Pilot Test of a Dual Frame Two-Phase Mail Survey of Anglers in North Carolina

Final Report

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Introduction and Study Objectives

The National Marine Fisheries Service of the National Oceanic and Atmospheric Administration (NOAA Fisheries) administers several ongoing data collection efforts designed to estimate saltwater fishing participation (number of people who went marine recreational fishing at least once within the calendar year), fishing effort (number of angler trips), and catch (numbers of finfish caught, harvested, and released) in the U.S. The Marine Recreational Fisheries Statistics Survey (MRFSS) is a nationwide program with two independent components, a Coastal Household Telephone Survey (CHTS) to assess fishing effort, and an access-point intercept survey to assess catch per unit effort. Data from the two surveys are combined to estimate total fishing effort, participation, and catch by species.

In a review of the MRFSS conducted by the National Research Council (NRC, 2006), panel members suggested major revisions of the methods used in data collection. In particular, the CHTS, a random digit dial (RDD) survey of households, was criticized because of its under-coverage and inefficiency. The CHTS design suffers from inefficiency, due to the low rate of saltwater angler participation among the general population, as well as potential coverage bias, due to its sampling only coastal county residences and landline-based telephone numbers. The NRC report endorsed mandatory registration of all saltwater anglers. In the absence of a complete registry, the NRC recommended dual-frame procedures, and suggested sampling from incomplete lists of saltwater anglers (e.g. state saltwater license databases) and state resident or household frames (e.g. RDD frames or address-based sample frames).
The three major sources of under-coverage in the current CHTS are (1) households that do not reside in the coastal counties, (2) coastal county households without landline telephone service

(Blumberg and Luke (2010) estimate this at 26.5% of U.S. households at the end of 2009), and (3) coastal county households with landline numbers that are excluded in standard RDD list-assisted samples (Fahimi, Kulp, and Brick (2008) estimate about 20% of all landline telephone households are not in the standard RDD frame). The current survey approach accounts for under-coverage of the CHTS sample frame by adjusting estimates upward using expansion factors derived through the independent access-point intercept survey. The NRC (2006) indicated that these expansion factors are susceptible to a variety of errors.

Besides its potential coverage error, the CHTS is inefficient, as a small percentage of households participate in marine recreational fishing. As noted by the NRC report:

> Random digit dialing, even limited to coastal county residences, is not the most efficient way to gather angler effort information. In urban areas, less than 1 in 20 of the telephone intercepts reaches an angler. Improving the process whereby anglers are identified and contacted would not only improve the quality of the estimates but should also reduce costs. Remedies exist for other inefficiencies as well. For example, under the current sampling regime, identifying an angler costs more than the taking of information once the angler has been identified. (NRC, 2006, p. 30)

To compensate for the shortcomings of the CHTS, NOAA Fisheries has developed a dual-frame telephone survey approach that integrates the CHTS with surveys that sample from lists of licensed anglers. These angler license directory surveys (ALDS) are more efficient than the CHTS in terms of identifying saltwater anglers, but are susceptible to coverage error since state licensing programs exempt anglers in certain categories (for example minors or disabled) from licensing requirements

The dual-frame telephone survey approach provides better coverage than either the CHTS or ALDS alone. However, the methodology is limited by the quantity and quality of telephone numbers included in ALDS sample frames. During the most recent waves of fielding, nearly 25% of cases in the study area resulted in non-contacts due to “bad telephone numbers” (Not in Service, Business Phone, Wrong Number or Missing Number). In addition, determination of the overlap of the frames (households that could be selected from both sample frames) is difficult in telephone surveys due to the occurrence of bad numbers and cell phone numbers on the license frames. Knowing whether a unit is in the overlap is essential for calculating selection probabilities of sampled units. The dual-frame telephone survey attempts to overcome this shortcoming by asking respondents questions aimed at determining whether they are in the overlap. An inability or unwillingness to answer these questions accurately is a potential source of measurement error that could result in biased estimates. A final concern with the dual-frame telephone survey approach is the decline in response rates to telephone surveys in general, and the CHTS in particular. Since 2003, CHTS response rates in NC have decreased from 39% to 25%[1]. Response rates for the ALDS have not been much better, hovering around 30% over the past two years.

Given these concerns, an alternative dual-frame survey using mail rather than telephone was proposed. The pilot study of this alternative is the focus of this paper. Mail surveys have several potential benefits over telephone surveys in a dual-frame approach, including, 1) cost reductions, 2) greater coverage, and 3) an increased likelihood of identifying overlapping frame units through address matching. Recent evidence also suggests that mail self-administered surveys have the potential to improve response rates over comparable telephone surveys (e.g., Link, et al, 2008).

[1] During the same time period, response rates for the CHTS sample for all states along the Atlantic and Gulf coasts have decreased from 31% to 18%.

Recent Use of Address Based Samples

Increased interest in the use of address-based sampling (ABS) in the U.S. for surveys of the general population has been spurred by decline in response rates for telephone surveys (a trend that began in the 1980s) coupled with the increasing cost of attempts to convert non-respondents. In addition, an increasing percentage of households that are “cell phone only” --and thus excluded from standard RDD samples -- have resulted in a downward trend in coverage for standard RDD telephone surveys. At the same time, improvements to databases of U.S. household addresses have facilitated their use for sampling households. A number of studies have examined the feasibility of using address-based sampling in place of listing households in sampled segments prior to sampling for in-person surveys (Iannacchione, Staab, and Redden, 2003; Kennel and Li, 2009). These studies have generally concluded that ABS is a viable alternative for sampling households in the U.S.

Even more recently, several surveys have explored using the ABS to sample households for both mail and telephone data collections. One approach has been to replace RDD samples with an ABS sample, recruit households by telephone (for those that can be matched using commercial lists) or mail, and then conduct data collection in the mode used regularly in the survey. This approach has been used in the U.S. Nielsen TV Ratings Diary Survey (Link, et al, 2009) and by Knowledge Networks (DiSogra, Callegaro, and Hendarwan, 2009). According to internal analyses conducted by Nielson, the ABS method improved coverage from 70% using an RDD design to 98% with the ABS design, and representation of younger adults increased from a penetration rate of 8.8% to 13.5%. The change to the mixed mode approach did not result in any change in the overall response rate to the extended diary survey.

Another approach uses the ABS frame with an all mail mode of data collection. Link et al. (2008) used this method as an alternative to the traditional RDD method for the Behavioral Risk Factor Surveillance Survey. The National Household Education Survey (Montaquila et al. 2010) and the National Survey of Veterans (Han et al. 2010) use a two-phase mail survey to interview subgroups of the population, as does the current study, which follows a first-phase mail screener to identify eligible households by a second-phase mail survey to interview a sample of those that are eligible.

Study Objectives

The pilot test is intended to examine the feasibility of conducting an angler effort survey incorporating an ABS mail approach, with special interest in the dual frame components of the methodology. It uses a mail survey with samples selected from the general household frame (the ABS) and from a license frame. One goal is to assess the response rates that can be achieved using a mail survey for screening and identifying anglers in the general population, and for conducting an extended interview with these anglers. The dual frame nature of the design allows for exploration of potential nonresponse error resulting from households with avid anglers responding at a higher rate than other households.

A second goal is related to the combining of the samples from the two frames to produce efficient estimates. Accuracy of methods to determine if sampled households are on both frames are investigated. The pilot study also provides data about the amount of undercoverage of the CHTS, albeit limited to a small sample in only one state.

Sample and Study Design

Sample Design

The target population for the survey is North Carolina (NC) saltwater anglers, both those living in households in NC as well as those living outside the state. The current CHTS attempts to survey this population by means of an RDD sample of households that live in counties along the coast, while the ALDS attempts to survey this population by means of a telephone survey of licensed saltwater anglers. The address frame used for this pilot is derived from the USPS Delivery Sequence Files (DSF). One of the advantages of using the ABS is the relatively cost efficient sampling from all households in NC, not just coastal county households.[2]

The dual frame approach used in the pilot study samples households that are in the union of the address frame and the license frame, neither of which is limited to coastal counties. The union of the frames consists of three domains: households in the address frame but not in the license frame (S_1), households in the license frame but not the address frame (S_2), and households in both frames (S_{12}). If the address frame were complete, then S_2 would be empty except for licensed anglers who reside outside of NC.

Samples were selected independently from the two frames, and estimates of the total numbers of participants and fishing effort (number of trips) were made for each of the three domains. From the address frame, estimates are made for domains S_1 and S_{12}; from the license frame estimates are made for S_2 and S_{12}. Since both frames estimate the characteristics for the overlap domain (S_{12}), these two are averaged to produce a more precise estimate for S_{12}. The three estimates are then

[2] The CHTS could include non-coastal county households; however, the efficiency of such an RDD design, in which the yield is less than 10% of households with an active angler, results in an extremely cost inefficient design. The use of a mail screening survey offers a cost-efficient means to reach the elusive angler sample in the non-coastal counties.

summed to produce estimates for the total population. In this study, we investigate the similarity of the estimates from the two frames for S_{12}, but do not produce combined estimates.

The Address Frame

A stratified sample was selected from the address frame, with different sampling rates in the strata. Addresses in the coastal counties were in the first stratum, and addresses in the remaining counties were in the second stratum. A total of 900 addresses of the 774,652 on the frame were selected in the coastal stratum, and 900 of the 3,055,903 addresses were sampled in the second stratum. The selected addresses constitute the first-phase sample from the ABS.

The second phase sample included adult anglers (saltwater fished in the previous year) in households that responded to the mail screener. One angler was sampled from each household that reported saltwater fishing by an adult during the previous 12 months. A supplemental sample of anglers was selected by sampling another adult angler in a subset of households that reported saltwater fishing by more than one adult in the previous year.

The License Frame

The license frame, which the state maintains as a part of its administrative records system, is a list of individuals who were licensed to participate in saltwater fishing in NC during the reference period (November – December, 2009). A database containing 551,060 registered anglers was provided by NC's Division of Marine Fisheries. While anyone on this file was licensed for saltwater fishing in NC, some of them may never have fished but held licenses for other types of activities that also bestowed the license for fishing. The types of licenses are discussed later.

Before samples were selected from the license file, it was processed to make it suitable as a sampling frame. The following steps were followed:

- Duplicates (records with the same core data: name, date of birth, and mailing address) were deleted.
- Records without core data were deleted.
- Persons under the age of 18 were deleted.
- Addresses were "normalized" to be in the standard formats used by the postal service.
- Records were stratified by county (coastal, non-costal, or out-of-state strata), and unique household identifiers were assigned to anglers with a common mailing address or telephone number

Frame processing resulted in a total of 456,474 unique angler records, distributed among coastal (184,593), non-coastal (239,450) and non-resident (32,431) strata.

The file was sorted by address and a systematic sample of 450 anglers was selected in each stratum. The ordering was done to minimize the possibility of including unidentified duplicated household listings. As in the ABS, a supplemental sample of anglers was selected. A second angler was selected in every sampled household identified as having more than one licensed angler.

Data Collection Procedures

A screening survey was mailed to all 1800 ABS sample addresses in the fall of 2009[3]. Consistent with the methods suggested in Dillman, Smyth, and Christian (2008), the household was mailed an instrument that included a cover letter and a $1 cash incentive. The household was asked to complete the questionnaire and mail it back in the envelope provided.

Mailing of the screener was split into two batches, with 900 addresses in each batch. The first batch was mailed November 10, 2009 and the second on November 20, 2009. The batches were mailed at different times to examine the effect of delay between the screener and the angler interview in the two phase mail survey. This is an issue that only arises in two phase mail samples and is discussed later in the analysis. Sample units in both batches were exposed to the same treatment: (1) an initial mailing of the screener questionnaire; (2) a reminder postcard mailed 1 week after the initial mailing; and (3) a second mailing of the screener questionnaire to non-respondents two weeks after the mailing of the postcard, accompanied by a non-response conversion letter.

[3] All data collection instruments are included in the attached methodology report.

Randomly selected anglers from each fishing household identified in the ABS screener as well as anglers sampled from the license frame were mailed an Angler Survey, beginning January 4, 2010. Similar to the screening data collection protocol, the Angler Survey data collection consisted of (1) the original mailing of the survey instrument (different letters for the ABS and License frame sample units), including a $1 incentive for participation; (2) a reminder postcard (one week later); (3) a second mailing of the survey two weeks following the postcard reminder (that included a modified cover letter, but no additional incentive); (4) and a final questionnaire, delivered by Federal Express 2-day delivery.

Appendix A shows the sample disposition for the ABS sample screener (Table A-1), the ABS angler survey (Table A-2), and the License sample (Table A-3) by stratum. A detailed report of the methodology used in the study, including detailed information concerning the de-duplication of the sampling frames, is included as an attachment to this report.

Findings

Matching and Domain Identification

A critical issue in the development of estimates from dual frame designs is the accurate identification of elements in each frame as well as those units which appear in both frames (overlap). Often, the identification of overlap between frames relies on data reported by the respondents. This approach is currently being explored in tests estimating fishing effort from telephone dual frame surveys. In the case of the present study, we were able to identify the overlap via matching of ABS addresses to addresses in the license frame. Both methods of identifying overlapping units are subject to errors that affect the quality of the dual frame estimators. We begin by looking at the matching of addresses and then discuss the accuracy of the self-identification of domain membership by respondents.

One way to assess the quality of the matching as a method of identifying overlap is to compare the estimate of the total number of licensed anglers from the ABS sample, both overall and by stratum, to the known number of licensed anglers in NC. A complication is that the matching is done by address, while the units of the license frame are the individuals holding licenses. To compare the number of ABS sampled addresses that are matched to the number from the license frame, we first convert the person-level license frame size to a household level size. To do this, we estimated the average number of adult licensed anglers per household from the license frame by stratum (in the coastal stratum the average was 1.19 and in the non-coastal stratum it was 1.16). The number of anglers in the stratum was divided by this average to estimate the number of angler households in each stratum.

Table 1 shows the estimated number of households with licensed anglers in each stratum for the first phase ABS sample, along with the NC license frame counts, where the license frame estimate is adjusted to be at the household level. The table shows that overall the matching was very close to unity, with the ABS sample estimate of licensed addresses being just 1.06 times the adjusted number from the license frame. This suggests the approach is effective (the 1.06 estimate is not significantly different from unity). The ratio in the coastal stratum is estimated to be 0.88 and is statistically different from unity, while the non-coastal stratum estimate is 1.19. We expected the matching error to be primarily one-directional, with some addresses not matching due to errors in the license frame and vagaries in matching. However, the tabulation suggests that the matching is either of very high quality or the matching error goes in both directions. This result supports the initial rationale of matching addresses and is consistent with the premise that the dual frame domain membership is accurately obtained from this procedure.

Table 1. Estimated number of addresses in the overlap from the ABS first phase sample and from the license frame

Stratum	ABS sample	License frame	Ratio of ABS to license	95% CI lower limit	95% CI upper limit
Total	381,326	360,610	1.06	0.90	1.21
Coastal	136,854	154,975	0.88	0.79	0.98
Non-coastal	244,472	205,635	1.19	0.93	1.45

Response Rates

We begin the analysis by examining weighted response rates[4] for the two frames and across the strata. The response rates are shown in Table 2. The study achieved an overall response to the screener of 45.6% and an extended interview response rate of 72.5% for an overall response rate for the ABS sample of 33.1%. This rate exceeds the comparable CHTS telephone response rate for Wave 6 in NC of 25.4%. Among those sampled from the license frame, we achieved a response rate of 58.2%, also exceeding the ALDS response rate for NC during the same wave of 30.1%[5].

[4] Weighted by the base weight and using AAPOR response rate RR3 (AAPOR, 2009).
[5] Note that the ABS mail survey and the CHTS are limited to NC residents whereas the license mail survey and the ALDS include anglers from out of state who have a NC saltwater fishing license.

These response rates are encouraging. They suggest that the angler population can be reached via a self-administered mail survey, that coverage of the population is possible via an ABS with a self-administered mail questionnaire, and that response rates may improve, especially for the license frame, over those of a telephone survey.

Table 2. Response Rates by Frame and Stratum. Geo-coding, Batch, License Match and Number of Anglers Sampled (all response rates weighted by base weight)

<table>
<tr><th></th><th colspan="3">ABS Frame</th><th rowspan="2">License Frame</th></tr>
<tr><th></th><th>Screener</th><th>Angler Survey</th><th>Overall</th></tr>
<tr><td>Overall</td><td>45.6%</td><td>72.5%</td><td>33.1%</td><td>58.2%</td></tr>
<tr><td>Stratum</td><td></td><td></td><td></td><td></td></tr>
<tr><td>Coastal</td><td>48.4%</td><td>70.1%</td><td>34.0%</td><td>57.3%</td></tr>
<tr><td>Non-Coastal</td><td>44.9%</td><td>73.9%</td><td>33.2%</td><td>57.6%</td></tr>
<tr><td>Out of State</td><td>NA</td><td>NA</td><td>NA</td><td>67.7%</td></tr>
<tr><td>Geo-coding</td><td></td><td></td><td></td><td></td></tr>
<tr><td>Borders Ocean</td><td>48.9%</td><td>73.8%</td><td>36.0%</td><td>53.7%</td></tr>
<tr><td>Coastal, not border</td><td>48.1%</td><td>67.0%</td><td>32.3%</td><td>59.9%</td></tr>
<tr><td>Other</td><td>44.9%</td><td>73.9%</td><td>33.2%</td><td>58.8%</td></tr>
<tr><td>Batch</td><td></td><td></td><td></td><td></td></tr>
<tr><td>First</td><td>46.4%</td><td>75.1%</td><td>34.9%</td><td>NA</td></tr>
<tr><td>Second</td><td>44.8%</td><td>70.1%</td><td>31.4%</td><td>NA</td></tr>
<tr><td>License Match</td><td></td><td></td><td></td><td></td></tr>
<tr><td>Match</td><td>65.5%</td><td>70.1%</td><td>45.9%</td><td>NA</td></tr>
<tr><td>No Match</td><td>43.2%</td><td>73.4%</td><td>31.7%</td><td>NA</td></tr>
<tr><td>Number of Sampled Anglers</td><td></td><td></td><td></td><td></td></tr>
<tr><td>1 Angler HH</td><td>NA</td><td>68.4%</td><td rowspan="2">31.2%</td><td>66.5%</td></tr>
<tr><td>1 Angler/2+ HH</td><td>NA</td><td>74.8%</td><td>NA</td></tr>
<tr><td>2 Anglers/2+HH</td><td>NA</td><td>74.3%</td><td>34.0%</td><td>56.5%</td></tr>
</table>

Response rates from the mail surveys did not vary by stratum for either frame, with the exception of higher rates among anglers from out of state within the license frame. We also examined response rates by a three category geo-code, examining those who live in a county that directly borders the ocean, those in the coastal stratum, but not directly adjacent to the ocean, and all others. These geo-code categories also showed no significant differences in response rates.

As noted, the screening interview for the ABS sample was conducted in two batches, with the initial batch mailed about 10 days before the second. The second phase mailing for both batches was done at the same time, so the first batch respondents had a longer time period between the first and second phase mailings. As expected, the first phase response rates were not significantly different between the two batches (46.4% and 44.8%, respectively). Differences between the second phase response rates were also not significant, perhaps because of the small sample sizes. The direction of the difference, with a higher angler survey response rate for Batch 1 as compared to Batch 2 (75.1% and 70.1%) suggests that a longer lag time between the screening interview and the extended interview may be beneficial with respect to increasing the second phase response rate. This finding warrants further study as we explore the use of ABS for two phase designs.

In households with more than one adult angler, we sometimes sampled two anglers for participation in the second phase angler survey[6]. There was no difference in response rates among anglers in the ABS second phase sample as a function of number of anglers sampled in the household. However, when more than one angler was sampled from a household in the license frame, the response rate was 10 percentage points lower than for the anglers who were the sole recipients of the angler survey request (56.5% vs. 66.5%).

[6]For the ABS we sometimes sampled one and other times sampled two anglers, while in the license frame we always sampled two anglers when there were two present.

Avidity Bias

With surveys that focus on a specific segment of the population, there is always concern about differential nonresponse related to participation in the behavior of interest to the study. Previous studies (e.g., Thomson, 1991; Fisher, 1996; Connelly, Brown, and Knuth, 2000) have demonstrated avidity bias in angler surveys. In this context, avidity bias would result from a higher propensity to respond by avid anglers when surveyed about fishing. To examine this, the ABS sample units were matched to the NC license frame to determine whether those with NC saltwater fishing licenses were more likely to participate in the survey than those without a license. Overall, 12.8% of the ABS sample was matched to the license file, with a higher match rate in coastal counties (17.7%) than non-coastal counties (8.0%). The quality of the matching was very good, as discussed previously.

Table 2 indicates that the screener response rate was over 20 percentage points higher for households that were matched to the license frame than those that were not (65.5% vs. 43.2%). However, the second phase response rate of adults who said they had fished in the last year did not differ significantly by whether they matched to the license frame. Because of the large first phase difference in response rates, the overall response rate did show a significant difference, 45.9% vs. 31.7%.

This is an important finding with respect to the feasibility, as well as the benefits, of using a residential address frame to estimate the total number of anglers and the total number of recreational fishing trips. If the respondents to the first phase sample are adjusted to account for nonresponse without accounting for the avidity bias, then the effect will be to overestimate the number of anglers and angler trips because the avid anglers are over-represented in the sample. Because we were able to successfully match the ABS sample to the license frame, we were able to adjust the first-phase nonresponse weights for the ABS sample to account for differential nonresponse between avid (households with at least one licensed) and non-avid (households with no licensed anglers) households. As described below, this greatly reduces the potential for avidity bias in estimates of the total number of anglers and the total number of fishing trips. However, the adjustment does not account for avid anglers who could not be matched to the license frame, or for differential avidity of licensed anglers. Research on methods to avoid this potential source of nonresponse bias is needed, for example, by examining the effect of screening focusing on a broader range of topics than just angling.

It should be noted that avidity bias may also be present in other surveys that sample from residential household frames, including the Coastal Household Telephone Survey. A research project is currently underway that will attempt to match CHTS sample units to license frames in NC and LA by telephone number and address. Successfully matching the CHTS sample to license frames will help to identify and quantify avidity bias in the CHTS, as well as allow survey managers to develop adjustments to nonresponse weights that will account for avidity bias in the survey.

We also wanted to explore differential response rates among those sampled from the license frame and those sampled from the ABS frame who matched to the license frame to determine if different types of licensure were associated with differential response rates. There are numerous saltwater license types in NC and they may be informative about avidity bias in the license frame. Some licenses are for salt water fishing only, while others are combination licenses that also permit the holder to freshwater fish or hunt. Some types provide lifetime licensure, while others are purchased annually.

Table 3 presents the response rates for the ABS screener and the License frame angler survey by strata and license type. No clear pattern overall emerges. We do see that among the license frame, the combination license holders tend to respond at a lower rate than those who hold other types of licenses. Within each of the strata of the license frame, those who held a 10-day license responded at a higher rate than other license holders. Higher response rates among these respondents with highly targeted licenses are consistent with the hypothesis that anglers who have fished recently have higher propensity to respond to the survey. These findings are evidence that not all anglers on the license frame are equally likely to respond to the survey and also have implications for nonresponse bias from the license frame. With respect to angler response rates among the ABS sample that linked to the license frame, we also see some variability in response rates by license type. However, among this sample, we see the lowest response rates among those with the 10-day license. Although the findings are mixed with respect to response rates by license type, the findings do suggest that greater reduction of nonresponse bias might be obtained by using information about license type in nonresponse adjustment.

Table3. Screener Response Rates (ABS Frame) and Angler Response Rates (License Frame) by Sample Strata and License Type

	ABS Frame: Matched			License Frame			
	Coastal Strata	Non-Coastal Strata	Overall	Coastal Strata	Non-coastal Strata	Out of State Strata	Overall
License Type							
Combo	68.0%	66.1%	66.7%	55.3%	56.6%	59.0%	56.3%
Saltwater	64.8%	57.3%	61.0%	58.7%	59.5%	69.8%	60.3%
10 day		55.2%	55.2%	100.0%	66.7%	70.4%	70.6%
Annual	68.5%	69.5%	69.0%	52.8%	61.3%	69.0%	57.6%
Lifetime	62.8%	60.6%	61.2%	63.7%	55.2%	60.3%	58.1%
Unmatched	44.1%	43.2%	43.4%				

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Missing Data Rates

In considering a shift away from an interviewer-administered telephone survey to a self-administered mail survey, data quality, specifically missing data rates (associated with incomplete questionnaires or incorrect skip patterns) as well as inconsistent data are a concern. We examined missing data rates for several key variables. We defined missing data rates as either no response or an indication of a "don't know" response. These rates ranged from a low of less than 2% for questions concerning whether or not the respondent had participated in recreational saltwater fishing during the reference period to over 25% for questions concerning valid recreational saltwater fishing license ownership for the reference period.

Respondents were asked two summary questions concerning fishing effort during the wave: (1) whether they gone saltwater recreational fishing in North Carolina during the wave (November 1 – December 31, 2009) and (2) for those who had gone fishing during the reference period, they were asked to simply circle the dates on a calendar indicating that they had fished that day. The later information was then summarized during data processing to produce a total number of trips taken by the respondent. Instructions following the collection of this summary data requested that the respondent complete detailed trip information for the four most recent angling trips taken.

In light of this two-step process for obtaining effort information, a second form of missing data consists of those cases in which the respondent indicates multiple angling trips during the wave (indicated by circling the dates of the trips on a calendar) but then failing to complete the detailed trip information for the four most recent trips taken. We found that for 1.2% of the cases, the two pieces of information were inconsistent.[7] For 11 of the 884 anglers (0.2%) who recorded no information on the calendar, detailed information for 1 or more trips was provided. Conversely, 15 of the 270 anglers (5.6%) who circled at least one date on the calendar provided no information for the detailed trip questions, and 98 of these anglers (36%) detailed fewer than they reported on the calendar. There was a particularly serious omission rate for those anglers reporting many trips. Of the 139 anglers who reported 4 or more trips in the calendar, 71 (51%) provided detailed information for fewer than 4 trips.

Comparisons for Under-covered Populations

One of the criticisms of the current CHTS estimates is the lack of coverage of persons living in non-coastal counties and coastal residents living in households without landlines. In this section

[7] Obviously, we can only examine inconsistencies to a limited extent since avid anglers could indicate a high number of trips (> 4) on the calendar but then only report details for the most recent four trips. However, avid anglers who reported a high number of trips but then failed to complete the detailed sets of questions for the four most recent trips are classified as inconsistent.

we examine the demographic and behavioral characteristics of anglers as a function of geographic location and landline service among the ABS sample members[8].

We begin by looking at the percentage of anglers who fished during the year that would be excluded by each reason. The mail survey estimated that 64.5% of all anglers who fished during the year resided in non-coastal counties and would be excluded from the CHTS. An estimated 21.4% of anglers reported in the mail survey that they did not have a landline in their home. By examining the size of the union of these two domains, we estimated that 69.3% of anglers reported in the ABS would be excluded from the CHTS (i.e., only 30.7% of NC anglers reside in coastal households with landlines). Similarly, the mail survey provides an opportunity to assess the coverage of state license databases as sample frames. The mail survey estimated that 57% of the anglers who fished during wave 6 did not possess a saltwater fishing license. The source of this undercoverage in NC likely includes minors (<16) and anglers who fished on state fishing piers, both of which are exempted from state licensing requirements. Based on the ABS frame, we estimate that the CHTS and ALDS surveys in North Carolina exclude about 35% of anglers and approximately 38% of trips (that is, noncoastal anglers without licenses or coastal anglers without licenses or landline telephones). Clearly, the ABS mail survey has a great deal to offer to improve coverage compared to the current RDD and ALDS designs[9].

Table 4 presents the estimated demographic, angler licensure, and fishing activity characteristics for the subset of the ABS frame who fished in the last year, for NC as a whole and by stratum..

[8] All estimates are weighted to account for the probability of selection and for nonresponse.
[9] We can also examine the percentage of active anglers in the wave who would have been missed in the CHTS. The mail survey estimated that 44.3% of all anglers who fished during the wave resided in non-coastal counties, and 11.4% of the coastal residents who fished during the wave did not have a landline in their home. As a result 52.6% of anglers in the ABS who fished during the wave would be excluded from the CHTS.

Although demographic characteristics of the coastal and non-coastal anglers are similar, the incidence of fishing during the wave for the coastal anglers was 2.5 times the rate of non-coastal anglers (37.4% vs. 14.4%). However, among those who did fish during the wave, we find similar levels of effort; however, small sample sizes for the estimation of effort limits the power to detect differences.

Table 4. Demographic composition, Angler Participation Rates, Licensure, and Average number of days fishing from the ABS sample, by geographic location (standard errors in parentheses)

	ABS Frame: all respondents (n=152)	**Coastal Counties (n=105)**	**Non-Coastal Counties (n=47)**
Gender: Male	76.9% (4.5)	78.7% (4.6)	75.9 (6.5)
Gender: Female	16.4% (3.9)	14.9% (3.7)	17.3% (5.6)
Gender :Missing	6.7% (2.8)	6.4% (2.4)	6.8% (4.1)
Age: 18-44	16.8% (4.3)	15.8% (3.6)	17.3% (6.3)
Age:45 and older	77.2% (4.8)	79.4% (3.8)	75.9% (7.1)
Age :Missing	6.1% (2.8)	4.8% (2.0)	6.8% (4.1)
Anyone in household Salt Water Fishing in 2009? [A]	25.4% (1.9)	40.8% (2.6)	21.4% (2.2)
NC License, past 12 months?	64.2% (5.7)	76.9% (4.6)	57.1% (8.4)
NC License, past 12 months for Saltwater Fishing?	54.7% (5.8)	65.6% (5.0)	48.7% (8.4)
NC Saltwater Fishing License: Valid November, 2009?	38.9% (5.2)	59.4% (5.1)	27.5% (7.4)
Salt Water Fishing During Wave?	24.2% (4.5)	37.4% (5.4)	16.9% (6.2)
Average number of days spent fishing, during wave, per angler[B] **……by boat**	1.66 (0.27)	1.97 (0.26)	1.26 (0.49)
……by shore	1.92 (0.61)	2.66 (0.99)	0.99 (0.48)
……total trips	3.58 (0.73)	4.63 (1.03)	2.26 (0.90)

Note: All estimates limited to those who reported fishing during the 2009, except as noted.

[A] Based on information obtained in the screening interview among all screening respondents; n = 685, 357, 328 for the three columns

[B] Among those anglers who fished during the wave; n = 49, 41, 8 for the three columns

We also compared the demographic and behavioral characteristics between those with and without landlines, once again limited to the ABS sample frame. The results, reported in Table 5, show that 21.4% of those NC residents who saltwater fished in 2009 do not have a landline telephone. Anglers in NC with no landline phones are more likely to be female and younger, as compared to anglers with landline phones. The incidence of fishing during Wave 6 for those with landlines was twice that for those with no landline phones. Across all other measures of angling behavior and licensure, those without landlines are similar to those with landline phones.

The findings from Tables 4 and 5 suggest that the rate of angling among those in non-coastal counties in NC is less than those in coastal counties and that those without landline phones are less likely to have fished during the reference period than those with landline phones. Still, a majority of NC anglers do not reside in coastal county households with landline phones. Once again, small sample sizes limit our ability to draw sharp conclusions about what proportion of fishing effort takes place in households not covered by the current CHTS.

The observed differences in demographic characteristics and fishing incidence between anglers who are and who are not covered by the CHTS do not necessarily indicate that fishing effort estimates derived through the CHTS are biased. The CHTS adjusts for undercoverage by expanding estimates of fishing effort upward by correction factors derived through an access-point angler intercept survey (APAIS) of completed fishing trips. Specifically, intercepted anglers are asked for their state and county of residence, as well as whether or not their household has a landline telephone. CHTS estimates are then expanded by the inverse of the

Table 5. Demographic composition, Angler Participation Rates, Licensure, and Average number of days fishing for ABS frame by landline phone status (standard errors in parentheses)

	ABS Frame: all respondents (n=152)	Landline Phone (n=123)	No Landline Phone (n=27)
Gender: Male	76.9% (4.5)	80.9% (4.5)	65.4% (12.9)
Gender: Female	16.4% (3.9)	10.7% (2.9)	33.4% (12.9)
Gender:Missing	6.7% (2.8)	8.4% (3.6)	1.2% (1.2)
Age: 18-44	16.8% (4.3)	14.4% (4.6)	26.8% (10.74)
Age:45 and older	77.2% (4.8)	78.0% (5.4)	72.0% (10.8)
Age: Missing	6.1% (2.8)	7.6% (3.5)	1.2% (1.2)
Anyone in household Salt Water Fishing in 2009? [A]	25.4% (1.9)	26.3% (2.2)	25.8% (4.1)
NC License, past 12 months?	64.2% (5.7)	61.8% (6.4)	73.9% (13.4)
NC License, past 12 months for Saltwater Fishing?	54.7% (5.8)	50.2% (6.4)	71.2% (13.4)
NC Saltwater Fishing License: Valid November, 2009?	38.9% (5.2)	37.9% (5.8)	41.0% (12.8)
Salt Water Fishing During Wave?	24.2% (4.5)	28.0% (5.5)	13.1% (5.9)
Average number of days spent fishing, during wave, per angler [B] **……by boat**	1.66 (0.27)	1.59 (0.30)	2.19 (0.41)
……by shore	1.92 (0.61)	1.47 (0.36)	5.45 (4.13)
……total trips	3.58 (0.73)	3.06 (0.53)	7.64 (4.32)

Note: All estimates limited to those who reported fishing during the 2009, except as noted.
[A] Based on information obtained in the screening interview among all screening respondents; n = 685, 516, 149 for the three columns

[B] Among those anglers who fished during the wave; n = 49, 42, 7 for the three columns ratio of CHTS-covered trips (trips taken by anglers in coastal households with landlines) to total trips (CHTS-covered trips, as well as trips taken by anglers from non-coastal counties or households without landline phones)[10]. These expansion factors are unbiased provided the sample of angler trips derived from the APAIS is representative of all angler trips. Sampling from the ABS provides an excellent opportunity to test the assumption that APAIS samples are representative. However, sample sizes in the present pilot study were insufficient to support this analysis.

Dual Frame Considerations

The reasons for using a dual frame design are to improve coverage and reduce the cost for achieving more precise estimation of angler effort. The license frame provides a mechanism for identifying the group of interest efficiently because anglers occur in a small fraction of households. But the license frame is incomplete for saltwater recreational anglers, so it must be used together with the general population ABS to control the bias due to noncoverage.

Here, we describe some of the issues that arise in the dual frame system in the presence of nonsampling errors. The special effects of nonsampling errors on dual frame estimates have only recently been discussed in the sampling literature (see Lohr, 2009; Brick et al. forthcoming). In this section we explore the implications of certain nonsampling errors in the pilot study. In our concluding comments, we describe possible changes to the survey design and implementation that

[10] A similar approach is used to expand effort estimates derived from the ALDS; expansion factors are derived from angler-reported information about the possession of a saltwater fishing license. This approach is also potentially susceptible to reporting error based upon an inability or unwillingness to provide accurate information about license status as discussed in this report.

could alleviate some of the biases that they cause. Statistical adjustments are also being investigated, but design modifications that would eliminate or reduce the errors would be preferable.

As noted before, the overlap is the population of anglers residing in NC who have a license (more specifically, are on the license frame with sufficient information to be eligible for sampling). This assumes that all the licensed anglers in the state are in housing units that are on the ABS, a reasonable assumption based on data on coverage of households using the ABS in NC. The non-overlapping component of the ABS frame is the set of anglers residing in NC who did not have a license; the non-overlapping component of the license frame is the set of NC license holders who reside outside of the state. Our analysis begins by concentrating on the overlap component since this is relevant only in dual frame surveys.

In the pilot study, we can identify and partially quantify two sources of nonsampling errors that could bias estimates for the overlap domain. The first is nonresponse, resulting in bias due to differential response rates associated with avidity. Earlier we showed that ABS addresses matched to the license frame responded at a higher rate than those that did not. We also found that response propensity in the license frame sample depended on the type of license in a way that was consistent with avidity differences. We expand on our earlier discussion focusing on the size of avidity bias for estimates from a dual frame estimator.

A second source of bias in the dual frame estimator is error in matching the ABS sample units to the license frame. Matching is required to determine which units in the ABS are in the overlap. As discussed earlier, one of the rationales for using a self-administered mail survey is that address matching to the license frame is less error-prone than telephone number matching.

Effects of Avidity Bias

We first examine evidence about the magnitude of nonresponse bias in estimation of fishing effort in NC. Estimation of effort, defined as the number of trips, requires accurate assessment of the number of active anglers, as well as the number of trips those anglers make. If active anglers respond to the survey at a higher rate than others, or if anglers who respond take more trips than nonrespondents, then the estimate of number of trips would be biased upward. Though samples from both frames could suffer from this source of nonresponse bias, it would be expected to be more severe in the ABS frame because the variability in avidity is likely to be greater there than in the license frame.

Table 6 shows information about avidity bias in the first of those components, estimation of the number of active anglers. The first three rows of the table present independent estimates from the two frames of the number of licensed anglers who fished in the wave for the overlap, overall and by stratum. License status for both frames is based on being on the license frame rather than the response to the interview questions about license status (for the ABS this required the address match to an address on the license frame). To qualify as having fished in the wave, we also required that the angler responded that they fished during the past year (the data were not fully edited so a few cases did not meet this logical requirement).

For the ABS estimate, we produced a nonresponse adjustment by forming weighting classes that included both geographic information (proximity to the ocean) and match status, both of which should account for some avidity bias (these weights were used in previous analyses). Even with this adjustment, the ratio of the ABS estimate to the license sample estimate is about 1.35 overall and in each stratum, indicating the ABS sample estimates more anglers fished in the wave than is estimated from the license sample.

Table 6. Estimated number of licensed anglers in the overlap who fished in the wave by screener nonresponse adjustment method

	ABS sample	License sample	Ratio of ABS to license	95% CI lower limit	95% CI upper limit
ABS first phase matching adjustment					
Total	102,918	75,391	1.37	0.62	2.11
Coastal	58,801	42,571	1.38	0.60	2.16
Non-coastal	44,117	32,820	1.34	-0.04	2.73
ABS first phase geographic adjustment					
Total	135,595	75,391	1.80	0.80	2.80
Coastal	73,877	42,571	1.74	0.73	2.74
Non-coastal	61,717	32,820	1.88	-0.02	3.78
ABS first phase no cells adjustment method					
Total	138,999	75,391	1.84	0.83	2.86
Coastal	78,098	42,571	1.83	0.77	2.90
Non-coastal	60,901	32,820	1.86	-0.02	3.73

To get some idea of the potential magnitude of the avidity bias, the bottom portion of the table shows the same quantities with the ABS estimates computed using different nonresponse weighting classes; the middle three rows of estimates use cells based on geography but not match status and the last three rows uses no weighting classes at all. The ratios of the estimates that use a nonresponse adjustment based only on geography are closer to 1.80, consistent with greater overestimation of anglers when the nonresponse adjustment procedure does not account for avidity as completely. When no weighting classes are used, the ratios are slightly higher still. Because of the small sample sizes, however, the 95% confidence intervals are very wide. Since the license frame estimates are likely to be subject to some avidity bias as well, the table shows bias from differential nonresponse (as a function of fishing activity) is potentially serious.

Table 7 summarizes information about the size of the second component of potential avidity bias, the estimation of average number of trips per active angler. The table shows that ratios of the estimates of average number of trips per angler from the two frames are in the opposite direction from those shown in Table 4 (i.e., the ratios are less than unity rather than greater than unity). We also observed that the weighting cells have little effect on these estimates of average trips. The three sets of rows in the table by screener adjustment method show this result. It appears that active anglers responding in the ABS sample fish either less frequently or about the same as those from the license frame.

For estimating total trips (the product of the number of anglers and their average number of trips), the ABS and license samples are closer than either of the two components because the ratios of the components partially offset each other. A standard dual frame estimation strategy is to average the two estimates for the overlap to produce a more precise estimate of this population. Since the components of the two frames are different, averaging the two estimates could give a biased estimate of the number in the overlap. In theory, the two estimates of the overlap are assumed to both be unbiased, so a question arises about the appropriateness of simply combining the estimates from the two frames given these results. We discuss the dual frame estimators and their biases and variances below.

Table 7. Estimated average number of trips per active angler in the overlap who fished in the wave by screener nonresponse adjustment method (standard errors in parentheses)

	ABS sample n=25	License sample n=117	Ratio of ABS to license	95% CI lower limit	95% CI upper limit
ABS first phase matching adjustment method					
Average shore trips	1.01 (0.31)	2.31 (0.28)	0.44	0.14	0.73
Average boat trips	1.83 (0.44)	2.09 0 (0.30)	0.88	0.38	1.37
Total trips	2.84 (0.59)	4.40 (0.37)	0.65	0.36	0.93
ABS first phase geographic adjustment method					
Average shore trips	0.99 (0.31)	2.31 (0.28)	0.43	0.14	0.72
Average boat trips	1.80 (0.44)	2.09 0 (0.30)	0.86	0.37	1.35
Total trips	2.79 (0.60)	4.40 (0.37)	0.63	0.34	0.93
ABS first phase no cells adjustment method					
Average shore trips	1.00 (0.31)	2.31 (0.28)	0.43	0.14	0.72
Average boat trips	1.82 (0.44)	2.09 0 (0.30)	0.87	0.38	1.36
Total trips	2.82 (0.59)	4.40 (0.37)	0.64	0.35	0.93

Before leaving this subject, it is interesting to note that by using the match status in nonresponse adjustment reduced the avidity bias in the ABS sample and made the estimates of the overlap more similar. An adjustment of this type is more difficult to implement in a dual frame telephone approach because the matching is subject to greater error. One way to do this with a telephone frame is to attempt to match the telephone numbers from the telephone sample to the telephone numbers on the license frame. The two main problems with this approach are: (1) the telephone numbers on many license frames are incomplete and out-of-date making matching difficult, and (2) many people may be reached by telephone on multiple telephone numbers (cell numbers and landline numbers) so that the telephone sampled might not be the telephone number included in the license frame. Another way of accomplishing the matching is to rely on the angler to indicate whether they have a license or not and consider this response to determine license status. As we discuss in this report, anglers may not report their license status accurately (as discussed later there are substantial errors of omission and commission).

Next we assess the accuracy of determining overlap membership from data reported by respondents. The overlap consists of licensed anglers residing in NC, and the only characteristic required from the respondent (in the absence of a method of matching) is possession of a valid saltwater license for the wave. As noted earlier, this is a method that is currently being used in dual frame telephone samples and might be more precise than matching by telephone number.

While only the quality of self-reported information about licensure among the ABS respondents is in question, the respondents in both frames were given the same questionnaire in the pilot. Thus, respondents from both frames provide information about the error rates of overlap identification. The sample from the license frame and the matched ABS frame both provide estimates of the false negative rate for the licensure question (i.e., the proportion of validly licensed respondents who claim they do not have a license). The ABS sample also provides an estimate of the false positive rate for the licensure question (i.e., the proportion of respondents who do not have a valid license but claim they do); this quantity may not be estimated as accurately as the false negative rate because of the small sample size and issues in matching addresses.

Table 8 provides estimates for three licensure questions (if the respondent has a NC fishing license, has a NC recreational saltwater fishing license, and has a NC recreational saltwater fishing license for the reference period, Wave 6, November –December 2009). First we examine the false negative rates estimated from both frames. The first column shows the estimated percentage claiming they have a valid license for the license frame respondents who reside in NC. The second column shows the same percentage for the ABS respondents who match to the license frame. All respondents in these two columns should report “yes” to all three questions. The upper half of the table shows estimates for the overlap population who saltwater fished in 2009, and the bottom half for those who fished in the wave. From the license frame, we estimate that about 15% of those who fished during the year and 10% of those who fished during the wave reported erroneously that they did not have a license to do so. The comparable estimates from the ABS frame were about 30%. This suggests that there may be a higher false negative rate from the ABS sample than from the license sample, although the sampling errors are so large that the difference is not significant.

Table 8. Percent of Respondents in from both frames reporting ownership of various NC fishing licenses (standard errors in parentheses)

	License frame: Resident licenses	ABS frame: match to license frame	ABS frame: not match to license frame
Among Respondents Who Fished during 2009:	**(n=435)**	**(n=60)**	**(n=92)**
NC Fishing License	95.5 (1.1)	85.5 (7.4)	---
NC Saltwater License	90.0 (1.6)	72.2 (4.9)	---
NC Saltwater:Wave	85.3 (1.9)	69.8 (4.3)	27.9 (6.4)
Respondents Who Fished in Wave 6, 2009:	**(n=122)**	**(n=25)**	**(n=22)**
NC Fishing License	94.4 (2.1)	70.8 (13.8)	---
NC Saltwater License	90.4 (2.8)	70.8 (13.8)	---
NC Saltwater:Wave	89.5 (2.9)	70.8 (13.8)	46.0 (17.0)

The last column of the table gives estimates from the ABS sample for those who did not match to the license frame, providing information about false positive rates. It shows that 28% of the anglers who fished during the year and 46% of the anglers who fished during the wave and did not have a license (at least they did not match by address) erroneously reported having a license. With so few respondents in these cells, the sampling errors are very large.

Given the small sample sizes, the estimates of error rates for some subpopulations are very tentative, but there are some mechanisms that might support higher error rates in the matched ABS sample than in the license frame. The data collection procedures varied somewhat between the samples in subtle ways that may have influenced responses. For example, in the license frame sample, the mail was addressed to the angler by name and there was no first phase mailing. In addition, the matching from the ABS is by address not by angler, and households with more than one angler may have both licensed and unlicensed anglers, leading to the appearance of more error. Additional studies with larger sample sizes are needed to more adequately determine the magnitude and sources of the errors. However, the pilot does show that relying on respondents to self-identify their domain membership is a source of error that can be greatly reduced by the address matching in the ABS approach. This finding suggests that the current approach used to match sample frames in the dual-frame telephone survey design is insufficient.

Because we have additional information as to the nature of the license held by respondents in the license frame, we can also examine factors that may be related to the quality of reporting about licensure. Table 9 shows estimates of false negative rate by the type of saltwater fishing license held by the individual made from the license frame. Overall, the highest rate of accurately reporting licensure was for the broad category of "NC Fishing License," with the poorest reporting for the wave specific saltwater fishing license. This is as expected, since the wave-specific reporting requires the respondent to retrieve information not only about the type of license, but the valid dates for that license. Those anglers who held licenses specific to saltwater fishing tended to be more accurate than those who held combination licenses. Once again, this is not an unexpected finding given that the question wording for holders of recreational saltwater fishing license closely matches the nature of the license they hold, making the reporting task cognitively easier than for those with combination licenses. Non-lifetime licenses must have been purchased sometime during the past 12 months, making the reporting task more salient and of higher quality for respondents with those types of licenses than for lifetime license holders. Finally, we see that among all respondents, non-resident license holders were more likely to report accurately than NC residents. We might speculate that the nature of the licenses for the non-resident groups differs (e.g., one week vs. one year) and by definition, requires travel from outside the state, once again adding to the saliency of the license.

Table 9. Estimated Percentage of saltwater license holders from the license frame who claim they do not have a valid license, by type of license (standard errors in parentheses)

	Total	**Saltwater Only**	**Combo**	**Lifetime**	**Not a Lifetime**	**Resident**	**Non-Resident**
Respondents who say they don't have a…	**Respondents Who Fished during 2009**						
	(n=718)	(n=527)	(n=191)	(n=99)	(n=619)	(n=435)	(n=383)
NC Fishing License	4.5 (1.0)	4.0 (1.1)	5.3 (1.8)	1.7 (1.7)	5.2 (1.2)	4.5 (1.1)	5.6 (1.4)
NC Saltwater License	9.9 (1.5)	6.0 (1.3)	15.8 (3.0)	12.5 (5.0)	7.1 (1.3)	10.0 (1.6)	8.6 (1.7)
NC Saltwater Wave	15.1 (1.7)	13.0 (1.9)	18.3 (3.2)	22.9 (5.0)	13.4 (1.8)	14.7 (1.9)	19.0 (2.4)
	Respondents Who Fished in Wave 6, 2009						
	(n=227)	(n=179)	(n=48)	(n=17)	(n=219)	(n=122)	(n=105)
NC Fishing License	5.3 (1.9)	5.0 (2.2)	5.8 (3.4)	0 (NA)	6.6 (2.1)	5.6 (2.1)	3.2 (1.7)
NC Saltwater License	9.1 (2.5)	6.6 (2.5)	13.5 (5.3)	22.9 (11.8)	7.2 (2.3)	9.6 (2.8)	5.3 (2.2)
NC Saltwater Wave	10.9 (2.6)	9.4 (2.8)	13.6 (5.3)	22.9 (11.8)	9.3 (2.4)	10.5 (2.9)	13.8 (3.4)

Dual Frame Estimators

Above, we explored some of the key error components for dual frame estimators, and found the domain identification (with and without a license) among the ABS to be of relatively high quality but the response patterns from the two frames to be somewhat different. In the overlap, the respondents from the ABS sample appear to be more likely to have fished in the wave but to have gone on fewer trips than the respondents from the license sample. As a result, the consequences for the bias and variance for estimating total trips from a dual frame estimator are not clear.

To better understand the consequences for the dual frame estimators we created three dual frame estimators. The estimators were all of the simple form of averaging the overlap estimates from the two frames to produce an overlap estimate, and then adding the non-overlap estimates from the separate frames. . More specifically, let $\hat{y}_{12}^{1}$ and $\hat{y}_{12}^{2}$ be the weighted estimates of the overlap domain from frame 1 (the ABS frame) and frame 2 (the license frame), respectively, then an average or composite dual frame estimator is $\hat{y}_{ave} = \hat{y}_1 + \hat{y}_2 + \lambda \hat{y}_{12}^{1} + (1-\lambda)\hat{y}_{12}^{2}$, with $0 \le \lambda \le 1$, where the subscript 1 denotes the non-overlap component from the ABS frame and 2 is the non-overlap component from the license frame. Lohr (2009) provides a good discussion of these estimators.

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The typical assumption is that $\hat{y}_1$ and $\hat{y}_b$ $\hat{y}_2$ are unbiased for the totals in the two nonoverlapping domains, and $\hat{y}_{12}^{1}$ and $\hat{y}_{12}^{2}$ are both unbiased for the total in the overlap domain. If this set of assumptions holds, then $\hat{y}_{ave}$ is an unbiased estimator of the total. To produce estimates of characteristics using weights, the weights for units in the overlap that are sampled from frame 1 are multiplied by λ and the weights for overlap units sampled from frame 2 are multiplied by $(1-\lambda)$.

Our main concern is that the assumption that $\hat{y}_{12}^{1}$ and $\hat{y}_{12}^{2}$ are both unbiased for the total of the overlap domain may not hold, since the estimated number of anglers and average trips per angler from the overlap differ by frame. The assumption of unbiasedness for the non-overlap component estimates is also a concern, but we do not have any evidence from the survey to evaluate it.

As a simple method of evaluating the effect the choice of the compositing factor might have on the bias and standard errors of the estimates, we created three dual frame estimators with $\lambda = 0.2$, 0.5, and 0.8. The standard choice might have been to choose $\lambda = 0.2$, since about this percentage of the overlap cases were from the ABS frame (25 of the 142 who fished in the wave). Because the weights were so much larger for the ABS cases and their contribution to the variance might be large, another reasonable choice might have been closer to $\lambda = 0.5$. The choice of $\lambda = 0.8$ was used to investigate a compositing factor that was very different from these more reasonable factors.

Table 10 gives estimates of the number of anglers, the percent of anglers, the number of trips (boat, shore and total), and the mean number of trips by stratum and overall for the three estimators. The first two columns give the estimates and their standard errors computed using $\lambda =$ 0.5. All of the estimates of standard errors were computed using replication methods. The next two columns give the ratio of the estimates for $\lambda = 0.2$ and $\lambda = 0.8$ to the estimates to $\lambda = 0.5$. When these estimates equal unity, it means the choice of λ did not affect the magnitude of the estimates. Scanning over the column shows the effect on the magnitude from the choice of the λ is not very large, with only the few bolded estimates outside of the range (0.95 to 1.05). The last two columns show the effect on the precision of the estimates by taking the ratio of the standard errors of the estimators using $\lambda = 0.2$ and $\lambda = 0.8$ to the standard errors to $\lambda = 0.5$. Once again, there are few ratios outside of the range (0.95 to 1.05) and those are in bold. It appears that the standard errors for the estimators using $\lambda = 0.8$ are somewhat more affected by the choice of the compositing factor than the other estimators, as might be suspected.

In general, the estimators and standard errors seem to be fairly robust to the choice of the compositing factor, especially for the two more reasonable choices of $\lambda = 0.2$ and $\lambda = 0.5$. One explanation for this robustness is the fact that the overlap only has 37% of all the anglers who fished during the last year. (This estimated percentage varies slightly depending on the choice of the compositing factor). The non-overlap component from the license frame is about 5% of the total angler estimate while the non-overlap component from the ABS is about 58%. If the license frame were more complete, then the overlap would be a larger component of the total and the dual frame estimators and standard errors might be less robust; i.e., changes in the compositing factor might be more important to bias and standard errors of the dual frame estimators. However, under the current circumstances we can be fairly confident that the compositing factor can be chosen using standard methods without introducing large biases or inefficiencies.

Table 10. Dual Frame Estimates of Anglers and Trips By Compositing Factor.

		Computations with Composite Factor λ=0.5		Ratio of Estimates to Composite with λ=0.5		Ratio of std Error to Composite with λ=0.5	
Estimate	**Stratum**	estimate	std err	λ=0.2	λ=0.8	λ=0.2	λ=0.8
Number of Anglers	**Coastal**	122,625	18,562	0.96	1.04	**0.94**	**1.12**
	Non-Coastal	101,894	37,497	0.97	1.03	0.97	**1.06**
	Out of State	10,225	939	1.00	1.00	1.00	1.00
	Overall	234,743	42,035	0.96	1.04	0.97	**1.07**
Percent of Anglers	**Coastal**	52.24	9.82	1.00	1.00	1.01	1.03
	Non-Coastal	43.41	10.37	1.00	1.00	1.01	1.02
	Out of State	4.36	0.89	1.03	0.97	1.05	0.99
Number of Boat Trips	**Coastal**	246,294	45,182	0.97	1.03	1.00	**1.09**
	Non-Coastal	131,565	68,746	0.98	1.02	0.98	1.04
	Out of State	14,432	3,142	1.00	1.00	1.00	1.00
	Overall	392,291	83,215	0.98	1.02	1.00	1.05
Number of Shore Trips	**Coastal**	342,487	144,812	0.98	1.02	1.00	1.01
	Non-Coastal	151,760	68,235	**1.18**	**0.82**	1.01	1.00
	Out of State	35,094	4,623	1.00	1.00	1.00	1.00
	Overall	529,341	159,098	1.04	0.96	1.00	1.01
Number of Total Trips	**Coastal**	588,781	165,348	0.98	1.02	1.00	1.02
	Non-Coastal	283,325	132,638	1.09	0.91	1.00	1.02
	Out of State	49,525	6,813	1.00	1.00	1.00	1.00
	Overall	921,631	211,898	1.01	0.99	1.00	1.02
Mean Number of Boat Trips	**Coastal**	2.01	0.237	1.01	0.99	1.04	**1.09**
	Non-Coastal	1.29	0.583	1.02	0.99	1.03	0.99
	Out of State	1.41	0.246	1.00	1.00	1.00	1.00
	Overall	1.67	0.265	1.01	0.99	1.03	1.02
Mean Number of Shore Trips	**Coastal**	2.79	1.041	1.03	0.98	1.04	0.97
	Non-Coastal	1.49	0.596	**1.22**	**0.79**	**1.09**	0.97
	Out of State	3.43	0.298	1.00	1.00	1.00	1.00
	Overall	2.25	0.622	**1.08**	**0.93**	1.04	0.98
Mean Number of Total Trips	**Coastal**	4.8	1.066	1.02	0.98	1.03	0.98
	Non-Coastal	2.78	1.103	**1.12**	**0.88**	1.05	0.99
	Out of State	4.84	0.406	1.00	1.00	1.00	1.00
	Overall	3.93	0.748	1.05	0.95	1.04	0.98

Magnitude of clustering

In the pilot survey, data were collected from more than one angler in some households in both frames, when they were identified. This allowed for an investigation of the similarity between the responses obtained from two anglers in the same household. In addition, a previous study (Lin, 2009) had shown that in the CHTS, the responses for multiple anglers in the same household have such high correlation that there is some question about whether or not attempts to obtain information from multiple anglers is even worthwhile. We wanted to see if that remains true with the self-administered mail survey. We believed that the within-household correlation might be reduced in the mail survey, due to the fact that the responses for multiple anglers are often obtained from a single household respondent in the telephone survey, and in the mail survey each individual angler received his or her own questionnaire. In this section, we describe how we estimated the level of clustering for both angler and trip characteristics within a household from the mail survey. Then we compare those estimates to similar estimates for the telephone survey from the same time period.

Clustering of angler behavior within household

Because *ICC* (intra-cluster correlation) is defined only for clusters of equal size, we use a more general measure of clustering, the adjusted R^2, denoted R_a^2, to describe the effect. This parameter is defined (Lohr 2010, p. 175) as

$$R_a^2 = 1 - \frac{MSW}{S^2}, \qquad (1)$$

where *MSW* and S^2 are defined as in an analysis of variance; i.e.,

$$MSW = SSW/(K-N) = \sum_{i=1}^{N}\sum_{j=1}^{M_i}(y_{ij}-\bar{y}_{iU})^2 \Big/ (K-N)\,, \tag{2}$$

$$S^2 = SST/(K-1) = \sum_{i=1}^{N}\sum_{j=1}^{M_i}(y_{ij}-\bar{y}_U)^2 \Big/ (K-1)\,, \tag{3}$$

$K = \sum_{i=1}^{K} M_i$ = # of secondary sampling units (anglers) in the population, N = number of psu's (households) in the population, and M_i = # of ssu's in the i^{th} psu. Because these parameters are to be estimated from a complex design, weights are needed, and each frame and variable requires its own estimator due to differences in the designs.

First we consider estimation of R_a^2 for number of shore trips, boat trips, and total trips in the license frame. In this case, we actually don't know for sure the number of licensed anglers within each household. However, the sample from the license frame was matched to the total license frame, and whenever an address match was found, the second angler was also sampled. The angler-level weighting of this sample then assumed that exactly two licensed anglers were present in every household in which a match was found. Thus we assume M_i = 1 or 2 for all households in the license frame. Note that households with only one angler make no contribution to SSW, but they do make a contribution to SST. There are two reasonable ways to estimate R_a^2 for this frame. One is that we use all households, with the one-angler households contributing only to S^2 but not *MSW*. This would also require estimating N, the number of households represented on the license frame.[11] A second approach is to compute R_a^2 only for that subset of the

[11] This could be done using the method in the JOS paper. That is, we could estimate the average number of licensed anglers per household for each stratum and divide the total number of anglers on the frame in each stratum by this quantity, and sum them over strata.

population that contains multiple licensed angler households; i.e., those for which M_i = 2. We take this approach, since it makes the results from the two frames more comparable, due to the fact that the proportion of households having multiple licensed anglers on the license frame may differ from the proportion of households having multiple anglers in the address frame. Thus we estimate

$$M\hat{S}W = S\hat{S}W/(2\hat{N}_m - \hat{N}_m) = \sum_{i=1}^{n_m}\sum_{j=1}^{2} w_{ij}(y_{ij} - \bar{y}_{iU})^2 \Big/ \hat{N}_m \qquad (4)$$

and

$$\hat{S}^2 = \frac{S\hat{S}B + S\hat{S}W}{2\hat{N}_m - 1}, \qquad (5)$$

(see Lohr 2010, p. 177) where n_m is the number of sampled households with 2 licensed anglers, $\hat{N}_m = \hat{K}_m / 2 = \sum_{i=1}^{n_m}\sum_{j=1}^{2} w_{ij} / 2$ is the estimate of the number of households in the population with 2 licensed anglers, and $S\hat{S}B = \sum_{i=1}^{n_m}\sum_{j=1}^{2} w_{ij}(\bar{y}_{iU} - \hat{\bar{y}}_U)^2$, where $\hat{\bar{y}}_U$ is the estimate of population mean from the complex design.[12] The first three rows of Table 11 show the components of these estimators, as well as the resulting estimateestimates of R_a^2 for the effort variables in the license frame.

Recall that differences in the design of the license and ABS sample caused differences in estimates of effort. In the ABS frame, only those anglers who fished during the past year were sampled in the second phase, which made the 2nd phase ABS anglers potentially more avid than the anglers sampled from the license frame. To make the two samples

[12] The estimates for the sums of squares for the complex design can be obtained using SAS PROC SURVEYREG's ANOVA table.

more comparable, we produced estimates of effort for the license frame that first filtered on the flag indicating whether or not the angler had fished in the last year. It seems reasonable that the same difference in design might cause different estimates of the clustering parameter as well. Therefore, we also made estimates of R_a^2 for the population of households containing two licensed anglers who have fished in the last year. These estimates were calculated from (4) and (5), but this time for the population of households containing two *active* licensed anglers. The results for these estimates are shown in rows 4 through 6 of Table 11. The differences in the correlations for the two populations are slight.

Table 11. Computation of R_a^2 for angler effort variables for License frame

Population	Variable	n_m	$\hat{K}$	$\hat{N}_m$	$S\hat{S}W$	$S\hat{S}B$	R_a^2
All licensed anglers in multiple angler hh's	Shore trips	134	82,540	41,270	82,145	158,867	0.32
	Boat trips	134	82,540	41,270	129,636	194,110	0.20
	Total trips	134	82,540	41,270	330,029	446,974	0.15
All active licensed anglers in multiple angler hh's	Shore trips	102	59,345	29,672	79,284	150,573	0.31
	Boat trips	102	59,345	29,672	129,636	186,443	0.18
	Total trips	102	59,345	29,672	327,167	418,003	0.12

Next we consider estimation of R_a^2 for the ABS frame. A different estimation method is required due to a difference in the design that was used to sample anglers within households, and the information available about the size of the household clusters. In the ABS frame, two anglers were sampled from a subset of the multiple angler households in the sample, and a single angler was sampled from the rest. In all cases, the number of anglers in the household was known. The angler weights that were calculated for the

ABS sample used the information about the number of anglers in the household, and so varied from one household to another, even within the same stratum and non-response weighting class. As with the license frame, we can use respondents in all households to estimate S^2 defined in (3), or only those respondents who contribute to estimation of MSW; i.e., those in households from which we sampled two members. As before, we chose the latter method. Thus the parameters being estimated will again be for the subset of the ABS frame residing in households with at least two adult active anglers. The estimators of the parameters in (2) and (3) are thus

$$M\hat{S}W = S\hat{S}W / (\hat{K}_m - \hat{N}_m) = \sum_{i=1}^{n_m} \sum_{j=1}^{2} w_{ij} (y_{ij} - \hat{\bar{y}}_{iU})^2 \Big/ (\hat{K}_m - \hat{N}_m) \qquad (6)$$

and

$$\hat{S}^2 = \frac{S\hat{S}B + S\hat{S}W}{\hat{K}_m - 1} \qquad (7)$$

(see Lohr 2010, p. 177) where n_m is the number of sampled households with 2 licensed anglers, $\hat{\bar{y}}_{iU}$ is the estimate of mean for household i, $\hat{K}_m = \sum_{i=1}^{n_m} \sum_{j=1}^{2} w_{ij}$, and

$\hat{N}_m = \hat{K}_m / \hat{\bar{M}}_m$, where $\hat{\bar{M}}_m = \sum_{i=1}^{n_m} w_i M_i / \sum_{i=1}^{n_m} w_i$ is an estimate of the average number of anglers in households with multiple anglers, and w_i is a household weight computed from the angler weights (w_i = (2/ # of adult anglers in hh)*w_{ij}). These estimates are used to form an estimate of R_a^2 as shown in (1). The results are shown in Table 12. Note that the sample size is much smaller in this case than the license sample; only 17 households in the sample had responses from 2 active anglers, so the estimates have high variability.

Table 12. Computation of R_a^2 for angler effort variables for ABS frame

Population	**Variable**	$\boldsymbol{n_m}$	$\hat{K}$	$\hat{N}_m$	$S\hat{S}W$	$S\hat{S}B$	R_a^2
All active licensed anglers in multiple angler hh's	Shore trips	17	177,747	78,270	438,928	453,380	0.12
	Boat trips	17	177,747	78,270	63,512	86,729	0.24
	Total trips	17	177,747	78,270	549,682	591,967	0.14

The estimates of R_a^2 for boat trips and total trips are very similar to those for the license frame, while the estimate of R_a^2 for shore trips is slightly lower, though the small sample size for the ABS frame may be the cause of this.

Clustering of trip-level characteristics within angler

Next we consider estimation of R_a^2 for trip-level characteristics. There are two levels of clustering for trips: within angler and within household clustering. The analysis here estimates the correlation of trip characteristics within angler. As noted earlier, the respondents were asked to profile only their four most recent trips. This does provide some information about the clustering within angler on characteristics such as public/private access or time of return. However, the profiled trips are not a probability sample of trips made in the wave. Despite this, we did use the data to make estimates of R_a^2. To the extent that the four recalled trips have similar characteristics to a random sample of trips made by the angler, the estimates will be valid.

