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NONMARKET VALUATION OF MARINE RESOURCES IN THE NORTHEAST:
IMPLICATIONS FOR RECREATIONAL FISHERIES MANAGEMENT AND
OFFSHORE WIND ENERGY DEVELOPMENT

BY
ANDREW CARR-HARRIS

A DISSERTATION SUBMITTED IN PARTIAL FULFILLMENT OF THE
REQUIREMENTS FOR THE DEGREE OF
DOCTOR OF PHILOSOPHY
IN
ENVIRONMENTAL AND NATURAL RESOURCE ECONOMICS

UNIVERSITY OF RHODE ISLAND

2019

DOCTOR OF PHILOSOPHY DISSERTATION
OF
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2019

ABSTRACT

Sound natural resource management considers the full range of costs and benefits of policy action. Understanding these implications as they pertain to nonmarket goods or externalities requires an accurate assessment of consumer preferences and behavior. The chapters of this dissertation ascertain this knowledge in the context of recreational Atlantic striped bass fishery management and offshore wind development in the northeast United States.

Atlantic striped bass are the most prominent and heavily targeted recreational species found along the coast from Maine to North Carolina. Yet due in part to heavy recreational fishing effort, the species may be currently overfished. Given this status, it is pertinent to explore the concurrent impacts of potential policy action on angler participation, angler welfare, and recreational fishing mortality such that efficient compromises between conservation and socioeconomic objectives of fisheries management can be made.

In Chapter 1, we evaluate the economic incentives faced by recreational striped bass anglers using data from a recently-administered recreational striped bass angler survey. We estimate angler preferences for and the nonmarket value of keeping and releasing small (22"), medium (29"), and trophy-sized (38") fish. We find that for each size-class, anglers prefer keeping to releasing striped bass and that the nonmarket value of Atlantic striped bass increases exponentially with catch size. Illuminating the tradeoffs made by recreational striped bass anglers, our results indicate that one harvestable trophy-sized fish can be exchanged for about two medium-sized or three small ones.

Chapter 1 also sheds light on an important issue that arises when using discrete choice experiment data to evaluate angler preferences; namely, the influence of including versus excluding catch-and-release regulations on ensuing parameter estimates in models of angler utility. We find that failing to control for such regulations leads to counterintuitive estimates of the marginal utility of releasing striped bass. Finally, while choice experiment survey data is used extensively in the literature on recreational demand modeling, little attention has been paid to survey non-response bias on welfare estimates. We spearhead this issue using data collected from survey non-respondents during a telephone pre-screening interview.

In Chapter 2, we integrate the main results from Chapter 1 and historical catch data into an aggregate demand model to examine the broad effect of recreational striped bass fishing policies. We simulate the recreational Atlantic striped bass fishery and measure the relative effect of alternative sets of fishing regulations on angler welfare, angler participation, fishing mortality, and mature female fishing mortality. By comparing fishery outcomes across several recreational fishing policies, we assess policy-induced economic and biological tradeoffs that have yet to be considered jointly by managers of the fishery. We find that a wide range of economically efficient policies are available when the primary purpose of proposed policy action across the studied region is to control total recreational fishing mortality. When proposed management action is intended to curtail mature female recreational fishing mortality, however, proposed policy action that does not account for potential economic consequences can lead to inefficient outcomes, as exemplified by several of the policies analyzed lying inside the efficient frontier of welfare and female spawning stock removal volume. The

findings in Chapter 2 illuminate the practicality of assessing angler behavioral responses as a means of selecting efficient regulations, particularly when fisheries managers seek to balance socioeconomic goals with multiple conservation objectives.

Chapter 3 of this dissertation, currently in review at *Resource and Energy Economics*, addresses one previously unanswered question related to offshore wind energy development: that is, to what extent do offshore wind farms (OSWFs) impact local tourism? We examine how the Block Island Wind Farm, the United States' first operational OSWF, has impacted the short-term housing rental market. Using data from Airbnb, we estimate a difference-in-differences model that compares rental activity in Block Island to that in three nearby tourist destinations in Southern New England before and after construction. Estimation results suggest that following its construction, the Block Island Wind Farm caused a significant increase in nightly reservations, occupancy rates, and monthly revenues for Airbnb properties in Block Island during the peak-tourism months of July and August but had no effect in other months. The findings from this case study indicate that offshore wind farms can act as an attractive feature of a location, rather than a deterrent, and provide an important data point for the ongoing debate surrounding tourism impacts of OSWFs.

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CHAPTER 1

ESTIMATING THE RECREATIONAL VALUE OF KEEPING AND RELEASING ATLANTIC STRIPED BASS

1 Introduction

The status of Atlantic striped bass as the most prominent and heavily targeted recreational species found along the coast from Maine to North Carolina is represented poorly by the absence of studies addressing the policy-relevant economic research questions posed by the fishery's governing body. A few previous studies estimate the nonmarket value of catching additional striped bass (U.S. EPA 2004; Gautum and Steinback 1998; Bockstael et al. 1989), but due to data limitations, these studies do not examine how recreational anglers value fish they keep relative to fish they release nor assess the extent to which these values vary with catch size. Consequently, results of these studies provide little insight into the implications of changing recreational Atlantic striped bass fishing regulations. We fill this research gap by estimating the recreational value of keeping and releasing small, medium, and trophy-sized striped bass, providing a platform on which to "[evaluate] striped bass angler preferences for size of harvested fish and tradeoffs with bag limits" (ASMFC 2018) and therefore inform managers of this fishery.

We take a stated preference (SP) approach to nonmarket valuation by estimating angler preferences and values using choice experiment data obtained from a recently-administered striped bass angler survey. These estimates indicate that, for each size-class,

anglers prefer keeping to releasing striped bass. We also find that the nonmarket value of keeping and releasing striped bass increase almost exponentially with catch size.

Estimated marginal rates of substitution indicate that anglers are willing to exchange one harvestable trophy-sized fish for about two medium-sized or three small ones. We also assess the extent to which our welfare calculations are affected by survey non-response bias, an issue that has been largely overlooked in nonmarket valuation studies applied in recreational fisheries contexts. These results show little evidence to suggest that survey non-response bias infiltrates our estimates of angler willingness-to-pay.

Our investigation of the recreational value of keeping and releasing trophy striped bass is timely given that (a) the most recent estimate of total female SSB is below the binding management threshold, indicating that the stock is overfished (ASMFC 2018), and (b) trophy-sized striped bass are almost exclusively part of the spawning stock (Bigelow and Schroeder 1953). Our results intuitively indicate that anglers place a high recreational value on harvesting trophy striped bass, but we also find that releasing fish of this size is of considerable nonmarket value. We estimate these values such that accurate inferences can be drawn about the potential economic impact of proposed regulations, particularly those designed explicitly to protect the spawning population.

The rest of this paper is organized as follows: in the next section, we provide background information about the Atlantic striped bass fishery. Section 3 discusses the methods and data source used for analysis. The modelling approach is described in Section 4 and we interpret our results in Section 5. In Section 6, we assess the influence of non-response bias on welfare estimates and we conclude in Section 7.

2 Fishery Overview

Atlantic striped bass (*Morone saxatilis*) are an anadromous, highly-migratory species found along the U.S. east coast from Maine to North Carolina. During spring and early summer, the bulk of the population spawns in estuarine waters of the Chesapeake Bay and its tributaries, the Delaware River, and the Hudson River. After spawning, adults migrate north, as far as the Bay of Fundy, Canada, following prey and cooler waters. In the fall, adults move southward on their migratory path and return to spawning grounds to overwinter.

Partly due to their wide geographical range, Atlantic striped bass are among the most popular recreational species in the northeast and mid-Atlantic region of the United States. Recreational fishing trips targeting or catching striped bass consistently surpass twenty million annually, a level of effort that conduces high recreational striped bass harvest volume. In fact, from 2012 to 2016, the average annual recreational harvest volume of Atlantic striped bass was the largest among all recreationally targeted species in the U.S. (NMFS 2017).

Commercial landings typically account for a quarter of total striped bass harvest volume, thus excessive recreational harvest is a perpetual concern for the Atlantic States Marine Fisheries Commission (ASMFC). The ASMFC sets biological targets and thresholds for female spawning stock biomass (SSB) and the rate of fishing mortality (F). They then translate these reference points into a set of standard recreational regulations for the coastwide fishery but allow coastal states to implement alternative, conservation equivalent regulations. If either biological reference points surpasses its threshold, the ASMFC is obliged to adjust coastwide regulations such that these conservation objectives

can be met.

The most notable regulatory change in recent years was prompted by results of the stock assessment for 2012. In addition to revealing a steady decline in female SSB below target levels since 2006, the stock assessment projected with high probability that female SSB would fall below its threshold in subsequent years if the rate of fishing mortality remained at 2012 levels. As a precautionary measure to conserve the spawning population, the ASMFC approved Addendum 4 to Amendment 6 of the fishery's management plan, which called for a 25% reduction in harvest from 2012 levels in coastal states beginning during the 2015 fishing season (ASMFC 2014). Managers expected that in addition to conserving the population of spawning fish by reducing fishing mortality, Addendum 4's mandate would effectively protect a strong 2011 year-class. In response to the mandate, many coastal states adopted a one-fish, 28" or longer daily recreational possession limit during 2015, as shown in the bottom panel of Figure 1.

Results of the 2016 stock assessment update proved the Addendum 4 measures successful. Coastwide harvest of Atlantic striped bass in 2015 was reduced by 22.4% relative to 2012 levels, and all sectors achieved or exceeded their harvest reduction goal except for the Chesapeake Bay recreational sector, within which harvest increased by 53.4% relative to 2012 (ASMFC 2016b). Total F in 2015 was estimated to be 0.16, below both its target (0.18) and threshold (0.22) level (ASMFC 2016a). Female SSB in 2015 was estimated to be 58,853 metric tons (mt), which is below its target of 72,032 mt and above its threshold of 57,626 mt.

However, improvements to the status of the stock engendered by the Addendum 4 measures were short-lived. Preliminary results of the 2018 benchmark stock assessment,

which introduced a two-stock statistical catch-at-age model rather than the single-stock approach used previously, show that in 2017, female SSB and F for the Delaware Bay/Hudson River stock, and female SSB and F_{ocean} (but not $F_{\text{Chesapeake Bay}}$) for the Chesapeake Bay stock surpassed the biological threshold level (ASMFC 2018). Hence, it is likely that the Delaware Bay/Hudson River and Chesapeake Bay stock are currently overfished, the Chesapeake Bay stock is experiencing overfishing in the ocean but not in the Chesapeake Bay, and the Delaware Bay/Hudson River stock is experiencing overfishing.

Figure 1. Recreational striped bass fishing regulations during 2014 (top) and 2015 (bottom) (ASMFC 2016b, 2015b).

3 Methods and Data

3.1 Nonmarket Valuation

Preferences for nonmarket goods and amenities can be evaluated using revealed preference (RP) or stated preference (SP) methods. RP methods use data on observed behavior, while SP methods use data derived from survey questions that are carefully constructed such that preferences and values can be identified. One RP approach for nonmarket valuation commonly employed in the context of recreational fisheries is the travel cost model. Travel cost models relate the choice to fish at a specific site, or the number of fishing trips taken at a site over a period, to access costs and a vector of other site characteristics that typically include catch or harvest rates (Gautum and Steinback 1998; Bockstael et al. 1989; Loomis 1988). Estimates from these models can be used to calculate the marginal value of site characteristics, which provides a basis on which to infer the economic implications of policy-induced changes in such characteristics.

However, when analysts seek to evaluate the potential impact of previously unobserved policies or if there exists nonrandom variation in the attributes of interest across sampled fishing sites, the observational data needed to pursue RP methods is inadequate and SP methods must be adopted. In our case, both reasons necessitated primary SP data collection and analysis. To begin, our research objectives include estimating angler preferences for and willingness-to-pay (WTP) values of changes in trip quality caused by policies that have yet to be implemented in the recreational Atlantic striped bass fishery. Additionally, information about the length of striped bass that are released by recreational anglers is limited and, for our purposes, unreliable. The few states along the coast from Maine to North Carolina that collect these data do so through

voluntary angler logbook programs, yet the nonrandom nature of these samples make ensuing utility parameter estimates susceptible to selection bias.

Thus, within a SP framework we employ and analyze data obtained from a discrete choice experiment (DCE) survey. After presenting respondents with two or more hypothetical, multi-attribute alternatives, DCE questions ask respondents to choose or rank their most preferred alternative. Each alternative is comprised of a combination of attribute levels, the ranges of which are carefully selected to fulfill policy-relevant research objectives. Responses to DCE questions can be used to evaluate choice behavior, preferences, and WTP values for marginal changes in attribute levels (Louviere et al. 2000).

Several studies have employed a DCE to evaluate angler preferences for different aspects of the recreational fishing experience. Because they cover a wide range of species and fishery-specific research objectives, these studies differ in terms of the attributes used to explain angler preferences. In general, the attributes of interest to fisheries economists typically include catch or harvest rates and regulations. Angler preferences for marginal changes in catch and regulations have been estimated jointly for summer flounder in the Northeast (Massey et al. 2006; Hicks 2002), trout and grayling in Norway (Aas et al. 2000), paddlefish in Oklahoma (Cha and Melstrom 2018), trout in Michigan (Knoche and Lupi 2016), and pacific halibut and salmon in Alaska (Lew and Larson 2012; Lew and Seung 2010). In addition to catch rates and regulations, other studies have evaluated non-consumptive aspects of recreational fishing, such as hooking and losing, or seeing a target species (Goldsmith et al. 2018; Duffield et al. 2012). Lew and Larson (2015) exclude catch attributes from the utility function and estimate Alaskan charter boat angler

preferences and WTP for alternative bag and size limit restrictions.

Some economists have examined the interface between recreational catch and regulations by estimating the nonmarket value of fish that may be kept and of those that must be released. These studies consistently reveal that the recreational value of keeping fish is higher than that of releasing fish for a range of recreational species in the U.S. (Lee et al. 2017; Lew and Larson 2014; Anderson and Lee 2013; Anderson et al. 2013; Jarvis 2011). Carter and Liese (2012) further differentiate catch disposition into keep, release due to a minimum size limit, and release due to catch exceeding the bag limit for four recreational species in Florida. For all four species, they estimate higher WTP values for keeping fish rather than releasing fish and for one of the species, they estimate WTP values that differ considerably between the two release dispositions.

We build on this body of literature by estimating keep and release parameters for three size-classes of fish, an approach that is most closely related to Anderson and Lee (2013) and Anderson et al. (2013). The authors of these studies estimate size-specific keep parameters for several species in Washington but, to avoid estimating a very large number of parameters, aggregate the number of fish that must be released by weight. Hence, they assume that “anglers do not care whether, for example, 20 pounds of fish released come from one or two fish”. Based on anecdotal evidence and confirmed by our results, this is an untenable assumption about the recreational fishery for Atlantic striped bass.

3.2 Data

The data we use to evaluate recreational striped bass fishing preferences comes

from a dual mode, i.e., mail and web-based, angler survey that was implemented in late 2016. We randomly selected survey participants into our sample frame from a database comprised of all recreational anglers who were licensed or registered for saltwater fishing during 2015 in any of the ten coastal states from Maine to Virginia.

Prior to implementation, we tested the survey instrument by conducting two focus group sessions each in Massachusetts, New York, and Maryland with recreational striped bass anglers. We intentionally selected focus group participants who differed in terms of gender, age, and striped bass fishing experience to obtain feedback from a diverse mix of anglers. Based on their feedback, we conformed the survey language to regional differences in dialect to ensure consistent interpretation of survey questions. We also used the feedback to design contextually realistic and straightforward choice experiment questions.

Questions in final survey instrument were split into three sections: (1) recreational saltwater fishing experiences and opinions, (2) the discrete choice experiment (DCE), and (3) demographics. Each of the four DCE questions presented respondents with three options: two recreational striped bass fishing trips options and an option to not go recreational striped bass fishing. The DCE questions instructed respondents to compare the three options and to indicate their first and second-most preferred option. An example choice question is displayed in Figure 2 and one version of the complete angler survey can be found in the Appendix.

3.3 Survey Implementation

We used a stratified random sampling approach to reach the target population of

recreational striped bass anglers. From each state license or registration frame, we drew survey participants in proportion to that state's contribution to the total number of recreational striped bass fishing trips taken during 2015 across the study region, as shown in Table 1. Closely following the methods outlined in Dillman et al. (2009), we made up to six contacts with an original sample of 2,200 anglers: a telephone pre-screening interview, an advance letter or email invite, an initial survey mailing or email invite, a reminder letter or email, a second survey mailing or email invite, and a final reminder letter or email.¹ The survey implementation timeline is detailed in Table 2.

¹ All survey mailings provided respondents with the option to participate in the web version of the survey. All email correspondences contained a web-link to the survey.

B. Your Striped Bass Fishing Trip Preferences

In this section we want to know about your preferences for different types of striped bass fishing trips. Scenarios B1 - B4 will present you with three options (A, B, and C).
Option A and Option B: Single-day striped bass fishing trips with different regulations, catch, and trip costs.
Option C: Not going striped bass fishing.

For each scenario, compare the features of the three options and then answer the questions below. Do not compare the options on one page to the options on any other pages.

B1

FEATURES	Option A	Option B	Option C
<u>Striped bass legal daily limit</u> The number and size of striped bass that you are legally allowed to keep.	2 between 20" and 28" + 1 between 28" and 36"	2 between 20" and 28" + 1 equal to or longer than 28"	Do not go striped bass fishing
<u>Number of striped bass you catch (length per fish)</u> Some of these fish must be released if they are not within the legal daily limit.	4 (22") <i>You could keep two of these fish</i> 1 (29") <i>You could keep this fish</i> 1 (38") <i>This fish must be released</i>	2 (22") <i>You could keep both of these fish</i>	
<u>Number of other legal-sized fish you catch</u> Any species of fish you might catch while fishing for striped bass. All of these fish could be legally kept.	2		
<u>Trip cost</u> All fishing-related, transportation, and other expenses including bait, tackle, fuel, food, and beverages. This cost would not cover anyone else who may fish with you.	\$85	\$45	

1 If you were presented with these three options, which one would you choose? (Choose only one option.)

Option A

☐

Option B

☐

Option C

☐

2 If your choice was the **ONLY** striped bass fishing trip available to you during the next 12 months, how many trips would you take? (Enter "0" if you wouldn't take any.)

▶ # trips during the next 12 months:

3 What would be your second choice? (Choose one option other than your first choice.)

Option A

☐

Option B

☐

Option C

☐

Skip to 3

Figure 2. Example DCE question

The first potential point of contact with survey participants, a telephone pre-screening interview, allowed us to determine eligibility and thus focus survey efforts on anglers with relevant fishery experience. Based on responses to the first question of the telephone survey, we deemed ineligible those who indicated not having fished for striped bass within the past three years and excluded these anglers from subsequent solicitation procedures. After establishing eligibility, we solicited anglers' primary method of striped bass fishing, total number and targeted striped bass fishing trips taken in the past 12 months, likelihood of striped bass fishing next season, age, and income; in Section 6, we

use this information to assess the extent to which survey non-response bias affects our welfare estimates. Then, we invited those who completed the interview to participate in the full version of the angler survey. If willing to participate, respondents indicated their preference for receiving a mail, a web, or both a mail and a web version of the survey. From the original sample frame of 2,200, we called the 2,085 licensees with telephone information. Of the 577 people who completed the interview, 325 were eligible to participate in the survey. These interviews proved effective at boosting response rates; the survey completion rate among unscreened anglers was only 29%, while 55% of screened anglers completed the full survey.

Due to a low response rate from the original sample, we drew a supplemental, web-only sample of 1,000 anglers. These anglers received an advance email invitation to participate in the survey, a first reminder email, and a final reminder email. The overall survey response rate, which excludes ineligible participants, the deceased, those with non-working email addresses, and those with undeliverable mailing addresses, is 22.7%. It is likely, however, that many non-respondents were ineligible. When adjusted for estimated ineligibility based on the results of the telephone pre-screening interview, the survey response rate is approximately 35%.

Table 1. Survey distribution, response rates, and composition of final sample by state.

State	2015 striped bass trips/surveys distributed (%)	Final survey disposition			Estimation sample (%)
		Completed: eligible (%)	Completed: ineligible (%)	Did not complete survey [†] (%)	
CT	8.77	6.33	8.33	9.28	4.90
DE	0.91	0.58	0.00	0.94	0.64
MA	21.86	28.06	15.63	20.07	28.14
MD	19.68	15.11	14.58	21.09	16.42
ME	4.82	4.60	7.29	4.93	5.12
NH	1.64	1.58	2.08	1.67	1.28
NJ	21.36	28.92	29.17	19.42	25.59
NY	9.86	6.76	14.58	10.63	6.40
RI	4.82	4.03	2.08	5.05	7.46
VA	6.27	4.03	6.25	6.92	4.05
Total (#)	1,869,821/3,200	695	96	2456	469

Notes: 2015 striped bass trips were estimated using the unadjusted MRIP data (released prior to July 8th, 2018) that was available during the survey sampling procedure. Composition of final sample by state based on responses to the question: “In what area do you go recreational fishing for striped bass most often?”

[†]Includes surveys mailed to the deceased, ineligible, non-respondents, those who refused to participate, and those whose mailings were returned undeliverable

Table 2. Survey implementation schedule.

Contact Type	Date
Telephone pre-screening interview/email invitation	11/23/2016 – 12/17/2016
Reminder email to pre-screened anglers	12/7/2016 – 12/27/2016
Advance letter to unscreened anglers	12/23/2016
Email invitation to unscreened anglers	12/27/2016
First survey mailing to screened anglers/advance letter recipients	12/30/2016
Second email invitation to unscreened anglers	1/3/2017
Postcard reminder to anglers included in the initial survey mailing	1/5/2017
Second survey mailing to non-respondents	2/17/2017
Email invitation to all non-respondents	2/24/2017
Supplemental sample email invitation	3/13/2017
Supplemental sample email reminder	3/20/2017
Final email reminder to all non-respondents	4/3/2017

3.4 Experimental Design

The attributes used to create the DCE questions are shown in Table 3. They include catch of 22", 29", and 38" (hereafter small, medium, and trophy) striped bass, the minimum and maximum size limit, the bag limit for striped bass longer than the minimum size limit (bag limit), the bag limit for striped bass shorter than the minimum size limit but longer than 20" (small-fish slot limit), the number of other legal-sized fish caught, and the trip cost. Our fractional-factorial experimental design for main effects and selected interactions selected the subset of all attribute-level combinations that maximized the statistical efficiency of ensuing model parameters (Kuhfeld et al. 1994).² We removed from the design choice scenarios that included a dominant fishing trip alternative, as well as trip alternatives in which the number of striped bass kept and released could not be calculated from the combination of striped bass catch and regulatory attributes.³ To ensure that DCE questions presented respondents with conceivable sets of regulations, we removed scenarios in which the total possession limit (bag limit + small-fish slot limit) was greater than three striped bass. The procedure yielded 72 choice scenarios, blocked into 18 unique sub-versions of the survey that each contained four DCE questions.

² We generated the design in SAS using the Kuhfeld macros (Kuhfeld 2010).

³ An example of such an alternative would contain the following attribute levels: small striped bass catch=2, medium striped bass catch=1, bag limit=1, small-fish slot limit=1, minimum legal size=30". In this example, it is not possible to determine whether a respondent would keep two small striped bass or one small and one medium striped bass.

Table 3. Choice experiment attributes and levels.

Attribute	Levels
Catch (# fish)	
Small, 22", striped bass	0, 2, 4
Medium-sized, 29", striped bass	0, 1, 2
Trophy-sized, 38", striped bass	0, 1, 2
Other legal-sized fish	0, 2, 4
Striped bass regulations	
Minimum legal size	28", 30"
Maximum legal size	None, 36"
Bag limit (# fish $\geq$ min. size)	1, 2
Small-fish slot limit (# fish $> 20"$ and $\leq$ min. size)	0, 1, 2
Trip cost (\$)	10, 20, 30, 40

Notes: Trip cost levels shown are for shore/kayak version of the survey. Private/head boat cost levels were 25, 45, 65, and 85. Charter boat cost levels were 75, 100, 125, and 150.

After creating the design, we modified the DCE questions displayed in final survey version because several focus group participants had trouble answering DCE questions in which trip cost levels fell outside the range of costs that these participants typically incurred. For example, participants who fish for striped bass primarily from the shore became perplexed when presented with alternatives whose cost reflected a private or charter boat fishing trip. These findings portended widespread cognitive burden that we expected would dampen the survey response rate and threaten the reliability of the data because, unlike many other recreational species in the region, striped bass are heavily targeted both by both shore-based and boat anglers.⁴ We therefore displayed in the final survey version trip cost levels based on respondents' primary method of striped bass fishing, if known. We generated ranges of trip costs associated with shore and kayak,

⁴ In any given year, directed striped bass fishing trips taken on a boat (private, charter, and party) and from the shore typically account for about 60% and 40% of the total number of directed striped bass fishing trips taken across the study region, respectively.

private and party boat, or charter boat fishing trips. In the web version of the survey, we linked the range of displayed trip costs levels to a preceding survey question such that all web-survey respondents answered DCE questions appropriately tailored to their indicated primary method of striped bass fishing. To mail survey respondents who completed the telephone pre-screening interview and thus indicated their primary method of striped bass fishing, we sent surveys containing an appropriate range of trip costs. Some mail survey recipients who did not complete the telephone pre-screening interview, however, answered DCE questions which displayed trip costs that were misaligned with those they typically incurred.

4 Modelling Approach

4.1 Random Utility Models

The choice experiment method is grounded in Lancasterian consumer theory, which postulates that the overall utility provided by a good is determined by the part-worth contribution from each observable attribute (Lancaster 1966). In addition to the observable attributes of a good, an unobserved component specific to each decision maker influences choice. We analyze our DCE data using random utility maximization (RUM) models that decompose utility into its observable and unobservable components (McFadden 1973). RUM models assume that when faced with multiple alternatives, individual n will select alternative i that maximizes utility, U_{ni} :

$$U_{ni} > U_{nj} \quad \forall j \neq i. \quad (1)$$

Partitioning U_{ni} into its two component parts, the choice of alternative i is such that

$$V_{ni} + \varepsilon_{ni} > V_{nj} + \varepsilon_{nj} \forall j \neq i, \quad (2)$$

where $V(\cdot)$ is a function typically specified to be linear in parameters, $V_{nj} = \beta x_{nj}$, that relates observed attributes to utility and ε captures the utility derived from all other unobservable factors. The utility parameters β measure the relative importance of the attributes x_{nj} that describe alternative j . Because ε is stochastic, it is not possible to determine absolute levels of utility; however, probabilistic inference about individuals' choices can be made under the standard assumption for logit models that these terms are distributed i.i.d Type I extreme value. From Equation 2, the probability that angler n selects fishing alternative i is

$$Prob(Y_{ni}) = Prob[(\varepsilon_{nj} - \varepsilon_{ni}) < V(\beta x_{nj}) - V(\beta x_{ni})] \forall j \neq i. \quad (3)$$

Train (2003) calculates this probability for a multinomial logit (MNL) model as

$$Prob(Y_{ni}) = \frac{e^{V_{ni}}}{\sum_{j=1}^J e^{V_{nj}}}. \quad (4)$$

The MNL model, however, is subject to several behavioral restrictions based on the assumption that error terms are distributed i.i.d. Type I extreme value. This assumption implies that unobserved factors are uncorrelated over alternatives and uncorrelated over time in repeated choice situations. It also leads to the MNL model exhibiting the properties of independence of irrelevant alternatives (IIA) and proportional substitution, which in most cases are poor representations of individual decision-making processes. Another limitation of the MNL model is its inability to accommodate unobserved preference heterogeneity for the attributes x_{nj} that may exist across a sample

of decision makers.

To relax these behavioral restrictions and allow for heterogeneity in preferences, we estimate a random parameters logit (RPL) model. We partition the full set of attributes, x_{nj} , into x'_{nj} and x''_{nj} , which denote attributes with fixed utility parameters, β' , and attributes with randomly distributed utility parameters, β''_n , respectively. The parameters in β''_n become the sum of a population mean parameter, b , and a deviation parameter with zero mean, μ_n , which represents an angler's preferences relative to average preferences across the sampled population. This decomposition captures stochastic taste variation in the sampled population because preferences for the attributes in x''_{ni} are assumed vary across respondents given a sequence of observed choices. The utility angler n receives by selecting fishing trip alternative i in the RPL model is given by

$$\begin{aligned} U_{ni} &= V_{ni} + \varepsilon_{ni} \\ &= \beta' x'_{ni} + \beta''_n x''_{ni} + \varepsilon_{ni} \\ &= \beta' x'_{ni} + b x''_{ni} + \mu_n x''_{ni} + \varepsilon_{ni}. \end{aligned} \tag{5}$$

The stochastic portion of utility in Equation (5) is $\eta_{ni} = \mu_n x''_{ni} + \varepsilon_{ni}$, a non-zero term that incorporates unobserved preference heterogeneity in the attributes x''_{ni} . This specification for the error component relaxes the IIA assumption because, given the common influence of μ_n , utility is correlated over alternatives: $\text{Cov}(\eta_{ni}, \eta_{nj}) = E(\mu_n x''_{ni} + \varepsilon_{ni})(\mu_n x''_{nj} + \varepsilon_{nj}) = x''_{ni}' \Omega x''_{nj}$, where Ω is the covariance of μ_n . We specify error components to independent across alternatives by restricting off-diagonal elements of Ω to be zero.

