the previous stock assessment, which, of course, is now also out-of-date, or using the Great Red Snapper Count information, and so you're going to have to do one or the other. The question is basically which estimate of abundance is likely to be most accurate and not whether the Great Red Snapper Count checks every box that you would want it to.

I do think that caution is warranted, and so I am completely onboard with the comments that Benny and Luiz and Doug and others have expressed, but, as you will see with the Center presentation, when we get to it, there are some ways to address that. Either we can use a P*-type approach with an expanded variance, or there's other means. We can assume a certain fraction of the fishing effort can occur in that uncharacterized area and moderate the catch advice that way.

We can also look at maybe more conservative reference points for the ABC advice, and not for the OFL, and what I mean by that, for instance, is the stock assessment estimated a smaller, but highly-resilient, stock, on a per capita basis. What it appears we have now is a huge cryptic biomass, as Steve Murawski had pointed out earlier, and so, instead, we have probably a less-resilient stock, in a per capita basis, but a huge reservoir that was seeding the population in the fished areas.

There's ways to account for that, probably using a less-aggressive reference point, like F 40 or 45 percent would be appropriate for ABC advice, but we have all of that in the Center presentation, and I think it will be clearer once we get to it.

Just, again, I wanted to clarify that best scientific information available doesn't mean that it has to be perfect. The question is, is it better, likely to be more accurate, than the previous stock assessment. Thank you.

CHAIRMAN POWERS: Thank you. We are well aware of the presentations coming and the discussion, and so we'll keep that in mind. Doug Gregory.

MR. GREGORY: Thank you again, and I will try to be more clear. What Clay is saying is basically what I'm thinking, and maybe I am being too persnickety here, but the first two days, and a

little bit of this morning, was the review of the Great Red Snapper Count. I am looking at the terms of reference, and the SSC is not mentioned there, and I assume that was for the technical team.

When we made our motion back in January, I think we envisioned having the council conduct a review workshop that included some SSC members with particular expertise, and I don't think we envisioned this joint meeting that we've had, and so it's like we've completed this first part, and it's not up to us to bless or reject this study, and it is what it is, and it's -- I applaud it like everybody else has, but our job is to see how this information can be incorporated into an interim assessment, or is it so complex that it should wait for a research assessment, a research track?

I just don't see us spending time going over the terms of reference again, and that could take up the rest of the week, to try to answer all those questions, but let's move forward to see -- It's there, and we can use the information, and we can look at what National Marine Fisheries Service has done, as far as trying to incorporate it into an assessment, and determine there what can be used or not for the assessment purposes, and so I'm not rejecting the study, but I am just saying we need to move on.

If you look at the terms of reference for the SSC meeting, it's all about the assessment, and so there's two distinct meetings here, and I just feel like we're in the second meeting. Thank you.

CHAIRMAN POWERS: I think there is a bit of a dichotomy here, and I am sure that Ryan can weigh-in again, but, essentially, the bottom line is do we want to utilize some part, or all, of the Great Red Snapper Count in this afternoon's discussion to make catch recommendations for 2021, and, I mean, to me, it's pretty -- That's the bottom line, and some of the other detail -- When we talk about the terms of reference, again, I have some confusion about what was terms of reference for us, the SSC, and what was terms of reference for the consultants. Ultimately, the bottom line is do we utilize some or all of this data as we go over things this afternoon and tomorrow.

MR. GREGORY: To that point, Mr. Chair, how can we do that until we get to that discussion point and get to those presentations? We have yet to see how they are being incorporated, and we can make a decision then, and I don't see how, a priori, we can say this part we can use and that part we can't. Thank you.

CHAIRMAN POWERS: Well, that's a point. Ken Roberts.

DR. ROBERTS: Thank you, Mr. Chairman. I ask your tolerance, and I have to step away here for a couple of hours, and so probably when you're discussing something that I want to make a comment on, and so I am the sole manager of a trust, and we have a meeting in about thirty minutes, and two hours will be taken up, and so I may miss some of the discussion.