We estimated R_a^2 as shown in (1), but this time the two mean squares must be defined differently:

$$MSW = SSW/(T-K) = \sum_{i=1}^{N}\sum_{j=1}^{M_i}\sum_{k=1}^{T_{ij}} (y_{ijk} - \bar{y}_{ijU})^2 \Big/ (T-K)\,, \tag{8}$$

$$S^2 = SST/(T-1) = \sum_{i=1}^{N}\sum_{j=1}^{M_i}\sum_{k=1}^{T_{ij}} (y_{ijk} - \bar{y}_U)^2 \Big/ (T-1)\,, \tag{9}$$

where T is the total number of trips in the population, y_{ijk} is a characteristic of the k^{th} trip made by the j^{th} angler in household i (referred to henceforth as the $(i,j)^{\text{th}}$ angler), and $\bar{y}_{ijU}$ is the mean of all trips made by that angler. To estimate MSW and S^2, we used only those anglers who made at least two trips in estimation of both sums of squares. Thus

$$M\hat{S}W = S\hat{S}W/(\hat{T}-\hat{K}) = \sum_{i=1}^{n}\sum_{j=1}^{m_i}\sum_{k=1}^{t_{ij}} w_{ijk}(y_{ijk} - \hat{\bar{y}}_{ijU})^2 \Big/ (\hat{T}-\hat{K}) \tag{10}$$

and

$$\hat{S}^2 = \frac{S\hat{S}B + S\hat{S}W}{\hat{T}-1}\,, \tag{11}$$

where n and m_i are the number of households and anglers in the subsample of anglers with multiple trips, t_{ij} is the number of trips reported by the $(i,j)^{\text{th}}$ angler, $\hat{\bar{y}}_{ijU}$ is the estimate of mean for the trips of the (i,j)$^{\text{th}}$ angler, $S\hat{S}B = \sum_{i=1}^{n}\sum_{j=1}^{m_i}\sum_{k=1}^{t_{ij}} w_{ijk}(\bar{y}_{ijs} - \hat{\bar{y}}_U)^2$,

$\hat{T} = \sum_{i=1}^{n}\sum_{j=1}^{m_i}\sum_{k=1}^{t_{ij}} w_{ijk}$, and $\hat{K} = \sum_{i=1}^{n}\sum_{j=1}^{m_i} w_{ij}$. The weight w_{ijk} was constructed by assuming that the profiled trips are a random sample of all trips made by the angler, yielding

$$w_{ijk} = w_{ij} * (\text{\# of trips made by angler } (i,j))/(\text{\# of trips profiled by angler } (i,j))\,. \tag{12}$$

We also made a second estimate of R_a^2 only for that subset of anglers who reported all their trips, to see if the (untrue) assumption that the sampled trips were a random sample of all the angler's trips made a substantial difference in the estimate.

For completeness, we present in Table 13 a summary of the four variables we will be examining for within angler correlation: the number of anglers on each trip reported (TOT), whether or not the trip was (or ended) at a public site (PUB), whether or not it ended between 6:00 p.m. and 6:00 a.m. (LATE), and whether it included an accompanying child (CHILD). The trips accessed through the two frames do appear to be quite different, with those from the license frame more likely to be at public sites and less likely to end during night hours, which suggests they are more closely aligned with the trips profiled by the intercept survey. The trips accessed through the license frame appear to be less likely to include additional family members than those encountered through the ABS frame.

Table 13. Estimates of trip characteristics for the two frames

Sample from:	**Mean # of anglers on trip (sd)**	**Proportion of trips in public site (sd)**	**Proportion of trips ending at night (sd)**	**Proportion of trips including a child (sd)**
License frame	1.6 (0.1)	0.81 (0.04)	0.21 (0.04)	0.15 (0.03)
ABS frame	3.0 (0.7)	0.67 (0.12)	0.41 (0.15)	0.50 (0.14)

Table 14 summarizes the calculations (using (1), (8) and (9)) for estimating R_a^2 for the 4 variables described in Table 13. These calculations were carried out for the samples from each frame.

Table 14. Computation of R_a^2 for angler effort variables for two frames

Sample from…	Variable	$\hat{T}$	$\hat{K}$	$S\hat{S}W$	$S\hat{S}B$	R_a^2
Trips accessed through LIC frame for domain of anglers reporting >1 trip	TOT	258,551	48,780	87,477	378,619	0.72
	PUB	258,551	48,780	16,609	26,028	0.52
	LATE	258,551	48,780	12,732	28,631	0.62
	CHILD	258,551	48,780	7,310	30,448	0.76
Trips accessed through ABS frame for domain of anglers reporting > 1 trip	TOT	828,266	216,378	35,201	3,597,963	0.99
	PUB	828,266	216,378	7,574	175,568	0.94
	LATE	828,266	216,378	11,757	189,238	0.92
	CHILD	828,266	216,378	14,085	192,896	0.91

The results show that the trips made by an angler tend to be quite similar. This is especially true for the trips taken by anglers in the ABS frame.

Comparison with Telephone Frame

Simultaneously with the mail survey experiment, a dual frame telephone survey was conducted, which collected similar data about anglers and their fishing trips. The two frames were an RDD frame (CHTS) and the license frame (ALDS). The CHTS chose telephone numbers only from coastal households, while the ALDS sample drew from all licensees whose telephone numbers could be discerned from the license frame. We used the data from that survey to make estimates of R_a^2 , for total number of trips and for two of the trip characteristic variables (whether or not the trip ended at a public site, PUB, or between 6:00 p.m. and 6:00 a.m., LATE), which we compared with those from the mail survey.

There were differences in the sample designs of mail and telephone that make the measures of correlation apply to different populations, and therefore which may not be directly comparable. In the telephone survey, information was collected about every angler in the household, so that clusters of more than two anglers were possible. Since there was no matching to the license frame, there was no way to identify who was licensed and who was not in the CHTS, so correlations were computed for all anglers in the household, whether they were licensed or not. In the collection of trip characteristics, anglers were required to recall all the trips he/she took, rather than the four most recent ones[13]. So by design, there should have been no sampling at the last stage, but rather a complete observation of trips within anglers. However, many anglers did not provide information for all trips. During Wave 6, 2009 in NC, 71.51% of trip records were imputed (i.e. not profiled).Instead, weights were created to account for the missing trips, based on the number of trips reported by the angler, and effectively the trips that were reported were treated as though they were a random sample of trips for the purpose of estimating R_a^2.

The estimator of R_a^2 that we used for angler characteristics in both frames was the same as that shown in (6) and (7), except that the upper limit of the inner sum in (6) can be larger than 2, since data was collected about all the anglers in a household. The estimator of R_a^2 that we used for trip characteristics in both frames was the same as that shown in (10) and (11). The weight associated with the $(i,j,k)^{\text{th}}$ trip is defined as in (12), though the absent profiles were due to nonresponse, rather than from the instruction to profile only the most recent trips.

Results are shown in Table 15 for the angler characteristic, total number of trips, for both frames. A comparison of Tables 9, 10, and 15 shows that the correlation of effort within household is much larger for the telephone than for the mail survey, as expected.

[13] Note that proxy reported information is accepted for the telephone surveys if the individual angler(s) can not be interviewed. In these cases, a respondent may be reporting about his or her own trips as well as those of other household members.

Table 15: Computation of R_a^2 for angler effort for the two telephone frames:

Sample From...	Variable	n_m	$\hat{K}$	$\hat{N}_m$	$S\hat{S}W$	$S\hat{S}B$	R_a^2
All anglers in multi-angler hh in ALDS	Total trips	82	93,697	41,994	53,501	1,703,411	0.945
All anglers in multi-angler hh in CHTS	Total trips	30	12,184	5423	67.99	1,284,853	0.999

For completeness, summary data for the two trip characteristic variables for the two telephone frames is shown in Table 16.

Table 16. Estimates of trip characteristics for two frames

Sample From:	Proportion of trips in public site (sd)	Proportion of trips ending at night (sd)
Anglers with > 0 trips in ALDS	0.80 (0.04)	0.11 (0.02)
Anglers with > 0 trips in CHTS	0.69 (0.07)	0.11 (0.04)

Table 17 displays the estimated R_a^2 for the trip characteristic variables for the telephone frames. They are much smaller than the correlation among anglers in the same households. Comparison with Table 14 shows that the correlations of characteristics among trips by the same angler is similar for the mail and telephone frames in the license frame, but not for the CHTS/ABS frame. The anomaly seems to be the correlation for the ABS frame, which is unusually high, and much higher than the correlation for the CHTS frame. One could imagine that tourists to the coast from non-coastal counties may take more similar trips, especially since they were instructed to report only their last 4 (consecutive) trips. The CHTS would contain no such non-coastal anglers in its sample, while the ABS does contain such anglers. Still the magnitude of this difference is hard to explain.

Table 17. Computation of R_a^2 for angler effort variables for two frames

SAMPLE FROM…	VARIABLE	$\hat{T}$	$\hat{K}$	$S\hat{S}W$	$S\hat{S}B$	R_a^2
Trips accessed through ALDS frame for domain of anglers reporting > 0 trips	LATE	132,775	20,226	5,065	8,270	0.55
	PUB	132,775	20,226	10,797	17,627	0.55
Trips accessed through CHTS frame for domain of anglers reporting >0 trips	LATE	537,895	126,644	21,578	29,947	0.45
	PUB	537,895	126,644	22,698	67,284	0.67

These findings about correlation suggest that a design which attempts to sample more than one angler from the same household is more cost effective for the mail survey than the telephone survey, for estimating effort. For trip characteristics, this does not appear to be so. However, the latter finding comes with the caveat that the method of sampling trips for an angler differed by mode. The mail survey asked respondents to describe their four most recent trips, which may explain why the trip characteristics would be more similar to each other than the characteristics of all trips made during the wave, which were requested of telephone respondents.

Discussion

The primary goal of the pilot study was to examine whether a self-administered two phase study could be successfully implemented to estimate fishing effort among NC anglers in the fall of 2009, with an eye toward improving both the coverage and the response rates currently achieved via telephone surveys. With respect to response rate and the feasibility of conducting a two-phase self administered survey among anglers, the response rates presented in Table 2 clearly indicate that such a design is feasible and offers a potential alternative to the RDD design currently used by MRIP. Both the two-phase approach used with an ABS frame as well as the single-phase approach based on a license frame yielded response rates that exceed the current response rates achieved via telephone data collection (CHTS and ALDS, respectively). But the response rates from the ABS sample also raise concerns about avidity bias, an issue in angler surveys regardless of the mode and method of data collection. We also see a pattern (albeit not significant) similar to findings from other studies (Montaquila et al., 2010) that a longer lag time between the screener survey field period and the mailing of the extended survey instrument may be beneficial with respect to response rates. The small sample for the field test limits our ability to draw additional conclusions or recommendations with respect to the details of fielding a two-phase dual frame study by mail, but does provide sufficient positive findings to motivate further research in this area.

Other indications of data quality, specifically missing data rates or data inconsistencies did not signal a red flag. We saw relatively low levels of missing data, with the exception of detailed trip reports for avid anglers. However, both the CHTS and ALDS telephone surveys are plagued with similar problems, with respondents either not providing detailed reports for each trip or opting for the response option that all trips are similar. Regardless of the mode of data collection, attempting to collect detailed trip level information for a two month recall period for avid anglers is difficult and may require a reconsideration of the data elements to be collected for these anglers.

With respect to the dual frame nature of the study, the study had two goals: to estimate the improvement in coverage the two frames provide and to examine means by which to identify the overlap among elements across the two frames. Here too we found significant gains via the use of a dual frame design consisting of a license frame and an addressed-based frame in comparison to the current CHTS and ALDS sample designs. The findings support the improvement in the identification of frame overlap via the use of addresses as compared to self-reported fishing licensure. Thus, the use of a self-administered mail survey (based on addresses from an ABS frame and a license frame) facilitates improved identification of overlapping sample members as compared to what is possible for a dual-frame telephone survey.

The findings clearly support empirical results that have been well established in the literature, namely the presence of avidity bias in surveys of recreational anglers. We are planning to test a revised household screener that allows respondents to provide information about other recreational activities besides fishing. The goal of the revised instrument is to reduce the fishing avidity bias in the ABS sample. For both the ABS and license sample, we plan to use the type of license in nonresponse adjustment to reduce nonresponse bias. We hope that both of these steps may reduce the differences in the estimates for the overlap domain.

The major limitation of the study is its small sample sizes for active anglers during the wave. This limitation makes it difficult to precisely estimate fishing effort and reduces our ability to understand differences in fishing behavior as a function of geography and ownership of landline phones. In addition, the small sample size makes it impossible to assess the degree to which the current approach to coverage adjustment in the MRIP, that is, the use expansion factors based on the APAIS, is fully representative of all fishing trips. While we see indications of differences in these population subgroups which have traditionally been under-covered in the telephone surveys, we cannot address the extent to which their actual fishing behavior differs.

As is true for many exploratory pilot studies, the goal was not to be able to provide the definitive answer with respect to a redesign of the current MRIP telephone surveys. Rather, the pilot was successful in examining the feasibility of moving away from the telephone to a self-administered two-phase survey. It also clearly demonstrated the utility of this design in the context of a dual frame sample. The success of the two-phase mail survey, especially with respect to the dual frame design, shows the substantial potential for improving future angler surveys.

Recommendations

The 2009 two phase dual frame study conducted in North Carolina was a first step toward exploring sample and design options to address coverage, efficiency, and other issues that were raised in the report of the National Research Council. As noted above, the size of the sample limits our ability to offer definitive recommendations for a full scale redesign of the MRIP program, but the findings do suggest the following:

1. **This study, as with other empirical studies, clearly indicates that surveys of anglers are subject to avidity bias. As noted above, we recommend further experimental studies to reduce avidity bias (e.g. broaden the base of the screener questionnaire) and further examination of how to reduce avidity bias through the use of license type information in nonresponse adjustments.**
2. **We suspect that the avidity bias evident in the mail survey also exists for CHTS and ALDS. We recommend implementing studies to test for avidity bias in CHTS and ALDS.**
3. **Matching household sample frames to license frames, regardless of whether using a dual-frame approach or a single-frame approach is a good approach to adjust for avidity bias. We would recommend this for surveys conducted by either the telephone or mail; however, telephone surveys would need to reverse link to addresses to facilitate this matching.**
4. **Conduct follow-up studies with sufficient sample sizes to test the assumption that the APAIS (intercept) survey is representative of all trips (e.g. do the trips that we can cover in the APAIS (public access) adequately represent all trips?). Sufficient sample sizes would also facilitate more robust estimation of trip-level information and comparisons of the effort levels and characteristics of trips by frame and for subgroups not currently covered by the CHTS.**
5. **Not addressed in the present study is the need for timely data. Clearly a shift toward self-administered mail surveys comes at the potential cost of longer field periods than comparable telephone surveys. This is particularly true**

when the survey involves the need to screen households (the two phase ABS frame design). Future studies should examine the relative speed of the CHTS/ALDS design compared to a mail mode (or possibly mixed mode).

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Appendix A: Disposition of ABS and License Sample Units

Table A-1. Screener Disposition for ABS Sample, Both Waves, by Stratum

Disposition	Coastal Counties	Non-Coastal Counties	Total
Total Completes	357	328	685
With Anglers	154	74	228
Without Anglers	203	254	457
Refusals	14	8	22
Bad Address	78	79	157
Unknown/No Response	451	485	936
Totals	900	900	1800

Table A-2. Angler Survey Disposition for ABS Sample, by Stratum

Disposition	Coastal Counties	Non-Coastal Counties	Total
Total Completes	130	57	187
With Trips	43	8	51
Without Trips	87	49	136
Refusals	1	3	4
Bad Address	5	3	8
Unknown/No Response	47	16	63
Totals	183	79	262

Table A-3. Angler Survey Disposition of License Sample, Both Waves, by Stratum

Disposition	Coastal Counties	Non-Coastal Counties	Out of State	Total
Total Completes	316	307	343	966
With Trips	76	43	108	227
Without Trips	240	264	235	739
Refusals	5	9	5	19
Bad Address	51	41	47	139
Unknown/No Response	164	167	107	438
Totals	536	524	502	1562

Dual Frame Mail Survey of Fishing Effort: Project Documentation

Issued under Contract No. DG133F-09-RQ-0666

Conducted on behalf the National Oceanic and Atmospheric Administration, National Marine Fisheries Service, Office of Science and Technology

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Introduction

Since 1981, the Federal government has relied upon telephone-based general population interviews to estimate fishing effort and catch by marine recreational anglers. However, increasing issues with telephone frame coverage has caused the National Marine Fisheries Service (NMFS) to investigate alternate methodologies which may lead to increased efficiency and reduced coverage error.

As a part of this effort, the Dual-Frame Mail Survey of Fishing Effort pilot study was awarded to ICF Macro under the Blanket Purchase Agreement DG133F-09-RQ-0666.

Project Background

Historically, recreational fisheries estimates have been developed through two main components:

- An access-site intercept study (the Atlantic Coast Access Point Angler Intercept Survey, APAIS) which documents angler activity and catch; and
- Telephone surveys of fishing effort such as the Coastal Household Telephone Survey (CHTS) which primarily operate as a weighting factor to expand angler data to represent activity across all recreational fisheries.

One of the key statistics derived from the CHTS is the incidence of saltwater recreational anglers living in the coastal regions of the country. This is obtained through a relatively efficient random digit dialing (RDD) methodology targeting relevant coastal counties. However, coverage errors may weaken the integrity of resulting statistics. Specifically:

- The CHTS only incorporates traditional land–line telephone numbers in its sample frame. The National Center for Health Statistics estimated that, at the end of 2008, about one-in-seven American households received all or most calls using cellular telephones. The demographics of these households are statistically unique from those which can be contacted using a traditional landline telephone number (Blumberg & Luke, 2009).
- The CHTS limits the sample frame to areas with the highest concentrations of anglers. Specifically, non-coastal anglers and anglers active in northern states during winter months do not have a probability for selection.

In addition, the RDD effort lacks the ability to efficiently profile adequate numbers of anglers needed to produce effective fisheries management information. A significant investment is required to produce precise figures regarding a wide variety of fishing behaviors.

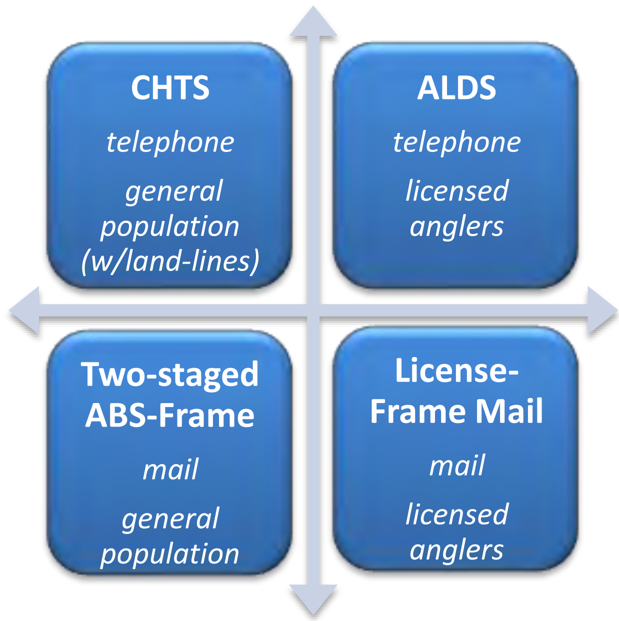
License-based angler frames promise to be a primary component to resolving MRFSS' methodological issues as the program is refined as part of the Marine Recreational Information Program (MRIP) initiative. Since early 2007, the Angler Directory License

Survey has supplied data similar to the CHTS, economically providing additional details about fishing behaviors by utilizing state-based registration databases as sample frames. However, this dual-frame approach does not resolve all MRFSS coverage issues. Specifically, registration laws provide exemptions to some anglers and not all active anglers register with the state, thereby weakening state databases.

Survey Design

A dual-frame multi-stage collection methodology has been designed to mirror current CHTS and ALDS activity, adapting the current telephone methodology to a mail-based approach.

- Similar to the CHTS, a general population survey identifies households with residents recently participating in fishing activities.
- A follow-up survey sent to anglers identified in the household survey provides detail of recent activity.
- The same follow up survey sent to select registered anglers efficiently increases the amount of angler data.



During analysis, data resulting from each mail effort may be assessed in relation to the data collected via its analogous sampling frame or collection procedure.

Sample Design: Delivery Sequence File

In order to obtain an accurate estimate of the incidence of anglers in the general population, sampling was conducted using the Delivery Sequence File (DSF) from the United States Postal Service (USPS). The DSF includes addresses with both single-family style addresses and multi-unit residential property addresses such as for apartments, condominiums, and trailer properties. Non-city style addresses (i.e. post office boxes) are not included. The Census Bureau reports that in areas where city-style addresses are prominent, people who receive mail at post office boxes will often also receive postal mail at their city-style address. This assertion has been backed by other researchers who have concluded that most people who maintain a post office box also receive postal mail at their physical residence (Iannacchione, Staab, & Redden, 2003).

Records selected from the DSF were limited to households in the State of North Carolina. Addresses were stratified into Coastal/Non-Coastal classifications consistent with CHTS sampling during November and December in North Carolina. A total of 1,800 households were selected, split evenly between the two strata.

Coastal Counties		Non-Coastal Counties			
013 Beaufort	103 Jones	001 Alamance	051 Cumberland	101 Johnston	159 Rowan
015 Bertie	107 Lenoir	003 Alexander	057 Davidson	105 Lee	161 Rutherford
017 Bladen	117 Martin	005 Alleghany	059 Davie	109 Lincoln	165 Scotland
019 Brunswick	129 New Hanover	007 Anson	063 Durham	111 McDowell	167 Stanly
029 Camden	131 Northampton	009 Ashe	067 Forsyth	113 Macon	169 Stokes
031 Carteret	133 Onslow	011 Avery	069 Franklin	115 Madison	171 Surry
041 Chowan	137 Pamlico	021 Buncombe	071 Gaston	119 Mecklenburg	173 Swain
047 Columbus	139 Pasquotank	023 Burke	075 Graham	121 Mitchell	175 Transylvania
049 Craven	141 Pender	025 Cabarrus	077 Granville	123 Montgomery	179 Union
053 Currituck	143 Perquimans	027 Caldwell	081 Guilford	125 Moore	181 Vance
055 Dare	147 Pitt	033 Caswell	085 Harnett	127 Nash	183 Wake
061 Duplin	149 Polk	035 Catawba	087 Haywood	135 Orange	185 Warren
065 Edgecombe	155 Robeson	0 Chatham	089 Henderson	145 Person	189 Watauga
073 Gates	163 Sampson	039 Cherokee	093 Hoke	151 Randolph	193 Wilkes
079 Greene	177 Tyrrell	043 Clay	097 Iredell	153 Richmond	197 Yadkin
083 Halifax	187 Washington	045 Cleveland	099 Jackson	157 Rockingham	199 Yancey
091 Hertford	191 Wayne				
095 Hyde	195 Wilson				

Sample Design: Angler Registry Frame

In order to conduct the Licensed Angler Study, a database containing approximately 551,060 million registered anglers was provided by North Carolina’s Division of Marine Fisheries. In order to prepare the sample file for sampling, the following steps were completed.

- Duplicate records matching on core information such as name, date of birth, and mailing address were also deleted.
- Records lacking fundamental information such as name, date of birth, and mailing address were eliminated from the file.
- Anglers under the age of 18 were excluded.
- Addresses were “normalized” using Satori Software’s “Mailroom Toolkit” which is designed to correct minor deviations from standard formats used by the USPS.
- Records were classified into appropriate coastal, non-coastal, or out-of-state strata groups.

- Unique household identifiers were assigned to anglers who share a common mailing address or telephone number.

The sample draw for the license-frame survey involved an n^{th} selection procedure for each stratum. A file listing households was sorted by address in order to minimize the possibility of including unidentified duplicate household listings. 450 records were selected from each stratum and designated as "original sample."

Supplemental sample was obtained from other anglers living in the same households as the original sample. At most one additional angler was selected for each household, with up to 100 secondary anglers permitted per stratum. Counts are listed in Table 1.

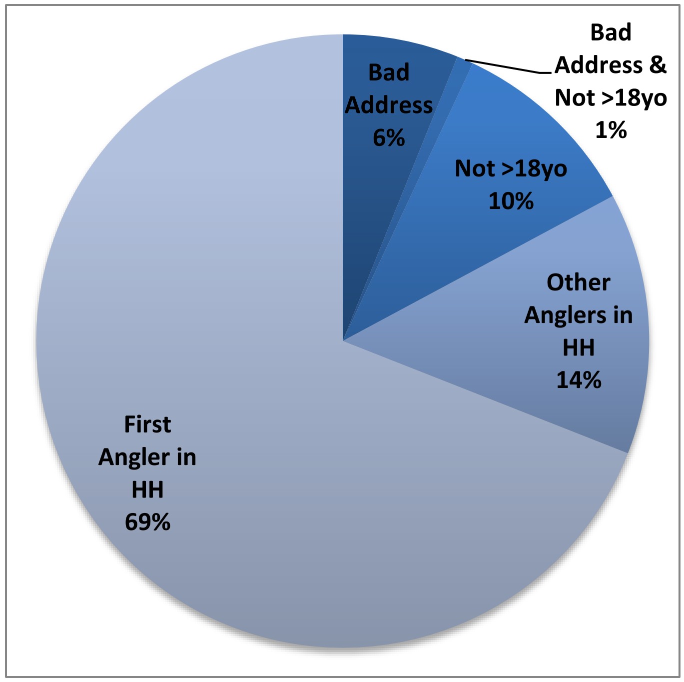


Table 1

Supplemental Records	Count
Coastal	86
Non-Coastal	74
Out-of-State	52

Questionnaire Design

Three primary data forms were designed for the study:

- An initial household screener for the ABS sample
- An angler survey for individuals identified through the household screener or using the North Carolina Angler Registry
- A trip form associated with the angler survey which captured details regarding up to four recent outings.

Questions in the mail survey were selected from key measures in the CHTS instrument. Wording modifications were required to adapt an interviewer guided telephone survey script to a self administered paper form.

Design of the initial household screener and the angler survey involved:

- Printing on 11" x 17" white paper later folded into a four page 8.5" x 11" booklet.
- A front cover incorporating the study name, NOAA logo, OMB approval number and expiration date, and informed consent information including an assurance of confidentiality. The front cover was printed in color.

- A back cover printed in color listing commonly asked questions including items involving sampling procedures, study purpose, anticipated time burden, and contact information for the survey sponsor.
- An interior spread clustering questions on the right-hand page. Major question groups were presented in shaded text boxes with response areas appearing in white. The booklet's control number appeared vertically as a form number along the crease of the booklet where it was protected from mutilation.
- The only design element appearing on the obverse of the front cover was a bar code of the respondent control number. The bar code was overlaid with a stencil of a fish, transforming it into a graphical element unlikely to be tampered with by a respondent.

A supplemental form for recording details of up to four recent trips was included with the angler questionnaire. The 11" x 17" page was printed on tan paper with black ink and folded so that all questions about each of the trips appeared independently on one 8.5" x 11" page.

Other designed components of the survey efforts included:

- A 10" x 13" white outbound envelope. The return address referenced "A Study of Fishing in NC" with the ICF Macro office location listed in the return address. NOAA's logo was prominently displayed next to the return address. The envelope was clearly marked with a "Return Service Requested" stamp to facilitate accurate classification of undeliverable pieces. Adhesive labels showing the respondent's address incorporated a unique numeric identifier to help ensure survey materials were properly matched to envelopes.
- A 9" x 12" business reply envelope (BRE). This BRE directed returns to "A Study of Fishing in NC" at the same ICF Macro office location printed on the outbound envelope.
- Cover letters. Five different cover letters were designed to motivate:
    - Households receiving an initial survey instrument,
    - Non-responding households receiving a replacement form,
    - Anglers receiving an initial angler activity survey instrument,
    - Non-responding anglers receiving a replacement forms, and
    - Non-responding anglers receiving a third and final form.

  An electronic letterhead included the NOAA logo, address, telephone number, and web address printed in color. Each motivational message displayed the signature of the NOAA's Fisheries Statistics Division's Chief, David Van Voorhees. Letters were personalized with an inside address (including the respondent's name if known).

- Postcards. Approximately one week after receiving an initial household or angler survey packet, respondents received a postcard reiterating the importance of response. Postcards were printed on white cardstock and prominently displayed the NOAA logo.

Images of survey material can be found in the appendices.

Data Collection

Assembly protocols

Household survey packets sent to the ABS participants included a cover letter, survey booklet, and business reply envelope. Initial surveys to households also included a one dollar bill clipped to the front of the packet. Outbound envelopes were stuffed with the BRE flap at the bottom of the envelope, cradling other components to ensure their orderly removal by the respondent.

Angler survey packets were assembled in a similar manner. A personalized cover letter, angler questionnaire and trip detail form were stacked and tucked into the lip of a BRE. As with the household study, initial mailings also included a dollar bill clipped to the front of the packet.

Survey materials for each mailing were sorted and printed in order of a process control number. Pieces were batched in groups of 100 and released to assembly staff by a process supervisor. If any materials were left over after assembling a batch of 100, the cause of the discrepancy was investigated and corrected. A supervisor performed a quality assurance check on approximately one out of every 10 envelopes noting proper nesting of materials and matching of all control numbers.

After assembly, packets were sealed and metered. A first-class postage rate was used in order to generate a positive impact on response rates (Fox, Crask, & Kim, 1988) and avoid possibly delays in delivery associated with second-class, third-class, or bulk mail postage rates.

Mailing protocols (issuance)

Household Sample

In an effort to optimize the timing between the household screener and angler follow-up surveys, ABS sample was split into two equal groups. Initial surveys for the first group were sent eight weeks prior to the start of the angler effort. Fielding to the remainder of the ABS sample was completed in a compressed timeline of only six weeks.

	Group 1 Extended Fielding	Group 2 Compressed Fielding
Count	900	900
Date of initial mailing	November 10, 2009	November 20, 2009
Date of postcard mailing	November 16, 2009	November 30, 2009
Date of replacement form	November 30, 2009	December 14, 2009
Fielding Window	8 weeks	6 weeks
Number of Completes	360	351

Households selected for the ABS survey were sent packets containing a $1 incentive for participation. Approximately one week later, the same households received a postcard with a reminder to complete the survey. The status of returned questionnaires were checked into a process control system using various codes including completed interview, refusal to participate, and unable to be delivered by the Postal Service. Non respondents were sent replacement survey packets including an updated cover letter but no dollar bill.

Data from all returned surveys were entered to permit the creation of a list of identified anglers. The data file was compared to the check in system to ensure a complete file for sampling.

Angler Sample

The second stage of the project involving the sampling of anglers used the same mailing procedures for anglers identified in the ABS household survey and in the North Carolina licensed angler frame. Because multiple anglers were sampled in some households, materials were personalized to include the names of anglers. If the names of anglers were not provided in the ABS household study, name fields were hand edited to include specifications such as "male angler" or "eldest female angler".

Initial packets were sent with a $1 incentive for participation. All sampled anglers received a postcard reminder to complete the survey approximately one week later. If a form had not been returned within 3 weeks, a replacement packet using a modified cover letter was sent without the monetary incentive. Those who did not return a survey within seven weeks were sent a second replacement form with a final request for participation. This last appeal was sent using Federal Express 2-day delivery.

	Anglers from ABS sample frame	Anglers from NC License Frame
Count	262	1562
Date of initial mailing	January 4, 2009	
Date of postcard mailing	January 12, 2009	
Date of replacement form	January 25, 2009	
Date of final replacement	February 18, 2009	
Number of Completes	191	985

Process Control Procedures

The mailing of all survey items and the receipt of all survey forms (regardless of completion status) were logged into a process tracking system. When available, bar code readers were used to automatically enter control numbers and minimize errors in documentation. Status codes included specific actions (e.g. mailing of initial survey packet) as well as outcome codes consistent with guidelines set by the American Association for Public Opinion Research (AAPOR).

All returned BREs were opened and grouped into batches corresponding to the day's receipts. An initial check of surveys ensured reasonable completeness and blank forms were logged into the tracking system as "refused interview". Each survey was scanned for errors or inconsistencies. Directive clarifications for data entry staff were written directly on the survey, initialed and dated by the reviewer in a distinguishable colored pencil.

Data Entry

A data entry program was created using specialized research software and incorporated range and logic checks. These checks can be described as hard edits, soft edits, and consistency checks:

- Hard Edits represent a finite permissible range for the response and trigger an error message if an unallowable value is entered into the program.
- Soft Edits represent response values that may be valid, but are viewed as extreme. These values trigger an "unlikely" message when entered by the data entry person. Data entry personnel review these responses for verification prior to entering them as data.
- Internal Consistency Edits represent programmed checks to ensure responses are consistent throughout the survey. Since these contradictions may reflect data

recorded on the form by the respondent, consistency checks operate like soft edits, flagging the data entry personnel to possible errors but not preventing the recording of data.

Standard codes for illegible or missing values were incorporated for each question. Each survey was entered into the system twice. Inconsistencies between data records were rectified to ensure digitized files accurately reflected the information provided by the respondent on the paper survey. In the case that coding decisions were not immediately clear to the data entry staff person, project management would clarify guidance directly on the survey form along with their initials and the date.

Data Cleaning Procedures

ICF Macro employed limited data cleaning on data files:

ABS Household Survey

- If the number of anglers with recent activity was detailed in Q2, Q1 may be coded to indicate the presence of anglers.

Angler Survey

- Given an indication of recent participation (e.g. in Q7 or Q8), Q1 may be marked to indicate 2009 recreational saltwater fishing activity
- If dates of trips were marked in the Q8 calendars, Q7 could be marked to indicate recreational saltwater fishing in North Carolina during November and/or December
- If valid trips were detailed, the following assumptions could be made:
  - Q1: Respondent participated in recreational saltwater fishing
  - Q7: Indication of recreational saltwater fishing in North Carolina during November and/or December
  - Q8a & Q8b: dates of saltwater activity

Trip Detail

- It was required that non-missing dates of trips must occur during November or December. Trips from other months were considered invalid.
- Missing trip dates may be transcribed from Q8 of the angler survey provided the angler made four or fewer trips and the mode of trips (boat, shore) were sequenced as expected.
- Fishing on a boat (Q2) could be assumed if details of a boating trip were provided in Q2a and Q2b.
- Fishing from the shore (Q3) could be assumed if details of a shore trip were provided in Q3a, Q3b, and Q3c.

- Additional household anglers for the trip (Q6) could be assumed if the additional fishers were described in Q6a and Q6b.

Production of the Data File

Data files were constructed with one record per selected piece of sample. Questionnaire variables for non-respondents appear as missing values within the data file. Final files were checked for consistency with process control databases. Values exceeding logical and reasonable tolerances were compared to original forms to ensure the fidelity of information.

Final data files were built to include all data from the dual-frame mail survey with one record for each sampled unit. In addition to data from the survey instrument, the following were provided:

- A unique record ID assigned to anglers,
- A household identification numbers,
- Angler number,
- Sample source (ABS-frame or license-frame),
- Stratum,
- AAPOR-based outcome codes,
- Original/supplemental record classification, and
- Reverse-matched telephone number.

A complete data dictionary can be found in *Appendix H: Data Dictionary* on page 36.

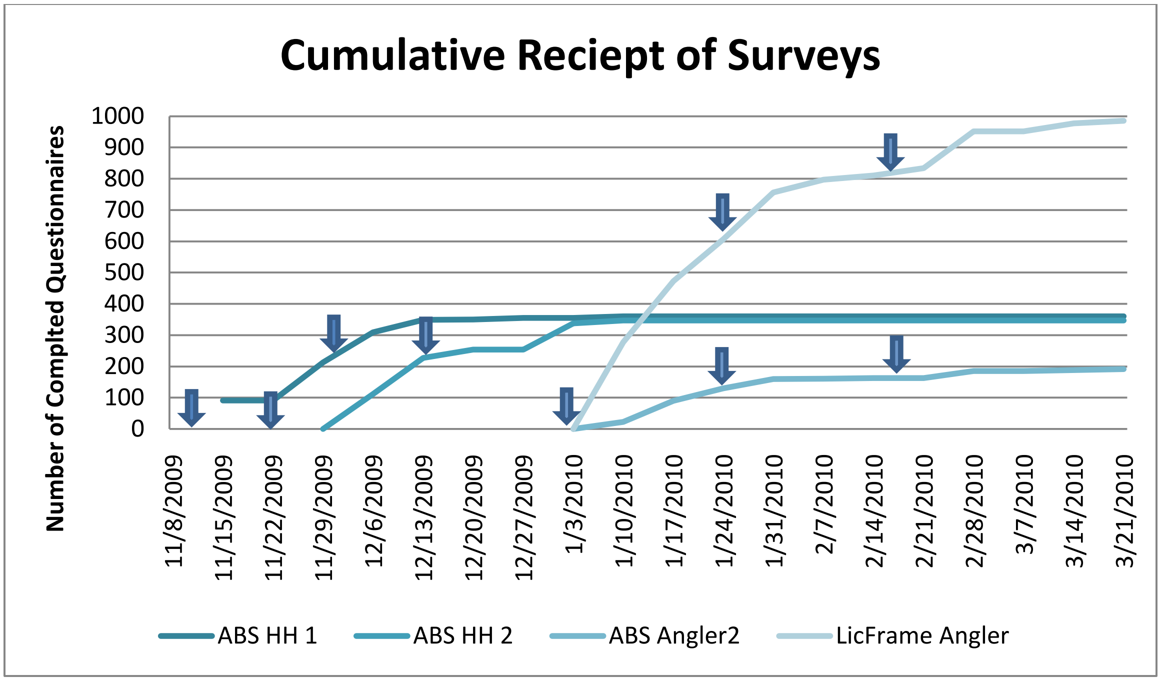
The data file will be delivered in SAS format with final content, coding, formatting, and naming conventions developed in conjunction with NMFS.

Survey Response

The survey protocol for ABS Household study resulted in a 42% response rate (measured in completes over presumably delivered surveys). The rates for the extended and compressed fielding periods were near identical. It appears that most respondents sent back forms within four weeks of the initial mailing.

A 74% response rate was achieved when contacting anglers identified in the household survey. The same survey administered to anglers identified in the license frame produced a response rate of 68%. While the majority of respondents returned forms within four to five weeks, a third mailing via Federal Express produced a swell of returns at the end of the fielding period. Approximately 10% - 15% of total returns resulted from the third mailing.

The graph below shows the cumulative receipt of surveys from each of the four efforts. Arrows mark the dates of questionnaire mailings.



Final Status of Records and Response Rates

The following tables account the final outcomes of the sample associated with each survey effort.

	ABS HH Screener	Group 1 Extended Fielding	Group 2 Compressed Fielding
Total Sent	1,800	900	900
Complete: HHs with anglers	229	113	116
Complete: HHs w/o anglers	456	235	221
Refusal	22	12	10
Undeliverable	157	77	80
Unknown outcome	936	463	473
	42%	42%	41%

	ABS Angler Study	License Frame Angler Study
Total Sent	262	1,562
Complete: Recent activity	51	227
Complete: no recent activity	137	739
Refusal	3	19
Undeliverable	8	139
Unknown outcomes	63	438
____________________	74%	68%

Limitations of the Study

There are several inherent sources of error commonly recognized in mail-based research.

Language

According to the 2006-2008 American Community Survey 3-Year Estimates, 10% of North Carolina residents speak a language other than English at home (U.S. Census Bureau). Printed materials were in English only creating a barrier to those who cannot read the language.

Coverage

Although the ABS frame contains a comprehensive set of mailing addresses, coverage issues may result through sources such as illegal housing units or households that only receive mail through a post office box.

Because the fishing activity of households in the ABS sample frame is collected using a two stage design, the completeness of the angler data file is dependent on responses to the household screening study. Non-respondents and those who go fishing for the first time in a year after completing the household screener reduce the coverage of the angler study.

Coverage issues associated with the Licensed Angler frame come from several key sources. Minors under the age of 18 are excluded from sample through license exemptions and filtering of the sample frame. Members of the Armed Forces on temporary military leave are not required to obtain a license and therefore will not appear in the registry. Illegal activity performed by those without a fishing license cannot be captured using this sample frame. Issues with the same frame, such as incorrectly entered mailing information, may be associated with specific licensing sites and could precipitate exclusion from the sampling frame. Anglers who have recently moved may be less likely to be included in final data files.

Non-Response

As with other research studies that attempt to provide close measures of representative samples, refusal rates are of concern for this study. It is commonly cited that response rates for surveys have been dropping significantly in recent years. While weighting of data will minimize many distortions, it is commonly accepted that there will be distinct differences between the attitudes and opinions of those who complete the study verses those who refuse to do so. Therefore, any response rate less than 100% indicates some level of inaccuracy in the final data. In the same vein of reasoning, the refusal of any specific question during a survey compromises the precision of its measure.

Limited protocols

The ABS Household study received two questionnaire mailings while the Angler studies received three questionnaire mailings. The final distributions for each stage resulted in significant levels of response suggesting additional completes could be obtained through additional outreach. However, this is not to say that the cost of efforts would create a proportionate benefit.

For most respondents, fishing activity will be fully documented using the current form detailing the most recent four trips. However, earlier trips of more avid anglers may not be captured. Errors could result if undocumented trips were distinct or imputed values do not match actual activity.

Response bias

Respondents can also control the accuracy of the data depending on the level of consideration and seriousness to which they approach answering the questions. Although the questionnaire forms were designed to aid cognitive processing (e.g. through the display of a calendar to mark dates of fishing activity), ultimately the respondent controls how accurate their responses are in representing their recent activities. While the added delay between activity and reporting may cause greater immediate recall issues when compared to the telephone survey, the format of a paper self administered survey should ultimately make it easier for a respondent to verify event details (e.g. by reviewing schedules, though discussions with other members of a trip, etc.).

Other sources of error involve the design of the questions themselves. Although questions originated from the long-standing CHTS, wording needed to be adjusted to accommodate a paper-based methodology. Questions and response categories should be relatively easy for most individuals to comprehend, however some respondents could have difficulty accurately responding to some questions. Unlike the CHTS, this is a self-administered questionnaire which prohibits clarification of items.

Considerations for Future Data Collection Efforts

The following may be considered for future iterations of the project:

- Continued testing of household screener fielding schedule. The number of anglers from the ABS sample qualified to receive an angler survey may quickly change, especially during springtime months. The impact of a compressed fielding period should continue to be investigated.
- Cognitive interviewing to improve the questionnaire. Topic areas might include:
  - Methods for insuring better matches between dates on the angler survey and trip detail, possibly by listing months on the trip detail as close-ended responses.
  - Clarification for reporting in-state and out-of-state trips. Currently, Q7 in the angler survey specifies trips taken in North Carolina. Respondents may inconsistently provide information about out-of-state trips in following questions.
  - Improved ways to indicate county of trip. This may include displaying a county-level map of the state where the respondent may fill in the location of the trip.
- A non-response telephone follow-up. It is common to see over 50% of mailing addresses matched to a telephone number. A large percentage of records drawn from the licensed angler registry include a telephone number. In order to maximize response, respondents could receive a reminder call requesting that they complete the survey, allowing the respondent to complete by telephone. This option could be implemented economically given the fact that the CHTS and ALDS provide the basis for the CATI system.

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Appendix A: Key Dates (timeline)

Event	Date
ABS HH Group 1: Initial Survey Packet	November 10, 2009
ABS HH Group 1: Postcard	November 16, 2009
ABS HH Group 1: Replacement Packet	November 30, 2009
ABS HH Group 2: Initial Survey Packet	November 20 , 2009
ABS HH Group 2: Postcard	November 30, 2009
ABS HH Group 2: Replacement Packet	December 14, 2009
Angler Survey: Initial Survey Packet	January 4, 2010
Angler Survey: Postcard	January 12, 2010
Angler Survey: Replacement Packet	January 25, 2010
Angler Survey: FedEx Replacement	February 15, 2010
End of Collection	March 26, 2010

Appendix B: Disposition Report

ABS Household

Outcome	Extended Fielding	Compressed Fielding	Combined
1.1 Complete (net)	348	341	689
1.1.1: Complete with Anglers	113	117	230
1.1.2: Complete without Anglers	235	224	459
2.1 Refusals	12	10	22
3.3 Undeliverable addresses	78	82	160
TOTAL COUNT	438	433	871

Angler Survey

Outcome	ABS Sample	Licensed Based Frame	Combined
1.2 Complete (net)	188	966	1,154
1.2.1: Complete with Anglers	51	227	278
1.2.2: Complete without Anglers	137	739	876
2.1 Refusals	3	19	22
3.3 Undeliverable addresses	8	139	147
TOTAL COUNT	199	1,124	1,323

Appendix C: Material for Household Questionnaire Packets

Household questionnaire packets were comprised of:

- A customized cover letter from NOAA,
- A booklet style questionnaire, and
- A business reply envelope (BRE).

Initial mailings also included a dollar bill.

November 30, 2009

North Carolina Resident
«street»
«city», «state» «zip5»

Dear North Carolina Resident,

I am writing to ask you for your help in a study being conducted for the National Oceanic and Atmospheric Administration. This study is part of an effort to learn more about recreational fishing activities in North Carolina.

The purpose of this questionnaire is to identify people who fish. However, it is important for us to obtain responses from people who do fish as well as those who do not participate in recreational saltwater fishing. Your address was randomly selected from a list of all home addresses in North Carolina. Your household represents thousands of other households like yours. Only with participation by everyone selected will the findings from the study represent everyone in North Carolina. If there are people who fish in your household, we may send them a second questionnaire to learn about their recreational saltwater fishing experiences. We have enclosed a small token of appreciation as a way of saying thanks for your help.

Your answers are completely confidential and will be used for statistical purposes only in accordance with the Privacy Act of 1974. You are not required to answer any question that you feel is an intrusion of your privacy.

If you have any questions or comments about this study, we would be happy to talk with you. Please contact Rob Andrews at his number (301-713-2328) or you can write to us at the address at the bottom of this letter.

Thank you very much for your help with this important study. Please return your completed questionnaire in the postage paid envelope provided.

Sincerely,

Dave Van Voorhees,
Chief, Fisheries Statistics Division

«ID»

1315 East-West Hwy, Silver Spring, Maryland 20910
Phone: 301-713-2328 Internet: www.st.nmfs.gov

November 30, 2009

North Carolina Resident
«street»
«city», «state» «zip5»

Dear North Carolina Resident,

About three weeks ago we sent a questionnaire to your household in an effort to learn more about recreational fishing activities in North Carolina. According to our records, your household has not yet returned a completed questionnaire.

Your address was randomly selected from a list of all home addresses in North Carolina. Only with participation by everyone selected will the findings from the study represent everyone in North Carolina. Many North Carolinians have told us about their fishing behaviors. Your completed questionnaire will contribute to our understanding of the state's fishing activity.

This study being conducted for the National Oceanic and Atmospheric Administration. It is important for us to obtain information from people who do fish as well as those who do not participate in recreational saltwater fishing. If there are people who fish in your household, we may send them a second questionnaire to learn more about their recreational saltwater fishing experiences.

Your answers are completely confidential and will be used for statistical purposes only in accordance with the Privacy Act of 1974. You are not required to answer any question that you feel is an intrusion of your privacy.

If you have any questions or comments about this study, we would be happy to talk with you. Please contact Rob Andrews at his number (301-713-2328) or you can write to us at the address at the bottom of this letter.

We hope that you will take a few minutes to fill out and return the questionnaire soon. If, for any reason, you prefer not to answer it, please let us know by returning a note or blank questionnaire in the postage paid envelope provided.

Sincerely,

Dave Van Voorhees,
Chief, Fisheries Statistics Division

«ID»

1315 East-West Hwy, Silver Spring, Maryland 20910
Phone: 301-713-2328 Internet: www.st.nmfs.gov

Commonly Asked Questions

Q How did you get my address?

A *Your address was randomly selected from among all of North Carolina's addresses. It was selected using scientific sampling methods to represent other households in your part of the state.*

Q Nobody in my household fishes. Should I respond to the survey?

A *Yes. It is important that we gather information about households that do not fish as well as those who do. Once we receive your completed questionnaire, you will not be sent any additional mailings such as replacement questionnaires.*

Q Why can't you interview another household instead of mine?

A *In order to make sure final results of the study are accurate, receiving information about your household is important to us. Households selected for this study were chosen using scientific sampling methods and your responses cannot be replaced by others.*

Q How will the information I provide be used?

A *Information from this study will be used to improve the monitoring of North Carolina's fishing activity. All information will be kept confidential; It will be combined with information from other households to produce statistical summaries and reports.*

Q How much time will this survey take?

A *On average, it should take less than five minutes for you to respond, including the time for reviewing instructions, and completing and reviewing the collection of information.*

Q Who is sponsoring the survey?

A *This study is sponsored by the National Oceanic and Atmospheric Administration. Questions regarding the study can be directed to Rob Andrews at National Oceanic and Atmospheric Administration by calling 301-713-2328.*

OMB No: 0648-0052 Approval Expires 04/30/2011

A Study of Fishing In North Carolina

This study is being conducted with the assistance of the National Oceanic and Atmospheric Administration in accordance with the Magnuson-Stevens Fishery Conservation and Management Act of 2006. Your participation is voluntary. All responses will be kept confidential under the Privacy Act of 1974.

The information you provide will be combined with information provided by other participants to produce statistical summaries and reports.

A Study of Fishing in North Carolina

This form is for households with and without people who fish. It just takes a few minutes and your answers will be confidential.

1. In the last 12 months has anyone living in your household, including children and adults, been recreational saltwater fishing in the U.S. or a U.S. territory?

☐ Yes

☐ No ➞ *(please go to question 6)*

Recreational saltwater fishing refers to fishing primarily with hook and line for pleasure, amusement, relaxation, or home consumption in oceans, bays, inlets, canals, intra-coastal waterways, and brackish portions affected by the tides. Inland saltwater bodies includes sounds, passes, inlets, bays, estuaries, brackish portions of rivers, and other areas of salt or brackish water like bayous and canals.

2. How many people in this household (including children and adults) have participated in recreational saltwater fishing during the past 12 months?

☐☐ *Number of people who have fished*

Please tell us the following for each member of the household who has gone saltwater fishing during the past 12 months. If more than four household members have participated in recreational saltwater fishing during the past 12 months, please list the four oldest participants.

	Fisher 1	Fisher 2	Fisher 3	Fisher 4
3. What is the person's first name?	☐	☐	☐	☐
4. What is the person's sex?	☐ Male ☐ Female	☐ Male ☐ Female	☐ Male ☐ Female	☐ Male ☐ Female
5. How old is the person?	☐ Less than 16 ☐ 16-18 ☐ 18-24 ☐ 25-34 ☐ 35-44 ☐ 45-54 ☐ 55-64 ☐ 65 or older	☐ Less than 16 ☐ 16-18 ☐ 18-24 ☐ 25-34 ☐ 35-44 ☐ 45-54 ☐ 55-64 ☐ 65 or older	☐ Less than 16 ☐ 16-18 ☐ 18-24 ☐ 25-34 ☐ 35-44 ☐ 45-54 ☐ 55-64 ☐ 65 or older	☐ Less than 16 ☐ 16-18 ☐ 18-24 ☐ 25-34 ☐ 35-44 ☐ 45-54 ☐ 55-64 ☐ 65 or older

6. For statistical purposes, we are interested in learning about your telephone usage. Is there at least one phone inside your home that is currently working and not a cell phone?

☐ Yes

☐ No

Thank you! Please return this form in the postage paid envelope provided or mail to:
A Study of Fishing in North Carolina
116 John Street, Suite 800
New York, NY 10038

Appendix D: Household Survey Reminder Postcard

Date

Last week a questionnaire was sent to you on behalf of the National Oceanic and Atmospheric Administration. Your household was randomly selected from a list of all households addresses in the state of North Carolina.

If you have already completed and returned the questionnaire to us, please accept our sincere thanks. If not, please do so today. Information collected in this study will be used to learn more about recreational fishing activities in North Carolina.

If you did not receive a questionnaire, or if it was misplaced, please call Rob Andrews at his number (301-713-2328).

Thank you.

Dave Van Voorhees
Chief, Fisheries Statistics Division

NORTH CAROLINA RESIDENT
Street
Town, City, #####

Appendix E: Material for Angler Questionnaire Packets

Questionnaire packets for anglers were comprised of:

- A customized cover letter from NOAA,
- A booklet style questionnaire for detailing angler activity,
- A booklet-style questionnaire for detailing up to 4 recent trips,and
- A business reply envelope (BRE).

Initial mailings also included a dollar bill.

January 6th, 2009

«NAME»
«ADDRESS»
«CITY», «ST» «ZIP»

Dear «Name__Proper»

I am writing to ask you for your help in a study being conducted for the National Oceanic and Atmospheric Administration. This study is part of an effort to learn more about recreational fishing activities in North Carolina.

Earlier this year, your address was randomly selected from among all of the home addresses in the state of North Carolina. From among those households that responded to our earlier questionnaire, we selected a random sample of recreational saltwater anglers to learn about the frequency of their fishing trips and some information about those trips, such as where and when the fishing trip occurred. Information collected in this study will be used to evaluate the impact of recreational fishing on natural fishing resources and help improve fisheries management policies. We have enclosed a small token of appreciation as a way of saying thanks for your help.

Your answers are completely confidential and will be used for statistical purposes only in accordance with the Privacy Act of 1974. You are not required to answer any question that you feel is an intrusion of your privacy.

If you have any questions or comments about this study, we would be happy to talk with you. Please contact Rob Andrews at his number (301-713-2328) or you can write to us at the address at the bottom of this letter.

Thank you very much for your help with this important study. Please return your completed questionnaire in the postage paid envelope provided.

Sincerely,

Dave Van Voorhees,
Chief, Fisheries Statistics Division

<ID>

1315 East-West Hwy, Silver Spring, Maryland 20910
Phone: 301-713-2328 Internet: www.st.nmfs.gov

January 5th, 2009

«NAME»
«ADDRESS»
«CITY», «ST» «ZIP»

Dear «Name__Proper»

I am writing to ask you for your help in a study being conducted for the National Oceanic and Atmospheric Administration. This study is part of an effort to learn more about recreational saltwater fishing activities in North Carolina.

Your name was randomly selected using scientific sampling methods from among a list of persons who has purchased a saltwater fishing license in the state of North Carolina during the past year. We are contacting a random sample of recreational saltwater anglers to learn about the frequency of their fishing trips and some information about those trips, such as where and when the fishing trip occurred. Information collected in this study will be used to evaluate the impact of recreational fishing on natural resources and help improve fisheries management policies. We have enclosed a small token of appreciation as a way of saying thanks for your help.

Your answers are completely confidential and will be used for statistical purposes only in accordance with the Privacy Act of 1974. You are not required to answer any question that you feel is an intrusion of your privacy.

If you have any questions or comments about this study, we would be happy to talk with you. Please contact Rob Andrews at his number (301-713-2328) or you can write to us at the address at the bottom of this letter.

Thank you very much for your help with this important study. Please return your completed questionnaire in the postage paid envelope provided.

Sincerely,

Dave Van Voorhees,
Chief, Fisheries Statistics Division

«ID»

1315 East-West Hwy, Silver Spring, Maryland 20910
Phone: 301-713-2328 Internet: www.st.nmfs.gov

Commonly Asked Questions

Q How did you get my address?

A *Your address was either randomly selected from a database of licensed anglers in North Carolina or you completed a similar mail survey in the past two months. Addresses were selected using scientific sampling methods to represent other households in your part of the state.*

Q Nobody in my household fished in the past few months. Should I respond to the survey?

A *Yes. It is important that we gather information about households that do not fish as well as those who do. Once we receive your completed questionnaire, you will not be sent any additional mailings such as replacement questionnaires.*

Q Why can't you interview another household instead of mine?

A *In order to make sure final results of the study are accurate, receiving information about your household is important to us. Households selected for this study were chosen using scientific sampling methods and your responses cannot be replaced by others.*

Q How will the information I provide be used?

A *Information from this study will be used to improve the monitoring of North Carolina's fishing activity. All information will be kept confidential; It will be combined with information from other households to produce statistical summaries and reports.*

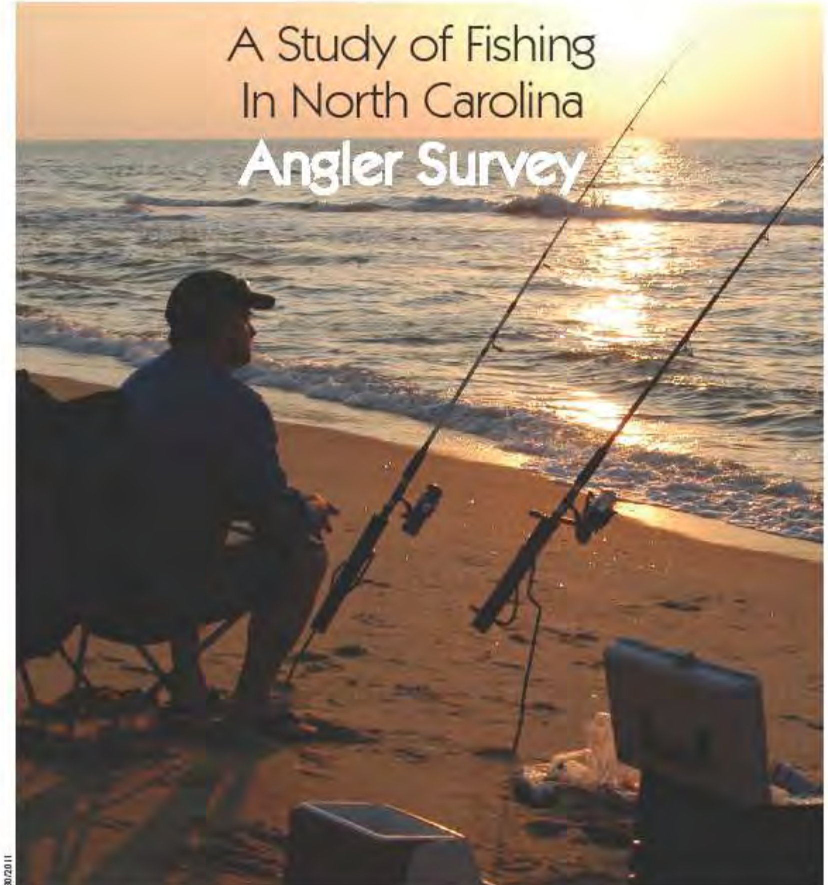
Q How much time will this survey take?

A *On average, it should take less than five minutes for you to respond, including the time for reviewing instructions, and completing and reviewing the collection of information.*

Q Who is sponsoring the survey?

A *This study is sponsored by the National Oceanic and Atmospheric Administration. Questions regarding the study can be directed to Rob Andrews at National Oceanic and Atmospheric Administration by calling 301-713-2328.*

OMB No: 0648-0052 Approval Expires 04/30/2011



This study is being conducted with the assistance of the National Oceanic and Atmospheric Administration in accordance with the Magnuson-Stevens Fishery Conservation and Management Act of 2006. Your participation is voluntary. All responses will be kept confidential under the Privacy Act of 1974.

The information you provide will be combined with information provided by other participants to produce statistical summaries and reports.

A Study of Fishing in North Carolina: Angler Survey

This form will take just a few minutes to complete and your answers will be confidential.

Thank you for completing this survey. Here are some terms we will be using:

- Saltwater fishing includes fishing in oceans, sounds, or bays, or in brackish portions of rivers. This does not include fishing in freshwater, or for shellfish, such as crabbing
- Recreational fishing means the primary purpose of the fishing is for fun or relaxation, as opposed to providing income from the sale of fish
- Private boat refers to any boat owned by an individual where no fee is paid to the individual for the use of the boat

1. During 2009, did you participate in *recreational saltwater fishing*?

☐ Yes
☐ No

2. Are you…?

☐ Male
☐ Female

3. How old are you?

☐ Less than 16
☐ 16 – 17
☐ 18 – 24
☐ 25 – 34
☐ 35 – 44
☐ 45 – 54
☐ 55 – 64
☐ 65 or older

4. During the past 12 months, did you have a fishing license for the state of North Carolina?

☐ Yes
☐ No → *(PLEASE GO TO QUESTION 7)*

5. Was this particular license for *recreational saltwater fishing*?

☐ Yes
☐ No → *(PLEASE GO TO QUESTION 7)*

6. Was this license valid for the month of November this year?

☐ Yes
☐ No

7. Between November 1st, 2009 and December 31st, 2009, did you go *recreational saltwater fishing* in North Carolina or from a *private boat* launched in North Carolina?

☐ Yes
☐ No

Even if you did not go saltwater fishing during November or December, it's important for us to get a response from every person selected for this study. Please return your now completed questionnaire to us in the enclosed prepaid return envelope.

8a. Please circle the day(s) when you went *recreational saltwater fishing* using a *private boat* on the calendar below.

MONTH	S	M	T	W	R	F	S
NOVEMBER	01	02	03	04	05	06	07
	08	09	10	11	12	13	14
	15	16	17	18	19	20	21
	22	23	24	25	26	27	28
	29	30					
DECEMBER			01	02	03	04	05
	06	07	08	09	10	11	12
	13	14	15	16	17	18	19
	20	21	22	23	24	25	26
	27	28	29	30	31		

8b. Please circle the day(s) when you went *recreational saltwater fishing* from the shore, pier, dock, or any other land based fishing on the calendar below.

MONTH	S	M	T	W	R	F	S
NOVEMBER	01	02	03	04	05	06	07
	08	09	10	11	12	13	14
	15	16	17	18	19	20	21
	22	23	24	25	26	27	28
	29	30					
DECEMBER			01	02	03	04	05
	06	07	08	09	10	11	12
	13	14	15	16	17	18	19
	20	21	22	23	24	25	26
	27	28	29	30	31		

Please use the enclosed tan colored booklet to provide information about fishing trips you took during November and December. The booklet contains room for reporting your most recent four trips.

4th Most Recent Trip

The following questions ask information about fishing trips taken during the previous two months. If you fished between November 1, 2009 and December 31, 2009 please fill out information for your four most recent saltwater recreational fishing trips. Once completed, please return your questionnaire to us in the enclosed prepaid return envelope.

1. Please enter the date of your most recent trip:

Month ☐☐ Day ☐☐

2. Did you fish from a boat on this date?
- ☐ Yes
- ☐ No ➔ (PLEASE GO TO QUESTION 3)

2a. What state and county were you in when the boat landed and you stepped off of the boat?

State
County, Parish, Island or Municipality Name

2b. Does the public have access to the place where the boat landed?
- ☐ Yes, Public Access
- ☐ No, Private Access

3. Did you fish from the shore on this date?
- ☐ Yes
- ☐ No ➔ (PLEASE GO TO QUESTION 4)

3a. Where were you fishing?

State
County, Parish, Island or Municipality Name

3b. Was that from a...?
(CHECK ALL THAT APPLY)
- ☐ Pier
- ☐ Dock
- ☐ Jetty or Breakwater
- ☐ Bridge or Causeway
- ☐ Other man-made structure
- ☐ Bank or Beach

3c. Does the public have access to this place?
- ☐ Yes, Public Access
- ☐ No, Private Access

4. Was most of your fishing effort this day in ...?
(CHECK ONLY ONE)
- ☐ Ocean, within 3 miles from the shore
- ☐ Ocean, more than 3 miles from the shore
- ☐ Sound
- ☐ River
- ☐ Bay
- ☐ Inlet
- ☐ Someplace else

5. At approximately what time did this fishing trip end?
(CHECK ONLY ONE)
- ☐ Midnight – 3:00 a.m.
- ☐ 3:00 a.m. – 6:00 a.m.
- ☐ 6:00 a.m. – 9:00 a.m.
- ☐ 9:00 a.m. – Noon
- ☐ Noon – 3:00 p.m.
- ☐ 3:00 p.m. – 6:00 p.m.
- ☐ 6:00 p.m. – 9:00 p.m.
- ☐ 9:00 p.m. – Midnight

6. Did anyone else from your household, including children and adults, fish with you on this date?
- ☐ Yes
- ☐ No

6a. Who else from your household fished with you on this date?
(CHECK ALL THAT APPLY)
- ☐ Spouse/partner
- ☐ Child(ren)
- ☐ Other household member

6b. Including you, how many household members went saltwater fishing on this date?

Number of household members ☐☐

1. Please enter the date of your most recent trip:

Month ☐☐ Day ☐☐

2. Did you fish from a boat on this date?
- ☐ Yes
- ☐ No ➔ (PLEASE GO TO QUESTION 3)

2a. What state and county were you in when the boat landed and you stepped off of the boat?

State
County, Parish, Island or Municipality Name

2b. Does the public have access to the place where the boat landed?
- ☐ Yes, Public Access
- ☐ No, Private Access

3. Did you fish from the shore on this date?
- ☐ Yes
- ☐ No ➔ (PLEASE GO TO QUESTION 4)

3a. Where were you fishing?

State
County, Parish, Island or Municipality Name

3b. Was that from a...?
(CHECK ALL THAT APPLY)
- ☐ Pier
- ☐ Dock
- ☐ Jetty or Breakwater
- ☐ Bridge or Causeway
- ☐ Other man-made structure
- ☐ Bank or Beach

3c. Does the public have access to this place?
- ☐ Yes, Public Access
- ☐ No, Private Access

4. Was most of your fishing effort this day in ...?
(CHECK ONLY ONE)
- ☐ Ocean, within 3 miles from the shore
- ☐ Ocean, more than 3 miles from the shore
- ☐ Sound
- ☐ River
- ☐ Bay
- ☐ Inlet
- ☐ Someplace else

5. At approximately what time did this fishing trip end?
(CHECK ONLY ONE)
- ☐ Midnight – 3:00 a.m.
- ☐ 3:00 a.m. – 6:00 a.m.
- ☐ 6:00 a.m. – 9:00 a.m.
- ☐ 9:00 a.m. – Noon
- ☐ Noon – 3:00 p.m.
- ☐ 3:00 p.m. – 6:00 p.m.
- ☐ 6:00 p.m. – 9:00 p.m.
- ☐ 9:00 p.m. – Midnight

6. Did anyone else from your household, including children and adults, fish with you on this date?
- ☐ Yes
- ☐ No ➔ (PLEASE CONTINUE TO YOUR NEXT MOST RECENT TRIP)

6a. Who else from your household fished with you on this date?
(CHECK ALL THAT APPLY)
- ☐ Spouse/partner
- ☐ Child(ren)
- ☐ Other household member

6b. Including you, how many household members went saltwater fishing on this date?