We make two additional modifications based on the nature of our data. First, we

treat the data as a panel because respondents answered up to four choice questions. The unconditional probability that respondents make their observed sequence of choices, $I = \{i_1, \dots, i_T\}$ in scenarios $t = 1, \dots, T$, given the vector of fixed parameters, is the product of the logit formulas:

$$Prob(Y_{nl}|\beta') = \int_{\beta_n''} \left(\prod_{t=1}^T \left[\frac{e^{V_{ni_{lt}}}}{\sum_j e^{V_{njt}}} \right] \right) f(\beta_n''|\theta) d\theta, \quad (6)$$

where θ are parameters that describe the density of the random parameter distribution $f(\beta_n''|\theta)$ (Train 2003). Second, we exploit additional information from respondents' full ranking of alternatives, which we infer through their selection of a first- and second-most preferred alternative in each choice scenario. Each choice scenario is decomposed, or “exploded”, into $J - 1$ statistically independent pseudo-observations which therefore increases the number of sample observations. Compared to those that use unranked data, choice models estimated using ranked data have been shown to improve the precision, and thus reduce sampling variance of estimated utility parameters (Chapman and Staelin 1982). The unconditional probability of a respondent ranking the three alternatives in choice scenario t from most- to least-preferred as j_1, j_2 , and j_3 , in that order, is

$$\begin{aligned} Prob(Y_{nt}|\beta') &= \int_{\beta_n''} \left(\frac{e^{V_{nj_1}}}{\sum_{j=j_1, j_2, j_3} e^{V_{nj}}} \times \frac{e^{V_{nj_2}}}{\sum_{j=j_2, j_3} e^{V_{nj}}} \right) f(\beta_n''|\theta) d\theta \\ &= \int_{\beta_n''} \left(\prod_{j=1}^2 \left[\frac{e^{V_{nj}}}{\sum_{k=j}^3 e^{V_{nk}}} \right] \right) f(\beta_n''|\theta) d\theta. \end{aligned} \quad (7)$$

With these modifications, the unconditional choice probabilities for our panel rank-ordered RPL model is expressed as

$$Prob(Y_n|\beta') = \int_{\beta_n''} \left(\prod_{t=1}^T \left(\prod_{j=1}^2 \left(\frac{e^{V_{njt}}}{\sum_{k=j}^3 e^{V_{nkt}}} \right) \right) \right) f(\beta_n''|\theta) d\theta. \quad (8)$$

4.2 Model Specification

A principal goal of this analysis is to evaluate angler preferences for keeping and releasing striped bass, thus we generate size-specific keep and release variables based on the catch and regulatory attributes included in the experimental design. As discussed previously, we removed from the design scenarios in which these variables could not be determined unambiguously. We model fishing trip utility as a function of the number of the number of small, medium, and trophy striped bass kept and released (*Small keep*, *Medium keep*, *Trophy keep*, *Small release*, *Medium release*, and *Trophy release*), the number of other legal-sized fish caught (*Other catch*), the trip cost (*Cost*), and the opt-out alternative (*Opt – out*):

$$\begin{aligned} U_{njt} = & \beta_1 \text{Small keep}_{njt} + \beta_2 \text{Medium keep}_{njt} + \beta_3 \text{Trophy keep}_{njt} \\ & + \beta_4 \text{Small release}_{njt} + \beta_5 \text{Medium release}_{njt} + \beta_6 \text{Trophy release}_{njt} \\ & + \beta_7 \text{Other catch}_{njt} + \beta_8 \text{Cost}_{njt} + \beta_9 \text{Opt_out}_{njt} + \varepsilon_{njt}, \end{aligned} \quad (9)$$

where n indexes respondent, j indexes alternative, and t indexes choice scenario.

Equation (9) assumes that regulations affect fishing trip utility only indirectly by determining the number and size of striped bass kept and released. Preliminary testing of models that included regulatory attributes, however, revealed this to be a restrictive assumption, as these models revealed significant relationships between select regulatory variables and utility. Additionally, as we will see in the Results section, the coefficient on *Medium release* changes sign when we include regulatory attributes, which suggests that this estimate may be confounded with other determinants of fishing trip utility.

Given these findings, we modify Equation (9) by including regulatory variables. These variables—one for each of the three striped bass catch sizes—control for zero-fish bag limits and are derived from the minimum and maximum size limit attributes included in the experimental design. To control for the presence of a zero-fish bag limit for small and medium striped bass, we include the variables *Min28* and *Min30* that equal one if the minimum size is 28” or 30”, respectively, and the small slot bag limit equals zero.⁵ The coefficient on *Min28* measures the differential impact to angler utility of a 28” minimum size limit, which restricts harvest of small striped bass, relative to a 20” minimum size limit. Likewise, the coefficient on *Min30* measures the differential impact to angler utility of a 30” minimum size limit, which restricts that harvest of both small and medium-sized striped bass, relative to a 20” minimum size limit. To control for the presence of a zero-fish bag limit for trophy striped bass, we include the indicator variable *Max36*, which equals one if the maximum size limit is 36”. The coefficient on this variable measures the differential impact to angler utility of a 36” maximum size limit relative to a scenario with no maximum size limit. With these variables included, our baseline model of fishing trip utility becomes

$$\begin{aligned}
U_{njt} = & \beta_1 \textit{Small keep}_{njt} + \beta_2 \textit{Medium keep}_{njt} + \beta_3 \textit{Trophy keep}_{njt} \\
& + \beta_4 \textit{Small release}_{njt} + \beta_5 \textit{Medium release}_{njt} + \beta_6 \textit{Trophy release}_{njt} \\
& + \beta_7 \textit{Other catch}_{njt} + \beta_8 \textit{Cost}_{njt} + \beta_9 \textit{Opt_out}_{njt} \\
& + \beta_{10} \textit{Min28}_{njt} + \beta_{11} \textit{Min30}_{njt} + \beta_{12} \textit{Max36}_{njt} + \varepsilon_{njt}.
\end{aligned} \tag{10}$$

⁵ This is necessary as the definition of attribute is defined as “The number of striped bass equal to or longer 20” and shorter than the minimum size restriction”.

4.3 Estimation Sample

From the full sample of survey respondents, we remove those who indicated not having fished for striped bass within the past three years and focus instead on eliciting the preferences of anglers who are more likely to be affected by changes in striped bass fishing conditions. Furthermore, including in the sample anglers who did not recreationally fish for striped bass recently may evoke sample selection bias, as these anglers, if identified a telephone pre-screening interview, were intentionally excluded from subsequent sampling procedures. We also remove respondents who always selected the opt-out alternative as their most-preferred alternative despite considerable variation in attribute levels across choice scenarios, which is indicative of protest behavior. For reasons discussed in Section 3.4, we exclude observations from mail respondents who answered DCE questions containing trip cost levels that did not reflect these respondents' indicated primary method of striped bass fishing. Our final estimation sample consists of 469 anglers.

Table 4 displays demographic and fishing-related information about our sample. It also includes results from a two recent angler surveys to which we compare the characteristics of our sample. One of these surveys was directed at recreational striped bass anglers licensed in CT and MA (Murphy et al. 2015), and was one directed at the population of recreational anglers in the U.S. (Lovell et al. 2016). The striped bass anglers in our sample have a mean age of 54.3, which is consistent with the median age of sampled anglers from Murphy et al. (2015), and spent an average of 26.7 days fishing for saltwater species in the past 12 months. Both of these characteristics are comparable to the nationwide statistics in Lovell et al. (2016). In contrast to the population of U.S.

recreational anglers at large, however, the anglers in our sample are predominantly more male, slightly more affluent, and have attained higher levels of education. While these differences engender concerns about sample representativeness, that they have surfaced previously from a sample of more than twenty thousand randomly-intercepted striped bass and other anglers (Gautum and Steinback 1998) bolsters confidence that our sample is not anomalous. Like the sampled recreational striped bass anglers from Murphy et al. (2015), anglers in our sample have been fishing for striped bass for about 22 years and spent an average of 14.7 days fishing for striped bass during the past 12 months. Finally, more than 90% of the anglers in our sample fish for striped bass primarily from the shore (36.2%) or from a private boat (54.6%).

Table 4. Characteristics of sample anglers and the U.S. population of anglers.

Characteristic	Sample anglers	Recreational striped bass anglers licensed in CT and MA (Murphy et al. 2015)	U.S. population of anglers (Lovell et al. 2016)
Gender (% male)	92.8	96	85.5
Age (mean years)	54.3	54 (median)	53.5
Household income (%)			
< \$20,000	3	N/A	7
\$20,000 - \$99,999	41	N/A	57
\$100,000+	52	N/A	36
Did not answer	4	N/A	N/A
Education (%)			
Less than high school graduate	1.71	N/A	7.4
High school graduate or GED	18.8	N/A	21.7
Some college no degree, associate/technical degree	32.4	N/A	27.3
Bachelor's degree	27.1	31	25.5
Master's degree or higher	18.1	N/A	18.1
Did not answer	1.9	N/A	N/A
Days saltwater fished past 12 months (mean days)	26.7	N/A	27.8
Days striped bass fished past 12 months (mean days)	14.7	16 (days fished in previous fishing season)	N/A
Years of saltwater fishing (mean in years)	N/A	N/A	31.5
Years of striped bass fishing (mean in years)	22	23	NA
Primary striped bass fishing mode (%)			
Shore	36.2	N/A	N/A
Kayak	2.9	N/A	N/A
Private motorized boat	54.6	N/A	N/A
Charter boat	4.2	N/A	N/A
Head or party boat	1.9	N/A	N/A

5 Results

5.1 Utility Parameter Estimates

Table 5 displays estimation results from four model specifications. The model in Column (1) is defined by Equation (9) and excludes striped bass regulations. Column (2)

adds the maximum size limit variable and Column (3) adds the two minimum size limit variables. Column (4) is defined by Equation (10) and includes all three regulatory variables. We follow the relevant literature and specify striped bass keep and release parameters to be normally distributed (Lee et al. 2017; Lew and Larson 2014; Anderson and Lee 2013; Carter and Liese 2012), which captures the most important sources of heterogeneity in the context of this study, and we treat the other parameters as fixed.⁶ We estimate all models using NLOGIT version 5.

Across all columns of Table 5, the estimated parameters on the non-striped bass attributes are stable and behave as expected. The positive and statistically significant coefficients on *Other catch* suggest that catching other species of fish while fishing for striped bass is a boon to angler utility. Trip cost parameters, which represent the marginal utility of price, are negative and statistically significant. Estimated coefficients on the opt-out variable, which represent the utility from choosing not to fish, are negative, significant, and intuitively suggest that striped bass anglers prefer fishing for striped bass when such an opportunity is available. Another result that is common across specifications is the magnitude and statistical significance of the standard deviation coefficients on the striped bass catch variables. These estimates indicate considerable unobserved preference heterogeneity for keeping and releasing striped bass across the population of sampled anglers.

⁶ Alternative models in which all non-cost parameters are specified to be normally distributed yielded qualitatively similar results but at the expense less precisely estimated coefficients.

Table 5. Utility parameter estimates from panel rank-ordered mixed logit model.

Variable	(1)	(2)	(3)	(4)
<i>Mean parameters</i>				
Small keep	0.383*** (0.050)	0.380*** (0.050)	0.242*** (0.060)	0.241*** (0.060)
Medium keep	0.504*** (0.059)	0.501*** (0.059)	0.348*** (0.061)	0.347*** (0.062)
Trophy keep	0.606*** (0.081)	0.526*** (0.101)	0.653*** (0.080)	0.586*** (0.099)
Small release	0.067*** (0.022)	0.070*** (0.022)	0.088*** (0.023)	0.090*** (0.023)
Medium release	-0.084* (0.047)	-0.084* (0.047)	0.129** (0.058)	0.127** (0.059)
Trophy release	0.247*** (0.035)	0.286*** (0.047)	0.242*** (0.034)	0.275*** (0.046)
Other catch	0.152*** (0.016)	0.152*** (0.016)	0.151*** (0.016)	0.151*** (0.016)
Cost	-0.017*** (0.002)	-0.017*** (0.002)	-0.018*** (0.002)	-0.017*** (0.002)
Opt-out	-2.933*** (0.130)	-2.982*** (0.128)	-3.170*** (0.130)	-3.209*** (0.129)
Max. 36"		-0.132 (0.088)		-0.110 (0.085)
Min. 28"			-0.109 (0.088)	-0.106 (0.088)
Min. 30"			-0.644*** (0.106)	-0.637*** (0.107)
<i>Standard deviation parameters</i>				
Small keep	0.961*** (0.068)	0.961*** (0.068)	0.967*** (0.067)	0.967*** (0.067)
Medium keep	1.168*** (0.066)	1.168*** (0.066)	1.156*** (0.063)	1.156*** (0.063)
Trophy keep	1.258*** (0.094)	1.249*** (0.094)	1.247*** (0.093)	1.242*** (0.093)
Small release	0.411*** (0.025)	0.411*** (0.025)	0.407*** (0.024)	0.408*** (0.024)
Medium release	0.508*** (0.060)	0.504*** (0.059)	0.512*** (0.059)	0.509*** (0.058)
Trophy release	0.679*** (0.043)	0.679*** (0.042)	0.678*** (0.042)	0.678*** (0.041)
Log likelihood	-2010.540	-2010.057	-2003.305	-2002.968
McFadden Pseudo R ²	0.358	0.358	0.360	0.360
AIC	4051.100	4052.100	4040.600	4041.900

Notes: Number of observations is 1,747. Number of individuals is 469. 500 Halton draws used to maximize the simulated log-likelihood. *, **, and *** represent significance at the 10%, 5%, and 1% level of significance, respectively.

The model in Column (1) excludes regulatory variables. Estimated marginal utilities of keeping striped bass are intuitive—anglers prefer keeping larger striped bass to smaller striped bass. However, the model yields a puzzling pattern of release parameters. The coefficients on the variables for *Small release* and *Trophy release* are positive, significant, and have magnitudes that align with *a priori* expectations, but the coefficient on *Medium release* is negative and statistically significant. This latter estimate implies that catching and having to release medium-sized striped bass negatively affects angler utility. Results from other model specifications in Table 5, however, suggest that this estimate is confounded with the impact of catch-and-release only regulations, discussed in more detail below.

Column (2) adds *Max36* to the model. The estimate for this variable is negative yet statistically insignificant, implying that the anglers in our sample are insensitive to harvest restrictions on trophy-sized striped bass. This finding is consistent with the results of a recent survey directed at striped bass anglers in Massachusetts and Connecticut. In their research, Murphy et al. (2015) find that over 55% (71%) of the sampled recreational striped bass anglers are supportive or have neutral opinions toward a proposed maximum recreational size restriction of 36" (36" to 44"). Compared to Column (1) estimates, the coefficient on *Trophy keep* decreases slightly in magnitude and its standard error increases, and both the magnitude and standard error of the coefficient on *Trophy release* increase slightly. Estimated coefficients on the other striped bass catch variables change very little moving from Column (1) to Column (2).

In Column (3), we replace *Max36* with the two minimum size restriction variables. The coefficient on *Min28* is negative but statistically insignificant, which

suggests that if at least one medium striped bass can be kept, the average angler is indifferent to regulations that permit harvest of small striped bass. One potential explanation for this result is that most recreational striped bass anglers are accustomed to fishing in the absence of small harvest slot regulations. More specifically, as shown in Table 1, our sample reflects the population in that it is composed largely of individuals who fish in waters north of Delaware, where small harvest slots are seldom implemented. In contrast to the parameter on *Min28*, the estimated parameter on *Min30* is negative, significant, and relatively large in magnitude, indicating that anglers are highly averse to harvest restrictions on medium-sized striped bass. This finding may be driven by angler sensitivity to changes in the “status-quo”, as most coastal states have adopted a 28” recreational minimum size limit in recent years (ASMFC 2017, 2016, 2015). Nonetheless, this finding is consistent with other studies that model angler utility as a function of both catch and catch-and-release regulations (Cha and Melstrom 2018; Knoche and Lupi 2016).

The results in Column (3) also illuminate the effect in including minimum size restriction variables on the estimated striped bass catch parameters. Compared to the estimates in Column (1), the coefficients on *Small keep* and *Medium keep* decrease in magnitude, those on *Trophy keep* and *Small release* increases slightly in magnitude, and there is almost no change in the magnitude of the *Trophy release* parameter. The most striking difference between Columns (1) and (3) is in the estimated impact of *Medium release* on angler utility. Where in Column (1) it is negative and significant, the coefficient on *Medium release* in Column (3) is positive, significant, and greater in absolute magnitude. This estimate implies that, after controlling for catch-and-release

only regulations, angler utility is positively affected by catching and releasing medium-sized striped bass, a finding that is not in isolation; Jarvis (2011) reveals a similar directional change in coefficient estimates on pollock and haddock release variables when regulatory variables are excluded and included in the utility specification. Taken together, the three striped bass release coefficients in Column (3) intuitively suggest that anglers prefer catching-and-releasing larger striped bass to smaller striped bass. This finding is more sensible than that uncovered in Columns (1) and (2) and plausibly reflects the sportfishing nature of the recreational Atlantic striped bass fishery. Finally, Column (3) reveals anglers' relative preferences for small, medium, and trophy striped bass to be nearly identical across catch dispositions. Specifically, an increase in striped bass catch size from small to medium leads to a 43% and 47% relative increase in utility for fish that are kept and fish that are released, respectively; an increase in striped bass catch size from medium to trophy yields an 88% relative increase in angler utility levels. These findings lend credence to the set of utility parameters estimated by the specification in Column (3).

Column (4) considers all three size regulation variables. The coefficients on these variables are consistent with estimates in Column (2) and Column (3), in which these variables enter separately. Estimated coefficients on the small and medium striped bass catch variables align with Column (3) estimates, and coefficients on the trophy catch variables fall within the range of those estimated by the other specifications in Table 5.

We select a preferred specification by comparing information criteria and model fit statistics between the models in Table 5. All models perform reasonably well, as the goodness of fit statistic, McFadden's pseudo R^2 , is and remains high across columns.

Compared to those in Columns (1) and (2), the models in Columns (3) and (4) have lower AIC values, which implies a greater support for these models. While AIC values are similar for these two models, a likelihood ratio test for including *Max36* in Column (4) suggests that this variable does not lead to an improved model fit. Additionally, the model in Column (3) yields more precise estimates of the striped bass catch coefficients than the model in Column (4), thus we select the model shown in Column (3) as our preferred specification.

While our preferred specification performs well and yields intuitive results, the directional change of the coefficient on *Medium release* between Column (1) and Column (3) of Table 5 warrants additional attention. When *Min30* is omitted from the model, as in Column (1) of Table 5, the estimated effect of *Medium release* on angler utility is negative and statistically significant. Yet the results in Column (3) suggests that, relative to a 20” minimum size limit, a 30” minimum size limit yields a strong and adverse effect on angler utility. Taken together with the estimated coefficient on *Medium release* in Column (3), it seems that, rather than catching-and-releasing medium striped bass, it is imposing a 30” minimum size limit that reduces angler utility. However, because the variable *Medium release* includes fish that are released due to a 30” minimum size limit as well as those released in excess of a positive bag limit, the specification in Column (3) cannot separate the effect of releasing medium striped bass when $Min30 = 1$ from the effect of *Min30* itself.

We therefore display in Table 6 results from two additional models that isolate these impacts. In each, we disaggregate *Medium release* into two separate variables: *Medium release zero BL*, which represents the number of medium striped bass released

due to a zero-fish bag limit (when $Min30 = 1$), and *Medium release positive BL*, which represents the number of medium striped bass released above a positive bag limit (when $Min30 = 0$). The model in Column (1) of Table 6 excludes regulations and the model in Column (2) adds the two minimum size regulations.

In Column (1) of Table 6, the coefficient on *Medium release zero BL* is negative and statistically significant, while that on *Medium release positive BL* is positive yet insignificant. This would suggest that the negative impact to angler utility from a marginal increase in *Medium release* revealed in Column (1) of Table 5 is driven by catching-and-releasing medium striped bass. The model in Column (2) of Table 6, however, estimates positive and statistically equivalent coefficients on *Medium release zero BL* and *Medium release positive BL*. It also estimates a negative and significant coefficient on *Min30* that, along with other coefficient estimates, is consistent with our preferred specification from Table 5. The results in this column are evidence that the estimated impact of *Medium release* on angler utility shown in Column (1) of Table 5 is confounded with the impact of catch-and-release regulations for medium striped bass. Hence, including the variable *Min30* in our model seems essential for disentangling the effect of catching-and-releasing medium striped bass from the effect of catch-and-release only regulations, a finding of which further supports the selection of Column (3) of Table 5 as our preferred specification.

Table 6. Utility parameter estimates from panel rank-ordered mixed logit model with disaggregate medium release variables.

Variable	(1)	(2)
<i>Mean parameters</i>		
Small keep	0.346*** (0.051)	0.227*** (0.061)
Medium keep	0.433*** (0.062)	0.347*** (0.063)
Trophy keep	0.572*** (0.083)	0.652*** (0.084)
Small release	0.077*** (0.022)	0.096*** (0.023)
Medium release zero BL	-0.177*** (0.064)	0.144 (0.100)
Medium release positive BL	0.133 (0.083)	0.131 (0.083)
Trophy release	0.232*** (0.035)	0.236*** (0.035)
Other catch	0.152*** (0.016)	0.156*** (0.016)
Cost	-0.018*** (0.002)	-0.018*** (0.002)
Opt-out	-3.070*** (0.134)	-3.22*** (0.133)
Min. 28"		-0.108 (0.090)
Min. 30"		-0.671*** (0.132)
<i>Standard deviation parameters</i>		
Small keep	0.968*** (0.067)	0.965*** (0.067)
Medium keep	1.204*** (0.064)	1.213*** (0.063)
Trophy keep	1.315*** (0.105)	1.319*** (0.103)
Small release	0.418*** (0.025)	0.414*** (0.025)
Medium release zero BL	0.540*** (0.078)	0.583*** (0.076)
Medium release positive BL	0.587*** (0.091)	0.605*** (0.090)
Trophy release	0.767*** (0.044)	0.761*** (0.044)
Log likelihood	-2007.025	-2001.144
Mcfadden Pseudo R ²	0.359	0.361
AIC	4048.0	4040.3

Notes: Number of observations is 1,747. Number of individuals is 469. 500 Halton draws used to maximize the simulated log-likelihood. *, **, and *** represent significance at the 10%, 5%, and 1% level of significance, respectively.

5.2 Relative Values of Keeping Striped Bass

Recreational striped bass fishery managers are interested in understanding the tradeoffs anglers are willing to make between the number and size of fish that can be kept so that these tradeoffs can be considered when designing regulations. Hence, Table 7 displays estimates of the marginal rate of substitution (MRS) between each pair of striped bass keep variables. These estimates imply the rate at which different sizes of harvestable striped bass can be exchanged while holding angler utility constant. An estimated MRS of one, for example, would indicate that two size-classes of harvestable striped bass perfect are substitutes in that they can be exchanged on a one-to-one basis.

The MRS between attributes x_1 and x_2 is the ratio of the partial derivative of the utility function with respect to x_1 to the partial derivative of the utility function with respect to x_2 . Because we specify striped bass parameters to be normally distributed, Table 7 displays estimates of the mean MRS and 95% confidence intervals obtained using the Krinsky and Robb (1986) approach. We randomly draw observations from a multivariate normal distribution parametrized with the mean coefficients and covariance matrix of our preferred specification. Then, using these observations denoted $\tilde{\beta}_{x_1}$ and $\tilde{\beta}_{x_2}$, we calculate the MRS. We repeat this process 5,000 times to obtain an estimate of the mean MRS between attributes x_1 and x_2 ,

$$\text{Mean } MRS_{x_1, x_2} = \left(\sum_{i=1}^{5000} \frac{\tilde{\beta}_{x_1}}{\tilde{\beta}_{x_2}} \right) \times 5000^{-1}. \quad (11)$$

95% confidence intervals are based on percentiles of the simulated distribution.

All estimates displayed in Table 7 are significant at the 1% level of confidence. The first row indicates an angler willingness-to-exchange of one trophy, harvestable

striped bass for approximately three small ones. This result implies that, under certain conditions, a three-fish bag limit for small striped bass could compensate anglers for full harvest restrictions on trophy-sized striped bass. Based on the current objectives and status of the fishery, the estimate in the second row of Table 7 is perhaps more policy-relevant. The estimate in this row implies that anglers are willing to forego one harvestable trophy fish if compensated with about two that are medium-sized. Thus, a two fish, 28" possession limit accompanied by a restriction on trophy harvest may be a viable regulatory alternative for managers seeking to relax current one-fish 28" regulations, protect larger fish, and hold angler utility levels relatively constant. Finally, the third row shows that, to hold utility constant after giving up one harvestable medium-sized striped bass, the average angler must harvest 1.52 small striped bass.

Table 7. Mean MRS between striped bass keep attributes.

Ratio	Mean MRS	95% CI
Trophy keep:small keep	2.906***	(1.665, 5.399)
Trophy keep:medium keep	1.934***	(1.320, 2.827)
Medium keep:small keep	1.52***	(0.866, 2.681)

Notes: Mean MRS and 95% confidence intervals calculated using the Krinsky-Robb approach with 5,000 replications. *, **, and *** represent significance at the 10%, 5%, and 1% level of significance, respectively.

5.3 Angler Welfare

In addition to measuring the extent to which different size-classes of harvestable fish can be exchanged while holding angler utility constant, a primary objective of this analysis is to evaluate the nonmarket value of keeping and releasing striped bass. This information is critical for inferring the potential economic implications of changes in

regulations. We therefore use estimates from our preferred model specification to calculate angler WTP for marginal changes in striped bass fishing trip attributes. Table 8 displays mean WTP values and 95% confidence intervals, both calculated using the approach described in Section 5.2.

All marginal WTP values for striped bass catch attributes are significant at the 1% confidence level except for medium release, which is significant at the 5% level. Preference rankings among size-classes of striped bass revealed in Table 8 mirror those uncovered by our preferred model specification—trophy striped bass are substantially more valuable to anglers than medium-sized striped bass, the latter of which are only moderately more valuable to anglers than small striped bass. Angler WTP for a one-fish increase in the number of small, medium-sized, and trophy striped bass kept is \$13.80, \$19.80, and \$37.26, respectively. Angler WTP for releasing an additional small, medium-sized, and trophy striped bass, is \$5.05, \$7.32, and \$13.84, respectively.

Table 8. Mean WTP for striped bass fishing trip attributes.

Attribute	Mean WTP	95% CI
Small keep	13.80***	(7.35, 20.43)
Medium keep	19.80***	(13.50, 26.14)
Trophy keep	37.26***	(28.72, 47.06)
Small release	5.05***	(2.40, 8.03)
Medium release	7.32**	(0.84, 14.12)
Trophy release	13.84***	(9.48, 18.88)
Other fish	8.67***	(6.44, 11.49)
28" minimum size (1=yes)	-6.31	(-16.91, 3.41)
30" minimum size (1=yes)	-36.92***	(-52.04, -23.56)

Notes: Mean WTP and 95% confidence intervals calculated using the Krinsky-Robb approach with 5,000 replications. *, **, and *** represent significance at the 10%, 5%, and 1% level of significance, respectively.

Disaggregated across size-class and catch disposition, these WTP estimates provide a more detailed depiction of the recreational value of striped bass than that which currently exists. Hence, we cannot compare directly our results to those found previously, but a brief review of the extant literature is necessary. Gautum and Steinback (1998) examine how the aggregate recreational value the striped bass fishery could be affected by policies that increase the environmentally determined catch rate, such as the 1985-1989 Atlantic striped bass fishery moratorium. Using telephone and intercept survey data, they estimate a RUM model and find that angler WTP for catching additional striped bass is \$5.57.⁷ Using only the intercept survey data, they also estimate a travel cost model; this specification yields a \$7.42 angler WTP value for catching additional striped bass. More

⁷ These values are inflation-adjusted 2016 dollars and represent WTP for one-fish increase in historical average catch rate. Because they are derived from models that allow for diminishing marginal utility of catch, WTP for the first fish caught is slightly higher.

recently, the U.S. Environmental Protection Agency (EPA) investigate the effects of reduced impingement and entrainment on recreational fishing opportunities in the mid-Atlantic (U.S. EPA 2004). They estimate angler WTP for catching additional striped bass to \$20.79 and \$20.73 for boat and shore-based anglers, respectively, both values of which are considerably higher than those estimated in Gautum and Steinback (1998). Thus, in addition to providing more policy-relevant recreational use values for striped bass fishing, our results serve to mitigate the ambiguity that permeates current understandings of the nonmarket value of striped bass.