Anyway, my point is that I think the studies are good enough to inform the council, with some confidence, about the direction that they should be going, in terms of increased harvest, increased availability to the user groups of red snapper. I am not sure that is debatable, and we seem, to me, to be arguing more about how far one would go. In other words, how much you can move in the direction of utilizing more fish and then how quickly you could do it.

To me, my view of things is the direction is really clear, from the report. How fast one would go and how far one could go are actually the critical things, and I was pleased to see Dr. Stunz's last thing on moving forward about research recommendations for future studies, and I think those research recommendations would more likely be fruitful if they focus on not the direction, but how quickly and how much speed -- How far you can go as we move forward.

I don't think that will come unless there's a pretty clear plan laid out for filling in some of these gaps, particularly what Benny was talking about earlier, about the acoustics and some oceanographic things, which are totally missing in the report, and how we organize, as a nation, to get this group of people continued working on this project and making those kinds of research gains to answer the question of how quickly and how far can we go in releasing more fish to the user groups.

It was a tremendous effort by NGOs and scientists and whatnot to get the first appropriation, and I don't know, and I'm not involved in it right now, but I'm concerned that that same energy and inertia is missing for Stage 2, and I think, if the council knew that that was being organized, and likely to be funded, that certain aspects of this group could be continued and focused over the next several years, and that would be of great use for the council to know, also. I thank you, Mr. Chairman, and I'm sorry to have to leave for a couple of hours, but I don't really have any choice. Thank you.

CHAIRMAN POWERS: Thank you. John Mareska.

MR. MARESKA: Thank you, Mr. Chairman. **I sent a motion to Jessica, and she could put that up on the board.** I agree with a lot of the sentiments that have been expressed, and I feel like we have done a review, and I don't think there's going to be a review with the research track, and I think it was pointed out that this is -- I am trying to stay away from the best scientific information available statement.

It was a very detailed and well-designed study. Yes, it's not perfect, and I think, as Clay was pointing out, there are things that can be done to improve it, but I am hoping this motion kind of wraps up some of the sentiments that have been expressed, and I hope it finds the spirit of kind of what you were alluding to a few minutes ago, and, if you want to hold off on this and let some other people speak to it, I'm fine with that as well.

CHAIRMAN POWERS: Thank you, John. Before we get into discussion and asking for a second, I want to make clear a couple of items there. One of them is you refer to the interim analysis, and, by our terminology, to me, the interim analysis is where you adjust the CPUEs, that second form of analysis, and so I want to make clear what you're referring to there.

I don't think you're referring to that. Basically, the interim analysis doesn't use the GRSC population estimate, and so I think what you're referring to is just sort of what we're doing in the interim and not, quote, unquote, the interim analysis.

The second thing that I would make a comment about is the modifications to the variance, and it's unlikely that we're going to see those before June, essentially, and I'm not sure how we might deal with that, and so, if you want to comment on that, and then we'll go forward with your motion and ask for a second. Thank you. John.

MR. MARESKA: Yes, and the interim was exactly as you expressed. It's what we would potentially look at in the second part of this meeting as an SSC, and the changes, I guess, to the variance and corrections by the investigative team would be more for incorporation into SEDAR 74, rather than this interim catch advice that we'll be building on, hopefully, in a while.

CHAIRMAN POWERS: Okay. Thank you. Can I ask for a second?

MR. BLANCHET: I will second that.

CHAIRMAN POWERS: Thank you. Ryan, do you have some procedural guidance, hopefully?

MR. RINDONE: Yes, and this is procedural guidance. Whether to use the Great Red Snapper Count population estimate and its variance in the interim analysis would be something that would be completely under the purview of the SSC.

For the sake of this review though, determining whether the population estimate and its variance, after being modified as recommended by this review panel, be incorporated into SEDAR 74 is something that could be recommended by the review panel, and that would constitute some satisfaction of the terms of reference. Again, we're trying to punctuate this review by having some sort of either consensus or majority statement about the work that's been done, considerate of the comments that have been made about the changes that are necessary and the responses from the project team with respect to those reviewer comments.

As far as whether to use this information for informing catch advice, that's still the SSC's decision exclusively, and that's not something that the independent consultants are involved in, and that responsibility, under Magnuson actually, falls exclusively to the SSC, to recommend catch advice to the council.