Number of household members ☐☐ ➔ (PLEASE CONTINUE TO YOUR NEXT MOST RECENT TRIP)

2nd Most Recent Trip

1. **Please enter the date of your most recent trip:**
   Month ☐☐ Day ☐☐
2. **Did you fish from a boat on this date?**
   - ☐ Yes
   - ☐ No ➡ *(PLEASE GO TO QUESTION 3)*

   2a. **What state and county were you in when the boat landed and you stepped off of the boat?**

State
County, Parish, Island or Municipality Name

   2b. **Does the public have access to the place where the boat landed?**
   - ☐ Yes, Public Access
   - ☐ No, Private Access
3. **Did you fish from the shore on this date?**
   - ☐ Yes
   - ☐ No ➡ *(PLEASE GO TO QUESTION 4)*

   3a. **Where were you fishing?**

State
County, Parish, Island or Municipality Name

   3b. **Was that from a...?**
   (CHECK ALL THAT APPLY)
   - ☐ Pier
   - ☐ Dock
   - ☐ Jetty or Breakwater
   - ☐ Bridge or Causeway
   - ☐ Other man-made structure
   - ☐ Bank or Beach

   3c. **Does the public have access to this place?**
   - ☐ Yes, Public Access
   - ☐ No, Private Access
4. **Was most of your fishing effort this day in ...?**
   (CHECK ONLY ONE)
   - ☐ Ocean, within 3 miles from the shore
   - ☐ Ocean, more than 3 miles from the shore
   - ☐ Sound
   - ☐ River
   - ☐ Bay
   - ☐ Inlet
   - ☐ Someplace else
5. **At approximately what time did this fishing trip end?**
   (CHECK ONLY ONE)
   - ☐ Midnight – 3:00 a.m.
   - ☐ 3:00 a.m. – 6:00 a.m.
   - ☐ 6:00 a.m. – 9:00 a.m.
   - ☐ 9:00 a.m. – Noon
   - ☐ Noon – 3:00 p.m.
   - ☐ 3:00 p.m. – 6:00 p.m.
   - ☐ 6:00 p.m. – 9:00 p.m.
   - ☐ 9:00 p.m. – Midnight
6. **Did anyone else from your household, including children and adults, fish with you on this date?**
   - ☐ Yes
   - ☐ No ➡ *(PLEASE CONTINUE TO YOUR NEXT MOST RECENT TRIP)*

   6a **Who else from your household fished with you on this date?**
   (CHECK ALL THAT APPLY)
   - ☐ Spouse/partner
   - ☐ Child(ren)
   - ☐ Other household member

   6b. **Including you, how many household members went saltwater fishing on this date?**

   Number of household members
   ☐☐ ➡ *(PLEASE CONTINUE TO YOUR NEXT MOST RECENT TRIP)*

3rd Most Recent Trip

1. **Please enter the date of your most recent trip:**
   Month ☐☐ Day ☐☐
2. **Did you fish from a boat on this date?**
   - ☐ Yes
   - ☐ No ➡ *(PLEASE GO TO QUESTION 3)*

   2a. **What state and county were you in when the boat landed and you stepped off of the boat?**

State
County, Parish, Island or Municipality Name

   2b. **Does the public have access to the place where the boat landed?**
   - ☐ Yes, Public Access
   - ☐ No, Private Access
3. **Did you fish from the shore on this date?**
   - ☐ Yes
   - ☐ No ➡ *(PLEASE GO TO QUESTION 4)*

   3a. **Where were you fishing?**

State
County, Parish, Island or Municipality Name

   3b. **Was that from a...?**
   (CHECK ALL THAT APPLY)
   - ☐ Pier
   - ☐ Dock
   - ☐ Jetty or Breakwater
   - ☐ Bridge or Causeway
   - ☐ Other man-made structure
   - ☐ Bank or Beach

   3c. **Does the public have access to this place?**
   - ☐ Yes, Public Access
   - ☐ No, Private Access
4. **Was most of your fishing effort this day in ...?**
   (CHECK ONLY ONE)
   - ☐ Ocean, within 3 miles from the shore
   - ☐ Ocean, more than 3 miles from the shore
   - ☐ Sound
   - ☐ River
   - ☐ Bay
   - ☐ Inlet
   - ☐ Someplace else
5. **At approximately what time did this fishing trip end?**
   (CHECK ONLY ONE)
   - ☐ Midnight – 3:00 a.m.
   - ☐ 3:00 a.m. – 6:00 a.m.
   - ☐ 6:00 a.m. – 9:00 a.m.
   - ☐ 9:00 a.m. – Noon
   - ☐ Noon – 3:00 p.m.
   - ☐ 3:00 p.m. – 6:00 p.m.
   - ☐ 6:00 p.m. – 9:00 p.m.
   - ☐ 9:00 p.m. – Midnight
6. **Did anyone else from your household, including children and adults, fish with you on this date?**
   - ☐ Yes
   - ☐ No ➡ *(PLEASE CONTINUE TO YOUR NEXT MOST RECENT TRIP)*

   6a **Who else from your household fished with you on this date?**
   (CHECK ALL THAT APPLY)
   - ☐ Spouse/partner
   - ☐ Child(ren)
   - ☐ Other household member

   6b. **Including you, how many household members went saltwater fishing on this date?**

   Number of household members
   ☐☐ ➡ *(PLEASE CONTINUE TO YOUR NEXT MOST RECENT TRIP)*

Appendix F: Angler Survey Reminder Postcard

Date

Last week a questionnaire was sent to you on behalf of the National Oceanic and Atmospheric Administration. Your household was randomly selected from a list of all households addresses in the state of North Carolina.

If you have already completed and returned the questionnaire to us, please accept our sincere thanks. If not, please do so today. Information collected in this study will be used to learn more about recreational fishing activities in North Carolina.

If you did not receive a questionnaire, or if it was misplaced, please call Rob Andrews at his number (301-713-2328).

Thank you.

Dave Van Voorhees
Chief, Fisheries Statistics Division

NORTH CAROLINA RESIDENT
Street
Town, City, #####

Appendix G: Coding of Text Questions

All responses to questions in the survey were pre-coded with the exception of location of fishing trip. Responses were coded to the county level using Federal Information Processing Standards (FIPS) codes. FIPS codes for North Carolina are provided below.

37001 Alamance County	37051 Cumberland County	37101 Johnston County	37151 Randolph County
37003 Alexander County	37053 Currituck County	37103 Jones County	37153 Richmond County
37005 Alleghany County	37055 Dare County	37105 Lee County	37155 Robeson County
37007 Anson County	37057 Davidson County	37107 Lenoir County	37157 Rockingham County
37009 Ashe County	37059 Davie County	37109 Lincoln County	37159 Rowan County
37011 Avery County	37061 Duplin County	37111 McDowell County	37161 Rutherford County
37013 Beaufort County	37063 Durham County	37113 Macon County	37163 Sampson County
37015 Bertie County	37065 Edgecombe County	37115 Madison County	37165 Scotland County
37017 Bladen County	37067 Forsyth County	37117 Martin County	37167 Stanly County
37019 Brunswick County	37069 Franklin County	37119 Mecklenburg County	37169 Stokes County
37021 Buncombe County	37071 Gaston County	37121 Mitchell County	37171 Surry County
37023 Burke County	37073 Gates County	37123 Montgomery County	37173 Swain County
37025 Cabarrus County	37075 Graham County	37125 Moore County	37175 Transylvania County
37027 Caldwell County	37077 Granville County	37127 Nash County	37177 Tyrrell County
37029 Camden County	37079 Greene County	37129 New Hanover County	37179 Union County
37031 Carteret County	37081 Guilford County	37131 Northampton County	37181 Vance County
37033 Caswell County	37083 Halifax County	37133 Onslow County	37183 Wake County
37035 Catawba County	37085 Harnett County	37135 Orange County	37185 Warren County
37037 Chatham County	37087 Haywood County	37137 Pamlico County	37187 Washington County
37039 Cherokee County	37089 Henderson County	37139 Pasquotank County	37189 Watauga County
37041 Chowan County	37091 Hertford County	37141 Pender County	37191 Wayne County
37043 Clay County	37093 Hoke County	37143 Perquimans County	37193 Wilkes County
37045 Cleveland County	37095 Hyde County	37145 Person County	37195 Wilson County
37047 Columbus County	37097 Iredell County	37147 Pitt County	37197 Yadkin County
37049 Craven County	37099 Jackson County	37149 Polk County	37199 Yancey County

Appendix H: Data Dictionary

ABS Household Screener

There is one record for every sampled address, regardless of the final outcome associated with the record.

Question	Field Name	Description	Coding Scheme
	HH_ID	Unique household identifier	
	MATCH_FLG	Was the household address successfully matched to the license frame? Is the household on both sample frames?	Yes=1, No=0
	STRATUM	Coastal, non-coastal, out-of-state	Coastal=1, Non-Coastal=2, Out-of-state=3
	RES_ST	State of residence	37 = North Carolina
	RES_CNTY	County of residence	
	RES_ADDRESS	Address of residence	
	HH_STATUS	Disposition of sample (complete with anglers, complete no anglers, refuse, non-contact, bad address)	1.1.1 = Household with angler 1.1.2 = Household with no angler 2.1 = Refused 3.3 = Mailing returned undelivered
Q1	FISH12_FLG	Fishing household flag. Did anyone in the household fish during previous 12 months?	Yes=1, No=0, 8 = Missing
Q2	FF12	How many people in HH fished during previous 12 months?	
Q6	HH_PHN_FLG	Does HH have a landline telephone?	Yes=1, No=0, 8 = Missing
	REC_DATE	Date questionnaire was received by contractor	
	MAIL_DATE	Date questionnaire was mailed by contractor (initial mailing)	
	SURV_YEAR	Survey year	
	SURV_WAVE	Survey wave	
	SAMP_WT	Sample weight (N/n)	
	FRM_SIZE	Number of HH units on sample frame for stratum (N)	
	BATCH	Wave 1 or Wave 2	

ABS Angler

There is one record for every angler identified in the household screener, regardless of whether or not the angler was sampled or returned a questionnaire.

Question	Field Name	Description	Coding Scheme
	HH_ID	Unique household identification number	
	ANG_ID	Unique identification for anglers within a household	
	HH_ANGLERS	Number of anglers uniquely identified in screener questionnaire (screener Q3).	
Q4	GENDER		Male=1, Female=2
Q5	AGE		Less than 16 = 1 16 -- 17 = 2 18 -- 24 = 3 25 -- 34 = 4 35 -- 44 = 5 45 -- 54 = 6 55 -- 64 = 7 65 or older = 8
	SAMP_FLG	Identifies anglers that were sampled from angler frame.	Primary angler=1, Supplemental angler=2, Not sampled=3
	ANG_STATUS	Final disposition of second-stage sample (complete with trips, complete no trips, refusal, non-contact, etc.)	1.2.1 = Trips taken in the 2 month period 1.2.2 = No trips taken in the 2 month period 2.1 = Refused 3.19 = Nothing ever returned 3.3 = Mailing returned undelivered
	REC_DATE	Date questionnaire was received by contractor	
	MAIL_DATE	Date questionnaire was mailed by contractor (initial mailing)	
Q1	FISH_YEAR_FLG	Did angler fish during 2009?	Yes=1, No=0, 88 = Missing
Q4	LICENSE_FLG	Did angler have a NC fishing license during previous 12 months (Y/N)?	Yes=1, No=0, 88 = Missing
Q5	SALT_LIC_FLG	Was license for recreational saltwater fishing? (Y/N)	Yes=1, No=0, 88 = Missing
Q6	WAVE_LIC_FLG	Was license valid during	Yes=1, No=0, 88 = Missing

		November 2009? (Y/N)	
Q7	FISH_WAVE_FLG	Did angler fish during the wave (wave 6, 2009)?	Yes=1, No=0, 88 = Missing
Q8	BOAT_TRPS	Number of private boat trips during the wave	
Q9	SHORE_TRPS	Number of shore trips during the wave	

License Angler

There is one record for every angler identified in the household screener, regardless of whether or not the angler was sampled or returned a questionnaire.

Question	Field Name	Description	Coding Scheme
	SURV_YEAR	Survey year	2009
	SURV_WAVE	Survey wave	6
	LIC_ST	License state (will be 37 (NC) in all cases for pilot study)	CRFL Infant = 1 CRFL Youth = 2 Res CRFL = 3 Res CRFL 10-Day = 4 Res CRFL Adult = 5 NonRes CRFL = 6 NonRes CRFL 10-Day = 7 NR CRFL Adult = 8 Age 65 CRFL = 9 Disabled Vet CRFL = 10 Totally Disabled CRFL = 11 Perm Disabled State Fish w CRFL = 12 Uni Adlt Care Hme Inland/CRFL = 13 Uni Blind Inland/CRFL = 14 Unified Inland/CRFL = 15 Unified Sptm/CRFL = 16 Ltime Unified Inland/CRFL = 17 Ltime Comp Inland Fish w/CRFL = 18 Subsis Inland/CRFL Waiver = 19 Disabled Combo H/F/CRFL Basic = 20 Sportsman Infant w CRFL = 21 Unified Sptm/CRFL Infant = 22 Sportsman Youth w CRFL = 23 Unified Sptm/CRFL Youth = 24 Res Sportsman Adult w CRFL = 25 Res Uni Sptm/CRFL Adult = 26 NonRes Sportsman Adult w CRFL = 27 NR Uni Sptm/CRFL Adult = 28 Unified Age 65 Sptm/CRFL = 29 Res Ltime Over 70 Sportsman w CRFL = 30 Ltime H/F/Trap/CRFL Disabled Vet = 31 Lifetime Comp Over 70 Fish w CRFL = 32 Disabled Sportsman w CRFL = 33 Uni Disabled Vet Sptm/CRFL = 34 Uni Totally Disabled Sptm/CRFL = 35
	REC_DATE	Date questionnaire received	
	MAIL_DATE	Date questionnaire was sent (initial mailing)	
	ANG_ID	Unique angler identification	

	HH_ID	Unique household identifier	
	ANG_STATUS	Final disposition of sample (complete with trips, complete no trips, refusal, non-contact, etc.)	1.2.1 = No trips taken in the 2 month period 1.2.2 = Trips taken in the 2 month period 2.1 = Refused 3.19 = Nothing ever returned 3.3 = Mailing returned undelivered
	STRATUM	Coastal, non-coastal, out-of-state	Coastal=1, Non-Coastal=2, Out-of-state=3
	RES_ST	State of residence	
	RES_CNTY	County of residence	
Q1	FISH_YEAR_FLG	Did angler fish during 2009?	Yes=1, No=0, 88 = Missing
Q2	GENDER		Male=1, Female=2
Q3	AGE		Less than 16 = 1 16 -- 17 = 2 18 -- 24 = 3 25 -- 34 = 4 35 -- 44 = 5 45 -- 54 = 6 55 -- 64 = 7 65 or older = 8
Q4	LICENSE_FLG	Did angler have a NC fishing license during previous 12 months?	Yes=1, No=0, 88 = Missing
Q5	SALT_LIC_FLG	Was license for recreational saltwater fishing?	Yes=1, No=0, 88 = Missing
Q6	WAVE_LIC_FLG	Was license valid during November 2009?	Yes=1, No=0, 88 = Missing
Q7	FISH_WAVE_FLG	Did angler fish during the wave (wave 6, 2009)?	Yes=1, No=0, 88 = Missing
Q8	BOAT_TRPS	Number of private boat trips during the wave	
Q9	SHORE_TRPS	Number of shore trips during the wave	
	SAMP_WT	Sample weight (N/n)	
	FRM_SIZE	Number of anglers on sample frame for stratum (N)	

Trip Information

Anglers provide detailed trip information for up to four recent trips. There is one record per trip.

Question	Field Name	Description	Coding Scheme
	SURV_YEAR		
	SURV_WAVE		
	HH_ID		
	ANG_ID		
	TRIP_ID	Unique identifier for each trip within an angler	
Q3B	MODE		Pier=1 Dock = 2 Jetty or Breakwater = 3 Bridge or Causeway = 4 Other man-made structure = 5 Bank or beach = 6
Q2/Q3	MODE_FX	Shore or private boat	Yes=1, No=0
Q1	TRIP_DATE	Date of trip	11/1 - 12/31
	FRAME	Is trip for an angler sampled from the license frame or the ABS frame?	ABS=1, License=2
2A/3A	TRIP_ST	State of trip	North Carolina
2A/3A	TRIP_CNTY		
2B/3B	ACCESS	Private/public	Yes, public access = 1 No, private access = 2
Q4	AREA		Ocean, within 3 miles from the shore = 1 Ocean, more than 3 miles from the shore = 2 Sound = 3 River = 4 Bay = 5 Inlet = 6 Someplace else = 7
	AREA_X		

5	RTN_TIME	Return time (time trip ended)	Midnight -- 3:00 am = 1 3:00 am -- 6:00 am = 2 6:00 am -- 9:00 am = 3 9:00 am -- Noon = 4 Noon -- 3:00 pm = 5 3:00 pm -- 6:00 pm = 6 6:00 pm -- 9:00 pm = 7 9:00 pm -- Midnight = 8
6	ADD_ANG_FLG	Did anyone else from your household fish with you (Y/N)	Yes=1, No=0
6A_1	SPOUSE_FLG	Did sampled angler fish with spouse in this trip?	6a = 1, 6a = 2,3
6A_2	CHILD_FLG	Did sampled angler fish with child on this trip?	6a = 2, 6a = 1,3
6A_3	OTHER_FLG	Did sampled angler fish with an other household member?	6a = 3, 6a = 1,2
6B	TOT_ANG	Total number of household members fishing on trip	

Appendix I: Tabulations of Key Variables

Household Questionnaire

Disposition of Sample		
	Frequency	Percent
Household with angler	228	12.7%
Household with no angler	457	25.4%
Refused	22	1.2%
Mailing returned undelivered	157	8.7%
No response	936	52.0%
Total	1800	100%

Did Anyone in the Household Fish During the Previous 12 Months?		
	Frequency	Percent
Yes	228	32.2%
No	457	64.6%
Missing	22	3.1%
Total	707	100.0%

Does the Household have a Landline Telephone?		
	Frequency	Percent
Yes	516	75.3%
No	149	21.8%
Missing	20	2.9%
Total	685	100.0%

How Many People in Household Fished During the Previous 12 Months?		
	Frequency	Percent
1	80	35.1%
2	89	39.0%
3	32	14.0%
4	14	6.1%
5	5	2.2%
6	4	1.8%
7	1	.4%
8	2	.9%
10	1	.4%
Missing	228	12.7%

Angler Questionnaire

Disposition of Sample				
	License		ABS	
	Frequency	Percent	Frequency	Percent
Trips taken in the 2 month period	227	14.5%	51	14.0%
No trips taken in the 2 month period	739	47.3%	137	37.6%
Refused	19	1.2%	3	.8%
Nothing ever returned	438	28.0%	63	17.3%
Mailing returned undelivered	139	8.9%	8	2.2%
No response			102	28.0%
Total	1562	100%	364	100%

Gender of the Respondent				
	License		ABS	
	Frequency	Percent	Frequency	Percent
Female	164	16.6%	34	17.8%
Male	735	74.6%	141	73.8%
Missing	86	8.7%	16	8.4%

Did the Respondent Perform in Recreational Saltwater Fishing in 2009?				
	License		ABS	
	Frequency	Percent	Frequency	Percent
Yes	718	23.4%	152	79.6%
No	230	72.9%	31	16.2%
Missing	37	3.8%	8	4.2%

License Type		
	Frequency	Percent
Residential CRFL	366	23.4
Residential CRFL 10-day	11	.7
Residential CRFL Adult	4	.3
Non-residential CRFL	282	18.1
Non-residential CRFL 10-day	115	7.4
Age 65 CRFL	87	5.6
Disabled Vet CRFL	4	.3
Totally Disabled CRFL	6	.4
Perm Disabled State Fish w CRFL	15	1.0
Uni Blind Inland / CRFL	1	.1
Unifed Inland / CRFL	40	2.6
Unified Sptm / CRFL	87	5.6
Lifetime Unifed Inland / CRFL	1	.1
Lifetime Comp Inland Fish w CRFL	15	1.0
Subsidized Inland / CRFL Waiver	35	2.2
Disabled Combo H/F/CRFL Basic	11	.7
Sportsman Infant w CRFL	39	2.5
Sportsman Youth w CRFL	29	1.9
Residential Sportsman Adult w CRFL	145	9.3
Residential Uni Sptm / CRFL Adult	6	.4
Non-residential Sportsman Adult w/ CRFL	26	1.7
Non-residential Uni Sportsman / CRFL Adult	2	.1
Unified Age 65 Sportsman / CRFL	54	3.5
Residential Lifetime Over 70 Fish w/ CRFL	122	7.8
Lifetime Comp Over 70 Fish w/ CRFL	45	2.9
Disabled Sportsman w/ CRFL	10	.6
Uni Disabled Vet Sptm / CRFL	4	.3
Total	1562	100.0

Age of the Respondent				
	License		ABS	
	Frequency	Percent	Frequency	Percent
Less than 16	2	.2%	1	.5%
16 – 17	1	.1%		
18 – 24	42	4.3%	13	6.8%
25 – 34	86	8.7%	19	9.9%
35 – 44	136	13.8%	33	17.3%
45 – 54	206	20.9%	37	19.4%
54 – 64	196	19.9%	45	23.6%
65 or older	226	22.9%	28	14.7
Missing	90	9.1%	15	7.9%

Has the Respondent Fished in NC During the Past 12 Months?				
	License		ABS	
	Frequency	Percent	Frequency	Percent
Yes	871	88.4%	132	69.1%
No	31	3.1%	43	22.5%
Missing	83	8.4%	16	8.4%

Was the License for Recreational Saltwater Fishing?				
	License		ABS	
	Frequency	Percent	Frequency	Percent
Yes	731	10.9%	106	55.5%
No	107	74.2%	22	11.5%
Missing	147	14.9%	63	33.0%

Was the License Valid During November 2009?				
	License		ABS	
	Frequency	Percent	Frequency	Percent
Yes	675	68.5%	84	44
No	51	5.2%	23	12
Missing	259	26.3%	84	44

Did the angler fish during Wave 6, 2009?				
	License		ABS	
	Frequency	Percent	Frequency	Percent
Yes	227	23%	49	25.7%
No	738	74.9%	136	71.2%
Missing	20	2.0%	6	3.1%

Trip Questionnaire

Was the Trip from the Shore or Private Boat?		
	Frequency	Percent
Shore	385	59.8%
Boat	252	39.1%
Missing	7	1.1%
Total	644	

Collapsed Mode of Fishing		
	Frequency	Percent
Ocean, less than 3 miles from the shore	306	47.5%
Ocean, more than 3 miles from the shore	48	7.5%
Inland trip	231	35.9%
Missing	8	1.2%
More than one response checked	51	7.9%

Public Access for Boat and Shore Trips				
	Boating Trips		Shore Trips	
	Frequency	Percent	Frequency	Percent
Yes, public access	179	71.0%	340	86.7%
No, private access	60	23.8%	38	9.7%
Missing	13	5.2%	14	3.6%
Total	252		392	

Mode of Shore Trip		
	Frequency	Percent
Pier	74	16.5
Dock	31	6.9
Jetty or Breakwater	18	4.0
Bridge or causeway	26	5.8
Other man-made structure	8	1.8
Bank or beach	276	61.5
Missing	16	3.6
Total	449	

Fishing Area of Trip		
	Frequency	Percent
Ocean, within 3 miles from the shore	306	47.5%
Ocean, more than 3 miles from the shore	48	7.5%
Sound	79	12.3%
River	66	10.2%
Bay	5	.8%
Inlet	59	9.2%
Someplace else	22	3.4%
Missing	8	1.2%
More than one response checked	51	7.9%

Time the Trip Ended		
	Frequency	Percent
Midnight – 3:00 am	7	1.1%
3:00 am – 6:00 am	6	.9%
6:00 am – 9:00 am	27	4.2%
9:00 am – Noon	75	11.6%
Noon – 3:00 pm	121	18.8%
3:00 pm – 6:00 pm	286	44.4%
6:00 pm – 9:00 pm	82	12.7%
9:00 pm – Midnight	11	1.7%
Missing	6	.9%
Invalid answer (multiple responses)	23	3.6%

Was Anyone in the Household who was also an Angler?		
	Frequency	Percent
Yes	332	47.0%
No	303	51.6%
Missing	9	1.4%

Did the Angler's Spouse Fish with the Respondent on this Trip?		
	Frequency	Percent
Spouse	156	45.1%
Child / Children	121	35.0%
Other	69	19.9%
Total	346	100.0%

Total Number of Household Members Fishing on the Trip		
	Frequency	Percent
1	346	53.7%
2	190	29.5%
3	57	8.9%
4	22	3.4%
5	6	.9%
7	4	.6%
8	4	.6%
10	2	.3%
12	1	.2%
Missing	12	1.9%
Total	644	100.0%

A Comparison of Recreational Fishing Effort Survey Designs

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February 2012

Limitations of Use

The findings in this report have limitations. The comparisons of effort estimates described in this report are based upon a single wave of data collection in two states. Our findings may not be indicative of survey results in other states or waves. The surveys compared in this report were not administered in a controlled, experimental setting designed specifically to measure differences in sources of error between the survey designs. Rather, our purpose was to test the effectiveness of alternative survey methodologies for collecting recreational fishing effort data. Any attempt to model or apply the resulting changes in effort estimates to other states or waves may be an inappropriate use of this report.

Contents

Executive Summary

A primary objective of the Marine Recreational Information Program (MRIP) is to implement improved surveys of recreational fishing effort. To that end, MRIP has funded several pilot studies to develop and test the feasibility of alternative data collection designs with a goal of increasing the efficiency of data collection and the accuracy of survey estimates. A focus of the research program has been to improve coverage of the population while also reducing nonresponse and measurement error. The objective of this report is to synthesize the results of the completed pilot studies, assess differences in the resulting estimates within a framework of survey errors, and provide recommendations for future testing and implementation.

To date, MRIP has considered four data collection designs for collecting recreational shore based and private boat fishing effort data: 1) the Coastal Household Telephone Survey (CHTS), which is the ongoing random-digit-dial (RDD) survey administered by NOAA Fisheries, 2) the Angler Licensed Directory Telephone Survey (ALDS), which samples from lists of licensed or registered saltwater anglers, 3) dual-frame telephone surveys, which integrate CHTS and ALDS sampling in a dual-frame design, and 4) dual-frame mail surveys, which sample from angler license frames and residential address frames. Because the components of the dual-frame surveys are sampled independently, we are also able to consider the effectiveness of single-frame, license surveys (ALDS and license mail survey) and general population surveys (CHTS and ABS).

All of these survey designs have been administered to collect data for a common time period (November-December, 2010) in common states (Louisiana and North Carolina), which provides an opportunity to make direct comparisons of survey estimates. Our goal was to examine, both qualitatively and quantitatively, the potential sources of survey error for each of the designs and explain, to the extent possible, observed differences in estimates within the context of these errors. The largest observed differences were between estimates generated from the CHTS and ABS general population samples. Subsequently, much of the assessment focused on explaining differences between these two survey designs. Comparisons between the license frame survey estimates (ALDS and license mail survey) revealed less substantial differences, but provided insight into the observed differences between mail and telephone survey designs.

In general, the mail survey designs produce larger estimates of fishing effort than the corresponding telephone survey designs, particularly for estimates of shore-based fishing effort generated from the general population samples. The larger estimates of effort are driven by differences in the estimated number of anglers rather than the estimated mean trips per angler.

Nonresponse, incomplete coverage, and measurement error were examined to evaluate the observed differences in survey estimates. Evidence of nonresponse bias was found for both the ABS and CHTS designs, as avid anglers are more likely to respond to the surveys than non-anglers. While nonresponse bias is a concern, it is unlikely to contribute significantly to the observed differences between the ABS and CHTS estimates of effort. Similarly, both mail and telephone survey designs are susceptible to bias resulting from noncoverage, with a greater potential for bias in the CHTS due to the exclusion of non-landline households and households in noncoastal counties. As with nonresponse, noncoverage is a concern but does not appear to be responsible for large differences between the CHTS and ABS.

We concluded that differential biases due to measurement errors were likely to be the largest source of differences between the CHTS and ABS estimates. Specifically, we hypothesize that inaccurate responses to the telephone survey screening questions are producing biases in the estimates largely due to recall/salience effects. This error has a greater impact on estimates of shore fishing effort than boat fishing effort because boat fishing trips are more salient than shore fishing trips. The mechanism for this bias appears to be related to the tasks imposed on the telephone survey respondent. Specifically, telephone survey respondents, answering a "cold" telephone request, are asked to recall recreational fishing activity for all members of the household with minimal time to consider the request. Because the CHTS screening questions are administered to whomever answers the phone, it is very possible that the respondent did not personally participate in any or all the recreational fishing trips taken by the members of the household, and he or she may not recall or be aware of the fishing activities of other household members. This would result in an underestimate of fishing incidence and subsequently the estimated number of anglers who fished in the wave. In contrast, respondents to the mail survey have more time to consider the survey request, and the mail instrument provides a visual cue in the form of a calendar to aid in recall. In addition, we believe the mail questionnaire is more likely to end up in the hands of someone within the household who fishes or is likely to know about the fishing activities of other household members. Because the surveys were not administered in a controlled, experimental setting, we cannot confirm this hypothesis with existing data. However, comparisons among the survey results consistently support this hypothesis.

While we do not have external data sources to confirm that one approach has less bias than another, our investigations and hypotheses lead us to believe that the mail survey estimates are subject to less bias across all sources of error than the telephone survey estimates. Since the dual-frame approach is efficient in terms of identifying anglers, the dual-frame mail survey design is a reasonable alternative to the CHTS. However, we recommend testing a single-phase, stratified alternative to the dual-frame design that changes how the license frames are used, as well as the mailing procedures. Rather than using the license databases directly for sampling, we propose to use them to stratify ABS samples. Stratifying ABS sample into matched and unmatched strata will allow us to sample at different rates, effectively maintaining the efficiency of sampling directly from the license frame while avoiding some of the potential biases and complexities associated with the dual-frame design.

Conclusions/Recommendations

The review of survey methods and results has led us to the following conclusions and recommendations:

- While both general population surveys are susceptible to bias resulting from noncoverage, the potential for bias is greater in the CHTS due to the exclusion of non-landline households and non-coastal county households. We did not find evidence to suggest noncoverage bias accounted for differences in the survey estimates, but noncoverage from excluding non-landline households is likely to continue to increase and this could lead to larger noncoverage biases in the CHTS in the future.
- In the states we studied, angler license frames are very incomplete and not suitable to be used exclusively as sample frames for recreational fishing surveys at this time. Undercoverage rates of license frames ranged from 40-50% in North Carolina and from 5-70% in Louisiana.
- Nonresponse error due to avidity bias is a concern in both the ABS and CHTS. Nonresponse adjustment methods, such as those used in the ABS, should be used to reduce avidity bias. Our analysis did not find that differential nonresponse bias contributed significantly to the observed differences between ABS and CHTS estimates.
- The large differences between CHTS and ABS estimates appear to be due primarily to measurement errors. The respondent tasks are very different for telephone and mail surveys, which is likely to result in differential bias due to differences in recall ability and the salience of different types of fishing activity.
- While we do not have external data sources to confirm that one approach has less bias than another, our investigations and hypotheses lead us to believe that the mail survey estimates are subject to less bias across all sources of error than the telephone surveys.
- Frame matching errors in the dual-frame designs are likely to result in a small overestimate of fishing effort for the dual frame mail survey. Since the dual-frame approach is efficient in terms of identifying anglers, the dual frame method is a reasonable alternative design to the CHTS.
- We recommend testing a single-phase, stratified alternative to the dual-frame design that changes how the license frames are used and the mailing procedures. Rather than using the license databases directly for sampling, we propose to use them to stratify ABS samples. Stratifying ABS sample into matched and unmatched strata will allow us to sample at different rates, maintaining the efficiency of sampling directly from the license frame while avoiding some of the potential biases and complexities associated with the dual-frame design.

1. Introduction

Traditionally, marine recreational fishing effort data for the U.S. Atlantic Coast and the Gulf of Mexico have been collected by NOAA Fisheries through the Marine Recreational Fisheries Statistics Survey's (MRFSS) Coastal Household Telephone Survey (CHTS). The CHTS utilizes a random digit dialing (RDD) telephone survey approach to contact residents of coastal county households and collect information on fishing activities that occurred within a two-month reference period (wave). In recent years, the efficiency and effectiveness of RDD surveys in general, and the CHTS specifically, have been questioned due to declining rates of coverage and response.

In a review of the MRFSS conducted by the National Research Council (NRC) of the National Academies of Science, reviewers noted that the CHTS design suffers from inefficiency due to the low rate of saltwater angler participation among the general population, as well as potential coverage bias due to the survey's limitation to coastal county residences and landline-based telephone numbers (National Research Council 2006). The review further recommended the development of and subsequent sampling from a comprehensive list of registered saltwater anglers or, in the absence of such a list, implementation of dual-frame procedures that include sampling from both lists of licensed saltwater anglers and residential household frames.

The Marine Recreational Information Program (MRIP) has designed and tested several different sampling alternatives to address concerns about the CHTS. The objectives of this document are to:

- summarize the various fishing effort survey design alternatives developed through MRIP;
- provide an overview of common sources of survey error and their potential impacts on estimates;
- assess observed differences in fishing effort estimates generated through the different survey design alternatives within the context of survey errors; and,
- Suggest additional design alternatives for consideration by MRIP leadership that may better address potential sources of error identified in this review.

Below, we outline the various approaches to collecting fishing effort data that are currently being used or studied by MRIP. The next section provides a framework of common survey errors used to explore the differences in estimates produced from the different data collection designs. The third section presents the estimates from the different surveys and analyzes the differences with respect to measurement, coverage, response, and matching errors. Given the complexity of the analysis, we include a synopsis of the findings at the end of this section. The final section proposes alternative design options based on the findings of the analyses with the goal of finding solutions that may minimize the most important errors identified.

1.1 Coastal Household Telephone Survey

The CHTS, which was implemented by NOAA Fisheries in 1981, is a cross-sectional, random-digit dial (RDD) telephone survey of coastal county residences (residences in counties within 25-50 miles of coast). Sampling is stratified by state and county, and the data are collected for a two-month reference period (wave). The survey utilizes computer assisted telephone interviewing (CATI) to contact households and collect information on recreational saltwater fishing activity, including the number of people who participate in saltwater fishing and the number of shore and private boat fishing trips they take (Van Voorhees et al., 2002).

Once a household has been contacted for a CHTS interview, the initial respondent is asked a series of questions to determine if anyone in the household participated in saltwater fishing during the two month reference wave. Specifically, the respondent is sequentially asked the following conditional questions:

1. How many people in this household go fishing?
2. How many people in your household, including children and adults, have been recreational saltwater fishing in the last 12 months anywhere in the US or in a US territory?
3. Thinking just about the past 2 months, how many of the people living in your household, including children and adults, have been recreational saltwater fishing in the last 2 months in the US or a US territory?

If the responses to all three of these questions are affirmative, then each household member who fished during the wave is sampled and an attempt is made to collect detailed information about his or her fishing activity. Specifically, each angler is asked to report the total number of days fished during the wave, then asked a series of questions about each individual trip, including the date and fishing mode, beginning with the most recent trip and working backward through the wave. The complete CHTS questionnaire is included as Appendix A.

Because the CHTS is limited to coastal counties, estimates of total fishing effort are dependent upon expansion factors derived through an independent intercept survey of completed fishing trips.

Specifically, CHTS estimates are adjusted upward by the inverse of the ratio of CHTS-covered trips (intercepted trips taken by anglers in coastal households) to total trips (CHTS-covered trips plus intercepted trips taken by anglers from non-coastal counties).

1.2 Angler License Directory Telephone Survey

As noted by the NRC (2006), a more efficient approach for surveying anglers is to sample directly from lists of individuals who are licensed to participate in saltwater fishing. Working collaboratively with the Gulf States Marine Fisheries Commissions, the Gulf Coast states, and the North Carolina Division of Marine Fisheries, MRIP has designed and tested Angler License Directory Telephone Surveys (ALDS), which sample from state databases of licensed anglers. The ALDS was implemented as a pilot project in Florida, Alabama, Mississippi and Louisiana in 2007 and expanded to North Carolina in 2008. Currently, the survey is being administered in LA and NC.

The data collection procedures for the ALDS are nearly identical to the CHTS, with the exception of the screening portion of the survey; the ALDS requests to speak with the individual licensed angler by name and then proceeds to determine if the angler, or any other individuals who reside in the same household as the angler, fished during the wave. As with the CHTS, trip details are collected through episodic recall beginning with the most recent trip.

As predicted, the ALDS is more efficient than the CHTS at contacting anglers. However, exemptions to state licensing requirements, as well as incomplete and inaccurate contact information for individuals included on the sample frames, create gaps in the coverage of the survey.

1.3 Dual-Frame Telephone Survey

As noted above, the CHTS and the ALDS, considered individually, do not provide complete coverage of the angler population. To compensate for potential sources of coverage error in the CHTS and ALDS, MRIP has developed an estimation design that integrates CHTS and ALDS sampling in a dual-frame design (Lai and Andrews 2008). The union of the CHTS and ALDS sample frames defines three domains: 1) anglers who can only be sampled from the CHTS frame (unlicensed anglers who reside in coastal counties covered by the CHTS); 2) anglers who can only be sampled from the ALDS frame (licensed anglers who reside outside of the coverage area of the CHTS); and, 3) anglers who can be sampled from both the CHTS and ALDS frames (licensed anglers who reside in coastal counties). A fourth domain includes anglers who cannot be sampled by either the CHTS or ALDS (unlicensed anglers without landline telephones within the CHTS coverage area and unlicensed anglers residing outside the coverage area of the CHTS). This design is currently being implemented in NC and LA, and has also been tested in the other states where the ALDS and CHTS have been conducted concurrently, including FL, AL and MS, as well as Washington.

While the dual-frame telephone survey design certainly increases the coverage over either the CHTS or the ALDS, the methodology is not without limitations. As mentioned, the union of the CHTS and ALDS sample frames excludes a segment of the angling population, creating a potentially significant gap in coverage. Previous studies suggest that up to 38% of fishing trips in NC are taken by anglers who are excluded from either the CHTS or ALDS (Andrews et al. 2010). In addition, partitioning anglers into the appropriate domains, and subsequently adjusting sample weights, is based upon the survey respondents' willingness and ability to classify themselves as licensed or unlicensed anglers. This has been demonstrated to be an unreliable approach for defining dual-frame domains (Andrews et al. 2010) and subsequently calculating unbiased survey weights.

1.4 Dual-Frame Mail Survey

An alternative to the dual-frame telephone survey is to identify and contact anglers through a dual-frame mail survey design. MRIP initially tested the feasibility of a dual-frame mail survey design in NC in 2009, and conducted a follow-up study aimed at enhancing response rates and the timeliness of responding in NC and LA in 2010.

The specific details of the dual-frame mail survey design are described elsewhere (Andrews et al. 2010). Briefly, anglers are sampled both from state databases of license saltwater anglers and from residential address frames maintained and made commercially available by the United States Postal Service. The address-based sample (ABS) is matched to the license databases by searching the license frame for the same address and/or telephone number (for the cases in which a telephone number can be located through a commercial service for the ABS sampled address).

The License frame sampling is conducted in a single phase; sampled anglers are mailed a brief questionnaire that asks respondents to report the number of days fished from the shore and from a boat during a two-month reference wave. The instrument used in the mail mode is substantially different from the CHTS and ALDS instruments. The impact of these differences on survey estimates is discussed in some detail below in section 3.

The ABS sampling is conducted in two phases. Residential addresses are sampled and mailed a screening questionnaire to identify individuals who fished during the previous twelve months. Anglers identified in the screening phase are sent a second-phase questionnaire that is identical to the license sample questionnaire.

The screening and angler questionnaires are included as appendices, B and C, respectively.

1.5 Comparisons of Survey Estimates

All of the surveys described above have been administered in overlapping geographic locations (LA and NC) and time periods (wave 6, 2010), which allows us to directly compare estimates generated through the various designs. In addition, because the components of the dual-frame designs are independent (e.g. ABS mail sample is independent from the license mail sample), we can compare components within a dual-frame design, as well as compare components among dual-frame designs. For example, we can compare estimates from the ABS component of the dual-frame mail survey to both license mail estimates (within dual-frame design comparison) and CHTS estimates (among dual-frame design comparison). There are some situations where the comparisons are limited to specific geographic regions; for example, the CHTS only covers households in coastal counties, limiting some comparisons to these counties. The geographic limitations of the comparisons are noted as appropriate.

The differences between some of the estimates were substantial enough that a review of the differences, and an attempt to reconcile these differences, was deemed necessary. This review was developed in the tradition of an investigation of differential error sources, and data from the surveys was used in the evaluation. The next sections briefly introduce the sources of error considered in the review and describe observed differences between survey estimates within the context of these errors. The surveys were not designed to provide experimental evidence about specific error sources, so most of the evaluations are observational in nature. As a result, the conclusions drawn are tentative. Every attempt was made to be even-handed in the review, but this type of analysis is invariably affected by the reviewers' experiences and opinions. The final section is a presentation of two alternative designs that might perform differently from the current design based on the analysis conducted.

2. Survey Error

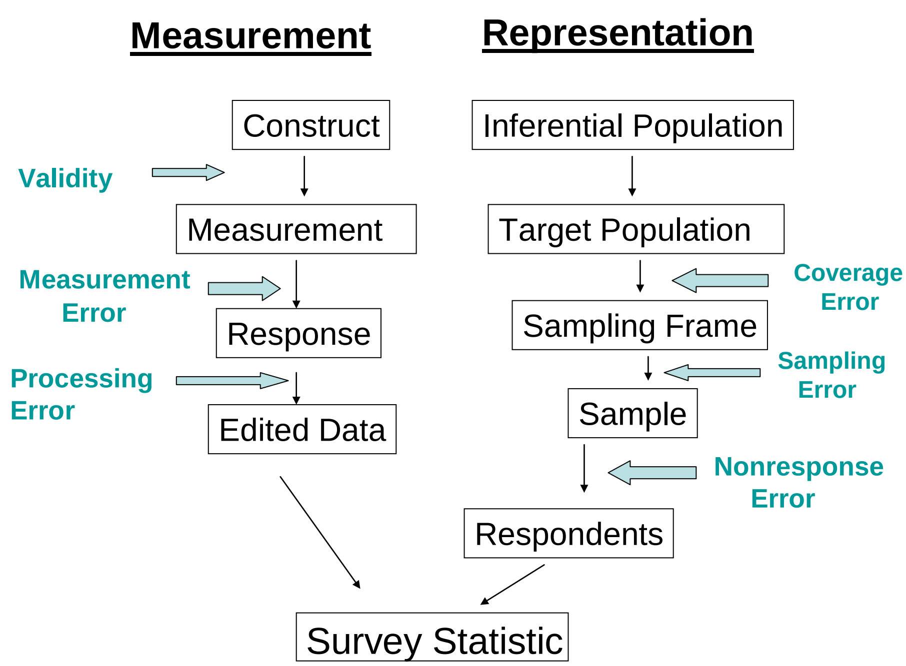
It is useful to establish a common language concerning sources of survey error when comparing estimates from substantially different survey designs. A common conceptual framework that is often used (Groves, 1989) is that of Total Survey Error (or mean squared error) –the sum of all variable errors and all biases (more precisely, the sum of variance and squared bias). Bias is the type of error that affects the statistic in all implementations of a survey design; variable error arises because achieved values differ over the units (e.g. sampled persons; interviewers used; questions asked) that are the sources of error.

Most methodologists further classify errors in terms of errors of observation (or representation) and errors of non-observation (or measurement). Coverage error, sampling error, nonresponse error, and adjustment error all fall within the framework of errors of representation; measurement error encompasses all sources of error that lead to a difference between the edited response derived from the survey and the "true value" of the construct of interest. Coverage error refers to issues related to the sampling frame—the extent to which all members of the population of interest have a non-zero probability of being sampled from the frame. Although one can have both under- as well as over-coverage, the focus of most coverage investigations is related to who (or what) is not covered by a particular frame. Coverage error is a function of both the proportion of the population not covered by the frame and the extent to which those who are not covered differ from those who are covered. Similar to coverage error, nonresponse error is a function of both the proportion of the sample that does not respond to the survey request and the extent to which those who do respond differ from those who do not on the characteristics of interest to the study. Sampling error exists in all sample surveys and simply reflects the variability associated with the selection of a particular sample from the distribution of all possible samples, given a specific design.

The sources of measurement error (or errors of observation) include the interviewer, the instrument (both the individual questions and the overall questionnaire), the respondent, and processing error. Interviewer errors are those errors related to the variation in the delivery and recording of the questions by interviewers across respondents; for example, failure to read a question, changes in intonation either across interviewers or across respondents within an interviewer, or errors in the recording of an answer. Instrumental errors (both individual questions and the overall questionnaire) arise from wording of questions, wording of response options, the flow of the questionnaire (e.g., order effects), as well as the mode and method of data collection. Respondent errors – those arising from the respondent –may be due to errors in recall ability, knowledge (when proxy reporting), motivation of the respondent to report accurately, saliency of the event to the respondent, social desirability bias (e.g., the willingness to report socially undesirable information), as well as respondent fatigue. Processing error –the least well studied of the sources of survey error –arise from the editing and processing of data.

Figure 2.1 in the NRC report "Review of Recreational Fisheries Survey Methods," reproduced from Groves, et al (2009) illustrates the life cycle of a survey from a quality perspective. For convenience the figure is included here as Figure 1.

Figure 1. Survey Process from a Quality Perspective



Source: Groves, et al, 2009

3. Comparisons and Analysis

Estimates of total angler trips by state, geographic domain and fishing mode for the CHTS and ABS are provided in Table 1. In the CHTS, coastal resident effort is estimated directly through telephone survey data. Because the coverage of the survey is limited to coastal counties, estimates of noncoastal resident effort are generated by expanding coastal resident effort upward by correction factors generated through an onsite survey of completed fishing trips. Table 1 and all subsequent tables and figures show the expanded estimates from the CHTS, unless explicitly noted.

Table 1. Comparison between CHTS and ABS for estimated total angler trips (in thousands).

	CHTS (000's)	ABS (000's)	Ratio (ABS:CHTS)
Overall	1129	2640	**2.3***
North Carolina	421	1334	**3.2***
Private Boat	200	474	**2.4***
Coastal	157	308	2.0
Noncoastal	43	167	3.9
Shore	221	860	**3.9***
Coastal	117	493	**4.2***
Noncoastal	104	367	**3.5***
Louisiana	708	1306	**1.8***
Private Boat	584	608	1.0
Coastal	504	457	0.9
Noncoastal	80	151	1.9
Shore	124	699	**5.6***
Coastal	102	587	**5.7***
Noncoastal	22	112	5.1

* Ratio is significantly different from 1.0 at the α=0.05 level.

ABS estimates of the total number of angler trips are significantly greater than CHTS estimates in aggregate over all comparison cells and overall for each of the two states. Within the state of North Carolina, we see significant differences by fishing mode whereas in Louisiana, the difference is only significant in the reporting of shore fishing trips. The increased reporting of shore trips for the ABS sample in North Carolina persists across geographic domains whereas in Louisiana, only the coastal geographic domain exhibited significantly more angler trips.

CHTS and ABS estimates of mean trips per angler and the total number of anglers who fished during the wave are provided in Table 2. The table is limited to the coastal stratum to correspond to the geographic coverage of the CHTS and to make the comparison more direct. For private boat fishing, ABS and CHTS estimates of mean trips per angler are not significantly different. For shore fishing, ABS estimates of mean trips per angler are not significantly different from CHTS estimates in LA, but are significantly greater in NC. ABS estimates of the total number of anglers who fished during the wave are significantly larger than CHTS estimates for both states and modes, with the exception of private boat fishing in LA, for which the estimates are not significantly different.

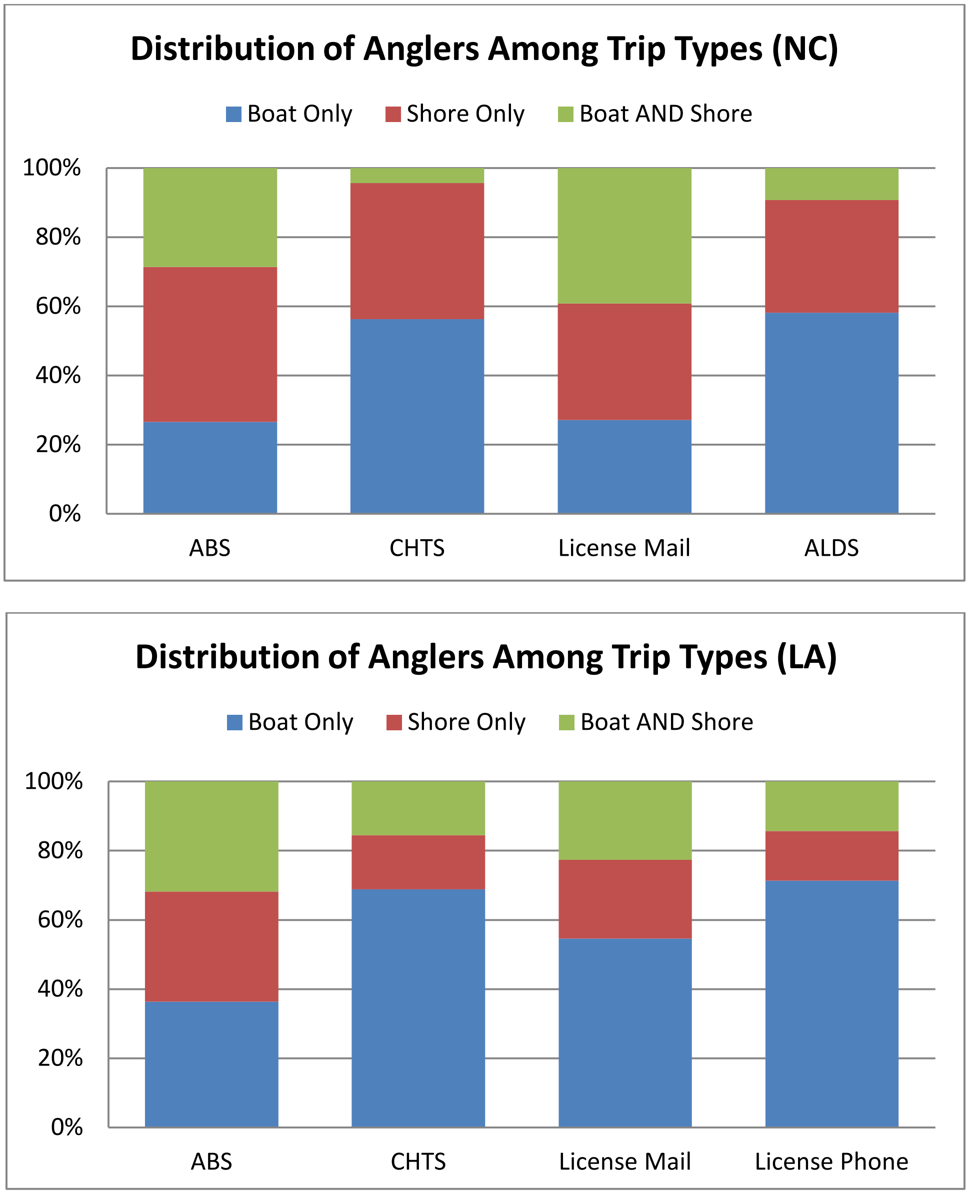
Table 2. Estimated mean trips per angler and total anglers (000's) who fished by mode for the CHTS and ABS, Coastal County Residents.

	Mean Trips per Angler			Total Anglers (000's)		
	CHTS (SE)	ABS (SE)	ABS:CHTS	CHTS (SD)	ABS (SD)	ABS:CHTS
NC Private Boat	4.75 (0.69)	5.43 (0.90)	1.14	30.34 (4.51)	57.83 (7.06)	**1.91***
NC Shore	3.81 (0.48)	6.97 (1.01)	**1.83***	28.25 (5.40)	76.96 (8.12)	**2.72***
LA Private Boat	5.32 (0.93)	4.27 (0.38)	0.80	88.30 (13.78)	108.00 (9.70)	1.22
LA Shore	3.82 (0.77)	6.05 (0.73)	1.58	24.63 (5.31)	101.12 (9.50)	**4.11***

* Ratio is significantly different from 1.0 at the α=0.05 level.

Figure 2 demonstrates the distribution of anglers among types of fishing activity for the ABS mail, CHTS, license mail and ALDS. Anglers who reported fishing during the wave were classified into one of the following categories: 1) Fished only in private boat mode, 2) fished only in shore mode, or 3) fished in both private boat mode and shore mode. There are substantial differences in the types of reported fishing activity among the types of surveys. Specifically, more anglers reported participating in both boat and shore fishing in the mail surveys (ABS mail and license mail) than the telephone surveys (CHTS and ALDS). Generally, the higher incidence of anglers who reported both types of fishing activity in the mail surveys is at the expense of anglers who only reported boat fishing, which is considerably lower in the mail surveys than the phone surveys. This observation is consistent across states, although it is more pronounced in NC than in LA. In NC, the proportion of anglers reporting only shore fishing was relatively consistent across surveys. This was not case in LA, where more anglers reported only shore fishing in the mail surveys than in the phone surveys.

Figure 2. Distribution of anglers among type of recreational saltwater fishing trips for four independent data collections, Wave 6, 2010, Coastal County Residents.



Tables 3 and 4 present the estimated number of angler trips, anglers, and average number of trips per angler for the license frame surveys, similar to those given in tables 1 and 2 for the general population surveys. Since the license frame surveys are not restricted to the coastal counties for either of the two modes, the estimates are of all anglers licensed in the states. While often significant, the differences between estimates are generally smaller than those observed in the CHTS and ABS general population surveys. As a result, in the remainder of this section, we focus on differences between the ABS and

CHTS and try to explain those differences in terms of potential sources of biases for the different survey designs. While the differences between the license mail survey and ALDS are less pronounced they may provide insight into differences between the ABS and CHTS. These results are presented and discussed accordingly.

Table 3. Comparison between ALDS and License Mail Survey for estimated total angler trips (in thousands).

	License Mail (000's)	ALDS (000's)	Ratio (License Mail:ALDS)
Overall	1787.8	1075.0	**1.7***
North Carolina	799.9	478.4	**1.7***
Private Boat	281.5	180.3	**1.6***
Coastal	187.0	118.7	1.6
Noncoastal	85.7	59.7	1.4
Outstate	8.8	1.9	**4.7***
Shore	518.3	298.2	**1.7***
Coastal	270.2	149.7	1.8
Noncoastal	188.1	102.2	**1.8***
Outstate	60.0	46.3	1.3
Louisiana	987.9	596.5	**1.7***
Private Boat	537.4	433.7	1.2
Coastal	433.2	351.9	1.2
Noncoastal	60.0	72.8	0.8
Outstate	44.2	9.0	**4.9***
Shore	450.5	162.9	**2.8***
Coastal	402.3	134.3	**3.0***
Noncoastal	30.9	23.8	1.3
Outstate	17.3	4.8	3.6

* Ratio is significantly different from 1.0 at the α=0.05 level.

Table 4. Estimated mean trips per angler and total anglers (000's) who fished by mode for the ALDS and License Mail Survey.

	Mean Trips per Angler			Total Anglers (000's)		
	ALDS (SE)	License Mail (SE)	License Mail:ALDS	ALDS (SE)	License Mail (SE)	License Mail:ALDS
North Carolina						
Private Boat	2.67 (0.31)	3.80 (0.40)	1.42	67.54 (6.69)	74.98 (8.13)	1.11
Coastal	3.17 (0.52)	4.18 (0.55)	1.32	37.42 (4.67)	45.20 (5.72)	1.21
Non Coastal	2.05 (0.25)	3.28 (0.63)	1.60	29.19 (4.70)	26.68 (5.71)	0.91
Non Resident	2.00 (0.00)	2.83 (0.34)	**1.42 ***	0.93 (0.93)	3.10 (0.90)	3.35
Shore	4.51 (0.74)	4.75 (0.36)	1.05	66.06 (6.74)	112.17 (9.24)	**1.70***
Coastal	6.44 (1.91)	5.43 (0.64)	0.84	23.23 (4.47)	49.74 (5.77)	**2.14***
Non Coastal	2.96 (0.36)	3.99 (0.48)	1.35	34.50 (4.73	49.76 (7.04)	1.44
Non Resident	5.56 (1.99)	4.87 (0.37)	0.88	8.34 (1.72)	12.66 (1.63)	1.52
Louisiana						
Private Boat	4.10 (0.42)	4.69 (0.61)	1.14	105.88 (4.58)	119.16 (8.06)	1.13
Coastal	4.36 (0.53)	5.02 (0.80)	1.15	80.75 (3.91)	89.75 (7.47)	1.11
Non Coastal	3.38 (0.47)	3.65 (0.59)	1.08	21.55 (1.97)	17.52 (2.84)	0.81
Non Resident	2.50 (0.42)	3.72 (0.50)	1.49	3.58 (1.32)	11.90 (1.66)	3.32
Shore	5.58 (1.35)	6.44 (0.89)	1.15	29.20 (4.38)	71.99 (7.22)	**2.47***
Coastal	5.85 (1.66)	6.97 (1.07)	1.19	22.95 (3.91)	59.45 (6.86)	**2.59***
Non Coastal	5.33 (2.09)	3.75 (0.91)	0.70	4.46 (1.69)	8.43 (1.98)	1.89
Non Resident	2.67 (1.67)	4.30 (0.97)	1.61	1.79 (0.99)	4.11 (1.08)	2.29

* Ratio is significantly different from 1.0 at the α=0.05 level.

3.1 Differential Bias due to Measurement Errors

An important consideration in all comparisons of estimates from different surveys is the effect of measurement errors. Since the CHTS and ALDS are telephone surveys and the ABS and license mail surveys are self-administered mail surveys, the data collection mode and the effects of the interviewers are key differences that need to be considered. In addition, the questionnaires used in the telephone surveys and in the mail surveys differ significantly. However, the surveys were not administered in a controlled, experimental setting designed specifically to test for mode or interviewer effects. Consequently, assessment of measurement error is subject to confounding influences of other types of error (e.g., nonresponse error)

The tasks imposed on the respondents in the mail surveys are dramatically different from those in the telephone surveys. In particular, in the self-administered mail survey the respondent is asked to report the number of days fished by fishing mode (shore and/or boat) during a two-month reference period, and respondents are only asked to provide information about his or her own trips; proxy reporting is not permitted although it cannot be controlled. In contrast, telephone survey respondents are initially asked to report the total number of days fished during the same two-month reference period and then asked to provide details, including the fishing mode, for each trip through episodic recall (although there are mechanisms to reduce the response burden for similar trips). In addition, telephone survey respondents may answer for other members of the household (proxy responses), regardless of whether or not the actual respondent participated in fishing activity during the reference wave. The telephone and mail surveys differ in other aspects as well. For example, the mail respondent can immediately see the survey request in its totality and can recognize that the request is relatively simple and not very time-consuming. The telephone respondent must wait to see how the interview unfolds and may or may not have much faith in the interviewer's declaration about the length of the survey. Thus, the demands on the respondents, the respondent rules (who can report on the trips), and the context for telephone surveys are very different from those in the mail surveys.

We try to address many of these issues within the framework of measurement error, even if this is not a completely accurate moniker. We begin with some hypotheses related to the generic observed differences in the estimates between the mail and telephone surveys. In particular, we focus on some of the biggest differences noted in Tables 1, and 2 and Figure 2. In doing this, we will include various measurement error topics such as respondent rules and proxy reporting.

The largest differences between ABS and CHTS estimates are for total trips, mean trips per angler for shore fishing, and the distribution of anglers among the types of fishing activity, with the mail surveys estimating many more anglers who took shore trips. Figure 2 indicates that the distribution of anglers who took both shore and boat trips during the reference wave differs also, and we will explore this in more detail below.

Consistent with previous literature (Schwarz, Stack, Hippler, and Bishop 1991), we would anticipate that context has a larger impact on the telephone survey than the mail survey. Since the telephone interviews are sequential, the order of the questions might influence responses. We briefly set up the context of the telephone survey interview and then present hypotheses related to this context (see the instruments in the appendix for more details).The CHTS begins by asking a respondent a series of household-level screener questions to determine if anyone in the household has been fishing during the previous two months, and if anyone in the household had a recreational saltwater fishing license that was valid during the reference period (respondents are sequentially asked how many people in the household fish, how many people in the household fished during the previous 12 months and how many people fished during the previous 2 months). If the screening questions determine that the household is a fishing household, then the interviewer attempts to administer angler-level questions to each household member that fished during the wave. Specifically, each person is asked if he or she had a saltwater fishing license that was valid during the previous two months, and on how many days during the past two months he or she fished both within state and in another state. They are then asked the date of their most recent trip and if the fishing on that date was from a boat (if yes, some details on the boat trip are requested). They are then asked if they (also) fished from shore on that date, or if the only fishing on that date was from the shore. The same pattern is followed for each day of fishing, with a profile attempted for each trip that occurred during the reference wave. The same person may respond for his/her activities and then respond for others in the household, in that order.

Respondents to the mail survey are also asked if they had a recreational saltwater fishing license. However these questions are asked later in the instrument, after questions about the number of days fished in each mode. In addition, respondents to the mail survey can view the entire questionnaire before answering any of the questions, which is one of the hypothesized reasons that context effects tend to be lower in self-administered mail surveys than in modes involving the use of an interviewer.

License Question Hypothesis

Since the license question arises very early in the telephone interview and much later and less prominently in the mail instrument, we hypothesized that asking about a license might suppress responses about fishing in the CHTS compared to the ABS. In other words, CHTS respondents who weren't licensed may not report fishing activity because doing so might somehow be viewed as illegal or socially undesirable. If this hypothesis is correct, then we would expect the estimated percent of anglers who reported having a fishing license to be much higher in the CHTS than the ABS.

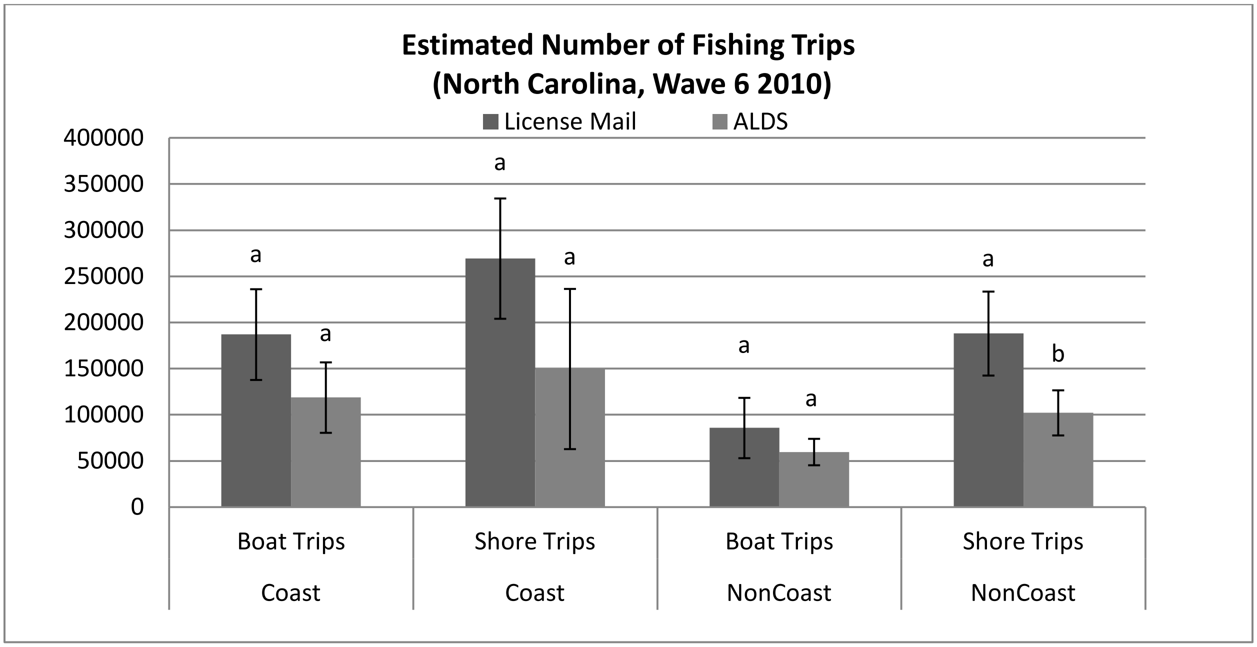
Table 5 shows estimates of the proportion of coastal resident anglers that reported having a license for saltwater fishing in the ABS and CHTS. The estimates show that CHTS respondents who reported fishing are also more likely to have reported having a fishing license than ABS respondents, although the differences are not exceptionally large. In the CHTS, nearly all (98%) respondents who reported fishing during the wave also reported that they had a fishing license, while 87% of ABS respondents reported both fishing and having a saltwater fishing license. While the differences are in the direction of the hypothesis, the fact that most anglers in both surveys report having a license implies that the differences are not likely to be major influence on the differences in the estimates.

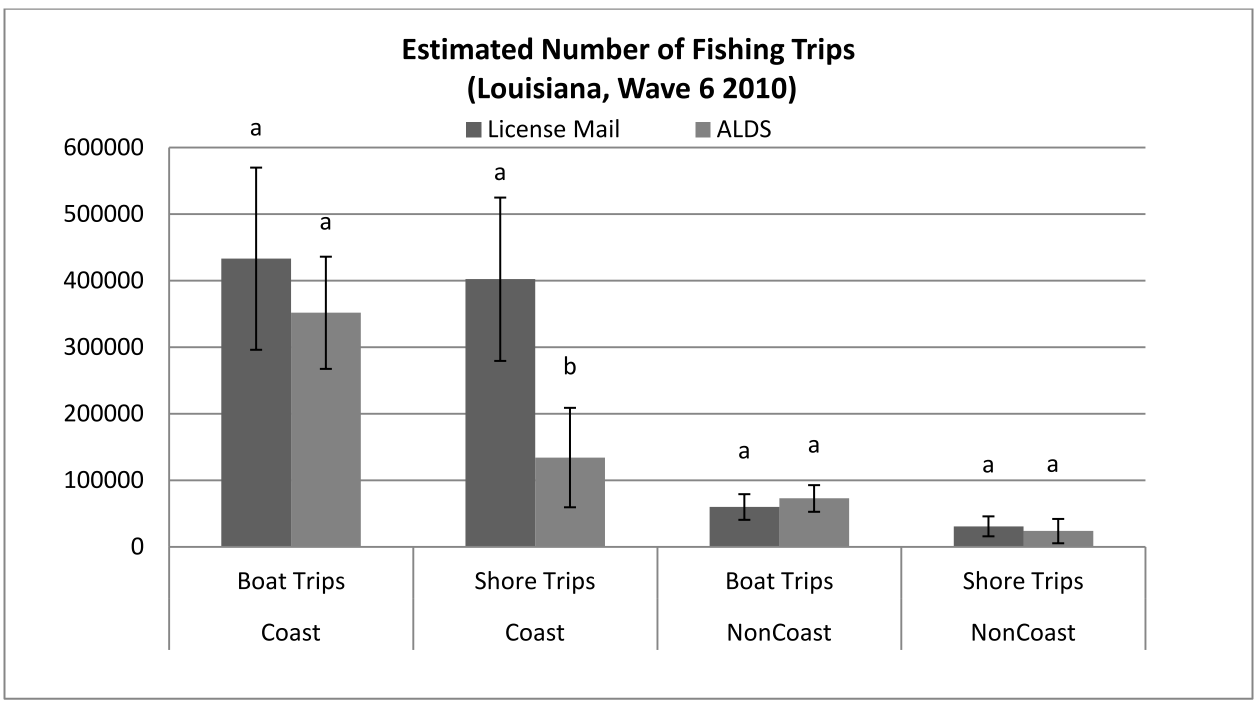
Table 5. Proportion of anglers who reported having a recreational saltwater fishing license that was valid during the reference wave, Wave 6, 2010 (n=total number of respondents, including both those were licensed and unlicensed). Coastal counties only.