In the remaining rows of Table 8, we find that angler WTP for a one-fish increase in the number of other fish caught on a striped bass fishing trip is \$8.67. Estimated WTP for *Min30* is statistically significant, implying that anglers would require a \$36.92 discount to hold welfare constant with an increase in the minimum size limit from 20" to 30". The statistically insignificant WTP estimate associated with *Min28* implies that no angler discount would be required to hold utility constant with an increase in the minimum size limit from 20" to 28.

Table 8 reveals a large degree of overlap in WTP confidence intervals for *Small keep* and *Medium keep*, which suggests that these values may indistinguishably different. We formally test for differences in WTP across keep sizes in Table 9. As expected, keeping trophy striped bass is worth significantly more to anglers than keeping smaller ones: the difference in WTP between keeping trophy and medium-sized striped bass is \$17.55, and the difference in WTP between keeping trophy and small striped bass is \$23.61. The difference in WTP between keeping medium-sized and small striped bass, however, is insignificantly different than zero. Hence, we cannot reject the hypothesis

that the recreational values anglers place on keeping small and medium striped bass are equivalent.

Table 9. Mean WTP differences between striped bass keep attributes.

	Difference in mean WTP	95% CI
Trophy keep - small keep	23.61***	(12.54, 35.91)
Trophy keep - medium keep	17.55***	(7.77, 27.93)
Medium keep - small keep	6.09	(-1.93, 14.14)

Notes: Differences in mean WTP and 95% confidence intervals calculated using the Krinsky-Robb approach with 5,000 replications. *, **, and *** represent significance at the 10%, 5%, and 1% level of significance, respectively.

6 Assessing Survey Nonresponse Bias

The validity of inferences drawn from any nonmarket valuation study that relies on survey data rests on the assumption that estimated utility parameters represent the preferences of the study population at large. One potential source of bias that can threaten valid inference is the existence of systematic differences between respondents and non-respondents that affect both the propensity to respond to the survey and the preference parameters estimated from the realized sample of respondents (Groves 2006). We utilize data collected from survey non-respondents during the telephone pre-screening interview to investigate whether angler preferences and WTP values are influenced by variation in demographic and fishing-related characteristics that affect individuals' propensity to respond to the survey.

We follow the framework in Abdulrahman and Johnston (2016) and implement a two-stage procedure for assessing survey non-response bias. In the first stage, we estimate a binomial logit model where survey response is a function of age, income,

primary method of striped bass fishing, and likelihood of fishing in the next twelve months. In the second stage, we estimate a modified version of our preferred RUM model specification that includes interaction terms between trip attributes and predicted response propensities from the first stage. The coefficients on the interactions measure the degree to which mean marginal utilities of trip attributes vary with the predicted likelihood of responding to the survey.

To understand how mean welfare estimates are potentially affected by survey non-response bias, we use the second-stage model specification to evaluate WTP at (1) the average predicted response propensity for the realized sample (respondents) and (2) the average predicted response propensity for full sample (respondents and non-respondents), which we assume represents the average propensity to respond for the population of recreational striped bass anglers. We interpret the difference between (1) and (2) as the magnitude of the impact of survey non-response bias on angler welfare estimates.

6.1 Propensity to Respond

We model the propensity to respond to the angler survey as a function of demographic and fishing-related variables. Since the model requires information about respondents and non-respondents, potential explanatory variables are limited to those derived from questions asked on both the telephone pre-screening interview and in the full survey. In addition to the first question which determined survey eligibility, the telephone pre-screening interview asked questions about anglers' (1) primary method of striped bass fishing, (2) total number of recreational fishing trips taken in the past 12 months, (3) total number of recreational striped bass fishing trips taken in the past 12

months, (4) likelihood of striped bass fishing next season, (5) age, and (6) income. However, some respondents in our main estimation sample, as well as some non-respondents who completed the telephone interview, did not provide answers to all six questions. Fifty respondents did not indicate the total number of saltwater fishing trips nor the number of striped bass fishing trips they took in the past 12 months, thus we exclude these variables from the response propensity model. After removing those who did not answer at least one of the other four questions, we are left with a sample composed of 447 respondents and 110 non-respondents.

Comparing the characteristics of each group, which are displayed in Table 10, suggests the potential existence of systematic differences. We find no significant differences between the two groups in terms of days spent saltwater fishing in the past 12 months (p-value = 0.073) or primary method of striped bass fishing ($\chi^2(4) = 6.240$, p-value = 0.182). However, respondents and non-respondents differ significantly in the number days striped bass fishing in the past 12 months (p-value = 0.007), age (p-value = 0.00), income (p-value = 0.018), and likelihood of fishing in the next 12 months ($\chi^2(4) = 42.946$, p-value = 0.00). These observed differences justify further examination into the influence of survey non-response on our main results.

Table 10. Demographic and fishing-related information collected from respondents and non-respondents.

Variable	Respondents included in full sample (N=469)	Non- respondents (N=143)
Days saltwater fished past 12 months (mean days)	26.73 (n=419)	19.89 (n=139)
Days striped bass fished past 12 months (mean days)	14.75 (n=419)	8.48 (n=137)
Primary striped bass fishing mode (# individuals)(% of sample)		
Shore	170 (36.25)	64 (44.76)
Kayak	14 (2.99)	3 (2.10)
Private motorized boat	256 (54.58)	64 (44.76)
Charter boat	20 (4.26)	10 (6.99)
Head or party boat	9 (1.92)	2 (1.40)
Did not answer	0 (0.00)	0 (0.00)
Likelihood of recreational striped bass fishing during the next 12 months (# individuals)(% of sample)		
Certain to go fishing	307 (65.46)	53 (37.06)
Very likely	111 (23.67)	53 (37.06)
Somewhat likely	41 (8.74)	25 (17.48)
Very unlikely	9 (1.92)	7 (4.90)
Definitely will not go fishing	1 (0.21)	4 (2.80)
Did not answer	0 (0.00)	1 (0.70)
Age (mean age)	54.35 (n=460)	44.27 (n=135)
Household income (# individuals)(% of sample)		
Less than \$20,000	13 (2.77)	5 (3.50)
\$20,000 to \$39,999	26 (5.54)	13 (9.09)
\$40,000 to \$59,999	53 (11.30)	21 (14.69)
\$60,000 to \$79,999	44 (9.38)	12 (8.39)
\$80,000 to \$99,999	67 (14.29)	14 (9.79)
\$100,000 to \$149,999	112 (23.88)	21 (14.69)
\$150,000 to \$199,999	80 (17.06)	10 (6.99)
\$200,000 or more	54 (11.51)	15 (10.49)
Did not answer	20 (4.26)	32 (22.38)

Proceeding to the first-stage, we estimate a binomial logit model in which the propensity that individual n responds to the survey is specified as

$$P_n(y_n \neq 0|Z_n) = \alpha Z_n + e_n, \quad (12)$$

where the dependent variable, y_n , is discrete and equals one if an individual responds to the survey and zero otherwise, Z_n is a vector of demographic and fishing related variables whose relative impact on response propensity is measured by the parameters in α , and e_n is an independently and identically distributed error term.

Table 11 displays odds ratio estimates from Equation (12). All estimates are statistically significant at the 5% level of confidence or higher except for that on the indicator variable for primary fishing method, which equals one if an individual fishes for striped bass from a kayak, private boat, charter boat, or party boat, and zero otherwise. *Income* is measured using the midpoint of each response category⁸ and the magnitude of its estimate indicates that for each a \$10,000 increase in household income, the odds an individual responds to the survey by a factor of 1.039. Age is also positively correlated with response propensity; a one-year increase in individuals' age increases the odds of responding the survey by a factor of 1.056. Intuitively, response propensity decreases with decreases in individuals' likelihood of fishing in the next 12 months. Compared to those who are certain to go fishing, the odds that individuals who will "definitely will not go fishing" respond to the survey are lower by roughly a factor of 12. In the final step of the first-stage, we use the model defined by Equation (12) to obtain individuals' predicted probability of responding to the survey conditional on the covariates in Z_n , which is

⁸ We set the highest income category, \$200,000 or more, to \$225,000.

calculated as

$$\hat{P}_n(y_n \neq 0|Z_n) = \frac{\exp(\alpha Z_n + e_n)}{1 + \exp(\alpha Z_n + e_n)}. \quad (13)$$

Table 11. Results from response propensity model.

Variable	Odds Ratio (standard error)
Primarily fish for striped bass on a boat (1=yes)	1.086 (0.260)
Income (\$10,000s)	1.039** (0.020)
Age	1.056*** (0.009)
Likelihood of recreational striped bass fishing during the next 12 months [†]	
Very likely	0.378*** (0.100)
Somewhat likely	0.247*** (0.084)
Very unlikely	0.209** (0.134)
Definitely will not go fishing	0.083** (0.099)
Constant	0.307** (0.150)
Number of observations	557
Pseudo R ²	0.162

Notes: [†]baseline is “Certain to go fishing”. *, **, and *** represent significance at the 10%, 5%, and 1% level of significance, respectively

6.2 Response Bias in Marginal Utilities and WTP Values

The second stage integrates the predictions from Equation (13) into the RUM

model defined by equation 10.⁹ We interact all non-cost attributes (X_{njt}) with $\widehat{P}_n$ and the utility function is written as

$$U_{njt} = \beta_k X_{njt} + \beta_8 Cost_{njt} + \beta_l X_{njt} \widehat{P}_n + \varepsilon_{njt}. \quad (14)$$

Here, marginal utilities of the fishing trip attributes are given by $(\beta_k + \beta_l \widehat{P}_n)$ and the marginal utility of trip cost is measured by β_8 . The coefficients in β_l indicate the degree to which variation in predicted response propensity differentially affects the marginal utility of fishing trip attributes. To assess whether such variation affects welfare estimates, we use the Krinsky and Robb approach discussed in Section 5.2 and, for each attribute, evaluate WTP at (1) the average predicted response propensity for the realized sample (respondents), $\widehat{P}_{mr}$, and (2) the average predicted response propensity for full sample (respondents and non-respondents), $\widehat{P}_{ma}$:

$$WTP_{ma} = \frac{\beta_k + \beta_l \widehat{P}_{ma}}{\beta_8}, \quad (15)$$

$$WTP_{mr} = \frac{\beta_k + \beta_l \widehat{P}_{mr}}{\beta_8}. \quad (16)$$

For each attribute, we calculate the difference between WTP_{ma} and WTP_{mr} . This measures the degree to which marginal WTP values differ between the realized sample and the population based on demographic and fishing-related variations in response propensity, and is written as

$$WTP_{diff} = \frac{\beta_k + \beta_l \widehat{P}_{ma}}{\beta_8} - \frac{\beta_k + \beta_l \widehat{P}_{mr}}{\beta_8} = \frac{\beta_k + \beta_l (\widehat{P}_{ma} - \widehat{P}_{mr})}{\beta_8}. \quad (17)$$

⁹ Alternatively, the inverse of predicted response propensity can be used to weight observations prior to estimation. Estimates from this specification, displayed in the Appendix, are consistent with those estimated by our preferred specification.

Table 12 displays results from the unrestricted model, defined by Equation (10), which includes interaction terms between fishing trip attributes and predicted response propensities. For reasons discussed above, however, this sample includes fewer individuals than the main estimation sample used in Table 5. Therefore, Table 12 also displays results from a restricted model that excludes the additional interaction terms. Comparing results from the restricted model to those from our preferred specification, shown in Column (3) of Table 5, allows us to assess the extent to which estimated fishing trip preferences differ between the full and reduced sample.

Results of the restricted model in Table 12 are broadly consistent with the results of our preferred specification. Except for the statistically insignificant coefficient on *Medium release*, estimated parameters from the restricted model maintain the sign, level of significance, and approximate magnitude as their analogues in Column (3) of Table 5. Because each specification utilizes a different sample, we cannot explicitly test for differences in utility model parameters, yet the consistency of estimation results between the two specifications bolsters our confidence in the validity of the inferences drawn from this analysis.

The coefficients on the interaction terms included the unrestricted model can be interpreted as the deviation in mean marginal utilities associated with a one percentage point change in the predicted likelihood of response. Most of these coefficients, however, are statistically insignificant. We interpret this as evidence that, for these attributes, estimated mean marginal utilities do not suffer from the presence of non-response bias. The statistically significant and negative coefficient on *Other catch* $\times$ *Score* indicates that the baseline level of utility from catching other species of legal-sized fish on striped

bass trips is lower for those who have a higher propensity to respond to the survey.

Table 12. Results from panel rank-ordered mixed logit model with propensity score interactions.

Attribute	Restricted		Unrestricted	
	Mean	St. Dev.	Mean	St. Dev.
<i>Main effects</i>				
Small keep	0.246*** (0.061)	0.938*** (0.068)	-0.203 (0.384)	0.933*** (0.069)
Medium keep	0.298*** (0.063)	1.167*** (0.062)	0.087 (0.330)	1.134*** (0.062)
Trophy keep	0.662*** (0.084)	1.253*** (0.101)	0.973** (0.459)	1.244*** (0.101)
Small release	0.089*** (0.023)	0.400*** (0.025)	-0.039 (0.148)	0.396*** (0.025)
Medium release	0.092 (0.060)	0.501*** (0.060)	0.605 (0.384)	0.492*** (0.061)
Trophy release	0.242*** (0.035)	0.677*** (0.041)	0.184 (0.205)	0.687*** (0.042)
Other catch	0.147*** (0.016)		0.379*** (0.107)	
Cost	-0.017*** (0.002)		-0.016*** (0.002)	
Opt-out	-3.155*** (0.130)		-0.723 (0.617)	
Min. 28"	-0.098 (0.090)		-0.797 (0.550)	
Min. 30"	-0.689*** (0.108)		-0.591 (0.752)	
<i>Interactions</i>				
Small keep × Score			0.537 (0.450)	
Medium keep × Score			0.238 (0.389)	
Trophy keep × Score			-0.390 (0.537)	
Small release × Score			0.151 (0.174)	
Medium release × Score			-0.606 (0.448)	
Trophy release × Score			0.069 (0.244)	
Other catch × Score			-0.277** (0.124)	
Opt-out × Score			-2.933*** (0.732)	
Min. 28" × Score			0.843 (0.647)	
Min. 30" × Score			-0.133 (0.865)	
Log likelihood	-1937.26		-1924.27	
McFadden Pseudo R ²	0.358		0.362	
AIC	3908.5		3902.5	

Notes: Number of observations is 1,684. Number of individuals is 447. 500 Halton draws used to maximize the simulated log-likelihood. *, **, and *** represent significance at the 10%, 5%, and 1% level of significance, respectively.

Likewise, those who are have a higher propensity to respond to the survey are less likely to choose the no-fish trip alternative, as indicated by the negative and significant coefficient on $Opt - out \times Score$. Taken together with the findings in Table 11 that reveal response propensity to be positively correlated with likelihood of fishing for striped bass in the next 12 months, these results intuitively suggest that, compared to those less likely to go fishing, individuals who are more likely to go striped bass fishing next year receive less utility from (1) catching non-striped bass species while fishing for striped bass and (2) not fishing for striped bass.

We now examine the extent to which variation in response propensity based on demographic and fishing-related characteristics affects welfare calculations. Columns (2) and (3) of Table 13 displays mean WTP values for fishing trip attributes based on the restricted model. In Columns (4) and (5), we show the results of Equations (15) and (16), respectively. The sixth column of Table 13 displays differences between mean WTP evaluated at $\widehat{P}_{ma}$ and mean WTP evaluated at $\widehat{P}_{mr}$, calculated from equation 17. We duplicate WTP estimates from our main analysis in the second column of Table 13 to assess differences in welfare calculations between the full sample (Column 2) and the reduced, non-response sample (Column 3).

As revealed previously by comparing utility model parameters, we find no substantial differences in the magnitudes of WTP values between those derived from the full and non-response sample except for that pertaining to a marginal change in *Medium release*, which becomes insignificant when estimated using the non-response sample. Across fishing trip attributes, WTP values evaluated at the mean propensity score of respondents and non-respondents (Column 4) are consistent with those evaluated

Table 13. Estimates of and differences in mean WTP evaluated at the average predicted response propensity for the realized sample (respondents) and full sample (respondents and non-respondents).

Attribute	Restricted		Unrestricted		
	Main sample	Non-response sample	All (WTP_{ma})	Respondents (WTP_{mr})	Difference in means (WTP_{diff})
Small keep	13.80*** (7.35, 20.43)	14.80*** (8.04, 21.74)	13.84*** (6.57, 21.31)	14.93*** (7.96, 22.25)	-1.10 (-3.08, 0.72)
Medium keep	19.80*** (13.50, 26.14)	17.82*** (11.28, 24.72)	16.92*** (9.61, 24.03)	17.28*** (10.45, 24.16)	-0.44 (-2.01, 1.12)
Trophy keep	37.26*** (28.72, 47.06)	39.84*** (30.42, 50.88)	40.39*** (30.76, 51.67)	39.49*** (29.95, 50.31)	0.79 (-1.28, 2.98)
Small release	5.05*** (2.40, 8.03)	5.37*** (2.41, 8.67)	5.07*** (2.07, 8.51)	5.35*** (2.45, 8.70)	-0.31 (-1.06, 0.40)
Medium release	7.32** (0.84, 14.12)	5.51 (-1.61, 12.69)	7.25* (-0.14, 15.06)	6.08 (-1.29, 13.71)	1.24 (-0.56, 3.14)
Trophy release	13.84*** (9.48, 18.88)	14.70*** (10.05, 20.46)	14.73*** (9.49, 21.29)	14.79*** (9.72, 21.13)	-0.14 (-1.13, 0.88)
Other fish	8.67*** (6.44, 11.49)	8.88*** (6.37, 11.99)	9.64*** (6.90, 13.22)	9.05*** (6.54, 12.31)	0.57** (0.069, 1.11)
28" minimum size (1=yes)	-6.31 (-16.91, 3.41)	-5.93 (-17.58, 4.79)	-7.52 (-20.34, 3.98)	-5.82 (-18.13, 5.38)	-1.69 (-4.45, 0.94)
30" minimum size (1=yes)	-36.92*** (-52.04, -3.56)	-41.80*** (-60.03, -27.44)	-43.11*** (-62.24, -26.87)	-43.14*** (-61.91, -27.96)	0.30 (-3.21, 3.80)
# observations	1,747	1,684	1,684	1,684	1,684
# individuals	469	447	447	447	447

Notes: Mean WTP, differences in means, and 95% confidence intervals (below in parenthesis) calculated using the Krinsky-Robb approach with 5,000 replications. *, **, and *** represent significance at the 10%, 5%, and 1% level of significance, respectively.

at the mean propensity score of respondents (Column 5). Differences between Column (4) and Column (5) estimates, shown in Column (6) of Table 13, can be interpreted as the degree to which survey non-response bias affects estimated WTP values. Estimates in Column (6) are statistically insignificant for all attributes except *Other catch*, yet the magnitude of this coefficient trivial. Overall, the results in Column (6) provide

considerable evidence against the notion that our estimates of WTP are affected by bias related to survey non-response propensity.

7 Conclusion

To meet the demands of fishery managers, this study “[evaluates] striped bass angler preferences for size of harvested fish and tradeoffs with bag limits” (ASMFC 2018). We separately identify the recreational value anglers place on keeping and releasing small, medium, and trophy striped bass using data from a recent choice experiment survey. In line with the results from choice experiment studies focused on other recreational species, model estimates indicate that anglers place a higher value on striped bass that may be kept than on those that must be released. However, in contrast to the results of some of these studies indicating that angler welfare increases less than proportionally with catch size (Goldsmith et al. 2018; Anderson and Lee 2013), we find that the recreational value of keeping or releasing striped bass increases almost exponentially with fish size, which likely reflects the sportfishing nature of the Atlantic striped bass fishery. Additionally, we find little evidence to suggest that our welfare estimates suffer from the presence of survey non-response bias.

Accounting for the recreational value of catching-and-releasing striped bass is necessary to accurately assess the extent to which harvest restrictions affect anglers. One finding pertinent to this claim is the relatively high recreational value anglers place on catching-and-releasing trophy-sized striped bass. Our estimates indicate that releasing a trophy striped bass is slightly more valuable to anglers than keeping a small one, and only slightly less valuable than keeping a medium-sized one. While anglers will incur a

considerable welfare loss from catching-and-releasing a trophy-sized striped bass as opposed to keeping that same fish, the results suggest that such a loss can be largely recouped if an angler catches and keeps a medium striped bass on the same trip. We do not investigate the underlying factors driving the high catch-and-release value of trophy striped bass, but one possible explanation is the ostensibly widespread support for conservation-minded fishing practices given by regulatory and other agencies across the study region. In addition to being implicit in recreational regulations that fully or partially restrict trophy harvest, many states encourage such practices through voluntary catch-and-release award programs or tournaments; in Maryland, for example, recreational anglers who release alive a striped bass longer than 40" can receive the Governor's Striped Bass Conservation Award. Some volunteer conservation organization employ similar tactics; Striper Forever's Release a Breeder Club, for example, recognizes anglers who release unharmed striped bass longer than 36" in total length.

Our study provides additional insight into the effects of including versus excluding catch-and-release regulations in models of angler utility applied to choice experiment data. When we exclude catch-and-release regulations from the model, the estimated impact to angler utility from releasing medium-sized striped bass is negative. Our preferred model specification, however, which includes catch-and-release regulations, estimates a positive relationship between angler utility and releasing medium-sized striped bass, the magnitude of which intuitively falls within the range of that associated with releasing small and trophy-sized striped bass. Estimates from this model indicate that angler utility is negatively affected by catch-and-release regulations for medium-sized (29") striped bass, yet unaffected when such regulations are applied to

small or trophy striped bass. While specific to the context of this study, these findings highlight the importance of controlling for catch-and-release regulations in models of angler utility that rely on DCE data.

While the results of our study can be used to infer the potential economic impact of changes in recreational striped bass fishing regulations, some limitations exist. First, when generating keep and released variables from the DCE data, anglers are assumed to harvest all fish that can be legally retained, but further research is needed to understand whether this is a realistic depiction of angler behavior. Second, evaluating the extent to which changes in striped bass regulations affect aggregate trip demand and angler welfare requires considering the proportion of fishing trips that may be affected by such changes, which is outside the scope of this study. In the next chapter of this dissertation, however, we use results from this analysis to evaluate aggregate fishery outcomes conditional on changes in recreational Atlantic striped bass regulations.

CHAPTER 2

SHORT-RUN ECONOMIC AND BIOLOGICAL IMPLICATIONS OF RECREATIONAL ATLANTIC STRIPED BASS FISHING POLICY

1 Introduction

Recreational fisheries managers often attempt meet short-run conservation objectives by adjusting daily possession and size limit regulations. These policy actions influence aggregate recreational fishing demand by altering the incentives faced by individual anglers. If inaccurately predicted or left unaccounted, however, policy-induced shifts in demand may undermine managers' attempts to meet intended conservation objectives or result in policies that overly reduce angler welfare. To predict the concurrent impacts of policy action on demand, welfare, and fishing mortality—key linkages in the coupled social-ecological system of recreational fishing (Fenichel et al. 2013; Hunt et al. 2013)—it is necessary to evaluate the incentives faced by anglers.

We evaluate the incentives faced by recreational Atlantic striped bass anglers and use this knowledge to predict aggregate, short-run economic and biological effects of changing possession (bag) and size limits. Our predictive model of angler behavior links recreational striped bass fishing policy in Massachusetts, Rhode Island, and Connecticut to individual, trip-level outcomes. The model is parameterized with the results of a recent choice experiment survey, where angler participation and welfare are conditional on trip cost and the number and size of striped bass kept and released. It simulates the fishery under actual 2015 policy conditions, imposes alternative 2015 bag and size limits, and

calculates resultant changes in angler welfare and demand. We use estimates of the change in recreational demand occurring under alternative 2015 policy conditions to compute expected levels recreational fishing mortality. By comparing model outputs across several bag and size limit combinations, we illustrate the biological and economic tradeoffs created by different recreational Atlantic striped bass fishing policies.

A central research objective is to assess the effect of different harvest-size restrictions on angler welfare and female spawning stock biomass (SSB). The recreational Atlantic striped bass fishery provides an excellent canvas for illustrating these tradeoffs because (a) the most recent estimate of female SSB, while preliminary, is below the binding management threshold, indicating that the stock is overfished (ASMFC 2018), (b) trophy-sized striped bass are almost exclusively part of the spawning stock (Bigelow and Schroeder 1953), and (c) the recreational value of striped bass increases exponentially with the size of fish kept or released. Thus, this fishery is particularly suited for investigating the economic and biological implications of full or partial harvest restrictions on trophy-sized striped bass.

Results indicate that striped bass angler welfare is highly responsive to changes in the baseline minimum size limit because such policy adjustments strongly influence the rate at which angler encounter harvestable striped bass. Conforming to intuition, we find that harvest slot policies, which specify both a minimum and maximum size limit, more effectively mitigate mature female fishing mortality than minimum length only policies. But when these types of policies specify a narrow legal size range, they may lead to inefficient outcomes in terms of angler welfare and total recreational fishing mortality. We also find instances where two or more policies yield similar impacts to angler welfare

and recreational fishing mortality but differ considerably in their effect on female SSB. These and other findings highlight the importance of accounting for both angler welfare and the biological characteristics of the stock when proposed policy action seeks to efficiently reach socioeconomic and conservation goals of fisheries management.

The rest of this paper is organized as follows: in the next section, we provide background information about the recreational Atlantic striped bass fishery. In Section 3 and 4, we discuss the relevant literature examining the biological and economic impacts of recreational fishing regulations. Section 5 discusses the data used to estimate the angler behavioral model and we interpret results from this model in Section 6. Section 7 describes our procedure for evaluating aggregate impacts of recreational striped bass fishing. In Sections 8, we discuss results of the aggregate demand model and we conclude our analysis in Section 8.

2 Fishery Background

Atlantic striped bass are the most prominent and heavily targeted recreational species found along the coast from Maine to North Carolina. Typically caught or targeted on more than 20 million recreational fishing trips annually, striped bass are also one of most-harvested recreational species in the region; in fact, from 2012 to 2016, annual average harvest volume of Atlantic striped bass was the largest among all recreationally targeted species in the U.S. (NMFS 2017).

Given the popularity of Atlantic striped bass as a recreational target, excessive recreational harvest is a perpetual concern for the fishery's governing body, the Atlantic States Marine Fisheries Commission (ASMFC). The ASMFC sets biological targets and

thresholds for the rate of fishing mortality (F) and level of female spawning stock biomass (SSB). They then translate these biological reference points into a set of standard recreational regulations for the coastwide fishery but allow coastal states to implement alternative, conservation equivalent regulations. This regulatory flexibility typically results in a variety of state-level recreational striped bass bag and size limits, the latter which include minimum lengths, harvest slots, and protected harvest slots, as shown in Figure 1.¹⁰

The most notable regulatory change made in recent years was prompted by results of the stock assessment for 2012. In addition to revealing a steady decline in female SSB below target levels since 2006, the stock assessment projected with high probability that female SSB would fall below its threshold in subsequent years if the rate of fishing mortality remained at 2012 levels. As a precautionary measure to conserve the spawning population, the ASMFC approved Addendum 4 to Amendment 6 of the fishery's management plan, which called for a 25% reduction in harvest from 2012 levels in coastal states beginning during the 2015 fishing season (ASMFC 2014). Managers expected that in addition to conserving the population of spawning fish by reducing fishing mortality, Addendum 4's mandate would effectively protect a strong 2011 year-class. In response to the mandate, many coastal states adopted a one-fish, 28" or longer daily recreational possession limit during 2015.

Results of the 2016 stock assessment update proved the Addendum 4 measures successful. Coastwide harvest of Atlantic striped bass in 2015 was reduced by 22.4%

¹⁰ Minimum size limits specify a minimum length of legally harvestable fish, harvest slots specify a minimum and maximum length of legally harvestable fish, and protected harvest slots specify a minimum and maximum length of fish that cannot be legally harvested.

relative to 2012 levels, and all sectors achieved or exceeded their harvest reduction goal except for the Chesapeake Bay recreational sector, within which harvest increased by 53.4% relative to 2012 (ASMFC 2016b). Total F in 2015 was estimated to be 0.16, below both its target (0.18) and threshold (0.22) level (ASMFC 2016a). Female SSB in 2015 was estimated to be 58,853 metric tons (mt), which is below its target of 72,032 mt and above its threshold of 57,626 mt.