Here, and, Mr. Mareska, I don't know if you want to consider any modifications to your motion, just from this procedural guidance, but consideration of the interim analysis here I think might be procedurally premature, but, as far as the rest of the motion is concerned, I think that's appropriate under the guise of the terms of reference and what we have asked of and directed the review body, which is all of you, to consider.

MR. MARESKA: Right, and so I think what I was just trying to get at was the fact that we would be reviewing this interim catch advice that was derived from the estimates of the Great Red Snapper Count, and so I can do some wordsmithing to fix that situation right there.

CHAIRMAN POWERS: Okay, John, and hopefully Harry will agree with this too, but why don't you kind of go back and revise this, and not necessarily on the screen, because there were several other people that wished to talk, and I suspect they're not really addressing this motion, per se, and so let me go ahead to them while you revise that, as you see fit, if that's okay. Dave Chagaris.

DR. CHAGARIS: Thank you. I just wanted to take a minute to kind of express where I stand on this, but, first, I do want to congratulate Dr. Stunz and the research team on executing such a large-scale and complex study.

We knew, going into this, that there were many pitfalls and challenges, but you all made it through from start to finish, and you really can't overstate the breadth of knowledge that has been gained from this process and this study, and it will be a foundational study for fisheries research and monitoring in the Gulf and beyond, and so congratulations on that. Thanks to the research team and the reviewers for the last couple of days. It was very productive and very informative.

For me, perfection was never a bar for acceptance. I mean, I'm comfortable with a healthy amount of uncertainty and error, because it's inherent in everything that I and we do, but, in several cases, I feel the estimates are far from perfect. In some regions, or strata, the best we can say is that the estimate is likely in the ballpark, as no data were collected, and, in other cases, the statistical analysis was deemed inappropriate, or major assumptions weren't demonstrated to be met.

In that regard, I don't feel that we've seen the final estimates yet, and I think we should allow the team time to incorporate feedback from this review process and revise the analysis accordingly, and also tighten up the report, which others have suggested.

As to the bias and the abundance estimate, I generally agree with the research team that decisions made in the study likely led to an underestimate in densities. However, there are areas where abundance could be biased high. For example, if our habitat proportions, or our reef counts, are not correct for species and age compositions and that were not actually representative.

As to the precision, I do agree with most that the CV of 11 percent is an underestimate, but I also don't think that it will completely blow up when the data are re-analyzed, and this issue of CV is important for the assessment model, and I'm interested to see how the abundance estimate squares with all of the other data streams that we have on red snapper.

I mean, remember that we cannot detach ourselves from the assessment model, and we need it to determine stock productivity and productivity-based reference points, and also for making

catch projections, and so I honestly don't think the book is closed until we see it integrated into the assessment model, and, ideally, that could be explored sooner than later, and even outside of the assessment process, and Dr. Cadrin has provided some good suggestions on moving forward with that.

I anticipate that, with the new recreational landings that the model will be able to accommodate this higher abundance estimate without sacrificing fits to other data or violating some of our assumptions about life history and population dynamics.

Then, lastly, I just wanted to say that we are reviewing this information with the backdrop of providing tactical management advice in this interim analysis. Personally, I don't feel the results are ready for that yet, and maybe they will be in a matter of weeks or months, but we should also be thinking about how this information can factor into the strategic decision-making and our thought process, the thought process for this committee, which I think is what Dr. Porch was alluding to.

For example, we'll be seeing some projection scenarios and ABC advice, and I'm sure we'll see a range of different scenarios, and what we have learned, over the last couple of days, might factor into which of those scenarios we select, or, for managers, whether or not they want to take more risk or be more precautionary, now that we have this new information behind us. That's all I have to say for now. Congrats, again, to the research team, and thank you for the last couple of days.

CHAIRMAN POWERS: Thank you. Jim Nance.

DR. NANCE: Thank you, Dr. Powers. I appreciate being able to be here the last two days and listen to all these presentations, and I think this is, in my mind, one of the best research efforts I have seen over my years.