	CHTS (n)	ABS (n)
North Carolina	0.97 (121)	0.87 (165)
Boat	0.94 (67)	0.86 (76)
Shore	1.00 (54)	0.88 (89)
Louisiana	0.98 (171)	0.85 (254)
Boat	0.99 (139)	0.87 (136)
Shore	0.97 (32)	0.82 (118)

The ALDS questionnaire is nearly identical to the CHTS questionnaire. However, we would not expect the mechanism generating the license hypothesis to operate in either the license mail survey or the ALDS, since everyone who was surveyed was licensed. Figure 3 and Table 3 show that the license mail surveys estimates more trips than the ALDS, but the differences are not as large as the differences between the CHTS and ABS, and significant differences are limited to shore fishing. This finding provides some additional support to the hypothesis that the license question suppresses reported fishing activity in the CHTS, even though the evidence is neither overwhelming nor quantifiable.

Figure 3. . Estimates of total angler trips for licensed anglers in North Carolina (i) and Louisiana (ii. Within domains (state/stratum/fishing mode), estimates with different letters are significantly different at the α=0.05 level, Wave 6, 2010.





Proxy Reporting Hypothesis

We speculated that proxy responses might give rise to differences in the mean number of trips (persons for whom reports are obtained by proxies might report fewer trips) between the surveys since proxy reporting was more likely to occur for the telephone survey than for the self-administered mail survey. In the literature, self and proxy reports diverge as a function of (1) shared experience; (2) salience of the event; and/or (3) level of communication between the self and proxy. Increased reliance on proxy reports might lead to suppressed reporting of shore fishing trips and higher reports of boat fishing in the telephone surveys. The mechanism for this would be that proxy respondents might be less likely to know about shore fishing trips than boat trips due to the more salient nature of boat trips. This would lead to under-reports of shore-based trips compared to self-responses. However, if fishing trips were shared experiences, we would expect no difference in the rate of fishing trips for those reported by self versus those reported by proxy.

To investigate this we compared the proportion of trips reported by respondent type (self or proxy) and by trip mode (see table 6). Contrary to the hypothesis, proxy respondents were actually more likely to report shore trips than respondents who reported for themselves. We also explored the distribution of the number of trips by mode (not shown) and the proxy distribution was no more heavily skewed toward boat trips than that of the self-responses. A proxy reporting hypothesis is not supported by these data.

Table 6. Proportion of reported trips by mode for self and proxy CHTS interviews with coastal county residents, Wave 6, 2010.

	Private Boat	Shore
North Carolina		
Self	0.59	0.41
Proxy	0.48	0.52
Louisiana		
Self	0.77	0.23
Proxy	0.72	0.29

Imputation Hypothesis

The CHTS and the ALDS have high missing data rates due to the repeating sequence of questions about each trip. Respondents (and possibly interviewers) may find this design burdensome and terminate interviews prior to discussing all trips. The telephone surveys account for incomplete interviews through hot-deck imputation; using the total number of trips reported as the basis, trips are imputed first from completed trip profiles of the same respondent and then from completed trip profiles of respondents within the same household. Fishing mode was imputed for approximately 70% of fishing trips reported in the CHTS and ALDS during wave 6, 2010. We hypothesized that the relatively large magnitude of imputation in the telephone surveys, combined with the sequence of questions in the surveys (if the respondent says they have taken a trip they are first asked if that trip was a boat trip), and the greater salience of boat trips contributed to the higher reports of boat trips in the telephone than in the mail.

Table 7 shows the proportion of reported trips among fishing modes for complete and incomplete CHTS interviews. In complete interviews, trip mode was provided for all reported trips. For incomplete interviews, not all reported trips were discussed, so mode had to be imputed. If the hypothesis has merit, we would expect the incomplete interviews to have a higher proportion of boat trips than the complete trips. The table shows that the distributions of trips among modes are virtually identical for complete and incomplete interviews, providing no evidence to support the hypothesis. We also examined the proportions separately by self and proxy respondents and found the relationship was the same.

Table 7. Proportion of reported trips among modes for complete and incomplete CHTS interviews (Wave 6, 2010), Coastal County Residents.

	Private Boat	Shore
North Carolina		
Complete	0.53	0.47
Incomplete	0.50	0.50
Louisiana		
Complete	0.77	0.23
Incomplete	0.81	0.19

Recall Memory/Saliency Hypothesis

Here we discuss two measurement error topics, recall bias and saliency bias, which are rarely discussed together. In this particular instance, the two potential sources of error are closely related, and both could create bias that is differential between the surveys. Recall bias might arise because the respondent task is different for the mail and telephone surveys. In the mail survey the respondent is asked separate questions to determine the total number of trips by fishing mode during the reference period. Next to each of these questions is a calendar depicting the two months of the reference period that provides the respondent with a visual image to aid recall. In the telephone interview, the respondent is asked a single question to determine the total number of trips during the reference period. It is not until the detailed questions about individual trips that the mode of fishing is requested and recorded. While the mail survey affords the respondent time to consider a total for each mode, the telephone mode requires a summary judgment across all modes with minimal time to consider the request.

The differences in the respondent task across the two data collection modes may be further exacerbated by the differential salience of shore vs. boat trips. The salience hypothesis suggests that boat trips are more salient than shore trips (as we mentioned in the discussion of the proxy hypothesis boat trips may be more memorable events that are stored and retrievable from memory in ways that shore trips are not). To cause a differential bias in the estimates of trips by mode, we hypothesize that anglers with only shore trips might not remember such trips when answering a “cold” telephone request about the trips they have taken, while the mail response can be contemplated longer, resulting in more reports of shore fishing.

If the relatively greater reporting of shore trips for the ABS sample was a function of the different approaches to measurement used in the mail and the telephone surveys, we should see the same pattern of differential reporting for the licensed angler samples (comparing the mail license angler survey to the ALDS). As can be seen in Table 4, there is no evidence of higher *rates* of shore fishing (mean trips per angler) in the license mail survey compared to the ALDS. However, similar to Table 2, we consistently observe a greater *number* of shore-based anglers in the license mail survey than the ALDS. Thus we suspect the source of the difference may be responses to the screening questions presented to the respondent at the outset of the telephone interview. The consistency of the results across the two surveys tends to support the hypothesis that saliency affects the responses differently depending on data collection mode.

We hypothesize that inaccurate responses to the telephone survey screening questions are resulting in recall/salience bias. We further assert that recall/salience bias has a greater impact on estimates of shore fishing effort than boat fishing effort. One approach to test this hypothesis is to assess the responses to the screening questions by gender. If the respondents to the screening questions are more likely to be female, then it might suggest that female respondents are less likely to report fishing in general, and more likely to exclude shore trips due to the lower salience of these events (both the mail and telephone surveys find that men are more likely to fish than women, and this is consistent across fishing modes.)

Table 8 shows the distribution of responses to the CHTS screening questions by gender. Women are more likely than men to be the person who answers the phone and responds to the screening questions about household fishing activity. In both Louisiana and North Carolina, nearly two-thirds of the initial respondents are female, a percentage which is consistent with other RDD studies. An interesting finding is that women are less likely than men to respond affirmatively to any of the fishing screening questions; the rates at which women respond affirmatively to the general saltwater fishing question, the 12-month saltwater fishing question and the 2-month saltwater fishing question are 40-45%, 10-26%, and 34-44% lower, respectively, than the rates of men. The cumulative effect of these observations over all screening questions (later questions are conditional upon affirmative responses to earlier questions) is that the rate at which women report household fishing during the 2-month wave is 72% lower than the rate of men.

This observation could be the result of different compositions of the households. For example, households with women respondents could be less likely to have men present. However, it is also consistent with the recall/saliency hypothesis; women are less likely to fish than men and subsequently may not remember or may not be aware of the fishing activities of other members of the household. The impact of this may be greater for shore fishing than boat fishing, which generally requires a larger investment in both time and money and may be more salient to other members of the household.

If this is the case, or at least a contributing factor, then it might be considered a “gatekeeper effect.” The generic question of whether nonresponse bias is introduced in screening surveys like the CHTS has been raised often, with little in terms of resolution. For example, in 1999 at the Joint Statistical Meeting a session on this topic found some strong evidence for gatekeepers reducing the coverage of the target population in one survey (Horrigan et al., 1999), no evidence in another survey (Meier,1999), and mixed results (Judkins et al., 1999) in a review of several surveys. While we do not know of research that specifically addresses it, we assume the gatekeeper effect is less of an issue in a mail screening survey, where we believe the questionnaire is more likely to end up in the hands of someone within the household who fishes or is likely to know about the fishing activities of other household members.

Given the rate at which women are the respondents to the CHTS, the salience hypothesis could explain some of the observed differences between CHTS and ABS estimates, and also why similar differences are not as evident in the comparison between the mail and telephone surveys of licensed anglers. If women are screening fishing households out of the CHTS, as suggested by the differences in rates of reported household fishing between men and women, then the CHTS is underestimating fishing incidence and subsequently the number of anglers who fished in the wave. As discussed, the impact of this could be greater for shore fishing than boat fishing. Since this is not a designed experiment, the data are merely in the direction consistent with the hypothesis rather than confirmatory of it.

Table 8. Percent of CHTS responding households that reported fishing during the wave by gender of initial respondent, Coastal Households Only.

State	Initial Respondent Male		Initial Respondent Female	
	% of Sample	% Reporting Fishing	% of Sample	% Reporting Fishing
North Carolina	36.5	11.5	63.5	3.7
Louisiana	35.9	17.5	64.1	4.3
Total	36.2	14.3	63.8	4.0

As noted previously, the differences between the ALDS and license mail survey are not as pronounced as the differences between the ABS and CHTS. One of the interesting differences between the CHTS and ALDS telephone surveys is the initial set of screening items. The ALDS asks to speak with a specific individual, the sampled licensed angler, rather than accepting any adult respondent to the initial set of items. As such, the ALDS is not as susceptible to a “gatekeeper effect” as the CHTS. The more subtle differences between the ALDS and license mail survey further support the possibility that the differences may be associated with the screening approaches taken in the surveys.

Finally, another possibility is that infrequent shore trips might be suppressed because they are less frequent and salient (if an angler goes shore fishing often salience is not relevant). To explore this, we compared the distribution of the number of shore trips from the mail ABS (coastal counties) and the CHTS. If the hypothesis were true we might find a smaller percentage of the CHTS respondents with one or two trips. This comparison failed to support the hypothesis; the percentage of respondents in the CHTS with one or two trips was greater than or equal to the percentage in the ABS mail survey.

3.2 Differential Bias due to Noncoverage

While both the mail and telephone surveys have noncoverage issues, the differences in the sources and rates of noncoverage are starkly different for the ABS and CHTS surveys; for the license samples, these differences are less pronounced. The CHTS is a landline RDD sample and only samples households in coastal counties. Since about one in three U.S. households did not have a landline by the end of 2010 (Blumberg and Luke 2011), the exclusion of cell-only households is potentially significant. The exclusion of non-coastal households is also substantial. Adjustments are made for both of these sources of noncoverage as discussed below.

The ABS also has some undercoverage, including the omission of some addresses from the commercial address files. This may be more concentrated in the rural populations where we find that fishing is more prevalent. A second source of undercoverage in the ABS results from the two-phase design that screens households for fishing prior to the reference wave. Some people may fish in the wave but screen themselves out of the ABS sample because they didn't fish in the 12 months prior to the wave. This however, is not a feature of the frame but rather how the sample using the ABS frame was implemented in this survey. One other issue is that the ABS is limited to persons over 18 years old, while the CHTS surveys all anglers irrespective of their age. Since the age of the angler is not obtained in the CHTS it is not possible to compare the estimates from the two surveys by age of the angler. The inclusion of children in the CHTS clearly increases its coverage and thus would increase the difference between the ABS and CHTS estimates beyond that already observed rather than account for some of the observed differences. Once again, this was a design decision for the 2009 and 2010 studies and impacts the present comparisons but could be altered in future implementations of an ABS sample.

Noncoverage Bias: ABS Mail and CHTS

Iannacchione (2011) reviews coverage rates from surveys using USPS files as the frame and states that mail surveys offer near complete coverage of the U.S. household population. He notes that overcoverage due to households having two addresses that receive mail (a street address and a P.O. Box) is likely to be a bigger issue than undercoverage for mail surveys. Even though people living in coastal households are more likely to be rural and also to participate in saltwater fishing, it appears that the ABS provides a frame with relatively minor coverage losses due to this source.

The other source of potential undercoverage is the use of a retrospective question concerning saltwater fishing to determine eligibility. In the two-phase mail survey the screener is mailed prior to the end of wave and some people may not have fished in the last 12 months (the screener item) but may fish in the next two or three weeks that are remaining during the reference period of interest. The 2010 mail screener included a prospective question about fishing in the next three months to help assess the potential for undercoverage. In both states and strata (coastal and non-coastal), about 3 to 5 percent of the households reported that someone in the household might fish in the next 3 months but no one in the household had fished in the last 12 months. Because the question asked about 3 months rather than the next few weeks (the in-scope period) and prospective questions are not very reliable as predictors, we believe that this exclusion is relatively minor for the estimates of the ABS. Furthermore, the noncoverage of the ABS would increase the difference between the ABS and CHTS and does not account for the observed difference.

For the CHTS the exclusion of the cell phone and non-coastal counties is more problematic, and the difference varies by state. In a state like Louisiana, nearly half of the population lives in coastal counties, while in North Carolina only about 20 percent of the population resides in coastal counties. Although non-coastal counties are not sampled in CHTS, an adjustment is made by expanding estimates of coastal fishing effort upward by correction factors derived through an access-point intercept survey of completed fishing trips. Specifically, intercepted anglers are asked for their state and county of residence, and CHTS estimates are then expanded by the inverse of the ratio of CHTS-covered trips (trips taken by anglers in coastal households) to total trips (CHTS-covered trips, as well as trips taken by anglers from non-coastal counties). For example, if 80% of the intercept anglers live in coastal counties then the CHTS estimate is inflated by 1/0.8=1.25. The exclusion of cell phone only households uses a different approach described below.

The total effort estimates by stratum based on the ABS (Table 1) showed that a substantial percentage of the fishing effort was by non-coastal residents, but this differed by fishing mode and state. The ABS estimated that in Louisiana about 75% of boat trips and 84% of shore trips were by coastal residents; in the CHTS the corresponding percentages (derived from the intercept surveys) were 86% for boat trips and 82%for shore trips. In North Carolina the ABS estimated that 65% of boat trips and 57% of shore trips were by coastal residents; the CHTS estimated that 79% of boat trips and 53% of shore trips were by coastal residences. The errors on these estimates are likely to be large so it is difficult to determine whether the adjustments fully adjust for the exclusion of the non-coastal counties. However, it seems fair to conclude that the adjustment for noncoverage in the CHTS based on the data from the intercept survey is not a major factor in accounting for the observed differences between CHTS and ABS estimates.

Since the CHTS only samples landlines, the estimates from this survey also have to be adjusted to account for the substantial loss of coverage resulting from cell-only households. This is implemented by poststratifying the CHTS weights, which have already been adjusted to account for the exclusion of the non-coastal counties, to the number of total households in the state. The implicit assumption is that the fishing activities of the landline sample are the same as the activities in the excluded households. If this assumption is valid, then the residual noncoverage bias due to the exclusion of the cell phone households would be small.

To examine this, estimates of mean fishing trips were computed from the ABS sample by whether the household had a landline or not. The contribution of the households excluded from the CHTS but included in the ABS can be estimated from these data. Note that the way the ABS is weighted produces estimates of the total population that fished in the wave, but does not produce estimates of the total population of all adults (although this could be done). Thus, the estimates from the ABS are of the percent of anglers who live in cell-only households and the percent of fishing trips taken by anglers who live in cell-only or nontelephone households.

Table 9 shows that within a domain, the estimated percentage of anglers and the percent of trips by phone status are fairly consistent. This implies that the anglers from the households excluded from the CHTS take trips at roughly the same rate as the included population. This is one critical assumption that is made in the adjustment of the CHTS estimates. The second assumption made in the CHTS is that the fishing population and nonfishing population are covered by the CHTS at the same rate (i.e., the fishing population has the same rate of cell-only households as the non-fishing population). This assumption cannot be tested from the ABS data because the estimates are only for those who reported fishing in the past 12 months. For example, it is possible, but perhaps unlikely, that households that fish are more likely to be cell-only than those that do not fish. Despite the uncertainty associated with the second assumption, there is no evidence that undercoverage of the CHTS due to non-landline households is a significant contributor to the observed differences between the ABS and CHTS.

Table 9. Percent of Anglers and trips with no landline telephone service, ABS Wave 6, 2010, Coastal County Residents.

State/Mode	Angler	Trips	Ratio of Trips:Anglers
NC Total	35.9	35.3	0.98
NC Boat	38.5	43.2	1.12
NC Shore	32.9	29.3	0.89
LA Total	38.4	43.6	1.14
LA Boat	34.4	31.7	0.92
LA Shore	42.2	49.7	1.18

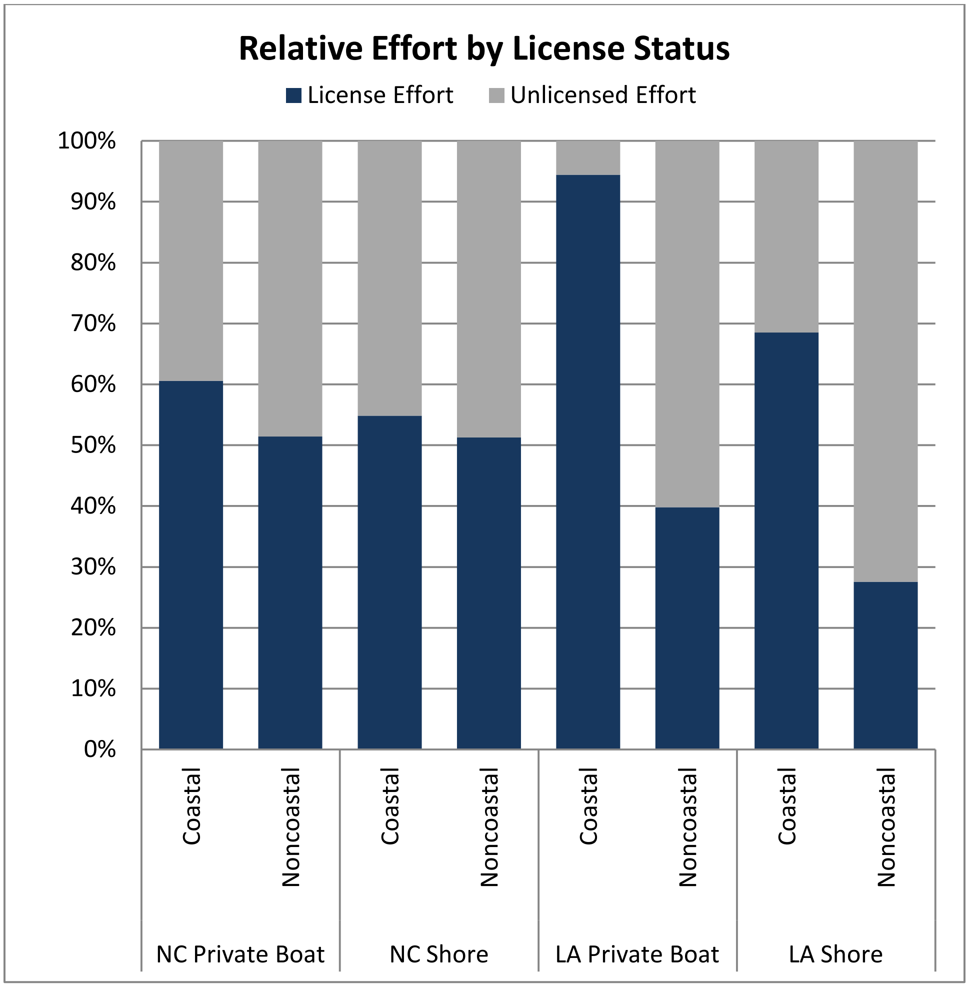
Noncoverage Bias: License Mail and ALDS

The same license frames are used for the license mail survey and ALDS. Nonetheless, there are some differences that could be attributed to coverage, at least in the sense that the licensed angler could not be reached because of insufficient data on the frame to contact the person. The license frames do not have a current and valid telephone number for about 25 percent of the anglers, making them inaccessible by telephone. We classify this as a nonresponse problem rather than a coverage problem in this discussion because the angler can be sampled but not contacted. In the mail survey, nearly all of the sampled anglers can be contacted by mail (although less than 10 percent of the sampled anglers may have the mail returned as being no longer at that address and for other similar reasons).

To assess the extent of undercoverage related to the use of the license frames for sampling anglers in general, we compared the relative distribution of effort between licensed and unlicensed anglers by domain (Figure 4). The estimates presented in Figure 4 are generated through the ABS and license mail surveys. Effort estimates for licensed anglers were derived through the angler license mail survey, while estimates for non-licensed angers were derived by subtracting license estimates from total effort estimates, which were derived through the ABS mail survey and include both licensed and unlicensed fishing activity.

In North Carolina, the distribution of effort between licensed and unlicensed anglers is fairly consistent among strata and modes, with unlicensed fishing activity accounting for 40-50% of the total effort estimates. This contrasts sharply with LA, where fishing activity by unlicensed anglers varies considerably among strata and fishing modes, accounting for less than 5% of total fishing effort for private boat fishing by coastal residents up to nearly 75% of total effort for shore fishing by noncoastal residents. Despite the variability in coverage among domains, these results clearly demonstrate that fishing activity by unlicensed anglers is substantial and cannot be ignored by sampling exclusively from state databases of licensed anglers for either telephone or mail surveys.

Figure 4. Relative distribution of effort between licensed and unlicensed anglers, Wave 6, 2010 mail surveys.



3.3 Differential Bias due to Nonresponse

Unit response rates for each of the surveys are presented in Table 10. Response rates for the ABS screener and the CHTS were calculated using AAPOR RR3[1]. For the ABS screener, '*e*' was calculated separately for addresses that could and could not be matched to a telephone number, and for the CHTS, '*e*' was calculated separately for telephone numbers that could and could not be matched to an address.

[1] The terminology used in this section is from the American Association of Public Opinion Research's "Standard Definitions: Final Dispositions of Case Codes and Outcome Rates for Surveys." The report is at www.aapor.org.

For the license sample and 2nd phase ABS sample, we assumed that all sample units were eligible and consequently calculated response rates using AAPOR RR1.

Table 10. Unit response rates, Wave 6, 2010.

	ABS Frame			License Frame		
	Screener	Angler Survey	Total ABS	Mail	ALDS (phone)	CHTS (RDD)
Overall	46.69	65.4	30.54	49.3	28.9	17.57
North Carolina	48.07	68.9	33.12	50.6	30.9	21.7
Coastal	49.19	68.9	33.89	51.39	31.25	21.7
Noncoastal	47.8	68.9	32.93	48.44	30.32	NA
Nonresident	NA	NA	NA	62.97	34.48	NA
Louisiana	43.78	60.1	26.31	47.2	25.35	14.06
Coastal	45.48	62	28.20	47.7	24.28	14.06
Noncoastal	41.76	55.9	23.34	45.2	26.9	NA
Nonresident	NA	NA	NA	50.9	28.2	NA

Overall, the response rate for the ABS screener was 46.7%, and the response rate for the 2nd phase ABS angler questionnaire was 65.4% for an overall response rate for the ABS sample of 30.5%. This compares to an overall response rate of 17.6% for the CHTS. Sampling from the License frame resulted in overall response rates of 49.3% for the mail mode and 28.9% for the telephone mode.

These response rates are all relatively low, introducing the potential for bias due to nonresponse error. The mail surveys have response rates that are up to twice that of the CHTS survey. In addition, compared to the ABS survey, the CHTS response rates would probably be lower if it were not restricted to landline telephone households (AAPOR 2010). However, response rates alone are poor indicators of nonresponse bias, and it is even possible that the lower response rate survey could be less biased than the higher one (Groves 2006).

Nonresponse bias in estimates of means and proportions only occurs when response rates are differential across domains, and those domains are correlated to the characteristic being estimated. In these surveys, these conditions would exist if those who fish more often are also more likely to respond to the survey than those who don't fish or those who fish less often. For estimates of totals, such as total fishing effort, nonresponse bias may be even more of a problem since totals are always underestimated unless some type of nonresponse adjustment is made (Brick and Jones, 2008). If the adjustment does not account for differential nonresponse related to the outcome measure, then the bias for the estimated total can be in either direction.

Total fishing effort can be written as the product of the number of anglers who fished in the time period and the average number of trips they took. The survey estimates of totals can be biased if either or both of these components of total effort are over-estimated or under-estimated. Overestimation in recreational fishing surveys is common and is referred to as avidity bias, which is a form of saliency bias in more generic survey terminology and is discussed in our measurement error section. We concentrate on avidity bias here as it relates to unit nonresponse because it is likely to be a major source of nonresponse bias.

Avidity Bias: ABS Mail and CHTS

The only existing measure of avidity bias available at this time is obtained by comparing response rates from the general population surveys (ABS mail and CHTS) by whether or not the household could be matched to the license frame for the state. This is an imperfect measure of avidity because it classifies households as avid solely by whether they live in a household with at least one licensed angler. In addition, there are other issues, such as matching error, that affect these estimates of bias and are discussed later. Despite its limitations, this measure of avidity bias provides some insight into the effects of nonresponse bias.

For this analysis we restrict the ABS sample to the coastal stratum to be consistent with the geographic coverage of the CHTS. We also focus mainly on the estimation of the number of anglers. Estimates of mean trips per angler are not highly variable by matching status.

Table 11 provides response rates for the CHTS and ABS by matching status. The overall response rate for the matched ABS address cases was 1.57 times that of the unmatched address cases (44.1% compared to 28.1%), where this accounts for both the screening rates (59.7% matched and 45.3% unmatched) and the extended response rates (73.9% matched and 62.1% unmatched). For the CHTS the ratio of the response rates for the matched to the unmatched was similar at 1.48, where the response rates were 24.1% for the matched households and 16.3% for the unmatched households.

Table 11. Response rates by license match, Wave 6, 2010.

	Screener		Angler Survey		Total ABS		CHTS	
	Matched	Unmatched	Matched	Unmatched	Matched	Unmatched	Matched	Unmatched
Overall	59.7	45.3	73.9	62.1	44.1	28.1	24.1	16.3
North Carolina	62.4	46.8	76.6	66.3	47.8	31.0	31.4	19.8
Coastal	61.0	46.9	77.3	64.3	47.1	30.1	31.4	19.8
Noncoastal	64.0	46.8	75.9	67.2	48.6	31.4	NA	NA
Louisiana	55.3	42.2	70.8	55.0	39.2	23.2	17.4	13.5
Coastal	57.3	43.3	69.9	57.6	40.0	24.9	17.4	13.5
Noncoastal	51.0	40.9	73.8	50.3	37.6	20.6	NA	NA

Montaquila et al. (2008) used ratios of rates like these to approximate the magnitude of nonresponse bias in estimates. Using their formulation and assuming that the estimated percent of anglers is 25%, and a ratio of response rates of 1.6 between the matched and unmatched samples, results in an overestimate of about 30 percent. Instead of estimating that 25% of the coastal households have active anglers, the higher response rate for avid anglers yields an estimate of about 33%, an absolute bias of nearly 8 percentage points.

Since the response rate ratios between matched and unmatched households for the ABS and CHTS are both considerably greater than one, we would expect estimates from both surveys to be biased due to this source of nonresponse error. However, the ABS employed a nonresponse weighting adjustment to account for this potential source of nonresponse bias while the CHTS did not. This was done by defining nonresponse adjustment cells by whether or not the household was matched to the license frames. This adjustment reduces the effect of avidity bias substantially for the ABS; a pilot study conducted in North Carolina in 2009 demonstrated that adjusted ABS estimates of the number of anglers who fished in a wave were 25% lower than unadjusted estimates (Andrews et al., 2010). As a result, avidity bias in the ABS is the residual after accounting for the license population, i.e., only avid anglers in households that could not be matched to license frames could have contributed to avidity bias in the ABS.

We note that the CHTS estimates could use the same types of nonresponse adjustments as used in the ABS sample to reduce avidity bias. The adjustments are likely to be slightly less efficient due to higher matching errors, as evidenced by the lower response rate ratio between matched and unmatched households. This is discussed in more detail below, but the estimates would undoubtedly have lower nonresponse bias due to avidity. In fact, Andrews et al. (2011) demonstrated that CHTS estimates of total fishing effort employing this type of nonresponse weighting adjustment were 13% lower than unadjusted estimates over a three year period from 2008-2011. However, the lack of an adjustment for avidity bias in the traditional CHTS design is clearly an important difference between the ABS and CHTS and a likely contributor to the observed differences in estimates.

A second consideration is that errors in matching the ABS sample to license databases have an effect on the ratios of the response rates and the size of the bias. As discussed later, about 13.4% of the CHTS sample can be matched to the license frame, which accounts for approximately 66% of the total number of anglers on the license frame[2]. For the ABS, about 14.5% of the coastal sample matched to the License frame, which accounts for about 77% of the license frame. We assume that both would match at 100%, within sampling error, if there were no other errors. The matching errors are largely the result of errors in the frame data that was used for matching (address and telephone number). The response rate ratio is a function of this error. For example, the cases that should have matched to the license frame but didn't due to matching errors are likely to respond at a higher rate than "true" unmatched cases (i.e. they are likely to respond at the rate of the cases that could be matched). This artificially increases the response rate for the unmatched domain and subsequently depresses the ratio described above and the estimated avidity bias. Based on the simple percentage matched, it is possible that matching error is more prevalent for the CHTS than the ABS, and thus the effect is larger for the CHTS. However, since less than 20% of the general household population is on the license frame, the overall effect of this matching error through response rates is not very large.

Overall, differences between the ABS mail estimates and the CHTS telephone estimates can be attributed to differences in the ways the estimates are adjusted rather than to the underlying response propensities between the two surveys. Both surveys suffer from differential response rates due to the propensity of households with avid anglers to respond at a higher rate than other households. The adjustment of the weights for the ABS sample significantly reduces the estimated number of anglers (specifically those with licenses). However, since the ABS estimates are higher than CHTS estimates in terms of estimated numbers of anglers and total effort, the effect of avidity bias as postulated above would increase the differences between the surveys if the CHTS estimates were adjusted in the same way. The evidence in this case does not explain the observed difference between the ABS and CHTS as

[2] Based upon matching, the estimated number of licensed anglers from the CHTS sample is 66% of the actual number of individual anglers on the license frames.

much as it suggests that the difference would be even larger if not for the differential avidity bias adjustment.

Avidity Bias: License Mail and ALDS

Like the general population surveys, the license mail surveys have higher response rates than the telephone surveys. Across the two states, the license mail survey response rate was 1.7 times higher than the ALDS response rate (49.3% for the mail and 28.9% for the telephone). The ratio was relatively consistent across states and strata, ranging from 1.6 to 2.0. As noted above, this does not necessarily imply that the mail survey has smaller nonresponse biases.

Avidity bias is possible even from within the License frame, since some license holders may fish more often than others and may have a greater propensity to respond to the survey. We might expect avidity bias to be less problematic for these surveys because everyone on the License frame is more likely to participate in at least some type of outdoor recreational activity. In fact, the differences between the estimates of the number of anglers as computed in the license mail survey and the ALDS are much smaller than the differences between the ABS and CHTS discussed above. Most are not statistically significant, and the big differences are mode-specific (shore trips), which suggests a different error source rather than unit nonresponse.

It is obvious that possession of a fishing license in the samples cannot be used as a measure to assess avidity bias since, by definition, all sampled individuals have a license. An approach we examined for these surveys was to use the type of license to create nonresponse weighting adjustment categories, assuming that anglers with some types of license were more likely to be frequent saltwater anglers than others. Andrews et al. (2010) describe an initial investigation of this in the 2009 pilot study in North Carolina and suggested that despite inconsistent results the approach was worth further study.

For the current study, we defined categories based upon the duration of the license (e.g. lifetime, annual, short-term) and the scope of privileges that the license permitted (e.g. saltwater fishing only, combination licenses, etc.). The categories were designed such that anglers within each category were expected to be similar in terms of both propensity to respond to the survey and fish.

After adjusting the weights within these categories, estimates of total fishing effort were recalculated and compared to the original estimates. The differences in effort between the two weighting procedures were small and generally not substantive. One hypothesis consistent with this result is that avidity bias is not large in the license mail survey and doing the revised weighting adjustment is ineffectual. Another possible explanation is that avidity bias is present, but not highly correlated with the type of license. Due to the null effect in the mail survey, the same type of weighting adjustment was not considered for the telephone survey.

We conclude that avidity bias is not likely to be a major source of nonresponse bias for the license samples. At the least, our investigation has not been able to detect avidity bias. More importantly, we found no evidence that differences between the licensed mail estimates and the ALDS telephone estimates of effort are related to avidity bias.

While avidity bias may not be a major concern in the license samples, there may be other sources of differential unit nonresponse in these samples. For example, the ALDS uses the telephone number in the license frame and it may be either a landline or cell phone number. It is possible that cell phones have lower response rates than landlines, but we do not have any data on this specific issue.

3.4 Differential Bias due to Matching Errors in Dual-Frame Designs

The current dual frame designs have overlapping domains and produce “unbiased” estimates for the overlap domain from the two surveys. Those estimates are then averaged or composited to produce unbiased and more precise estimates for the overlap domain. For the mail surveys, the overlap is the group of licensed anglers who reside in the state and have an address that can be used to send the mail questionnaire. For the telephone surveys, it is the group of licensed anglers who have a telephone number that can be used to reach them rather than an address. Conceptually, these two are similar, but operationally there are differences that might induce differential bias. The main culprit is the ability to match the general population samples (either the RDD or ABS) to the license frame. Before we describe the matching issue in more detail we cover some related issues.

Self-reported Domains

Matching the general population survey to the license frame is difficult. An alternative option that is worth considering is to rely on the general population survey respondents to report whether or not they have a license and use this to define the overlap. One problem with relying on self-reports is that no data on the domain are obtained for those that do not respond. Since the response rates are very different for the two samples (e.g., the ABS response rate is much lower than the license survey response rate), there is a serious potential bias if we ignore this (see Brick et al. 2011 for the same problem but in the context of cell and landline dual frame surveys). Brick et al. (2011) suggest using an adjusted compositing factor based on the differential response rates to reduce bias, but this has not been explored in the current context.

A second issue, and part of the reason the alternative compositing factor has not been investigated more thoroughly, is that respondents do not necessarily report their license status accurately. Andrews et al. (2010) investigated this in the pilot study in North Carolina and found both under-reporting and over-reporting of license status. Until this phenomenon is better understood, it is difficult to implement any estimation scheme that relies on self-reported license status.

Matching Bias

We refer to matching bias as the error in dual frame estimates that occurs because units that should or should not be identified as part of the overlap are misclassified: some units should have been included in the overlap and are not appropriately down-weighted, and some units should have been excluded and are down-weighted when they should not be. Both types of error are possible, but we observed that in the 2010 survey the failure to match was likely to be the dominant error. Thus, we expect over-estimation because units were excluded from the overlap and not down-weighted appropriately..

Note that the matching error discussed here does not affect comparisons between ABS and CHTS estimates except when we are talking specifically about dual frame estimates. In the nonresponse bias section, we did discuss matching error as a source of nonresponse bias. We are not discussing that error at this time, but instead are considering the effect on dual frame estimates.

As mentioned earlier, about 13.4% of the CHTS sample was matched to the license frame accounting for about 66% of the total License frame. For the ABS, about 14.5% of the coastal sample matched to the license frame, which accounts for about 77% of the license frame. The CHTS sample is lower due to the exclusion of the non-telephone population and the imperfect link between telephone numbers and addresses (less than two-thirds of valid phone numbers can be linked to an address that was used in matching). Part of the problem of matching telephone numbers is the prevalence of multiple phone numbers in a household when we include both cell and landline numbers.

Given the adjustments in the CHTS for noncoverage, it is difficult to specify the magnitude of the matching error on estimates of totals for the dual frame telephone surveys. Instead, we concentrate on the effect of matching errors on estimates from the mail dual frame survey. The overlap constitutes

38% of the number of total trips as estimated from the ABS. Let's assume that 10% of the sample that are in the overlap are mis-classified into the non-overlap domain due to matching error. These cases should have their weights reduced by a factor of 2. If matched properly, the estimate of total trips would be reduced by less than 4 percent, hardly a substantial difference given the other sources of error. Even if the matching error was 33%, the reduction in the number of total trips would be less than 12 percent. Furthermore, since the error is not one directional as assumed in these calculations, the errors of overmatching would reduce any bias due to matching error implied by these figures.

While matching error is likely to result in an overestimate in the dual frame design, the effect is not large for the dual frame mail survey, at least not in the two states that were tested. If the percentage of total trips in the overlap were larger, as is likely to be the case as state license frames become more complete, then the effects would be more substantial. This finding suggests that the dual-frame approach, with the efficiencies it brings in terms of identifying anglers at a relatively high rate, is likely to be a reasonable alternative design to the CHTS in terms of coverage, at least while the license frames are being improved. However, we propose an alternative design below that maintains the efficiency and coverage of the dual-frame design, but eliminates much of the complexity and the potential for matching bias.

3.5 Summary of Differences and Errors

Differences in estimates of fishing effort between the mail and telephone surveys are large. ABS mail survey estimates of total angler trips are significantly greater than CHTS estimates overall, and the differences are especially large for estimates of shore fishing. The differences are largely due to the estimated number of anglers, rather than the estimated mean trips per angler. The mail surveys (both ABS mail and license mail) result in higher estimates of participation in both shore and boat fishing than the comparable telephone surveys (CHTS and ALDS). The differences are more pronounced in the general population surveys (ABS and CHTS) than the license surveys.

When such differences exist for estimates of phenomenon such as fishing that are relatively rare activities, a common approach has been to assume that "more is better" whenever social desirability bias would lead to under-reporting. However, this approach does not always apply. For example, Leigh, Gillmore, and Morrison (1998) examine differences between diary and retrospective recall approaches for estimating alcohol consumption and sexual activity and conclude that errors for the two characteristics go in opposite directions due to the important measurement errors. In this case, alcohol consumption in excess is a socially undesirable characteristic and is generally under-reported while sexual activity is over-reported due to prestige bias. In our analysis, we did not assume that higher estimates of fishing effort were necessarily better and took a balanced approach.

We explored the potential influence of measurement error, noncoverage error and nonresponse error on the observed differences between mail and telephone survey estimates. We also assessed matching error, but this only affects dual frame estimates, so it is not central to our review. We found evidence of nonresponse bias in both the ABS and CHTS, and we observed that the propensity of more avid anglers to respond appears to affect both surveys roughly equally. The ABS estimates are at least partially adjusted for this type of bias while the CHTS estimates are not. However, the impact of avidity bias on CHTS estimates is in the opposite direction from the observed differences between ABS and CHTS estimates (i.e. adjusting for avidity bias in CHTS would make the estimates more different). We conclude that while nonresponse bias is an issue of concern in recreational fishing surveys it is not a major contributor to the differences between the ABS and CHTS estimates.

For noncoverage, we found the potential for error in both general population surveys, but with greater potential in the CHTS because it excludes households without landline telephone service and only samples coastal households. CHTS estimates are adjusted to account for these exclusions, and these adjustments seem reasonable, but one crucial assumption, that the survey covers the fishing and non-fishing populations at the same rate, cannot be evaluated from our data. Clearly, the adjustments improve the CHTS estimates substantially and appear to make them more comparable to the ABS estimates, which do not suffer from the same sources of noncoverage errors. We again found no evidence that undercoverage is a significant contributor to the observed differences between the ABS and CHTS. Of course, with the use of cell phones continuing to rise each year, relying on this type of adjustment has considerable risks.

We found that the most likely contributor to the differences between telephone and mail survey estimates is the measurement approach, although the evidence for this is not overwhelming. The tasks imposed on the respondents are dramatically different for the telephone and mail surveys. To evaluate the potential for measurement errors to account for the differences, we examined the respondent tasks and developed hypotheses that could be tested, at least approximately, to shed some light on the mechanisms at work.

One hypothesis was that the placement of the license question very early in the telephone interview, compared to the later and less prominent position in the mail instrument, might suppress responses from persons without a license, and subsequently depress the telephone estimates. When tested, we found that the differences are in the direction of the hypothesis, but they are not very substantial.

Another hypothesis was that proxy reporting, which is permitted in the telephone survey, might give rise to differences in the mean number of trips between the surveys and produce higher reports of boat fishing in the telephone surveys. No evidence for this was found; persons for whom the data were collected by proxy had approximately the same mean number of trips as those who responded for themselves, and the distribution of trips by mode for the proxies was no more heavily skewed toward boat trips than self-responses.

A third hypothesis was that incomplete and imputed responses in the CHTS, when combined with the structure of the questions in the telephone interview, might be responsible for some of the differences between the CHTS and ABS. We hypothesized that incomplete CHTS responses might have a higher proportion of boat trips than the complete trips. However, there was no evidence to support this hypothesis as the distributions of trips among trip modes were virtually identical for complete and incomplete interviews.

Finally, we explored recall bias and saliency bias. Although the nature of the recall task is essentially the same for estimating the total number of trips for the ABS and CHTS, the CHTS requires episodic recall to determine the fishing mode of each trip, although no testable relationships were found to explore this. The salience component of the conjecture is that boat trips are more salient than shore trips, and anglers with only shore trips might not remember such trips when answering a "cold" telephone request about the trips taken.

We hypothesized that the responses to the screening questions by gender might be indicative of this type of error because females are less likely to fish and may be especially likely to exclude shore trips if they are lower salience events. We found that in the CHTS women are more likely than men to be the person who responds to the screening questions about household fishing activity and are less likely than men to respond affirmatively to any of the fishing screening questions. The cumulative effect was that the rate at which women report household fishing during the 2-month wave was 72% lower than the rate of men. While this could be due various reasons, it was consistent with the hypothesis. We also assume that the error might be greater for shore fishing than boat fishing because of the larger investment associated with boat fishing, making such trips more salient than shore fishing trips

The CHTS and ALDS telephone surveys have very different screening items, but are nearly identical otherwise. We suspect that the ALDS approach of speaking with a specific individual (e.g. the sampled license holder) reduces the recall/saliency error differential between the mail and telephone surveys. Again, this is consistent with the much larger differences between the CHTS and ABS mail estimates than the license frame survey estimates.

Our general conclusion is that measurement errors are very different in the current mail and telephone general population surveys, and these differences are responsible for most of the differences in estimates. We especially suspect that the screening approaches in the mail and telephone surveys are at the heart of the differences. While we do not have external data sources to confirm that one approach has less bias than another, our investigations and hypotheses lead us to believe that the mail survey estimates are subject to less bias (across all sources of error) than the telephone surveys.

4. Design Alternatives

The review of the differences between the mail and telephone surveys has provided some insights into the potential for using different designs to reduce errors in angler effort surveys. Two alternative design options are discussed below. We begin with the alternative that is already being tested starting in early 2012, since the findings from these analyses have implications for the way we view this alternative.

4.1 Mixed Mode Alternative

The approach being tested in 2012 is to use a mail survey to screen the general household population to identify anglers and then divide the respondents into random subsamples and conduct the second-phase angler survey by both telephone and mail. The license surveys, which only have one phase, will be similarly subsampled into random telephone and mail treatments.

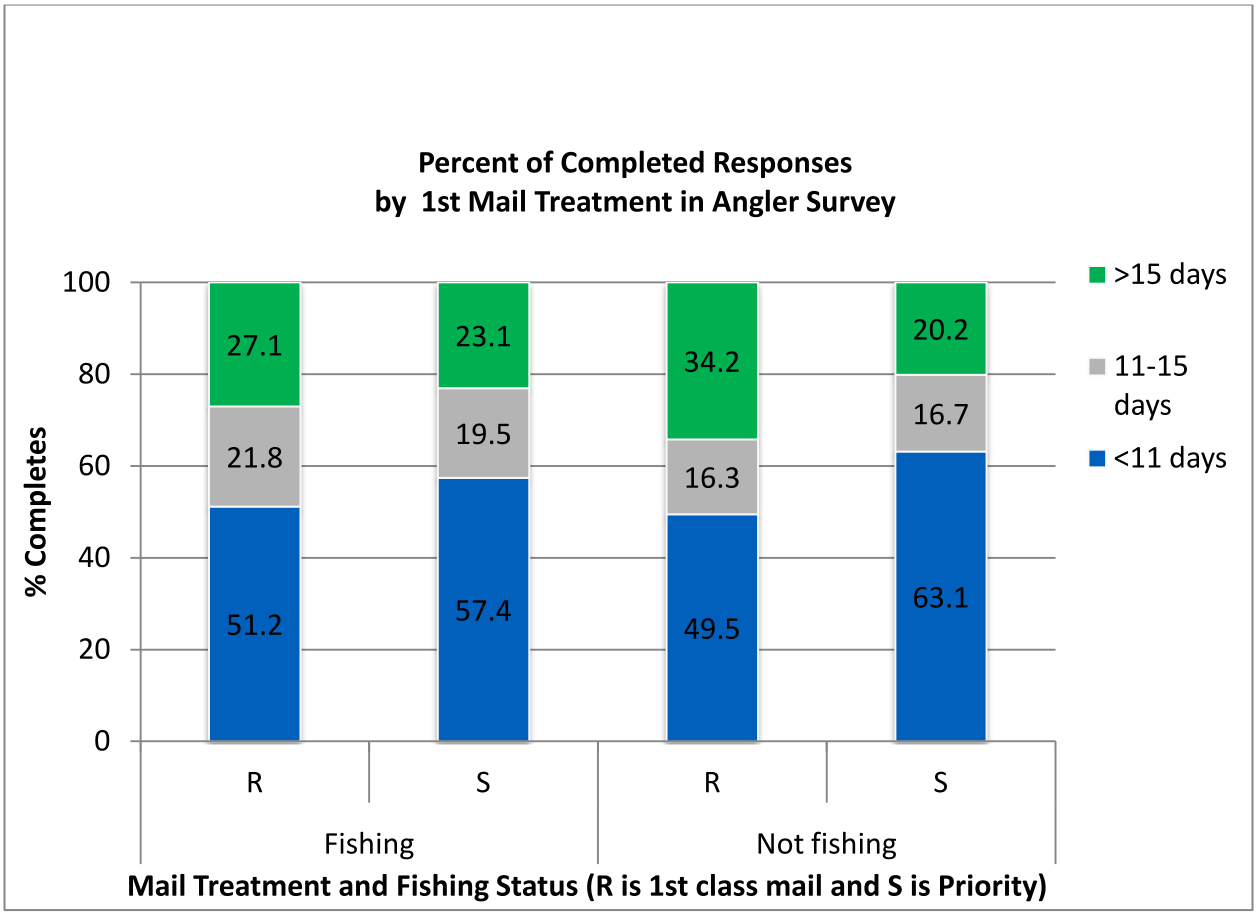
The main rationale for testing this approach is to increase the timeliness of the data collection, which is a key concern when the estimates are required quickly to support management action. If the telephone approach to the second phase is successful, then the estimates can be produced in the same time frame as current CHTS estimation. The corresponding approach being considered for the mail surveys is to use the early returns from the second-phase mail survey to produce preliminary estimates that will be adequate for the same purpose.

Figure 5 shows the percentage of all the second-phase responses in the 2010 mail surveys that were completed and returned by the elapsed time from the initial mailing by whether the adult fished in the wave or not. The survey tested both regular 1st class mailing and special Priority mailing, and the graph shows the results for both of these conditions. Of primary interest in this context is the result that about 70 percent of all the responses were obtained within 15 days of mailing, with the lowest percentage being 65 percent. The percentages who fished are also relatively stable supporting the idea that preliminary estimates based upon early responses might be valid.

Another very important feature of this alternative design is that it moves the screening operation to the self-administered mail mode. Our review of the differences in the previous section concluded that the telephone screening could be responsible for many of the differences between the ABS and CHTS estimates. This difference is eliminated under this alternative. In addition, the angler questionnaires are also being drastically altered for the telephone component of the mixed-mode approach, simplifying the response tasks to be more consistent with those imposed on the mail survey respondents.

These two changes will be confounded, and it will be difficult to ascribe specific differences in the estimates to changes in one of the two phases of the survey. The license frame surveys should help to clarify these effects because this survey does not have a mail screener, and it will use the revised angler telephone survey questionnaire. Thus, any differences between the ALDS (which continues without changes in the interview) and the mixed mode telephone interviews should be easier to attribute to the new instrument. A limitation of this design is that it will continue to be susceptible to matching bias resulting from frame matching errors, as described above.

Figure 5 . Distribution of returned angler questionnaires in the ABS survey by elapsed time between first survey mailing and receipt of completed questionnaire, Wave 6, 2010.



4.2 Single-Phase Mail Survey Alternative

A more radical alternative is to change the design of the mail survey from a two-phase sample to a single-phase sample. Before describing this alternative single-phase mail survey design, it is worthwhile to review the rationale that led to the adoption of the two-phase method for surveys of anglers. One reason for using a two-phase approach is that fishing is a relatively rare phenomenon and sending multi-page questionnaires to households to obtain responses may be more expensive and obtain lower response rates than the two-phase method. The two-phase approach uses a simple and short screener with a more extensive questionnaire sent only to anglers identified in the first phase. A second reason is that some households have more than one angler, which requires sending more than one questionnaire per household. This adds to the expense as noted above; it may also result in some loss of control of the sample in households with multiple anglers. Questions would arise on whether all the anglers in the households responded or not. With two phases, adults can be subsampled from multiple angler households based on the responses from the first phase. Finally, the first-phase responses provide data to personalize the second- phase angler survey to the specific adult and reduce reliance on proxy responses.

The single-phase approach seems more feasible now because several changes have been made in the angler survey instrumentation. First and foremost, the angler questionnaire itself has been revised substantially and is now shorter than it was before (see the appendix for the 2010 angler survey – it is only three or four pages of items for each angler). The reduction of the size and content of the angler questionnaire makes it considerably less expensive to send to a general population sample than the earlier version. It also may make it possible to achieve response rates as high as or even higher than the two-phase approach because the package will not appear to be bulky and may not be perceived of as imposing a major burden on the household. While this is conjecture that needs to be tested, the shorter angler questionnaire certainly improves the chances of achieving higher response rates in a single-phase survey.

The remaining advantage of the two-phase approach that cannot be addressed with a single-phase alternative is the ability to know the number of anglers in the household and personalize the questionnaires to avoid proxy responses. It might be possible to include questions on other anglers in the household in the angler questionnaire itself (to deal with households with multiple anglers in which only one responds), but this needs to be explored. Specifically, the questionnaire items must be developed to obtain household-level data but not change the distinct advantages of a short angler questionnaire.

Nonresponse in a single-phase survey may also be qualitatively different from that in the two-phase surveys that have been studied to date. A serious concern is the potential for avidity bias to be more substantial in a single-phase survey. The 2010 two-phase ABS survey attempted, with little success it must be admitted, to reduce avidity bias by placing the fishing questions within a larger, outdoor recreation context. The single-phase survey cannot do this since the angler questionnaires are included in the initial mailings. The 2010 survey found evidence of avidity bias as measured by license status at both the first and second phase of the survey. The first phase is most troubling because the second phase can be addressed somewhat by nonresponse adjustments using the first-phase responses. An outstanding question is whether the single-phase survey will have more substantial nonresponse bias than the two-phase design.

Design features in the one-phase survey may be developed to help reduce this possibility. One particularly important component in the survey may be the use of incentives in the initial mailing. The literature on incentives generally does not show big effects in terms of reducing nonresponse bias, even though it is consistently effective in raising response rates in mail surveys. One of the most convincing examples of the ability of nominal incentives to reduce nonresponse bias is reported by Groves et al. (2006) and it parallels the situation faced in the angler effort surveys. Groves et al. (2006) showed that a $2 prepaid incentive in a mail survey of birding reduced the “avidity bias” in that survey substantially and thus improved estimates of totals.

The sample design we recommend for the one-phase survey is a stratified alternative to the dual frame approach that changes how the license frames are utilized. The goal of the stratified alternative is to retain the efficiency of sampling from the license frame while avoiding some of the potential biases and complexities associated with the dual-frame design. The current dual-frame approach is to sample independently from the general population (either RDD or ABS frames) and from the license frame, and then combine the overlap population (those on both frames) using a composite estimator. A problem with this design is that the identification of the overlap is difficult and error-prone. As we discussed previously, matching is required to identify the overlap because self-reported license status has a host of errors. Even with matching, errors in matching addresses and telephone numbers may result in biases in the estimates.

A solution to this problem is to use the license frame data for stratification rather than in the dual-frame structure described above. In the alternative design, sample is selected from the general population survey at a rate that will allow for subsampling. For purposes of illustration, let's assume we sample the general population with a sampling fraction of three times the rate needed for the target sample size. This sample is then matched to the license frame, and the sampled households are classified as either matches or non-matches. All of the matched households are retained in the sample, and the non-matched households are subsampled such that only one-third are retained in the sample. Essentially, the license frame has been sampled at three times the rate of the general population, which increases the efficiency of the survey. Because the matching is only used to determine the sampling rate, matching errors will only impact the efficiency of data collection; they will not result in biased estimates. This is a potentially substantial benefit over the dual-frame design, where matching errors are likely to introduce biases. This approach will be especially effective when sampling from the ABS frame, which is relatively complete. The design may be less suitable for RDD surveys, which are more susceptible to undercoverage.[3] The current license frame is also used to sample anglers with licenses who reside outside the state of the license. This group of anglers is not in the overlap and poses no overlap issues. It is recommended that out-of-state residents with licenses continue to be sampled directly from the license frame.

The stratification approach also provides some sampling flexibility that the current dual frame approach does not have. In particular, state license frames that are not up-to-date are less problematic in this design than in the current dual frame method. Assuming the household is still on the general population frame, the out-of-date license frame affects only the variance of the estimates because the newly licensed households, which would be absent from the license frames, are included in the non-matched strata and subsequently sampled at a lower rate than would be desired. However, they are assigned

[3] The only concern potential for bias is that some households are on the License frame but are not on the general population frame. The ABS frame has high coverage as discussed in the previous chapter, while the RDD frame is less complete and this could cause problems in the context of RDD surveys.

weights consistent with their sampling status so that the estimates are unbiased. Loss in precision resulting from out-of-date license frames can be compensated for by increasing the overall sample size, although this is accompanied by an increase in survey cost. With the current dual frame approach, the date of the license is a source of error that may result in biases due to matching errors. Of course, an out-of-date license frame is still a potential source of bias for sampling non-resident anglers, but this is the case regardless of how the available license is used for sampling.

The stratified alternative is especially well suited to the single-phase survey because the approach to all households is the same, regardless of whether the household is matched or not matched. In the current dual frame design, licensed anglers are sample as individuals rather than at the household level and in a single phase[4]. Finally, it is worth noting that the sampling design proposed is not new; it is called a dual frame sample with screening prior to data collection by Lohr (2009).

There are several issues that must be addressed to implement this design. One issue is data collection costs resulting from additional sampling and matching. These costs may be partially offset by gains in sampling efficiency. Another issue is determining the rate of oversampling such that gains in efficiency are maximized. Results from previous pilot studies may help determine optimum sampling levels. Perhaps the biggest challenge is developing the appropriate instruments for a single-phase survey. This could involve sending multiple questionnaires to all sampled households or a more innovative approach that uses a single questionnaire that accommodates multiple anglers. To deal with the timeliness issue, the preliminary estimates approach described in the mixed mode alternative would have to be used.

Despite some of the challenges and unknowns associated with the single-phase survey, we believe it has many advantages that warrant careful evaluation. We believe it has the potential to address many of the challenges that surveying angler effort presents.

[4] In the current mail dual frame design, unlicensed anglers who live in a household with a licensed angler are covered when sampled from the ABS frame only because the licensed angler sample is a single-phase survey that does not cover other anglers in the household.

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Appendix A: CHTS Questionnaire

Hello. I'm calling to conduct a survey for the National Marine Fisheries Service of the U.S. Department of Commerce.
[AS NEEDED: May I please speak with an adult in the household?]

We are collecting information for use in conservation of coastal resources and we would appreciate your help with this important study. Before we begin, I want to assure you that your answers will be kept confidential, and this call may be monitored for quality assurance.

We want to gather information about recreational saltwater fishing. Saltwater fishing includes fishing in oceans, bays, and brackish portions of rivers. This does not include fishing in freshwater, or for shellfish, such as crabbing. Recreational fishing means the primary purpose of the fishing is for fun or relaxation, as opposed to providing income from the sale of fish.

SECTION 1 - INTRODUCTION AND SCREENING

Q1 How many people in this household go fishing?
{If R end interview}

1	*{total response, range 1-20}*	
0	none	*{Set AngCat=1}*
99	R	*{Terminate; code as Resistant}*

Q2 Have I reached you in *{restore county name}* county?

1	YES	
2	NO	
8	DK	
9	R	*{Terminate; code as Resistant}*

Q3 Is this your permanent residence?
{If R end interview}

1	YES	
2	NO	
8	DK	
9	R	*{Terminate; code as Resistant}*

Q4 How many people in total, including yourself, live in your household?
Please include those people who fish and who don't fish.

1 *{total response}*

98	DK	*{Terminate; code as Resistant}*
99	R	

We want to gather information from people who have been recreational saltwater fishing. Saltwater fishing includes fishing in oceans, sounds, or bays, or in brackish portions of rivers. This does not include fishing in freshwater, or for shellfish, such as crabbing. Recreational fishing means the primary purpose of the fishing is for fun or relaxation, as opposed to providing income from the sale of fish.

Q5 How many people in your household, including children and adults, have been recreational saltwater fishing in the last 12 months anywhere in the US or in a US territory?

1-20		
0	ZERO	*{go to Q7/Gender }*
98	DK	*{Terminate; code as Resistant}*
99	R	*{Terminate; code as Resistant}*

Q6 Thinking just about the past 2 months, how many of the people living in your household, including children and adults, have been recreational saltwater fishing in the last 2 months in the US or a US territory?

[Maximum = 20. If response is greater than 5, prompt to confirm number of people who have been recreational saltwater fishing in the last 2 months.]

1-20	*{range=1 to Q16 response}*	
0	NONE	
98	DK	
99	R	{If R end interview, schedule callback}

Q7 INTERVIEWER: Record gender of respondent

1 male
2 female

Q8 During the past 12 months, did anyone in the household have a FISHING LICENSE for the state of {restore state of residence}?

1	YES	
2	NO	*{Go to Q11}*

8	DK	*{Go to Q11}*
9	R	*{Terminate; code as Resistant}*

Q9 Were any of the licenses valid during this period for Recreational Saltwater Fishing?"

1	YES	
2	NO	*{Go to Q11}*
8	DK	*{Go to Q11}*
9	R	*{Terminate; code as Resistant}*

Q10 Were any of these licenses valid between {conditional restore: w1="January, w2=March, w3= May, w4=July, w5= October, w6= December"}12th?

1	YES	
2	NO	*{Go to Q11}*
8	DK	*{Go to Q11}*
9	R	*{Terminate; code as Resistant}*

Q11 I'd like to ask each person who has been recreational saltwater fishing in the last 2 months a few questions about their fishing trip(s). What are the first names of the people in your household who have been recreational saltwater fishing in the past 2 months?

[If respondent will not give names, use identifiers such as mother, father, oldest child, second oldest child, etc]

1	*{record names}*	
8	DK	
9		*{suspend with "resistant" message}*

SECTION 2 - MAIN QUESTIONNAIRE

Q12 Are you one of the people in your household who has been saltwater fishing in the last 2 months?

1	YES	
2	NO	*{Ask to speak with angler}*
8	DK	*{Terminate; code as Resistant}*
9	R	*{Terminate; code as Resistant}*

Q13 *{skip if only 1 2month angler in HH}*

First, did all of the fishermen in your household take all of their fishing trips together over the last 2 months?

1	YES	
2	NO	
8	DK	
9	R	

Q14 During the past twelve months, did you have a FISHING LICENSE for the state of *{restore state of residence}* ?

1	YES	
2	NO	*{Go to Q17}*
8	DK	*{Go to Q17}*
9	R	*{Go to Q17}*

Q15 Was this particular license for Recreational Saltwater Fishing?

1	YES	
2	NO	*{Go to Q17}*
8	DK	*{Go to Q17}*
9	R	*{Go to Q17}*

Q16 LIC_ANG3 *{If LIC_ANG2 = 1 then ask:}*
Was this license valid between *{conditional restore: w1="January", w2="March", w3="May", w4="July", w5="September", w6="November"}* 1st and *{conditional restore: w1="February", w2="April", w3="June", w4="August", w5="October", w6="December" }* 12th?

1	YES	
2	NO	*{Go to Q17}*
8	DK	*{Go to Q17}*
9	R	*{Go to Q17}*

{LABEL **LOOP1_START**} – {ANGLER PROFILING STARTS HERE}

Q17 On how many days in the past two months, between *{restore TODAY-2 Months}* and *{restore TODAY-1}*, did you (s/he) go saltwater fishing in *{restore state}* or in a boat launched from *{restore state}*?

1-62	*{record response}*	
0	NONE	*{Conclude Interview}*
98	DK	
99	R	*{Terminate; code as Resistant}*

Q18 On how many days in the past two months, between *{restore TODAY-2 Months}* and *{restore TODAY-1}*, did you (s/he) go saltwater fishing in any coastal state or territory of the US other than *{restore state}* or from a boat launched from another coastal state or territory of the US?

1-62	*{record response}*	
0	NONE	*{Conclude Interview}*
98	DK	
99	R	{*Terminate; code as Resistant}*

{LABEL **TRIPLOOP START**} – {<u>TRIP PROFILING STARTS HERE</u>}

*Q*19 When did you (s/he) last go saltwater fishing? I have a calendar with me in case we need to look up some of the specific dates.

1	*{record month}*	
99	R	{*Terminate; code as Resistant}*

Can you tell me the date of the **saltwater fishing** trip prior to that one?

1	*{record month}*	
66	NO MORE TRIPS during time period	
99	R	*{*skip to *LABEL TripLoop End}*

Q20 [INTERVIEWER: record day. If respondent can't remember the day, ask if it was a weekday or weekend. You may prompt for answers by using your calendar]

1	*{record day}*	*{range=1 through 31}*
2	If weekday, enter WD	*{record WD}*
3	If weekend, enter WE	*{record WE}*
98	If DK, enter DK	*{record DK}*
99	R	

Q21 On that day, did you (he/she) fish from a boat?

1	YES	
2	NO	*{Go to Q23}*

8	DK	*{Go to Q23}*
9	R	*{Terminate; code as Resistant}*

Q22 *{Ask if fished from a boat}*

Was that from a ...

1	Party or head boat -- CATEGORY B	
2	Charter boat -- CATEGORY B	
3	Private boat -- CATEGORY C	
4	Rental boat -- CATEGORY C	
5	Boat - don't know what type -- CATEGORY C	
8	DK	
9	R	*{Terminate; code as Resistant}*

Q23 On that day, did you (also) fish from the shore?

1	YES	
2	NO	*{Go to Q25}*
8	DK	*{Go to Q25}*
9	R	*{Terminate; code as Resistant}*

Q24 Was that from a …

1	Pier	
2	Dock	
3	Jetty / Breakwater / Breachway	
4	Bridge / Causeway	
5	Other manmade structure	
6	Bank / Beach	
8	DK	
9	R	*{Terminate; code as Resistant}*

Q25 Now I'd like to ask you a series of questions about the {restore mode} trip you (s/he) took on that day.

Q26 Did the boat return to *{restore state}*?

1	YES	
2	NO	*{Go to Q28}*
8	DK	*{Go to Q27}*
9	R	*{Terminate; code as Resistant}*

Q27 To what coastal state or US territory did the boat return?

1	Alabama
2	Alaska
6	California
9	Connecticut
10	Delaware
12	Florida
13	Georgia
15	Hawaii
22	Louisiana
23	Maine
24	Maryland
25	Massachusetts
28	Mississippi
33	New Hampshire
34	New Jersey
36	New York
37	North Carolina
41	Oregon
44	Rhode Island
45	South Carolina
48	Texas
51	Virginia
53	Washington
72	Puerto Rico
55	Other - inland state or non-US territory
98	DK
99	R

Q28 To what **coastal** county did your boat return?

1	*{coastal county list displayed}*
99998	DK
99999	R

Q29 Does the public have access to the place from which the boat left, or is it private access?

1	public has access	
2	private access only	*{Go to Q31}*
3	Military [do not read]	
7	STOP RECORDING TRIP DETAILS	
8	DK	*{Go to Q32}*

9 R

Q30 Was it a launch ramp, boat slip, dock or mooring, private property unlocked marina or something else?

1 launch ramp
2 boat slip
3 dock or mooring
4 private property unlocked marina
5 something else
7 STOP RECORDING TRIP DETAILS
8 DK
9 R

Q31 Was it from a personal residence or dock, a private locked-gate marina, a private property unlocked marina, or something else?

1 personal residence or dock
2 a private locked-gate marina
3 a private property unlocked marina
4 something else
7 STOP RECORDING TRIP DETAILS
8 DK
9 R

Q32 What time did the boat return?

1 1 am
2 2 am
3 3 am
4 4 am
5 5 am
6 6 am
7 7 am
8 8 am
9 9 am
10 10 am
11 11 am
12 12 pm (NOON)
13 1 pm
14 2 pm
15 3 pm
16 4 pm
17 5 pm

18	6 pm	
19	7 pm	
20	8 pm	
21	9 pm	
22	10 pm	
23	11 pm	
24	12 am (MIDNIGHT)	
25	depends on tide	
77	Stop recording trip details	{goto end triploop}
98	DK	
99	R	

Q33 Was most of the boat fishing effort that day in the ocean, sound, river, bay or inlet?

1	ocean/ gulf
2	sound
3	river
4	bay
5	inlet, including inter-coastal waterways and canals
6	other *{specify}*
8	DK
9	R

Q34 {*Ask if [Q60a = 1/Ocean,Gulf]}*
Was most of the fishing less than or greater than THREE miles from shore?

1	*THREE miles or less from shore*
2	*Greater than THREE miles from shore*
8	DK
9	R

{LABEL **TripLoop End**} – {TRIP PROFILING ENDS HERE}

{LABEL **77**} – {IF INTERVIEW IS BROKEN OFF}

Q35 For the remaining {restore number of remaining trips not discussed} days, could you at least please tell me how many times and in what state and county or US territorial island you fished from a party/charter boat, a private/rental boat, and the shore?