However, improvements to the status of the stock engendered by the Addendum 4 measures were short-lived. Preliminary results of the 2018 benchmark stock assessment, which introduced a two-stock statistical catch-at-age model rather than the single-stock approach used previously, show that in 2017, female SSB and F for the Delaware Bay/Hudson River stock, and female SSB and F_{ocean} (but not $F_{\text{Chesapeake Bay}}$) for the Chesapeake Bay stock surpassed the biological threshold level (ASMFC 2018). Hence, it is likely that the Delaware Bay/Hudson River and Chesapeake Bay stock are currently overfished, the Chesapeake Bay stock is experiencing overfishing in the ocean but not in the Chesapeake Bay, and the Delaware Bay/Hudson River stock is experiencing

3 Relevant Biological Literature

Given the current status of the Atlantic striped bass fishery and prevailing management objectives, it is pertinent to explore the short-run impacts to angler welfare, total fishing mortality, and mature female fishing mortality of alternative legal harvest size restrictions. In the Atlantic striped bass and other recreational fisheries, minimum length only, harvest slots, and other types of harvest size restrictions are to employed to prevent recruitment overfishing, a condition in which the spawning stock is depleted to a

level at which future recruitment declines strongly (Allen et al. 2013). Of course, the appropriate specification of the legal harvest size restrictions depends on biological characteristics, current stock conditions, and management objectives of the fishery in question. Yet increasing attention has been paid to the biological and fishing-quality ramifications of minimum length only (ML) and harvest slot (HS) policies in recreational fisheries management.

Compared to ML policies, HSs have been shown to maintain more natural age structures, more positively affect spawning and recruitment potential, produce higher harvest numbers and trophy catch, lead to lower discard mortality, and distribute sex-biased fishing exploitation more evenly across both sexes for a variety of recreational species (Arlinghaus et al. 2010; Pierce 2010; Wilde 1997; Koehn and Todd 2012; Morson et al. 2017). Gwinn et al. (2015) evaluate the differential effect of ML versus HS policies on fishery outcomes for a range of representative fisheries. They simulate an age- and size-structured fish population model under multiple exploitation level and life-history parameterizations. For each exploitation level (low and high) they define three management objectives (harvest-oriented, trophy-catch oriented, and a compromise between the two former objectives), choose the objective-meeting ML and HS policy, and calculate fishery and conservation metrics at that regulation. Most relevant to the current study are Gwinn et al. (2015)'s simulation results pertaining to the life-history parametrization of "large-bodied fish with slow growth, late maturation, and high levels of density-dependent recruitment compensation (e.g. striped bass *Morone saxatilis*, Moronidae)". Across the three management objectives, each evaluated under two exploitation levels, they find that compared to ML policies, HSs lead to more desirable

outcomes in terms of recreational harvest levels, trophy catch, spawning potential ratio, and the proportion of fecundity produced by the older population, but less desirable outcomes in terms of biomass yields. Despite the latter finding and taken together with results of other life-history parameterizations, the authors posit that for range of management objectives, HS policies can produce more favorable compromises between fishing-quality and conservation outcomes than ML policies.

Naturally arising from these studies is a question that has yet to be addressed thoroughly in the current body of literature. That is, to what extent does the aggregate economic value of recreational fishing vary with the imposition of minimum length only versus harvest slot policies? We address this question in the context of the recreational Atlantic striped bass fishery by quantifying both economic and biological returns to a variety of minimum length only and harvest slot policies and by doing so, compliment the stream of recent biological literature on the topic.

4 Relevant Economic Literature

Many of the economic studies concerned with assessing the potential economic effect of recreational fishing regulations estimate angler preferences or willingness-to-pay (WTP) values for marginal changes in fishing trip characteristics. As fishery managers are often concerned with the potential effect of new policies, many economists have employed stated preference (SP) methods for nonmarket valuation (Hicks 2002; Aas et al. 2000; Cha and Melstrom 2018; Knoche and Lupi 2016; Lew and Larson 2014, 2012, 2015; Lew and Seung 2010; Duffield et al. 2012; Goldsmith et al. 2018). SP methods allows researchers to evaluate angler preferences for and behavioral responses to virtually

any hypothetical policy scenario because, in contrast to revealed preference methods that require data on observed behavior, they rely on data obtained from individual responses to survey questions, carefully designed to compensate for missing or inadequate observational data.

While estimating angler WTP values is a viable way to understand the value an average angler places on catching, harvesting, or releasing fish, or on alternative sets of regulations, these values poorly describe broad economic effects of policy-induced changes in fishing trip quality. Some studies infer these effects by inserting into the utility function and incrementally changing average historical values of the explanatory variables (Goldsmith et al. 2018; Gautum and Steinback 1998). Because discrete choice models are nonlinear in explanatory variables, however, inserting average historical values of these variables can lead to biased estimates of the average response (Train 2003).

Additionally, adequately evaluating the broad economic effects of regulatory change requires considering the randomness in catch and hence the number of fishing trips that may be affected by such changes, as in a few studies on the topic. McConnell et al. (1995) estimate angler WTP values for catching additional fish using results from travel cost model and, separately, model catch at a particular fishing site as a function of the historical catch rate, time spent fishing, and experience. The effect of a bag limit is introduced by allowing it to truncate the distribution of fish kept which, under the assumption that anglers receive utility from keeping their catch, shifts the expected mean catch rate. To calculate the resultant welfare impacts while accounting for randomness in catch, McConnell et al. (1995) evaluate the behavioral model at different distributions of

catch, but the model operates under the implicit assumption that the recreational value to anglers of releasing fish is zero. More recently, Anderson et al. (2013) integrate marginal values of keeping and releasing fish, estimated using DCE data, and historical catch data into a simulation model to evaluate angler WTP to avoid fishery closures for rockfish and other species in the Puget Sound of Washington. Using the same data and a similar methodology, Anderson and Lee (2013) evaluate angler WTP for increases in salmon bag limits and test for differences in harvest values between wild and hatchery-reared salmon.

Some economists have sought to understand how regulations affect anglers as well as fish stocks (Homans and Ruliffson 1999; Anderson 1993). Woodward and Griffin (2003) develop a theoretical model of angling behavior with which they derive the short- and long-term implications of recreational bag and size limits on future stock levels. Their theoretical results suggest that bag limits are always as effective as size limits at reducing fishing mortality, but for both types of policies, the actual stock implications depend partly on whether fishing quality and angler effort are substitutes or compliments. Woodward and Griffin (2003) then apply their theoretical model to an empirical context by estimating a bioeconomic model of the Gulf of Mexico red snapper fishery. Modelling fishing demand as a function of travel costs, household income, expected catch rates, anglers' fishing experience, and boat ownership, they find that the effects of regulations on future stock levels and angler welfare is highly dependent on the discard mortality rate; when discard mortality rates are high, size limits policies can lead to outcomes that fall below the efficient frontier of welfare and spawning stock levels. Like McConnell et al. (1995), however, and due a lack of available data, their model implicitly assumes that the marginal value to anglers of releasing their catch is zero.

A few recent studies consider both randomness in catch and the recreational value of releasing fish when evaluating the aggregate effects of changes in regulations. Providing the framework for the current research, these studies simulate fishery outcomes under alternative regulatory conditions by parameterizing an aggregate demand model with the results of a choice experiment analysis. Holzer and McConnell (2017) examine how alternative assumptions about summer flounder anglers' risk preferences for harvest uncertainty affect welfare and participation predictions. The authors explicitly introduce summer flounder catch uncertainty by defining its levels in the DCE with ranges of possible outcomes, as opposed to with discrete values. Estimates from utility models specified under the assumption of risk aversion indicate that their sample is, on average, risk averse to random variation in summer flounder harvest. To introduce the effect of policy changes in the northeast U.S. recreational summer flounder fishery as shifting the entire catch distribution, which consequently affects both the mean of and dispersion in expected harvest, their simulation model specifies anglers' expected level of harvest and release as averages over multiple draws of the choice occasion. After repeating the simulation procedure under alternative risk preference parameterizations, they find that failing to account for angler risk aversion for uncertainty in harvest, when such preferences hold, can lead to misestimated predictions of welfare changes in response to changes in regulations.

Lee et al. (2017)'s bioeconomic model of recreational cod and haddock fishing in the northeast is currently used to determine fishing regulations for these species (83 *Federal Register* 18972). In this illustration of the model, they evaluate the efficacy of actual and alternative 2014 policies at reaching allowable catch limits (ACL) for both

species. They find that while both the actual and proposed 2014 policies had similarly minimal effects on stock levels three years in the future, only the actual 2014 policy met target ACLs for both species. Underlying this result, however, was an assumed 0% haddock discard mortality rate that was subsequently revised by managers to 50%. Simulation results based on the revised estimate reveal that neither the actual nor proposed 2014 policies would have successfully met the recreational haddock ACL, a finding that exemplifies how management success can depend on the time lag in obtaining new scientific information.

The structure of our aggregate demand model is similar to that employed by Holzer and McConnell (2017) and Lee et al. (2017) in that angler welfare and behavior responds to policy-induced shifts in the rate at which legal-sized fish are encountered. Yet we build on the framework established in these studies by incorporating keep and release parameters for small, medium, and trophy-sized fish such that angler behavior is also influenced by policy-induced changes in the proportion of small, medium-sized, and trophy-sized striped bass that constitute the harvestable population. This modification is essential given the findings in Section 6 that suggest the recreational value of keeping and releasing striped bass increases exponentially with the size of fish caught.

5 Angler Behavioral Model

We model of angler behavior using the choice experiment data and random utility maximum (RUM) framework described in Chapter 1. Fishing trip utility is specified as a function of the number of the number of small, medium, and trophy striped bass kept and released (*Small keep*, *Small release*, *Medium keep*, *Medium release*,

Trophy keep, and *Trophy release*, respectively), the number of other legal-sized fish caught (*Other catch*), the trip cost (*Cost*), and the opt-out alternative (*Opt_out*). We also include two indicators variables, *Min28* and *Min30*, which measure the differential impact to fishing trip utility from a 28" and 30" minimum size limit relative to the impact of 20" minimum size limit, respectively. The utility that angler n receives from alternative j , in choice scenario t , is:

$$\begin{aligned}
 U_{njt} &= V_{njt} + \varepsilon_{njt} \\
 U_{njt} &= \beta_1 \textit{Small keep}_{njt} + \beta_2 \textit{Medium keep}_{njt} + \beta_3 \textit{Trophy keep}_{njt} \\
 &\quad + \beta_4 \textit{Small release}_{njt} + \beta_5 \textit{Medium release}_{njt} + \beta_6 \textit{Trophy release}_{njt} \\
 &\quad + \beta_7 \textit{Other catch}_{njt} + \beta_8 \textit{Cost}_{njt} + \beta_9 \textit{Opt_out}_{njt} \\
 &\quad + \beta_{10} \textit{Min28}_{njt} + \beta_{11} \textit{Min30}_{njt} + \varepsilon_{njt}.
 \end{aligned} \tag{1}$$

where the indirect utility function, $V_{njt} = \beta'x_{njt}$, relates observed attributes to utility, β' measures the relative importance to anglers of the attributes x_{njt} that describe alternative j , and ε captures the utility derived from all other unobservable factors.

Equation (1) is estimated using a random parameters logit (RPL) specification, which allows some or all parameters to be randomly distributed across the population of sampled anglers. We specify the striped bass catch parameters to be normally distributed, which captures the most important sources of heterogeneity in the context of this study, and treat the other parameters as fixed.¹¹ Because respondents selected both a first- and second-most preferred alternative in each of up to four DCE questions, we treat the data as a panel and specify the response variable to be a full ranking of alternatives, as

¹¹ Alternative models in which all non-cost parameters are specified to be normally distributed yielded qualitatively similar results but at the expense less precisely estimated coefficients.

opposed to as a single choice. Compared to those that use unranked data, choice models estimated using ranked data have been shown to improve the precision, and thus reduce sampling variance of estimated utility parameters (Chapman and Staelin 1982).

Table 14 displays estimation results of Equation (1). The mean parameters on the non-striped bass attributes behave as expected. Catching other species of fish while fishing for striped bass is a boon to angler utility, as indicated by the positive and statistically significant coefficient on *Other catch*. The trip cost parameter, which represents the marginal utility of price, is negative and statistically significant. We infer from the negative and significant parameter on *Opt_out* that striped bass anglers prefer fishing for striped bass when such an opportunity is available. The coefficient on *Min28* is negative but statistically insignificant, which suggests that if at least one medium striped bass can be kept, the average angler is indifferent to regulations that permit harvest of small striped bass. One potential explanation for this result is that most recreational striped bass anglers in our sample and across the population are accustomed to fishing in the absence of small-fish harvest slot regulations. In contrast to that on *Min28*, the estimated parameter on *Min30* is negative, significant, and relatively large in magnitude, indicating that anglers are highly averse to prohibitions on the harvest of any striped bass shorter than 30". This result may be driven by angler sensitivity to changes in the status-quo, 28" recreational minimum size restriction adopted by most states in recent years (ASMFC 2017, 2016, 2015). Nonetheless, this finding is consistent with other studies that model angler utility as a function of both catch and catch-and-release regulations (Cha and Melstrom 2018; Knoche and Lupi 2016).

Table 14. Utility parameter estimates from panel rank-ordered mixed logit model.

Variable	Mean Parameters	Standard Deviations
Small keep	0.242*** (0.060)	0.967*** (0.067)
Medium keep	0.348*** (0.061)	1.156*** (0.063)
Trophy keep	0.653*** (0.080)	1.247*** (0.093)
Small release	0.088*** (0.023)	0.407*** (0.024)
Medium release	0.129** (0.058)	0.512*** (0.059)
Trophy release	0.242*** (0.034)	0.678*** (0.042)
Other catch	0.151*** (0.016)	
Cost	-0.018*** (0.002)	
Opt-out	-3.170*** (0.130)	
Min. 28"	-0.109 (0.088)	
Min. 30"	-0.644*** (0.106)	
Num. Observations	1,747	
Num. individuals	469	
Log likelihood	-2003.305	
McFadden Pseudo R ²	0.360	
AIC	4040.600	

Notes: 500 Halton draws used to maximize the simulated log-likelihood.

*, **, and *** represent significance at the 10%, 5%, and 1% level of significance, respectively.

All estimated keep and release parameters in Table 14 are positive, indicating that the recreational Atlantic striped bass fishery is executed both for sport and for personal consumption. A closer look at the magnitudes of these estimates reveals that angler utility increases almost exponentially with the size of fish kept or released. Additionally, the estimates intuitively suggest that for each size-class, anglers prefer keeping and releasing

larger striped bass to smaller ones. Table 14 also reveals the relatively high returns to angler utility from catching-and-releasing trophy striped bass. For example, the marginal utility of releasing a trophy-sized striped bass is virtually identical to that of keeping a small striped bass. Finally, the statistically significant standard deviation coefficients on each of the striped bass catch variables indicate considerable unobserved preference heterogeneity across the population of sampled anglers

7 Simulation Procedure

7.1 Overview and Study Area

To evaluate potential impacts of alternative 2015 striped bass regulations on angler welfare and participation, total recreational removals, and mature female recreational removals, we integrate the estimates in Table 14 and historical catch and effort data into an aggregate demand model. The model simulates the striped bass fishery at the trip-level under actual 2015 policy conditions and compute baseline metrics of interest. It then imposes alternative 2015 policies that affect expected angler participation and recreational fishing mortality by altering the number and size of striped bass kept and released, relative to these outcomes under the actual 2015 policy. By summing and computing differences in expected levels of angler participation, angler welfare, and recreational harvest and release across trips across actual and alternative policies, we reveal the external, short-term economic and biological costs of recreational Atlantic striped bass fishing policy.

We simulate the 2015 recreational striped bass fishery in Massachusetts, Rhode Island, and Connecticut, which together accounted for about 32% of the estimated

20,282,426 recreational striped bass fishing trips taken during 2015 in the ten coastal states from Maine through Virginia.¹² We focus on these three states because, unlike those further north or further south, each implemented the same set of striped bass regulations in 2014 and 2015. This makes the simulation procedure more tractable than it would be otherwise and allows for a straightforward test of the accuracy of its predictions, which we employ in Section 7.3.

7.2 Data and Procedure

Simulated fishing trips are first assigned a level of striped bass catch that is randomly drawn from probability distributions created using publicly-available, 2015 Marine Recreational Information Program (MRIP) data. We exclude from these data trips that record catching more than 15 striped bass, which account for 1.6% of the total number of directed striped bass trips taken in 2015 across the study region. Given observed differences in catch-per-trip levels, we generate separate catch distributions for boat- and shore-based fishing trips.¹³ Dividing the n trips that caught c striped bass from fishing mode m by the total number of directed trips taken from mode m gives a probability mass function,

$$Prob[catch]_m = \frac{n_{cm}}{\sum_{c=0}^C n_{cm}}. \quad (2)$$

These distributions are smoothed using a LOWESS (Cleveland 1979) and shown in the top panel of Figure 3.

¹² We use the Marine Recreational Information Program (MRIP) data to estimate recreational effort, defined as fishing trips on which striped bass was caught or was the primary target.

¹³ We aggregate private, charter, and head boat trips when generating the boat-based catch-per-trip distribution.

Figure 3. 2015 striped bass catch-per-trip (top) and catch-at-length (bottom) distributions.

After catch levels are assigned, fish sizes are randomly drawn from a catch-at-length distribution created using MRIP and state-level volunteer angler logbook (VAL) data. Because MRIP data contain few recorded lengths of released striped bass, we first

combine and calculate harvest and release proportions-at-length using 2015 VAL data collected by the Connecticut Volunteer Angler Survey Program and the Massachusetts Sportfish Data Collection Team (SADC) Program. Raw VAL data is displayed in Figure A2 in the Appendix. We then multiply these proportions by MRIP-based estimates of total 2015 harvest and releases across the study region. The total number of length- l fish caught (C_l) in the study region during 2015 is

$$C_l = \left[\left(\frac{release_l^{VAL}}{\sum_{l=i}^L release_l^{VAL}} \right) * total\ release^{MRIP} \right] + \left[\left(\frac{harvest_l^{VAL}}{\sum_{l=i}^L harvest_l^{VAL}} \right) * total\ harvest^{MRIP} \right]. \quad (3)$$

Calculating Equation (3) for shore and boat modes separately resulted in qualitatively similar catch-at-length distributions. We therefore aggregate MRIP and VAL data across fishing modes. The probability of an angler catching a length- l striped bass, LOWESS-smoothed and shown in the bottom panel of Figure 3, is

$$Prob[length = l] = \frac{C_l}{\sum_{l=i}^L C_l}. \quad (4)$$

We impose actual and counterfactual regulations that, along with catch, determine the realized number and size of striped bass kept and released on each simulated trip. Striped bass catch is then allocated into one of three size bins—small ($\leq 25''$), medium ($26''$ - $34''$), and trophy ($\geq 35''$)—that correspond to definitions of the striped bass catch attributes used in the choice experiment, but we retain catch lengths to calculate the age, sex, and maturity distribution of total recreational removals.

Simulated anglers harvest legal-sized striped bass as they are encountered until the bag limit is reached and discard subsequent catch. They do not selectively harvest or

high-grade.¹⁴ Based on anecdotal and empirical evidence, however, we do incorporate volunteer release behavior, the practice of releasing legal-sized fish despite not reaching the bag limit. We randomly select choice occasions having positive legal catch level and reclassify harvested fish as released fish until a pre-specified rate of voluntary release is reached. Although this reclassification affects predicted levels of recreational fishing mortality, it does not affect participation or welfare estimates because we assume that the value anglers place on a voluntarily-released striped bass is at least that of keeping that same fish.¹⁵ We specify the rate of voluntary release to be 0.25 based on calculations from Connecticut Volunteer Angler Survey Program data that span the period 2013-2016.¹⁶ Finally, simulated trips are assigned a representative, mode-specific trip cost derived from the most recent angler expenditure survey in which this information is available (Lovell et al. 2013).¹⁷

After deriving the striped bass catch and other variables corresponding to those used in the behavioral model, we compute the expected utility of each choice occasion, given by Equation (5). Following Train (2003), the probability of observing the trip,

¹⁴ Selective harvesting is when anglers discard legal-sized fish to retain the ability to harvest fish of a more preferred size; high-grading is when anglers retain but then discard legal-sized fish to harvest fish of a more preferred size.

¹⁵ It is reasonable to assume that rational anglers would not voluntarily release a legal-sized striped bass if they receive more utility from harvesting that same fish.

¹⁶ Using this data, we calculated the rate of voluntary release across the four years by examining trip logs that recording catching the exact number or fewer legal-sized striped bass permitted under the bag limit. In 2013 and 2014, the daily possession limit in Connecticut was two fish equal to or longer than 28"; in 2015 and 2016, the daily possession limit in Connecticut was one fish equal to or longer than 28". After aggregating numbers of legal-sized fish kept and released on these trips across the four years, we calculated the overall portion of voluntary released striped bass to be 48%. It is likely, however, that anglers who participate in the Connecticut Volunteer Angler Survey Program are more avid than the population of anglers at large, and therefore may be more inclined to catch-and-release legally harvestable fish. Thus, we use a more conservative estimate of voluntary release in the simulation model.

¹⁷ Specifically, costs are a weighted average of each state's average trip cost (minus lodging expenses and tournament fees) with weights proportional to the number of trips taken in each state. We generate a separate cost for shore and boat modes, the latter which reflects a weighted average across private, charter, and head boat trips.

conditional on the number and size of striped bass kept and released and the trip cost, is

$$p_t = \frac{\exp(V_{njt})}{1 + \exp(V_{njt})}. \quad (5)$$

The expected number of length- l striped bass harvested (released) on each choice occasion is the probability-weighted number of length- l fish retained (discarded).

Summing these values across choice occasions gives the total number of length- l striped bass harvested and released under a given policy scenario:

$$Harvest_l = \sum_{t=1}^T (p_t \times harvest_{lt}), \quad (6)$$

$$Release_l = \sum_{t=1}^T (p_t \times release_{lt}). \quad (7)$$

We apply the 0.09 discard mortality rate used in the striped bass stock assessment to $Release_l$ to determine dead releases-at-length. Adding dead releases-at-length to harvest-at-length and summing these values across length-classes gives total recreational removals, defined below.

$$Total\ recreational\ removals = \sum_{l=i}^L [Harvest_l + (Release_l \times 0.09)] \quad (8)$$

To calculate mature female recreational removals, we convert removals-at-length to removals-at-age using an age-length key created by combining data from three separate 2015 striped bass age-length keys provided each by the Massachusetts' Division of Marine Fisheries, New York's Department of Environmental Conservation Division of

Marine Resources, and Rhode Island's Division of Fish and Wildlife.¹⁸ We then multiply removals-at-age by the female sex proportions-at-age (s_a) and proportions mature-at-age for females (m_a) indices provided in striped bass stock assessments and sum these values across age-classes, as in Equation (9) below. We express total and mature female recreational removals in terms of biomass weight by translating numbers-at-age to weights-at-age using stock assessment conversion indices.

$$\text{Mature female removals} = \sum_{a=1}^A [(\text{Total recreational removals})_a \times s_a \times m_a]. \quad (9)$$

A principal goal of this paper is to understand the impact of recreational striped bass regulations on angler welfare. Thus, for each simulated trip we compute compensating variation (CV), which in our case indicates the level of compensation required to hold anglers' expected utility constant after a policy-induced change in fishing trip quality. Following Haab and McConnell (2002), CV for choice occasion i is

$$CV_i = \frac{1}{\beta_{cost}} \left[\ln \left(\sum_{j=1}^J \exp(V_{ij}^1) \right) - \ln \left(\sum_{j=1}^J \exp(V_{ij}^0) \right) \right], \quad (10)$$

where j indexes alternatives and V_{ij}^0 and V_{ij}^1 is anglers' expected utility under current and changed regulatory conditions, respectively. CV_i is summed across all choice occasions to infer the aggregate effect of regulatory adjustment on angler welfare.

7.3 Model Calibration

We calibrate the model by randomly selecting a subset of choice occasions such

¹⁸ Figure A2 in the Appendix displays length-age conversions based on the combined age-length data.

that $\sum_t^T p_t$ approximates the actual number of fishing trips taken in the study region during 2015. This process is employed using samples of shore- and boat-based choice occasion separately given differences in costs and catch-per-trip between the two fishing modes. The calibrated model predicts the occurrence of 4,045,220 shore- and 2,427,158 boat-based choice occasions, which closely matches the 4,045,181 and 2,427,178 respective trips taken from each mode across the study region during 2015.

Simulation model calibration diagnostics are shown in Table 15. The model overestimates the total number of striped bass caught by about 7%, which results in overestimates of total harvest, mature female harvest, release, and total removal numbers by 21%, 16%, 5%, and 13% relative to actual 2015 levels, respectively. Despite these overestimates, however, the model underestimates recreational release weight by 20%. The calibration diagnostics for mature female recreational releases provides some insight into the possible source of this discrepancy. That is, the model underestimates total numbers and weight of mature female striped bass released by 33% and 51% compared to 2015 levels, respectively, which suggests that the likelihood of voluntarily releasing striped bass may increase with the size of fish caught, a behavior that is unaccounted for in the model. This potential source of error, however, is compensated by overestimates of total catch, hence predicted levels of mature female removal numbers and weight align closely with actual 2015 levels.

Table 15. Simulation model calibration diagnostics.

	Model	2015 Actual	Error (%)
Total catch (numbers)	9,151,419	8,578,012	-6.7
Total catch (pounds)	54,306,593	63,780,363	14.9
Harvest (numbers)	837,951	693,135	-20.9
Harvest (pounds)	10,460,637	8,980,707	-16.5
Mature female harvest (numbers)	500,906	432,359	-15.9
Mature female harvest (pounds)	7,644,967	6,799,069	-12.4
Releases (numbers)	8,313,468	7,884,877	-5.4
Releases (pounds)	43,845,956	54,799,656	20.0
Mature female releases (numbers)	1,355,476	2,021,500	33.0
Mature female releases (pounds)	13,925,708	28,236,467	50.7
Total removals (numbers)	1,586,163	1,402,774	-13.1
Total removals (pounds)	14,406,773	13,912,676	-3.6
Mature female removals (numbers)	622,899	614,294	-1.4
Mature female removals (pounds)	8,898,281	9,340,351	4.7

Notes: Statistic calculated using MRIP data and information contained in the 2016 stock assessment update (ASMFC 2016) and the 2018 preliminary stock assessment (ASMFC 2018). More information about these data and the derivation of Table 15 statistics is given in Table A2 in the Appendix.

As another way to gauge the accuracy of our model at predicting changes in fishery outcomes, we compare actual and predicted changes in fishery outcomes that occurred between 2014 and 2015 when recreational striped bass fishery managers in Massachusetts, Rhode Island, and Connecticut decreased the daily bag limit from two to one fish, 28" or longer in total length. We run the simulation procedure using a catch-per-trip distribution generated using 2014 MRIP data and the 2015 catch-at-length distribution described in Section 7.2. The model is calibrated to the number of shore- and boat-based fishing trips taken across the study region in 2014.

Table 16 shows actual and predicted changes in fishery outcomes that occurred in

the study region between 2014 and 2015. Predicted changes in fishing trips, harvest weight, release numbers, and mature female removal weight approximate the actual changes that occurred between the two years. However, the model overestimates the observed decrease in recreational harvest numbers, total removals numbers and weight, and mature female removals numbers and it underestimates the observed increase in recreational releases numbers and weight. These discrepancies may be an artifact of assuming a constant, a 0.25 rate of voluntary release across simulated policies, whereas the actual rate may be higher when anglers can keep two rather than one striped bass per day. This hypothesis is somewhat validated by responses to a non-DCE question included in the angler survey that asked respondents to indicate the number of small (20" to 26"), medium- (27" to 36"), and trophy-sized (37" and 43") striped bass they would actually keep if they caught and could legally harvest two of each size-class; on average, respondents indicated that they would keep 0.96, 1.21, and 1.00 out of two harvestable small, medium, and trophy-sized striped bass caught on a trip, respectively, which suggest the rate of voluntary release may be closer to 0.5 when the bag limit is two. Were we to incorporate this behavior into the simulation, predicated changes in recreational harvest numbers, total removals numbers and weight, mature female removals numbers, and recreational releases numbers and weight would more closely align with actual changes between the two years. Nonetheless, similarity between actual and predicted changes in the two metrics of interest, total removal numbers and mature female removal weight, bolsters confidence in the model's ability to illuminate tradeoffs between angler welfare and recreational fishing mortality created by different types of recreational Atlantic striped bass fishing policy.

Table 16. Actual and predicted changes in fishery outcomes between 2014 and 2015.