The things that I wanted to point out, I think this study has shown, very well, that there is a large -- There seems to be a large, cryptic abundance of red snapper that is not fished, the magnitude of which I think is uncertain, and that's why I am having the problem.

I think the magnitudes that are being shown are uncertain, but I think they're certainly usable, and that's kind of where I'm coming from. I think the study is well done, and the -- I think it has portions that we can use to inform the assessments and be able to look at catch from the assessments, with some incorporation of the data from this study, and so thank you.

CHAIRMAN POWERS: Thank you. Before I get to Camp, it seems that we're getting caught up in procedural things about what's a review and what isn't, and what the terms of reference were and what is not, and I am inclined to say this is an SSC process, and we can make scientific recommendations as we see fit.

If there is a terms of reference that we are unable to do, or unwilling to do, then that's our prerogative, and so, ultimately, what we're really being asked for is not necessarily -- I mean, we've gone through this review, and the record of it is going to be extremely useful for both future assessments and also for the research team, as they develop their reporting of this in June, or sooner or later.

What we are really being asked by the council is not all the details about what constitutes best available information, but it's basically should we utilize this information to modify catch advice, and, if so, do that, and that's essentially the bottom line, and so, in a sense, in my mind, if we just stopped the meeting right here and went on to the SSC meeting, where we have a presentation of actually implementing these population estimates, in several different forms, into the projections for 2021, then we would make a decision at that point about whether that's the best available information to provide catch advice for 2021. I think, in some ways, we're making this a lot more difficult than it really needs to be, and so that's my rant for the morning, and next up was Camp Matens.

MR. MATENS: Good morning, gentlemen. Dr. Powers, thank you very much for recognizing me. I really can't comment about the scientific aspects of this, and shouldn't, but what I would like to comment on is, if one of the goals of this exercise, this whole exercise, is to make a decision vis-à-vis the 2021 season, as a former council member, I can say that they're going to be under the gun.

We just have a short period of time here to either give those people something or not give them something, and, if we can move forward, I would encourage us to do so.

The second, kind of a smaller thing, is there's been discussion about the fish on pipelines are not being prosecuted, and, at least where I am, those fish are prosecuted, and we know, as a fisheries group, particularly where the pipeline junctions are, where one pipeline crosses another, and those are hot spots, and valve stations. Those are hot spots.

Now, I don't know if the commercial guys prosecute those or not, because, with their thirteen-inch limit, they may not want to fool with those bigger fish, but that's where the bigger fish are. Further, when you go to an oil rig, particularly an oil rig in relatively deep water, yes, the snapper are on the bottom, but there's a bunch of small snapper mixed into them.

If you want to target larger fish, they're going to be at intermediate levels, mostly at thermoclines. When you get under a thermocline, if you're diving, it looks like a big mirror, and you can't see through it, and those fish know that, and they're in there. A lot of big fish are doing that, and they're in there, and so thank you very much for allowing me to bring this forward, and I applaud everyone here for all the work that they're doing to try to make this happen. Thank you.

CHAIRMAN POWERS: Thank you. John Mareska.

MR. MARESKA: Mr. Chairman, I just sent the modified motion to Jessica. Hopefully it addresses all the concerns, procedurally, that Ryan pointed out.

CHAIRMAN POWERS: Thank you.

MS. MATOS: I haven't gotten it yet.

CHAIRMAN POWERS: Okay. While we're waiting then, Benny Gallaway.

DR. GALLAWAY: I wanted to say that I really appreciated Clay's comments on how these population estimates might be viewed and utilized in the stock assessment. We haven't seen that presentation, or that paper, yet, and so I appreciate your comments.

CHAIRMAN POWERS: It's not really a stock assessment. Excuse me. I interrupted you, Benny. Go ahead, Benny.

DR. GALLAWAY: The major findings of the study said that there's a large, cryptic population over uncharacterized bottom, and I concur with that, and I just don't know if we -- When you say we want to use the Great Red Snapper Count estimate in the stock assessment, will that be viewed as the exploitable stock is three-times larger than we thought, and so will the expectation be that the catch rates should be increased by a factor of three? These kinds of things -- I am not quite sure how they will be dealt with, and so that's part of my rationale for wanting to know more as to how we would use this information.