1 respondent will continue	
2 need to change number of initial trips	*{set change=1}}*
9 R	*{skip to LABEL LANGUAGE}*

Q36 [Record the TOTAL number of days actually fished from *{restore recall period start date}* through *{restore recall period end date}*.]

1 Record response

Q37 Of the remaining trips, how many were in party or charter boats?

1 record response *{range is 0 to 62}*
98 DK *{skip to Q40}*
99 R *{skip to Q40}*

Q38 In what state or US territory were the majority of your party or charter boat trips?

1 Alabama
2 Alaska
6 California
9 Connecticut
10 Delaware
12 Florida
13 Georgia
15 Hawaii
22 Louisiana
23 Maine
24 Maryland
25 Massachusetts
28 Mississippi
33 New Hampshire
34 New Jersey
36 New York
37 North Carolina
41 Oregon
44 Rhode Island
45 South Carolina
48 Texas
51 Virginia
53 Washington
72 Puerto Rico
55 Other - inland state or non-US territory
98 DK
99 R

Q39 To what county?

99998 DK
99999 R

Q40 Of the remaining trips, how many were in private or rental boats?

1	record response	*{range is 0 to 62}*
98	DK	*{skip to Q43}*
99	R	

Q41 In what state or US territory were the majority of your private or rental boat trips?

1 Alabama
2 Alaska
6 California
9 Connecticut
10 Delaware
12 Florida
13 Georgia
15 Hawaii
22 Louisiana
23 Maine
24 Maryland
25 Massachusetts
28 Mississippi
33 New Hampshire
34 New Jersey
36 New York
37 North Carolina
41 Oregon
44 Rhode Island
45 South Carolina
48 Texas
51 Virginia
53 Washington
72 Puerto Rico
55 Other - inland state or non-US territory
98 DK
99 R

Q42 To what county?

99998 DK

99999 R

Q43 Of the remaining trips, how many were from the shore?

1	record response	*{range is 0 to 62}*
98	DK	*{skip to Q46}*
99	R	

Q44 In what state or US territory did you do the majority of your shore fishing?

1	Alabama
2	Alaska
6	California
9	Connecticut
10	Delaware
12	Florida
13	Georgia
15	Hawaii
22	Louisiana
23	Maine
24	Maryland
25	Massachusetts
28	Mississippi
33	New Hampshire
34	New Jersey
36	New York
37	North Carolina
41	Oregon
44	Rhode Island
45	South Carolina
48	Texas
51	Virginia
53	Washington
72	Puerto Rico
55	Other - inland state or non-US territory
98	DK
99	R

Q45 what county?

99998 DK
99999 R

Q46 [INTERVIEWER: Record language of this survey]
1 English
2 Spanish

{LABEL **Loop1-End**} – {ANGLER PROFILING ENDS HERE}

{LABEL **CLOSING**}

Q47 *{All 2--month angler households get the phone line questions, as well as 10% of households that do not house 2-month anglers.}*
Not including cell phones, how many different telephone numbers are there in your home?

1 TOTAL NUMBER OF LINES {Range 1-97}
98 DK
99 R

Q48 Of these *{Restore Q47}* telephone numbers, how many are never used for talking and instead are always connected to a fax machine or computer modem?

1 TOTAL NUMBER OF LINES
98 DK
99 R

Q49 Of the remaining *{restore (PH_A – PH_B)}* telephone numbers, how many are for business use only?

1 TOTAL NUMBER OF LINES
98 DK
99 R

Q50 I calculate that you have *{restore Q47 – Q48 – Q49}* residential telephone lines. Does this sound right?
[IF NEEDED: Your best guess is fine.]

1 YES
2 NO
98 DK
99 R

Thank you for your assistance. That concludes this survey. Have a good day/night.

Appendix B: Mail Survey Screener Questionnaire

Commonly Asked Questions

How did you get my address?
Your address was randomly selected from all North Carolila's addresses usilg scientific sampling. You and your household represent many other households in your part of the State.

How much time will this survey take?
On average, it should take less than five minutes to complete, including reviewing instructions, and answering the questions.

Nobody in my household participates in outdoor recreational activities. Should I still respond to the survey?
Yes. It is important that everyone who receives this short questionnaire complete it and return it. For this survey to be scientific, we need basic information about all househokls selected for the survey - regardless of whether they participate in outdOOf reaeationaf activities.

Why can't you interview another household instead of mine?
For the results to be scientific, we need all households who receive this short questionnaire to complete Ille questionnaire and send it back.

Who is sponsoring the survey?
This study is being sponsored by the National Oceanic and Almospheric Administration (NOAA). NOAA's missK>n is to understand and predict changes in the Earth's environment and conserve and manage coastal and marine resources to meet our Nation's economic, social and environmental needs

How will the information I provide be used?
Your answers will help identify individuals in your state who participate in outdoor recreational activities. Those individuals who participate in outdoor recreational activities could receive a second survey. The second survey will collect information on how outdoor reaeational resources are actually used. This information will help us manage these resources for the lut\Ne.

Your answers are oompletety confidential and will be used only for this study in accordance with the Privacy Act of 1974. Questions about completing Illis questionnaire can be directed to Howard King tol free at 1 -TT19.

Louisiana
Outdoor Recreation Survey

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the information ,... pro;,ido ,,;n be coni,;ned with information provided by other pomcipa,ls to produce statistical summaries and -

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OMB # 0648-0052
Exp. Date 4/30/2011

Outdoor Recreation Survey

Start Here

The National Oceanic and Atmospheric Administration is studying the recreational activities of Louisiana residents.

- Please return this form even if no one in the household participates in the recreational activities listed.
- This survey should be filled out by an adult household member living at this address.
- Please use blue or black pen if available.

1. How many adults ages 18 and older live in this household?

☐☐

- Please answer questions 2–6 for each adult in the household. For more than 3 adults, please report for the 3 oldest adults.

	Adult 1	Adult 2	Adult 3
2. What is this person's first name or initials?	☐☐☐☐☐☐☐☐☐	☐☐☐☐☐☐☐☐☐	☐☐☐☐☐☐☐☐☐
3. Is this person male or female?	☐ Male ☐ Female	☐ Male ☐ Female	☐ Male ☐ Female
4. What was this person's age on his or her last birthday?	☐☐ Age	☐☐ Age	☐☐ Age
5. In the last 12 months, has this person participated in:			
a. Backpacking, Climbing, or Hiking?	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No
b. Recreational Boating?	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No
c. Recreational Freshwater Fishing?	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No
d. Recreational Saltwater Fishing?	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No
e. Hunting?	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No
6. In the next 3 months, which of these activities do you think this person will participate in? Please check all that apply.	☐ Backpacking, Climbing or Hiking ☐ Recreational Boating ☐ Recreational Freshwater Fishing ☐ Recreational Saltwater Fishing ☐ Hunting ☐ None of these activities	☐ Backpacking, Climbing or Hiking ☐ Recreational Boating ☐ Recreational Freshwater Fishing ☐ Recreational Saltwater Fishing ☐ Hunting ☐ None of these activities	☐ Backpacking, Climbing or Hiking ☐ Recreational Boating ☐ Recreational Freshwater Fishing ☐ Recreational Saltwater Fishing ☐ Hunting ☐ None of these activities

Mail to:

Thank you for completing this survey. Please return your form in the postage paid envelope provided or mail to:

Outdoor Recreation Survey
1009 Slater Rd, Suite 110
Durham, NC 27703

57483

0000000000

Appendix C: Mail Survey Angler Questionnaire

Louisiana
Saltwater Fishing Survey

This study is being conducted with the assistance of the National Oceanic and Atmospheric Administration in accordance with the Magnuson-Stevens Fishery Conservation and Management Act of 2006. Your Participation is voluntary. All responses will be kept confidential under the Privacy Act of 1974.

The information you provide will be combined with information provided by other participants to produce statistical summaries and reports.

COMMONLY ASKED QUESTIONS

Why did I get this survey?

You were selected from a list of anglers who are licensed to fish in saltwater in Louisiana. Surveys from license lists are an important way of understanding uses of marine resources in Louisiana.

Who is sponsoring the survey?

This study is sponsored by the National Oceanic and Atmospheric Administration (NOAA). NOAA's mission is to understand and predict changes in the Earth's environment and conserve and manage coastal and marine resources to meet our Nation's economic, social and environmental needs.

I have not fished in the last few months. Should I still respond to the survey?

Yes. We understand that all licensed anglers may not have fished during the recent months, but it is important that all licensed anglers who receive this questionnaire complete it and return it. For this survey to be scientific, we need basic information about all licensed anglers who are selected for the survey whether they fished recently or not.

How will the information I provide be used?

Your answers will help the National Oceanic and Atmospheric Administration improve its stewardship of Louisiana's marine resources. Your answers are completely confidential and will be used for this study in accordance with the Privacy Act of 1974.

How much time will this survey take?

On average, it should take less than 10 minutes to complete, including reviewing instructions and answering the questions.

Can't you ask another person instead of me?

For the results to be scientific, we need all licensed anglers who receive this questionnaire to complete the questionnaire and send it back. We greatly appreciate your efforts to help by providing important information for improving the stewardship of marine resources.

I have other questions. Who can I talk to?

Questions about completing this questionnaire can be directed to Howard King toll-free at 1-888-640-7719.

(4)

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Please use a blue or black pen.

1. During the **past 12 months,** did you do any recreational **salt water fishing?**

   ☐ Yes

   ☐ No → GO TO question 8

Saltwater fishing: fishing in oceans, sounds, or bays, or in brackish portions of rivers. This does not include fishing in freshwater, or for shellfish, such as crabbing.

2. Between **November 1 and December 31, 2010,** did you go recreational saltwater fishing in Louisiana from the **shore?**

   ☐ Yes

   ☐ No → GO TO question 5

Shore includes docks, bridges, causeways, beaches, banks or any other shore-based structure or area.

3. Between **November 1 and December 31, 2010,** on how many days did you go recreational saltwater fishing in Louisiana from the shore?

   ☐☐

   Total days fished from shore in Louisiana

November 2010

S	M	T	W	T	F	S
	1	2	3	4	5	6
7	8	9	10	11	12	13
14	15	16	17	18	19	20
21	22	23	24	25	26	27
28	29	30				

December 2010

S	M	T	W	T	F	S
			1	2	3	4
5	6	7	8	9	10	11
12	13	14	15	16	17	18
19	20	21	22	23	24	25
26	27	28	29	30	31	

4. We are interested in some details about the Louisiana shore-based fishing you did between November 1 and December 31, 2010.

   For the next few questions, please answer these questions about the **most recent shore-based** fishing you did in Louisiana between November 1 and December 31, 2010.

   a. Does the public have access to the place where you fished?

      ☐ Yes

      ☐ No

   b. At approximately what time did you **stop** fishing?

      ☐ Between 2 a.m. and 8 a.m.

      ☐ Between 8 a.m. and 2 p.m.

      ☐ Between 2 p.m. and 8 p.m.

      ☐ Between 8 p.m. and 2 a.m.

   c. Was there a child under 18 with you who also fished?

      ☐ Yes

      ☐ No

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5 Between **November 1 and December 31, 2010,** did you go recreational saltwater fishing **from a private or rental boat** that returned to shore in Louisiana? Do not include charter boat trips – charter trips have a captain or crew.

☐ Yes

☐ No → GO TO question 8

6 Between **November 1 and December 31, 2010,** on how many days did you go recreational saltwater fishing from a private or rental boat that returned to shore in Louisiana?

☐☐

Total days fished from a private boat that returned to Louisiana

November 2010

S	M	T	W	T	F	S
	1	2	3	4	5	6
7	8	9	10	11	12	13
14	15	16	17	18	19	20
21	22	23	24	25	26	27
28	29	30				

December 2010

S	M	T	W	T	F	S
			1	2	3	4
5	6	7	8	9	10	11
12	13	14	15	16	17	18
19	20	21	22	23	24	25
26	27	28	29	30	31	

7 We are interested in some details about the fishing trips you took by boat. For the next few questions, please answer about the **most recent** fishing trip you took by a boat between November 1 and December 31, 2010. We are only interested in boat trips which returned to shore in Louisiana.

a. Does the public have access to the place where the boat landed?

☐ Yes

☐ No

b. At approximately what time did you return to shore?

☐ Between 2 a.m. and 8 a.m.

☐ Between 8 a.m. and 2 p.m.

☐ Between 2 p.m. and 8 p.m.

☐ Between 8 p.m. and 2 a.m.

c. Did the trip include one or more children under the age of 18 who fished?

☐ Yes

☐ No

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8 At any time during the **past 12 months,** did you have a recreational saltwater fishing license for the state of Louisiana, including a multipurpose license or a lifetime license that includes recreational saltwater fishing?

- ☐ Yes
- ☐ No ➡ GO TO question 11

9 During the past 12 months, which of the following types of Louisiana fishing licenses did you have? *Please check all that apply.*

- ☐ Annual Basic Fishing License (including LA disabled basic fishing license, non-resident basic fishing license, and military basic fishing license)
- ☐ Annual Saltwater License (including LA disabled saltwater fishing license, non-resident saltwater fishing license, and military saltwater fishing license)
- ☐ Annual Senior Fish/Hunt
- ☐ Annual LA Sportsman's Paradise License
- ☐ Lifetime Fishing
- ☐ Lifetime Hunt/Fish
- ☐ Lifetime Senior Hunt/Fish
- ☐ Charter Passenger 3-day License
- ☐ Charter Skiff 3-day License
- ☐ Non-resident Basic Fish Trip (1 day)
- ☐ Non-resident Saltwater Trip (1 day)
- ☐ Other type of license, including all fishing gear licenses

10 If you had a 1-day or 3-day license, for what dates was it valid?

Start Date: ☐☐ / ☐☐ / ☐☐
month day year

End Date: ☐☐ / ☐☐ / ☐☐
month day year

48931

11 Finally, a few questions about yourself and your household.
Are you:

- ☐ Male
- ☐ Female

12 How old are you? Are you:

- ☐ 18 - 24
- ☐ 25 - 34
- ☐ 35 - 44
- ☐ 45 - 54
- ☐ 55 - 64
- ☐ 65 or older

13 For statistical purposes, we are interested in learning about your telephone service. Which category best describes the telephone service for your household:

- ☐ Regular or landline phone only
- ☐ Cellular phone only
- ☐ Both landline and cellular phone
- ☐ No working phone service

Thank you for completing this survey. The information you provide will help NOAA meet its mission of stewardship of the nation's offshore living marine resources and their habitat.

Please return your completed survey in the enclosed self-addressed envelope or mail to:

Louisiana Saltwater Fishing Survey
1009 Slater Rd, Suite 110
Durham, NC 27703

Thank You!

Continued Development and Testing of Dual-Frame Surveys of Fishing Effort

Testing a Dual-Frame, Mixed-Mode Survey Design

Final Report

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1. Executive Summary

The data collection design tested in this study was developed as a potential alternative to the Coastal Household Telephone Survey (CHTS), the current methodology used by NOAA Fisheries to estimate marine recreational fishing effort. The design is based upon the results of previous MRIP pilot studies, which demonstrated that mail surveys that sample from residential address frames and state angler license databases provide greater coverage and result in higher response rates than the CHTS. The objectives of the study were to; 1) evaluate the feasibility of the data collection design as a potential alternative to the CHTS, 2) characterize the effects of data collection mode, including telephone and mail, on response rates, timeliness and survey measures, and 3) assess the survey coverage of state angler license databases.

Results from the study continue to demonstrate that mail survey designs are feasible for collecting recreational fishing data and estimating fishing effort. Final response rates for the mail survey component of the study were higher than the telephone component and eclipse telephone survey response rates after about three weeks of data collection. In addition, preliminary estimates derived from early mail survey returns were not significantly different from final estimates, demonstrating that a mail survey can generate valid preliminary estimates within the current estimation schedule for the CHTS.

The impact of data collection mode on survey measures requires further investigation. We hypothesize that differences between telephone and mail estimates are the result of differential recall and coverage errors, and suggest that telephone samples are more susceptible to bias resulting from these errors. This hypothesis is speculative and was not tested in the present study.

As in previous studies, total fishing effort estimates - number of angler trips - generated solely from license samples were considerably lower than estimates generated from samples of residential addresses (ABS or address-based samples). We explored differences between license and ABS effort estimates in terms of survey error and suggest that under-coverage of license frames resulting from license exemptions and unlicensed fishing is the most likely source of the differences. Subsequently, we conclude that within the South Atlantic region, sampling exclusively from state license databases is likely to result in an underestimate of total fishing effort.

Finally, matching errors, resulting in misclassification of sampling domains (e.g., license only; ABS only, both license and ABS), continue to be a challenge and are likely to result in biased estimates. These errors must be addressed, either through improved matching procedures or development of alternative estimators that reduce the impact of misclassification errors, before dual-frame designs can be considered as an alternative to the CHTS.

The following are specific recommendations and conclusions:

1. Mail surveys are a feasible alternative to telephone surveys for collecting recreational fishing effort data. Mail surveys result in higher response rates than telephone surveys, and preliminary mail survey estimates can be generated in a timeframe consistent with the current CHTS estimation schedule.
2. Incorporating angler license databases into data collection designs increases the efficiency of recreational fishing surveys. While sample frames derived from license databases may be incomplete due to unlicensed fishing activity, samples of licensed individuals and households with licensed anglers are much more likely to report fishing than general population samples. Supplementing household samples with information from license databases should increase the efficiency of data collection while maintaining coverage of the entire population.
3. Frame matching errors are a recurring problem and potential source of bias in dual-frame sampling designs. Frame standards, which were implemented during the study, will help minimize matching errors.
4. Further study is needed to better understand the impact of data collection mode on survey measures. However, we hypothesize that differences between telephone and mail estimates are the result of recall error and coverage error, and that telephone samples are more susceptible to biases resulting from these errors. These hypotheses were not tested in the present study.
5. In the South Atlantic region, it is currently not feasible to sample exclusively from state license databases. In the present study, total effort estimates derived from license samples were considerably lower than ABS estimates. We attribute these differences to coverage error resulting from license exemptions and unlicensed fishing activity.
6. Cash incentives provide a substantial boost in response rates for mail surveys and should be considered in any mail survey design.
7. The single-phase ABS design with screening prior to data collection proposed by Brick et al. (2012) should be tested as an alternative to dual-frame designs. Such a design is not susceptible to bias resulting from matching error and is likely to result in higher response rates than the two-phase design.

2. Introduction

In a dual-frame survey, independent samples are selected from two sample frames, and the resultant data are combined to estimate population totals or means. Often, the goal of a dual-frame design is to maximize both efficiency and coverage, particularly when sampling a rare population (Lohr 2009). Previous MRIP pilot studies (Andrews et al. 2010; Brick et al. 2012a, Brick et al. 2012b) have demonstrated the benefits of dual-frame, mail survey designs for sampling recreational anglers and collecting recreational fishing effort data. The dual-frame sampling design, which samples from comprehensive lists of residential addresses and state databases of licensed anglers, provides greater coverage and efficiency than the random-digit-dial (RDD) frame used for the Coastal Household Telephone Survey (CHTS), and mail surveys result in considerably higher response rates and may be less susceptible to measurement errors than telephone interviews (Brick et al. 2012b).

Despite these benefits, concerns persist that a mail survey cannot satisfy customer needs for timely estimates. The dual-frame, mixed-mode survey was designed to address these concerns by measuring the impact of data collection mode on response rates, survey measures, and the timeliness of data collection, in a controlled, experimental setting. In many mixed-mode designs, sample units are offered a choice of reporting mode, either concurrently or sequentially, with a goal of reducing coverage bias, nonresponse and/or cost (de Leeuw 2005). In the current study, sample units were not offered a choice of reporting mode, but were allocated into exclusive telephone or mail treatments, which allowed direct comparisons between modes on measures of survey quality. The goal of this design was to assess differences between telephone and mail modes in terms of response rates, timeliness and survey measures.

The objectives of the dual-frame mixed-mode pilot study were to, 1) continue to test and document the general feasibility, including both benefits and limitations, of dual-frame, mail survey designs for collecting recreational fishing effort data, 2) examine the impact of data collection mode (mail and telephone) on survey response and measurement, 3) determine the timeframe in which a mail survey can deliver reliable estimates, 4) evaluate the completeness and quality of state angler license databases in the study states, and 5) test for nonresponse bias in mail survey designs.

3. Methods

3.1. Sampling Design

The dual-frame mixed-mode survey (henceforth referred to as the “fishing effort survey”) was conducted in four states (NC, SC, GA and FL) in the South Atlantic Region. In each state, fishing effort data, including the number of trips by fishing mode, were collected for six independent two-month reference waves, beginning with wave 1 (Jan/Feb), 2012, and continuing through wave 6 (Nov/Dec), 2012. The survey utilized a dual-frame design that sampled from state databases of licensed saltwater anglers (license frame) and residential address frames (address-based samples or ABS). The union of the license and ABS frames consists of three domains; households in the address frame but not in the license frame, households in the license frame but not the address frame, and households in both frames (overlap domain).

The ABS frame is derived from the United States Postal Service, Delivery Sequence File (DSF), and includes all residential addresses within the study area[1]. For each state and wave, sampling was stratified at the county level into coastal and non-coastal strata[2]. Geographic stratification within states provides an opportunity to sample strata at different rates and subsequently increase the efficiency of data collection. For example, historical estimates from the Marine Recreational Fisheries Statistics Survey (MRFSS) demonstrate that 65-90% of recreational saltwater fishing trips in the study states are taken by residents of coastal counties within those states.

Sampling from the ABS frame was conducted through a two-phase data collection model. In the first phase, a household screener questionnaire is mailed to a sample of residential addresses. The questionnaire (Appendix A) identifies eligible anglers – adult residents who fished during the previous year or are likely to fish during the next three months. The screener questionnaire collects information for up three adults per sampled address. For each wave and state, a random sample of 6,000 addresses is selected from the ABS frame and matched, by address and telephone number, to the state's directory of licensed anglers. This matching identifies the domain for each sampled address – addresses on both frames (matched sample) and addresses on only the address frame (unmatched sample). Table 1 provides the first-phase ABS sample sizes by state and sub-state stratum for each reference wave.

[1] ABS samples were purchased from a commercial vendor licensed by the USPS to distribute the computerized delivery sequence file. The sample included "residential only" and "primary residential with some business" addresses.

[2] Counties included in the coastal and noncoastal strata varied by wave. During waves 1, 2 and 6, all counties within 25 miles of the coast are included in the coastal stratum. In waves 3-5, counties within 50 miles of the coast are included in the coastal stratum.

Table 1. Number of sampled addresses by stratum and survey wave for the first-phase ABS sampling.

Stratum	Wave 1	Wave 2	Wave 3	Wave 4	Wave 5	Wave 6
North Carolina	6,000	6,000	6,000	6,000	6,000	6,000
Coastal	1,199	1,873	2,634	2,619	2,618	1,873
Non-Coastal	4,801	4,127	3,366	3,381	3,382	4,127
South Carolina	6,000	6,000	6,000	6,000	6,000	6,000
Coastal	1,963	2,856	2,856	2,856	2,856	2,856
Non-Coastal	4,037	3,144	3,144	3,144	3,144	3,144
Georgia	6,000	6,000	6,000	6,000	6,000	6,000
Coastal	388	791	791	791	791	791
Non-Coastal	5,612	5,209	5,209	5,209	5,209	5,209
Florida	6,000	6,000	6,000	6,000	6,000	6,000
Total	24,000	24,000	24,000	24,000	24,000	24,000

All eligible adults identified in the screener phase were sampled for the second-phase or topical survey, which collects details about recreational saltwater fishing activity that occurred during two-month reference waves. To permit comparisons between telephone and mail data collection modes, the topical ABS sample, which consists of individual anglers rather than households, was randomly distributed between telephone and mail treatments after allocating sample with no known telephone number to the mail treatment[3]. Table 2 provides the topical ABS sample sizes by state and stratum for each reference wave.

Table 2. Second-phase ABS sample sizes by stratum and survey wave[4].

Stratum	Wave 1	Wave 2	Wave 3	Wave 4	Wave 5	Wave 6
North Carolina	416	465	441	585	600	619
Coastal	147	221	249	350	331	266
Non-Coastal	269	244	192	235	269	353
South Carolina	414	508	456	614	546	599
Coastal	221	340	299	396	345	409
Non-Coastal	193	168	157	218	201	190
Georgia	242	282	298	454	348	415
Coastal	34	77	76	113	91	105
Non-Coastal	208	205	222	341	257	310
Florida	549	533	560	743	884	920
Total	1,621	1,788	1,755	2,396	2,378	2,553

[3] Allocation to telephone and mail treatments is done at the household level so that multiple individuals at the same address do not receive survey requests from different modes.
[4] Sample sizes reflect 1st phase ABS sample sizes, 1st phase ABS household response rates, and the eligibility rates of individuals within responding households.

The license frames are derived from state databases of adults who were licensed to participate in saltwater fishing in the study states between the beginning of each reference wave and the time the sample frame is created, approximately one month prior to the end of each wave. Sampling, which is conducted in a single phase, is stratified by state (state of licensure) and sub-state region of residence. License frame sampling also includes a stratum for licensed anglers who reside outside the state of licensure (nonresident anglers). As with the topical ABS sample, the license sample is randomly distributed between telephone and mail treatments, and sampled individuals are asked to describe saltwater fishing activity that occurred during the reference wave. Table 3 provides license frame sample sizes by state and stratum for each reference wave.

Table 3. License frame sample sizes by stratum and survey wave.

Stratum	Wave 1	Wave 2	Wave 3	Wave 4	Wave 5	Wave 6
North Carolina	1,072	1,072	1,072	1,072	1,070	1,072
Coastal	448	446	618	611	687	478
Non-Coastal	565	577	405	395	349	531
Nonresident	59	49	49	66	34	63
South Carolina	1,072	1,071	1,072	1,072	1,072	1,072
Coastal	458	450	540	440	440	435
Non-Coastal	581	567	491	613	631	634
Nonresident	33	54	41	19	1	3
Georgia	1,072	1,072	1,072	1,072	1,072	1,072
Coastal	162	171	223	268	146	138
Non-Coastal	890	882	824	780	920	915
Nonresident	20	19	25	24	6	19
Florida	1,071	1,072	1,072	1,072	1,065	1,072
Coastal	931	927	937	1008	940	976
Nonresident	140	145	135	64	125	96
Total	4,287	4,287	4,288	4,288	4,279	4,288

3.2. Data Collection Procedures

The purpose of the ABS screener survey was to identify likely saltwater anglers from among the general household population. To accomplish this, the screener instrument asked about past and likely future participation in a variety of outdoor recreation activities, including saltwater fishing. All residents who reported saltwater fishing during the previous 12 months or likely participation in saltwater fishing during the next three months were eligible for the topical sample. Initially, the screener instrument also requested a telephone number that could be used for the follow-up, topical survey. We suspected that asking for a telephone number had a negative impact on screener response rates, so we eliminated this question following wave 4[5].

Screening of the ABS sample begins at the start of each reference wave to allow for adequate time to compile the topical sample (i.e., identify eligible anglers). Sampled addresses receive multiple mailings, including an initial mailing of the screener questionnaire, a reminder postcard one week after the initial mailing, and a second mailing of the screener questionnaire to nonrespondents two weeks after the mailing of the postcard. Screener questionnaire mailings, which are administered via regular, first class mail, include a cover letter stating the purpose and importance of the survey, the survey instrument and a post-paid return envelope. A $1.00 cash incentive was included in the initial survey mailing beginning with wave 4 and continuing through wave 6. When available, sampled addresses were augmented with the name of a household resident, and survey packets were mailed to the named individual[6]. This was a substantive design change from previous MRIP effort survey pilot studies, in which first phase ABS mailings were addressed to “state resident”, for example, NC resident (Andrews et al. 2010; Brick et al. 2012b).

The topical survey is administered through either Computer Assisted Telephone Interviewing (CATI) or completed and returned mail questionnaires. Telephone numbers were obtained either from the screener survey, as described above, or through commercial directory matching[7]. Regardless of data collection mode, data collection is retrospective for the most recent two-month reference wave, and survey questions for each mode were developed to be as similar as possible to minimize the potential for differential interpretation of the meaning of questions.

The first mailing for the mail treatment begins one week prior to the end of each reference wave. This ensures that respondents receive survey materials by the end of the wave. The sequence of mailings includes an initial mailing of the topical questionnaire, a reminder postcard one week after the initial mailing, and a second mailing of the topical questionnaire to nonrespondents two weeks after the mailing of the postcard. The initial mailing is administered via USPS Priority Mail. All subsequent mailings are administered via regular, first class mail. In previous studies (Andrews et al. 2010; Brick et al. 2012), a $1.00 cash incentive was included in the initial mailing for both the license samples and the second phase ABS samples. However, we chose not to include incentives for these samples in the present study.

All telephone interviewing for the topical survey begins on the day following the end of each wave and continues for 10-11 days. Calls are scheduled among days and times such that the

[5] Dropping the telephone question had a very modest impact on screener response weights that was far outweighed by the negative impact on the quality of telephone numbers in the topical phase.
[6] The ABS vendor attempted to identify a name for each sampled address.
[7] For waves 5 and 6, telephone number was obtained only through commercial directory matching.

likelihood of completing an interview is maximized. The telephone instrument is designed to collect information about saltwater fishing activity that occurred during the wave.

3.3. Data Delivery

To determine if a mail survey design can produce unbiased and reasonably precise estimates within the current timeframe of the Coastal Household Telephone (CHTS), we produced estimates generated from both partial and complete survey data. Partial data included complete data for the CATI treatment and mail surveys that were returned within 15-21 days[8] from the initial survey mailing.

4. Key Findings

4.1. Survey Eligibility and Efficiency

Over the past three decades, the CHTS has demonstrated that during a given two-month period, residents in fewer than 10% of households participate in recreational saltwater fishing. Subsequently, recreational saltwater anglers are generally considered a rare population and are inefficient to sample using traditional household survey designs (Lohr 2009). Multiple-frame designs that sample from both partial lists of likely participants and general population frames are often used to increase the efficiency of data collection while maintaining coverage of the entire population (Kalton and Anderson 1986; Lohr 2009).

In the present study, we attempted to increase efficiency by sampling from lists of licensed saltwater anglers. We also identified households from within the general population that included at least one licensed saltwater angler by matching ABS samples to saltwater license directories. While this matching was intended to support estimation – dual-frame estimation requires that the domain of sample units be known – it also provided an opportunity to compare fishing activity among domains. Table 4 compares overall fishing prevalence (percentage of respondents over all phases of sampling that reported fishing during the reference wave) overall, by state and by wave, for license samples and the matched and unmatched domains of the ABS samples. Overall, fishing prevalence was considerably higher in the license sample (37.2%) than in either the matched (21.9%) or unmatched (6.6%) ABS samples. The relatively high rate of reported fishing activity in the license sample demonstrates that sampling directly from license databases can be much more efficient than screening general household samples for anglers. Similarly, sampling from the matched domain is more efficient than sampling from the unmatched domain. We expected fishing activity in the matched ABS and license samples to be the same – theoretically, the two samples are from the same population. We attribute differences

[8] For wave 1, preliminary data included mail surveys that were returned within 15 days of the initial survey mailing. For subsequent waves, preliminary data included mail survey returned within 21 days of the initial mailing.

in estimates between the samples to matching errors, which are described more fully below. Matching errors, resulting in misclassification of sample into domains, can result in biased estimates and are a potential limitation of the dual-frame design (Lohr 2009). However, the results presented here demonstrate the potential of both license frames and frame matching for increasing the efficiency of angler surveys.

4.2. Frame Matching and Domain Identification

Previous MRIP pilot studies identified frame-matching errors as a potential source of bias in dual-frame sampling designs (Andrews et al. 2010; Brick et al. 2012b). Matching errors occur because the fields used to match ABS samples to license frames, including address and telephone number, may be missing, incomplete or inaccurate on one or more of the frames. The result of matching errors is that domains for some sample units are misclassified, and sample weights are not adjusted to reflect selection probabilities; some units are inappropriately excluded from the overlap domain and are not down-weighted (we refer to this as under-matching), and some units are inappropriately included in the overlap and are down-weighted when they should not be (over-matching).

Errors resulting from both under-matching and over-matching have been reported for dual-frame fishing surveys (Brick et al., 2012b; Andrews et al., 2010). It's interesting to note that both types of errors are likely to have the same net effect; overestimation of fishing effort. In the case of under-matching, sample units with licensed anglers, which are more likely to report fishing, are excluded from the overlap domain and are not appropriately down-weighted[9]. This effectively results in over-representation of fishing households in the sample. In contrast, over-matching results in under-representation of non-fishing households; sample units without licensed anglers, which are less likely to report fishing, are included in the overlap domain and are inappropriately down-weighted.

In the present study, 7.7% of sampled addresses matched to the state license databases. In other words, an estimated 7.7% of households included at least one resident who was licensed to fish within his or her state of residence during the reference waves. Matching rates were highest in South Carolina (9.9%), followed by Florida (8.6%), North Carolina (7.9%) and Georgia (4.5%).

We identified matching errors by comparing the estimated number of households with licensed anglers, derived from ABS samples, to actual counts of unique addresses on the license frames (Table 5). The accuracy of matching varied by state, but in nearly every comparison, estimated values were significantly higher than actual counts, suggesting that over-matching was the dominant error. The mechanism for over-matching appears to be related to the criteria used to identify matching cases. Specifically, ABS sample cases were identified as matches if at least one of the following conditions was satisfied: 1) the ABS record contained the same primary

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[9] Households in the overlap domain are down-weighted because they can be selected from each of the two sample frames; i.e. they have higher selection probabilities.

street address, zip code, and state of residence as a record in the corresponding license frame; or 2) the ABS record contained the same telephone number (obtained through commercial directory matching), zip code, and state of residence as a record in the corresponding license frame. Secondary street address, which is more specific than primary street address and includes apartment numbers, was excluded from the matching criteria. Subsequently, any ABS sample unit that was part of a multi-unit dwelling, such as an apartment building or condominium, was identified as a match if the license frame included anyone who resided at the multi-unit dwelling. A cursory review of records on the license frames that matched to an ABS sample unit revealed an abundance of apartments, suggesting that the exclusion of secondary address from the matching criteria was a significant source of over-matching. We excluded secondary address from the matching criteria because information within the field was inconsistently reported and formatted, and including the field would likely have resulted in significant under-matching. Variation in the extent of matching errors among states may be due to the relative occurrence of multi-unit dwellings within the individual states. According to numbers reported by the Census Bureau (http://quickfacts.census.gov/qfd/index.html), the percentage of total housing units that are within multi-dwelling structures is higher in FL (29.9%) and GA (20.5%) than in NC (17.0%) and SC (17.6%). Coincidentally, the extent of over-matching errors is greater in FL and GA than in NC and SC.

Because telephone number is included in the matching criteria, exclusions or inaccuracies in this field can also contribute to matching errors. We observed that 56.7% of records on the GA license frame were missing a telephone number, followed by SC (27.2%), NC (15.3%) and FL (8%). We assessed the accuracy of telephone numbers by examining final dispositions of sample units within the CATI treatment and observed less variation among states; in FL, 18.7% of sample cases were classified as "bad number"[10], followed by NC (16.8%), SC (16.5%), and GA (14.0%). While these results suggest that missing and incorrect telephone numbers are a likely source of error in the matching process, it's unlikely that these errors contributed to the observed over-matching. Rather, we would expect errors in telephone number, which is the most specific level of resolution within the telephone matching condition, to result in under-matching. It's difficult to quantify the impact of telephone number errors on match rates in the current study due to the extent over-matching resulting from the address match.

It's noteworthy that the magnitude of over-matching increased between waves 3 and 4. While the matching protocols did not change during this period, procedures used to validate and standardize address records on the license sample frames were implemented between waves 3 and 4. Further examination is needed to understand how these procedures impacted the matching process.

[10] Bad numbers included dedicated fax lines, non-working numbers, non-working/disconnected numbers, temporarily out-of-service, and business numbers.

The results from this study identify several challenges that should be addressed if dual-frame sampling designs are to be considered as a possible alternative to the CHTS. First, estimators for dual-frame designs assume that domain membership of sample units is known and accurate. If this assumption is false, then population estimates may be biased (Lohr 2009). The results of this and prior studies (Andrews et al. 2010; Brick et al. 2012b) demonstrate that defining domain membership, whether through a priori matching of sample frames or responses to survey questions, is complicated and subject to error. These errors must be addressed, either through improved matching procedures or development of alternative estimators that reduce the impact of misclassification errors. Next, the decision to exclude secondary addresses from the matching criteria highlights the need to improve the quality of address information on the license frames. Implementing USPS postal addressing standards (USPS 2010) will increase matching accuracy and provide maximum flexibility for selecting matching criteria. Finally, the quality of telephone numbers must improve if license databases are to be used for sampling purposes. Missing and non-working telephone numbers result in matching errors in dual-frame designs, whether data are collected through mail surveys or telephone interviews. In addition, nonresponse and/or non-coverage resulting from missing or inaccurate telephone numbers introduce the potential for bias in telephone surveys, regardless of the sample design.

4.3. Response Rates and Timeliness

An objective of this study was to assess the feasibility of the mail survey design in terms of response rates and the timeliness of generating estimates. The current pilot study achieved screener response rates of 37.3% and topical survey response rates of 46.4% for an overall response rate of 17.3% for the ABS sample (Table 6). The overall response rate for the license sample was 33.1%. While lower than anticipated, these rates exceed the CHTS response rate of 13.3% for the same geographic region and time period. These response rates are considerably lower than the rates achieved in previous studies that tested similar mail survey designs. Andrews et al. (2010) reported response rates of 58.2% for license samples and overall response rates of 33.1% for ABS samples, including screener response rates of 45.6% and topical survey response rates of 72.5%. Brick et al. (2012b) reported similar rates; 49.3% for license samples and 30.5% for ABS samples, including screener response rates of 46.7% and topical survey response rates of 65.4%.

We suggest that notable design features and design modifications contributed to the lower-than-expected response rates. First, both Andrews et al. (2010) and Brick et al. (2012a) included a third mailing of the topical instrument, which increased topical survey response rates by as much as 10 percentage points[11]. Future administration of mail survey designs should consider the tradeoff between the costs of additional mailings, which can be considerable, and the benefits of higher response rates. Second, previous tests of the dual-frame mail survey design addressed all

[11] Brick et al. (2012b) included an experimental treatment to quantify the impact of a third mailing on response rates.

ABS screener mailings to "State Resident". In the present study, we attempted to augment ABS samples, through commercial directory matching, with the name of a household resident and addressed survey materials to that individual when such information was available. This design feature was intended to increase response rates. However, Link et al. (2008) demonstrated that including a surname on survey materials resulted in lower response rates and suggested that household members may be more likely to discard survey materials if name matching is inaccurate. A third design feature that likely had a considerable impact on response rates was the use of incentives. Both Andrews et al. (2010) and Brick et al. (2012b) included a $1.00 cash incentives in all initial survey mailings – ABS screener survey, ABS and license topical surveys. The present study did not include incentives for any survey mailing until wave 4, when a $1.00 cash incentive was added to the initial mailing of the ABS screener survey. Subsequently, response rates for the screener survey increased from an average of 31.3% for waves 1-3 to an average of 43.7% for waves 4-6, rates very similar to those reported previously (Figure 1). The incentive also decreased the response time for the screener survey (Figure 2). The median response times with and without the incentive were 14 days and 20 days, respectively. We suspect that differences in overall response rates between the current and previous studies would have been further minimized had we included an incentive in the topical survey mailings. Finally, the use of CATI interviewing in the present study may have impacted overall response rates in two ways. First, the response rates reported for the present study include both mail and CATI data collection for the topical ABS and license surveys. Excluding the CATI treatment from the response rate increases the overall rates to 22.0% and 38.1% for the ABS and license samples, respectively. Second, including a CATI treatment likely impacted the characteristics of the mail sample, which may have resulted in lower response rates. As mentioned, topical ABS sample was randomly assigned to mail or CATI treatments after sample with no known telephone number was allocated to the mail treatment. As a result, the mail sample included a relatively lower proportion of sample units with a known telephone number than would be expected in a randomly selected mail sample. Previous studies (Hagedorn et al. 2009; Brick et al. 2011) have demonstrated that response rates are higher for samples that can be matched to a telephone number. We suggest that sampling constraints imposed by the dual-mode design had a negative impact on response rates. We explored this hypothesis by comparing response rates for the full topical ABS mail sample to rates for the portion of sample that include a matched telephone number and found that overall response rates for the telephone matched sample were 2.8 percentage points higher than rates for the full sample (22% vs. 19.2%). We further suggest that the decision to remove the telephone question from the screener instrument following wave 4 exacerbated this impact. The intent of this design modification was to increase response rates at the screener phase of data collection, and to a modest extent, this appears to have been successful as screener response rates increased slightly between wave 4 (43.2%) and waves 5 (45.1%) and 6 (44.1%). However, a more pronounced effect was observed at the topical phase, where response rates, including both modes of data collection, decreased from an average of 49% for waves 1-4 to 41% for waves 5-6. An obvious explanation for this decrease is that self-reported telephone numbers provided by screener respondents are more accurate than numbers obtained through directory matching. This explanation is supported by the response rates achieved in the topical ABS CATI treatment, which decreased from an average of 41% during waves 1-4 to 33% for waves 5-6. A review of final CATI dispositions reveals that telephone numbers obtained through directory matching are more likely than self-reported numbers to be classified as "bad numbers" (18.1% vs. 9.7%). Response rates in the topical ABS mail treatment also decreased after the telephone question was eliminated from the screener instrument. Because the telephone question was eliminated, a larger percentage of the total topical ABS sample, sample units with no known telephone number, was automatically assigned to the mail treatment. Consequently, the wave 5 and wave 6 mail samples included relatively more units without known telephone numbers than the prior waves, which resulted in even lower response rates, as described above. Several of these explanations are anecdotal and would require further investigation to quantify the impact of specific design features on response rates. However, it seems likely that modifications to the data collection design contributed to the differences in response rates between the current and previous studies.

The present study included comparisons between mail and CATI to quantify differences in response rates between modes and assess the timeliness of a mail survey design. Consistent with observations from recent studies (Link et al. 2008) final response rates for the mail treatment were consistently higher than the CATI treatment in both the ABS and license samples (Table 7). Final mail response rates eclipsed CATI response rates by 4.8% and 10.7% for the ABS and license samples, respectively[12]. Differences in response rates between CATI and mail were even larger when the comparison is limited to ABS sample cases for which a telephone number could be located through directory matching. CATI and mail response rates for these cases, which represent the population that can be covered by a telephone survey, were 13.4% and 22.0%, respectively. While response rates alone cannot predict or measure nonresponse bias, a higher response rate decreases the risk of nonresponse bias (Groves 2006). Furthermore, mail survey response rates for both the topical ABS and license samples eclipsed CATI response rates after about three weeks of data collection (Figure 3). This demonstrates that a mail survey can collect a similar amount of data as a telephone survey in a relatively short timeframe, and suggests that preliminary estimates, generated from partial mail survey data, can be produced in a timeframe consistent with the current CHTS data collection and estimation schedule[13].

A concern about using partial data to generate estimates is that mail survey respondents who complete the survey within a few weeks of receiving the questionnaire may be different from those who wait longer to return the survey. This type of nonresponse bias has been documented previously for populations of hunters (Filion 1976) and anglers (Fisher 1996). In both cases,

[12] For the ABS samples, we compared overall CATI and mail response rates, which is the product of screener response rates and topical survey response rates. The screener survey was administered via mail for both CATI and mail topical treatments.

[13] CHTS effort estimates are generally available 45 days following the completion of the reference wave.

those who responded later, after additional attempts to complete surveys, were less likely to have participated in the measured behavior.

To assess differences in fishing activity between early and late responders, we compared estimates of survey measures, including fishing prevalence, and mean private boat and shore trips per angler, between early responders (those who responded within 3 weeks of the first survey mailing) and all responders (Table 8). There were no significant differences between preliminary and final estimates overall or within states for any of the survey measures, demonstrating that "early" and "late" responders are not substantially different in terms of reported fishing activity. While this result does not suggest that the overall mail survey design is immune to nonresponse bias, it does demonstrate that point estimates derived from preliminary mail survey data are not likely to be substantially different from final estimates, produced after data collection has been completed. This result is consistent with the observation that mail surveys are feasible if "data are needed in a couple of weeks" (de Leeuw 2008), and provides further evidence that a mail survey design is a feasible alternative to telephone surveys for producing recreational fishing statistics in a timely manner. A caveat to this conclusion is that final mail survey estimates are likely to be more precise than preliminary estimates as the number of completed surveys (i.e., sample size) increases.

4.4. Mode Effects on Survey Measures

When considering multiple data collection modes, or switching modes in an ongoing survey, care must be taken to ensure that survey measures are not impacted by the reporting mechanism. Dillman et al. (2009) and de Leeuw (2005) suggest that different data collection modes can result in very different responses, particularly when comparing visual vs. aural or interviewer-administered vs. self-administered modes. We attempted to minimize the impact of survey mode on responses by making the instruments as similar as possible, keeping the survey relatively short and straightforward and avoiding categorical responses that could result in primacy or recency effects (Dillman et al. 2009). Despite these efforts, we observed differences between CATI and mail treatments for some survey measures (Table 9). Specifically, in the license sample, estimates of fishing prevalence in the mail treatment were significantly higher than CATI estimates overall and in SC and FL. This result is consistent with the findings of Brick et al. (2012b), who reported that mail surveys resulted in larger estimates of active anglers[14] than telephone surveys for independent samples selected from a frame of licensed saltwater anglers. Brick et al. (2012b) hypothesized that measurement errors resulting from differences in screening approaches were responsible for the observed differences between telephone and mail interviews and introduced the concept of a "gatekeeper effect", where the initial respondent to a telephone interview provides inaccurate responses to screening questions – in this case, questions about fishing, effectively screening the household or individual out of the eligible sample. This

[14] Estimates of active anglers are the product of fishing prevalence (% of respondents reporting fishing) and the size of the sample frame (N).

hypothesis is supported by results from a follow-up telephone survey of licensed anglers that measured more household-level fishing activity when screener questions[15] asking about fishing activity were administered to the sampled angler than when they were administered to the person who answered the phone (Andrews and Foster, unpublished). In the current mixed-mode study, surveys targeted the sampled angler; survey materials were addressed to a named individual in the mail treatment, and telephone interviewers asked for the sampled angler by name in the CATI treatment. In addition, proxy reporting was not permitted for either treatment, although this is difficult to control in a mail survey. We would expect these design features to minimize any bias resulting from a gatekeeper effect, as the respondent, who is also likely to be the sampled angler, is likely to know about his or her own fishing activity.

An alternative explanation for the observed differences relates to the tasks imposed upon respondents in the respective treatments. Sampling from the license frames is conducted in a single phase with no advance notice. In the mail treatment, the respondent is in control of the interview. Subsequently, respondents have time to carefully consider the questions and review schedules or calendars that may help respond to the survey request (de Leeuw 2005). In fact, the mail questionnaire includes a recall aid in the form of a calendar depicting the two-month reference wave next to the questions about fishing activity. In telephone interviews, the interviewer is in control, and respondents are generally expected to answer questions immediately, without the benefit of memory cues or aided recall. Cursory cognitive processing resulting from the nature of the telephone interview may result in recall error and fewer reports of fishing activity (de Leeuw 2005).

A final possibility is that differences between CATI and mail treatments are the result of differential nonresponse bias between the data collection modes. For example, individuals who didn't fish during a reference wave may be less inclined to respond to a mail survey than a telephone survey. Brick et al. (2012b) considered this type of nonresponse bias, referred to as "avidity bias", when exploring differences between telephone and mail estimates generated from samples of licensed anglers and concluded that avidity bias is not a significant concern for samples of licensed anglers. Mail survey response rates in the current study are considerably lower than those observed by Brick et al. (2012b) due to design changes described previously. However, as previously mentioned, we did not observe differences in reported fishing activity between early and late responders, which suggests that avid anglers are no more inclined to respond to the survey than less avid or non-anglers.

The differences for fishing prevalence between CATI and mail license samples contrast with results from the ABS samples, where differences between CATI and mail treatments were neither significant, nor systematic for fishing prevalence. The questionnaires used for the license

[15] Screening questions sequentially ask how many people in the household fish, how many people in the household fished during the previous 12 months, and how many people in the household fished during the previous 2 months.

and topical ABS samples were identical. However, the screening procedures for identifying anglers were considerably different for the two frames. For the ABS sample, screening for anglers was conducted in a completely separate phase via a household mail survey. Subsequently, individuals included in the topical survey sample, in both CATI and mail treatments, were previously exposed to a survey from the same sponsor and expressed previous participation or likely future participation in recreational saltwater fishing. Receipt or completion of the screener survey may have provided a memorable event or fixed point against which subsequent fishing behavior (i.e., fishing during the reference wave) was measured and reported in the topical survey. In fact, individuals in the topical ABS sample are reminded about their participation in the screener survey, either in the cover letter for the mail treatment or the introduction to the CATI interview. In this sense, the screener questionnaire may have served as a memory cue that aided in recall of fishing activity and muted or eliminated any effects of data collection mode on survey measures, not unlike the use of a bounded interview design (e.g., National Crime Victimization Survey; Consumer Expenditure Survey) (Neter and Waksberg 1964).

The results from the topical ABS survey support the suggestion that differential nonresponse bias does not contribute to the differences between data collection modes in the license samples for estimates of prevalence. The topical ABS and license samples are similar in that they were both selected from lists of likely saltwater fishing participants; licensed anglers for the license sample and self-reported anglers for the ABS sample[16]. Given their presumed interest in saltwater fishing, we might expect individuals on the two frames to have similar propensities to respond to the survey request, in which case, we would expect the impact of nonresponse bias on estimates to be consistent across sample frames. This also provides further evidence that the screening approach, which is the same across treatments in the topical ABS, but substantially different across treatments in the license survey, contributes to the differences in fishing prevalence in the license survey, as suggested by Brick et al. (2012b).

We also observed differences between CATI and mail for estimates of mean shore trips per angler, for those anglers that reported shore fishing during the wave. Previously, we suggested that CATI estimates of fishing prevalence may be susceptible to recall bias, and that the effect is likely to be greater in the license treatment, where the CATI interview is the first survey contact. The nature of the CATI interview may similarly limit respondents' ability to accurately recall the number of discrete fishing events that occurred during the reference wave, particularly if those events aren't especially memorable. We suggest that differences between modes in estimated fishing activity could be mainly limited to shore fishing because shore fishing is a less memorable than private boat fishing – private boat fishing generally requires a greater investment in both time and money, which is likely to increase the salience of the activity. However, we do not expect the effect of this recall bias to be limited to the ABS samples. In fact, based upon our earlier discussion, we expect recall bias to have a greater impact on the

[16] Some individuals in the ABS samples were also licensed to fish.

license samples, where respondents are asked to describe fishing activity, including enumeration or estimation of the number of fishing events, without the benefit of memory cues or a previous survey contact.

One possible explanation for this anomaly is that proxy reporting is permitted for the topical ABS CATI sample in some circumstances – proxy reporting is not permitted in the license sample[17]. We observed that, on average, proxy respondents reported fewer trips than self-respondents. However, proxy reporting accounted for less than 1.5% of total reporting, and eliminating proxy responses from our analysis did not significantly impact the outcomes. Based upon these results, we cannot conclude that the differential mode effects for the ABS and license samples are the result of proxy reporting, although the extent of proxy reporting may be larger than we are aware.

A second possibility is that differences in demographic characteristics, resulting from differential coverage of the two samples, are responsible for the differences in estimated shore fishing activity between CATI and mail treatments. Topical ABS samples are randomly distributed between CATI and mail treatments after sample units without a known telephone number are assigned to the mail treatment. This design could impact mode comparisons if sample units without telephone numbers, which are restricted to the mail treatment, fish more or less than sample units with telephone numbers. For example, Blumberg and Luke (2013) report that residents of wireless-only households are more likely to be younger and single than residents of households with landline telephones. Estimates of fishing effort could be impacted if characteristics such as these are correlated with recreational fishing activity. For waves 1-4, telephone number was obtained through the ABS screener survey – screener survey respondents were asked to provide a telephone number - as well as through commercial directory matching. We suspected that asking respondents to provide a telephone number had a negative impact on screener response rates, so we eliminated this question following wave 4, and telephone number was obtained solely through directory matching for waves 5 and 6. The survey datasets identify whether or not a telephone number could be identified through directory matching, but do not identify sample units that provided a telephone number in the screener survey. Since directory matching was the sole criteria used to identify telephone numbers for waves 5 and 6, we examined mail survey data for these waves to compare fishing activity between households with and without a telephone number. The comparison indicates that respondents with a matched telephone number reported fewer shore fishing trips (4.8 trips per angler) than respondents without a matched telephone number (5.4 trips per angler). While this difference is not significant, it is in the right direction to contribute to the differences in reported shore fishing activity between mail and CATI treatments and suggests that CATI estimates may be biased due to coverage error. We also note that differences in private boat fishing between respondents with

[17] Proxy reporting is permitted in the ABS CATI sample if the proxy respondent resides at the same addresses as the intended respondent, and he or she also is included in the topical ABS sample.

and without telephones are smaller; 4.1 trips per angler for respondents with a telephone and 4.3 trips per angler for respondents without a matched telephone number.

We cannot identify a single source of survey error that accounts for differences between CATI and mail estimates. Rather, we suggest that a combination of errors, including both measurement error and coverage error, are likely to contribute to the differences. We further suggest that estimates of shore fishing activity are more susceptible to error than estimates of private boat fishing due to differences in the nature of these activities. Specifically, private boat fishing requires greater investment in both time and money than shore fishing and is likely more memorable. Additional study is needed to more fully assess these hypotheses.

4.5. Coverage of State License Databases

An objective of this study was to assess the adequacy of state license databases for sampling recreational anglers. Previous studies (Brick et al. 2012b) suggested that a significant portion of recreational saltwater fishing trips are taken by unlicensed anglers. In the current study, we compared total effort estimates derived from the license samples to estimates derived from the ABS samples, which include all anglers, regardless of whether or not they had a fishing license (Table 10). These comparisons affirm the results reported by Brick et al. (2012) that total effort estimates derived from ABS samples are consistently larger than license estimates. Overall, ABS estimates were nearly twice as large as license estimates for shore fishing and 1.75 times larger for private boat fishing; differences between ABS and license estimates were larger for shore fishing than private boat fishing in each state. Differences between ABS and license estimates were largest in GA, where ABS estimates were nearly 5 times larger than license estimates for both private boat and shore fishing, and smallest in NC, where ABS estimates were approximately 1.5 times larger than license estimates for both types of fishing.

We considered different types of survey error to explain the differences between ABS and license estimates. An obvious source of bias in the license survey is coverage error resulting from anglers who fish without a license. Currently, all states included in this study require, with limited exceptions, a fishing license for anyone who participates in recreational saltwater fishing. Exemptions to licensing requirements include minors 16 years of age or younger, individuals who fish on for-hire vessels such as charter boats, individuals who fish from state-licensed fishing piers, and Florida residents age 65 or older (only applies to fishing activity within the state of Florida). Coverage error in the license survey could result from either exempted segments of the population as described above, or individuals who fail to comply with licensing requirements.

The survey instruments excluded charter boat fishing, and minors less than 18 were excluded from both sample frames, so exemptions for these categories of fishing are not likely to contribute to the observed differences between the two samples. The fact that differences between ABS and license estimates are larger for shore fishing than private boat fishing suggests that the license exemption for pier fishing is a source of coverage bias – there is no such exemption for any type of private boat fishing. However, we did not characterize different types of shore fishing activity, so we can't fully quantify the magnitude of bias resulting from this exemption. Finally, we explored the impact of the senior exemption in FL on license estimates by comparing the distribution of trips among age categories for the two samples. Based upon the license sample, approximately 2% of total fishing trips were taken by seniors older than 65. In contrast, seniors accounted for more than 23% of total fishing trips in estimates derived from the ABS sample. This suggests that the senior exemption in FL is a potentially large source of coverage bias in license estimates.

Effort estimates derived from license samples will also be biased if individuals fail to comply with licensing requirements. We attempted to characterize unlicensed anglers by comparing age and gender distributions across samples for those respondents who reported fishing during the reference waves. In each state, there were relatively more female anglers in the ABS samples than the license samples, suggesting that unlicensed anglers are disproportionately female. Comparisons of age distributions were inconsistent among states. In FL, the ABS sample includes a much higher percentage of seniors (65+) than the license sample – 24% vs. 2.7%. This suggests that senior anglers are more likely to fish without a license than younger anglers, which is consistent with the senior license exemption described above. This result contrasts with the characteristics of the samples in SC and GA, where younger anglers are more likely to fish without a license than seniors. Finally the age compositions of the two samples in NC are very similar; relative differences in age composition were less than 4% for all age classes. Coincidentally, ABS and license effort estimates are most similar in NC.

We also considered that differential nonresponse bias (avidity bias) may contribute to the observed differences in estimates between the license and ABS samples. Previous studies (Andrews et al. 2010; Brick et al. 2012b) demonstrated that households with licensed anglers are more likely to respond to a survey about fishing than households without licensed anglers and also more likely to report fishing during the reference period. Consequently, estimates of fishing effort will be biased if nonresponse weights are not adjusted to account for this differential nonresponse. We observed similar differences in response rates and topical survey eligibility between matched and unmatched households and adjusted nonresponse weights accordingly, presumably minimizing the effect of avidity bias on estimates generated from ABS samples. Consequently any residual avidity bias in the ABS sample would be limited to anglers in households that could not be matched to state license frames. It should be noted that estimates generated from license samples may also be impacted by avidity bias, although we suspect that the impacts are minimal, as described above. While we can't rule out nonresponse error as a potential contributor to differences between ABS and license estimates, it seems more likely that coverage error, resulting from license exemptions and illegal fishing activity, has a larger impact.

5. Discussion

Recreational saltwater fishing is a relatively rare occurrence among the general population, which presents challenges for collecting recreational fishing data in an efficient manner (Kalton and Anderson 1985; Lohr 2009). Results from this study continue to demonstrate that mail survey designs are feasible for collecting recreational fishing data, and that incorporating angler license databases into the sampling design provides a useful mechanism for increasing efficiency. The ABS sample frame provides nearly complete coverage of U.S. housing units, minimizing the potential for under-coverage error (Iannacchione 2011), and multi-frame designs can improve data collection efficiency, particularly when sampling rare populations (Lohr 2009). Final response rates for the mail survey treatment were higher than the CATI treatment and eclipsed CATI response rates after about three weeks of data collection, demonstrating that a mail survey design can match the current CHTS estimation schedule. Furthermore, the lack of differences between preliminary and final mail survey estimates provides assurance that preliminary point estimates, derived from partial survey data, will be similar to final estimates, produced after data collection has been completed.

While these benefits support further exploration of dual-frame, mail survey designs as a potential replacement for traditional RDD approaches, several challenges persist. For example, matching errors, resulting in misclassification of sampling domains, continue to be a challenge and are likely to result in biased estimates. More complete and accurate sample frames will minimize matching errors and decrease the risk of bias resulting from under-coverage and nonresponse. In addition, the present study achieved total response rates, considering both phases of ABS sampling, of 17.3%, which are only modestly higher than CHTS response rates. Including a cash incentive in topical survey mailings would likely have resulted in significantly higher response rates – previous MRIP pilot studies achieved total response rates of more than 30%, and a cash incentive increased screener response rates in the present study by 10-15%. However, the response rate is also an artifact of the two-phase ABS design, where total response rates are the product of screener and topical survey response rates. Brick et al. (2012b) proposed an alternative design that maintains the efficiency and coverage of the dual-frame, two-phase design but addresses concerns about matching errors and poor response rates. In the proposed design, address samples are matched to angler license databases by address and telephone number. Augmenting sample in this manner screens the ABS sample prior to data collection, effectively stratifying the sample into households with and without licensed anglers (Lohr 2009). This provides an opportunity to optimize sampling, making the data collection more efficient (Kalton and Anderson 1986). For example, addresses that match to license databases can be sampled at a higher rate than unmatched households, maximizing the collection of fishing information while maintaining the coverage of the ABS frame. Because the license information is only used to stratify the ABS sample, matching errors will only impact the efficiency of data collection; matching errors will not result in biased estimates. Brick et al. (2012b) also suggest collecting data from the ABS sample in a single phase, which would likely achieve considerably higher response rates than the two-phase approach. We would expect response rates similar to those obtained in the first phase ABS sample, all of which were over 40% when a $1.00 cash incentive was included in the survey mailing.

Finally, no comparison between data collection modes would be complete without a discussion about survey costs. The costs incurred for the present study do not provide an accurate representation of data collection costs for an ongoing survey due to the experimental nature of the project. Furthermore, we did not differentiate operational costs between the two survey modes. However, Link et al. (2007) reported that the operational costs of a telephone survey were 12% higher than the costs of a mail survey in an experiment comparing address-based sampling and random-digit-dialing. Similarly, deLeeuw (2008) suggests that mail surveys require fewer personnel than telephone surveys, which should translate into lower costs. We would expect the cost of a mail survey to be equal to or slightly less than the cost of a comparable telephone survey.

Specific conclusions and recommendations include the following:

1. Mail surveys are a feasible alternative to telephone surveys for collecting recreational fishing effort data. Mail surveys result in higher response rates than telephone surveys, and preliminary mail survey estimates can be generated in a timeframe consistent with the current CHTS estimation schedule.
2. Incorporating angler license databases into data collection designs increases the efficiency of recreational fishing surveys. While sample frames derived from license databases may be incomplete due to unlicensed fishing activity, samples of licensed individuals and households with licensed anglers are much more likely to report fishing than general population samples. Supplementing household samples with information from license databases should increase the efficiency of data collection while maintaining coverage of the entire population.
3. Frame matching errors are a recurring problem and potential source of bias in dual-frame sampling designs. Frame standards, which were implemented during the study, will help minimize matching errors.
4. Further study is needed to better understand the impact of data collection mode on survey measures. However, we hypothesize that differences between CATI and mail estimates are the result of recall error and coverage error, and that telephone samples are more susceptible to biases resulting from these errors. These hypotheses were not tested in the present study.
5. In the South Atlantic region, it is currently not feasible to sample exclusively from state license databases. In the present study, total effort estimates derived from license samples were considerably lower than ABS estimates. We attribute these differences to coverage error resulting from license exemptions and unlicensed fishing activity.
6. Cash incentives provide a substantial boost in response rates for mail surveys and should be considered in any mail survey design.

7. The single-phase ABS design with screening prior to data collection proposed by Brick et al. (2012) should be tested as an alternative to dual-frame designs. Such a design is not susceptible to bias resulting from matching error and is likely to result in higher response rates than the two-phase design.

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Tables and Figures

Table 4. Overall fishing prevalence - percent of respondents that reported fishing during the reference wave. For ABS samples, fishing prevalence represents both phases of sampling and is the product of screener eligibility and topical survey prevalence.

	ABS Sample		License
	Unmatched	Matched	Sample
Overall	6.6	21.9	37.2
State			
North Carolina	3.3	20.7	27.5
South Carolina	4.8	10.9	18.4
Georgia	2.2	6.3	11.2
Florida	10.5	29.0	50.4
Wave			
Wave 1	6.3	13.1	28.7
Wave 2	7.3	20.4	36.4
Wave 3	7.6	30.8	39.0
Wave 4	6.0	23.6	47.9
Wave 5	6.3	21.4	39.8
Wave 6	5.7	23.7	36.7

Table 5. Estimated and actual number of addresses with licensed anglers for each state and wave. Estimates are significantly different from actual counts in every comparison except wave 2 in North Carolina at the α=0.05 level.

	ABS Sample Estimate (000's)	License Frame (000's)	Ratio of ABS to License	95% CI of Lower Limit	95% CI of Upper Limit
North Carolina					
Wave 2	423	406	1.04	0.96	1.13
Wave 3	401	363	1.10	1.01	1.20
Wave 4	251	185	1.36	1.21	1.51
Wave 5	386	328	1.18	1.08	1.28
Wave 6	260	218	1.19	1.07	1.31
South Carolina					
Wave 2	227	264	0.86	0.79	0.93
Wave 3	231	273	0.85	0.78	0.91
Wave 4	228	170	1.34	1.24	1.45
Wave 5	195	153	1.27	1.16	1.38
Wave 6	198	153	1.29	1.18	1.41
Georgia					
Wave 2	167	131	1.28	1.12	1.45
Wave 3	123	103	1.20	1.02	1.37
Wave 4	226	99	2.29	2.04	2.54
Wave 5	227	111	2.05	1.83	2.28
Wave 6	250	126	1.99	1.78	2.19
Florida					
Wave 2	190	831	0.23	0.19	0.27
Wave 3	742	640	1.16	1.06	1.26
Wave 4	848	571	1.49	1.36	1.61
Wave 5	981	621	1.58	1.46	1.70
Wave 6	1,130	715	1.58	1.47	1.69

Table 6. Mixed mode survey response rates by domain[18].

	ABS Sample			License
	1st Phase	2nd Phase	Overall	Sample
Overall	37.3	46.4	17.3	33.1
State				
North Carolina	36.9	49.1	18.1	34.5
South Carolina	36.2	47.2	17.1	36.0
Georgia	40.0	42.8	17.1	32.6
Florida	39.4	46.3	18.2	31.7
Wave[19]				
Wave 1	27.7	55	15.2	32.9
Wave 2	32.1	45.1	14.5	32.8
Wave 3	31.6	48.8	15.4	33.0
Wave 4	43.2	46.7	20.2	33.2
Wave 5	45.1	39.9	18.0	33.2
Wave 6	44.1	42.7	18.8	33.4
License Match				
Match	43.0	50.7	21.8	NA
No Match	36.8	45.5	16.7	NA

[18] The matched domain includes addresses that could be matched to angler license databases. Addresses in the unmatched domain could not be matched to a record in a license database.

[19] A $1.00 cash incentive was included in the initial 1st phase ABS mailing beginning in wave 4 and continuing through wave 6.

Figure 1. ABS Screener response rates (AAPOR RR3) by survey wave.