	% Δ between 2014 and 2015	
	Model	Actual
Number of trips	-0.68	-3.04
Harvest (numbers)	-30.95	-16.22
Harvest (pounds)	-30.72	-28.84
Releases (numbers)	0.43	3.71
Releases (pounds)	5.88	54.79
Total removals (numbers)	-19.71	-7.20
Total removals (pounds)	-24.07	-11.98
Mature female removals (numbers)	-25.67	-13.49
Mature female removals (pounds)	-26.60	-20.91

8 Policy Simulation

We simulate the effect of twenty-nine alternative recreational striped bass fishing policies on angler welfare, total recreational fishing removals, and female SSB recreational removal volume. Each policy analyzed specifies a one- or two-fish daily possession limit and a 20", 24", or 28" minimum size limit. We examine impacts to fishery outcomes from full or partial harvest restrictions on larger striped bass by specifying across the policies analyzed several types of maximum harvest size restrictions. In addition to minimum length only policies that exclude such a restriction, some policies specify a maximum size limit that is eight or sixteen inches longer than the minimum size limit; we refer to these policies as narrow and wide harvest slots, respectively. We also examine the effect of more complex, partial harvest restrictions on larger striped bass by simulating two-fish policies defined by two separate, adjacent narrow harvest slots for each fish in the bag limit, as well as policies defined by a narrow slot limit for the first fish in the bag limit only. The last type of size restriction analyzed

also partially restricts harvest of larger striped bass; these slot-option policies allow anglers to harvest either two smaller, or one smaller and one larger striped bass. Table A3 in the Appendix displays raw outcomes for each the 29 policies analyzed.

To easily assess the relative economic efficiency of changes in biological outcomes under the various policies analyzed, we plot simulated outcomes in Figures 4, 5, and 6. Figure 4 display each policy's short-term production plan in terms of welfare and recreational removal numbers, while Figure 6 displays production plans in terms of welfare and female SSB recreational removal weight. Figure 5 plots outcomes of the two-fish bag limit policies only, allowing us to decipher trends in the effect of different types of size restrictions on welfare and recreational fishing mortality while holding bag limits constant. Relative changes in total and mature female fishing mortality are expressed in terms of fish numbers and fish weights, respectively, as these metrics correspond to those used in the striped bass stock assessment for estimating the rate of fishing mortality and the level of female spawning stock biomass. We interpret these indices as inputs to the production of welfare such that in each figure, the outermost policies plotted shape the efficient frontier.

8.1 Simulation Results

Tradeoffs between angler welfare and total recreational removals created by the twenty-six policies analyzed are displayed in Figure 4. Overall, the figure implies a positive and linear relationship between the aggregate economic value of the fishery and recreational removals, with the least- and most-restrictive policies analyzed, A2 (2 fish $\geq$ 20") and E1 (1 fish 28-36"), predicted to yield the highest and lowest relative increase in

both outcomes, respectively.

Figure 4. Predicted changes in welfare and recreational removals under alternative 2015 policies in MA, CT, and RI. Actual 2015 policy of one-fish, 28" or longer

The simulated outcomes of policies H1 (1 fish 28-44") and E1 (1 fish 28-36") provide some insight into the primary research objective of evaluating the short-run

economic and biological returns to minimum length only versus harvest slot policies in the recreational Atlantic striped bass fishery. Policies H1 and E1 are the sole policies analyzed predicted to achieve a reduction in recreational fishing mortality relative to expected levels under the actual 2015 minimum-length-only policy. Furthermore, despite slight to moderate relative reductions in angler welfare that are predicted to occur under both policies, each policy lies along the efficient frontier. If accurate, this suggests that by implementing these policies, managers could consciously reduce the social value of the fishery in exchange for an efficient reduction in recreational fishing mortality.

Figure 4 reveals the sensitivity of angler welfare and recreational removals to changes in the minimum size limit. Across policy types, recreational fishing mortality increases incrementally from 2015 levels with each incremental four-inch decreases in the minimum size limit from 28". These changes are due to concurrent, disproportionate increases in the rate at which anglers encounter legally harvestable, yet smaller striped bass that reflect the shape of the 2015 striped bass catch-at-length probability distribution (Figure 3). Along with this effect and despite the lower nonmarket value of smaller striped bass compared to larger ones, decreasing the minimum size limit also leads to aggregate angler welfare gains because the nonmarket value of harvesting a striped bass is nearly thrice that of releasing the same fish across each of the three size-classes analyzed. Simulated outcomes of policies A1 (1 fish $\geq 20''$) and D1 (1 fish 20-36") also exemplify the responsiveness of angler welfare to decreases in the baseline, 28" minimum size limit. These policies constrain the bag limit to one, yet both are predicted to produce higher returns to angler welfare than all two-fish, 28" minimum size limit policies analyzed. Taken together, these results imply that, rather than the relative

difference in nonmarket value between small, medium, and trophy-sized fish, the effect of policy action on angler welfare is driven largely by how these actions affect the rate at which anglers encounter legally harvestable fish.

However, the relative economic efficiency of policy-induced changes in recreational fishing mortality also depends on rate at which anglers encounter legally harvestable striped bass. This can be seen in Panels A, B and C of Figure 5, where we plot the outcomes of policies defined by 20", 24", and 28" minimum size limit, separately. For example, by specifying a wide range of legal harvest sizes and not constraining the number of small or medium-sized striped bass that may be kept under the bag limit, minimum length only (A2, H2, and O2), slot-option (F2, M2, T2, G2, N2, and U2), and wide harvest slot policies (C2, J2, Q2) induce larger relative increases in recreational removals compared to other types of size restrictions in each of panels A, B, and C. Hence, for reasons discussed above, these policies also induce relatively large, positive changes in angler welfare. Conversely, policies defined by differentiated size restrictions for each striped bass in the bag limit (D2, E2, K2, L2, R2, and S2), while more expensive in terms of angler welfare compared to minimum length only, slot-option, and wide harvest slot policies, are among the most effective at mitigating recreational fishing mortality. This finding is intuitive because such policies constrain the number of frequently-encountered, small and medium-sized striped bass that may be harvested under the bag limit to one. Policies that fully direct harvest toward smaller fish, however, are excessively costly to both anglers and fish stocks, as all three narrow slot limit policies, policies B2 (2 fish 20-28"), I2 (2 fish 24-32"), and P2 (2 fish 28-36"), are

predicted to inefficiently generate angler welfare from recreational removals.

Figure 5. Predicted changes in welfare and recreational removals (Panels A, B, and C) and welfare and female SSB recreational removal weight (Panels D, E, and F) under alternative 2015 two-fish bag limit policies in MA, CT, and RI. Actual 2015 policy of one-fish, 28" or longer used as baseline policy.

Thus, directing effort toward or away from certain striped bass size-classes via specification of the size limit has important implications for angler welfare and recreational fishing mortality. When accompanying a one-fish increase in the current daily bag limit, size limits that allow but do not require both striped bass under the bag limit to be small or medium-sized will likely induce considerable net gains in angler welfare that come at the expense of high levels of recreational fishing mortality. Although the magnitude of change in both outcomes diminishes under size limits that allow anglers to harvest one small and medium-sized, or one medium- and one trophy-sized striped bass only, these changes remain efficient relative to expected outcomes of the other types of size limits analyzed. However, size limits requiring both fish in the bag limit to be small or medium-sized may lead to an inefficient utilization of the stock.

While Figures 4 and 5 show that several sets of efficient recreational striped bass fishing regulations are available when proposed policy action intends to influence total recreational fishing mortality, there exist many fewer efficient policy options when these actions seek to protect the fecund striped bass population. Figure 6 displays impacts of the simulated policies on angler welfare and female SSB removals. The number of policies forming the efficient frontier is reduced dramatically compared to that in Figure 4 and only six policies, each defined by a baseline minimum size limits of 20", are predicted to achieve efficient changes in female SSB removal volume relative to the actual 2015 policy. Across policy types, relative changes in female SSB removals volume increase in magnitude with each incremental, four-inch increase in the baseline minimum size limit because such policy adjustments direct harvest toward the mature female striped bass population. Due to the relatively low rate at which anglers encounter this

population of striped bass, however, we see no clear relationship between female SSB removal volume and angler welfare.

○: 20" minimum size	□: 24" minimum size	△: 28" minimum size
A1: 1 fish ≥ 20"	B1: 1 fish ≥ 24"	E1: 1 fish 28-36"
C1: 1 fish 20-28"	F1: 1 fish 24-32"	H1: 1 fish 28-44"
D1: 1 fish 20-36"	G1: 1 fish 24-40"	O2: 2 fish ≥ 28"
A2: 2 fish ≥ 20"	H2: 2 fish ≥ 24"	P2: 2 fish 28-36"
B2: 2 fish 20-28"	I2: 2 fish 24-32"	Q2: 2 fish 28-44"
C2: 2 fish 20-36"	J2: 2 fish 24-40"	R2: 1 fish 28-36" & 1 fish > 36"
D2: 1 fish 20-28" & 1 fish > 28"	K2: 1 fish 24-32" & 1 fish > 32"	S2: 1 fish, 28-36" & 1 fish > 36 to 44"
E2: 1 fish 20-28" & 1 fish > 28 to 36"	L2: 1 fish 24-32" & 1 fish > 32 to 40"	T2: 2 fish 28-36"; only 1 fish > 36"
F2: 2 fish 20-28"; only 1 fish > 28"	M2: 2 fish 24-32"; only 1 fish > 32"	U2: 2 fish 28-36"; only 1 fish > 36 to 44"
G2: 2 fish 20-28"; only 1 fish > 28 to 36"	N2: 2 fish 24-32"; only 1 fish > 32 to 40"	

Figure 6. Predicted changes in welfare and female SSB recreational removal weight under alternative 2015 policies in MA, CT, and RI. Actual 2015 policy of one-fish, 28" or longer used as baseline policy.

Assessing the incentives faced by anglers can uncover the important tradeoffs between welfare and conservation created by minimum length only and harvest slot restrictions, as exemplified by the outcomes of policies G1 (1 fish 24-40") and O2 (2 fish ≥ 28 "). The model predicts the two policies yielding roughly the same relative impact to angler welfare and total recreational removals, as shown in Figure 4. Yet Figure 6 shows a marked divergence in each policy's relative effect on female SSB removal volume. While policy O2 is predicted to induce a 38% increase, policy G1 is predicted to achieve a 30% decrease in female SSB recreational removal volume relative to the simulated outcome of the actual 2015 policy. These differential impacts reflect the interface between regulations and the biological characteristics that govern the natural growth and reproductive processes of Atlantic striped bass, thus highlighting the importance of accounting for such characteristics when proposed policy action intends to protect the fecund population.

As before, we plot the impacts to angler welfare and female SSB removals across each set of two-fish bag limit policies separately in panel D, E and F of Figure 5. These panels reveal the important influence of minimum and maximum size limits on female SSB removal volume. Policy O2 (2 fish ≥ 28 "), for example, is predicted to yield a 40% increase in female SSB removal volume relative to the simulated outcome of the actual 2015 policy. But the magnitude of this relative change dampens with each incremental, four-inch decrease in the minimum size limit, as policies H2 (2 fish ≥ 24 ") and A2 (2 fish ≥ 20 ") are predicted to achieve a 22% and 2% relative increase female SSB removal volume, respectively. We find even larger differential impacts to female SSB removals

when comparing changes that occur under policy O2, H2, and A2 with each policy's narrow harvest slot analogue: policy P2 (2 fish 28-36"), I2 (2 fish 24-32"), and B2 (2 fish 20-28"), are predicted to achieve a -14%, -42%, and -59% relative change in female SSB removal volume, respectively. Thus, whereas panels A, B, and C reveal narrow harvest slot policies leading to inefficient outcomes in terms of welfare and total recreational removals, here we find that, compared to others in panels D, E, and F, these policies are both efficient and among the most effective at protecting fecund striped bass. Finally, in each of panels D and E, policies D2 (1 fish 20-28" & 1 fish > 28") and E2 (1 fish 20-28" & 1 fish > 28-36"), and policies K2 (1 fish 24-32" & 1 fish > 32") and L2 (1 fish 24-32" & 1 fish > 32-40") are shown to inefficiently generate welfare from female SSB removal volume. By requiring one fish in the two-fish bag limit to be medium- or trophy-sized, these policies direct effort toward the fecund population. Given the relatively low frequency at which angler encounter this population, these policies provide little returns to angler welfare.

Figures 4, 5, and 6 illuminate the important, short-run biological ramifications of decreasing the current 28" minimum size limit or implementing harvest restrictions on medium- and trophy-sized striped bass. These policy adjustments effectively protect fecund striped bass and are likely to improve the population's spawning potential, at least in the short-run. But such adjustments are conducive to high rates of recreational fishing mortality for small- to medium-sized striped bass. This may yield adverse impacts to the stock structure in the medium- to long-run. While outside the scope of this study, integrating a biological growth model into the simulation would allow us to evaluate the medium- and long-run stock implications of recreational Atlantic striped bass fishing

policy.

9 Conclusion

In this study, we evaluated the short-run economic and biological impact of different types of recreational Atlantic striped bass fishing policy. We parameterized an aggregate demand model with the results of 2016 choice experiment survey to assess the relative impact of alternative 2015 policies to angler welfare, angler participation, and total and mature female recreational fishing mortality in Massachusetts, Connecticut, and Rhode Island. We selected as alternatives to the actual 2015 policy of one fish, 28” or longer, several one- and two-fish bag limit policies that varied in specification of the minimum and maximum size limit. We modelled trip-level angler behavior as a function of trip cost and the number of small, medium-sized, and trophy-sized striped bass kept and released. Met through incorporating these size-specific harvest and release preference parameters into the aggregate demand model, one research objective was to examine the economic and biological impact of full and partial harvest restrictions on trophy-sized striped bass that to-date have not been considered jointly in the policymaking process.

Angler welfare was found to be highly responsive to changes in the minimum size limit. Our model predicts that incremental, four-inch decreases in the minimum size limit from 28” induce considerable gains to angler welfare levels. These gains stem primarily from anglers encountering and harvesting legal-sized striped bass more frequently, hence such policy actions also generate high levels of recreational removals. Implementing size limits that fully or partially restrict harvest of medium- or trophy-sized striped bass were found to be effective at protecting the fecund population of striped bass. While one might

expect such harvest restrictions inducing considerable adverse impacts to angler welfare, we find some instances in which these costs are low relative to the short-run stock benefits they may provide.

We found that a wide range of efficient policies are available when the primary purpose of proposed policy action across the studied region is to control total recreational fishing mortality. When proposed management action is intended to curtail mature female recreational fishing mortality, however, proposed policy action that does not account for potential economic consequences can lead to inefficient outcomes, as exemplified by several of the policies analyzed lying inside the efficient frontier of welfare and female SSB recreational removals volume. This finding illuminates the practicality of assessing angler responses to regulatory stimuli to select efficient and effective policies, particularly when fisheries managers seeks to balance socioeconomic goals with multiple conservation objectives.

We assessed the potential economic and biological tradeoffs that are created by recreational striped bass fishing regulations such that they can be considered during the policymaking process. Yet anglers' decision-making processes is complex, and thus it was necessary to make several assumptions that potentially introduce bias in our results. First, we assumed that striped bass anglers harvest the first legal-size fish encountered up to the bag limit, but it is likely that some selectively harvest fish. Anglers might exhibit such behavior in response to encountering a school or "blitz" of striped bass while fishing, while others may simply prefer certain size-classes of striped bass to others. Second, we assumed a constant, 0.25 rate of voluntary release, but the actual rate may vary with the bag limit or the size of catch . Additionally, we specify a linear-in-catch

utility function based on the attributes and levels selected for the experimental design.

However, diminishing marginal utility of harvesting and release striped bass is perhaps a more accurate depiction of returns to angler utility. In the future, we hope to reassess the validity of these assumptions and update our analysis appropriately.

Our study is also limited by its relatively narrow geographical and temporal scope. We focus on one sub-region of the fishery but expanding our analysis to the coast wide fishery is the natural next step in this line of research. Additionally, while we evaluate the short-run effects of regulations on total and female SSB removals, these outcomes may differ in the medium- and long-run; changes in total recreational removals, for example, may be endogenous to changes in female SSB. By integrating a biological growth model into the simulation procedure, we could consider these dynamics and assess the future stock impacts of recreational striped bass fishing regulations.

CHAPTER 3

SUSTAINABILITY AND TOURISM: THE EFFECT OF THE UNITED STATES' FIRST OFFSHORE WIND FARM ON THE VACATION RENTAL MARKET

by

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is submitted to *Resource and Energy Economics*

1 Introduction

Although U.S. offshore wind currently accounts for only 0.03% of the 96.5 gigawatts of installed wind capacity in the country (American Wind Energy Association 2018), future growth in wind generation will likely be more concentrated offshore. The political climate is evolving with federal policies that encourage wind power development and with aggressive, state-level renewable energy objectives to source wind power offshore.¹⁹ The industry itself reached an important milestone on December 12, 2016 when America's first and to date only offshore wind farm (OSWF), the five turbine, 30 megawatt (MW) Block Island Wind Farm (BIWF), began generating electricity. Partially due to the success of BIWF, Massachusetts, Rhode Island, and Connecticut recently awarded contracts for 800, 400, and 200 MW OSWFs that are expected to be

¹⁹ Massachusetts requires state electricity providers to procure 1,600 MW of offshore wind capacity by 2027 (Massachusetts 2016); New York has committed to develop up to 2,400 MW of offshore wind power by 2030 (NYSERDA 2016); Maryland recently awarded two offshore wind projects the right to receive Offshore Wind Renewable Energy Credits as part of the state's Offshore Wind Energy Act of 2013 (Maryland 2013); New Jersey's governor signed an Executive Order on January 31, 2018 to promote the development of 3,500 MWs of offshore wind energy generation by 2030 (P. D. Murphy 2018).

operational by 2021, 2023, and 2023, respectively, assuming permits are granted. Other OSWF projects along the U.S. east coast are also forthcoming, including New York's recently approved 90-MW South Fork Wind Farm that could be operational in 2022 and Maryland's 120-MW OSWF project, Skipjack Wind, whose offshore construction will likely begin in 2021 with generation set for 2022.

Despite the progress, there are several impediments to widespread growth of U.S. offshore wind energy. The high levelized cost of producing offshore wind energy makes it difficult to compete with other energy sources without subsidies.²⁰ At the federal level, the absence of federally mandated offshore wind energy goals, the short-term and inconsistent nature of production tax credits, and the imposition of lease and royalty fee payments can discourage development (Portman et al. 2009). At the local level, community members and other stakeholder groups have fervently opposed proposed offshore wind energy facilities, as exemplified by failed development plans of Cape Wind off the coast of Massachusetts. OSWFs have been opposed for several reasons, ranging from the impacts to marine fauna, the loss of recreational and commercial fishing grounds, the environmental and human safety risks of ship-turbine collisions, and the effects on nearby property values.²¹ Snyder and Kaiser (2009) discuss several of the ecological and socioeconomic arguments used in favor of and against offshore wind power.

²⁰ Estimates suggest that the levelized cost of offshore wind is among the highest of all sources of energy production (U.S. Energy Information Administration 2018a).

²¹ The extent to which these claims materialize depend on site-specific factors, hence growing with the industry is a body of case studies investigating the ecological (Bergström et al. 2014; Lindeboom et al. 2011) and socioeconomic (Jensen et al. 2018) impacts of OSWF installations. In some sense, however, whether there is basis in the academic literature for these claims is irrelevant; valid or not, these claims can impede OSWF development.

In coastal communities, one of the most commonly voiced concerns is that OSWF development will deter tourists. Rudolph (2014) examines how stakeholders rationalized this apprehension during the planning phase of two OSWFs in Germany and Scotland. Opponents invoked several lines of reasoning for why the two OSWFs might detract from the area's desirability and therefore hurt the tourism industry, including that the wind farms would visually disturb the seascape, erode the area's cultural character and identity, or interfere with recreational activities like boating and fishing. Except for the latter, these concerns seem valid in the context of American OSWF development based on suggestive findings from a few recent studies (Parsons and Firestone 2018; Firestone et al. 2018; ten Brink and Dalton 2018). However, there exists no empirical evidence to substantiate the overall claim that OSWFs negatively affect tourism. Filling this research gap is critical because local conflicts about the impact of OSWFs on tourism can have important implications for where, and how far offshore, proposed offshore wind power facilities are located.

The purpose of this paper is to assess the effect of offshore wind development on tourism by examining the effect of the BIWF on the vacation rental market. The BIWF stands within Rhode Island state waters, approximately three miles off the coast of Block Island, and is visible from any location on Block Island that has a direct view, as well as from ferry rides to and from the mainland. We use data from AirBnb over the period October 2014 to December 2017, which spans before and after construction of the BIWF. Our method is rooted in a hedonic valuation framework, and we estimate a difference-in-differences (DD) model using three nearby tourist destinations as controls. Our specification includes property fixed effects to mitigate omitted variable bias, as well as

temporal variables that control for seasonality and trends in the vacation rental market.

Using this modelling approach, we focus purely on understanding visitor preferences for the BIWF and leave evaluating impacts to permanent residents for future work.

The model yields an island-wide treatment effect, which is most relevant for assessing tourism impacts in this context for two reasons. First, there are several impacts of the BIWF's presence, like the creation of new recreational fishing opportunities or the symbolization of progress toward clean energy, that are unrelated to visibility but might nonetheless stimulate overnight visits to the island. Second, the small geographical size of Block Island—about 10 square miles—allows for easy access to the best views of the turbines from any location on the island; hence, overnight visitors need not rent properties that are in direct viewshed to experience the wind farm.²² Moreover, it is likely that few Block Island AirBnb properties in our sample are in direct viewshed of the wind farm.

Block Island offers an excellent setting for examining visitor preferences for the BIWF because the tourism industry is the backbone of the local economy. While home to about 1,000 permanent residents, Block Island can host up to 20,000 visitors per day during peak summer season (New Shoreham Planning Board 2016). Thus, by establishing a baseline and examining post-construction movements in the vacation rental market relative to other tourist destinations, we infer how tourists, in aggregate, respond

²² To put this in perspective, visitors can traverse almost the entire island on a 16-mile bike loop that stops at the BIWF and all 12 of its other major sites.

to the wind farm.²³ If the overall tourist experience changes because of the BIWF, then the vacation rental market will change accordingly.

There are two noteworthy features of this analysis. First, our study evaluates multiple margins of adjustment, which contrasts with many previous hedonic studies applied to the vacation rental market that evaluate only price adjustments.²⁴ We estimate our model using five different dependent variables: booked price, number of nights available, number of nights reserved, occupancy rate, and revenue. Because the speed at which vacation rental prices respond to environmental shocks is unknown,²⁵ it is important to evaluate other margins of adjustment that may be more elastic. Furthermore, rental market adjustments may differ in the short-run (1 year) and the long-run (3-5 years). The price and availability of a rental property should be codetermined in the long-run. In the short-run, however, there may be a divergence in the various rental market metrics because landlords do not immediately respond to environmental shocks, but renters do. If this were the case, we would expect to see changes in the number of booked nights, occupancy rates, and revenues, but not in prices nor availability.

Second, our study is the first to empirically test the effect of offshore wind farms on tourism within a revealed preference framework. Other studies, reviewed in Section 2, have evaluated preferences for OSWFs using stated preference approaches, but these data

²³ Almost certainly, there are tourists that are attracted by and repulsed by the BIWF and everywhere in between. Our measures are aggregate, and we cannot distinguish preferences of individuals or even the proportion of tourists falling into different categories.

²⁴ Applications include hedonic pricing of: tourist activity and online reputation (Perles Ribes et al. 2018), rural recreation amenities (Nelson 2010), seascape amenities (J. M. Hamilton 2007), smoking prohibitions (Benjamin et al. 2001), access to coastal beaches (Taylor and Smith 2000), and land-uses in Spain (Bilbao-Terol et al. 2017), Belgium (Vanslembrouck et al. 2005) and France (LeGoffe 2000)

²⁵ To the best of our knowledge, no study has explored the dynamics of vacation rental property price responsiveness. While intuition may suggest that more transactions would lead to faster price changes, (Lang 2015) finds that amenity changes are capitalized more quickly for owner occupied housing than rental housing.

can be biased for many reasons, including recall error, motivated reasoning, or just outright lying. Especially in the case of renewable energy development, support for which can be tied to a person's political ideology (Kennedy 2017), results may be biased as respondents seek to influence outcomes. Biases in this manner have been documented with stated preference measures in similarly politically contentious issues of gun control and climate change (Kahan et al. 2017; Goebbert et al. 2012; Howe and Leiserowitz 2013; Lang 2014).

Our results suggest that construction of the BIWF led to significant increase in nightly reservations, occupancy rates, and monthly revenues for properties in Block Island during the peak-tourism months of July and August. Specifically, we estimate that, during each peak-tourism month of July and August following construction, the BIWF caused a seven-night increase in reservations, a nineteen percentage point increase in occupancy rates, and a \$3,490 increase in revenue for AirBnb properties in Block Island relative to AirBnb properties in control cities. In other months, treatments effects are statistically insignificant, though results are often consistent with positive effects. We find no significant movements in nightly price, despite this being likely the easiest margin of adjustment. Overall, there is little within-property, temporal variation in prices, suggesting prices are “sticky”, and that landlords are experiencing changes to other margins of the vacation rental market. While specific to this context, these findings mitigate concerns about negative effects of OSFWs on local tourism.

The paper proceeds as follows. In the next section, we review relevant literature. Section 3 discusses the data and methodology. We provide results in Section 4 and we conclude in Section 5.

2 Literature review

Our research is grounded in hedonic price theory, which postulates that the overall price of a good is determined by the part-worth contribution from each observable attribute (Rosen 1974). Hedonic analysis is among the most popular revealed preference approaches for evaluating preferences for non-market goods and environmental amenities. Applied to a context of residential housing prices, the hedonic pricing method (HPM) relates sale prices of housing transactions to a vector of property attributes that typically include intrinsic, locational, and environmental characteristics. Intrinsic characteristics are factors such as the size of the house, the size of the lot, the number of bathrooms, and the number of bedrooms. Locational characteristics can include the condition of nearby homes, the crime rate, and quality of schools. In the field of environmental economics, regressors of interest are one or more environmental characteristic that describes a non-market amenity, such as air quality, adjacent open space, and ocean views.

HPM has been applied to estimate the implicit value of a wide range of amenities and disamenities related to energy extraction and production: power plants (Davis 2011), fracking (Muehlenbachs et al. 2015; Boslett et al. 2016), air quality (Chay and Greenstone 2005; Bento et al. 2015); and transmission lines (Hamilton and Schwann 1995). Several studies use hedonic methods to infer the external cost of onshore wind turbine facilities. Those that employ a quasi-experimental identification strategy generally find insignificant or slightly negative property value impacts from turbine proximity (Dröes and Koster 2016; Hoen and Atkinson-Palombo 2017; Hoen 2014) or turbine view (Gibbons 2015; Lang et al. 2014). However, two recent papers suggest

larger housing price devaluations. Sunak and Madlener (2016) estimate a 9-14% decrease in values for properties “extremely” to “moderately” visually disturbed by wind turbines. Heintzelman et al. (2017) analyze upstate New York properties and find that the value of homes with a full or partial view of a turbine were reduced by about 17% following turbine construction. Jensen et al. (2018) is the only study to date that estimates property value impacts from offshore wind energy facilities within a hedonic valuation framework. Their results indicate that neither of two Danish OSWFs under study had any significant effect on the prices of primary or secondary homes.²⁶ While the bulk of HPM onshore wind studies indicate zero to negative price effect, this may not carry over to the vacation rental market because valuation may be a function of the time horizon spent around the turbines.²⁷ For example, utility gains from seeing the turbines for the first time or over the course of a couple of days of vacation may outweigh the loss of unfettered ocean views, but for a permanent resident, 10 years of lost views may outweigh everything else and lead to net utility losses.

Although HPM applications to offshore wind are limited, there is a substantial body of economic literature examining preferences for and tourism impacts of OSWFs. Most of these studies employ stated preference approaches, which use questionnaire responses to infer preferences and values. These approaches are appealing in the context of offshore wind development because observational data is limited or, as it was in the U.S. prior to the BIWF, non-existent. Yet the novelty of offshore wind development also raises concerns about the validity of evaluating its external cost using stated preference

²⁶ The two offshore wind farms under study in Jensen et al. (2018) consist of 72 and 90 turbines and are located approximately six and two miles offshore, respectively.

²⁷ More broadly, some amenities (e.g., local school quality) are expected to be reflected in the price of nearby housing but not in the price of nearby vacation rentals, and vice versa.

data. These data may be affected by the degree of respondent familiarity and experience with the good or amenity in question (Boyle et al. 1993; Cameron and Englin 1997), which is limited when it comes to OSWFs; nearly all the existing nonmarket valuation studies of OSWFs analyze stated preference data generated by individuals who lack any experience with this type of environmental amenity. Observational data, if representative of the population of interest, is not subject to this potential source of bias nor others, like sample selection bias, protest and strategic response bias, and hypothetical bias that may threaten valid inference. Moreover, it is generally argued that individuals' behavior in the market can convey information about their core preferences for nonmarket goods and amenities. We therefore believe our revealed preference approach to illuminating the socioeconomic impacts of OSWFs is a critical departure from the current body of literature. Nonetheless, it is important to review the existing economic literature that uses stated preference methods to infer such impacts. This stream of literature can be classified into two groups: the first estimates the implicit cost of visual disturbances from OSWFs and the second estimates the impact of these facilities on aggregate recreational visitation and beach use.