CHAIRMAN POWERS: This is why I went into my rant about parsing up this meeting this way. Like I said, I feel the review has been excellent and has provided information, and we understand, as much as we are going to understand at this point about what the results are, and then the next question is how do we integrate information into the catch advice?

From the council's perspective, it's that integration into the catch advice that would be considered, quote, unquote, best available information. Is this the motion? Okay.

MR. MARESKA: I just hope it makes it clear that it would be used in 74, and, yes, the SSC agrees that we should review the interim catch advice derived from the Great Red Snapper Count.

CHAIRMAN POWERS: Harry, do you still want to second this?

MR. BLANCHET: Yes.

CHAIRMAN POWERS: Okay. Thank you. Again, you have several "and" in there, and I want to make sure which "and" is referring to what, which phrase is referring to what. My concern, again, is the incorporating modifications to the variance. At this point, any modifications to the variance is not going to be included in interim catch advice that we're going to do for the next few days, and as long as that's understood.

MR. MARESKA: Yes, that's understood, and I probably need a comma after "investigative team".

CHAIRMAN POWERS: Okay. Is there any more discussion on this motion? Of course, these estimates will evolve, and the research will evolve, as SEDAR 74 goes through the process. Doug Gregory.

MR. GREGORY: I have a question for John. The way this reads, literally, is that the population estimates that are in this draft report are suitable as-is, without review or modification. That's the only part that gives me heartburn. If the motion read something like the SSC finds the Great Red Snapper Count study suitable for integration into the SEDAR 74, incorporating modifications to population estimates and variance, blah, blah, blah, blah, and so I just want to be clear what you're really saying here, because, literally, that gives me heartburn. Thank you.

MR. MARESKA: I am open to any wordsmithing corrections you

would like to put in there, but the corrections by the investigative team may be a lot of the issues that were raised, where people had concern, and I think the investigative team said they were going to go back and look at the data, or re-run some things, and so, if those things are done, then the resulting report that comes out in June hopefully would incorporate that, as well as the variance estimates around the population estimates. I am trying to general, and I'm not trying to be specific, Doug, and so please incorporate any suggestions you have in there.

MR. GREGORY: Well, let me try. Don't change the original motion, and let's just go beneath it and start. **The SSC finds the GRSC study suitable for integration into SEDAR 74, incorporating modifications to population estimates and variance.** That's the only change I would make, if you want to highlight what we inserted, and that might make it easier. Any objections to that?

MR. MARESKA: I do not. Harry?

MR. BLANCHET: I'm good with that.

CHAIRMAN POWERS: All right. Is there any other discussion in there?

DR. ANDERSON: Don't we need to say, "if it incorporates", or "when incorporation" of those things? It's conditional.

CHAIRMAN POWERS: Where did you want to --

DR. ANDERSON: I would say if it incorporates the modifications to population, if or when, as recommended. You can take out the "as", I guess. I am missing something, please tell me.

CHAIRMAN POWERS: Okay. All right. Again, I would really like -- To me, this is something that essentially we've already agreed to, in general framework, from people's reaction to this, and the SEDAR 74 will evaluate this information, with or without this motion. Well, we'll go on there. Benny Gallaway.

DR. GALLAWAY: Let me just read this a little more. Go to someone else, and I will get back to you.

CHAIRMAN POWERS: Okay. Kai Lorenzen.

DR. LORENZEN: I mean, I find it too vague, in terms of that "incorporating modifications", because we haven't actually

specified those modifications, and we have merely given suggestions as to how certain things could be improved, and I also don't think that -- I mean, certainly, as you pointed out, we don't actually have to address SEDAR 74, because that will be done, in any case, separately, and I don't think that we need to judge suitability at this stage, and I actually have sent a --

I would suggest a substitute motion that actually simply states our overall sort of review of these estimates, which, if that passes, I think it would address that part of the TOR, but it wouldn't, at this stage, make a judgment on incorporation. Do you want me to read it?

CHAIRMAN POWERS: Yes, please.