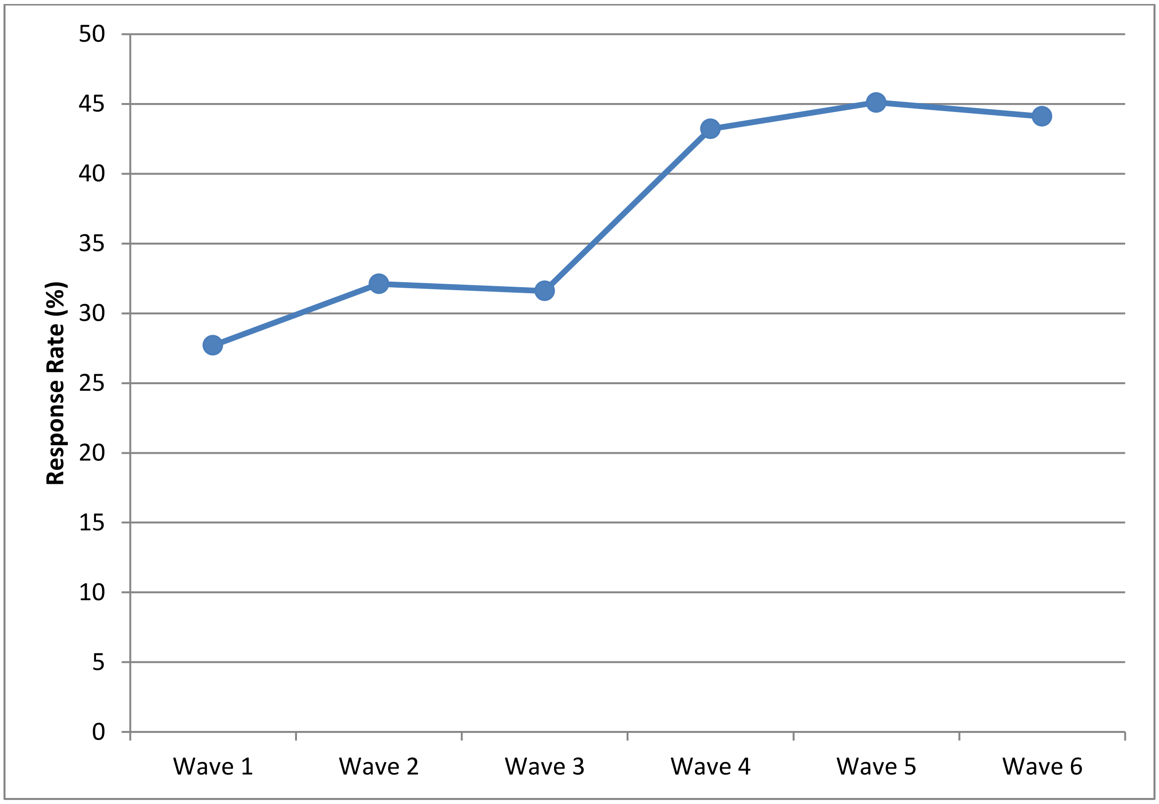

Figure 2. ABS screener survey – cumulative distribution of completed surveys over time.

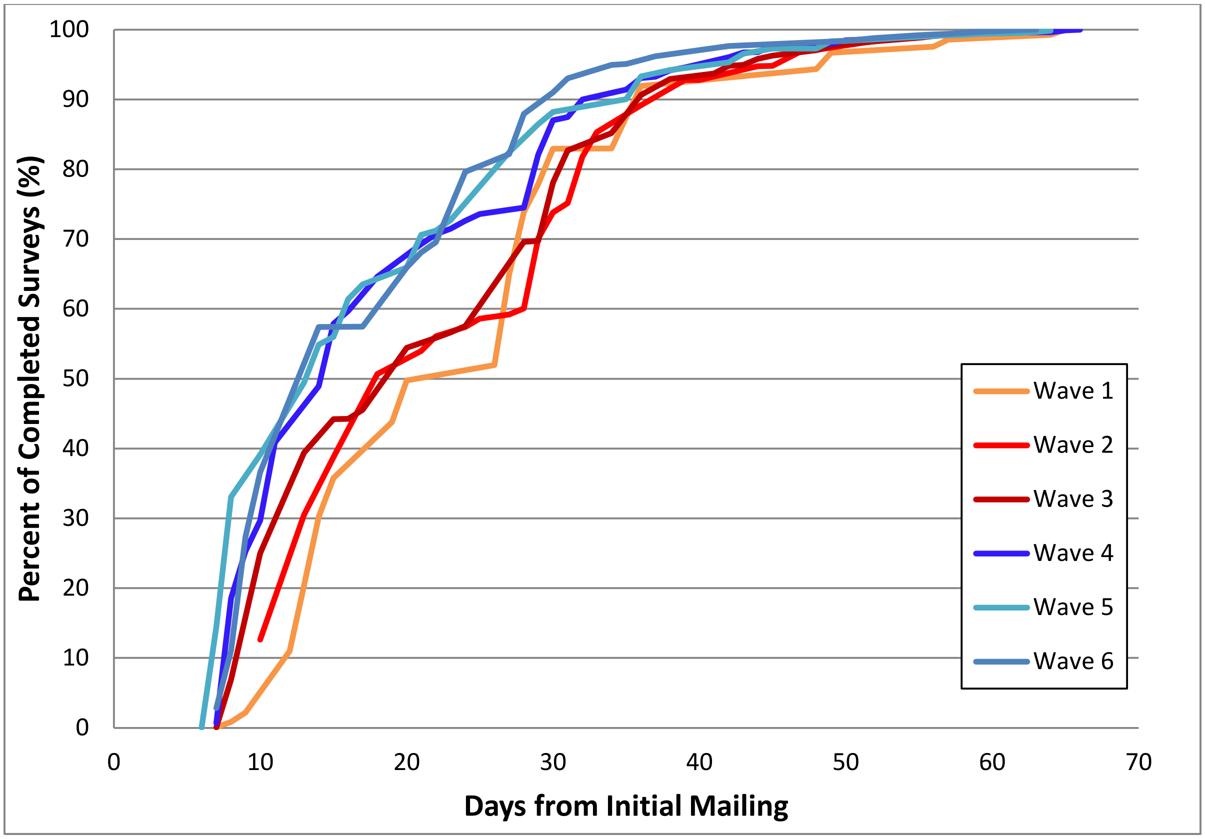

Table 7. Preliminary[20] and final overall response rates by data collection mode for the ABS and license sample data collections. Response rates for the ABS sample reflect response rates for the two phases of sampling.

	CATI %	Mail %	Total %
ABS Sample			
Preliminary	14.3	13.1	13.6
Final (all sample)	14.4	19.2	17.3
Final (with phone match)	13.4	22.0	16.9
License Sample			
Preliminary	27.4	22.4	24.8
Final	27.4	38.1	33.1

[20] Preliminary response rates reflect complete CATI data and mail surveys that were returned within three weeks of the initial survey mailing date.

Figure 3. Cumulative mail survey response rates for the second-phase ABS (blue) and license (red) samples over all states and waves. Dashed lines represent the final CATI response rates for the respective sample frames.

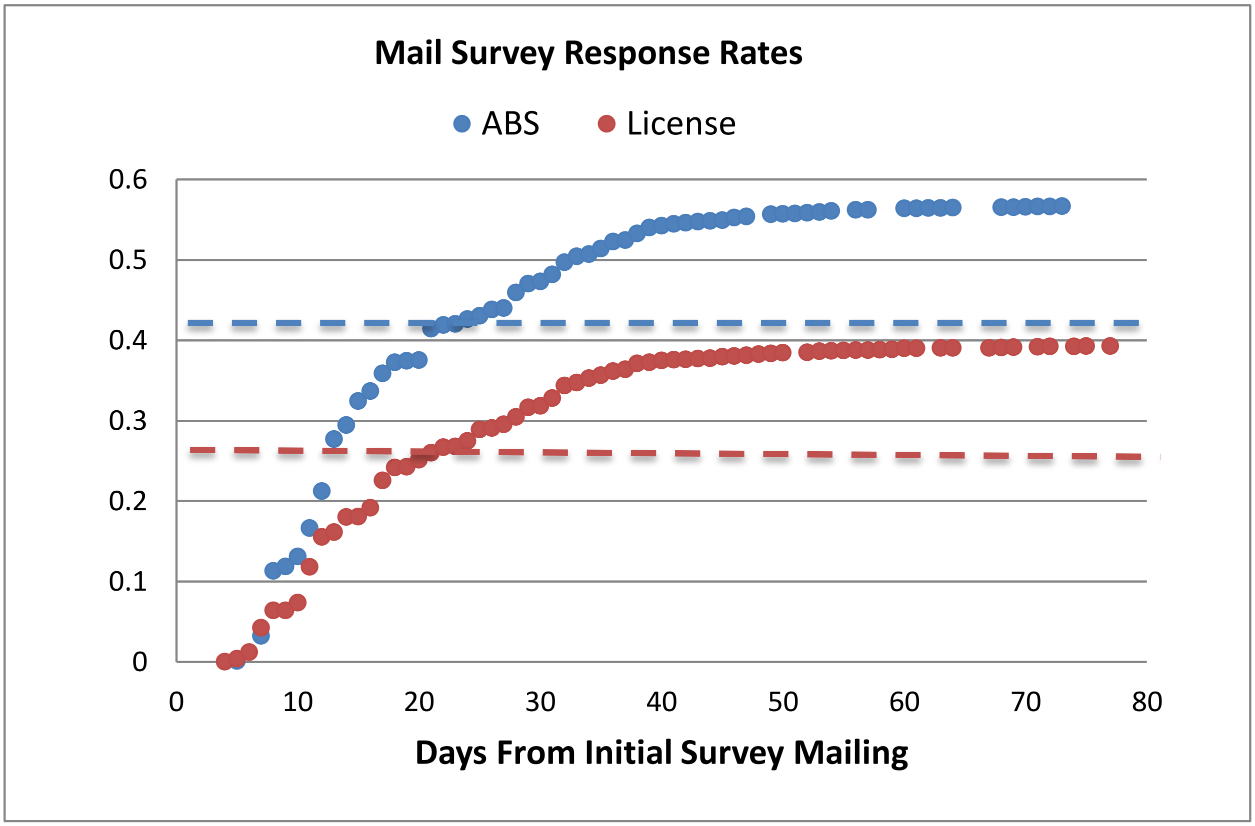

Table 8. Preliminary[21] and final estimates of fishing prevalence and mean private boat and shore trips per angler for anglers who reported fishing in the mode during the reference wave for the ABS, mail sample.

	North Carolina		South Carolina		Georgia		Florida		Overall	
	Prelim n=631	Final n=934	Prelim n=649	Final n=955	Prelim n=357	Final n=533	Prelim n=849	Final n=1,214	Prelim n=2,486	Final n=3,636
Fishing rate	17.2	18.2	24.5	12.8	12.9	11.7	31.9	33.6	27.0	27.6
Mean boat trips	3.7	4.1	3.1	3.9	3.5	4.1	4.7	4.7	4.4	4.5
Mean shore trips	5.1	4.9	4.5	5.5	3.4	3.7	6.6	6.8	6.0	6.2

[21] Preliminary and final estimates were derived from mail surveys that were returned within 3 weeks and 12 weeks of the initial survey mailing, respectively.

Table 9. Estimates of fishing activity by state and data collection mode for the final second-phase ABS and license samples. Mean shore and boat trips are for individuals who reported fishing in the mode during the wave. Significance: * indicates a significant difference (*p*<0.05) between CATI and mail estimates

	North Carolina		South Carolina		Georgia		Florida		Overall	
	CATI n=596	Mail n=934	CATI n=538	Mail n=955	CATI n=346	Mail n=533	CATI n=661	Mail n=1214	CATI n=2141	Mail n=3636
ABS Sample										
Prevlance	21.9	18.4	19.4	23.9	13.4	11.7	37.0	33.4	29.0	26.7
Mean boat trips	4.2	4.1	3.9	3.7	3.0	4.1	4.7	4.7	4.5	4.5
Mean shore trips	3.9	4.8	3.9	5.0	3.2	3.6	5.1	6.1	4.6	5.6*

	North Carolina		South Carolina		Georgia		Florida		Overall	
	CATI n=887	Mail n=1330	CATI n=906	Mail n=1433	CATI n=886	Mail n=1213	CATI n=797	Mail n=1234	CATI n=3476	Mail n=5210
License Sample										
Prevalence	26.0	29.0	14.0	22.4*	10.4	12.1	45.2	55.1*	33.5	40.9*
Mean boat trips	4.4	4.3	4.9	4.6	3.8	4.5	4.4	4.6	4.4	4.6
Mean shore trips	5.3	5.1	4.6	4.8	4.1	4.6	6.2	5.3	5.9	5.2

Table 10. Estimates of total fishing effort, in thousands of angler trips, for shore and private boat fishing. ABS estimates represent total fishing effort, including fishing by both licensed and unlicensed anglers. License estimates represent fishing activity by licensed anglers only. Percent coverage is the ratio of license estimates to ABS estimates.

	Private Boat			Shore Fishing		
	ABS	License	ABS:License	ABS	License	ABS:License
North Carolina	2,409	1,596	1.51	3,689	2,420	1.52
South Carolina	1,405	855	1.64	1,909	781	2.45
Georgia	1,166	240	4.85	1,223	251	4.88
Florida	19,139	8,487	2.26	20,127	8,209	2.45
Overall	24,119	13,836	1.74	26,948	13,811	1.95

Development and Testing of Recreational Fishing Effort Surveys

Testing a Mail Survey Design

Final Report

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1. Executive Summary

The mail survey design evaluated in this project is the culmination of several years' worth of testing and analysis to develop an alternative to the Coastal Household Telephone Survey (CHTS) for estimating marine recreational fishing effort. The objectives of the project were to: 1) test the feasibility of the design for collecting recreational fishing effort data and estimating fishing effort for shore and private boat anglers, 2) compare mail survey and CHTS results, including metrics of survey quality and estimates of marine recreational fishing activity, 3) describe, to the greatest extent possible, differences between mail survey and CHTS estimates in terms of sources of survey error, and 4) provide recommendations for follow-up action, including implementation of improved survey methods.

This report is intended to summarize the findings for a non-technical audience. For those interested in the details related to sampling, estimation, or instrument design, we have provided references to detailed reports and publications.

Results from the study continue to demonstrate that mail survey designs are a feasible alternative to telephone surveys for collecting recreational fishing data and producing population estimates in a timely manner. Overall, final mail survey response rates were nearly three times higher than CHTS response rates, and preliminary estimates, derived from partial data collected within two weeks from the end of the reference wave, were not significantly different from final estimates, demonstrating that a mail survey can generate stable fishing effort estimates within the current estimation schedule for the CHTS. In addition, the sampling design, which includes over-sampling of households with licensed anglers, is more efficient for collecting fishing data than simple random sampling currently used for the CHTS.

Overall, the mail survey estimate of total fishing effort was 4.1 times larger than the corresponding CHTS estimate. Differences between mail survey and CHTS estimates, which were relatively consistent among the states included in the study, can largely be attributed to differences in fishing prevalence – households in the mail survey sample were more likely to report fishing than households in the CHTS sample. We explored these differences within the context of survey error and conclude that the mail survey design is less susceptible than the CHTS to bias resulting from nonresponse and non-coverage. We also suggest that the nature of

the mail survey mode results in more accurate responses to questions about fishing activity than the CHTS, which expects respondents to answer questions on-the-spot, without the benefit of aided recall or memory cues. Finally, we demonstrate that the CHTS sampling levels and estimation strategy may introduce biases, particularly in low-activity waves, and we suggest that CHTS coverage correction factors, derived from a complementary onsite site survey of completed fishing trips to compensate for the geographic limitations of the CHTS, may result in biases in fishing effort estimates due to the exclusion of private access fishing sites from the onsite survey sample frame.

Given the potential for bias in the CHTS, we conclude that the mail survey design is a superior approach for monitoring recreational fishing effort. We also encourage continued testing and evaluation to assess additional sources of survey error and ensure that evolving advancements in survey methodology are considered and customer needs are satisfied.

2. Background

Traditionally, marine recreational fishing effort data for the U.S. Atlantic Coast and the Gulf of Mexico have been collected by NOAA Fisheries through the Coastal Household Telephone Survey (CHTS). The CHTS utilizes a list-assisted, random digit dialing (RDD) telephone survey approach to contact residents of coastal county households and collect information on fishing activities that occurred within a two-month reference period (wave). Specifically, households are screened to determine if any household members participated in marine recreational fishing during the previous 2 months, and each active angler is asked to recall, episodically, the number of saltwater fishing trips that were taken during the wave, as well as provide details about each trip.

In recent years, the efficiency and effectiveness of RDD surveys in general, and the CHTS specifically, have been questioned due to declining rates of coverage and response (Curtin et al. 2005; Blumberg and Luke 2013). A 2006 review by the National Research Council (NRC 2006) noted that the CHTS design suffers from inefficiency due to the low rate of saltwater angler participation among the general population, as well as potential coverage bias due to the survey's limitation to coastal county residences and landline-based telephone numbers (NRC 2006). In addition, response rates to the survey have declined considerably over the past decade, increasing the potential for nonresponse bias. To address these shortcomings, the NRC review recommended the development of and subsequent sampling from a comprehensive list of registered saltwater anglers or, in the absence of such a list, implementation of dual-frame procedures that include sampling from both lists of licensed saltwater anglers and residential household frames.

The Marine Recreational Information Program (MRIP) has designed and tested several different data collection alternatives to address concerns about the CHTS. Below, we outline the various approaches to collecting fishing effort data that have been studied by MRIP. More detailed descriptions of the data collection designs and comparisons of estimates and metrics of survey quality, such as response rates and coverage rates, are documented elsewhere (Brick et al. 2012a; Andrews et al. 2013).

2.1 Angler License Directory Telephone Survey

As noted by the NRC, a more efficient approach for surveying anglers is to sample directly from lists of individuals who are licensed to participate in saltwater fishing. Working collaboratively with the Gulf States Marine Fisheries Commissions, the Gulf Coast states, and the North Carolina Division of Marine Fisheries, MRIP has designed and tested Angler License Directory Telephone Surveys (ALDS), which sample from state databases of licensed anglers. The ALDS was implemented as a pilot project in Florida (FL), Alabama (AL), Mississippi (MS) and Louisiana (LA) in 2007 and expanded to North Carolina (NC) in 2008. The survey was most recently administered in in 2012.

The data collection procedures for the ALDS are nearly identical to the CHTS, with the exception of the screening portion of the survey; the ALDS requests to speak with the individual licensed angler by name and then proceeds to determine if the angler, or any other individuals who reside in the same household as the angler, fished during the wave. As with the CHTS, trip details are collected through episodic recall beginning with the most recent trip.

As predicted, the ALDS is more efficient than the CHTS at identifying anglers – in a recent reference wave, 46% of ALDS respondents reported fishing, while only 6.5% of CHTS respondents reported fishing during the same wave. However, exemptions to state licensing requirements and unlicensed fishing activity, as well as incomplete and inaccurate contact information for individuals included on the sample frames, create gaps in the coverage of the ALDS. Subsequent studies (Brick et al. 2012a; Andrews et al. 2013) have suggested that undercoverage due to unlicensed fishing activity may be as high as 70% in some states for certain types of fishing activity, and that as many as 20% of frame entries may be unreachable due to “bad” (missing, nonworking, wrong number) telephone numbers. In addition, response rates for the ALDS are only marginally higher than CHTS response rates.

2.2 Dual-Frame Telephone Survey

As noted above, the CHTS and the ALDS, considered individually, do not provide complete coverage of the angler population. To compensate for potential sources of coverage error in the CHTS and ALDS, MRIP developed an estimation design that integrates CHTS and ALDS sampling in a dual-frame design (Lai and Andrews 2008). The union of the CHTS and ALDS sample frames defines three domains: 1) anglers who can only be sampled from the CHTS frame (unlicensed anglers with landline phones who reside in coastal counties covered by the CHTS); 2) anglers who can only be sampled from the ALDS frame (licensed anglers who reside outside of the coverage area of the CHTS); and, 3) anglers who can be sampled from both the CHTS and ALDS frames (licensed anglers who reside in coastal counties). A fourth domain includes anglers who cannot be sampled by either the CHTS or ALDS (unlicensed anglers without landline telephones within the CHTS coverage area and unlicensed anglers residing outside the coverage area of the CHTS).

While the dual-frame telephone survey design increases the coverage over either the CHTS or the ALDS, the methodology is not without limitations. As mentioned, the union of the CHTS and ALDS sample frames excludes a segment of the angling population, creating a potentially significant gap in coverage - up to 38% of fishing trips in NC are taken by anglers who are excluded from either the CHTS or ALDS (Andrews et al. 2010). In addition, partitioning anglers into the appropriate domains and subsequently adjusting sample weights is based upon the survey respondents' willingness and ability to classify themselves as licensed or unlicensed anglers. This has been demonstrated to be an unreliable approach for defining dual-frame domains (Andrews et al. 2010) and results in survey weights that may produce biased estimates. Finally, the dual-frame telephone survey approach is susceptible to nonresponse bias due to the low response rates of the component surveys.

2.3 Dual-Frame Mail Survey

An alternative to the dual-frame telephone survey is to identify and contact anglers through a dual-frame mail survey design. MRIP initially tested the feasibility of a dual-frame mail survey design in NC in 2009, and conducted a follow-up study aimed at enhancing response rates and the timeliness of responding in NC and LA in 2010.

The specific details of the dual-frame mail survey design tested in 2009 and 2010 are described elsewhere (Andrews et al. 2010; Brick et al. 2012b). Briefly, anglers are sampled both from state databases of licensed saltwater anglers and residential address frames maintained and made commercially available by the United States Postal Service. The address-based sample (ABS) is matched to the license databases by searching the license frame for the same address and/or telephone number (for the cases in which a telephone number can be located through a commercial service for the ABS sampled address). This matching identifies those households that could be sampled from both frames.

For the studies conducted in 2009 and 2010, anglers were sampled from the license frame in a single phase, and the sampled anglers were mailed a brief questionnaire asking them to report the number of days fished from the shore and from a boat during a two-month reference wave. The ABS sampling was conducted in two phases. In the first phase, residential addresses in the state were sampled and mailed a screening questionnaire to identify individuals who fished during the previous twelve months. In the second phase, anglers identified in the screening phase were sent a second-phase questionnaire that was identical to the one sent to those sampled from the license frame.

Results of these pilot studies were encouraging; sampling from the ABS frame provides nearly complete coverage of the U.S. population, and response rates to the mail survey were greater than either the ALDS or CHTS (Andrews et al. 2010; Brick et al. 2012a). In addition, the ability to match ABS sample to license frames *a priori* by address matching provides a more effective means for defining domain membership that is not susceptible to recall error or inaccurate reporting. Frame matching also provides supplemental information for assessing nonresponse error for the ABS sample and for nonresponse weighting adjustment.

The dual-frame mail survey design provides many benefits over telephone survey approaches. However, frame matching is not 100% accurate, resulting in misclassification of domain membership for some sample units; generally frame units that could have been sampled from both frames are excluded from the overlapping domain due to a failure to match. Consequently, dual-frame weights are not down-weighted appropriately, resulting in an overestimation of fishing effort (Brick et al. 2012a). In addition, there were concerns that a mail survey design could not satisfy customer needs for timely estimates, although comparisons between early survey returns and later survey returns showed little difference in terms of fishing activity, suggesting that preliminary effort estimates could be produced within the timeframe required by customers.

2.4 Dual-Frame, Mixed-Mode Survey

To further address concerns about timeliness, as well as explore differences between mail and telephone data collection modes, MRIP administered a dual-frame, mixed-mode survey in 2012 (Andrews et al 2013). The sample design for the survey was nearly identical to the dual-frame mail survey – anglers were sampled from angler license frames and households were sampled from residential address frames. As with the dual-frame mail survey, the ABS sample was mailed a screening questionnaire to identify anglers at the sampled addresses. The methodology differed from the dual-frame mail survey in that anglers identified through household screening, as well as anglers sampled from the state license databases, were randomly allocated into telephone and mail treatment groups – anglers in the telephone treatment group were contacted and asked to provide information about recent recreational fishing trips through a telephone interview, and anglers in the mail treatment group were mailed a questionnaire that asked about recent recreational fishing activity. If no phone number for the sampled household was available, then the second phase was done by mail.

Results from the study continued to demonstrate that mail survey designs are feasible for collecting recreational fishing data and estimating fishing effort. Final response rates for the mail survey component of the study were higher than the telephone component and eclipsed telephone survey response rates after about three weeks of data collection (Andrews et al., 2013). In addition, preliminary estimates derived from early mail survey returns were not significantly different from final estimates, demonstrating that a mail survey can generate valid preliminary estimates within the current estimation schedule for the CHTS.

The impact of data collection mode on survey measures required further investigation. We hypothesized that differences between telephone and mail estimates were the result of differential recall and coverage errors, and suggested that telephone samples are more susceptible to bias resulting from these errors (Andrews et al. 2013).

3. Mail Survey with Screening Prior to Data Collection

The pilot tests described in the previous section were very informative and provided the basis for a revised design that is the focus of this report. The revised design again uses a mail questionnaire to collect data from households, but also addresses weaknesses identified in the prior studies. For example, the design uses the license frame in a way that eliminates biases resulting from inaccurate matching to the address frame. Furthermore, the mail data collection scheme and the questionnaire were revised to attempt to further increase response rates. These and other features of the design are described below.

The new design was tested in MA, NY, NC and FL beginning in wave 5 (Sep/Oct), 2012 and continuing through wave 6 (Nov/Dec), 2013. The objectives of the study are to assess the feasibility of the design in terms of response rates, timeliness, and efficiency, as well as examine the impact of different sources of survey error on estimates of fishing prevalence and total fishing effort.

3.1 Methods

The survey employed a dual-frame design with non-overlapping frames; residents of the target states - states included in the pilot study - were sampled from the United States Postal Service computerized delivery sequence file (CDS), and non-residents - individuals who were licensed to fish in one of the target states but lived in a different state - were sampled from state-specific lists of licensed saltwater anglers.

Sampling from the CDS utilized a stratified design in which households with licensed anglers were identified prior to data collection (Lohr 2009). The address frame for each state was stratified into coastal and non-coastal strata defined by geographic proximity to the coast[1]. For each wave and stratum, a simple random sample of addresses was selected from the CDS and matched to addresses of anglers who were licensed to fish within their state of residence[2]. Augmenting address samples in this manner effectively screened the sample into strata defined by the presence (matched) or absence (unmatched) of at least one licensed angler at an address. All matched addresses were retained in the sample and unmatched addresses were subsampled at a rate of 30%. Initial addresses samples were sufficiently large to support subsampling from the unmatched stratum. Screening the address sample prior to data collection and subsampling the resulting sub-populations at different rates (e.g., sampling addresses with licensed anglers at a higher rate) was expected to increase the efficiency of the design while maintaining the coverage of the address frame – two concerns identified by the NRC Review. Furthermore, because the

[1] For waves 1, 2 and 6 the coastal strata included all addresses in counties that were within 25 miles of the coast. For waves 3-5, the coastal strata included all addresses in counties that were within 50 miles of the coast.
[2] Matching was by exact address and/or telephone number when available.

matching was only used to determine the sampling rate, matching errors (e.g., not identifying some addresses with licensed anglers due to matching errors) will only impact the efficiency of data collection. This approach was a fairly substantive departure from the dual-frame sampling designs tested in prior pilot studies.

Non-resident anglers were sampled directly from state license databases. The sample frame for each of the targeted states consisted of unique household addresses that were not in the targeted state but had at least one person with a license to fish in the targeted state during the wave. For each state and wave, a simple random sample of addresses was selected.

For both the resident and non-resident samples, a questionnaire was developed to measure fishing activity within the targeted state. Household members that did not fish were asked to indicate that they had no trips. The questionnaire was totally revised from previous pilot studies and required only one step of data collection (previous pilots included two phases of data collection; a household screening phase to identify anglers and a second phase to collect detailed fishing information from anglers). In the new questionnaire, any adult in the household could respond for all household members. The mail survey collected fishing effort data for all household residents, including the number of saltwater fishing trips by fishing mode (shore and private boat), for two-month reference waves, beginning with wave 5, 2012 and continuing through wave 6, 2013. The single phase of data collection was designed to increase the timeliness and the response rates to levels above those observed in the earlier pilots.

The data collection procedures for residents and non-residents were identical. One week prior to the end of each wave, sampled addresses were mailed a survey packet including a questionnaire[3] (Appendix A), a cover letter stating the purpose of the survey, a cash incentive[4] and a business reply envelope. One week after the initial mailing, all households received either an automated telephone reminder call or a postcard reminder, depending on whether or not a telephone number could be matched by a commercial vendor to the sampled address[5]. A final survey packet, excluding the cash incentive, was sent to all nonrespondents three weeks after the initial mailing.

[3] The questionnaire included as Appendix A is the final version of the questionnaire that was tested in the study.
[4] Cash incentives are discussed in more detail below.
[5] All addresses for which a telephone number could be matched received the automated telephone reminder.

Cognitive interviews of both anglers and non-anglers were conducted at the outset of the study to explore respondent reactions to different versions of the survey instrument. The interviews resulted in multiple versions of the questionnaire, which were subsequently tested in an experimental design. In addition to the questionnaire experiments, we tested the impact of different levels of prepaid cash incentives on response rates and survey measures. The design and results of the questionnaire and incentive experiments are described in Appendix B. Based upon the results of the incentive experiment, we included a $2.00 prepaid cash incentive in the initial survey mailing for subsequent waves (Wave 1, 2013 – Wave 6, 2013). The comparisons to the CHTS presented below are for waves 4-6, 2013, after the initial questionnaire and incentive experiments were completed, and are based on the fielding of one version of the questionnaire with the use of the $2 incentive.

4. Findings

This section compares the outcomes from the pilot test of the mail survey design to the outcomes from the production CHTS, which was fielded concurrently in the pilot test states. The first outputs are related to survey quality and the second outputs are survey estimates. Unless otherwise noted, all estimates presented have been weighted. For the CHTS, the survey weights are the regular production weights, and for the mail survey, the weights include the base weights, nonresponse adjustments, and adjustments to control totals of the number of households within each study state.

4.1 Quality Metrics

Overall, the response rate for the mail survey was 40.4% (Table 1). Response rates ranged from 32% in NY to 45.4% in FL. Overall, the mail survey response rate was 2.8 times higher than the CHTS response rate of 14.1% for the same states and waves. The overall response rate for the license sample (nonresident anglers) was 47.5% and ranged from 46.7% in FL to 55.8% in MA.

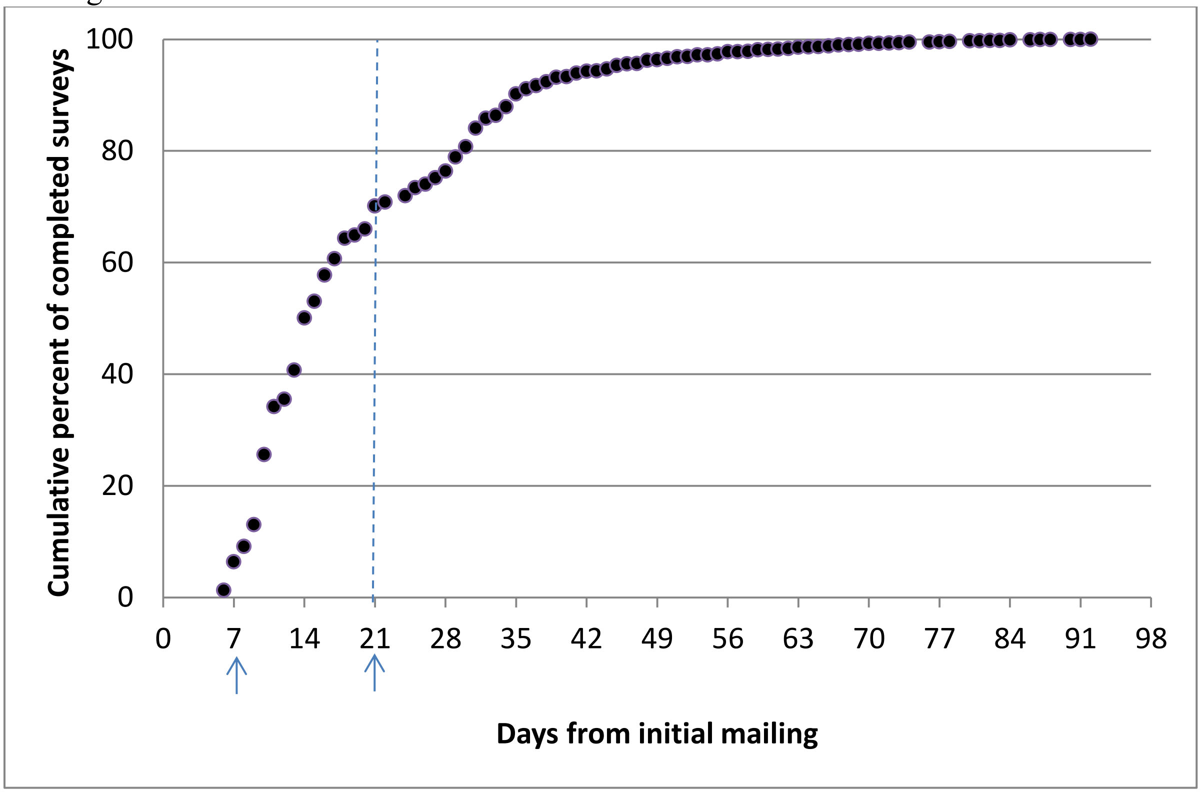
Table 1. Response rates, by state, from the CHTS and mail survey, for coastal counties and waves 4, 5, and 6, 2013.

	Mail		CHTS	
State	%	n	%	n
Florida	45.4	7,460	14.5	2,588,115
Massachusetts	40.6	6,279	13.1	275,967
New York	32.0	4,908	11.6	421,636
North Carolina	41.7	6,203	16.4	332,934
All	40.4	24,850	14.1	3,618,652

Note: American Association for Public Opinion Research Response Rate 3 (AAPOR RR3). Response rate formula excludes ineligible addresses and estimates the proportion of unknown cases that are actually eligible based upon known sample dispositions. Sample sizes reflect the total number of addresses and telephone numbers sampled for the mail survey and CHTS, respectively, regardless of eligibility.

The median response time for the resident mail survey was 14 days. Median response times were consistent among states. Approximately 72% of surveys were returned within 21 days of the initial survey mailing or within two weeks following the conclusion of the reference wave (Figure 1), resulting in a preliminary response rate of approximately 30%. This corresponds with the timing of CHTS data collection, which is conducted during the first two weeks following the end of the reference wave.

Figure 1. Cumulative distribution of mail survey returns from the timing of the initial survey mailing. The dashed vertical line represents the completion of data collection for the CHTS (2 weeks following the end of each wave). The arrows show the timing of the IVR/post-card reminder and mailing of the second questionnaire at 7 and 21 days, respectively, after the initial mailing



To assess the feasibility of generating mail survey estimates within the timeframe for producing CHTS estimates, we compared preliminary estimates of fishing prevalence (percent of household that reported fishing during the reference wave), derived from mail surveys returned within two weeks of the end of the reference wave, to final estimates, derived from complete survey data collected over a 12-week period (Table 2). Overall, the relative difference between preliminary and final estimates of fishing prevalence was approximately 3% (9.7% vs. 10.0%), and there were no significant differences between preliminary and final estimates, overall, at the state level or by fishing mode. These results demonstrate that preliminary estimates are consistent with final estimates, and that a mail survey is a feasible alternative to telephone surveys for producing recreational fishing statistics in a timely manner.

Table 2. Preliminary and final estimated fishing prevalence, by state, from the mail survey, for waves 4, 5, and 6, 2013.

State	Preliminary %	Preliminary SE	Final %	Final SE	*p*-value
Florida	16.4	0.9	16.3	0.7	0.9124
Massachusetts	8.2	0.8	8.2	0.6	0.9630
New York	5.0	0.6	5.5	0.5	0.2123
North Carolina	8.4	0.8	8.7	0.7	0.4799
All	9.7	0.4	10.0	0.4	0.1916

Note: Significance based upon results of a z-test where the standard deviation of the difference was computed taking into account the correlation due to the estimates containing a common subset of observations.

One of the goals of this study was to assess the effectiveness of the design for sampling saltwater anglers, a relatively rare population. Overall, addresses that matched to a license list were more likely than unmatched addresses to both respond to the survey (48.6% vs 34.1%) and report fishing during the reference wave (42.1% vs. 8.1%)[6,7]. These results suggest that matching was effective at defining sub-populations that were distinct with respect to fishing activity. We quantified the benefits of the design by comparing weighted and unweighted estimates of fishing prevalence. Overall, the unweighted estimate (16.0%), which reflects the relative occurrence of fishing households within the sample, was 1.6 times higher than the weighted estimate (10.0%), which reflects fishing activity within the population as a whole. In other words, the design was 1.6 times more likely to result in a survey completed by a fishing household than one would expect from a simple random sample of households. This factor can be further adjusted by changing the subsampling rate for the unmatched households, but this feature of refining the design was not an objective of this feasibility study.

We also calculated the design effect for estimates of fishing prevalence by comparing the estimated sample variance to the variance which would have been obtained from a simple random sample of the same size. For estimates of fishing prevalence, the overall design effect was 0.90, which suggests that the mail survey design can achieve the same precision as simple random sampling (i.e., the same effective sample size) with 10% less sample. A design effect of

[6] The impact of differential response between matched and unmatched households is discussed below.
[7] Response rates and prevalence rates are for both coastal and non-coastal residents.

less than 1.0 indicates that a sample design, including stratification, weighting, non-response adjustment, etc., is more efficient than simple random sampling.

4.2 Estimate Comparisons

While the CHTS is the basis for estimating total fishing effort for all anglers, the data collection of the survey is limited to counties within a specified distance of the coast – the CHTS estimates fishing effort by sampling residents of coastal counties[8]. Consequently, we limit direct comparisons between the CHTS and mail survey estimates to the coastal region. We also explore the impact of CHTS geographic coverage by comparing mail survey estimates to CHTS coverage correction factors. These factors are derived from the Access-Point Angler Intercept Survey (APAIS), an independent dockside survey of completed recreational fishing trips, and are used to expand the CHTS estimates to the full population.

Table 3 compares mail survey and CHTS estimates for several measures of interest. In the coastal counties covered by both surveys, the mail survey estimate of total fishing effort is approximately 4.1 times larger than the CHTS estimate (63,082,000 trips vs. 15,510,000 trips). The direction of differences between CHTS and mail survey estimates of total effort is consistent among states, although the magnitude of the differences varies from a factor of approximately 3.4 in NC to a factor of over 5 in NY. The direction of differences between CHTS and mail survey estimates is also consistent between fishing modes (private boat fishing and shore fishing), although differences are much more pronounced for shore fishing, where the mail estimate is larger than the CHTS estimate by a factor of 6.1 (40,425,000 vs. 6,642,000), than for private boat fishing, where the mail estimate is 2.6 times larger than the CHTS estimate (22,658,000 vs. 8,868,000).

We first examine the differences between CHTS and mail survey estimates of total effort by comparing the components of effort estimates. One component is fishing prevalence, or the percentage of households that reported fishing during a reference wave, and the other component is mean trips per fishing household. Among those households that reported fishing during a

[8] Generally, a coastal county is defined as a county that is within 25 miles of the coast. However, there are exceptions to this definition, including FL where all counties are considered coastal and NC, where the coastal region is expanded to 100 miles during periods of high fishing activity (June-October).

reference wave, CHTS and mail survey estimates of mean trips per household are similar – overall, mail survey estimates of mean trips are larger than CHTS estimates by a factor of 1.2 (11.2 trips vs. 9.0 trips). Estimates are also similar for households that reported fishing in a specific mode. For mean shore trips per household, mail estimates are larger than CHTS estimates by a factor of 1.1 (9.0 trips vs. 8.0 trips), and for mean boat trips per household, CHTS estimates are larger than mail estimates by a factor of 1.1 (8.3 trips vs. 7.7 trips).

In contrast, the mail survey estimate of overall fishing prevalence is 2.7 times larger than the CHTS estimate (12.8% vs. 4.8%). Collectively, these results suggest that households in the mail sample are much more likely to report fishing during a reference wave than households in the CHTS sample, but fishing behavior in the two samples is similar for those households that reported at least one fishing trip.

Consequently, we focus on exploring differences between the two surveys in estimated fishing prevalence – i.e., why do more households report fishing in the mail survey than the CHTS? There are several substantive differences between the CHTS and the mail survey designs that likely contribute to differences in estimated prevalence, notably the sample frames and data collection modes. In the following section, we examine the impact of these design features on survey estimates and describe the impacts in terms of survey error. We also explore the impact of CHTS geographic coverage on estimates of total state fishing effort, as well as the impact of stratification and sampling levels on CHTS estimates.

Table 3. Recreational fishing effort estimates by state, from the mail survey and CHTS, for coastal residents and waves 4, 5 and 6, 2013.

State	Mode and Method of Data Collection	Percent of Households Fishing In Wave	Mean Number of Anglers per Fishing Household	Total Trips	Mean Number of Trips per Household	Total Trips by Shore	Mean Trips by Shore per Household	Total Trips by Boat	Mean Trips by Boat per Household
Florida	ABS	16.31	1.78	39,244	11.3	25,973	9.05	13,271	7.29
	CHTS	6.22	1.78	9,730	9.01	4,042	8.06	5,688	8.17
Massachusetts	ABS	9.2	1.60	5,152	10.27	3,090	8.3	2,062	7.34
	CHTS	3.18	1.56	1,403	9.49	525	8.08	879	9.16
New York	ABS	7.9	1.70	11,784	11.24	6,807	8.99	4,977	9.38
	CHTS	2.4	1.58	2,319	9.66	1,131	9.54	1,188	8.24
North Carolina	ABS	14.48	1.57	6,903	11.38	4,555	9.13	2,348	7.1
	CHTS	6.73	1.78	2,058	8.34	944	6.58	1,114	8.4
All	ABS	12.77	1.73	63,082	11.21	40,425	8.98	22,658	7.65
	CHTS	4.8	1.74	15,510	9.04	6,642	8.03	8,868	8.29

5. Discussion

5.1 Sample Frames

The sample frame for the CHTS is comprised exclusively of landline telephone numbers. The NRC Review (2006) identified the increasing penetration of cell phones and subsequent abandonment of landline telephones as a potential source of bias in the CHTS. Since publication of the NRC report, landline use has continued to decline (Blumberg and Luke 2013). In contrast, the address frame used to sample residents of coastal states in the mail survey design includes all residential addresses serviced by the USPS, providing nearly complete coverage of U.S. households (Iannacchione 2011).

Based upon data collected through the mail survey, we estimate that 26.8% of coastal county households within the study states do not have landline telephone service (wireless-only households)[9] and are excluded from the CHTS sample frame. The percent of wireless households ranged from approximately 20% in MA and NY to approximately 31% in FL and NC. Non-coverage of wireless-only households will result in biased estimates of fishing activity if residents of wireless-only households fish more or less than residents of landline households.

Table 4 shows household fishing prevalence, estimated from mail survey data, by the type of telephone service. Overall, estimated fishing prevalence was 1.3 times higher for wireless-only households than landline households (15.2% vs. 11.9%). Higher fishing prevalence for wireless-only households is consistent, though not necessarily significant, among all states included in the study. These results demonstrate that non-coverage of wireless-only households from the CHTS sample frame is a source of bias resulting in an underestimate of fishing prevalence and total fishing effort.

[9] Addresses that could be matched to a telephone number by a commercial vendor were assumed to have landline telephone service regardless of survey responses to questions about type of household telephone service. This may result in an under-estimate of wireless only households.

Table 4. Estimated fishing prevalence, by state and type of telephone service, from the mail survey, for coastal counties and waves 4, 5, and 6 of 2013.

State	Landline		Wireless Only		
	%	n	%	n	*p*-value
Florida	15.3	1,926	18.4	696	0.0669
Massachusetts	9	1,796	9.2	357	0.9372
New York	7.9	1,045	8.3	217	0.8645
North Carolina	13.4	1,703	16.9	529	0.0809
Overall	11.9	6,470	15.2	1,799	0.0024

Note: Landline includes households that reported having landline telephone service as well as households that could be matched by a commercial vendor to a telephone number, regardless of reported telephone service. Significance based upon the results of a logistic regression model predicting the effect of type of household telephone service on reported fishing activity.

The impact of non-coverage bias in the CHTS is consistent with the direction of observed differences between CHTS and mail survey estimates of prevalence. However, non-coverage of wireless-only households in the CHTS can explain only a portion of the difference. Table 5 compares fishing prevalence for the full address sample within coastal counties, the portion of the address sample that either reported having a landline telephone or could be matched to a landline telephone number – i.e., households that would also be covered by the CHTS, and the CHTS. Comparisons between the full address sample and the "covered" address sample demonstrate the impact on survey estimates of non-coverage bias resulting from the exclusion of wireless-only households – estimated prevalence is approximately 8% higher for the full sample than the "covered" sample. Comparisons between CHTS estimates and the "covered" address sample, which coincide with the same population – households with landline telephone service, demonstrate that mail survey estimates of fishing prevalence are still 2.5 times larger than CHTS estimates (11.9% vs. 4.8%). Residual differences after accounting for non-coverage bias must be attributed to other sources of survey error.

Table 5. Estimated fishing prevalence, by state, from the full mail survey sample, the portion of the mail survey sample that would also be covered by the CHTS (households with landline telephones), and the CHTS, for coastal counties and waves 4, 5, and 6, 2013.

	Full Address Sample		"Covered" Addresses		CHTS	
state	%	SE	%	SE	%	SE
Florida	16.3	0.7	15.3	0.8	6.2	0.2
Massachusetts	9.2	0.7	9	0.8	3.2	0.7
New York	7.9	0.8	7.9	0.9	2.4	0.8
North Carolina	14.5	0.9	13.4	0.9	6.7	0.5
All	12.8	0.4	11.9	0.5	4.8	0.2

5.2 Survey Mode

The choice of survey mode can have different and sometimes substantial impacts on survey estimates. We use mode as a term to cover a diverse set of effects associated with the data collection such as differences in questionnaires and context. Dillman et al. (2009) and de Leeuw (2005) suggest that different data collection modes can result in very different responses to survey questions, particularly when comparing visual vs. aural or interviewer-administered vs. self-administered modes. The amount of time available to provide a response, visual or aural memory cues, and respondent interpretation of survey questions can all contribute to differential measurement between survey modes.

For residents of coastal counties, the largest differences between CHTS and mail survey estimates were for fishing prevalence. This finding is consistent with results from previous studies that measured higher fishing prevalence in mail surveys than telephone surveys (Brick et al. 2012a; Andrews et al. 2013). These studies suggested that differences in screening approaches between telephone and mail survey designs contributed to the observed differences in prevalence. Specifically, differences are partially attributed to a “gatekeeper effect”, where the initial respondent to a household telephone interview, who is asked a series of screener questions to determine if anyone in the household fished during the reference wave, may give inaccurate responses. The gatekeeper hypothesis is based upon the observations that the initial household respondent to the CHTS interview is overwhelmingly female, and households in which a female is the initial respondent are much less likely to report fishing than households in which a male is the initial respondent[10]. This hypothesis suggests a systematic bias in under-reporting of prevalence.

Andrews (unpublished) documented a gatekeeper effect in a telephone survey experiment, where the odds that a household reported fishing during the wave were 37% higher when household-level fishing questions were administered specifically to the sampled angler than when they were administered to the person who initially answered the phone (39.7% prevalence vs. 32.5%)[11]. The magnitude of the effect was likely minimized by the fact that the sample frame used for the study included cell phone numbers, which increased the likelihood that the person who initially answered the phone was also the sampled angler. The impact of the gatekeeper effect may be much larger in a RDD landline telephone survey such as the CHTS. Regardless of the magnitude, a gatekeeper effect in the CHTS is likely to result in underestimates of fishing prevalence, and consequently total fishing effort. The direction of the difference is consistent with the direction of differences between CHTS and mail survey estimates. While not tested, we assume that a gatekeeper effect is less problematic for household mail surveys, where the household has more time to consider the survey request, determine who should respond to the survey, and consult personal records or discuss the survey with other members of the household.

The gatekeeper effect may result from the tasks imposed upon the CHTS respondent. For example, the CHTS contacts households without prior notice, and the initial household respondent is expected to describe household-level fishing activity immediately, without the benefit of memory cues. This may result in cursory cognitive processing and failure to recall past events, particularly if those events are not especially memorable (de Leeuw 2005). As described, the recall error results from the nature of the CHTS interview and should produce under-reporting of household fishing activity at the screener stage. This hypothesis also suggests that the impact of recall error should be more pronounced for shore fishing, which is presumably less memorable than private boat fishing (Andrews et al. 2013). Lower salience of shore fishing could impact reporting at both the screener phase – e.g., households with only shore anglers may

[10] For example, during a recent CHTS wave, 62% of initial respondents were female, of which 3.3% reported household fishing during the wave. In contrast, 10.9% of male respondents reported household fishing activity.
[11] Estimated odds ratio of 1.37 (1.167,1.609) resulting from logistic regression model predicting the effect of screener respondent on reported fishing activity.

be more susceptible to the gatekeeper effect – and the topical phase – e.g., active anglers may be more likely to recall and describe boat fishing trips than shore fishing trips.

The impact of recall error and under-reporting of shore fishing trips at the topical phase may be exacerbated by the nature of the CHTS interview. Specifically, the CHTS interview consists of a series of household-level screening questions to identify fishing households, followed by individual interviews with each active angler to first estimate the total number of fishing trips taken by each angler and then sequentially characterize each individual fishing trip. In an experiment to assess recall error in the CHTS, Mathiowetz and Andrews (paper read at the Annual Meeting for the American Fisheries Society, 2011) observed that anglers provided details, including fishing mode, for fewer than 60% of reported trips, and that the percentage of estimated trips that are profiled decreases dramatically as the number of trips increases[12,13,14]. Given the financial and time commitments required for boat fishing, we hypothesize that anglers are more likely to recall and report details for boat fishing trips, resulting in under-representation of shore fishing activity in the CHTS data relative to boat fishing. This hypothesis is supported by the fact that differences between mail and CHTS estimates are considerably larger for shore fishing than private boat fishing (Table 3).

An alternative explanation for differential measurement between the CHTS and mail survey may be related to respondent interpretation and understanding of survey questions. Cognitive interviewing initiated prior to implementation of the mail survey demonstrated that anglers were very eager to provide information about fishing activity, even when that information was inconsistent with the questions being asked. For example, participants in cognitive interviews described fishing activity that occurred prior to the reference wave, outside of the reference state or in freshwater. The questionnaire was designed and modified to minimize reporting of out-of-scope fishing activity, and follow-up testing of different questionnaire versions suggests that these modifications were at least partially successful[15]. However, it is likely that some residual

[12] Reasons for incomplete trip profiling include mid-interview refusals, an inability to remember trip details, and volunteered reports that all trips are the same.
[13] 93% of reported trips were profiled for anglers who initially reported a single trip, while only 47% of trips were profiled for anglers who reported 5 trips.
[14] The CHTS compensates for incomplete trip information through a hot deck imputation process in which trip details for missing trips are imputed from a donor dataset comprised of complete trip records.
[15] mail survey estimates of fishing prevalence were lower in questionnaire versions that highlighted the scope of the survey request and/or provided space for respondents to document trips that were prior to the reference wave.

reporting error continues. This type of reporting error may be less likely in the CHTS, where the interviewer can confirm trip details. Reporting error resulting from misinterpretation of mail survey questions may contribute to differences between CHTS and mail survey estimates. A follow-up study, in which mail survey respondents will be re-interviewed via telephone, will be implemented during the spring of 2014 to assess the level that reported information is within the scope of the survey.

5.3 Nonresponse

In addition to impacting measurement, different survey modes may result in very different response rates. For example, mail survey response rates in the present study were nearly 3 times higher than CHTS response rates. While nonresponse rate is a poor predictor of nonresponse bias (Groves 2006), a higher nonresponse rate increases the risk for nonresponse bias. Consequently, the risk of nonresponse bias is higher in the CHTS than the mail survey design.

Nonresponse will result in bias if respondents and nonrespondents are different with respect to what is being measured. Previous mail surveys of anglers (Andrews et al. 2010, 2013; Brick et al. 2012a) have demonstrated that households with licensed anglers are both more likely to respond to a mail survey about fishing and more likely to report fishing activity during the reference period than households without licensed anglers. We observed similar results in the present study. Failure to account for this differential response between households with and without licensed anglers will result in nonresponse bias. By matching address samples to state license databases in the mail survey design, we effectively stratify the sample into sub-populations that are more similar with respect to fishing activity and response propensity than the sample as a whole. This formation of strata mitigates the impact of differential response between the two groups. Consequently, any nonresponse bias in the mail survey design will be residual after accounting for the population of licensed anglers.

The CHTS is also susceptible to nonresponse bias resulting from differential response between anglers and non-anglers. W.R. Andrews (paper read at the Annual Meeting for the American Fisheries Society, 2011) demonstrated that differential response between households with and without anglers resulted in an overestimation of fishing effort by as much as 17% in the CHTS.

Unlike the mail survey design, the CHTS does not account for differential response between sub-populations, resulting in nonresponse bias. However, the bias does not explain differences between CHTS and mail survey estimates, as it results in an overestimate of fishing effort in the CHTS.

We attempted to assess nonresponse bias in the mail survey design by conducting a nonresponse follow-up study. Each wave, a sample of 320 nonresponding addresses[16] was randomly selected and mailed a follow-up questionnaire[17]. The survey mailing, which resulted in a response rate of approximately 40%, was delivered via FedEX and included a $5.00 cash incentive. Table 6 compares fishing prevalence for the initial address samples and the follow-up study samples. Overall, estimates of fishing prevalence for the initial sample are approximately 1.1 times larger than estimates from the nonresponse sample (13.9% vs. 12.7%)[18]. There are no systematic differences between initial sample estimates and nonresponse sample estimates among states. Based upon these results, we have no evidence to suggest that nonresponse in the mail survey design results in nonresponse bias.

The combined mail survey response rate, including both the initial sample and the nonresponse follow-up sample, is approximately 64% (40% for the initial sample and 40% for the nonresponse follow-up sample). While we have not observed nonresponse bias in the mail survey, we can estimate the maximum possible nonresponse bias if we assume that all nonrespondents are non-anglers. In this scenario, the estimated prevalence is 7.76%, which corresponds to a maximum bias of approximately 5 percentage points (12.77% vs. 7.76%). This is not trivial (approximate 40% relative difference) considering the relatively low overall magnitude of fishing prevalence. However, even in this extreme case, the estimated prevalence for the mail survey is still 1.6 times larger than the CHTS estimate (7.76% vs. 4.8%), which suggests that factors other than nonresponse bias must contribute to the differences between CHTS and mail survey estimates.

[16] Nonresponse samples were distributed equally among states (80 addresses per state and wave).
[17] The questionnaire used for the nonresponse study was identical to questionnaire included in the initial mailings.
[18] The Full Sample estimates are the fully weighted estimates used in the rest of this section. The Nonresponse sample estimates are based on weights that account for the original sampling and for subsampling for the nonresponse bias study.

Table 6. Estimated fishing prevalence for the full mail survey sample and the nonresponse follow-up sample by state.

	Full Sample		Nonresponse Sample	
State	%	n	%	n
Florida	21.5	11,767	18.4	203
Massachusetts	11.0	11,094	13.2	216
New York	8.6	8,479	9.2	172
North Carolina	11.4	13,570	9.8	248
All	13.9	49,910	12.7	839

Note: Estimates are based upon data collected from 7 waves (wave 5, 2012-wave 5, 2013) and include information collected through multiple versions of the survey instrument. Consequently, estimates may differ from those reported elsewhere in the report.

Based upon the results of this and previous studies, we suspect that differential bias resulting from measurement errors contributes significantly to the observed differences between CHTS and mail survey estimates. While nonresponse is a concern, particularly for the CHTS, we do not believe that bias resulting from nonresponse contributes to the observed differences in estimates between survey designs.

5.4 Stratification and Sample Size

The previous sections explored potential impacts on survey estimates of non-sampling errors – coverage error, measurement error and nonresponse error - resulting from survey design features. We also considered the extent to which sample design and estimation strategies may impact survey estimates.

Within each coastal state, the CHTS is stratified by county, and the sample is allocated among counties in proportion to the square root of the number of occupied housing units within each county. While this strategy assures that sample is distributed among all coastal counties within a state, it also results in small sample sizes in some counties during some survey waves. Because recreational saltwater fishing is a relatively rare occurrence among the general population (<10%), small sample sizes can result in situations in which the likelihood of contacting at least one fishing household is extremely small. This is especially true during off-peak waves when fishing activity is particularly low (<1-2%). Because CHTS estimates are produced at the stratum level (i.e., county) and then aggregated to state estimates, we hypothesized that low sample sizes in the CHTS during low-activity waves result in a systematic underestimate of state-level fishing effort.

We tested this hypothesis by comparing base CHTS estimates to independent estimates derived from the CHTS methodology but with much larger samples in New York and North Carolina during wave 6, 2013, an historically low-activity fishing period. Table 7 provides results for the base and experimental CHTS samples. Overall, base sampling levels resulted in 10 counties with no reported fishing activity, while only a single county was classified as non-fishing at the larger, experimental sample sizes. Similarly, the experimental estimate of fishing prevalence was 13.6% larger than the base estimate, and experimental estimates were more than 10% greater than base estimates in both New York and North Carolina. While differences in estimated prevalence between base and experimental sample sizes are not significant, they are in the direction that supports the hypothesis as well as the suggestion that differences between mail survey and CHTS estimates may be partially the result of insufficient sampling levels to support the stratification and estimation design of the CHTS.

The CHTS estimation design – stratified random sampling with separate ratio estimates – is unbiased when sample sizes in each stratum are large (Cochran 1953). However, in practice, sample sizes in some strata may be insufficient to produce unbiased state-level estimates of fishing activity. A combined ratio estimate may be more appropriate when stratum sample sizes are small. In addition, county-level stratification and low fishing prevalence result in very high probabilities of not encountering a single fishing household at the sample sizes allocated to some counties. For rare populations, such as fishing households, relatively small random samples are likely to result in a distribution of estimates that is highly skewed with zero occurrences of the rare event (Christman 2009).

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Table 7. Comparison of survey results between base CHTS and experimental sampling levels by state for wave 6, 2013.

	Base Sample				Experimental Sample			
State	n	Avg. County n	No Fish County	Prevalence (%)	n	Avg. County n	No Fish County	Prevalence (%)
New York	920	92	4	1.26	4,299	430.1	0	1.45
North Carolina	1,578	43.9	6	5.86	3,994	111	1	6.52
All	2,498	68	10	1.98	8,293	270.6	1	2.25

Note: No Fish County is the number of counties in which no fishing households were contacted, and Avg. County n is the average sample size per county.

5.5 Geographic Coverage

Geographic coverage of the CHTS is limited to counties that are within a specified distance of the coast. This is done to maximize interviews with anglers and minimize data collection costs, as fishing activity is generally assumed to be more common for residents of coastal counties than noncoastal counties. To account for geographic non-coverage, CHTS estimates of coastal resident fishing effort are expanded by correction factors derived from the Access-Point Angler Intercept Survey (APAIS), an onsite survey of completed recreational fishing trips conducted at publicly accessible fishing or access sites (e.g. fishing piers, beach access sites, boat ramps, marinas, etc.)[19]. These correction factors attempt to account for fishing trips taken by residents of non-coastal counties within coastal states, as well as residents of non-coastal states. The correction factor has its own problems, especially since the sample frame for the APAIS is limited to publicly accessible sites. Estimates derived from the APAIS, including the residency correction factors, are susceptible to bias resulting from non-coverage of fishing trips taken at or returning to non-public sites.

In contrast to the CHTS, the sample frame for the mail survey includes all residential addresses within coastal states, so we assume that non-coverage bias for residents of coastal states is minimal (Iannacchione 2011). Non-resident anglers in the mail survey design are sampled exclusively from state database of licensed saltwater anglers, which are potentially susceptible to non-coverage resulting from license exemptions and unlicensed fishing activity among non-resident anglers.

[19] Within each state, CHTS estimates are expanded by the ratio of completed angler intercepts to completed angler intercepts with residents of coastal counties.

We examined the impact of geographic exclusions to the CHTS by comparing APAIS correction factors to analogous estimates derived from the mail survey (Table 8). Overall, the APAIS estimates that 76% of saltwater fishing trips in the study states are taken by residents of coastal counties who are "covered" by the CHTS, resulting in a correction factor ("Coastal ratio") of 1.3 (1.0/0.76). In contrast to the APAIS, the mail survey estimates that 88% of saltwater fishing trips in the study states are taken by residents of coastal counties, which corresponds to a coastal ratio of 1.14. Differences between APAIS and mail survey estimates in the relative distribution of effort by residency are highly variable among states – APAIS coverage correction factors are larger than analogous mail survey estimates in Florida and Massachusetts and smaller in New York and North Carolina.

Table 8. Percent of total saltwater fishing trips by residents of coastal counties and the ratio of total effort, including coastal, non-coastal and non-resident anglers, to coastal resident effort.

	Single Phase Mail			APAIS		
State	n	% Coastal	Coastal Ratio	n	% Coastal	Coastal Ratio
Florida	2,829	96.5	1.04	9,759	76.9	1.30
Massachusetts	2,684	87.5	1.14	3,203	75.6	1.32
New York	2,146	83.4	1.20	1,494	95.7	1.04
North Carolina	3,058	61.4	1.63	8,260	62.5	1.60
All	10,717	87.7	1.14	22,716	76.3	1.31

Note: Sample sizes reflect the combined number of completed surveys for the sample of resident addresses and the sample of non-resident licensees. The coastal ratio is the ratio of total angler trips to trips by residents of coastal counties. Coastal ratios derived from the APAIS are used to expand CHTS estimates to account for trips by non-resident anglers and residents of non-coastal states.

We further examine differences between APAIS correction factors and mail survey estimates separately for residents and non-residents of coastal states. Of saltwater fishing trips taken by residents of coastal states, the mail survey estimates that approximately 78% are by residents of coastal counties (Table 9). Among states, effort by coastal county residents varies from 64.3% in NC to 89.2% in MA. In contrast, the APAIS estimates that nearly 90% of trips are taken by coastal county residents; coastal residents accounted for 91.2%, 99.3% and 78.9% of total resident effort for MA, NY and NC, respectively. Assuming that other potential sources of bias in the mail design are uniform between coastal and non-coastal residences, these results suggest that the APAIS underestimates fishing activity by residents of non-coastal counties. This would result in an underestimate of total fishing effort. The magnitude of bias varies by state; APAIS samples provide a reasonable representation of anglers in MA along this dimension, but under-represent non-coastal residents in NY and NC.

The mechanism for this bias is not intuitive. Because the APAIS sample frame is limited to publicly accessible fishing sites, one may expect the sample to over-represent trips by residents of non-coastal counties, whose primary access to saltwater fishing is from public-access sites[20]. An alternative explanation for the difference between the mail survey and APAIS in the distribution of resident fishing effort is that mail survey respondents may be including in their counts fishing activities that are outside the scope of the survey, such as freshwater fishing. The distinction between saltwater and freshwater fishing can be subtle, particularly in inland water bodies such as estuaries and the brackish portions of rivers. New York provides an example of how difficult it can be to distinguish between fresh and saltwater fishing. New York anglers are required to register as saltwater anglers if fishing for saltwater species in marine or coastal regions of the state or for "migratory fish of the sea" in the tidal Hudson River and its tributaries[21] (http://www.dec.ny.gov/permits/54950.html). The tidal portion of the Hudson River extends to north of Albany, which is more than 100 miles beyond the most upstream fishing site on the APAIS sample frame. While fishing on much of the Hudson River does not qualify as saltwater fishing by the APAIS definition, anglers who fish on the Hudson River may report these trips a saltwater because they are required to register as saltwater anglers and they're fishing for saltwater species[22]. The reporting of fishing activities on water bodies such as the Hudson River, which extends well into the noncoastal portion of the state, could skew the distribution of effort toward noncoastal residents and explain differences between the mail survey and APAIS in the distribution of effort among types of residence.

[20] In contrast to coastal county residents who may have direct access to saltwater fishing via personal or community beaches, docks and/or boat slips that are inaccessible to APAIS interviewers.
[21] New York does not have a saltwater fishing license but does require saltwater anglers to enroll in a free registry.
[22] Anecdotal evidence collected during follow-up telephone interviews suggests that some anglers distinguish between salt and freshwater fishing based upon the species targeted, not the geographic location.

Table 9. Percent of total resident fishing trips by residents of coastal counties, estimated by the mail survey and the APAIS[23].

	Single Phase Mail		APAIS	
State	% Coastal	n	% Coastal	n
Massachusetts	89.2	2,629	91.2	3,203
New York	83.9	1,973	99.3	1,494
North Carolina	64.3	2,876	78.9	8,260
All	78.0	7,966	89.2	12,957

Table 10 shows the estimated percentage of total trips taken by non-resident anglers for the mail survey and the APAIS. Overall, the APAIS estimates that 19.8% of fishing trips in the study states are taken by non-resident anglers. In contrast, the mail design estimates that only 2.9% of trips are by non-resident anglers. These results suggest that either the license frames used to sample non-resident anglers are incomplete (i.e. many non-resident anglers fish without a license), or APAIS samples over-represent non-resident anglers. Both explanations are plausible, if not likely. For example, previous studies (Brick et al. 2012; Andrews et al. 2013) suggested that state license databases are incomplete as the result of license exclusions and illegal fishing activity. It is not clear if these omissions are as serious for non-residents and they are for residents. These sources of non-coverage will result in underestimation of total fishing effort for non-resident anglers. Similarly, the APAIS sample frame excludes private residences (e.g., private docks and boat slips, private marinas, etc.), which are likely to have a much higher proportion of resident anglers than public-access fishing sites. Over-representation of non-resident anglers in the APAIS will result in over-estimation of total fishing effort.

[23] Florida is excluded from the table because all counties are considered coastal and are included in the coverage of the CHTS.

Table 10. Percent of total fishing trips by non-resident anglers, estimated by the mail survey and the APAIS.

	Single Phase Mail		APAIS	
State	% Non-resident	n	% Non-resident	n
Florida	3.5	2,829	23.1	9,759
Massachusetts	1.9	2,684	17.1	3,203
New York	0.7	2,146	3.6	1,494
North Carolina	4.5	3,058	20.8	8,260
All	2.9	10,717	19.8	22,716

The consequences of limiting the CHTS to coastal counties are still somewhat unclear. We expect non-sampling errors in the mail design to be relatively uniform between coastal and non-coastal residences within a state, suggesting that estimates of the distribution of effort between coastal and non-coastal residents are unbiased. This implies that APAIS samples over-represent trips by coastal resident anglers, resulting in under-estimates of fishing effort. The impact of non-resident angling is less clear as both APAIS and mail survey estimates are susceptible to non-coverage bias – non-coverage of private access fishing sites in the APAIS and unlicensed anglers in the mail survey. Regardless of the source of differences, the APAIS attributes a larger proportion of total effort to non-resident anglers, resulting in larger correction factors and larger estimates of total fishing effort. The overall net differences between the APAIS and mail survey in the estimated distribution of effort by residency are variable among states, likely reflecting differences in the coverage of both state license databases and APAIS sample frames.