With the exception of a few studies that find mixed preferences for OSWFs (Fooks et al. 2017a; Westerberg et al. 2013), the first group of stated preference studies generally reveal OSWFs to be an environmental disamenity. These studies find that the visual disturbance from an OSWF located near the shore can generate considerable welfare losses for individuals, but these losses diminish as the distance of the OSWF from shore increases (Ladenburg and Dubgaard 2009, 2007; Krueger et al. 2011; Landry et al. 2012). Among this group, our study is perhaps most closely related to the work of

Lutzeyer et al. (2018), who evaluate potential responses of the vacation rentals market to OSWF development. They survey recent renters of oceanfront and ocean-view vacation properties in North Carolina and assess their preferences for future rentals with different utility-scale wind farm configurations using a choice experiment.²⁸ For all visible turbine configurations, utility parameter estimates are negative and significant, which suggests that this population of renters, on average, strongly prefers unobstructed views of seascape. This result is broadly consistent with Fooks et al. (2017b), who, using an incentive compatible elicitation mechanism, find that tourists prefer hotel rooms without a view of an onshore wind turbine to those with a view of a turbine. Lutzeyer et al. (2018) also estimate utility parameters using a latent class model. These results reveal substantial heterogeneity in preferences across respondent groups, ranging from repulsion for all visible turbine configurations to indifference and even attraction to certain visible configurations, relative to the status-quo of no visible turbines. However, positive utility estimates from this model never translate to statistically significant willingness-to-pay values for moving OSWF turbines closer to shore.

The second group of stated preference studies are less conclusive about the impact of OSWFs. Landry et al. (2012) estimate an aggregate demand model to assess the behavioral response of North Carolina residents to a widespread offshore wind energy development scenario: 100-turbine OSWFs located one mile off the coast of all major beach destinations in North Carolina. They find indistinguishable differences in the expected number of annual beach trips between the hypothetical windfarm scenario and the current, no-windfarm scenario. Most recently, Parsons and Firestone (2018) employ a

²⁸ The most intrusive visible OSWF configuration has 144 turbines and is located five miles offshore; the least intrusive visible OSWF configuration has 64 turbines and is located 18 miles offshore.

contingent behavior web survey to evaluate beachgoer perceptions about offshore wind development and behavioral responses to OSWFs at beaches along the U.S. east coast. Consistent with the findings from other studies, theirs suggest that wind farms located close to the shore, within about 13 miles, will lead to reductions in beach trips and economic losses in form of foregone beachgoer welfare.

One complication with accurately predicting the net impact of OSWFs on coastal recreational is the population of recreators may change. Parsons and Firestone (2018) estimate that, for an average beach, the first OSWF could generate nearly 13 million additional “curiosity trips” over the course of five to ten years from people who would not otherwise visit that beach. These estimates are not included in their main results, but the authors note that, if realized, an influx of curiosity trips of this magnitude would likely lead to net positive effects for many beaches. Other studies have also evidenced the potential for new OSWFs to attract tourists. In Lilley et al. (2010)’s intercept survey of Delaware beachgoers, 66% of out-of-state residents indicated being somewhat or very likely to visit a new or different beach at least once to see a 200-turbine OSWF located approximately six miles from the beach. In Firestone et al. (2009)’s mail survey, 84% of Delaware residents expressed being somewhat or very likely to visit a new or different beach at least once to see a 500-turbine OSWF located six miles from the beach.

It is difficult to draw conclusions about the projected impact of OSWFs on coastal recreation given the findings uncovered across the relevant stated preference literature. People prefer seascape horizons that are uncontaminated by wind turbines, but it remains unclear if and to what extent their behavior will change in response to OSWFs, as well as how many will be attracted to new OSWFs. Furthermore, many of the studies mentioned

above capture preferences prior to OSWF installation, and preferences and support may change once OSWFs are installed. For example, Firestone et al. (2018) survey residents of Block Island, near-coastal Rhode Island, and coastal Rhode Island both before and after operation of the BIWF to understand changes in and determinants of support for the BIWF. Compared to those in the pre-installation period, levels of support in the post-operation period increased across all three strata, yet only among the coastal Rhode Island stratum were these changes in opinion found to be statistically significant. The authors also find that a respective 83% and 78% of Block Island and non-Block Island residents who saw the BIWF “[liked] the way the turbines looked”, and this factor most strongly determined current support for the BIWF. In sum, the impacts of OSWFs on coastal recreation and tourism remains ambiguous. A concrete understanding of these impacts is vital for managers and developers of U.S. offshore wind resources to accurately assess externalities of OSWF development.

3 Data and Methods

In this section, we discuss the study context and data in relation to the econometric modelling strategy, sample construction, and identifying assumptions. Section 3.1 provides a timeline of events that guides our definition of the treatment period. Section 3.2 gives an overview of the data. We specify the econometric models in Section 3.3. Construction of the sample is outlined in Section 3.4 and sample characteristics are described in Section 3.5. Finally, Section 3.6 discusses the assumptions behind the DD estimator and potential threats to identification.

3.1 Timeline of Events

First established in 2004, Rhode Island's Renewable Energy Standard (RES) requires that 38.5% of the state's electricity come from renewable resources by the end of 2035. RES targets began in 2007, requiring electricity providers to source 3% of their retail sales from renewable resources, with incremental increases in target levels each year. To help meet the goals of the RES, in 2008 Rhode Island selected Deepwater Wind as the state's preferred offshore wind developer and initiated the Ocean Spatial Area Management Plan (Ocean SAMP), a marine zoning plan that provides management recommendations for developing and protecting Rhode Island's marine resources (Smythe and McCann 2018). Approved in 2011, the Ocean SAMP identified the waters off the southern coast of Block Island as having the highest wind speeds and lowest relative costs of development within RI state waters, and thus deemed this area viable for offshore renewable energy development. The Ocean SAMP designated this 13 square-mile area, which extends east to southwest of Block Island, a Renewable Energy Zone (REZ) (Coastal Resources Management Council 2010).

Following approval of the Ocean SAMP, Deepwater Wind surveyed the sea floor within the REZ to determine potential locations for the turbine foundations and the two underwater cables, one connecting Block Island to the BIWF and one connecting Block Island to mainland Rhode Island.²⁹ Deepwater Wind opted to locate the turbine array within southeast portion of the REZ to minimize environmental impacts and costs

²⁹ A fiber optic cable for high speed-internet access was included in the undersea cable connecting Block Island to mainland Rhode Island. Block Island renters having better internet connection due to the construction of the BIWF may lead to identification problems. However, our data cover the period when the necessary on-island infrastructure was not yet built, hence renters experienced no change in internet service quality due to the BIWF over the course of the study period.

(Deepwater Wind 2012). They submitted state and federal permit applications for the wind farm in 2012 and received the final permit needed to advance the project in September 2014. In March 2015, Deepwater Wind fully financed the BIWF project by securing more than \$290 million in loans.

Offshore construction of the BIWF project commenced in the summer of 2015. By the end of the 2015 offshore construction season, in early December, turbine foundations that protrude slightly from the water had been set in place. At this point, scheduled strategically to avoid overlap with the tourist season, onshore construction activities began and lasted through spring of 2016. The 2016 offshore construction season started in early August and ended soon after, on August 18, 2016, when Deepwater Wind installed the fifth and final 600-foot-tall, 6 MW wind turbine. On December 12, 2016, after several weeks of testing, the BIWF began providing wind-generated electricity to mainland Rhode Island. Block Island was connected to the BIWF's electrical grid in May of 2017, prior to which four diesel generators sourced the island's electricity needs. Now, because Block Island relies primarily on the electricity generated from the BIWF, these diesel generators operate only occasionally, which reduces air and noise pollution on one part of the island.

Our identification strategy involves comparing pre- and post-treatment rental activities, thus it is necessary to define when the treatment period begins, which is a bit ambiguous. In our case, the most important determinant of treatment-induced rental market adjustments is public awareness of the BIWF, so that tourists can take the information into account when deciding where to visit. The natural candidates are the

dates of completed construction and grid connection.³⁰ We choose to define treatment as completed construction because that is when the turbines are visible, but the Appendix discusses results from models that use an alternative treatment date defined by grid connection.

An additional event, unrelated to BIWF, is necessary to discuss. In March 2017, corporate representatives from Airbnb visited Block Island and Nantucket Island to increase the number of Airbnb listings in those locations. They were particularly focused on encouraging owners of existing boutique hotel and bed-and-breakfast properties to use the Airbnb platform. Their visit to Block Island seems to have had the intended effect because beginning in March 2017, the data reveal an influx of new Block Island Airbnb properties,³¹ most of which are boutique hotels or bed-and-breakfasts. This event motivates some key modelling decisions, and we discuss its relevance in more detail in Section 3.4.

3.2 Data

Airbnb is an online hospitality service that provides people with short-term lodging options from hosts seeking to rent out their rooms or properties. We obtained Airbnb rental data from AirDNA, a company that collects publicly available information about individual properties from the Airbnb website. AirDNA currently tracks the performance of roughly four million Airbnb listings worldwide through an automated scraping procedure that occurs every three days. The data cover a 39-month period

³⁰ A simple Google Trends query for “Block Island Wind Farm” confirms these milestones as important, as the weeks including August 18, 2016 and December 12, 2016 are the two highest points of search interest.

³¹ Figure A4 in the Appendix displays this graphically.

starting in October 2014, when AirDNA began collecting this information, to December 2017. Both daily and monthly data is provided, but we estimate our model using the monthly-aggregated data to ease interpretation of results.³²

The dataset contains two important types of information on each property: rental activities and property characteristics. Rental activities include nightly rates, monthly revenues, and whether nights are reserved, available but not reserved, or blocked by the host and thus unavailable for reservation. We use this information to generate our dependent variables. Property characteristics include city, number of bedrooms, number of bathrooms, minimum length of stay, maximum number of guests allowed, cleaning fee, extra people fee, security deposit, listing type (private room, entire place, etc.), and property type. There are a variety of property types included in the data and we aggregate them into four categories: bed-and-breakfasts, apartments, guest suites, and houses. Approximate latitude and longitude coordinates are also included, and we use these variables to calculate Euclidean distance to the coast. In Figure A5 in the Appendix, we plot these approximate locations to ascertain the visibility of the BIWF from our sample of Airbnb properties. Also included in Figure A5 is a visibility map of the area surrounding Block Island, adapted from Griffin et al. (2015). The figure suggests that few Block Island properties are in direct viewshed of the wind farm.

We estimate econometric models using five different dependent variables. These variables are measured at the monthly level and are defined as follows: (1) Available nights, which equals the sum of reserved and available nights, (2) Reservation nights, which equals the number of nights a property was booked, (3) Occupancy rate, which is

³² We obtain qualitatively similar results when we estimate our model using the daily data.

equal to Reservation nights divided by Available nights, (4) Average booked rate, which equals the average price of booked nights, and (5) Revenue, which is equal to total monthly Airbnb revenue.³³ Because owners determine directly the number of nights their property is available and its price, short-run changes in Available nights and Average booked rate might capture supply-side responses. Conversely, short-run changes in Reservation nights, Revenue, and Occupancy rate may be more representative of consumer demand. While these variables are of course related and determined by many of the same forces, our goal is to understand different margins of adjustment and get a broad picture of the whole story of how the vacation rental market responds to an environmental shock.

Our method is rooted in hedonic valuation; however, our data are not the standard property sales typically used with this method. As a first step to build confidence in our data and as exploration of implicit prices in the vacation rental market, we estimate a basic, cross-sectional hedonic regression with $\log(\textit{Average booked rate})$ on the left-hand side and property characteristics on the right-hand side. We use all observations occurring before construction of the BIWF.

The estimated coefficients in Table 17 generally follow the direction of a priori expectations, and thus bolster our confidence that valuable signals can be recovered from the data. Properties with greater numbers of bedrooms or bathrooms command higher rental rates. Those within 0.1 miles of the coast come with a substantial, roughly 30% rental premium. A one-person increase in the maximum number of guests allowed to stay

³³ *Average booked rate* and *Revenue* also include a per-visit cleaning fee, but additional fees charged for extra people are not visible on the Airbnb website and are therefore not included in the calculation of these variables.

at a property increases average booked rates by about 5%. After controlling for other rental rate determinants, rental rates for houses and bed-and-breakfasts are not statistically different than rental rates for apartments; guest suites, however, are booked at 13% lower average price than apartments. Compared to Block Island properties, rental rates are more than 40% lower in Narragansett, RI and Westerly, RI, and about 19% higher in Nantucket, MA. Average booked rates are highest relative to January in July, August, and September.

Table 17. Determinants of nightly booked rates: OLS estimation results.

Variable	Dependent Variable: log(Average booked rate)	
	Coefficient	Standard
Bedrooms	0.101***	(0.039)
Bathrooms	0.126***	(0.035)
Within 0.1 miles of coast (1=yes)	0.274***	(0.059)
Minimum stay	0.007	(0.006)
Maximum number of guests allowed	0.049***	(0.017)
Security deposit (\$100's)	0.024***	(0.007)
Extra people fee (\$100's)	0.040	(0.041)
House	0.018	(0.059)
Bed and breakfast	0.057	(0.072)
Guest suite	-0.131*	(0.075)
Nantucket	0.188**	(0.091)
Narragansett	-0.436***	(0.084)
Westerly	-0.480***	(0.106)
February	-0.017	(0.094)
March	-0.134	(0.089)
April	0.131	(0.081)
May	0.309***	(0.081)
June	0.287***	(0.082)
July	0.408***	(0.082)
August	0.406***	(0.082)
September	0.444***	(0.090)
October	0.300***	(0.094)
November	0.256**	(0.101)
December	0.312***	(0.082)
2015	0.108	(0.070)
2016	0.254***	(0.084)
Observations	2,188	
R-squared	0.701	

Notes: Sample contains property-months with at least one reservation night and is restricted to observations occurring prior to September 2016. Standard errors are clustered at the property level. *, **, and *** represent significance at the 10%, 5%, and 1% level of significance, respectively.

3.3 Econometric Models

We use a DD modeling strategy to examine the effect of the BIWF on the vacation rental market. We compare rental transactions in Block Island, the treated group,

to other tourist destinations, the control group, before and after construction of the wind farm. Control locations are Narragansett, RI, Westerly, RI, and Nantucket, MA. These cities are comparable to Block Island in that they are highly desirable summer vacation and tourist destinations in Southern New England. Figure 7 shows a map of all four cities and the approximate location of the BIWF. Narragansett and Westerly are located on the southern coast of mainland Rhode Island, approximately 10 miles from Block Island.³⁴ Nantucket is located approximately 20 miles off the coast of Cape Cod, Massachusetts and, like Block Island, offers a unique island experience to visitors. A standard DD equation applied to this context can be written as:

$$y_{ict} = \beta_1(BI_{ic} \times Post_construction_t) + \beta_2 BI_{ic} + \beta_3 Post_construction_t + X'_{ict}\theta + \varepsilon_{ict}, \quad (1)$$

where y_{ict} is the outcome variable for property i in city c during year-month t , BI_{ic} is a dummy variable that equals one if a property is in Block Island, and $Post_construction_t$ is a dummy variable that equals one if an observation occurred during the post-construction period. Although construction of the BIWF was completed on August 18, 2016, we specify the post-construction period to begin on September 2016 because our data are aggregated to the monthly level.³⁵ Property characteristics are contained in X_{ict} . Finally, ε_{ict} is the error term. We cluster errors at the property level to allow correlation across time within individual properties. The difference in rental market outcomes between Block Island and control groups cities, and between the pre- and post-treatment

³⁴ The BIWF can be seen from a few locations on the southern portion of Narragansett. From these locations, however, the turbines appear as an extremely small cluster on the horizon and can be perceived only under certain weather and sky conditions.

³⁵ In the Appendix, we provide results from models that exclude August 2016 from the sample given this treatment status uncertainty. These results are very similar to our main results.

period, are measured by β_2 and β_3 , respectively. β_1 is the coefficient of interest, and it measures the differential change in rental market outcomes from the pre-treatment period for Block Island properties relative to changes in rental market outcomes for properties in Narragansett, Westerly, and Nantucket.

Figure 7. Geographic location of treated and control locations and the BIWF turbines.

Equation (1) is a standard DD model, but we chose to strengthen it with several sets of fixed-effects and other control variables. First, we include property fixed effects that purge from the error term any unobservable time-invariant factors, such as nearby amenities and online appeal, that both affect rental market outcomes and differ across individual properties. Second, we include year-month fixed effects that capture region-wide temporal variation in rental activity. Such variation is particularly large in this

context because of the highly seasonal nature of the vacation rentals market. Third, we include city-specific time trends to control for rental market trends at the city level. These variables are critical for disentangling impacts of the BIWF from other potential location-specific growth trends. After including these variables, our new specification is

$$y_{ict} = \beta_1(BI_{ic} \times Post_construction_t) + X'_{ict}\theta + \gamma_t + \alpha_i + \delta_c t + \varepsilon_{ict}, \quad (2)$$

where terms are as described previously with the addition of α_i , the property fixed-effects, γ_t , the year-month fixed effects, and δ_c , which estimate the city-specific time trends. We find that models which include year-month fixed effects and city-specific time trends are, across the five dependent variables, broadly superior in terms model fit and Akaike Information Criteria (AIC) than those that omit one or both of sets of controls; Table A4 of the Appendix discusses the results of models that add these control variables sequentially.

All time-invariant property characteristics, including property location, distance to the coast, and property type are excluded from estimation due to the inclusion of property fixed-effects. Yet for a small portion of properties, listed amenities such as minimum length of stay, maximum number of guests, security deposit, cleaning fees, and fees for extra people do change over time, and hence we include them in X_{ict} .³⁶ If these time-varying property amenities are endogenous to treatment, however, their inclusion in model would violate the basic identification condition $E[X\varepsilon] = 0$ and render OLS estimates inconsistent. This is a plausible source of endogeneity for our study, considering that landlords in Block Island or elsewhere may have, in response to the

³⁶ Models for *Average booked rate* and *Revenue* exclude cleaning fees from the vector of time-varying property amenities because these fees are incorporated in the dependent variable.

BIWF, sought out additional means to make their properties more attractive—by decreasing the minimum length of stay or extra-people fee, for example. To address this concern, we first examined properties in the main estimation sample (Table 18) and found that only a few properties in Nantucket or Narraganset—no Block Island properties—varied their amenities over time (Appendix Table A6). Next, we estimated DD models like those defined by Equations (3) and (4) below but specified the time-varying property amenities as the dependent variable (Appendix Table A7). Although these models reveal negative and significant treatment effects for one of the five property amenity variables, these effects are driven by a few properties in Nantucket and the coefficient estimates are negligible in magnitude. Based on these findings, we take all time-varying property characteristic as exogenous to treatment.

The treatment effect in Equations (1) and (2) is an average across all months of the year. Because most rental market activity occurs during the tourist season,³⁷ we hypothesize that treatment effects may be different during this period compared to other times of the year. Hence, we specify two models that differentiate treatment effects by time of year. In the first, we interact the treatment effect term $BI \times Post_construction$ with indicator variables for summer and off-summer, where summer is defined as the months of June, July, August, and September. The second model is similar, but further differentiates peak (July and August) and off-peak (June and September) summer. We choose to specify these models such that the full effect of treatment in each season is represented by a single coefficient on a triple interaction term. These two models are defined below.

³⁷ To see this, Table A5 in the Appendix displays each month's contribution to the total sample revenue and reservation nights that accrued over 2015, 2016, and 2017.

$$\begin{aligned}
y_{ict} = & \beta_1(BI_{ic} \times Post_construction_t \times Off_summer_t) \\
& + \beta_2(BI_{ic} \times Post_construction_t \times Summer_t) \\
& + \beta_3(BI_{ic} \times Off_summer_t) + \beta_4(BI_{ic} \times Summer_t) \\
& + X'_{ict}\theta + \gamma_t + \alpha_i + \delta_c t + \varepsilon_{ict}
\end{aligned} \tag{3}$$

$$\begin{aligned}
y_{ict} = & \beta_1(BI_{ic} \times Off_summer_t \times Post_construction_t) \\
& + \beta_2(BI_{ic} \times July_Aug_t \times Post_construction_t) \\
& + \beta_3(BI_{ic} \times June_Sep_t \times Post_construction_t) \\
& + \beta_4(BI_{ic} \times Off_summer_t) + \beta_5(BI_{ic} \times July_Aug_t) \\
& + \beta_6(BI_{ic} \times June_Sep_t) + X'_{ict}\theta + \gamma_t + \alpha_i + \delta_c t + \varepsilon_{ict}
\end{aligned} \tag{4}$$

3.4 Sample Construction

The full dataset comprises 1,368 AirBnb rental properties and \$39.5 million in rental transaction revenue. Omitted from Equations (2), (3), and (4), however, are 630 properties that are active only during the post-treatment period and 120 properties that are active only during the pre-treatment period because for these properties, the within-property variation in pre- and post-treatment rental market outcomes necessary to identify a treatment effect does not exist. We refrain from estimating Equations (2), (3), and (4) without property fixed-effects, which would retain these properties in the sample, because, as discussed in Section 3.1, corporate representatives from AirBnb seem to have successfully persuaded many existing Block Island bed-and-breakfast properties to begin using the Airbnb platform during the post-treatment period, and thus we are missing important pre-treatment information for these properties. We also examined the 630 properties active only during the post-treatment period and found significant differences in means between treatment groups for almost all housing characteristic variables,

including a 45% higher proportion of bed-and-breakfast properties in Block Island. We would have additionally liked to examine the extensive margin by looking at new entrants into the market. However, given the coincidence of Airbnb's corporate visit to the island, we cannot separate the impact of that event from new entrants due to the wind farm. Thus, we focus only on the intensive margin, and leave the extensive for future research in a different setting.

We subsequently remove all bed-and-breakfasts from our sample because the outcome variables for these properties may be measured with error. We find an abundance of "blocked" property-nights, during both summer and off-summer months, in the rental histories of these properties. With near certainty, these properties can be rented year-round, so it is likely that some "blocked" nights indicate reservations arranged outside of the Airbnb platform.³⁸ If this type of measurement error is correlated with any of the independent variables, OLS estimates will be biased and inconsistent (Wooldridge 2013).³⁹ After removing bed-and-breakfasts, we have 590 properties in our sample.

To improve comparability between treated and control group properties, we remove control group properties whose number of bathrooms, number of bedrooms, or minimum length of stay are outside the range of values observed for treated group properties. These excluded properties have more than six bedrooms, more than five bathrooms, or require a minimum stay of more than seven nights. Our final sample consists of 558 Airbnb rental properties.

³⁸ Some of these properties in Block Island do use alternative rental platforms as confirmed by members of the Block Island Chamber of Commerce who have relationships with these property owners.

³⁹ The independent variable most likely to be correlated with the measurement error is the treatment group indicator, because these types of properties account for a substantially higher proportion of the remaining sample properties in Block Island (30%) than in other cities (6%).

3.5 Sample Characteristics

Table 18 assesses the degree of similarity between properties in the treatment and control group by displaying pre-treatment means and differences in means between groups. Variables are taken as averages across all pre-treatment months in which a property had at least one available night or reservation night. Block Island properties have fewer available and reservation nights by about 2.5 nights per month than control properties. Pre-treatment period monthly revenue is also lower in Block Island by about \$1,000 per month, which is intuitive given the differences in monthly reservation nights and the mean of average booked rates for Block Island properties (\$559). Pre-treatment occupancy rates and average booked rates are not statistically different between treated and control groups.

The housing characteristic control variables are well-balanced between groups. The average Block Island property has three bedrooms and two bathrooms, and requires a minimum stay of 3.6 nights, a roughly \$500 security deposit, and \$15 for each person above the maximum number of guests allowed. Twenty percent of properties in each treatment group are located within 0.1 miles of the coast. Each treatment group contains mostly houses, but apartments constitute a higher, though statistically insignificant, proportion of the sample in Block Island than in Narragansett, Westerly, and Nantucket.

Table 18. Summary statistics of property characteristics.

Variable	Pre-treatment means (standard deviations)		Difference in means
	Block Island	Control cities	
Available nights	21.19 (7.61)	23.72 (6.18)	-2.53** (1.18)
Reservation nights	2.85 (2.54)	5.40 (5.22)	-2.55*** (0.96)
Occupancy rate	0.18 (0.19)	0.23 (0.21)	-0.05 (0.04)
Revenue (\$)	1495.83 (1452.91)	2506.39 (3198.00)	-1010.56* (587.68)
Average booked rate (\$)	559.18 (304.65)	554.97 (469.85)	4.20 (97.04)
Bedrooms	2.93 (1.28)	2.85 (1.47)	0.08 (0.27)
Bathrooms	1.95 (1.06)	2.03 (1.09)	-0.08 (0.20)
Within 0.1 miles of coast (1=yes)	0.20 (0.41)	0.20 (0.40)	-0.00 (0.08)
Minimum stay (number of nights)	3.63 (2.06)	3.63 (2.11)	0.00 (0.40)
Maximum number of guests allowed	6.63 (2.24)	6.20 (3.11)	0.44 (0.58)
Security deposit (\$)	493.33 (365.24)	422.04 (521.72)	71.29 (96.61)
Extra people fee (\$)	13.67 (31.43)	12.79 (34.96)	0.87 (6.53)
House (1=yes)	0.80 (0.41)	0.87 (0.33)	-0.07 (0.06)
Apartment (1=yes)	0.20 (0.41)	0.11 (0.32)	0.09 (0.06)
Observations	30	528	558

Notes: Property characteristic variables are taken as average values across all pre-treatment months in which a property had one or more available or reservation night. For the variable Average booked rate, the number of observations across columns is 24, 447, and 471 due to some properties having zero rental transactions during the pre-treatment period. Standard errors below in parenthesis in the difference in means column. *, **, and *** represent significance at the 10%, 5%, and 1% level of significance, respectively.

3.6 Assumptions

While the results in Table 18 suggest that treated properties have common support along the spectrum of control group properties, the DD estimator relies on two untestable, identifying assumptions. First, we must assume that in the absence of treatment, differences in outcomes between treatment groups would remain constant over time. Support for this “common trends” assumption can be found by visually inspecting outcome trends during the pre-treatment period. Because the properties included in the sample change over time, instead of graphing raw outcome means, we estimate a version of Equation (2) that excludes the interaction term $BI \times Post_construction$, recover the residuals, and calculate differences in residuals between treatment and control.⁴⁰ Figure 8 plots these estimated differences with 95% confidence intervals.

⁴⁰ For completeness, Figure A6 in the Appendix displays graphs of raw outcome means between treated and control groups.

Figure 8. Pre-treatment trends in dependent variables. Notes: A version of Equation (2) that excludes the interaction term $BI \times Post_construction$ is estimated for each dependent variable and residuals are calculated. Figures plot differences in residuals between treatment and control by month. 95% confidence intervals plotted in gray. Vertical red lines indicate the onset of the treatment period.

Figure 8 reveals that treated and control groups have similar trends in Reservation nights, Occupancy rate, Average booked rate, and Revenue during the pre-treatment period.⁴¹ For these outcome variables, we find relatively large but statistically insignificant differences in some pre-treatment period months, but these differences likely reflect the small sample size of the treated group. Figure 8 also shows that differences in residuals for Reservation nights, Occupancy rate, Average booked, and Revenue are

⁴¹ For the *Average booked rate* plot, missing values of differences in residuals reflect months in which no Block Island properties transacted; missing confidence intervals reflect months in which only one Block Island property transacted.

largest—and statistically significant for all but the latter outcome—during the post-treatment period, which is unobserved in these models. Our DD model specification serves to identify the portion of this unobserved variation attributable to the BIWF. The one concerning result in Figure 8 is the large and statistically significant deviation in Available nights residuals that immediately precedes treatment. One possible explanation is that the construction phase of the BIWF rendered Block Island a less attractive tourist destination, prompting landlords in Block Island to reduce monthly availability. However, this seems unlikely because we see reductions in Available nights during the summer of 2015, when offshore construction began, for both groups (Appendix Figure A6). Alternatively, Block Island landlords may use other rental platforms as their primary means of renting out rooms during the summer, resulting in a fewer number of available nights during the summer than at other times of the year. This explanation is equally questionable because we observe Block Island-specific reductions in available nights during the summers of 2015 and 2016, but not in the summer of 2017 (Appendix Figure A6). Nonetheless, the treated group’s decrease in monthly availability during the months preceding treatment will result in DD estimators that overstate the effect of the BIWF on Available nights.