DR. LORENZEN: The review team (external consultants and SSC) considers that the Great Red Snapper Count provides a representative estimate of abundance for the eastern Gulf and likely a lower bound for the western Gulf. The review team also considers that the true uncertainty in both estimates is substantially larger than implied by the 11 percent CV stated in the report and that the estimate for uncharacterized bottom is particularly uncertain.

MR. GILL: Second, Mr. Chairman.

CHAIRMAN POWERS: All right. This is a substitute motion, and so the discussion is related to that, relative to the first one. Dave Chagaris.

DR. CHAGARIS: Thank you. My comment, I guess, was originally on the previous motion, but I think it's still relevant here, and my concern was that what is implied by the last statement of the previous motion about being suitable for consideration into interim catch advice. My concern was that that sort of commits us to accepting some interim catch advice without actually seeing how it's used, and so I do like this substitute motion better, because I think it addresses our consensus on the actual study itself.

CHAIRMAN POWERS: Thank you. Luiz.

DR. BARBIERI: Thank you, Mr. Chairman. I agree with what Dave Chagaris just said. I'm on the same page there with him on this, but I still have significant concerns, and I'm going to be brutally honest here, but I do feel that what came out of -- The estimates that came out of the uncharacterized bottom have -- The uncertainties there have exceeded what makes it suitable to

be considered for management advice.

It doesn't mean that all of that work was wasted and that knowledge wasn't gained and progress wasn't made or that that work is not valid scientific work, but it's simply the fact that we review and assess information and analysis that is used for management advice according to what we consider tolerable levels of uncertainty.

I feel that, in that case, the small sample sizes, the very, very low sampling fraction, that, for all valid reasons, have prevented this study from being completed in the way that you have enough sampling to characterize those habitats, and, to me, that generates concerns.

We as an SSC, in the past, have rejected full stock assessments, and we have rejected projections from the Science Center, not because we don't believe in our Science Center data or analysts, or because the analysis was just plain wrong, but it's because the uncertainty in the estimates we felt exceeded what we felt was necessary for it to be suitable for management advice, and that's my level of discomfort with the estimates that are coming out of the uncharacterized bottom, because, for all sorts of practical reasons, sample sizes had to be very small.

The area coverage of that sampling program had to be kept very small, and, in my view, what we have as uncertainty estimates coming out of that stratum are way, way below what they should really be. Thank you.

CHAIRMAN POWERS: Thank you. I will repeat my mantra. Essentially, what the presentations being made by the Center this afternoon is, if you believe the population estimate is this, then these are the ramifications. If you believe it's that, these are the ramifications. If you believe it's a third thing, these are the ramifications, and one of those is the overall GRSC estimate.

I would much -- I would have much preferred that we agreed to some sort of substitute motion like this, that this review has been extremely, extremely helpful to the scientific process, and then move on to how we're going to integrate it into the catch, but, again, I am ranting. Harry Blanchet.

MR. BLANCHET: Actually, I'm good with that substitute motion that you just provided, the substitute substitute. My concern with the substitute that's on the board is the western Gulf. The estimates for the western Gulf are driven very much by the

estimates for the unclassified bottom, and, since there's a limited number of samples in the western Gulf, and a lot of that area was imputed, rather than sampled, I am not sure that we can say that it is likely a lower bound for the abundance in the western Gulf.

I don't know that we really have a good grip on that very large number that was estimated for the western Gulf in the uncharacterized bottom. I think there's a lot out there, but I don't know that that is a lower bound.

CHAIRMAN POWERS: Thank you, and, again, because of the detail -- Well, Lee Anderson asked for the floor.

DR. ANDERSON: Thank you, Mr. Chairman. I have to say that my points have been raised earlier, and I think I approve of the substitute motion better than the other one, and now I will be quiet.

CHAIRMAN POWERS: I am skipping over Benny, because you said you wanted to -- I'm not sure if you still want to talk.

DR. GALLAWAY: I was just wanting to say that I certainly agree with the comments that Harry just made regarding the lower bound for the western Gulf, both for the reasons that he stated and, further, the hydroacoustic studies performed around the large structures typical of the western Gulf, and those have been independently verified, or validated, by mark-recapture estimates, which yield very similar estimates for the same habitat, and so I think there's -- The other comments about the uncertainty, he captured what I was ineloquently trying to say earlier. Thank you.