6. Conclusions and Recommendations

The mail survey design tested in this study is a feasible alternative to the CHTS and has numerous substantive advantages over the CHTS design. Overall, response rates for the mail survey were 2-3 times higher than the CHTS, and the design produced stable preliminary estimates within the current data collection and estimation schedule for the CHTS. Furthermore, matching household address samples to state license databases and over-sampling matched households effectively increased the likelihood of contacting fishing households.

In terms of survey error, we conclude that the mail survey design is less susceptible than the CHTS to bias resulting from nonresponse and non-coverage. We also found that the nature of the mail survey mode results in more accurate responses to questions about fishing activity than the CHTS, which expects respondents to answer questions on-the-spot, without the benefit of aided recall or memory cues. Furthermore, we have demonstrated that insufficient sampling in the CHTS in conjunction with the estimation scheme creates a functional bias that results in underestimates fishing activity. Table 11 summarizes sources of survey error, as well as the observed and/or hypothesized impact of bias on survey estimates for the CHTS and mail survey design.

Table 11. Summary of sources of error in the CHTS and mail survey designs.

Error Source	Direction of Bias Mail	Direction of Bias CHTS	Comment
Non-Coverage	NA	↓	Results from mail survey demonstrate that residents of wireless-only households are more likely to fish than residents of landline households.
Nonresponse	↑	↑	Based upon response rates, the risk for nonresponse bias is greater in the CHTS than the mail survey. Differential response between households with and without licensed anglers is mitigated in mail survey by treating populations as separate strata - there is no such adjustment in the CHTS. A nonresponse follow-up study did not identify nonresponse bias in the mail survey design. However, any nonresponse bias in the mail survey design is likely to result in an over-estimate of fishing effort.
Measurement	↑	↓	A "gatekeeper effect", resulting in under-reporting of household fishing activity, has been documented in telephone surveys of licensed anglers. We suggest that this source of measurement bias is greater in landline RDD telephone surveys. We also suggest that the mail mode facilitates recall of past fishing activity. The lack of interviewers in the mail survey may result in reports of fishing activity that are beyond the intended scope of the mail survey.
Sample Size	NA	↓	County-level stratification in the CHTS results in insufficient sample size to detect fishing activity in some strata during low-activity waves. This source of error would also impact the mail survey at small sample sizes.

In addition to direct comparisons between the CHTS and the mail survey in the geographic regions where the survey overlapped, we also explored the impact of geographic limitations of the CHTS on total effort estimates and determined that coverage correction factors, derived from the APAIS, are likely biased due to the exclusion of private access fishing sites from APAIS sample frames. Comparisons between the APAIS and mail survey of the distribution of effort between coastal and non-coastal resident anglers suggest that the APAIS sample over-represents trips by coastal resident anglers, which would result in under-estimates of total resident fishing effort. Comparisons between the two designs of the magnitude of non-resident angling are less clear and confounded by potential coverage bias in the mail survey resulting from unlicensed fishing activity by non-resident anglers.

Given the potential for bias in the CHTS, we conclude that the mail survey design is a superior approach for monitoring recreational fishing effort. Other designs, including dual-frame telephone surveys that sample from both landline and cell phone frames, were also considered as alternatives to the CHTS. However, these designs were not tested due to the expected low response rates, prohibitive costs, and the need to target anglers within specific geographic regions (AAPOR Cell Phone Task Force 2010).

The mail survey design described above also improved upon weaknesses identified in previous tests of mail surveys. For example, the response rate for the new design was considerably higher than previous mail surveys largely because it eliminated the screening mail instrument. The new design also eliminated the potential bias due to matching errors in the earlier dual-frame designs.

We believe the results reported here demonstrate the utility of the mail survey design. Nonetheless, we encourage continued development and testing. For example, additional questionnaire testing and varying the length of the reference period (e.g., one-month waves) could provide additional assessments of measurement errors. Similarly, testing alternative data collection modes, such as email and web surveys, could improve response rates and potentially provide cost savings. These types of evaluations will help ensure that advancements in survey methodology are considered and customer needs are satisfied.

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Appendix A – Single Phase Mail Survey Questionnaire

Please complete for all members of your household. Include those who fish and those who do not fish.

Household Member 4

11. What is this person's gender?

☐ Male
☐ Female

12. How old is this person?
If less than 1 year, mark 0 years.

☐☐ Age in years

13. Is this person of Hispanic, Latino, or Spanish origin?

☐ Yes, of Hispanic origin
☐ No, not of Hispanic origin

14. What is this person's race?
Mark one or more boxes.

☐ White ☐ Black, African American
☐ Asian ☐ American Indian or Alaska Native
☐ Native Hawaiian or Other Pacific Islander

Please think only about recreational saltwater fishing in <Merged State>.

15a. How many days did this person go recreational saltwater fishing from the shore in <Merged State>? The shore includes docks, bridges, causeways, beaches, banks, or any other shore-based place or area.

☐ No shore recreational saltwater fishing in last 12 months → Go to question 15b

☐☐ Number of days in July and August of 2013
☐☐ Total number of days in last 12 months

15b. How many days did this person go recreational saltwater fishing from a private or rental boat that returned to shore in <Merged State>? Do not include charter boats or commercial boats that have a captain or crew who help locate and catch fish.

☐ No private boat recreational saltwater fishing in last 12 months → Go to the next Household Member

☐☐ Number of days in July and August of 2013
☐☐ Total number of days in last 12 months

Go to Household Member 5

Household Member 5

11. What is this person's gender?

☐ Male
☐ Female

12. How old is this person?
If less than 1 year, mark 0 years.

☐☐ Age in years

13. Is this person of Hispanic, Latino, or Spanish origin?

☐ Yes, of Hispanic origin
☐ No, not of Hispanic origin

14. What is this person's race?
Mark one or more boxes.

☐ White ☐ Black, African American
☐ Asian ☐ American Indian or Alaska Native
☐ Native Hawaiian or Other Pacific Islander

Please think only about recreational saltwater fishing in <Merged State>.

15a. How many days did this person go recreational saltwater fishing from the shore in <Merged State>? The shore includes docks, bridges, causeways, beaches, banks, or any other shore-based place or area.

☐ No shore recreational saltwater fishing in last 12 months → Go to question 15b

☐☐ Number of days in July and August of 2013
☐☐ Total number of days in last 12 months

15b. How many days did this person go recreational saltwater fishing from a private or rental boat that returned to shore in <Merged State>? Do not include charter boats or commercial boats that have a captain or crew who help locate and catch fish.

☐ No private boat recreational saltwater fishing in last 12 months → Thank you!

☐☐ Number of days in July and August of 2013
☐☐ Total number of days in last 12 months

Thank you!

18989

OMB #0648-0652
Exp. Date 10/31/2015

<Merged State> Weather and Outdoor Activity Survey

Public reporting burden for this collection of information is estimated to average 10 minutes per response, including the time for reviewing instructions, searching existing data sources, gathering and maintaining the data needed, and completing and reviewing the collection of information. Send comments regarding this burden estimate or any other suggestions for reducing this burden to Rob Andrews, NOAA Fisheries Service, 1315 East-West Hwy., Silver Spring, MD 20910.

This is a voluntary survey, and responses are kept confidential as required by section 402(b) of the Magnuson-Stevens Act and NOAA Administrative Order 216-100, Confidentiality of Fisheries Statistics, and will not be released for public use except in aggregate statistical form without identification as to its source. Notwithstanding any other provisions of the law, no person is required to respond to, nor shall any person be subjected to a penalty for failure to comply with, a collection of information subject to the requirements of the Paperwork Reduction Act, unless that collection of information displays a currently valid OMB Control Number.

<Merged State> Weather and Outdoor Activity Survey

START HERE

- Please use a black or blue pen to complete this form.
- Mark ☒ to indicate your answer.
- If you want to change your answer, darken the box ■ and mark the correct answer.

1. How do members of this household obtain information about the weather, including current weather conditions, forecasts, and warnings? *Mark all that apply.*
   - ☐ Television
   - ☐ Radio
   - ☐ Newspaper
   - ☐ Internet
   - ☐ Other
2. During the past 12 months, has anyone in this household had to evacuate or seek shelter due to a severe weather event such as a tornado, hurricane, or thunderstorm?
   ☐ Yes ☐ No
3. In your area, how often do the advanced warnings you get for severe weather events allow you enough time to prepare properly?
   - ☐ All the time
   - ☐ Some of the time
   - ☐ Rarely
   - ☐ Never
4. During the past 12 months, has anyone in this household visited a public beach, national seashore, coastal state park, or other coastal nature reserve or protected area?
   ☐ Yes ☐ No
5. During the past 12 months, has anyone in this household been backpacking, climbing, or hiking?
   ☐ Yes ☐ No
6. During the past 12 months, has anyone in this household been hunting?
   ☐ Yes ☐ No
7. Which category best describes the telephone service for you and members of the household?
   - ☐ Regular or Landline phone only
   - ☐ Cellular phone only
   - ☐ Both Landline and Cellular phone
   - ☐ No working phone service
8. Which of the following best describes this house, apartment, or mobile home?
   - ☐ Owned with a mortgage or loan
   - ☐ Owned (without a mortgage)
   - ☐ Rented
   - ☐ Occupied without payment or rent
9. How long have you lived at this address?
   - ☐ 1 year or less
   - ☐ Less than 5 years, more than 1 year
   - ☐ 5 years or more
10. How many people, including all adults and children, live in this household?
    ☐☐ → Please continue and complete the questions in the next section for EACH HOUSEHOLD MEMBER, whether or not that household member saltwater fishes.

S	M	T	W	T	F	S
	1	2	3	4	5	6
7	8	9	10	11	12	13
14	15	16	17	18	19	20
21	22	23	24	25	26	27
28	29	30	31			

Please use the calendars to answer question 15.

S	M	T	W	T	F	S
				1	2	3
4	5	6	7	8	9	10
11	12	13	14	15	16	17
18	19	20	21	22	23	24
25	26	27	28	29	30	31

Please complete for all members of your household. Include those who fish and those who do not fish.

Household Member 1

11. What is this person's gender?
    - ☐ Male
    - ☐ Female
12. How old is this person?
    If less than 1 year, mark 0 years.
    ☐☐ Age in years
13. Is this person of Hispanic, Latino, or Spanish origin?
    - ☐ Yes, of Hispanic origin
    - ☐ No, not of Hispanic origin
14. What is this person's race?
    Mark one or more boxes.
    - ☐ White ☐ Black, African American
    - ☐ Asian ☐ American Indian or Alaska Native
    - ☐ Native Hawaiian or Other Pacific Islander

Please think only about recreational saltwater fishing in <Merged State>.

15a. How many days did this person go recreational saltwater fishing from the shore in <u><Merged State></u>? The shore includes docks, bridges, causeways, beaches, banks, or any other shore-based place or area.
- ☐ No shore recreational saltwater fishing in last 12 months → Go to question 15b
- ☐☐ Number of days in July and August of 2013
- ☐☐ Total number of days in last 12 months

15b. How many days did this person go recreational saltwater fishing from a private or rental boat that returned to shore in <u><Merged State></u>? Do not include charter boats or commercial boats that have a captain or crew who help locate and catch fish.
- ☐ No private boat recreational saltwater fishing in last 12 months → Go to the next Household Member
- ☐☐ Number of days in July and August of 2013
- ☐☐ Total number of days in last 12 months

Go to Household Member 2

Household Member 2

11. What is this person's gender?
    - ☐ Male
    - ☐ Female
12. How old is this person?
    If less than 1 year, mark 0 years.
    ☐☐ Age in years
13. Is this person of Hispanic, Latino, or Spanish origin?
    - ☐ Yes, of Hispanic origin
    - ☐ No, not of Hispanic origin
14. What is this person's race?
    Mark one or more boxes.
    - ☐ White ☐ Black, African American
    - ☐ Asian ☐ American Indian or Alaska Native
    - ☐ Native Hawaiian or Other Pacific Islander

Please think only about recreational saltwater fishing in <Merged State>.

15a. How many days did this person go recreational saltwater fishing from the shore in <u><Merged State></u>? The shore includes docks, bridges, causeways, beaches, banks, or any other shore-based place or area.
- ☐ No shore recreational saltwater fishing in last 12 months → Go to question 15b
- ☐☐ Number of days in July and August of 2013
- ☐☐ Total number of days in last 12 months

15b. How many days did this person go recreational saltwater fishing from a private or rental boat that returned to shore in <u><Merged State></u>? Do not include charter boats or commercial boats that have a captain or crew who help locate and catch fish.
- ☐ No private boat recreational saltwater fishing in last 12 months → Go to the next Household Member
- ☐☐ Number of days in July and August of 2013
- ☐☐ Total number of days in last 12 months

Go to Household Member 3

Household Member 3

11. What is this person's gender?
    - ☐ Male
    - ☐ Female
12. How old is this person?
    If less than 1 year, mark 0 years.
    ☐☐ Age in years
13. Is this person of Hispanic, Latino, or Spanish origin?
    - ☐ Yes, of Hispanic origin
    - ☐ No, not of Hispanic origin
14. What is this person's race?
    Mark one or more boxes.
    - ☐ White ☐ Black, African American
    - ☐ Asian ☐ American Indian or Alaska Native
    - ☐ Native Hawaiian or Other Pacific Islander

Please think only about recreational saltwater fishing in <Merged State>.

15a. How many days did this person go recreational saltwater fishing from the shore in <u><Merged State></u>? The shore includes docks, bridges, causeways, beaches, banks, or any other shore-based place or area.
- ☐ No shore recreational saltwater fishing in last 12 months → Go to question 15b
- ☐☐ Number of days in July and August of 2013
- ☐☐ Total number of days in last 12 months

15b. How many days did this person go recreational saltwater fishing from a private or rental boat that returned to shore in <u><Merged State></u>? Do not include charter boats or commercial boats that have a captain or crew who help locate and catch fish.
- ☐ No private boat recreational saltwater fishing in last 12 months → Go to the next Household Member
- ☐☐ Number of days in July and August of 2013
- ☐☐ Total number of days in last 12 months

Go to Household Member 4 on the next page

27138

Please use other side for additional household members

A mail survey design was implemented in Massachusetts, New York, North Carolina and Florida in October, 2012 to test a revised data collection design for monitoring marine recreational fishing effort. The survey, which collects information for two-month reference waves, included two experiments during the first two study waves, wave 5 (Sept-Oct 2012) and wave 6 (Nov-Dec, 2012), to test different survey design features aimed at maximizing efficiency and minimizing nonresponse error. Specifically, the experiments tested two versions of the survey instrument and four levels of cash incentives. Details of the experiments are provided below.

Instrument Testing

The study included an experiment to test two versions of the survey instrument. The objective of the experiment was to identify the instrument that maximized overall response rates while minimizing the potential for nonresponse bias resulting from differential nonresponse between anglers and non-anglers. One version of the instrument (Saltwater Fishing Survey) utilized a "screen out" approach that quickly identifies anglers (and non-anglers) and encourages participation by minimizing the number of survey questions, particularly for non-anglers. Person-level information, including details about recent fishing activity and limited demographic information, is collected for all household residents, but only if someone in the household reported fishing during the reference wave. The second version (Weather and Outdoor Activity Survey) utilized an "engaging" approach that encourages response by broadening the scope of the questions to include both fishing and non-fishing questions. This version collects person-level information for all residents of sampled households, regardless of whether or not household residents participated in saltwater fishing. Each wave, sampled addresses were randomly assigned to one of the two questionnaire types, which were evaluated in terms of response rates and reported fishing activity.

Table 1 provides the weighted response rates (AAPOR RR1 after excluding undeliverable addresses) and estimated fishing prevalence (percentage of households with residents who reported fishing during the wave) for the two versions of the instrument. Overall, the Weather and Outdoor Activity Survey achieved a significantly higher response rate than the Saltwater Fishing Survey, and there was no significant difference between instruments in estimated prevalence. The lack of a significant difference between instruments for estimated prevalence suggests that the gain in response for the engaging instrument cannot be attributed to increased survey participation by either anglers or non-anglers, but that both groups are more likely to respond to the Weather and Outdoor Activity Survey than the Saltwater Fishing Survey.

We also compared response rates and prevalence between instruments both among and within subpopulations defined by whether or not sampled addresses could be matched to state databases of licensed saltwater anglers – subpopulations expected to distinguish between households with anglers and households with no anglers or less avid anglers. As expected, both response rates and estimated prevalence were higher in the matched subpopulation than the unmatched subpopulation, confirming that a population expected to be interested in the survey topic - households with licensed anglers - is more likely to respond to a fishing survey and report fishing activity than a population that excludes licensed anglers[1]. Because we can identify household license status prior to data collection, we can account for differential nonresponse between matched and unmatched households in the estimation design by treating matched an unmatched domains as strata (Lohr 2009).

[1] The classification of sample into domains is dependent upon matching ABS sample to license databases by address and telephone number. This process is unlikely to be 100% accurate, so the unmatched domain is likely to include some households with licensed anglers. The unmatched domain also includes households with residents who fish without a license.

Table 1. Weighted response rates and estimated prevalence overall and by domain for two versions of the survey instrument.

	Saltwater Fishing Survey		Weather and Outdoor Activity Survey	
	(%)	(n)	(%)	(n)
Response Rate				
Overall	31.1 (0.4)	17,511	34.7 (0.4)*	17,510
Matched	45.4 (1.1)	3,160	45.0 (1.0)	3,247
Unmatched	30.3 (0.4)	14,351	34.0 (0.5)*	14,263
Prevalence				
Overall	13.4 (0.5)	5,943	14.1 (0.5)	6,498
Matched	49.9 (1.7)	1,491	48.5 (1.6)	1,552
Unmatched	11.2 (0.6)	4,452	12.2 (0.6)	4,946

Notes: (1) standard errors are in parentheses. (2) Domains are defined by matching ABS samples to state databases of licensed saltwater anglers.
*Significantly different from Saltwater Fishing Survey ($p<0.05$).

There were no significant differences between instruments for either response rate or prevalence within the matched domain, suggesting that the inclusion of non-fishing questions in the Weather and Outdoor Activity Survey did not have an impact on response by either anglers or non-anglers. In the unmatched domain, the response rate was significantly higher for the Weather and Outdoor Activity Survey than the Saltwater Fishing Survey. However, the higher response rate did not translate to lower or higher estimates of prevalence; estimates of prevalence were not significantly different between instruments within the domain. This suggests that the engaging instrument uniformly increased the probability of response for anglers and non-anglers within the unmatched domain.

Differential nonresponse to a survey request between subpopulations will result in nonresponse bias if the subpopulations are different with respect to the survey topic. In the tested design, we account for differential nonresponse between matched and unmatched households during sampling – matched and unmatched subpopulations are treated as independent strata. Consequently, the potential for nonresponse bias is limited to differential nonresponse between anglers and non-anglers within the matched and unmatched subpopulations. While the Weather and Outdoor Activity Survey achieved a higher response rate than the Saltwater Fishing Survey, both overall and within the unmatched subpopulation, the gains in response do not appear to result from a higher propensity to respond to the survey by either anglers or non-anglers. As a result, we cannot conclude that one of the instruments is more or less likely to minimize differential nonresponse between anglers and non-anglers. However, higher response rates decrease the risk for nonresponse bias and either lower data collection costs (for a fixed sample size) or increase the precision of estimates (for a fixed cost)[2]. Consequently, we conclude that the Weather and Outdoor Activity Survey is superior to the Saltwater Fishing Survey and recommend that the instrument be utilized for subsequent survey waves. Because it collects person-level information for all residents of all sampled households, the Weather and Outdoor Activity Survey also supports post-stratification of survey weights to population controls, which is an additional benefit of this recommendation.

Incentive Testing

The study included an experiment to test the impact of modest, prepaid cash incentives on survey response and survey measures. Each wave, sampled addresses were randomly allocated to incentive treatment groups of $1, $2, and $5, as well as a non-incentive control group. Incentives were only included in the initial survey mailing. As in the instrument experiment, the objective of the incentive testing was to identify an optimum level of incentive that maximizes overall response while controlling costs and minimizes the potential for nonresponse bias resulting from differential nonresponse between anglers and non-anglers. Response rates, estimated fishing prevalence and relative costs of completing an interview were compared among incentive treatments to quantify the impacts of incentives.

Table 2 shows weighted response rates and the results of a logistic regression model predicting the effects of incentives on the odds of obtaining a completed survey. Including an incentive in the initial survey mailing significantly increased the odds of receiving a completed survey, and the odds increased significantly as the incentive amount increased. Cash incentives of $1, $2,

[2] Assuming that fixed costs are the same for the two instruments, which was the case in the experiment.

and $5 increased the odds of receiving a completed survey by 63%, 93% and 137%, respectively.

Table 2. Weighted response rates and odds of receiving a completed survey by incentive amount.

Incentive	Response Rate (%)	n	Odds Ratio	95 % CI
$0	22.6	8,760	1.00	
$1	32.2	8,737	1.63*	(1.51, 1.77)
$2	36.0	8,738	1.93*	(1.78, 2.09)
$5	40.8	8,786	2.37*	(2.18, 2.56)

*Significantly different from the $0 control ($p$<0.05). Results of pairwise comparisons are as follows: $1>$0 (p<0.05), $2>$1 (p<0.05), $5>$2 (p<0.05).

Previous studies (Groves et al. 2006) have demonstrated that prepaid cash incentives can motivate individuals with little or no interest in a survey topic to respond to a survey request. Consequently, we hypothesized that incentives would have a larger impact on non-anglers than anglers, minimizing differential nonresponse between the two populations. We initially explored this hypothesis by comparing estimated fishing prevalence among incentive conditions, expecting that gains in response in the incentive conditions would translate to lower estimates of fishing prevalence. The results do not support this hypothesis; there were no significant differences in prevalence among incentive conditions (Table 3).

Table 3. Overall estimated fishing prevalence by incentive amount.

Incentive	Prevalence (%)	n
$0	12.8	2,154
$1	14.1	3,065
$2	13.6	3,415
$5	14.1	3,807

Note: Differences in prevalence among treatments are not significant (p=0.05)

We further explored the interaction of topic salience and incentives by examining response rates and estimated fishing prevalence for the incentive conditions within domains defined by whether or not sampled addresses could be matched to databases of licensed saltwater anglers. We expected incentives to have a more pronounced effect in the unmatched domain, a population less likely to have an interest in the survey topic, than in the matched domain. Table 4 shows that incentives increased the odds of receiving a completed survey in both the matched and unmatched subpopulations. However, the value of the incentive seems to be more important in the unmatched domain, where the odds of receiving a completed survey increased uniformly and significantly as the value of the incentive increased (\$0<\$1<\$2<\$5). In contrast, the incentive amount was less significant in the matched domain, where the odds of receiving a completed survey were relatively flat among incentive conditions. These results are consistent with our expectations and suggest that a population with a low propensity to respond to a fishing survey can be motivated to participate by cash incentives, and that the motivation may increase as the incentive amount increases.

Table 4. Odds of receiving a completed survey by level of incentive for sample that could and could not be matched to state databases of licensed anglers.

	Subpopulation	
Comparison Pair	Matched OR	Unmatched OR
\$1 vs. \$0	1.75**	1.63**
\$2 vs. \$0	2.01**	1.93**
\$5 vs. \$0	2.11**	2.39**
\$2 vs. \$1	1.15	1.18**
\$5 vs. \$1	1.21*	1.46**
\$5 vs. \$2	1.05	1.24**

Notes – The second value in the comparison pair is the reference value.
Significance: *p<0.05, **p<0.0001

As noted previously, we expected that the gains in response in the incentive conditions would translate to lower estimates of fishing prevalence, particularly in the unmatched subpopulation. Once again, the results are not consistent with expectations; differences in fishing prevalence among treatments were not significant in either the matched or unmatched domain (Table 5). The lack of an effect of incentives on fishing prevalence suggests that the gains in response associated with increasing incentive amounts are uniform between anglers and non-anglers. However, it's also possible that the gains in response are accompanied by an increase in measurement error; non-anglers may be more likely to report fishing behavior than anglers when an incentive is provided. This hypothesis was not tested and requires further investigation.

Table 5. Estimated fishing prevalence by incentive amount for a population of anglers (matched) and non-anglers (unmatched).

	Subpopulation			
	Matched		Unmatched	
Incentive	(%)	(n)	(%)	(n)
$0	49.2	533	10.7	1,621
$1	50.3	779	12	2,286
$2	48.6	837	11.6	2,578
$5	48.2	894	12.4	2,913

Note: Within subpopulations differences in prevalence among treatments are not significant (p=0.05).

We also examined the effect of cash incentives on overall data collection costs, specifically the direct costs of printing, postage, and the cash incentives themselves. Table 6 shows that the $5 incentive provided the largest gain in response, but the gain came at a relative cost of approximately $0.15 per completed interview. In contrast, the additional costs of the $1 and $2 incentives (20% and 38% higher cost than the $0 control, respectively) are more than offset by the associated gains in the number of completed surveys (42% and 58%, respectively). In other words, including a $1 or $2 cash incentive in the initial survey mailing actually decreased the cost of receiving a completed survey by 22% and 20%, respectively. These cost savings, which are conservative[3], could be used to lower overall data collection costs (for a fixed sample size) or increase the precision of survey estimates (for a fixed cost).

[3] The cost comparison assumes that the non-incentive direct costs (postage and printing) are the same for all survey treatments and does not reflect the fact that incentive conditions may not require as many follow-up mailings.

Table 6. Effect of incentives on data collection costs

Incentive Amount	Relative Cost Difference	Relative Difference in Completed Surveys	Relative Cost per Completed Survey
$0	1.00	1	$1.00
$1	1.20	1.42	$0.78
$2	1.38	1.58	$0.80
$5	1.90	1.75	$1.15

Note: relative differences reflect the ratio of quantities (cost, completes) in the experimental treatments to the zero dollar control.

Including a modest prepaid cash incentive in survey mailings clearly has a positive effect on survey response rates; the odds of receiving a completed survey increased significantly as the incentive amount increased. We expected the incentives to have a greater effect on non-anglers than anglers and decrease the potential for nonresponse bias by minimizing differential nonresponse between these two populations. However, the results of the experiment suggest that incentives increase response propensities for non-anglers and anglers equally. While this result does not support our hypothesis, it does demonstrate that incentives can increase the quantity of data without having a negative impact on survey measures. The experiment also demonstrated that incentives can decrease overall data collection costs. Based upon these findings, we conclude that a $2 incentive is optimal in terms of both maximizing response rates and minimizing data collection costs.

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Groves, R.M., M.P. Couper, S. Presser, E. Singer, R. Tourangeau, G.P. Piani, N. Lindsay. 2006. Experiments in producing nonresponse bias. Public Opinion Quarterly 70: 720-736.

Lohr, S. 2009. Multiple frame surveys. Pages 71-88 *in* D. Pfeffermann, editor. Handbook of statistics: sample surveys design, methods and applications, volume 29A. Elsevier, Amsterdam.

Project Team Response
7/31/2014

Peer Review Report for

"Development and Testing of Recreational Fishing Effort Surveys, Testing a Single-Phase Mail Survey Design"

Reviewed by

Dr. Michael Cohen, American Institutes for Research
Dr. Ronald Fesco, Ernst & Young LLP
Dr. Phillip Kott, RTI International

Introduction

This document combines the comments provided by three different peer reviewers of the MRIP Project Report entitled "*Development and Testing of Recreational Fishing Effort Surveys, Testing a Single-Phase Mail Survey Design*." The document provides verbatim reviewer comments without identifying the source of each comment.

Reviewer 1

This review of the report entitled "Development and Testing of Recreational Fishing Effort Surveys: Testing a Single-Phase Mail Survey Design" provides comments and suggestions on the methods, results and conclusions found in the report. The review does not include any working with the original data and thus does not encompass any validation of data or primary calculations with the data. The review examines only summary calculations found in the report and, accepting those as shown, assesses the reasonableness of methods, approach and use of results to reach conclusions about aspects of Recreational Fishing Effort Surveys (RFES), especially the recommendation to move to a mail survey design.

The report presents the results of an evaluation of a single phase mail survey design as an alternative to the Coastal Household Telephone Survey (CHTS) for estimating marine recreational fishing effort. The objectives identified in the report were to:

1) test the feasibility of a mail survey design for collecting recreational fishing effort data and estimating fishing effort for shore and private boat anglers,

2) compare single phase mail survey and CHTS results, including metrics of survey quality and estimates of marine recreational fishing activity,

3) describe, to the greatest extent possible, differences between single phase mail survey and CHTS estimates in terms of sources of survey error, and

4) provide recommendations for follow-up action, including implementation of improved survey methods.

This review will discuss the objectives in order and provide several other insights to conclude.

Generally, the analysis is done very well with considerable thought about identifying and measuring sources of differences between the surveys. I find no meaningful issues in the methodology used or the analyses and therefore provide brief comments on the 4 objectives above and I do not reiterate the various findings. Finally, I will discuss some ideas for future consideration.

OBJECTIVE 1) test the feasibility of a mail survey design for collecting recreational fishing effort data and estimating fishing effort for shore and private boat anglers

The authors (Andrews, Brick and Mathiowetz) describe a well conceived experimental approach to providing metrics to lead to decisions on survey approaches. They describe problems with the existing survey, especially low response rates, and identify issues that can further degrade quality of the existing design e.g., declining landline use. They make reasoned and convincing arguments, supported by the metrics, that response rates and response error are less of a problem with mail surveys and those improvements also reduce bias problems. The authors also show that the quality improvements can be achieved within the time frame required of the survey operations. I agree with their conclusion that a mail survey design is feasible and preferred.

The use of a $2 incentive was clearly justified by the analysis of experiments found in appendix B. Often incentive experiments fail to discuss overall cost relative to effect. Here, the authors provide a fair comparison taking cost into consideration. Further analysis of the impact on broader survey costs including the typically expensive follow up of nonrespondents for incremental incentives from $2 to $5 would add to the understand, but the gains in response at the $2 level would typically be cost effective, making the use in the design reasonable.

Response: We appreciate the reviewer's encouraging comments regarding the feasibility of the mail survey design.

OBJECTIVE 2) compare single phase mail survey and CHTS results, including metrics of survey quality and estimates of marine recreational fishing activity

The research appropriately examines design features that may impact differences between survey approaches. The analysis indicates that mail survey methods result in larger estimates of percent of households fishing while mean numbers of within household statistics vary with mean trips larger for mail and other items not particularly different. Reasons for the differences are hypothesized and explored in a balanced and fair manner.

While "quality" is not specifically defined in the report, most methodologists would consider cost, timeliness and relevance along with the usual focus on error sources. The authors have exhibited some cost improvements in the mail survey approach and that it meets timeliness needs. The authors explore various thoughts on response differences and bias sources (geographic, unlicensed anglers, etc.) finding that the mailing methods perform well and the responses may be more in line with the concepts desired.

Response: We appreciate the reviewer's positive comments regarding the analyses described in the report.

OBJECTIVE 3) describe, to the greatest extent possible, differences between single phase mail survey and CHTS estimates in terms of sources of survey error

As mentioned above, survey error is one of the quality dimensions. The report explores usual sources of error for the survey types. Identifying sources of error is an intuitive and experience based endeavor. The authors were creative and explored a commendable range of ideas. The range of finding are sufficient to support their conclusions regarding survey methodology changes.

Response: We appreciate the reviewer's positive comments regarding the findings described in the report.

OBJECTIVE 4) provide recommendations for follow-up action, including implementation of improved survey methods

The matching of ABS sample to license frames (p. 8) is a good idea and can be effective for stratification and sample allocation.

The main recommendation, using a single-phase mail survey, covers many potential improvements. This recommendation is supported and reasonable. The suggestion for continued development and testing (p. 32) is reasonable because there usually are changes to consider when moving to full scale implementation.

With the evolution of e-mail and web collection modes, the recommendation to explore such methods is reasonable. Methodologists such as Don Dillman are conducting current research that should be examined for applicability.

Response: We appreciate the reviewer's positive comments regarding the conclusions and recommendations described in the report.

COMMENTS

Bottom line, I can find nothing of concern in the methods, analyses or conclusions in the paper. That said, identifying error sources in surveys is difficult, but the authors explored a wide and thoughtful set of issues and make appropriate suggestions for further research. As such, I find no reason to be concerned about their suggestion to move to a mail survey approach and believe it would be a reasonable thing to do.

Response: We appreciate the reviewer's positive comments.

IDEAS

Consider development of a bridging survey approach. Estimates will be changing with a move to mail and the research is based on a subset of areas to be sampled. Methodology will likely evolve a bit as well. A bridge helps to keep the time series of estimates usable.

Response: We agree that a bridging approach would help transition from the CHTS to a new survey design.

The may be a number of co varying attributes related to response and fishing. Age comes to mind as it is likely related to landline or cell use. It may also be something that increases with age to a point at which infirmity reduces fishing. The age distribution in the study states may be impacting some of the results. FL and NC are more destination states for retirees from the north. Thus, age may be influencing some of the state differences found (e.g. Table 4) and mail could reduce the impact in states with an older population.

Response: We appreciate the suggestion to explore co-variates to fishing effort. Person- and household-level demographic information is collected in the mail survey instrument. We will continue to examine differences in fishing activity among sub-populations and explore ways to incorporate this information into the estimation design (e.g., raking survey weights to control totals).

The analysis of difference from APAIS should consider the non-coastal travelers reason to travel and method of travel. Someone driving can take poles for surf fishing and avoid piers etc. Those flying have a much more difficult time taking equipment. This could influence the APAIS results. Also some areas are more known for travel to surf fish - NC - and travel there may be more by personal vehicle and with gear. Other areas like Florida may be more by air travel.

Response: Neither the APAIS nor mail survey collects information about the method of travel or reason for travel. We will continue to explore differences in residency distributions between the mail survey and APAIS by state and fishing mode.

I'm not sure that I agree with footnote 15. I've never had a problem finding a non-APAIS place to surf fish near the hotel or condo wherever we stay. It may be instructive to look at differences by state for domain estimates for in-state vs. out-of-state people in the APAIS data.

Response: We will continue to explore differences in residency distributions between the APAIS and mail survey.

Another factor to consider may be the proportion of the state's population living near the coast. If large cities are coastal, surf fishing may dominate.

Response: We will continue to explore differences in residency distributions between the APAIS and mail survey.

The thought in the above comments is that other characteristics may be useful in further improving the survey design and information useful to collect. Exploring how fishing responses compare to other characteristics collected in the survey may provide more ideas.

Response: We appreciate this constructive suggestion.

Pay pier is not specifically mentioned in the questionnaire in Q 15a or b. Dock etc of 15a may not draw the memory out. I might not have considered the fishing pier experience when answering 15a and then it is not a part of 15b.

Response: We appreciate this constructive suggestion and will consider modifications to the survey instrument to improve the accuracy of reporting.

Reviewer 2

"Developing and Testing of Recreational Fishing Effort Survey Testing a Single Phase Mail Survey Design" reports on research designed to improve the way estimates of recreational fishing effort are made with an emphasis on the last test conducted in four states using what the authors call a "single-phase dual-frame mail survey." The research itself is sturdy and the results (that the new estimation strategy is far superior to what is done now) convincing. The report itself, however, has a number of flaws.

One flaw that afflicts many research reports is the inconsistent use of tense. This is understandable given that the research has already been done but the methods used can be repeated, so describing them in the present tense makes some sense. What makes the tense-use problem particularly acute here is that some of the methods described were tested before the method on which the report focuses. The reader would have an easier time understanding what is old and what is new if the past perfect where used ("anglers *had been* mailed") in describing previous methods tested. Instead, the present is used to describe a method that had been tested before the single-phase dual-frame mail survey, while single-phase dual-frame mail survey is later described in the past tense.

Response: The text was modified to more clearly distinguish between the current pilot study and previous pilot studies.

A second flaw is that the authors' single-phase dual-frame mail survey, although a mail survey, is not single phase (there is subsampling in certain strata) and only technically dual frame. There *are* two frames in a state, an address-based resident frame and a frame containing non-resident licensed saltwater anglers, but since these frames do not overlap, dual-frame methodology is not employed. Instead, these separate frame as used in creating disjoint strata.

Response: References to a single-phase were intended to reflect the fact that data were collected in a single phase. However, we agree that this description is confusing and contradicts with the sample design, which includes sub-sampling in certain strata. We eliminated references to the "single-phase design" and explicitly state that data were collected in a single phase.

References to the dual-frame design were not changed as the survey employed a dual-frame design with non-overlapping frames (the ABS frame and the non-resident license frames are the two non-overlapping frames).

There is much discussion of stratification, but not enough to satisfy this reader. What exactly were the strata in each state, the targeted stratum sampling rates, and the actual stratum response rates? Readers are lead to believe that weights were equal within strata and reflected both the within-stratum sampling and response rates but are never told so explicitly. Consequently, that reasonable approach to handling nonresponse is never justified. (The lack of details carries over to Appendix B, where readers are given very little information about a logistic regression used to draw many conclusions.)

Response: We appreciate the suggestion to include more technical details in the report. However, the intended audience for the report includes managers and administrators. Consequently, we did not want to overwhelm the audience with technical details. Technical details about the survey design will be documented elsewhere.

There is one minor technical error (excusing the use of "single-phase" because there is only a single phase of data collection) and a somewhat larger technical embarrassment in the report. The minor technical error is the suggestion on page 25 that the expectation operator on probability-sampling theory breaks down for very small prevalences. It does not, estimates remain unbiased. The problem is that they are not very accurate. Their relative variances are high, and their nonnormality makes coverage-interval construction from their variance estimates dubious.

Response: We agree the language about this bias was confusing. We have revised the text to indicate the bias is that of separate stratum ratio estimators (the poststratified estimator in this case at the county level). When stratum sample size is small in the denominator of a ratio estimator, it is biased. A combined rather than separate ratio estimator would avoid this bias but is not used in CHTS. Furthermore, because saltwater fishing is a relatively rare event among the general population, repeated samplings from the general population will result in a distribution of estimates that is skewed with zero occurrences of reported fishing activity – so the bias of the ratio estimator results in underestimation. We revised the report to more clearly state the impact of small sample sizes on CHTS estimates.

The somewhat larger embarrassment is that, contrary to the authors' assertion, the fraction of respondents engaged in fishing is not a reasonable measure of the efficiency of the single-phase-dual-frame-mail-survey estimation strategy because targeted anglers are down- weighted in the estimation. Good measures of the strategy's relative statistical efficiency are the design effects of the estimates it produces. The only design effect the authors report is, unfortunately, close to 1. Others, especially for estimates of the anglers themselves, are likely to be smaller (if correctly computed for the purpose of evaluating the design).

Matching address samples to lists of licensed anglers proved to be an effective way to sample anglers, a relatively rare population. The key statistic from the survey is a characteristic of anglers (the number of trips taken) and by having a larger sample of anglers we are able to increase the statistical efficiency of this estimate. A much larger address sample would have been required to achieve the same effective sample of fishing households if license matching (i.e., screening prior to data collection) was not possible. This would have required additional mailings and would have resulted in substantially higher costs. In this sense, the design was more efficient that simple random sampling. We revised the text to more clearly characterize the benefits of the design. We did include some design effects in the revision, but that measure is not related to cost efficiency in that the same design effect can be achieved with different costs.

Ultimately, however, these criticisms of the report are minor. As I wrote earlier, I found the report's conclusions convincing. I very much like what I can make out of the sampling and estimation strategy that the authors' recommend. The flaws in the report are statistical in nature. On the survey-methodology side, the report contains a commendable treatment of the problems and limitations involved in collecting the information desired.

Response: We appreciate the reviewer's positive comments about the report.

Reviewer 3

This well written and thoughtful report makes its main case overwhelmingly. The single phase mail survey (SPMS) is the clear winner when compared to the Coastal Household Telephone Survey (CHTS).

Response: We appreciate the reviewer's positive comments about the report.

Given the stark differences in marine fishing activity reported by the two surveys, there will be keen interest in how the differences break out by age, racial/ethnic, and sex groups. Are the young and elderly fishing off piers sometimes being missed? Are women and girls sometimes regarded as participants in marine fishing and other times just thought of as on- lookers? Do we know that racial/ethnic minorities are being represented fairly? There doubtless will be great interest in such questions.

Response: We will continue to examine the demographic characteristics of the sample and explore ways to incorporate this information into the estimation design.

Specific Comments:

Page 12, lines 5-7 from bottom: "median" is not explained correctly. It means that half the responses were received before the 14th day (or possibly on the 14th day, depending on the specifics of the definition).

Response: We have revised the report to accurately describe median response times.

On page 13, Figure 1, I did not understand the dots. There are many more dots after 20 days than before.

Response: Each dot represents a point in time. There are more dots after 20 days because the data collection continued for several additional weeks beyond 20 days. The figure shows the cumulative percentage of completed mail surveys over time and demonstrates that the vast majority (>70%) of completed surveys are returned within about three weeks of the initial mailing.

The last paragraph on page 23 makes perfect sense right up to the final "i.e.". The phrase "i.e., only individuals in households without licensed anglers could have contributed to nonresponse bias resulting from differential response between anglers and non-anglers" does not seem to me to follow from the rest of the paragraph nor do I think it is true. On rereading this some time after I wrote the previous two sentences, the point may be that unlicensed anglers mess up the nonresponse adjustment. I still do not think the quoted sentence is the right way to say it.

Response: We modified the sentence to more clearly articulate the benefit of frame matching on nonresponse weighting adjustment.

I disagree with the argument at the end of the first complete paragraph on page 25: "…we hypothesized that low sample sizes in the CHTS during low-activity waves result[s] in underestimates of state-level fishing effort." Small sample sizes will increase variance but not cause bias. It could happen that one would get a larger than average number (e.g. 2) of anglers, and they would have large weights.

Response: We address the impact of small sample sizes on CHTS estimates above.

I kept wanting to see discussion of possible measurement bias, and finally there is an excellent discussion in the paragraph beginning on page 28. But measurement bias could affect the earlier analyses so should be introduced sooner.

Response: We agree that measurement bias is a likely source of differences between mail survey and CHTS estimates. However, the discussion of measurement bias is largely hypothetical and based upon the results from previous pilot studies. The assessment of non-coverage bias is more direct and quantifiable. Consequently, we chose to discuss the impacts of non-coverage bias first.

It is remarkable (page B8, Table 6) that the $1 and $2 incentives lead to lower relative costs per completed survey compared to no incentive or $5 incentive. But I do not think one can conclude that the $5 incentive is sub-optimal (last line on page B8). It depends on the relative value one puts on maximizing response rates versus minimizing data collection costs. Even though (page B7, Table 5) the prevalence rate estimates do not differ significantly among the incentive levels, other estimates may be enhanced by a higher response rate.

Response: We agree that assigning a value to survey incentives involves a trade-off between cost considerations and data quality. For the purposes of this study, we determined that a $2.00 incentive had a greater relative value than the other incentive amounts. A $5.00 incentive would have resulted in a higher response rate, but the gains in additional sample would have been outweighed by the additional data collection cost. The $2.00 incentive resulted in the largest effective sample for a fixed data collection cost.

Editorial Comments:
Executive Summary, line 4: Either delete semi-colon or replace with colon.

Response: The semi-colon has been replaced with a colon.

On page 18, line 3 of second paragraph: I would change "(wireless households)" to "(wireless only households)".

Response: "Wireless households" has been replaced with "wireless-only households".

Page 25, last line of first complete paragraph: Change "results" to "result".

Response: "Results" has been changed to "result".

Page 33, second reference: I think the %20s in the URL should be spaces. Some systems changes spaces to %20s.

Response: The URL has been updated.

Page B5, Table 2, $2 Incentive line: Change "36" to "36.0".

Response: "36" has been replaced with "36.0".

Marine Recreational Information Program
FY-2014

Estimating Recreational Fishing Effort from Onsite Survey Data

Project: Estimating Recreational Fishing Effort from Onsite Survey Data

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03/28/2017

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1. Is it Influential Scientific Information?
N

2. Has it had sufficient Peer Review?
N

3. MRIP Certified

4. Report Title
Estimating Recreational Fishing Effort from Onsite Survey Data

5. Executive Summary

We investigated five methods of selecting time intervals within the study time period for counting complete angler-trips. In method 1, one time interval is selected randomly within the study time period for counting complete angler-trips. In method 2, two time intervals are selected randomly within the study time period for counting complete angler-trips. In method 3, the study time period is divided into two subperiods and one time interval is selected randomly from each of the two subperiods. In method 4, three time intervals are selected randomly within the study time period for counting complete angler-trips. In method 5, the study time period is divided into three subperiods and one time interval is selected randomly from each of the three subperiods. For method 1, 2, and 4, the estimate of complete angler-trips for that study time period is obtained by expanding the average count of complete angler-trips per unit time during the counting time interval by the duration of that study time period. For method 3 and 5, estimate of complete angler-trips for the study time period is obtained by summing the estimates of complete angler-trips for all subperiods. Complete angler-trips for each subperiod is estimated by expanding the average count of complete angler-trips per unit time during the counting time interval by the duration of that subperiod.

In our simulation studies, all five methods yielded unbiased estimate of complete angler-trips when the distribution of anglers' departure times is homogeneous. However, only method 1, 3, and 5 produced unbiased estimate of complete angler-trips when the distribution of anglers' departure times is nonhomogeneous. Methods 2 and 4 underestimated the true complete angler-trips when anglers leave the fishing access site at an increasing rate (i.e., the number of departures increases with time) and overestimated the true complete angler-trips when anglers leave the fishing access site at a declining rate (i.e., the number of departures decreases with time). The bias from method 4 was larger than that from method 2. The estimate of complete angler-trips from method 5 was more precise than the estimate from method 3, and the estimate from method 3 was more precise than the estimate from method 1. These results suggest that stratification of the study time period can increase the accuracy and improve the precision of the estimate of angler-trips when the distribution of anglers' departure times is nonhomogeneous.

6. Background

Recreational catches account for a relatively large percentage of the total catches by commercial and recreational fishing in many marine species including the economically valuable and overfished ones such as red snapper, red drum, and bocaccio (Coleman et al. 2004).

Quantifying recreational catch and effort is important to the studies of impact of recreational fisheries on marine resources and economic issues (Cooke and Cowx 2004) despite the challenges due to the large spatial and temporal variability of the recreational fisheries and the difficulty in collecting data on the areas fished and time spent (McCluskey and Lewison 2008).

The Marine Recreational Information Program (MRIP) developed and managed by the National Marine Fisheries Service (NMFS) is aimed to estimate saltwater recreational fishing catch and effort in all Atlantic and Gulf of Mexico coastal states. MRIP is designed as a complementary two-part survey comprised of an onsite component and a telephone component. The onsite survey is conducted by interviewing anglers at the fishing access sites, while the telephone survey is conducted by a random digit dial of coastal households for shore and private boat fishing modes or by a dial of randomly selected fishing boat captains for for-hire (e.g. charter) boat fishing mode. In the telephone survey, the interviewers collect data for the fishing activities of the past. The onsite survey is aimed to collect data for estimating catch rate, while the telephone survey is aimed to collect data for estimating fishing effort. With the fishing effort estimated from the telephone survey and the catch rate from the onsite survey, catch is estimated as the product of the fishing effort and the catch rate.

Although the MRIP onsite survey is designed to collect data for estimating catch rate, data required for estimating effort (e.g., count of anglers) have also been collected through this survey. As a result, it is possible to obtain an effort estimate from the onsite survey data for the fishing sites covered by the survey. Both the telephone survey and the onsite survey have advantages and disadvantages when they are used for estimating fishing effort. The telephone survey enables data collection from a larger number and geographically more widely distributed sample of anglers than could be obtained from the onsite survey. For example, the telephone survey enables data collection for fishing activities occurred at both public and private access sites, whereas the onsite survey can collect data for fishing activities occurring at only public access sites. However, despite its disadvantage, the onsite survey provides more instant results and higher response rate and is less prone to reporting errors than the telephone survey. It is of interest to know whether the effort estimate from the onsite survey can be used as a check to the effort estimate from the telephone survey to ensure that fishing effort is not over- or under- estimated by the telephone survey.

A previous study of effort estimation from onsite surveys by Hoenig et al. (1993) has been focused on the methods for estimating recreational angler-hours. However, for a telephone survey, collecting data of the past fishing activities at the level of fishing hours is usually difficult and induces reporting errors. For this reason, MRIP has historically chosen to quantify recreational fishing effort by anglers' number of fishing trips or, in a short name, angler-trips. In order to compare the effort estimate from the telephone survey with the effort estimate from the onsite survey, an effort estimate in angler-trips from the onsite survey is needed. Unfortunately, our knowledge of how and how well we can estimate angler-trips from an onsite survey is rather limited. In particular, the current MRIP onsite survey counts complete angler-trips at each randomly selected fishing access site. The counts are made only during randomly selected time intervals (ΔT's) within each 6-hour study time period (*T*). The common method for estimating the complete angler-trips during *T* for each site is to expand the average observed counts during these randomly selected ΔT's by the length of *T* (i.e., 6 hours). A major question concerning this method is how the distribution of the anglers' departure times from each fishing access site affects the accuracy of the estimate of complete angler-trips.

In this report, we investigate how different methods of selecting time intervals within the study time period for counting complete angler-trips will affect the accuracy of the estimate of complete angler-trips for that study time period under different distributions of anglers' departure times. We carry out this investigation using simulation studies. We assume that the primary sampling units are the combinations of site and study time periods and focus our study on a single such combination of site and time period. In practice, site-time period combinations are usually selected at random or with known inclusion probabilities. An estimate of total complete angler-trips over all site-time period combinations in the sampling frame is obtained by applying the sampling design features (e.g., stratification, clustering) and appropriate sampling weights to the data collected from the selected site-time period combinations.

7. Methods

The commonly used method for calculating complete angler-trips for a selected site-time period combination is

$$\varepsilon = \frac{T}{\Delta T} C \qquad (1)$$

where ε is the estimated complete angler-trips for that site-time period, T is the duration of the study time period, ΔT is the duration of interval count within T. Here, $\Delta T = T_1 - T_0$ where T_0 and T_1 are start time and stop time of counting. C is the count of complete angler-trips during ΔT.

Let N be the actual complete angler-trips during T. ε is an unbiased estimator of N if the anglers' departure times from the studied fishing access site during T are uniformly distributed (Appendix 1). However, the anglers' departure times are rarely uniformly distributed in the real world. For example, a majority of anglers may arrive to a fishing access site in the early morning and depart in the late afternoon. Consequently, ε will under-estimate N if we happen to select ΔT's only in the morning to count complete angler-trips.

To study how different methods of selecting time intervals within the study time period for counting complete angler-trips will affect the accuracy of the estimate of complete angler-trips for that time period under different distributions of anglers' departure times, we conducted simulation studies for the survey of a single combination of site and time period. We considered two situations when generating anglers' arrival and departure times: (1) anglers arrive and depart individually (e.g., for shore fishing modes), and (2) anglers arrive and depart in groups (e.g., for boat fishing modes). The procedures for the simulation studies are described as below. We describe the processes of generating the arrival and departure times for only anglers who arrive and depart individually. For grouped anglers who arrive and depart at the same time, we simply duplicated the arrival and departure times by the number of anglers in each group. The number of anglers in a group was assigned randomly from 2 to 6.

Step 1: Generating anglers' arrival times

We assumed that anglers' arrivals to the fishing access site follow a Poisson process with $\lambda = 4$ from 6 AM to noon and $\lambda_t = \lambda - 0.8\ t$ after noon. That is, the average number of arrivals per hour is 4 from 6 AM to 12 PM and then declines with time until when there is no more arrival at and after 5 PM. The Poisson process is homogeneous when λ is constant and non-homogeneous when λ changes with time. For a homogeneous Poisson process, inter-arrival time (the waiting time from the last arrival to the next) follows an exponential distribution with mean $1/\lambda$. We generated the homogeneous arrival times using this property of the Poisson process and the non-homogeneous arrival times using the "thinning" method introduced by Pasupathy (2011). With the "thinning" method we first generated anglers' arrival times from a homogeneous Poisson process with $\lambda=4$, and then reject an appropriate fraction of the generated anglers' arrival times so that the desired rate is obtained.

Step 2: Generating anglers' fishing hours

We generated anglers' fishing hours by assuming that the anglers' fishing hours follow a Weibull distribution with scale parameter $\lambda=2$ and shape parameter $\gamma=2$. We also assumed that each angler fished a minimum of 2 hours. The "risk" that an angler will quit fishing increases with the number of fishing hours when the shape parameter of the Weibull distribution is greater than 1.

Step 3: Generating anglers' departure times

Each angler's departure time from the fishing access site was generated by adding the number of hours spent in fishing to the arrival time. This method yields a non-homogeneous distribution of the anglers' departure times.

To compare the effects of nonhomogeneous departure times with the effects of homogeneous departure times on the estimate of complete angler-trips, we also generated a homogeneous distribution of anglers' departure times by assuming that the anglers' departure times follow a homogeneous Poisson process with $\lambda=4$. The homogeneous distribution of anglers' departure times was generated separately from the nonhomogeneous distribution.

Step 4: Selecting random count intervals

Time intervals (ΔT's) within the study time period *T* for counting complete angler-trips from the generated distribution of anglers' departure times were selected randomly by using the method proposed by Hoenig et al. (1993). That is, choose ΔT so that *T* is *k* (an integer) multiple of ΔT, and then select an integer *I* randomly in the closed interval [0, *k*-1] and schedule the count to begin at $I \Delta T$. In our simulation studies, we let T = 6 h, $\Delta T=0.5$ h, so the possible starting times for counting complete angler-trips were at 0, 0.5h, 1h, 1.5h, 2h, 2.5h, 3h, 3.5h, 4h, 4.5h, 5h, and 5.5h after the starting time of the study time period *T*.

We considered five methods of selecting ΔT's within *T* for counting complete angler-trips and investigated how different methods affected the accuracy of the estimate of complete angler-trips during *T*. These five methods are:

1. Choose 1 ΔT randomly within *T*,

2. Choose 2 ΔT's randomly within *T*,

3. Stratify *T* into two subperiods T_1 and T_2, and then choose 1 ΔT randomly from each subperiod,

4. Choose 3 ΔT's randomly within *T*,

5. Stratify *T* into three subperiods T_1, T_2, and T_3, and then choose 1 ΔT randomly from each subperiod.

We considered three study time periods (*T*): 8 AM to 2 PM, 10:30 AM to 4:30 PM, and 2PM to 8PM, respectively.

Step 5: Estimating total angler-trips

We estimated complete angler-trips from the generated distribution of anglers' departure times during *T* using counts of complete angler-trips from each of the five methods of selecting ΔT's within *T* for counting complete angler-trips, as described in step 4. We assumed that all anglers who leave from the fishing access site after completing fishing during ΔT's are counted.

For method 1, we estimated complete angler-trips during *T* using equation (1). For methods 2 and 4, we estimated complete angler-trips during *T* by

$$\varepsilon = \frac{T}{\sum_{i=1}^{n} \Delta T_i} \sum_{i=1}^{n} c_i \qquad (2)$$

where ΔT is the duration of the *i*th interval count within *T*, c_i is the count of complete angler-trips during ΔT_i, $n=2$ for method 2, and $n=3$ for method 4.

For methods 3 and 5, we estimated complete angler-trips during *T* by

$$\varepsilon = \sum_{i=1}^{n} \frac{T_i}{\Delta T_i} C_i \qquad (3)$$

where T_i is the length of the *i*th subperiod within *T*, and ΔT_i is the duration of the *i*th interval count within T_i. Similar to those in equation (2), c_i is the count of complete angler-trips during ΔT_i, $n=2$ for method 3, and $n=3$ for method 5. In our simulation studies, T_i are of equal length.

Step 6: Calculating bias

The bias in the estimate of complete angler-trips during *T* was calculated as the difference between the estimate ε and the true value *N*.

Step 7: Summarizing results

Steps 1 to 6 were repeated 10, 000 times. Finally, we calculated the mean of the 10,000 biases and standard error of the mean bias.

8. Results

Figure 1 shows the distribution of the anglers' arrival times to the fishing access site for all 10,000 replicates. As assumed, anglers arrived to the fishing access site at a constant rate from 6 AM to noon, and then at a declining rate from noon until when there were no more arrivals at and after 5 PM.

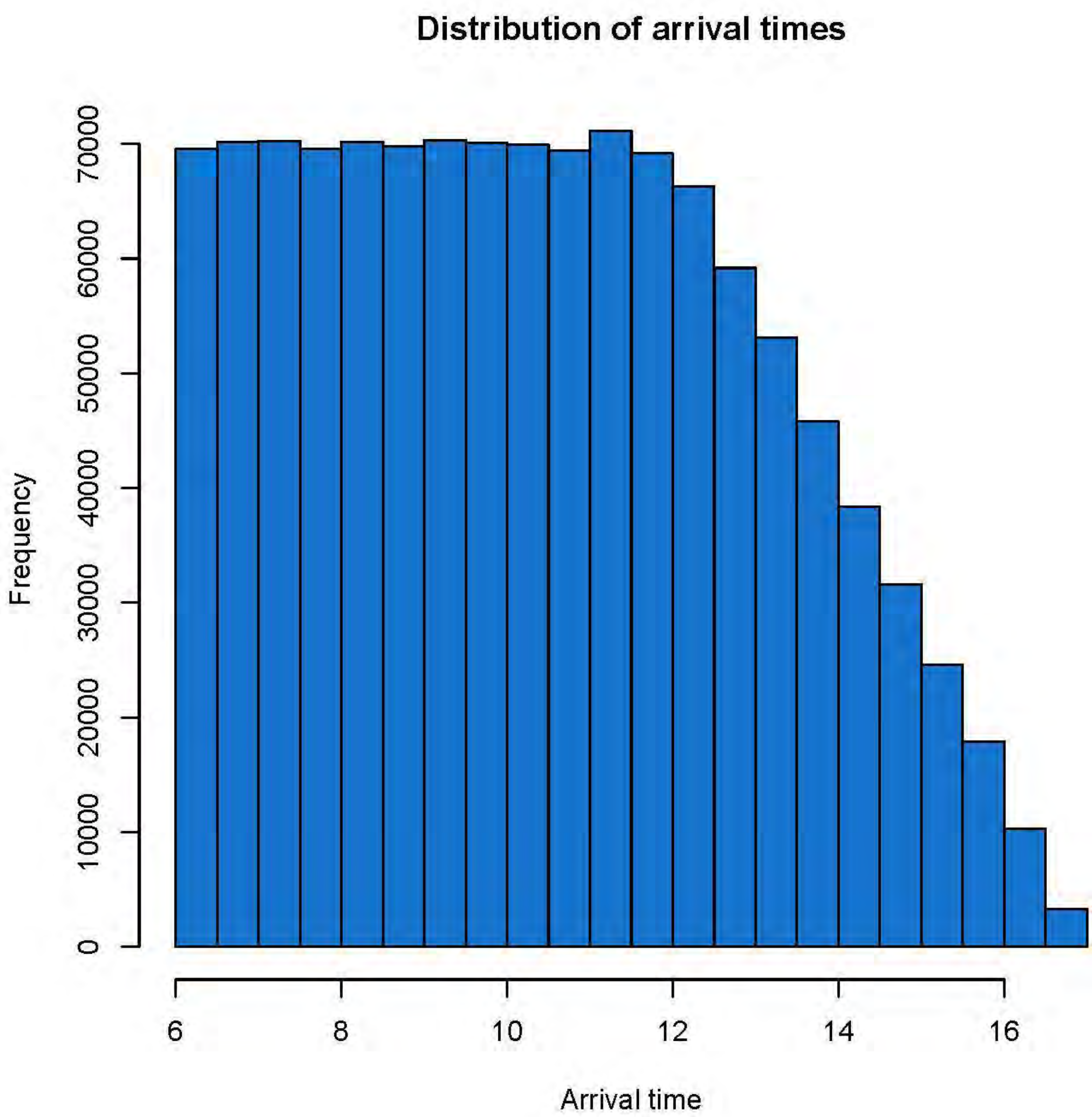

Figure 1. Distribution of the anglers' arrival times from 10, 000 replicates.

Figure 2 shows the distribution of the anglers' departure times from the fishing access site for all 10,000 replications. As assumed, the minimum number of hours of fishing for each angler was 2 hours. So, the departure time started from 8 AM. This figure shows the departure times for only until 8 PM. The anglers started leaving the fishing access site from 8 AM at an increasing rate until the rate peaked at around 2 PM. Then, the anglers continued leaving the fishing access site at a declining rate after around 2 PM.

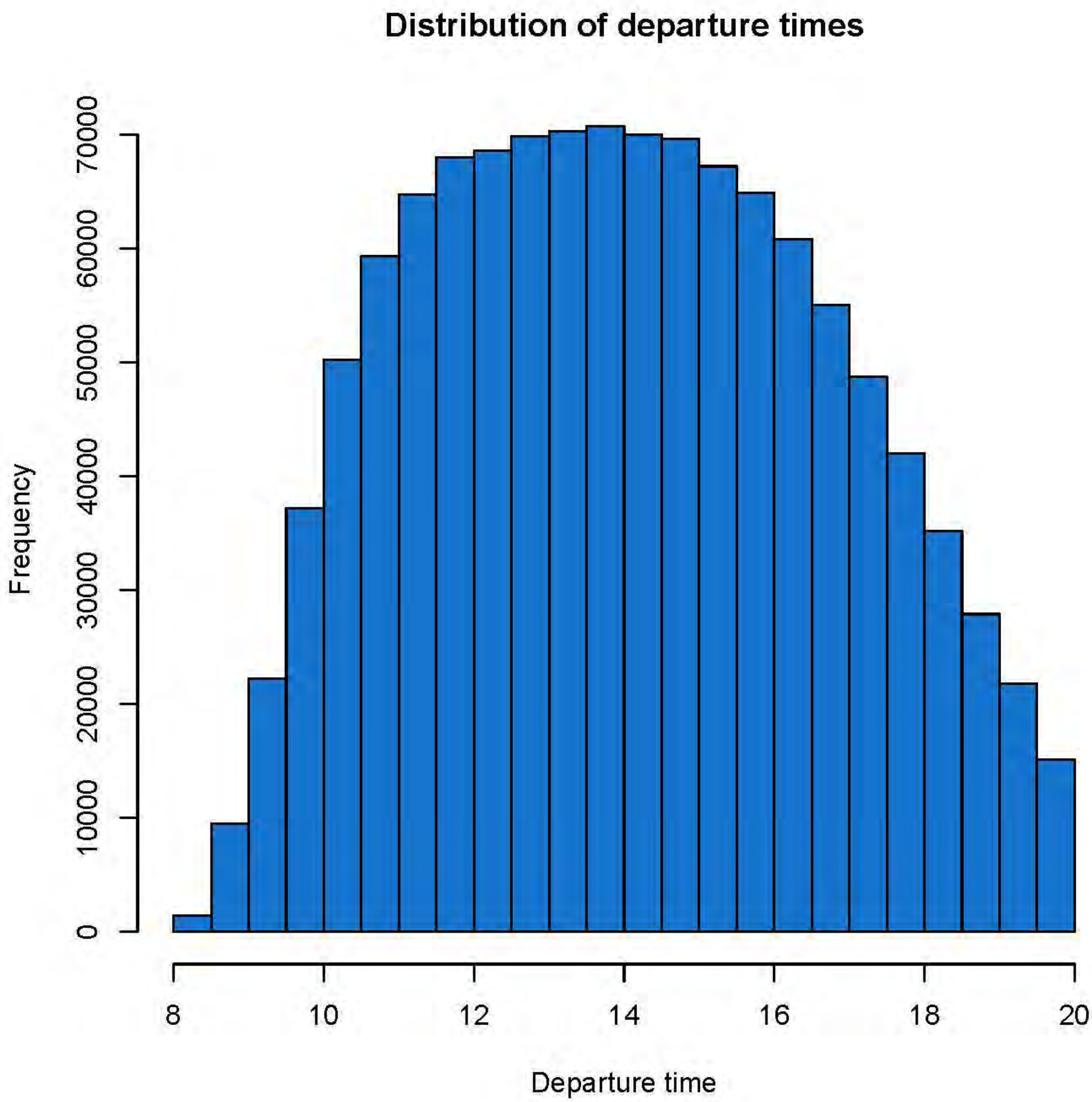

Figure 2. Distribution of the anglers' departure times from 8 AM to 8 PM from 10, 000 replicates.

We estimated complete angler-trips from the generated nonhomogeneous distributions of anglers' departure times during the study time period using each of the five methods of selecting time intervals within each study time period for counting complete angler-trips.

For the study time period 8 AM to 2 PM, the anglers left the fishing access site at an increasing rate (Figure 2). The mean observed value of complete angler-trips from the 10,000 generated nonhomogeneous distributions of angler's departure times was around 59 with SD = 16 for this time period (Table 1). The large value of standard deviation is caused by the differences in departure times between grouped anglers.

The mean biases in the estimates of complete angler-trips from these nonhomogeneous distributions of anglers' departure times for the study time period 8 AM to 2 PM were -0.1, -16.6, 0.6, -28.6, and -0.1, respectively, for methods 1 to 5. The standard errors for these mean biases were 0.6, 0.6, 0.4, 0.5, and 0.3, respectively (Figure 3). Methods 2 and 4 produced lower estimated complete angler-trips than the observed value. Bias in the estimate of complete angler-trips was larger for method 4 than for method 2. Methods 1, 3, and 5 all produced unbiased estimates of complete angler-trips. However, the estimate from method 5 was more precise than that from method 3, and the estimate from method 3 was more precise than that from method 1.

Table 1. Mean observed values of complete angler-trips from the 10,000 homogeneous and 10, 000 nonhomogeneous distributions of anglers' departure times.