The second major assumption necessary for casual inference in DD models is the stable unit treatment value assumption (SUTVA), which requires that treatment does not affect the outcome of the control group (Rubin 1980). In the context of our study, this means we assume that the BIWF had no impact on rental activities in Nantucket, Narragansett, or Westerly. However, there are two plausible scenarios that would lead to a SUTVA violation. First, tourists may view the control locations as substitutes for Block

Island. If they are attracted to the BIWF, then they may vacation on Block Island instead of their normal destination of Nantucket. Or, if they are repulsed by the BIWF, they may do the opposite. This substitution behavior would lead to exaggerated treatment effect estimates. A second possibility is that the BIWF is an attractive force even in control group cities. This is a concern particularly for Narragansett, as this is one of the main ports for ferries to Block Island. Tourists may be more likely to visit Narragansett knowing they can take a day-trip to Block Island to see the turbines. This SUTVA violation would lead to an underestimate of positive treatment effects. Given that we estimate positive treatment effects, the possible SUTVA violations have opposing effects, which renders any resulting bias ambiguous. While we cannot verify the SUTVA assumption holds, when we estimate models that omit Nantucket or Narragansett, the estimates change in the opposite way as would be expected if the hypothesized SUTVA violations were true. Thus, we proceed cautiously that the SUTVA holds.

4 Results

Table 19 presents our main results. Panel A reports estimates from Equation (3), and Panel B and Panel C come from Equation (4). All models include property fixed effects, property amenity variables that change over time, year-month fixed effects, and city-specific time trends.⁴²

We first discuss the results in Panel A. We find positive and significant summer and off-summer treatment effects on Available nights, and the range of point estimates imply an increase of between 2.7 and 6 available nights per month for Block Island

⁴² Results from estimating Equation 2 are displayed in Table A4 in the Online Appendix.

properties in response to the BIWF. However, these results are likely overestimates of the true effect of treatment on Available nights given the pre-treatment parallel trend issues discussed in Section 3.6. The summer treatment effect on Reservation nights is positive and statistically significant, and its coefficient indicates a 4.3-night increase in the number of reservations for the average Block Island property in each month from June through September. The coefficient representing the off-summer treatment effect on Occupancy rate is significant at the 10% level of confidence, indicating a seven percentage point decrease in occupancy rates for treated properties during off-summer months. In contrast, the summer treatment effect on Occupancy rate is positive but statistically insignificant. Estimated summer and off-summer treatment effects on Average booked rate are positive but insignificant, each with large standards errors.⁴³ Finally, Panel A shows a significant summer treatment effect on Revenue. The magnitude of this coefficient implies that construction of the BIWF induced monthly revenue gains of \$1,721 for Block Island properties relative to control group properties during the following summer months of June, July, August, and September.

⁴³ The large standard errors likely reflect the smaller sample size used in these models - we exclude property-month observations with zero rental transactions. Further, there is limited residual variation in prices remaining after controlling for property-specific factors, as shown in Table A4 in the Appendix.

Table 19. The effect of BIWF on the vacation rental market.

<i>Panel A: Summer and off-summer treatment effects</i>					
	Available nights	Reservation nights	Occupancy rate	Average booked rate	Revenue
BI×Post_construction×Off_summer	2.675* (1.494)	-0.164 (0.809)	-0.069* (0.039)	47.960 (34.959)	-55.881 (436.147)
BI×Post_construction×Summer	6.010*** (1.621)	4.312*** (1.264)	0.083 (0.052)	7.787 (47.337)	1721.120** (869.615)
Observations	10,019	10,019	10,019	4,385	10,019
R-squared	0.254	0.481	0.512	0.930	0.412
<i>Panel B: Off-summer, peak-summer, and off-peak summer treatment effects</i>					
	Available nights	Reservation nights	Occupancy rate	Average booked rate	Revenue
BI×Post_construction×Off_summer	2.065 (1.582)	-0.266 (0.791)	-0.055 (0.037)	32.351 (30.877)	-32.868 (378.234)
BI×Post_construction×July_Aug	7.416*** (2.280)	7.081*** (1.837)	0.188*** (0.071)	-18.750 (55.573)	3489.919** (1451.393)
BI×Post_construction×June_Sep	2.519 (1.766)	1.248 (1.263)	0.028 (0.052)	-5.771 (36.469)	75.870 (798.076)
Observations	10,019	10,019	10,019	4,385	10,019
R-squared	0.255	0.482	0.512	0.930	0.413
<i>Panel C: Restricted sample, June through September</i>					
	Available nights	Reservation nights	Occupancy rate	Average booked rate	Revenue
BI×Post_construction×July_Aug	8.935*** (3.001)	6.010*** (2.119)	0.131 (0.085)	-10.265 (70.513)	3398.752** (1687.622)
BI×Post_construction×June_Sep	4.339* (2.262)	-0.556 (1.763)	-0.068 (0.075)	-13.724 (36.679)	-550.277 (989.486)
Observations	3,923	3,923	3,923	2,649	3,923
R-squared	0.358	0.505	0.542	0.946	0.490

Notes: 'BI' stand for Block Island, 'Post_construction' is an indicator variable for the post-construction (treatment) period, 'Summer' is an indicator variable for the months of June, July, August, and September, 'Off_summer' is an indicator variable for the months of October through May, 'July_Aug' is an indicator variable for the months of July and August, and 'June_Sep' is an indicator variable for the months of June and September. Included in all regressions as controls are minimum stay (number of nights), maximum number of guests, security deposit (\$), extra people fee (\$), and cleaning fee (\$); however, regressions for Average booked rate and Revenue exclude cleaning fees, as these fees are incorporated in the outcome variable. All regressions include property fixed-effects, year-month fixed effects, city time trends, and a constant term. Standard errors are shown below in parenthesis and clustered at the property level. *, **, and *** represent significance at the 10%, 5%, and 1% level of significance, respectively.

Panel B of Table 19 presents a similar story, but indicates that all treatment effects are occurring in the peak tourism months of July and August. For Available nights, the treatment effect is 7.416 for July and August, but just 2.519 for June and September. In

the Reservation nights and Revenue models, we observe a similar pattern, but the treatment effects grow substantially in magnitude for July and August relative to Panel A. The magnitude of these coefficients implies that construction of the BIWF caused a seven-night increase in the number of nights booked and a \$3,490 increase in Airbnb revenue in each of July and August for Block Island properties relative to control group properties. These effects are considerable, as the seven-night treatment effect on Reservation nights represents a roughly 125% increase relative to the average number of Reservation nights among Block Island properties during pre-treatment months of July and August. This result is somewhat comparable to Parsons and Firestone (2018)'s findings that curiosity trips to a first OSWF project at larger beaches (five million visitors per year) along the U.S. east coast could lead to a 40% annual increase in beach trips, and that at smaller beaches (half a million visitors per year), the potential market for curiosity trips could lead to a 400% increases in annual beach trips.

Panel B also lends evidence to support demand increasing rather than supply-side adjustments. By disentangling the effect of treatment during the peak-tourism months of July and August from its average effect across all four summer months, the differential increase in Available nights over Reservation nights becomes smaller. As a result, and in contrast to Panel A, the coefficient representing the treatment effect on Occupancy rate during July and August in Panel B is positive and highly significant, indicating a 19 percentage point increase in occupancy rates during these months for Block Island properties, relative to the control group properties. In other months of the year, the effect of treatment on Occupancy rate is statistically insignificant. This finding implies that, during the peak-tourism months of July and August following construction, the BIWF

yielded a disproportionately higher effect on Reservation nights than on Available nights, which suggests that treatment-induced changes in Reservation nights are not driven purely by treatment-induced changes in Available nights. In other words, this finding is evidence that our results are driven by changes in consumer demand, as measured by changes in Reservation nights, as opposed to supply-side responses that are reflected by changes in Available nights.

Because the data generating process may differ between summer and off-summer months, the models in Panel C use a sample that is restricted to observations occurring from June through September. This sample captures almost 75% of sample Revenue and Reservation nights in Panel B. Except for those pertaining to Available nights, estimated peak-summer treatment effects in Panel C are attenuated compared to those Panel B, but results are broadly consistent between the two panels. Panel C reveals lower but comparable peak-summer treatment effects on Reservation nights and Revenue, which is further evidence that the effect of treatment is largely confined to the peak summer months of July and August. Like in Panels A and B, we see estimate no significant change in prices, which bolsters the idea that landlords set prices and stick to them while experiencing changes to other margins of the vacation rental market.

In sum, we broadly see increases in rental activity during July and August and no change in other months. This could indicate that rental activity in the months of September through June is unresponsive to the BIWF; however, it is more likely a byproduct of the sparsity of rental activity during these months relative to July and August. Each panel of Table 19 yields similar results, yet treatment effects on Reservation nights, Occupancy rate, and Revenue, are most precisely estimated when

differentiated between peak-summer (July and August), off-peak summer (June and September), and off-summer (October through May) months. Our preferred set of results are therefore those in Panel B.

As stated before, the focus of this paper is tourism and not impacts to permanent residents, and one reason for this is the ambiguity of our results applied to permanent residents. The positive treatment effects on Revenue could imply welfare gains. However, landlords may view the BIWF as a disamenity and decide to stay in their property less often and increase its availability on the rental market. If this leads to welfare losses that outweigh concurrent Airbnb revenue gains, the net effect on landowners would be negative. While this behavior is plausible, results in Figure 8 lend credence to the idea that construction of the BIWF had little effect on rental market participation. The figure shows that only in the model for Available nights do differences in residuals between treated and control group cities remain relatively constant during the post-construction period. We view this as additional evidence that our results driven primarily by changes in consumer demand.

4.1 Heterogeneity of impacts by property characteristics

If rental sorting behavior occurs across different segments of the population, there may be heterogeneity in the effect of the BIWF that depends on property characteristics. In Table 20, we investigate heterogeneity in the effect of treatment across two property characteristics: *Bedrooms*, which is the mean-centered number of bedrooms, and *Coast*, which is a dummy variable that equals one if a property is within 0.1 miles of the coast. Note that we examine heterogeneity with respect to *Coast* not to illuminate the

differential effects of treatment with respect to turbine visibility, as we cannot ascertain this factor from the data, but rather to discern whether different segments of the vacation rental market are more strongly affected by treatment than others. Specifically, properties located within 0.1 miles of the coast are, on average across all four cities, 27% more expensive than properties located further inland (Table 17), hence these properties are likely to accommodate a different segment of the renter population.

Each column of Table 20 shows results from two models. The models are specified by Equation (4), but they also include all two- and three-way interactions between the property amenity variable of interest, *BI*, *Post_construction*, and the seasonal indicator variables that are necessary to identify differential effects of treatment by season and property characteristic. These differential effects are measured by coefficients on the four-way interactions terms displayed in the table. Because estimated *Off_summer* and *June_Sep* treatment effects have been largely insignificant, Table 20 displays the estimated coefficient on the main and interacted *July_Aug* treatment effect only. Other variables are not displayed in Table 20 for ease of exposition. We also report under each set of estimates the linear combination of the two coefficients displayed. These estimates indicate the effect of treatment for properties with one bedroom above the mean or properties on the coast.

Table 20. Heterogeneity of BIWF treatment effects by property characteristic.

Variable	Available nights	Reservation nights	Occupancy rate	Average booked rate	Revenue
$\beta_1(\text{BI} \times \text{Post_construction} \times \text{July_Aug})$	5.025** (2.094)	6.943*** (2.130)	0.213*** (0.076)	-64.411 (45.133)	2600.966** (1191.415)
$\beta_2(\text{BI} \times \text{Post_construction} \times \text{July_Aug} \times \text{Bedrooms})$	0.950 (1.606)	0.666 (1.331)	0.057 (0.052)	39.547 (55.504)	1397.293 (1407.110)
$\beta_1 + \beta_2$	5.975*** (2.273)	7.609*** (1.863)	0.270*** (0.069)	-24.863 (81.244)	3998.258** (1869.507)
$\beta_1(\text{BI} \times \text{Post_construction} \times \text{July_Aug})$	5.959*** (2.172)	6.898*** (2.126)	0.199*** (0.076)	-44.627 (56.675)	3102.638* (1640.852)
$\beta_2(\text{BI} \times \text{Post_construction} \times \text{July_Aug} \times \text{Coast})$	-3.290 (4.371)	4.213 (4.785)	0.382 (0.301)	-7.015 (86.818)	6381.021** (2736.047)
$\beta_1 + \beta_2$	2.668 (3.795)	11.110*** (4.327)	0.581** (0.292)	-51.642 (69.043)	9483.660*** (2200.988)

Notes: Estimated interaction coefficients from two separate regressions, delineated by horizontal lines, are shown in each column. Estimated coefficients on other variables are not displayed, but a full table of results is available upon request. ‘BI’ stand for Block Island, ‘Post_construction’ is an indicator variable for the post-construction (treatment) period, ‘July_Aug’ is an indicator variable for the months of July and August, ‘Bedrooms’ is the mean-centered number of bedrooms, and ‘Coast’ is a dummy variable that equals one if a property is within 0.1 miles of the coast and zero otherwise. Numbers of observations across columns are listed in Panel B of Table 4. Included in all regressions as controls are minimum stay (number of nights), maximum number of guests, security deposit (\$), extra people fee (\$), and cleaning fee (\$); however, regressions for Average booked rate and Revenue exclude cleaning fees, as these fees are incorporated in the outcome variable. All regressions include property fixed-effects, year-month fixed effects, city time trends, and a constant term. Standard errors are shown below in parenthesis and clustered at the property level. *, **, and *** represent significance at the 10%, 5%, and 1% level of significance, respectively.

Overall, we see little evidence of heterogeneity across property characteristics, but with a couple suggestive findings. Differential treatment effects on *Available nights*, *Reservation nights*, *Occupancy rate*, and *Average booked rate* are statistically insignificant for each property amenity variable. However, models that disentangle treatment effects on *Reservation nights*, *Occupancy rate*, and *Revenue* between properties with and above the sample average number of bedrooms yield an interesting result: for each outcome variable, the coefficients on $(\text{BI} \times \text{Post_construction} \times \text{July_Aug} \times \text{Bedrooms})$ is positive and the total effect of treatment on properties having one more

bedroom than the sample average is significant at the 5% level or higher. These results imply that properties able to accommodate larger parties are more strongly affected by treatment than those able to accommodate smaller parties. They may also be an indication that treatment-induced changes in rental market outcomes are not driven purely by “curiosity trips” to the wind farm, which we would expect to be composed of smaller parties.

The coefficient on $(BI \times Post_construction \times July_Aug \times Coast)$ in the model for *Revenue* implies a significant, \$6,381 difference in the effect of treatment between properties located within and those located further than 0.1 miles from the coast. Additionally, the effect of treatment on *Reservation nights*, *Occupancy rate*, and *Revenue* for properties located within 0.1 miles from the coast properties is significant and considerably larger in magnitude than its effect on properties located further inland. Given these findings and that coastal proximity comes with a substantial rental premium, it is possible that the positive treatment impacts estimated by our preferred specification are driven largely by behavioral changes occurring among the high-income segment of the vacation renter population.

5 Conclusion

In this study we evaluate the impact of the BIWF on tourism as measured by changes in local AirBnb rental market activity. Within a hedonic valuation framework, we estimate a series of DD models using scraped AirBnb data. To uncover the full story of how the BIWF impacted the local rental market, we estimate each model using multiple

dependent variables, each of which derives from a confluence of supply- and demand-side adjustments.

We find that the installation of the BIWF acted not as a tourist deterrent, but as tourist attractant. Results from our preferred specifications indicate that during each peak-tourism month of July and August following its construction, the BIWF caused a seven-night increase in the number of nights reserved, a nineteen percentage point increase in occupancy rates, and a \$3,490 increase in revenue for AirBnb properties in Block Island relative to properties in control group cities.

While there are no other similar studies with which we can compare results, our findings align with several indications of public interest in the BIWF that are outside of the vacation rental market. The Block Island Ferry, local for-hire fishing boats, and helicopter charters have all capitalized on the BIWF by adding new tours around the wind farm. Because its underwater structures act as fish aggregators, the BIWF has created new fishing opportunities (ten Brink and Dalton 2018) and thus drawn praise from the recreational fishing community (Monti 2018, 2017). One for-hire fishing boat owner was pleasantly surprised about the impacts of the BIWF, saying that “the business level picked up more than [expected]” and that it “continues to grow” (Maritime Executive 2018). Representatives from other sectors of the tourism industry in Block Island expressed similar sentiments about the BIWF during recent focus group interviews (Smith et al. 2018). Another potential indicator of public interest is that information about the BIWF is emphasized on the Block Island Times website. Thus, taken within the broader context, our results are plausible reflections of wider interest in and economic gains from the BIWF.

Another factor that may be driving our results is the “warm glow” effect of OSWF development. Evidenced in a few recent studies, this effect is unrelated to the visibility or ecological impacts of OSWFs; rather, it derives from the positive feelings some may experience when supporting a renewable energy source. Parsons and Firestone (2018) find that the rationale behind 52% of respondents who indicated that a wind farm would improve their beach experience was knowing something good was being done for the environment; only 11% of these respondents cited as their rationale the aesthetic appeal of the turbines in the horizon. Additionally, the authors find little variation in the percentage of respondents who would switch from their current beach to an alternative one with an OSWF with respect to the distance of the OSFW from the beach, which is also consistent with the “warm glow” effect. Firestone et al. (2018) provide additional evidence of the “warm glow” effect after studying determinants of support for the BIWF, noting that “the description of the [Block Island] wind turbines that resonated most universally among both Block Island and coastal Rhode Island supporters [who had seen the turbines] was ‘symbolic of progress towards clean energy’”. Hence, it could be that our results are driven partly by increased visitation from individuals who like the feeling of supporting a clean energy source, but might not necessarily care about seeing the BIWF.

Our study is novel and a strong application of revealed preference data, however several limitations exist. Because the AirBnb rental property data used to proxy for tourism represents one segment of the tourist population, we are unable to capture behavioral responses from other important segments, like single-day visitors and those who book short-term lodging accommodations through other rental platforms. Research

using more comprehensive data is needed to explore whether preferences for the BIWF revealed in this study are representative of the tourist population at large. The data is also confined to a relatively short, roughly one-year post-construction time horizon. Updating our analysis using additional years of data would allow us to ascertain whether BIWF-related tourism impacts are transient or persistent.

The overarching objective of this research is to understand the effects of offshore wind energy development on tourism. However, because we focus on the BIWF, there are several factors that limit the external validity of our results, in the sense that our estimates may not apply to future OSWFs. First, our estimates come from the United States' very first OSWF, which may elicit more excitement or interest than subsequent developments. Second, future OSWFs in the U.S. will differ from the BIWF in terms of the number of turbines, installed capacity, proximity to and visibility from the shoreline and beach, and the physical and socioeconomic characteristics of the surrounding community. Thus, we urge caution when trying to generalize our results to future OSWFs. However, our results provide an important data point to the ongoing debate surrounding tourism impacts of OSWFs and provide a baseline for future work.

APPENDICES

The appendix for this dissertation provides supplemental figures, statistics, and results to the main text.

Appendix for Chapter 1

Table A1 shows estimation results from a panel rank-ordered mixed logit model in which observations are weighted by the inverse of predicted response propensity. Estimated utility parameters from this model are consistent with those estimated by our preferred specification.

Figure A1 displays an example of the 2016 recreational striped bass angler mail survey.

Appendix for Chapter 2

Table A2 shows data and their sources used to calculate actual fishery outcomes displayed in Table 15.

Figure A2 displays raw 2015 VAL data collected by the Connecticut Volunteer Angler Survey Program and the Massachusetts Sportfish Data Collection Team (SADC) Program.

Figure A3 displays length-age conversions based combined data from three separate 2015 striped bass age-length keys provided each by the Massachusetts' Division of Marine Fisheries, New York's Department of Environmental Conservation Division of Marine Resources, and Rhode Island's Division of Fish and Wildlife.

Appendix for Chapter 3

Figure A4 plot the number of new rental market properties in each month as a proportion of the number of properties that existed in October 2014 and includes all properties in the raw dataset. For reference, 7, 54, 21, and 7 AirBnb properties operated in Block Island, Nantucket, Narragansett, and Westerly during October 2014, respectively. The figure shows that rental market entrance is generally highest during the summer months. Trends are similar across cities until March 2017, when an influx of new AirBnb properties enter the Block Island market.

Figure A5 includes a map depicting the approximate location of sample AirBnb properties in Block Island (left) and a visibility map of the waters surrounding Block Island, adapted from Griffin et al. (2015) (right). Many sample properties are clustered around the main town and beach area on the eastern side of the island, from which the BIWF is not visible. The BIWF could be visible from a few, but likely no more six, properties located on the southern part of the island.

Figure A6 shows raw outcome means between treated and control groups using the sample of properties included in Table 18 in the main paper. The vertical red line marks the treatment date, September 2016.

Table A4 presents results from estimating Equation (2). The table has three columns, which each add covariates to the model. The first column includes property fixed effects and property variables that change over time, Column (2) adds year-month fixed effects, and Column (3) adds city-specific time trends. The table also has five panels, one for each of our dependent variables.

The results in Panel A of Table A4 imply that the BIWF lead to a statistically significant increase in *Available nights* for Block Island properties relative to control group properties. Estimates of this increase range from about two nights per month in Columns (1) and (2), to about five nights per month in Column (3). Within R-squared increases only slightly moving from Column (1) to Columns (2) and (3), which suggests that there is little within variation in *Available nights* over time once property fixed effects are included in the model. Columns (1) and (3) of Panel B show a significant treatment effect on *Reservation nights*. These coefficients indicate a treatment-induced monthly increase of 1.6 booked nights for the average Block Island property compared to the average control group property. Panel C indicates small and statistically insignificant effects of treatment on *Occupancy rate*. Panel E shows a positive effect of treatment on *Revenue*, though only statistically significant in the first column. Overall, these results suggest a weak increase in tourism due to BIWF.

Panel D of Table A4 presents results from models with *Average booked rate* as the dependent variable. The estimated coefficients are positive, but imprecisely estimated. This is consistent with the overall story of these results indicating a weak increase in tourism activity. However, there is another pattern worth discussing. Like the results for *Available nights* but unlike those for the other dependent variables, within R-squared increases only slightly with the inclusion of year-month fixed effects, which implies that prices do not change much over time, but reservations, occupancy rates, and revenues do. This indicates that property fixed effects explain a huge amount of variation in prices and there is little within variation remaining. These findings strongly support our use of multiple dependent variables to evaluate the effect of the BIWF on the vacation rental

market.

Results in Panels A, B, C, and E Table A4 inform our selection of control variables used in the specifications presented in the main paper. In these panels, the Column (3) specification is preferred because it yields the highest within R-squared and lowest AIC among the three specifications. We therefore include year-month fixed effects and city-specific time trends in model specifications presented in the main paper.

Table A5 shows each month's contribution to total 2015, 2016, and 2017 *Revenue* and *Reservation* using the sample of properties from Table 2 in the main paper. This table reveals rental market activity to be highly concentrated in the summer months of June, July, August, and September; within these months, rental market activity is highest during July and August. Findings in this table motivate our exploration of seasonal heterogeneity in the effect of treatment.

Table A6 shows the percent of sample properties in each city whose amenities changed over time. The table reveals that a very small proportion of properties in Nantucket and Narragansett varied these amenities over the course of the study period. No properties in Block Island varied these amenities over the course of the study period.

Table A7 shows estimation results from DD models akin to Equations (3) and (4) in the main paper but that use the time-varying property amenity variables as dependent variables. Construction of the BIWF significantly affected one of the five property amenity variables, minimum stay, but the magnitude of the estimated coefficient is very small. Additionally, there is such little variation in these amenities over the study period to be explained by the model that these relationships seem no more than spurious correlations. Thus, we take all five property amenities as exogenous to treatment.

Table A8 serves as a robustness check to Table 19 in the main paper by specifying an alternative treatment date, December 2016, that corresponds to when the BIWF was connected to the electrical grid. The results in Table A8 are broadly consistent with Table 19 results. In Panel A, estimates of the summer and off-summer treatment effect on *Occupancy rate* are statistically significant, unlike in Table 19 where only the latter effect is significant. The estimated summer treatment effect on *Revenue* in Panel A of Table A8 is smaller compared to Table 19.

In Panel B of Table A8, treatment effects during July and Augusts on *Reservation nights* and *Revenue* are attenuated compared to Table 19 estimates. Although the off-summer treatment effect on *Occupancy rate* is statistically significant, unlike in Panel B of Table 19, the effect does not translate to significant decreases in revenue, as implied by estimated coefficient in the *Revenue* model. Finally, Panel C of Table A8 shows exaggerated treatment effect during July and August on *Reservation nights*, *Occupancy rate*, and *Revenue* compared to the estimates in Panel C of Table 19. Ultimately, the findings in Table A8 support our primary set of results in the main paper. They confirm that the BIWF lead to significant increases in reservation nights, occupancy rates, and monthly revenue during the peak-tourism months on July and August for Block Island properties compared to control group properties.

Table A9 displays results from DD models that exclude August 2016 observations from the sample. These specifications are a robustness check on our main results given uncertainty about whether August 2016 is a treated or untreated month. We define the treatment period to begin when construction of the BIWF was complete, which occurred on August 18, 2016; in the main paper, however, we specify the first treated month as

September 2016 because our analysis uses monthly data. Overall, the results in Table A9 are consistent with those displayed in Table 19. When significant, estimates representing the effect of treatment during the peak-tourism months of July and August in Panel B and C are approximate in magnitude to those presented in the main paper.

North Atlantic Recreational Striped Bass Fishing Survey

Questions?...Call us at 800-229-5220 and select option 3,
or email Andrew.Carr-Harris@noaa.gov

To participate online, go to:
www.strippedbass.fishingsurvey.com

Figure A1. 2016 recreational striped bass angler mail survey.

Information Page

Atlantic striped bass

- Commonly called stripers or rockfish
- Spawning occurs during spring and early summer in rivers and coastal estuaries; many adults migrate northward after spawning and return south for the winter
- Typical recreational catch is 7-14 lbs., 21"-28" total length; largest caught was 81.9 lbs., 54" total length
- Trophy-sized catch generally considered to be at least 36" total length
- Size regulations vary by region/season*
- Possession limits ranged from 1 to 3 fish during the 2016 fishing season**

Recreational species that were commonly caught with striped bass in 2015**

Bluefish

Summer flounder
(fluke)

Black sea bass

White perch

Scup

Channel catfish

Pollock

Tautog

* Download **FISH RULES** (www.fishrulesapp.com) on your smartphone to view current regulations for all your favorite saltwater species before fishing or while on the water.

** In coastal states from Maine to Virginia.

Figure A1 (continued). 2016 recreational striped bass angler mail survey.

A. Your Recreational Saltwater Fishing Activities

The questions in this survey are about **your** recreational fishing activities and preferences. Except when asked, please do not include information about other household members or others who fish with you. **Saltwater** is defined as the open ocean or any portion of a bay, sound, or river that is saltwater or brackish water.

A1 How would you best describe yourself when it comes to recreational fishing for **saltwater** species?

- ☐ Beginner
- ☐ Intermediate
- ☐ Advanced
- ☐ Professional (licensed charter, guide, or head boat operator)

A2 Have you gone recreational fishing for striped bass in the **past three years**?

- ☐ Yes
- ☐ No

A3 Have you gone recreational fishing for **any** saltwater species in the **past 12 months**?

- ☐ Yes
- ☐ No → **SKIP to A7 on the next page**

A4 How many days did you go recreational fishing for saltwater species in the **past 12 months**? (Count partial days as full days, and only include days spent fishing in state or federal waters.)

▶ # days:

A5 On how many of your days from question **A4** did you target striped bass?

▶ # days:

A6 Which other saltwater species have you targeted in the **past 12 months**? (Select all that apply.)

☐ Summer flounder	☐ Atlantic croaker
☐ Winter flounder	☐ Cod / Haddock
☐ Scup	☐ Spot
☐ Black sea bass	☐ Weakfish
☐ Tautog	☐ Bluefin tuna
☐ Bluefish	☐ Little tunny
☐ Other (specify): _____	

Figure A1 (continued). 2016 recreational striped bass angler mail survey.

The rest of the questions in this section only pertain to your recreational **striped bass** fishing experiences and opinions.

A7 For how many years have you been recreational fishing for striped bass?

► # years:

A8 In what area do you go recreational fishing for striped bass **most often**? (Choose only **one**.)