CHAIRMAN POWERS: Thank you. Kai Lorenzen.

DR. LORENZEN: I just wanted to point out, again, that, obviously, this motion does not either oblige us to use that information or prevent us from it, and so it's really just trying to capture the sentiment on the information, and, obviously, I would be happy to accept friendly amendments, if we can improve the characterization of what's going on in the western Gulf. Thank you.

CHAIRMAN POWERS: Thank you. Jim Nance, with friendly revisions, hopefully.

DR. NANCE: I am just going to say, and thank you, Dr. Powers, and I like this substitute motion. The only question I have is,

is this an SSC motion, or are the external consultants also part of this motion?

CHAIRMAN POWERS: I have no idea.

DR. NANCE: The way it reads -- So it needs to be changed, I think, to the SSC considers.

MR. RINDONE: No, do not change it.

DR. NANCE: Don't change it?

MR. RINDONE: No.

DR. NANCE: Okay.

MR. RINDONE: The review team is the combination of the non-PI SSC members and the external consultants. That is who would be making the collective determination.

DR. NANCE: So they're going to vote on this also, Ryan?

MR. RINDONE: They would be included in that, yes.

DR. NANCE: Okay. Thank you.

CHAIRMAN POWERS: They have the opportunity to abstain, of course. All right. John Mareska.

MR. MARESKA: I've got a little bit of heartburn with the estimate of abundance for the eastern Gulf and a likely lower bound for the western Gulf, and that seems kind of contradictory to the report, which kind of indicated that everything was an underestimate, and so if Kai would be agreeable to just making it a representative estimate of abundance for the Gulf of Mexico, and strike "for the eastern Gulf and a likely lower bound for the western Gulf", and I think that may address some of the concerns that Harry and others had.

DR. LORENZEN: If I can respond to that, I think my concern is that, based on the reviewers' report and the calibration issue, I would not be comfortable saying that it's representative for the Gulf, and I am trying to address that by dividing it out like this. Thanks.

CHAIRMAN POWERS: All right. Again, we're getting caught up in the details here, where I don't think it's really important to what we're really trying to do, but Bob Gill.

MR. GILL: Thank you, Mr. Chairman, and so a suggestion relative to the --

MR. RINDONE: Bob, you fell off a cliff there.

MR. GILL: Sorry. I'm not sure what happened there. **My suggestion that I would like to proffer, as a friendly, if Kai and I guess me agree with, is to replace the "likely lower bound" with "a highly uncertain estimate".**

DR. LORENZEN: I would be okay with that.

MR. GILL: As the seconder, so am I.

CHAIRMAN POWERS: Okay. Benny.

DR. GALLAWAY: Could you insert a "however", after "Gulf" and between "the review team", where it reads: "However, the review team also considers that", blah, blah, blah.

CHAIRMAN POWERS: Okay. Thank you. This is -- Are there any other comments? If not, what I will ask is if there's any objections, and then I will ask if there is an abstentions. Are there any objections to this motion? No objections. Are there any abstentions? The hands-up, do you wish to talk, or are you saying that you're abstentions?

DR. POWERS: I am just saying that I abstain.

CHAIRMAN POWERS: Well, yes, and the PIs, I guess as a foregone conclusion, you're abstaining.

MR. MATENS: Dr. Powers, I would really like to see a vote on this.

CHAIRMAN POWERS: I thought we just did that.

MR. RINDONE: That's what this is, Dr. Powers. This is a vote.

CHAIRMAN POWERS: What we have now is no objections of the non-PI members, right now, we have three abstentions, Camp, Judd Curtis, and Doug Gregory.

MR. GREGORY: I'm not abstaining, but I just want to point out that Mary Christman is not online, and so --

DR. CHRISTMAN: Yes, I am.

MR. GREGORY: Am I wrong?

DR. CHRISTMAN: I'm online.

MR. GREGORY: Okay. I'm sorry.

DR. CHRISTMAN: You just didn't see my name.