Distribution of anglers' departure	Study time period	Mean	Standard Deviation
Nonhomogeneous	8 AM to 2 PM	59	16
	10:30 AM to 4:30 PM	80	18
	2 PM to 8 PM	58	16
Homogeneous	8 AM to 2 PM	63	17
	10:30 AM to 4:30 PM	63	16
	2 PM to 8 PM	63	16

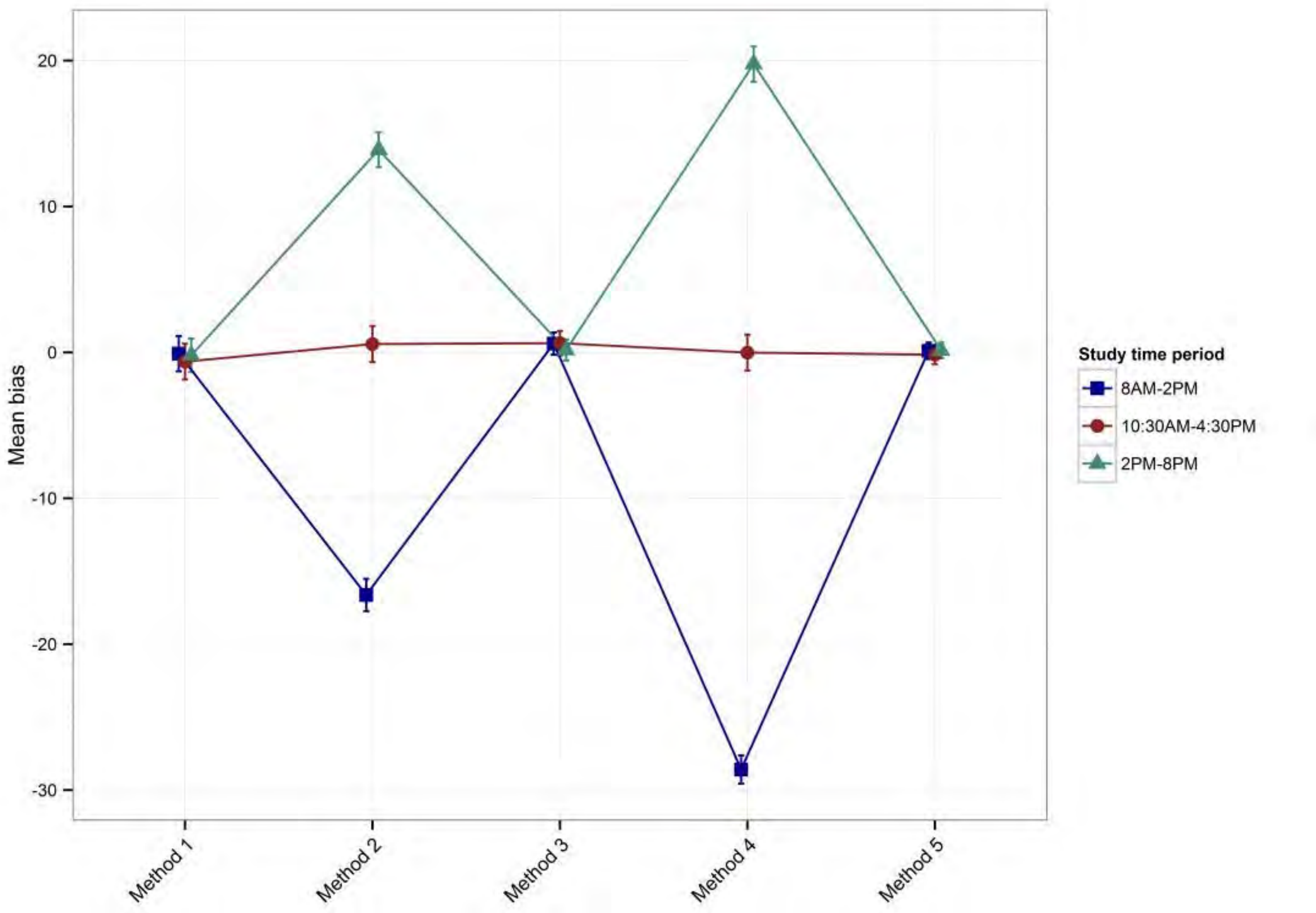

Figure 3. Mean biases in the estimate of complete angler-trips over 10, 000 replicates for the study time period 8 AM to 2 PM, 10:30 AM to 4:30 PM, and 2 PM to 8 PM. The vertical bars are 2 times the standard error. Method 1: choose 1 count interval (ΔT) randomly within the study time period (T); Method 2: choose 2 ΔT's randomly within T; Method 3: stratify T into two subperiods T_1 and T_2, and then choose 1 ΔT randomly from each subperiod; Method 4: choose 3 ΔT's randomly within T; Method 5: stratify T into three subperiods T_1, T_2, and T_3, and then choose 1 ΔT randomly from each subperiod.

For the study time period 10:30 AM to 4:30 PM, the anglers left the fishing access site first at an increasing rate from the early morning until around 2 PM, and then at a declining rate after around 2 PM (Figure 2). The mean observed value of complete angler-trips from the 10,000 generated nonhomogeneous distributions of angler's departure times was around 80 with SD = 18 for this time period (Table 1).

The mean biases in the estimates of complete angler-trips from these nonhomogeneous distributions of anglers' departure times for the study time period 10:30 AM to 4:30 PM were -0.6, 0.6, 0.6, -0.0, and -0.2, respectively, for methods 1 to 5. The standard errors for these mean biases were 0.6, 0.6, 0.4, 0.6, and 0.3, respectively (Figure 3). All methods produced unbiased estimates of complete angler-trips. However, similar to the results observed for the study time period 8 AM to 2 PM, the estimate from method 5 was more precise than that from method 3, and the estimate from method 3 was more precise than that from method 1.

For the study time period 2 PM to 8 PM, the anglers left the fishing access site at a decreasing rate (Figure 2). The mean observed value of complete angler-trips from the 10,000 generated nonhomogeneous distributions of angler's departure times was around 58 with SD = 16 for this time period (Table 1).

The mean biases in the estimates of complete angler-trips from these nonhomogeneous distributions of anglers' departure times for the study time period 2 PM to 8 PM were -0.2, 13.9, 0.2, 19.8, and 0.1, respectively, for methods 1 to 5. The standard errors for these mean biases were 0.6, 0.6, 0.4, 0.6, and 0.3, respectively (Figure 3). Methods 2 and 4 produced higher estimated complete angler-trips than the observed value. Bias in the estimate of complete angler-trips was larger for method 4 than for method 2. Methods 1, 3, and 5 all produced unbiased estimates of complete angler-trips. However, similar to the results observed for other time periods, the estimate from method 5 was more precise than that from method 3, and the estimate from method 3 was more precise than that from method 1.

To compare the effect of nonhomogeneous anglers' departure times with the effect of homogeneous anglers' departure times on the estimate of complete angler-trips, we also estimated complete angler-trips from the homogeneous distributions of anglers' departure times (not shown) generated separately from the nonhomogeneous distributions.

The mean observed value of complete angler-trips from the 10,000 homogeneous distributions of anglers' departure times was all around 63 per replicate with SD = 17 for the time periods 8 AM to 2 PM, 10:30 AM to 4:30 PM, and 2 PM to 8 PM (Table 1). We estimated complete angler-trips from these homogeneous distributions of anglers' departure times for only 8 AM to 2 PM. Because the anglers' departure times are homogeneously distributed and the mean observed values of complete angler-trips from the 10,000 homogeneous distributions of anglers' departure times are about the same for all three study time periods, the results for the time periods 10:30 AM to 4:30 PM and 2 PM to 8 PM would be similar to those for the time period 8 AM to 2 PM. The mean biases in the estimates of complete angler-trips from these homogeneous distributions of anglers' departure times for this study time period were 0.0, -0.2, -0.4, 1.0, and 0.0, respectively, for methods 1 to 5. The standard errors for these mean biases were 0.6, 0.6, 0.4, 0.6, and 0.3, respectively. All five methods produced unbiased estimates when the anglers' departure times are homogeneously distributed. Similar to the results observed for the nonhomogeneous distributions, the estimate from method 5 was more precise than that from method 3, and the estimate from method 3 was more precise than that from method 1.

9. Discussion/Conclusions/Recommendations

Results from our simulation studies suggest that expanding the average observed counts per unit time during multiple randomly selected time intervals within the study time period by the duration of that study time period (methods 2 and 4) can lead to biased estimates of complete angler-trips when the distribution of anglers' departure times has a monotonic trend. In our simulation studies, methods 2 and 4 underestimated the true complete angler-trips when anglers leave the fishing access site at an increasing rate (i.e., the number of departures increases with time), and overestimated the true complete angler-trips when anglers leave the fishing access site at a declining rate (i.e., the number of departures decreases with time).

There are two randomly selected time intervals within the study time period in method 2. There are three randomly selected time intervals within the study time period in method 4. The bias from method 4 was larger than that from method 2 in our simulation studies, suggesting that the more interval counts, the larger the bias when anglers' depart at a monotonically increasing or decreasing rate.

Methods 2 and 4 produced unbiased estimates of complete angler-trips when there are both up and down in the rate of anglers' departure in our simulation studies. The unbiasedness in these estimates is, however, caused by that the negative biases and the positive biases cancelled each other out.

In our simulation studies, estimates of complete angler-trips from methods 1, 3, and 5 were unbiased when the distribution of anglers' departure times is nonhomogeneous. In method 1, there is only 1 randomly selected time interval for counting complete angler-trips within the study time period. In methods 3 and 5, the study time period is stratified into subperiods and there is 1 randomly selected time interval for counting complete angler-trips within each of the subperiods. For method 1, the estimate of complete angler-trips for that study time period is obtained by expanding the average count of complete angler-trips per unit time during the counting time interval by the duration of that study time period. For method 3 and 5, estimate of complete angler-trips for the study time period is obtained by summing the estimates of complete angler-trips for all subperiods. The complete angler-trips for each subperiod is estimated by expanding the average count of complete angler-trips during the counting time interval by the duration of that subperiod.

Although methods 1, 3, and 5 all produced unbiased estimates of complete angler-trips when the distribution of anglers' departure times is nonhomogeneous, the estimate from method 5 was more precise than the estimate from method 3, and the estimate from method 3 is more precise than the estimate from method 1. These results suggest that stratifying the study time period can reduce bias in the estimate of complete angler-trips. The technique of stratified sampling has been widely used in complex surveys. Stratification based on the distribution of the population can increase the precision of the population estimate (e.g., total or mean) when the distribution of the population is nonhomogeneous (Lohr 2010). The results of our studies show that stratification of the study time period will not only increase the precision of the estimate of complete angler-trips, but also increase the accuracy of the estimate when there is a monotonic trend in the distribution of the anglers' departure times.

In methods 3 and 5, we stratified the study time period into subperiods of equal length. Alternatively, the study time period can be stratified based on the feature of the distribution of anglers' departure times. For example, if the majority of anglers tend to leave during certain time interval within the study time period, we can use that time interval as a subperiod. However, implementation of this approach in a real survey is often difficult because it is hard to predict the distribution of anglers' departure times, and, consequently, the interviewer usually carries out the survey tasks based on the assignments scheduled ahead of time.

All methods considered in this report produced unbiased estimates of complete angler-trips when the distribution of anglers' departure times is homogeneous. However, like for the distribution of nonhomogeneous anglers' departure times, the estimates from methods 3 and 5 were more precise than the estimates from other methods. Also, estimate from method 5 was more precise than the estimate from method 3.

Assumptions on the distributions of the anglers' arrival times and length of time spent in fishing were made to facilitate generating various non-homogeneous distributions of anglers' departure times from the fishing access site. Although these assumptions are probably unrealistic in real world, they will, however, not affect our conclusion that the distribution of anglers' departure times affects the accuracy of the estimate of complete angler-trips. Stratifying the study time period based on the distribution of anglers' departure times will increase the accuracy and improve the precision of the estimate of complete angler-trips when the distribution of anglers' departure times is homogeneous.

We assumed in our simulation studies that all anglers who leave from the fishing access site during the selected time intervals for counting are counted. In practice, counting all complete angler-trips while intercepting some of them to obtain catch and effort data may be difficult for one interviewer on a busy day, especially for boat fishing mode where anglers tend to leave from the fishing access site together. In such a situation, two interviewers may be required with, possibly, one responsible for interviewing anglers and the other responsible for counting.

Our simulation studies considered only some special distributions of anglers' departure times from a fishing access site. However, it is safe to generalize our conclusion that stratifying the study time period based on the distribution of anglers' departure times will help increase the accuracy and improve the precision of the estimate of complete angler-trips to other nonhomogeneous distributions of anglers' departure times.

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Marine Recreational Information Program
FY-2016

Electronic Reporting in Survey Research Applied to Estimating Fishing Effort

Project: Electronic Reporting in Survey Research Applied to Estimating Fishing Effort

J. Michael Brick - Author, *Westat*

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11. Appendix

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Electronic Reporting in Survey Research Applied to Estimating Fishing Effort

J. Michael Brick

Westat

August 2, 2018

1. Introduction

Changes in the survey environment have resulted in lower survey response rates at the same time expectations have increased that newer modes of communication should improve the quality and convenience for respondents. This convergence has fueled a great deal of survey research over the past few years.

The National Oceanic and Atmospheric Administration's National Marine Fisheries Service (NMFS) is responsible for producing high quality and timely data for assessment and management of marine fish stocks. The NMFS' Marine Recreational Information Program (MRIP) collects information on marine recreational angling. In its 2006 report, the National Research Council (2006) suggested major changes in the random-digit-dial telephone methodology that was used to estimate fishing effort. The recommendations included, amongst others, using data from angler registries for sampling purposes and research on panels to collect data from the same sampled units over time. The National Academies (2017) reviewed the program and the methodological innovations in the MRIP about a decade later and found substantial progress had been made since 2006. This new review continued the call for more research into electronic data collection, noting "electronic data collection should be further evaluated as an option for the Fishing Effort Survey, including smartphone apps, electronic diaries for prospective data collection, and a web option for all or just panel members."

1.1 Estimating Fishing Effort

Fishing effort is the total number of recreational saltwater fishing trips taken during a specified time period. This total is the product of total number of persons in the population who took one or more recreational saltwater fishing trips during the period and the mean number of trips that they took during the period. Since the total population is known from Census Bureau data, this computation requires estimating prevalence (the percent of the population that fish) and the mean number of trips of those who did fish. These two components – prevalence and mean number of trips – are the target quantities in the design of off-site fishing effort surveys.

When the original National Research Council review was undertaken in 2006, a landline telephone survey was used to make these estimates. The report identified the low response rates and coverage rates of the telephone survey as being problematic. An additional criticism of the telephone survey in 2006 was that it did not take advantage of auxiliary data on fishing such as state fishing license data files.

The MRIP conducted a series of experiments testing alternative methods of collecting effort data in the years following the review. Those studies showed that a mail survey had much better response and coverage rates than the telephone survey. They also devised a method to use data from state fishing license files to enhance the precision of the estimates. The survey methodology that emerged from this research program is called the Fishing Effort Survey (FES). After a transition period, the FES became the standard fishing effort survey of MRIP (for estimating the numbers of shore and private boat fishing trips made by anglers) beginning in 2018.

1.2 Structure of Report

This review discusses two options that MRIP could explore to address recent advances in data collection methods. The first option looks at electronic data collection using non-probability sampling approaches. Non-probability methods have increased in interest primarily due to the low cost of collecting data over the Internet from large numbers of persons. Traditionally, non-probability samples have not been acceptable for most federal government surveys[1]. The second option is to use probability sampling methods, but to use different modes and designs that will allow greater use of electronic reporting by respondents.

Our review separately examines both the non-probability and the probability options. The actual data collection methods for both options greatly overlap since both make extensive use of the Internet. However, the two options are distinct in how they use the data to make inferences to the population under study. The non-probability option must rely on model assumptions to make inferences, while the probability sampling option uses the sample design, adjusted by model assumptions to handle missing data, as the basis for inferences. Hybrid designs (Fahimi 2015) that attempt to combine small probability samples and larger non-probability samples have been suggested, but there is very little research that can be used to discuss hybrid designs. The next section gives some background that is the basis for the following sections.

2. Background

2.1 Probability Samples

The standard paradigm for producing high quality estimates of finite population statistics starts with selecting a probability sample from a well-defined sampling frame, and then uses design-based inference methods based on these selection probabilities. This approach is built on the framework originally described by Neyman (1934) and has been used for the production of official statistics in the United States and almost all countries for decades. The probability sampling paradigm has an accepted theoretical foundation that has been expanded upon over the years to deal with imperfections such as incomplete coverage and nonresponse. These weighting adjustments or 'fixes'

[1] OMB Statistical Standard 1.2 states that "Any use of nonprobability sampling methods (e.g., cut-off or model-based samples) must be justified statistically and be able to measure estimation error."

to deal with missing data due to nonresponse and incomplete coverage are model-based (in that they assume a model such as the response propensity within a weighting class is constant). Generally, the assumption boils down to assuming the data are missing at random (MAR). For example, a common nonresponse adjustment involves creating weighting classes and assuming all the units in the weighting class either have the same response rate (response homogeneity) or have the same distribution of the outcome variables. If either of these assumptions holds, then the estimates are unbiased (Särndal, Swensson, and Wretman, 1992) when the sample size is large. Even with modest deviations from these assumptions, the adjustments remove much of the bias in many applications (Särndal and Lundström, 2005).

The probability sampling paradigm has been very successful, at least in part due to attributes that make it especially applicable for official statistics. One of the most powerful attractions of this approach is that the sampling and inference procedures are objective, thus avoiding many of the opportunities to inject biases based on the views of the researchers conducting the survey. Even with departures from the idealized structure due to nonresponse and coverage errors, standard procedures have been implemented in most probability samples to address these deficiencies that are relatively objective (Brick 2011).

Probability samples do not have to employ the same data collection methods that have been used for decades, primarily mail, telephone and face-to-face interviews. In fact, probability samples have embraced new technologies over time, especially the computerization of the systems of data collection. Research in moving more of the data collection effort to the Internet has been very active in the last 10 years or so (Tourangeau et al. 2017).

An example includes commercialized panels of respondents (commercialized means that the vendor sells access to the panel for a fee) who respond to surveys by the web. The first in the U.S. to do this was the KnowledgePanel of GfK, which has been around for almost 20 years. More recently, NORC introduced a panel called Amerispeak. Both of these panels recruit a probability sample of household members and then ask them to respond to a variety of surveys over time using the Web. When an organization wishes to do a survey, the organization pays the vendor to request some or all of the panel members be sent the survey to complete. Thus, the panel members may respond to multiple surveys developed for different organizations during their tenure in the panel.

Another example is the use of mobile apps (software deigned to on a smartphone) or Web apps (software designed to run on Web browsers) to collect data from respondents in probability samples. The FoodAPS project is an example of a project for the Department of Agriculture exploring the use of Food Log, a web-based tool that can be accessed by a Web browser or an app (the app is only available for a tablet and smartphone device). In this project, interviewers visit sampled household and do screening, an initial interview, and train respondents for responding to a diary of food consumption. Household members can request a loan of a smartphone and/or a barcode scanner (to scan food-packaging barcodes). A sampled household records all foods acquired for one week. At the end of the data collection week, interviewers go back to the sampled household, conduct a final interview, collect any loaned equipment, and give out incentives. Daily text/email reminders are sent to sampled members thanking those who participate and informing them of the amount of incentive accumulated. For those who didn't complete the daily Food Log, the messages reminded them to complete the Food Log to keep earning incentives.

The Bureau of Labor Statistics (BLS) is exploring a similar approach for replacing its paper diary that collects data on consumer expenditures. BLS decided that an app was not appropriate due to privacy and confidentiality concerns but has a Web app (that runs on a browser) diary that closely resembles an app when used on a mobile device (but does not require respondents to download an app).

2.2 Non-probability Samples

Although probability samples have been used in most official statistics, other strategies that do not rely on probabilities of selection have been used in many commercial and in some official establishment surveys (Stephan and McCarthy, 1958; Knaub, 2007). Collectively, such designs are often designated as non-probability samples (Baker et al., 2013). Non-probability samples have become increasingly common in recent years, as organizations have taken advantage of the ability to attract people to respond by using opt-in volunteer sampling methods. Baker et al. (2010) and Callegaro et al. (2014, Chapter 1) describe techniques used to intercept and enroll people to respond to surveys for opt-in Internet designs. The key and novel feature of internet non-probability samples is the ability to obtain large numbers of respondents very quickly and inexpensively.

For example, the cost to complete an interview of say 5,000 respondents from an existing vendor of non-probability samples on the Internet may be roughly 1 percent of the cost of a telephone survey and well less than 0.5 percent of a face-to-face survey. These cost estimates vary greatly depending on the vendor, the type of survey, and many other factors. Very little information on costs for these surveys is available in the literature.

Furthermore, the data collection often takes less than one week, while telephone and face-to-face interviews often stretch over months. This huge difference in costs and speed of collection is the reason non-probability sampling on the Internet has generated so much interest. More specifics about online, opt-in samples are presented in the next section.

Non-probability sampling theory is not as developed or consolidated, and many applications are largely a-theoretical. In these cases, the idea seems to be that a large sample size, regardless of how it is obtained, is sufficient to provide "good" estimates. The very low cost of the samples is what makes them attractive.

Researchers who have examined the properties of non-probability samples suggest selection bias is the most substantial contributor to errors (Bethlehem 2010). Some researchers have explored the theories of matching and estimation from observational studies to address selection bias. Rivers (2007) proposed matching in the selection of the non-probability respondents to the survey, like that used in epidemiological studies. Rivers sampled from a large, representative probability sample (an already completed government survey with standard sampling weights) and then selected Internet cases that matched the characteristics of the probability sample (usually demographic characteristics). This method is also related to quota sampling, where predesignated targets of the number of respondents are set up and data collection ends when these numbers have been reached. For example, the targets for age by sex groups might be used as the quotas.

In addition to demographic variables, some have explored other types of variables for matching and for weighting. Fahimi (2015) suggested using behavioral and attitudinal measures such as the use of coupons in shopping and the number of hours spent on the internet. Weighting methods such as poststratification, raking, and propensity score adjustments are sometimes used (Lee and Valiant 2009; Brick 2015). With a matched sample as suggested by Rivers (2007), the non-probability respondents could be assigned the weights from the probability sample (so the non-probability respondents are essentially substitute respondents to a probability sample). Mercer et al. (2017) explores the relationship between non-probability sampling and causal inference, and Elliott and Valliant (2017) describe weighting methods. Despite these types of investigations, practitioners have not accepted a standardized approach to sampling or estimation theory for non-probability samples (Buelens, Burger, and van den Brakel 2018).

Computing precision estimates from non-probability samples is also somewhat controversial, with some advocating no precision estimates should be provided while others have explored alternatives such as Bayesian credible intervals. As a result, many different variants exist and comparing them is difficult.

2.3 Online Non-probability Samples

Online non-probability surveys are very common, with sources suggesting that the majority of surveys, many of which are commercial market research surveys, are done online (Callegaro et al. 2014). Baker et al. (2010) provide a nice summary of the methods used to construct the samples for both one-time surveys and panels. They describe five activities:

- recruitment of members,
- joining procedures and profiling,
- specific study sampling,
- incentive programs, and
- panel maintenance.

The recruitment of members is a key component and is essentially the counterpoint to sample selection in a probability sample. The methods may be very selective (recruitment campaigns using advertising on specific websites or even using offline advertising) or more general depending on the target sample. Usually a contingent incentive or sweepstakes is used to attract persons. Some use a technique called co-registration agreements. Essentially, a website compiles emails of their visitors through a voluntary sign-up process and "monetize" this list by selling the emails. Others use "affiliate hubs" – sites that offer access to a number of different online merchants. Still another method relies on search engines, where a company buys ads that appear alongside search engine results hoping visitors will agree to do a survey. A less frequently used method is to request people they have already recruited to ask their friends or relatives to join, where they offer some award if the member adds others (this method is related to snowball sampling and respondent-driven sampling in some ways).

River sampling is sometimes considered a different approach, but only because it is done in real-time. Baker et al. (2010) define it as usually recruiting respondents when they are online, so it is related to the other methods using ads mentioned above. However, river sampling generally does not involve panel construction, but simply tries to encourage people to go to do some survey as they are surfing the Web. Those who do river sampling seldom have access to the demographics of the visitors, and so they rely on other companies to make the survey invitations.

Another option that straddles the border between a one-time and panel survey is an aggregator. This type of company works to share respondents from various sources (usually panel members) with those doing a survey. An aggregator can provide a list of email addresses that can be used to recruit respondents. While some of the characteristics of the members may be known along with the email address, the survey is essentially a one-time survey from the perspective of the researcher.

After the person has expressed an interest in taking a survey, a double opt-in process is almost always required to avoid surveys being done by computers. Double opt-in requires the person to sign up to do the survey and provide an email. The person then must take some positive action such as providing information sent to the email to get into the survey.

Once the person agrees and gets through the sign-in procedures, most surveys use some form of quota sampling to determine who should complete a specific survey. River sampling methods may route respondents to various surveys based on the (multiple) survey quotas. The quotas for many surveys are client specified and may involve demographics or other criteria, although most one-time surveys do not involve complicated quotas.

After recruitment, online probability panels (discussed in Section 4) and non-probability panels only differ materially in terms of follow-up procedures. Probability samples often use reminders and follow-ups to boost response rates; these methods may be expensive (incentives etc.) and take a bit more time. Non-probability surveys generally do not use follow-up methods and rely on having enough cases come through the door to complete the survey. The survey is done when the targeted number of respondents (meeting the quotas) has been satisfied. Often, the time required is a matter of a few days.

Most online, non-probability sampling vendors offer both one-time surveys and surveys from panels. The main difference between one-time non-probability samples and panels for most surveys is that panels usually capture profile data that allow them to subset to the appropriate subgroup needed for a particular survey. These profile data are sometimes used to match respondents as discussed in the previous section. For example, YouGov uses its profile data in this fashion for some of its applications.

While probability samples and panels usually use design-based weighting and variance estimation methods to produce estimates, many opt-in samples do not use weights at all. Brick (2015) describes a model-based survey framework that attempts to address weighting issues. He provides approaches for evaluating the assumptions underlying the models and associated weights. Other statistical models including likelihood and Bayesian methods (Wang et al. 2014) could be used, but examples under these models are rare.

The key strengths of online, non-probability sampling methods are its low-cost, very speedy data collection, and ability to obtain a large number of respondents. The measurement properties of the data collection are roughly equivalent to those from probability samples (online surveys have the advantage of avoiding interviewer effects and the ability to offer a wide range of visual displays).

Statistically, non-probability sampling has serious limitations that apply to both one-time samples and to panels. Baker et al. (2010) cautioned that the method should not be used for making population estimates ("Researchers should avoid non-probability online panels when one of the research objectives is to accurately estimate population values."). Bethlehem (2010) echoes this sentiment. Baker et al. (2013) are more nuanced in their discussion of the potential use of non-probability samples ("Non-probability samples may be appropriate for making statistical inferences, but the validity of the inferences rests on the appropriateness of the assumptions underlying the model and on how deviations from those assumptions affect the specific estimates.")

A major problem is that the methods used to recruit respondents for online non-probability samples are highly variable and subject to rapid change (Craig et al. 2013). Even using the "same" method, say placing ads on specific websites, will not necessarily result in the same diversity of respondents over time. The traffic to websites is highly dynamic. Aggregators chose different sources based on availability. Even things such as search engines are continuously being revised and these revisions may have consequences for making estimates, but they may not be obvious. For example, early work using the Google Search engine suggested it could be an early indicator of the severity of flu in the U.S., but this predictability evaporated when the internal mechanism of the search engine was modified unbeknownst to the researchers (Lazer et al. 2014).

2.3 Comparing Probability and Non-probability Samples

Several empirical studies have been conducted over the last 10 years or so to assess the performance of non-probability samples. Some early work (e.g., Yeager et al., 2011) compared probability and non-probability sample estimates to benchmarks and generally found the probability sampling estimates were more accurate. A more comprehensive and up-to-date review of comparisons is given by Callegaro et al. (2014, Chapter 2). Their review suggests that probability sampling, even with relatively low response rates, gives estimates that are closer to benchmark values than non-probability samples. These results are not consistent across all types of estimates and the differences may not be meaningful in some cases.

The comparisons for other statistics such as measures of association and trends are more ambiguous, partially because there are few reliable benchmarks for comparison. Most researchers have simply compared association or trend measures for non-probability samples to those from probability samples. Callegaro et al. (2014, Chapter 2) includes a review of the few published studies that look at measures of association. The results are less consistent than point estimates across the sampling methods. Some studies note that while there are differences, they are not large enough to influence policy decisions. For example, see Miller et al. (2010). For trends across time, there is scant methodological research evaluating the quality of trends. Some argue that theoretically trends and measures of association should not be very different whether estimated from probability or non-probability samples. This is clearly a conjecture and the conditions under which this holds have not been stated clearly. Nevertheless, the Centers for Disease Control have used non-probability online panels to assess trends for certain rare groups (e.g., Ding et al., 2015).

3. The Non-probability Option for Fishing Effort Surveys

This section begins with a discussion of online, non-probability sampling methods that could be used to survey anglers to estimate recreational fishing effort. We then discuss an extension of this type of design to the general concept of citizen science (similar to crowd-sourcing concepts) and the use of smartphone apps for collecting data. Mobile and Web apps are a data collection method rather than a sampling method, but the current literature does not clearly make that distinction.

3.1 Estimating Fishing Effort with Online Non-probability Samples

Selection bias is the major concern in most non-probability samples, and it would be a very serious issue in estimating fishing effort with an online non-probability sample. Research into models attempting to deal with selection bias thus far have been generally unsuccessful. Propensity models and propensity score weighting adjustments attempt to deal with selection bias by modeling those that have access to the Internet (Lee and Valliant, 2009) or those more likely to use the web heavily. Most of the research suggests selection bias is more complex than just a coverage issue, so only dealing with access to the Internet is insufficient. Furthermore, just being on the Internet often (as most respondents to non-probability surveys are) is not very predictive of being a respondent to non-probability sample recruitment.

Modeling participation in a survey without the type of active recruitment used in probability sampling is extremely difficult with few, if any, examples of this being done effectively. The problem is magnified for a fishing effort survey because angler surveys are more likely than many other types of surveys to suffer from avidity bias. We define avidity bias in this context as the overestimation of fishing prevalence that results when anglers are more likely to participate in the survey than non-anglers are. Groves et al. (2006) demonstrated the potential bias due to avidity in a birding survey experiment with a probability sample. In that example, people who were birders participated more than those who were not. In a non-probability sample without active recruitment and materials to encourage all people to respond, it is very likely that the selection bias would be a very serious problem.

A panel non-probability sample could be used, but the main benefit of a panel is the availability of profile data. Since both anglers and non-anglers are needed to estimate the percent of the population that fished, profile data that classified people as likely anglers is expected to have little value. Furthermore, the online panel respondents would be aware of the nature of the survey and avidity bias would be problematic. Respondents tend to be more willing to complete surveys on topics that they are familiar with or interest them.

The focus so far has been on selection bias due to avidity even though the effort estimate has both a prevalence component and a mean number of trips of those who did take trips. The earlier redesign efforts for the effort survey using probability sampling and different modes following the 2006 review showed that prevalence was much less stable than the mean number of trips of anglers. Prevalence was affected by the sampling frame, mode, and questionnaire while the mean number of trips per angler was not. As described above, we suspect this relationship may be exacerbated for non-probability samples, and the rationale for the avidity bias effect on prevalence has been provided. Our hypothesis is that the mean number of trips would be relatively robust for non-probability samples because it is conditional on having fished in the time period. This is just an hypothesis because no evaluation has been conducted with a non-probability sample.

An alternative approach that could be used to attempt to deal with avidity bias in a non-probability effort survey is to model the outcome variables of interest, given the set of respondents. There are two major reasons why this approach may be problematic. First, assuming the model holds over all people rather than just the respondents to the effort survey is tenuous; this assumption is not testable in most cases because we only observe the respondents. The respondents to a non-probability sample are not likely to be similar to the whole population, especially due to the avidity biases. Second, the modeling requirements for non-probability surveys can be extensive and rely heavily on good auxiliary information to be effective. Fishing register or license data are the only related source of such auxiliary information (demographic data typically are not very useful for modeling effort). While the fishing register data has been shown to be very valuable in sampling for the FES, it has serious limitations for use in modeling fishing prevalence. In particular, some states have very different rules and enforcement about who can fish and what the consequences are for recreational fishing without a license, so models would have to be very local. Such models would be subject to the same types of problems as the Google Flu example discussed earlier because local changes in enforcement or procedures for processing the data could have a big effect on the estimates.

Another major concern is the subjectivity that would be inherent in the modeling. The effort estimate has substantial consequences and interested parties could propose different model assumptions that result in estimates that more align with their interests. Since most of the modeling assumptions cannot be tested effectively, this could make it difficult to defend the estimates. Even if standard methods were developed for producing estimates from non-probability samples, there would be potential legitimate disagreements about those methods.

Producing an estimate without substantial bias is difficult with non-probability samples, but estimating the precision of the estimate is even more problematic. The usual, design-based estimate of variance is not appropriate because the sample is not selected with known probabilities. Without a measure of precision, it is impossible to assess whether policy decisions from the survey data are based on random error or the true measurement of fishing effort.

Another troubling issue is that bias, either selection bias or nonresponse bias, is likely to be a large component of the error. In this situation, estimating accuracy or mean square error is preferred to estimating variance. This estimation task is even more difficult because biases are so hard to estimate from all types of surveys. Unfortunately, most non-probability samples are not designed to give any estimate of bias or variance. If they report anything, it is usually just the variance computed as if a simple random sample had been selected. This approach is misleading.

A very different approach was suggested by Liu et al. (2017) that utilizes a non-probability sample and a probability sample to estimate catch (the method could be revised to estimate effort). Liu et al. (2017) describe this method in terms of a capture-recapture design. A large (non-probability) sample is 'captured' and report about their trips. A probability sample of anglers is conducted and the percentage of respondents in the probability sample who are 'recaptured' (were reporters in both samples) is estimated. Under some conditions and assumptions about response, unbiased estimates of the number of trips could be computed using both the non-probability and the probability sample.

The authors of the article consider the method within the context of estimating catch rather than effort, but the extension is feasible at least in theory. Several important conditions would be very hard to satisfy. One issue is the requirement for independence between the capture and recapture samples. It is not clear that the volunteers who respond (see next section on apps for more on a way to generate self-reporters) would be willing to respond to a second survey about their effort if sampled in the probability sample. It is also unclear whether the volunteer non-probability sample would be large enough to support this type of procedure. Another key issue is the method of matching the persons who responded to each survey. If this matching is not simple, the measurement error in matching could result in substantial bias. Liu et al. (2017) discuss ongoing evaluations of the method for catch that may provide some insights into these concerns, and we recommend waiting for results from these studies before trying to adapt this method for effort surveys.

3.2 Angler Apps

With the proliferation of smartphones and apps within the last decade, some researchers have been exploring using data from angler apps to provide estimates for marine recreational fishing activities. This approach to data collection often is categorized as citizen science, where citizens directly participate in various aspects of science. Citizen science covers a wide variety of projects engaging the general public or citizens in the practice of science. The types of activities range from participatory action research to large Web-enabled efforts. Examples include bird monitoring and the search for planets (Crain et al. 2014; Newman et al. 2012).

The use of apps for monitoring various forms of recreational fishing activities has been explored. Venturelli, Hyder and Skov (2017) discuss the utility of angler apps and the challenges associated with using them for monitoring activities. They define angler apps as mobile apps "that allow anglers to record, share and network" their activities. They provide thoughts on the status of this approach and suggest that creating standards and guidelines is important to take advantage of apps. The first challenge they identify is recruitment and retention of the citizen scientists to collect the data. For recruitment, they suggest making the apps "easy, fun and social." This approach is consistent with gamification that has been used to increase interest in many different fields, especially training (Hamari 2014). Keusch and Zhang (2017) examine the effects more specifically for online surveys and show that the benefits, while generally positive, are not very clear.

For producing effort estimates, any app also needs to appeal to those who do not fish recreationally since the key estimate is the percent who fished in the given time period. This issue is not discussed in any of the literature we reviewed and is a critical weakness in estimating fishing effort using this method. Venturelli, Hyder and Skov (2017) also suggest ways for improving retention of those who do use the app at least once. Here again the approach is likely to have serious disadvantages; the authors note that only 5 percent of those who begin to use the app still use it after 3 months. If even anglers do not persist in using the app its use in this context for estimating fishing effort is very dubious.

While the use of apps for other purposes may be reasonable and appropriate (Papenfuss et al. 2015; Jiorle, Ahrens and Allen 2016), our review shows this method is ill suited to the estimation of recreational fishing effort. Even if the improvements suggested by Venturelli, Hyder and Skov (2017) are eventually accomplished, it is extremely difficult to see how data from a fishing app will be useful to estimate the percent of the general population who take recreational saltwater fishing trips in a specified time period. This conclusion does not imply that fishing apps have no value for estimating fishing effort. In the next section, we discuss the possibility of using apps for data collection when respondents are recruited through a probability sampling approach.

4. Probability Approaches Using Electronic Modes

As noted previously, the empirical evidence shows that probability samples produce estimates that are closer to benchmark estimates than those from non-probability samples, although in some cases the differences are not that large. Because of the need to obtain responses from both anglers and non-anglers for effort surveys, avidity could easily bias both probability and non-probability samples. It is likely that the active recruitment and follow-up procedures used in probability samples, which are atypical in non-probability samples, gives a major advantage to probability sampling.

The current fishing effort survey uses probability sampling, but there are methods that could be researched within this paradigm that might enhance the survey. Some research into these methods are being investigated currently, but the field tests are planned for fall 2018 so no results are yet available. The applicability of some of the new methods depend on whether a cross-sectional design (one-time) or longitudinal design is used. We begin with cross-sectional designs and then discuss longitudinal designs.

4.1 Cross-Sectional Probability Designs for Fishing Effort

NMFS began research on a mail survey to collect data on fishing effort following the NRC report in 2006. The mail survey replaced the decades old random digit dial telephone survey of landline households, the Coastal Household Telephone Survey, completely in 2018. The evidence from the research shows the mail methodology has major response and coverage advantages over the telephone survey. Other mode changes, largely within the current cross-sectional, probability design, are being studied in an effort to further modernize and reduce data collection costs. In particular, MRIP is planning tests for using the Web to collect data.

Dillman (2017) summarizes his view of the future direction for using newer modes, primarily the Web, for data collection from cross-sectional probability samples. He also outlines some of the most important challenges. One approach is to use address-based sampling (ABS), as used in the mail FES, to sample households and then mail materials urging the respondents to go to the Web to complete the survey request. If this Web-push is followed by nonresponse attempts to have the household complete a mailed instrument, then it can be categorized as a mixed mode, Web-push survey.

Dillman (2017) talks largely about these mixed mode surveys, but he notes that mail instruments still result in the highest response rates. The MRIP research conducted following the 2006 also investigated mixed mode surveys, but MRIP chose the mail only approach because it had the highest response rates and coverage rates and generally had lower nonresponse bias than mixed mode methods. In particular, the option to offer respondents a choice of whether to respond by Web or by mail almost always depresses response rates (Medway and Fulton 2012) and was not deemed to be best practice for the fishing effort survey. Even though mail is still considered the best mode for data collection, Dillman (2017) highlights that changes in society are making mixed mode options more attractive. We start by discussing a Web-only design, and then move on to a Web-push design.

The Web-only design samples addresses from the ABS frame (derived from the United States Postal Service delivery sequence files) and mails materials to the sampled addresses urging the household to go online to complete a survey over the Web. If high response rates are desired, then multiple requests and monetary incentives are essential.

Web-only designs are often unable to achieve response rates that are sufficiently high for most sponsors. The lower than desired response rate is especially problematic because most studies show that Web-only survey respondents are not balanced leading to nonresponse bias in the estimates (Messer and Dillman 2011). The respondents tend to be younger and more highly educated than the general population.

Despite these concerns, several surveys have used a Web-only design, especially when funds for conducting the survey are limited. In 2016, Westat conducted a Web-only survey called the American National Election Survey (ANES) where funds were available to offer relatively large monetary incentives (both prepaid and promised) to boost response rates. The response rate for this 45-minute (on average) survey was 44 percent. The details on the methods and incentives are contained in the ANES methodology report[2]. The incentives for the ANES are very unlikely to be acceptable to OMB or MRIP for the administration of a fishing effort survey. The vast difference between the burden of the ANES and the FES would also argue against using such large incentives. However, the ANES experience does suggest that a Web-only design could achieve acceptable response rates and sample balance in terms of respondents if monetary incentives of sufficient size could be used. Most of the literature on experiments on incentives in surveys is for mail and telephone surveys, but that evidence shows that response rates increase at a lower rate as the amount of the incentive increases. The NMFS studied the effects of incentives for mail surveys and found that $1 and $2 pre-paid incentives raised response rates significantly and were cost-effective (lower overall cost per respondent compared to no incentive). See the appendix for a summary of these findings. Future research on the appropriate amount of prepaid and promised incentives for a Web-only or Web-push design is an area of research that could be informative.

Another feature of the ANES Web-only design was that the instrument was designed to allow respondents to use their smartphone. With a lengthy survey like the ANES, smartphones are often not the preferred mode of response, but since some respondents will only respond using a phone, this seems to be an essential design component. The fishing effort data requirements are much lower than the ANES, but still pose some challenges for smartphone response. As mentioned earlier, NMFS is exploring smartphone data collection options in the planned field test.

The mixed mode, Web-push design begins exactly like the Web-only design, but after one or more attempts to push the household to respond online, a mail questionnaire is sent to increase response rates. Many surveys are currently using this approach. The National Household Education Survey,

[2] www.electionstudies.org/studypages/anes_timeseries_2016/anes_timeseries_2016_methodology_report.pdf

which was a model in many ways for the FES in converting a telephone survey to a mail survey, tested this approach in 2016 (see its methodology report[3]).

The Web-push design is fast becoming the new default for cross-sectional surveys. It utilizes the Web to capture data inexpensively at the first phase and then tries to increase response and decrease bias by following up with mail. Generally speaking, the method does not suffer from differential mode effects because both the Web and mail are self-administered and visual modes. MRIP is planning to explore this option and is planning to field test in these areas.

4.2 Longitudinal Probability Designs

A longitudinal survey design opens new possibilities for data collection that are not as feasible with a one-time cross-sectional survey. The typical justification for most longitudinal surveys is the capability of producing estimates that cannot be estimated well from cross-sectional surveys, especially estimates of change. Some of the types of estimates of change of interest are spells (e.g., how long were people unemployed), estimates of gross change (e.g., how many people moved into and out of a job during a time period), and precise estimates of net change (e.g., by retaining sample members the estimates of change in employment status are more precise due to the positive correlation in the reported employment status over the time periods). The fishing effort survey is not required to produce these types of estimates, yet some potential advantages still exist for a longitudinal design. These potential advantages include:

- Obtaining a larger proportion of the respondent set who are likely to fish while still being able to produce unbiased estimates of the percent of the population that fished.

[3] nces.ed.gov/pubs2018/2018100.pdf

- Encouraging a larger proportion of the respondent set to respond by electronic means (Web or app).
- Reducing the cost of data collection.

These potential advantages could arise, but they are not guaranteed. Additional research is needed to determine the effectiveness of a longitudinal approach. Below, we outline the longitudinal design that we anticipate would be most likely to achieve some or all of these goals.

Most longitudinal surveys require substantial effort at both recruiting new sample and sample maintenance (keeping the sample members responding and avoiding attrition bias). A rotating panel design in which new members are recruited for each wave of data collection and some existing panel members are removed from the panel provides a balance between these requirements and is recommended for the research.

Figure 1 gives a picture of a rotating panel design. Each row represents a set of respondents coming into the panel (in this case the address is the sampled unit rather than the persons living at the household at a particular time) and a column represents a wave of data collection. In this diagram, each set of respondents (responding households at the sampled addresses) comes into the panel and remains for four waves of data collection and then exits. By the fourth wave, the panel has reached its steady-state with an incoming set and three retained sets of respondents. As described below, a subsample of respondents can be retained rather than retaining all sampled households for four waves.

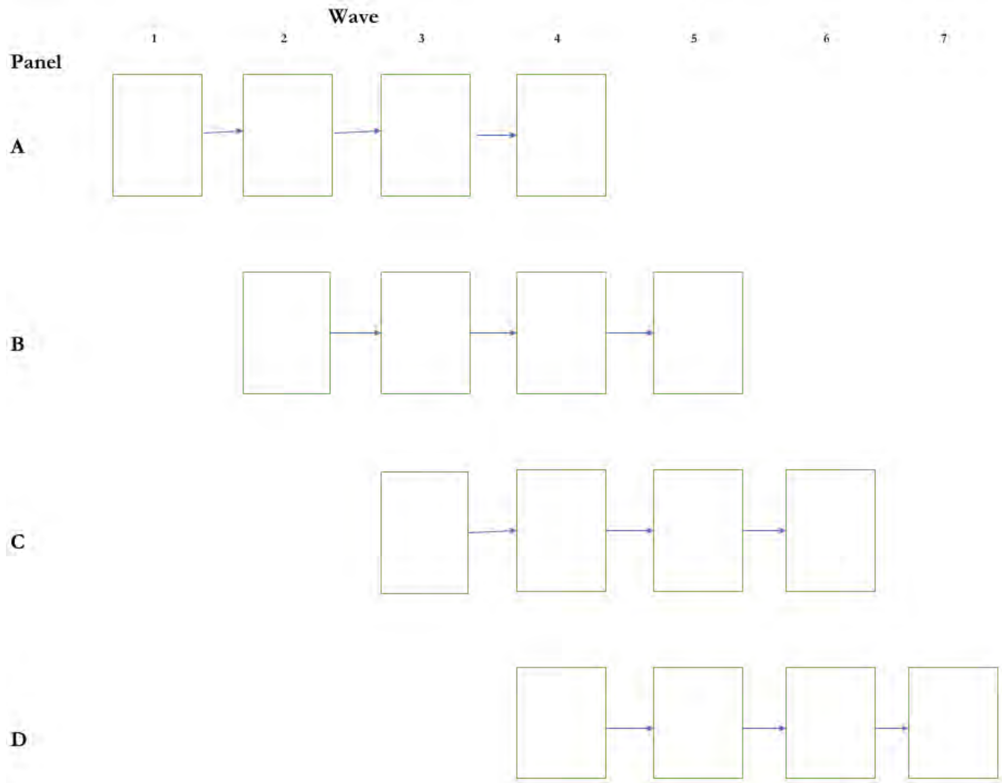

Figure 1. Rotating panel design schematic.

The key features of the particular rotating panel design suggested for the fishing effort survey are:

- In each time period, the sample would include an incoming or base wave of addresses to recruit new sample members, while also retaining a sample of respondents (addresses not people or households) from previous waves; respondents can be surveyed for a total of 4 waves (3 longitudinal follow-ups); all respondents who have completed 4 waves of data collection are dropped from the sample.
- The incoming wave is recruited using a Web-push design, and emails and text contact data are obtained from the household for subsequent rounds.
- A subsample of respondents from the incoming wave is retained for the subsequent three follow-ups; households with anglers and those who respond electronically can be all retained for follow-up and a subsample of the others are retained for follow-up. This both reduces costs and increase the sample size of anglers.
- Once the subsample is selected for the first follow-up, they are retained for the second and third follow-ups provided they respond; research on retaining subsampled households that fail to respond to the follow-ups in subsequent follow-ups should be conducted.
- The design can also be modified to include the households who are matched to the license registry data. These households are already much more likely to have anglers, so a possible scheme is to retain all of those households for the follow-ups regardless of fishing activity or mode of reporting. If this procedure is implemented, then a much smaller incoming sample is needed each wave to replace nonresponding and exiting panel members.

The effort data collection is relatively simple in terms of length and flow compared to many other surveys, although it has complexities in measurement such as recall of trips, the placement of the trips in time, and reporting for all adults in a household. Nevertheless, the survey is well-suited to electronic data collection and potentially the use of smartphones as a response mode. The Web-push design encourages sample members to respond electronically, and allows for the capture of contact information for those who respond by mail to the incoming wave so that they can be converted to electronic modes for the follow-ups.

Response rates to the incoming wave and retention of the subsampled households for the follow-ups is always an issue in longitudinal designs. A previous experiment with a mail longitudinal survey to estimate annual fishing participation rates suggests a reasonable response rate can be achieved with the rotating panel design (Andrews et al. 2016). Based on the experience with the ANES, we believe that an incentive program would greatly enhance the response rates to the study. We would encourage both a nominal prepaid incentive with the initial Web-push and a promised incentive for completing the survey. For example, a $2 incentive in the initial mailing and a promise of $5 for completing the survey (and each of the follow-ups) would probably both increase response rates by 10 percent or more and be cost effective. Lower response rates in the incoming and follow-up waves means larger numbers of addresses have to be sampled and surveyed and this increases costs. Research could be conducted to determine the best combination of pre- and post-paid incentives for the effort survey.

The earlier NMFS research with the mail longitudinal survey showed that initial or base wave respondents continued to respond to the follow-up surveys at roughly the same rate irrespective of whether they fished or not at the base wave. This result is very encouraging because it shows that the longitudinal design may help reduce avidity bias or at least not increase the bias. The wave estimate of the prevalence is a weighted combination of all the respondents to the particular wave, so it combines both initial respondents and the follow-up respondents. Of course, the follow-up respondents would be weighted to account for any subsampling for the follow-up that might be done.

An attempt was made to estimate the costs for the rotating design described above. We concluded that the data collection costs for the rotating panel design could be lower than for a repeated cross-sectional survey[4]. Clearly, these assumptions need to be examined empirically. The costs also depend on the subsampling rate for the follow-up. The precision of the estimates of effort is affected by the subsampling. A full statistical evaluation of these effects is needed to inform the design.

Furthermore, rotating panel surveys introduce other costs that are not part of the data collection costs per se. For example, the additional complexity of the design has implications for estimation procedures and the possibility of different types of measurement error (conditioning effects) that may require spending money that would not be spent in cross-sectional designs. Another cost that is not considered is the cost associated with changing from one design to another. When the effort survey transitioned from a telephone survey to a mail survey a series of activities were undertaken to aid in that transition including running the systems in parallel for a time and analytic studies to calibrate the estimates from the two designs.

4.3 Other Longitudinal Probability Designs Options

We also considered the use of an existing probability-based panel such as KnowledgePanel run by GfK or Amerispeak by NORC. These are general-purpose panels and do not appear to be suited for the needs of a fishing effort survey. For example, the Amerispeak panel currently has only about 10,000 members and would be too small for the state-specific needs of a fishing effort survey. The response rates for both panels are also much lower than NMFS might desire. Perhaps even more

[4] Note that the longitudinal survey used to model the costs was done completely by mail. The lack of experience using electronic data collection adds considerable ambiguity to these cost estimates.

importantly, if NMFS plans to continue to monitor fishing effort on a continuous basis, having a panel specifically designed for its own purpose is more appropriate and cost-effective.

A major advantage of having a panel specifically for fishing effort is that, assuming adequate numbers of people can be enlisted as panel members, some of the objectives of improving measurement identified in the National Academy studies can be explored beyond the reporting of data electronically. For example, the smartphone app could be tested in a survey environment where avidity bias does not severely limit the potential advantages of an app. The smartphone app has capabilities such as prompting the respondents each week to identify any eligible trips. The benefit of this would be to address recall bias. Another, albeit currently less feasible option, is that panel members who use other existing apps might be able to link directly to that data to capture data needed for the fishing survey. A longitudinal probability sample design that controls the potential bias due to nonresponse opens the door to making these improvements.

5. Summary

This review has examined methods for accomplishing some of the modernization goals that the National Academies (2017) encouraged in their study of MRIP's surveys of fishing effort. In particular, they called for more research into electronic data collection, including smartphones, electronic diaries, and a web option for all or just panel members. Our review separated the sampling for fishing effort surveys into probability and non-probability samples, with variations within these categories.

Non-probability samples can be characterized as data capture systems that amass responses without a selection scheme that give each unit in the population a known likelihood of selection. As a result, the design-based inference procedures are not valid with non-probability designs. Statistical models must be used to make inferences from non-probability samples.

Two types of non-probability samples that could be used to estimate fishing effort were examined in Section 3. The first is the online, opt-in non-probability sample that has been examined critically by many researchers (e.g., Baker et al. 2013). In most of these opt-in, online samples, selection bias is the major concern. Essentially, the lack of control of the process for recruiting respondents results in biases in the estimates because the respondents are not representative of the population; modeling has been generally unsuccessful in removing this bias.

We concluded that selection bias would be a very serious issue in estimating fishing effort with an online non-probability sample. Selection bias is more complex than coverage alone; being on the Internet often is not very predictive of being a respondent to non-probability sample recruitment. Selection bias might be further exacerbated for a fishing effort survey because surveys of this type already tend to suffer from avidity bias more than surveys of other topics. Using a non-probability panel is unlikely to reduce this bias because the type of profile data that these panels have available have little value for predicting fishing activity. An alternative or supplementary approach to deal with selection bias in a non-probability fishing effort survey is to posit statistical models, but this approach would face severe challenges. The modeling assumptions would rely on powerful auxiliary information, but these variables do not exist. Any models constructed would be difficult to test and highly subjective. For a government survey that has important policy implications, such subjectivity is not desirable.

The second type of non-probability sample uses data from angler apps to produce estimates. This non-probability sample would have even more challenges for producing fishing effort estimates because an app of this nature would have virtually no appeal to those who do not fish recreationally. This feature results in an extreme version of selection bias and would greatly overestimate the percent who fished in the given time period. It is a critical weakness in estimating fishing effort using this method. We do not recommend further consideration of this method at this time. However, since a mobile app is really a data collection mode rather than a sampling approach, apps do have potential when used within a probability sampling method as discussed below.

The current FES is a mail probability sample survey that replaced the decades old random digit dial probability telephone survey. This change occurred after research and experiments showed the mail methodology had major response rate and coverage advantages. Further work on modernizing the design has also begun by testing electronic data collection to a larger extent.

One approach is to continue to use address-based probability sampling but to mail materials urging the respondents to go to the Web to complete the survey request. A Web-only, where respondents can only reply by answering on the Web (either on a computer or smartphone), and a Web-push mixed mode survey, where nonrespondents to the Web-push can respond by mail, are being explored. Both of these designs maintain the probability sample but allow electronic data collection.

Another way to maintain a probability design but maximize electronic data collection is to move from the current cross-sectional survey with independent samples every wave to a longitudinal survey design. If respondents can be enrolled in a longitudinal design then it might be possible to encourage a large percentage of them to report using the Web, or perhaps even using a mobile app for the follow-up waves. If this is possible, then the costs of data collection may also be reduced. We proposed a rotating panel design as having the greatest potential and suggested some research and testing of this option. This research could help determine whether the advantages of a longitudinal design are substantial enough to offset the disruptive effect of change in a survey.

Overall, we believe that additional efforts to modernize and increase the use of electronic reporting is very worthy of research and field tests. The probability sample designs, even with the lower response rates that have been observed over time, have major advantages over non-probability designs for fishing effort surveys. We would urge concentrating resources in probability sample designs that using the Web as a mode of reporting, and rotating panel designs that again have the potential to increase electronic reporting.

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Appendix

Marine Recreational Information Program

Fishing Effort Survey

Experimental Testing

9/26/2013

The MRIP Fishing Effort Survey (MFES) was implemented in Massachusetts, New York, North Carolina and Florida in October, 2012 to test a revised data collection design for monitoring marine recreational fishing effort. The survey, which collects information for two-month reference waves, included two experiments during the first two study waves, wave 5 (Sept-Oct 2012) and wave 6 (Nov-Dec, 2012), to test different survey design features aimed at maximizing efficiency and minimizing nonresponse error. Specifically, the experiments tested two versions of the survey instrument and four levels of cash incentives. Details of the experiments are provided below.

Instrument Testing

The MFES included an experiment to test two versions of the survey instrument. The objective of the experiment was to identify the instrument that maximized overall response rates while minimizing the potential for nonresponse bias resulting from differential nonresponse between anglers and non-anglers. One version of the instrument (Saltwater Fishing Survey) utilized a “screen out” approach that quickly identifies anglers (and non-anglers) and encourages participation by minimizing the number of survey questions, particularly for non-anglers. Person-level information, including details about recent fishing activity and limited demographic information, is collected for all household residents, but only if someone in the household reported fishing during the reference wave. The second version (Weather and Outdoor Activity Survey) utilized an “engaging” approach that encourages response by broadening the scope of the questions to include both fishing and non-fishing questions. This version collects person-level information for all residents of sampled households, regardless of whether or not household residents participated in saltwater fishing. Each wave, sampled addresses were randomly assigned to one of the two questionnaire types, which were evaluated in terms of response rates and reported fishing activity.

Table 1 provides the weighted response rates (AAPOR RR1 after excluding undeliverable addresses) and estimated fishing prevalence (percentage of households with residents who reported fishing during the wave) for the two versions of the instrument. Overall, the Weather and Outdoor Activity Survey achieved a significantly higher response rate than the Saltwater Fishing Survey, and there was no significant difference between instruments in estimated prevalence. The lack of a significant difference between instruments for estimated prevalence suggests that the gain in response for the engaging instrument cannot be attributed to increased survey participation by either anglers or non-anglers, but that both groups are more likely to respond to the Weather and Outdoor Activity Survey than the Saltwater Fishing Survey.

We also compared response rates and prevalence between instruments both among and within subpopulations defined by whether or not sampled addresses could be matched to state databases of licensed saltwater anglers – subpopulations expected to distinguish between households with anglers and households with no anglers or less avid anglers. As expected, both response rates and estimated prevalence were higher in the matched subpopulation than the unmatched subpopulation, confirming that a population expected to be interested in the survey topic - households with licensed anglers - is more likely to respond to a fishing survey and report fishing activity than a population that excludes licensed anglers[5]. Because we can identify household license status prior to data collection, we can account for differential nonresponse between matched and unmatched households in the estimation design by treating matched an unmatched domains as strata (Lohr, 2009).

Table 1. Weighted response rates and estimated prevalence overall and by domain for two versions of the survey instrument.

	Saltwater Fishing Survey		Weather and Outdoor Activity Survey	
	(%)	(n)	(%)	(n)
Response Rate				
Overall	31.1 (0.4)	17,511	34.7 (0.4)*	17,510
Matched	45.4 (1.1)	3,160	45.0 (1.0)	3,247
Unmatched	30.3 (0.4)	14,351	34.0 (0.5)*	14,263
Prevalence				
Overall	13.4 (0.5)	5,943	14.1 (0.5)	6,498
Matched	49.9 (1.7)	1,491	48.5 (1.6)	1,552
Unmatched	11.2 (0.6)	4,452	12.2 (0.6)	4,946

Notes – (1) standard errors are in parentheses. (2) Domains are defined by matching ABS samples to state databases of licensed saltwater anglers.
*Significantly different from Saltwater Fishing Survey ($p<0.05$).

There were no significant differences between instruments for either response rate or prevalence within the matched domain, suggesting that the inclusion of non-fishing questions in the Weather and Outdoor Activity Survey did not have an impact on response by either anglers or non-anglers. In the unmatched domain, the response rate was significantly higher for the Weather and Outdoor Activity Survey than the Saltwater Fishing Survey. However, the higher response rate did not translate to lower or higher estimates of prevalence; estimates of prevalence were not significantly different between instruments within the domain. This suggests that the engaging instrument uniformly increased the probability of response for anglers and non-anglers within the unmatched domain.

Differential nonresponse to a survey request between subpopulations will result in nonresponse bias if the subpopulations are different with respect to the survey topic. In the MRIP Fishing Effort Survey, we account for differential nonresponse between matched and unmatched households during sampling – matched and unmatched subpopulations are treated as independent

[5] The classification of sample into domains is dependent upon matching ABS sample to license databases by address and telephone number. This process is unlikely to be 100% accurate, so the unmatched domain is likely to include some households with licensed anglers. The unmatched domain also includes households with residents who fish without a license.

strata. Subsequently, the potential for nonresponse bias is limited to differential nonresponse between anglers and non-anglers within the matched and unmatched subpopulations. While the Weather and Outdoor Activity Survey achieved a higher response rate than the Saltwater Fishing Survey, both overall and within the unmatched subpopulation, the gains in response do not appear to result from a higher propensity to respond to the survey by either anglers or non-anglers. As a result, we cannot conclude that one of the instruments is more or less likely to minimize differential nonresponse between anglers and non-anglers. However, higher response rates decrease the risk for nonresponse bias and either lower data collection costs (for a fixed sample size) or increase the precision of estimates (for a fixed cost)[6]. Consequently, we conclude that the Weather and Outdoor Activity Survey is superior to the Saltwater Fishing Survey and recommend that the instrument be utilized for subsequent survey waves. Because it collects person-level information for all residents of all sampled households, the Weather and Outdoor Activity Survey also supports post-stratification of survey weights to population controls, which is an additional benefit of this recommendation.

Incentive Testing

The MRIP Fishing Effort Survey included an experiment to test the impact of modest, prepaid cash incentives on survey response and survey measures. Each wave, sampled addresses were randomly allocated to incentive treatment groups of $1, $2, and $5, as well as a non-incentive control group. Incentives were only included in the initial survey mailing. As in the instrument experiment, the objective of the incentive testing was to identify an optimum level of incentive that maximizes overall response while controlling costs and minimizes the potential for nonresponse bias resulting from differential nonresponse between anglers and non-anglers. Response rates, estimated fishing prevalence and relative costs of completing an interview were compared among incentive treatments to quantify the impacts of incentives.

Table 2 shows weighted response rates and the results of a logistic regression model predicting the effects of incentives on the odds of obtaining a completed survey. Including an incentive in the initial survey mailing significantly increased the odds of receiving a completed survey, and the odds increased significantly as the incentive amount increased. Cash incentives of $1, $2, and $5 increased the odds of receiving a completed survey by 63%, 93% and 137%, respectively.

Table 2. Weighted response rates and odds of receiving a completed survey by incentive amount.

Incentive	Response Rate (%)	n	Odds Ratio	95 % CI
$0	22.6	8,760	1.00	
$1	32.2	8,737	1.63*	(1.51, 1.77)
$2	36	8,738	1.93*	(1.78, 2.09)
$5	40.8	8,786	2.37*	(2.18, 2.56)

*Significantly different from the $0 control ($p$<0.05). Results of pairwise comparisons are as follows: $1>$0 (p<0.05), $2>$1 (p<0.05), $5>$2 (p<0.05).

[6] Assuming that fixed costs are the same for the two instruments, which was the case in the experiment.

Previous studies (Groves et al., 2006) have demonstrated that prepaid cash incentives can motivate individuals with little or no interest in a survey topic to respond to a survey request. Subsequently, we hypothesized that incentives would have a larger impact on non-anglers than anglers, minimizing differential nonresponse between the two populations. We initially explored this hypothesis by comparing estimated fishing prevalence among incentive conditions, expecting that gains in response in the incentive conditions would translate to lower estimates of fishing prevalence. The results do not support this hypothesis; there were no significant differences in prevalence among incentive conditions (Table 3).

Table 3. Overall estimated fishing prevalence by incentive amount.

Incentive	Prevalence (%)	n
$0	12.8	2,154
$1	14.1	3,065
$2	13.6	3,415
$5	14.1	3,807

Note – Differences in prevalence among treatments are not significant (p=0.05)

We further explored the interaction of topic salience and incentives by examining response rates and estimated fishing prevalence for the incentive conditions within domains defined by whether or not sampled addresses could be matched to databases of licensed saltwater anglers. We expected incentives to have a more pronounced effect in the unmatched domain, a population less likely to have an interest in the survey topic, than in the matched domain. Table 4 shows that incentives increased the odds of receiving a completed survey in both the matched and unmatched subpopulations. However, the value of the incentive seems to be more important in the unmatched domain, where the odds of receiving a completed survey increased uniformly and significantly as the value of the incentive increased ($0<$1<$2<$5). In contrast, the incentive amount was less significant in the matched domain, where the odds of receiving a completed survey were relatively flat among incentive conditions. These results are consistent with our expectations and suggest that a population with a low propensity to respond to a fishing survey can be motivated to participate by cash incentives, and that the motivation may increase as the incentive amount increases.

Table 4. Odds of receiving a completed survey by level of incentive for sample that could and could not be matched to state databases of licensed anglers.

Comparison Pair	Subpopulation Matched OR	Unmatched OR
$1 vs. $0	1.75**	1.63**
$2 vs. $0	2.01**	1.93**
$5 vs. $0	2.11**	2.39**
$2 vs. $1	1.15	1.18**
$5 vs. $1	1.21*	1.46**
$5 vs. $2	1.05	1.24**

Notes – The second value in the comparison pair is the reference value.
Significance: *p<0.05, **p<0.0001

As noted previously, we expected that the gains in response in the incentive conditions would translate to lower estimates of fishing prevalence, particularly in the unmatched subpopulation. Once again, the results are not consistent with expectations; differences in fishing prevalence among treatments were not significant in either the matched or unmatched domain (Table 5). The lack of an effect of incentives on fishing prevalence suggests that the gains in response associated with increasing incentive amounts are uniform between anglers and non-anglers.

Table 5. Estimated fishing prevalence by incentive amount for a population of anglers (matched) and non-anglers (unmatched).

	Subpopulation			
	Matched		Unmatched	
Incentive	(%)	(n)	(%)	(n)
$0	49.2	533	10.7	1,621
$1	50.3	779	12	2,286
$2	48.6	837	11.6	2,578
$5	48.2	894	12.4	2,913

Note – Within subpopulations differences in prevalence among treatments are not significant (p=0.05)

We also examined the effect of cash incentives on overall data collection costs, specifically the direct costs of printing, postage, and the cash incentives themselves. Table 6 shows that the $5 incentive provided the largest gain in response, but the gain came at a relative cost of approximately $0.15 per completed interview. In contrast, the additional costs of the $1 and $2 incentives (20% and 38% higher cost than the $0 control, respectively) are more than offset by the associated gains in the number of completed surveys (42% and 58%, respectively). In other words, including a $1 or $2 cash incentive in the initial survey mailing actually decreased the cost of receiving a completed survey by 22% and 20%, respectively. These cost savings, which are conservative[7], could be used to lower overall data collection costs (for a fixed sample size) or increase the precision of survey estimates (for a fixed cost).

Table 6. Effect of incentives on data collection costs

Incentive Amount	Relative Cost Difference	Relative Difference in Completed Surveys	Relative Cost per Completed Survey
$0	1.00	1	$1.00
$1	1.20	1.42	$0.78
$2	1.38	1.58	$0.80
$5	1.90	1.75	$1.15

Note – relative differences reflect the ratio of quantities (cost, completes) in the experimental treatments to the zero dollar control.

Including a modest prepaid cash incentive in survey mailings clearly has a positive effect on survey response rates; the odds of receiving a completed survey increased significantly as the incentive amount increased. We expected the incentives to have a greater effect on non-anglers than anglers and decrease the potential for nonresponse bias by minimizing differential nonresponse between these two populations. However, the results of the experiment suggest that incentives increase response propensities for non-anglers and anglers equally. While this result does not support our hypothesis, it does demonstrate that incentives can increase the quantity of data without having a negative impact on survey measures. The experiment also demonstrated that incentives can decrease overall data collection costs. Based upon these findings, we conclude that a $2 incentive is optimal in terms of both maximizing response rates and minimizing data collection costs.

[7] The cost comparison assumes that the non-incentive direct costs (postage and printing) are the same for all survey treatments and does not reflect the fact that incentive conditions may not require as many follow-up mailings.

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