- ☐ Maine
- ☐ New Hampshire
- ☐ Massachusetts
- ☐ Rhode Island
- ☐ Connecticut
- ☐ New York - Long Island Sound / Atlantic Ocean / Hudson River south of GW Bridge
- ☐ New York - Hudson River north of GW Bridge
- ☐ New Jersey
- ☐ Delaware
- ☐ Maryland - Chesapeake Bay and its tidal tributaries
- ☐ Maryland - Susquehanna Flats / Lower Susquehanna River / Northeast River
- ☐ Maryland - Atlantic Ocean / coastal bays and their tidal tributaries
- ☐ Virginia - Atlantic Ocean
- ☐ Virginia - Chesapeake Bay / Potomac River and its tidal tributaries
- ☐ North Carolina - Atlantic Ocean
- ☐ North Carolina - Joint or inland waters

A9 During which month(s) do you go recreational fishing for striped bass? (Select all that apply.)

- ☐ January
- ☐ February
- ☐ March
- ☐ April
- ☐ May
- ☐ June
- ☐ July
- ☐ August
- ☐ September
- ☐ October
- ☐ November
- ☐ December

Figure A1 (continued). 2016 recreational striped bass angler mail survey.

A10 Please indicate your **primary** striped bass fishing mode. If you fish for striped bass from another mode more than ¼ of the time, please indicate your **secondary** fishing mode(s).

Striped bass fishing mode:	Shore including piers, jetties, surf, wade, etc.	Kayak or non-motorized boat	Motorized boat owned or rented by you, a friend, or a relative	Charter or guide boat fee paid for the entire boat and captain's services; usually 1-6 anglers per boat	Party or head boat passenger fee covers a seat on the boat; usually 25 or more anglers per boat
Primary (Choose only one.)	☐	☐	☐	☐	☐
Secondary (Select all that apply.)	☐	☐	☐	☐	☐

A11 Do you **own** a motorized boat that you use to go recreational fishing for striped bass?

- ☐ Yes
☐ No → **SKIP to A13**

A12 From where do you launch your motorized boat **most often**?

- ☐ Public boat landing/ramp
☐ Mooring ball
☐ Private dock
☐ Marina slip

A13 How frequently do you use each the following types of gear when you go recreational fishing for striped bass? (Choose one response for each gear.)

Striped Bass Fishing Gear		Never	Rarely	Sometimes	Often	Always
Plug or lure with...	...a single J-hook?	☐	☐	☐	☐	☐
	...multiple J-hooks?	☐	☐	☐	☐	☐
	...a single treble hook?	☐	☐	☐	☐	☐
	...multiple treble hooks?	☐	☐	☐	☐	☐
	...a treble hook <i>and</i> a J or circle hook?	☐	☐	☐	☐	☐
Bait with...	...circle hook(s)?	☐	☐	☐	☐	☐
	...J-hook(s)?	☐	☐	☐	☐	☐
	...treble hook(s)?	☐	☐	☐	☐	☐
	Fly?	☐	☐	☐	☐	☐

Figure A1 (continued). 2016 recreational striped bass angler mail survey.

A14 Have you ever caught a striped bass that was longer than 36"?

- ☐ Yes
☐ No

A15 If you caught and were legally allowed to keep the striped bass described in each scenario below, how many would you actually keep? (Choose one response for each scenario.)

Potential Keep Scenario	I would keep NONE of these fish	I would keep ONE of these fish	I would keep BOTH of these fish
2 striped bass, each between 20" and 26" in total length.	☐	☐	☐
2 striped bass, each between 27" and 36" in total length.	☐	☐	☐
2 striped bass, each between 37" and 43" in total length.	☐	☐	☐

A16 Please indicate your level of agreement with each of the following statements about recreational striped bass fishing.

Statement	Strongly Disagree	Disagree	Neutral	Agree	Strongly Agree
I'd rather catch (not necessarily keep) one 38" striped bass than two 29" striped bass.	☐	☐	☐	☐	☐
I'd rather catch (not necessarily keep) two 29" striped bass than four 22" striped bass.	☐	☐	☐	☐	☐
I'd still fish for striped bass even if I couldn't legally keep any.	☐	☐	☐	☐	☐

A17 How likely or unlikely are you to go recreational striped bass fishing during the **next 12 months**?

- ☐ Certain to go fishing
☐ Very likely
☐ Somewhat likely
☐ Very unlikely
☐ Definitely will not go fishing

Figure A1 (continued). 2016 recreational striped bass angler mail survey.

B. Your Striped Bass Fishing Trip Preferences

In this section we want to know about your preferences for different types of striped bass fishing trips. Scenarios B1 – B4 will present you with three options (A, B, and C).

Option A and Option B: Single-day striped bass fishing trips with different regulations, catch, and trip costs.

Option C: Not going striped bass fishing.

For each scenario, compare the features of the three options and then answer the questions below. Do not compare the options on one page to the options on any other pages.

B1

FEATURES	Option A	Option B	Option C
Striped bass legal daily limit The number and size of striped bass that you are legally allowed to keep.	2 between 20" and 28" + 1 between 28" and 36"	2 between 20" and 28" + 1 equal to or longer than 28"	
Number of striped bass you catch (length per fish) Some of these fish must be released if they are not within the legal daily limit.	4 (22") <i>You could keep two of these fish</i> 1 (29") <i>You could keep this fish</i> 1 (38") <i>This fish must be released</i>	2 (22") <i>You could keep both of these fish</i>	Do not go striped bass fishing
Number of other legal-sized fish you catch Any species of fish you might catch while fishing for striped bass. All of these fish could be legally kept.	2		
Trip cost All fishing-related, transportation, and other expenses including bait, tackle, fuel, food, and beverages. This cost would not cover anyone else who may fish with you.	\$85	\$45	

1 If you were presented with these three options, which one would you choose? (Choose only one option.)

Option A	Option B	Option C
☐	☐	☐

2 If your choice was the **ONLY** striped bass fishing trip available to you during the next 12 months, how many trips would you take? (Enter "0" if you wouldn't take any.)

► # trips during the next 12 months:

Skip to 3

3 What would be your second choice? (Choose one option other than your first choice.)

Option A	Option B	Option C
☐	☐	☐

Figure A1 (continued). 2016 recreational striped bass angler mail survey.

B3

FEATURES	Option A	Option B	Option C
<u>Striped bass legal daily limit</u> The number and size of striped bass that you are legally allowed to keep.	2 equal to or longer than 28"	1 between 20" and 30" + 1 between 30" and 36"	
<u>Number of striped bass you catch (length per fish)</u> Some of these fish must be released if they are not within the legal daily limit.	4 (22") <i>These fish must be released</i> 2 (38") <i>You could keep both of these fish</i>	1 (29") <i>You could keep this fish</i>	Do not go striped bass fishing
<u>Number of other legal-sized fish you catch</u> Any species of fish you might catch while fishing for striped bass. All of these fish could be legally kept.	2	4	
<u>Trip cost</u> All fishing-related, transportation, and other expenses including bait, tackle, fuel, food, and beverages. This cost would not cover anyone else who may fish with you.	\$65	\$45	

1 If you were presented with these three options, which one would you choose? (Choose only one option.)

Option A	Option B	Option C
☐	☐	☐

2 If your choice was the **ONLY** striped bass fishing trip available to you during the next 12 months, how many trips would you take? (Enter "0" if you wouldn't take any.)

► # trips during the next 12 months:

Skip to 3

3 What would be your second choice? (Choose one option other than your first choice.)

Option A	Option B	Option C
☐	☐	☐

Figure A1 (continued). 2016 recreational striped bass angler mail survey.

B4

FEATURES	Option A	Option B	Option C
Striped bass legal daily limit The number and size of striped bass that you are legally allowed to keep.	2 between 20" and 30" + 1 equal to or longer than 30"	2 equal to or longer than 30"	
Number of striped bass you catch (length per fish) Some of these fish must be released if they are not within the legal daily limit.	2 (22") <i>You could keep both of these fish</i> 1 (38") <i>You could keep this fish</i>	1 (38") <i>You could keep this fish</i>	Do not go striped bass fishing
Number of other legal-sized fish you catch Any species of fish you might catch while fishing for striped bass. All of these fish could be legally kept.		2	
Trip cost All fishing-related, transportation, and other expenses including bait, tackle, fuel, food, and beverages. This cost would not cover anyone else who may fish with you.	\$85	\$65	

1 If you were presented with these three options, which one would you choose? (Choose only one option.)

Option A	Option B	Option C
☐	☐	☐

2 If your choice was the **ONLY** striped bass fishing trip available to you during the next 12 months, how many trips would you take? (Enter "0" if you wouldn't take any.)

► # trips during the next 12 months:

Skip to 3

3 What would be your second choice? (Choose one option other than your first choice.)

Option A	Option B	Option C
☐	☐	☐

If you chose "Option C: Do not go striped bass fishing" as your first choice for any of the previous four scenarios (B1 - B4), what would you be more likely to do instead?

- ☐ Something other than saltwater fishing.
- ☐ Go saltwater fishing, but target a species other than striped bass.

Figure A1 (continued). 2016 recreational striped bass angler mail survey.

C. About You and Your Household

C1 Did you own a commercial striped bass fishing license in 2015 or 2016?

- ☐ Yes
- ☐ No

C2 In what year were you born?

► Year:

C3 Are you...?

- ☐ Male
- ☐ Female

C4 What is the highest level of education you have completed?

- ☐ Less than high school graduate
- ☐ High school graduate or GED
- ☐ Some college, no degree
- ☐ Associate degree (ex. AA or AS)
- ☐ Bachelor's degree (ex. BA or BS)
- ☐ Master's degree or higher

C5 How many adults and children (under 18) are there in your household including yourself?

► # Adults: # Children:

C6 Which of the following categories best describes your household's total annual income before taxes in 2015?

- ☐ Less than \$20,000
- ☐ \$20,000 to \$39,999
- ☐ \$40,000 to \$59,999
- ☐ \$60,000 to \$79,999
- ☐ \$80,000 to \$99,999
- ☐ \$100,000 to \$149,999
- ☐ \$150,000 to \$199,999
- ☐ \$200,000 or more

Thank you for completing the survey!

Your answers will help us improve the management of this fishery. Please return your completed survey in the postage paid envelope as soon as possible. If you have any questions or comments call us at 800-229-5220 and select option 3, or email Andrew.Carr-Harris@noaa.gov.

Figure A1 (continued). 2016 recreational striped bass angler mail survey.

Figure A2. Raw VAL data used to generate 2015 striped bass catch-at-length distribution.

Figure A3. Striped bass length-age conversions based on combined 2015 age-length keys from NY, MA, and RI by length.

Figure A4. New properties in proportion to the number of properties in October 2014.

Figure A5. Left: Approximate location of Block Island AirBnb properties, plotted in red, included in main estimation sample and the BIWF turbines, plotted in white. Right: favorable visibility areas over the 20-year lifetime of the BIWF, adapted from Griffin et al. (2015). For all map locations (cells), viewer days reflects the sum across all viewers of the number of days that each viewer, resident or visitor, can see a turbine at that location. Viewing is weighed more heavily for residents than for visitors.

Figure A6. Mean outcome trends by treatment group.

Table A1. Utility parameter estimates from weighted panel rank-ordered mixed logit model.

Variable	Mean Parameters	Standard Deviations
Small keep	0.233*** 0.056	0.966*** 0.061
Medium keep	0.278*** 0.055	1.168*** 0.058
Trophy keep	0.656*** 0.075	1.369*** 0.091
Small release	0.084*** 0.021	0.417*** 0.022
Medium release	0.127** 0.054	0.496*** 0.052
Trophy release	0.244*** 0.031	0.710*** 0.039
Other catch	0.157*** 0.015	
Cost	-0.017*** 0.002	
Opt-out	-3.086*** 0.115	
Min. 28"	-0.121 0.081	
Min. 30"	-0.672*** 0.101	
Num. Observations	1,684	
Num. individuals	447	
Log likelihood	-2475	
McFadden Pseudo R ²	0.180	
AIC	4983	

Notes: Observations from each individual are weighted by the inverse of their predicted response propensity. 500 Halton draws used to maximize the simulated log-likelihood. *, **, and *** represent significance at the 10%, 5%, and 1% level of significance, respectively.

Table A2. Data and derivation of removals-at-age and mature female removals-at-age numbers and weights.

Age	2015 proportions-at-age		Weights-at-age (lbs.) ¹	Female sex proportions-at-age ¹	Proportions mature-at-age for females ²	2015 removals-at-age		2015 mature female removals-at-age	
	Harvest ¹	Releases ¹				Numbers	Pounds	Numbers	Pounds
1	0.000	0.000	0.33	0.53	0	54	18	0	0
2	0.001	0.013	0.64	0.56	0	9,931	6,350	0	0
3	0.004	0.086	2.03	0.56	0	63,747	129,295	0	0
4	0.027	0.393	3.51	0.52	0.09	297,105	1,041,458	13,905	48,740
5	0.136	0.223	5.51	0.57	0.32	253,131	1,395,145	46,171	254,474
6	0.164	0.078	8.27	0.65	0.45	169,145	1,398,380	49,475	409,026
7	0.133	0.039	10.05	0.73	0.84	119,766	1,204,018	73,441	738,304
8	0.135	0.029	12.54	0.81	0.89	114,056	1,430,747	82,223	1,031,425
9	0.130	0.035	15.37	0.88	1	114,873	1,765,168	101,088	1,553,347
10	0.110	0.035	16.95	0.92	1	101,555	1,721,718	93,431	1,583,981
11	0.079	0.030	19.73	0.95	1	76,126	1,502,063	72,319	1,426,960
12	0.029	0.021	23.24	0.97	1	34,750	807,487	33,708	783,262
13+	0.052	0.017	31.13	1	1	48,534	1,510,831	48,534	1,510,831
Total						1,402,774	13,912,676	614,294	9,340,351

Notes: 2015 total striped bass recreational harvest and release numbers in MA, RI, and CT was 693,135 and 7,884,877, respectively. Removals-at-age numbers calculated as: (total harvest numbers × harvest proportions-at-age) + (total release numbers × release proportions-at-age × 0.09). Removals-at-age weight calculated as: removals-at-age numbers × weights-at-age. Mature female removals-at-age numbers calculated as: removals-at-age numbers × female sex proportions-at-age × proportions mature-at-age for females. Mature female removals-at-age weight calculated as: mature female removals-at-age numbers × weights-at-age.

¹ Sourced from 2016 striped bass stock assessment update.

² Sourced from preliminary 2018 stock assessment.

Table A3. Simulated fishery outcomes under alternative 2015 policies.

Policy	Regulation	CV (\$M)	Δ Num. of Expected Trips (%)	Δ Total Removals (%)		Δ Female SSB Removals (%)	
				Num. fish	Weight	Num. fish	Weight
A1	1 fish $\geq 20''$	16.74	1.02	37.62	-4.50	-21.95	-35.59
B1	1 fish $\geq 24''$	11.94	0.75	24.30	0.67	-9.28	-20.49
C1	1 fish 20-28''	6.49	0.36	29.95	-26.5	-49.53	-68.61
D1	1 fish 20-36''	12.89	0.77	35.44	-14.04	-32.20	-51.99
E1	1 fish 28-36''	-8.84	-0.58	-7.97	-21.06	-21.15	-34.01
F1	1 fish 24-32''	12.89	0.77	35.44	-14.04	-32.20	-51.99
G1	1 fish 24-40''	10.30	0.64	23.27	-4.61	-13.79	-29.74
H1	1 fish 28-44''	-0.76	-0.05	-0.67	-2.82	-1.95	-4.74
A2	2 fish $\geq 20''$	51.16	3.29	114.06	50.03	23.24	1.83
B2	2 fish 20-28''	32.27	2.06	94.12	6.91	-28.92	-59.09
C2	2 fish 20-36''	44.31	2.84	108.35	31.66	3.82	-28.44
D2	1 fish 20-28'' & 1 fish $> 28''$	40.15	2.55	84.05	45.97	29.83	17.82
E2	1 fish 20-28'' & 1 fish $> 28-36''$	30.41	1.91	72.94	21.12	5.22	-20.70
F2	2 fish 20-28''; only 1 $> 28''$	49.89	3.21	112.74	46.32	18.64	-3.70
G2	2 fish 20-28''; only 1 $> 28-36''$	43.69	2.80	107.67	30.10	1.66	-30.51
H2	2 fish $\geq 24''$	37.48	2.42	79.87	50.82	38.34	22.09
I2	2 fish 24-32''	20.40	1.30	62.21	9.30	-9.88	-42.09
J2	2 fish 24-40''	34.70	2.24	77.33	41.35	30.30	6.23
K2	1 fish 24-32'' & 1 fish $> 32''$	27.03	1.70	52.76	38.98	31.49	26.37
L2	1 fish 24-32'' & 1 fish $> 32-40''$	23.44	1.47	48.31	27.05	21.69	7.64
M2	2 fish 24-32''; only 1 $> 32''$	36.57	2.36	78.91	47.94	34.91	17.38
N2	2 fish 24-32''; only 1 $> 32-40''$	34.03	2.20	76.56	39.30	27.67	2.97
O2	2 fish $\geq 28''$	12.67	0.79	26.14	34.27	37.60	39.61
P2	2 fish 28-36''	-0.40	-0.05	11.21	-0.28	2.43	-13.96
Q2	2 fish 28-44''	11.46	0.71	24.72	29.59	34.15	32.06
R2	1 fish 28-36'' & 1 fish $> 36''$	6.08	0.36	11.30	18.78	19.87	25.32
S2	1 fish, 28-36'' & 1 fish $> 36-44''$	4.84	0.28	9.75	13.98	16.33	17.69
T2	2 fish 28-36''; only 1 $> 36''$	12.04	0.75	25.22	32.11	35.40	36.24
U2	2 fish 28-36''; only 1 $> 36-44''$	10.97	0.68	24.05	27.95	32.41	29.44

Notes: Actual 2015 policy was one fish $\geq 28''$ and is used as the baseline policy.

Table A4. The effect of BIWF on the vacation rental market.

<i>Panel A: Dependent variable is Available nights</i>			
	(1)	(2)	(3)
BI×Post_construction	1.866** (0.898)	2.157*** (0.831)	5.273*** (1.436)
Within R ²	0.012	0.073	0.075
AIC	68,179	67,607	67,598
<i>Panel B: Dependent variable is Reservation nights</i>			
	(1)	(2)	(3)
BI×Post_construction	1.601** (0.649)	0.896 (0.689)	1.552* (0.916)
Within R ²	0.006	0.297	0.298
AIC	67,545	64,140	64,131
<i>Panel C: Dependent variable is Occupancy rate</i>			
	(1)	(2)	(3)
BI×Post_construction	0.020 (0.027)	-0.010 (0.025)	-0.023 (0.042)
Within R ²	0.005	0.311	0.312
AIC	2,785	-822	-825
<i>Panel D: Dependent variable is Average booked rate</i>			
	(1)	(2)	(3)
BI×Post_construction	49.731 (49.718)	47.639 (50.354)	14.327 (39.455)
Within R ²	0.003	0.070	0.070
AIC	54,482	54,253	54,255
<i>Panel E: Dependent variable is Revenue</i>			
	(1)	(2)	(3)
BI×Post_construction	745.815** (318.160)	439.569 (330.980)	388.066 (578.829)
Within R ²	0.002	0.205	0.209
AIC	199,304	197,105	197,060
Year-month fixed-effects	N	Y	Y
City time trends	N	N	Y

Notes: 'BI' stand for Block Island, 'Post_construction' is an indicator variable for the post-construction (treatment) period. In panels A, B, C, and E, the number of observations is 10,019. In panel D, the number of observations is 4,385. Included in all regressions as controls are minimum stay (number of nights), maximum number of guests, security deposit (\$), extra people fee (\$), and cleaning fee (\$); however, regressions for Average booked rate and Revenue exclude cleaning fees, as these fees are incorporated in the outcome variable. All regressions include property fixed-effects and a constant term. Standard errors are shown below in parenthesis and clustered at the property level. *, **, and *** represent significance at the 10%, 5%, and 1% level of significance, respectively.

Table A5. Proportion of total 2015, 2016, and 2017 revenue and reservation nights, by month.

Month	Revenue (%)	Reservation nights (%)
January	0.4	0.8
February	0.4	0.8
March	1.0	1.2
April	3.2	3.2
May	7.4	7.6
June	11.5	12.3
July	23.6	22.2
August	25.1	23.8
September	15.4	14.6
October	5.5	6.6
November	2.9	3.1
December	3.6	3.9

Notes: Statistics reflect the sample of properties in Table 18 of the main paper.

Table A6. Percent of properties in estimation sample with changing property amenities, by city.

Property Amenity	Block Island	Nantucket	Narragansett	Westerly
Maximum number of guests	0	0	0.7%	0
Cleaning fee	0	1.8%	0	0
Minimum stay	0	4.5%	0	0
Security deposit	0	1.8%	0	0
Extra people fee	0	0.3%	0.7%	0

Notes: Statistics reflect the sample of properties in Table 18 of the main paper.

Table A7. The effect of BIWF on rental property amenities.

Panel A: Summer and off-summer treatment effects

	Max. Guests	Security Deposit	Extra People Fee	Minimum Stay	Cleaning Fee
BI×Post_construction	0.002	0.489	0.010	-0.014*	-0.005
×Off_summer	(0.002)	(0.468)	(0.034)	(0.007)	(0.299)
BI×Post_construction	0.005	0.666	0.071	-0.007	0.233
×Summer	(0.005)	(0.604)	(0.072)	(0.005)	(0.202)
Observations	10,019	10,019	10,019	10,019	10,019
R-squared	1.000	0.999	0.999	0.996	0.995

Panel B: Off-summer, peak-summer, and off-peak summer treatment effects

	Max. Guests	Security Deposit	Extra People Fee	Minimum Stay	Cleaning Fee
BI×Post_construction	0.002	0.630	0.013	-0.016**	-0.008
×Off_summer	(0.002)	(0.486)	(0.036)	(0.007)	(0.239)
BI×Post_construction	0.005	1.216	0.040	-0.019**	0.011
×July_Aug	(0.006)	(1.461)	(0.093)	(0.009)	(0.255)
BI×Post_construction	0.005	0.615	0.113	-0.003	0.364
×June_Sep	(0.005)	(0.430)	(0.085)	(0.005)	(0.284)
Observations	10,019	10,019	10,019	10,019	10,019
R-squared	1.000	0.999	0.999	0.996	0.995

Panel C: Restricted sample, June through September

	Max. Guests	Security Deposit	Extra People Fee	Minimum Stay	Cleaning Fee
BI×Post_construction	0.000	0.701	0.035	-0.012	0.403
×July_Aug	(.)	(1.146)	(0.038)	(0.011)	(0.535)
BI×Post_construction	0.000	0.105	0.032	0.003	0.766
×June_Sep	(.)	(0.325)	(0.035)	(0.005)	(0.776)
Observations	3,923	3,923	3,923	3,923	3,923
R-squared	1.000	0.999	0.999	0.997	0.996

Notes: 'BI' stand for Block Island, 'Post_construction' is an indicator variable for the post-construction (treatment) period, 'Summer' is an indicator variable for the months of June, July, August, and September, 'Off_summer' is an indicator variable for the months of October through May, 'July_Aug' is an indicator variable for the months of July and August, and 'June_Sep' is an indicator variable for the months of June and September. All regressions include property fixed-effects, year-month fixed effects, city time trends, and a constant term. Standard errors are shown below in parenthesis and clustered at the property level. *, **, and *** represent significance at the 10%, 5%, and 1% level of significance, respectively.

Table A8. The effect of BIWF on the vacation rental market, treatment date defined by grid connection (December 2016).

Panel A: Summer and off-summer treatment effects

	Available nights	Reservation nights	Occupancy rate	Average booked rate	Revenue
BI×Post_connection	1.823	-0.924	-0.064**	76.715	-505.499
×Off_summer	(1.198)	(0.855)	(0.032)	(70.753)	(368.677)
BI×Post_connection	5.370***	3.781**	0.101*	37.371	1597.148
×Summer	(1.490)	(1.548)	(0.061)	(68.482)	(1098.376)
Observations	10,516	10,516	10,516	4,579	10,516
R-squared	0.251	0.482	0.512	0.932	0.411

Panel B: Off-summer, peak-summer, and off-peak summer treatment effects

	Available nights	Reservation nights	Occupancy rate	Average booked rate	Revenue
BI×Post_connection	1.858	-0.899	-0.064**	74.875	-492.025
×Off_summer	(1.188)	(0.871)	(0.032)	(67.658)	(374.737)
BI×Post_connection	8.926***	6.362***	0.150*	25.262	2963.787*
×July_Aug	(1.927)	(2.000)	(0.079)	(87.378)	(1531.455)
BI×Post_connection	2.669	1.650	0.054	49.689	428.814
×June_Sep	(1.706)	(1.470)	(0.058)	(53.072)	(910.412)
Observations	10,516	10,516	10,516	4,579	10,516
R-squared	0.254	0.483	0.512	0.933	0.412

Panel C: Restricted sample, June through September

	Available nights	Reservation nights	Occupancy rate	Average booked rate	Revenue
BI×Post_connection	10.057***	8.423***	0.200*	65.910	4733.241**
×July_Aug	(3.334)	(2.865)	(0.115)	(115.804)	(1972.163)
BI×Post_connection	4.638	2.819	0.052	82.776	1342.639
×June_Sep	(3.240)	(2.589)	(0.099)	(77.701)	(1477.465)
Observations	3,643	3,643	3,643	3,643	3,643
R-squared	0.338	0.495	0.526	0.944	0.488

Notes: 'BI' stand for Block Island, 'Post_connection' is an indicator variable for the post-electrical grid connection (treatment) period, 'Summer' is an indicator variable for the months of June, July, August, and September, 'Off_summer' is an indicator variable for the months of October through May, 'July_Aug' is an indicator variable for the months of July and August, and 'June_Sep' is an indicator variable for the months of June and September. Included in all regressions as controls are minimum stay (number of nights), maximum number of guests, security deposit (\$), extra people fee (\$), and cleaning fee (\$); however, regressions for Average booked rate and Revenue exclude cleaning fees, as these fees are incorporated in the outcome variable. All regressions include property fixed-effects, year-month fixed effects, city time trends, and a constant term. Standard errors are shown below in parenthesis and clustered at the property level. *, **, and *** represent significance at the 10%, 5%, and 1% level of significance, respectively.

Table A9. The effect of BIWF on the vacation rental market; sample excludes August 2016 observations.

Panel A: Summer and off-summer treatment effects

	Available nights	Reservation nights	Occupancy rate	Average booked rate	Revenue
BI×Post_construction	1.877	-0.304	-0.061	32.170	-170.384
×Off_summer	(1.556)	(0.793)	(0.038)	(34.983)	(413.404)
BI×Post_construction	4.187**	3.328***	0.071	-19.643	1262.520
×Summer	(1.679)	(1.235)	(0.049)	(40.703)	(863.465)
Observations	9,185	9,185	9,185	3,891	9,185
R-squared	0.256	0.490	0.515	0.931	0.432

Panel B: Off-summer, peak-summer, and off-peak summer treatment effects

	Available nights	Reservation nights	Occupancy rate	Average booked rate	Revenue
BI×Post_construction	1.643	-0.153	-0.045	19.788	-7.080
×Off_summer	(1.597)	(0.788)	(0.037)	(32.190)	(366.229)
BI×Post_construction	5.956**	6.514***	0.186**	-47.543	3295.099**
×July_Aug	(2.368)	(1.926)	(0.078)	(54.532)	(1550.609)
BI×Post_construction	2.195	1.160	0.021	-21.651	45.473
×June_Sep	(1.736)	(1.275)	(0.052)	(38.939)	(805.603)
Observations	9,185	9,185	9,185	3,891	9,185
R-squared	0.257	0.491	0.516	0.931	0.432

Panel C: Restricted sample, June through September

	Available nights	Reservation nights	Occupancy rate	Average booked rate	Revenue
BI×Post_construction	8.265**	5.066**	0.099	-37.135	3145.601*
×July_Aug	(3.554)	(2.328)	(0.102)	(71.467)	(1833.962)
BI×Post_construction	4.557*	-0.750	-0.085	-31.861	-635.360
×June_Sep	(2.419)	(1.798)	(0.078)	(40.280)	(984.784)
Observations	3,891	3,891	3,891	2,224	3,891
R-squared	0.368	0.527	0.561	0.947	0.528

Notes: 'BI' stand for Block Island, 'Post_construction' is an indicator variable for the post-construction (treatment) period, 'Summer' is an indicator variable for the months of June, July, August, and September, 'Off_summer' is an indicator variable for the months of October through May, 'July_Aug' is an indicator variable for the months of July and August, and 'June_Sep' is an indicator variable for the months of June and September. Included in all regressions as controls are minimum stay (number of nights), maximum number of guests, security deposit (\$), extra people fee (\$), and cleaning fee (\$); however, regressions for Average booked rate and Revenue exclude cleaning fees, as these fees are incorporated in the outcome variable. All regressions include property fixed-effects, year-month fixed effects, city time trends, and a constant term. Standard errors are shown below in parenthesis and clustered at the property level. *, **, and *** represent significance at the 10%, 5%, and 1% level of significance, respectively.

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