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

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[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.

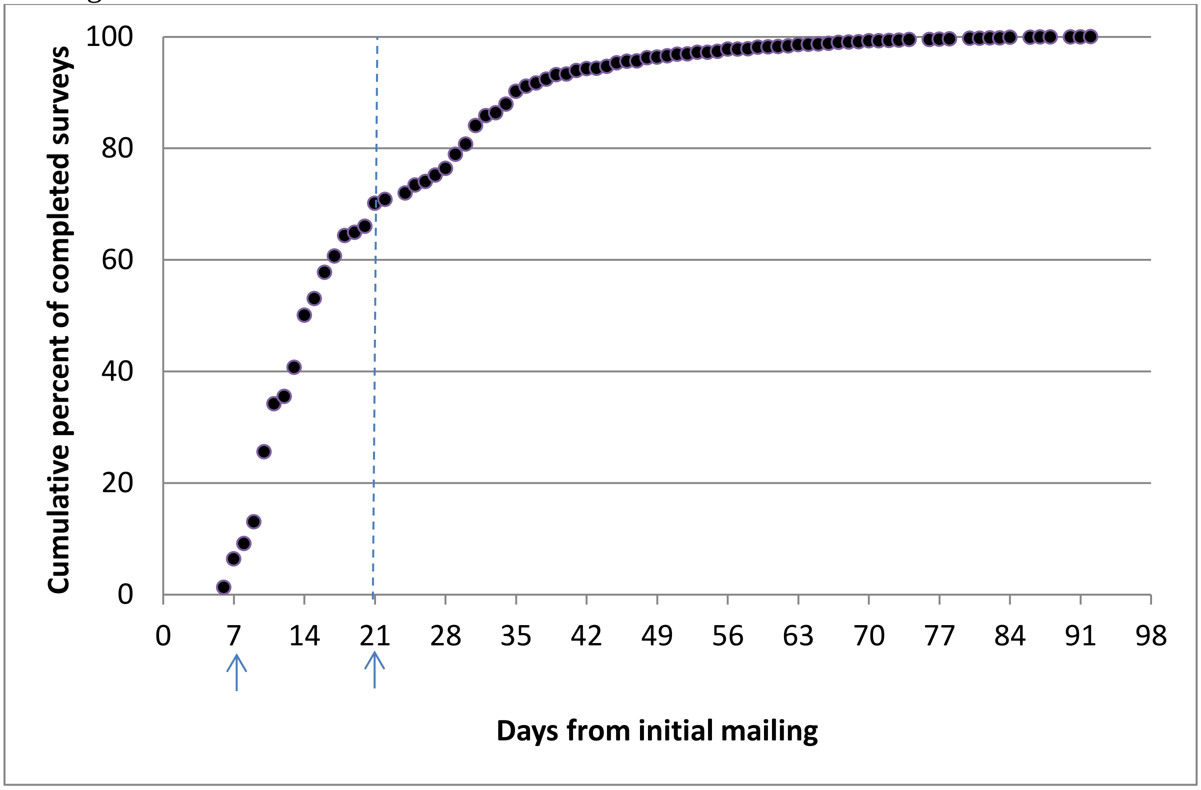
Table 1. Response rates, by state, from the CHTS and mail survey, for coastal counties and waves 4, 5, and 6, 2013.

	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.

	Preliminary		Final		*p*-value
State	%	SE	%	SE	
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.

State	Full Sample %	Full Sample n	Nonresponse Sample %	Nonresponse Sample 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.

State	Single Phase Mail % Non-resident	n	APAIS % 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	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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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.

JULY 2013

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.

AUGUST 2013

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

Please use other side for additional household members

27138

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.

References

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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 14^{th} day (or possibly on the 14^{th} 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".