CHAIRMAN POWERS: All right.

MR. MATENS: I'm sorry, Dr. Powers, but I really object. I really wanted to see a vote on this.

CHAIRMAN POWERS: I will defer to Ryan, but I considered this to be a vote.

MR. RINDONE: Dr. Powers, this is a vote, but it would be helpful though if those that are abstaining could say so. I think, so far, we have Sean and Judd as saying that they abstain, just since the other two -- If there are no objections, then those persons would be in favor, and, if there are abstentions, then those persons should be identified as such.

DR. LORENZEN: I thought we had established that the PIs abstained, by definition, and so there would be -- Either we have that as a statement upfront or we have to list all the PIs as abstaining, I guess.

MR. RINDONE: There were only five of them, and we've heard from two of them so far, and I believe the rest of them to also be on right now. I think Dr. Leaf has his hand up as well.

DR. LEAF: Only to the fact that -- Kai just stated it quite well, and so a blanket abstention or individual recognition, either one.

MR. RINDONE: The only other two people to whom that statement prior applied to were Will and Steven Scyphers.

CHAIRMAN POWERS: Okay. **My understanding is that the motion has passed.** Do others wish to be recognized? Camp, do you wish to be recognized?

MR. MATENS: Dr. Powers, thank you. Yes, I do. I really would have liked to see a vote on this, and my vote would have been no. However, since we've gone as far as we've gone, I wish to be put in the abstain bracket.

CHAIRMAN POWERS: Well, no. If you said you voted no, then --

MR. MATENS: No, I didn't say that. I never got a chance to vote no.

CHAIRMAN POWERS: I asked if there were any objections to this motion.

MR. MATENS: Well, I'm not asking to move this back, and maybe I just wasn't paying attention. However, my vote would have been no, but, since we are where we are, I want to be in the abstain category. Is that okay?

CHAIRMAN POWERS: I want you to vote however you want to vote, either abstention, no, or yes.

MR. MATENS: In that case, sir, I vote no.

CHAIRMAN POWERS: Okay. Thank you.

DR. LORENZEN: We should record that, that it carried with one objection.

CHAIRMAN POWERS: Yes. All right. Thank you. Ryan, did you have anything to interject, procedurally, at this point?

MR. RINDONE: Just that the vote had actually already concluded, but, if the vote is going to be recorded as it is, then we'll record it carried as twenty-one-to-one with five abstentions, and so this vote is concluded.

At this point, also, this would conclude the peer review portion of the Great Red Snapper Count, insofar as that relates to the participation of the independent consultants. Their participation, formally, at this point, would be over. If they feel like sticking around, they are more than welcome to do so. I believe that Dr. Frazer had something that he wanted to add.

DR. FRAZER: Thanks, Ryan. I just wanted to extend a thank you to all of the people that participated in this discussion, including all the members of the Great Red Snapper Count, the investigators there, the three reviewers, our independent consultants, as well as all of the members of the SSC.

It was a very, very important discussion, and I appreciate the constructive comments that were provided, and so, as was pointed out, this work will certainly be of value in the longer term,

but it also has utility in the short term, and so, again, I appreciate people recognizing that, and I'm sure that all of the stakeholders that are involved as well, and are participating and listening in, appreciate your efforts as well, and so I look forward to the next part of the meeting, and I hope you all enjoy your lunch.

CHAIRMAN POWERS: Thank you very much. Will Patterson wanted the floor, recognizing that he's holding up lunch.

DR. PATTERSON: Sorry to hold up lunch. I just think it's important that, in the report for this, the SSC is specific about what they mean by eastern and western Gulf, because we didn't produce eastern and western Gulf estimates in the report. We have state-specific estimates, and so which states do they mean by east versus west I think would be helpful for folks that are trying to interpret the intent of the SSC here.

CHAIRMAN POWERS: Thank you. All right. We are scheduled for an hour for lunch, and I would like to keep it at an hour, at this stage, and so we'll come back at 1:15 Eastern Daylight time, in which case we will initiate the other SSC meeting and begin with that agenda, and so thank you.

(Whereupon, the meeting recessed for lunch on April 1, 2021.)

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