The Influence of Projected Outcomes on Preferences over Alternative Regulations: Evidence from a Recreational Fishery

Zhenshan Chen, corresponding author, Postdoctoral Associate, Agricultural Economics, Mississippi State University. Lloyd-Ricks-Watson Building, Room 102, 255 Tracy Drive, Mississippi State, MS 39762. Email: zc352@msstate.edu, phone: 8604656776.

Pengfei Liu, Assistant Professor, Environmental and Natural Resource Economics, University of Rhode Island. Kingston Coastal Institute, 1 Greenhouse Road, University of Rhode Island, Kingston, RI 02881. Email: pengfei_liu@uri.edu.

Eric T. Schultz, Associate Professor, Ecology and Evolutionary Biology, University of Connecticut. 75 North Eagleville Road, Dept. of Ecology and Evolutionary Biology, U-3043, Storrs, CT 06269-3043. Email: eric.schultz@uconn.edu.

Jacob M. Kasper, Ecology and Evolutionary Biology, University of Connecticut. 75 North Eagleville Road, Dept. of Ecology and Evolutionary Biology, Storrs, CT 06269-3043. Email: jacob.kasper@uconn.edu.

Stephen K. Swallow, Professor, Agricultural and Resource Economics and Center for Environmental Sciences and Engineering, University of Connecticut. 1376 Storrs Road W.B. Young 303, Storrs, CT 06269-4021. Email: stephen.swallow@uconn.edu.

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Abstract

The expected outcomes arising from alternative policies on respondent choice have not been sufficiently accounted for in stated preference studies. We accordingly develop a framework to quantitatively assess the influence of outcome provision and illustrate with a choice experiment in a recreational fishery. The application suggests that participants are more likely to choose status quo and low-cost options when outcomes are not provided, and these conservative behaviors might reflect the higher dispersion in anglers' utility in Tautog fishing. Further investigations with latent class models suggest that the outcome provision makes a significant share of respondents change their choice pattern.

Keywords

Recreational fishery, Alternative regulation, Choice experiment, Projected outcome variables, Scale parameter, Status quo bias

JEL H41, Q22, Q57

1. Introduction

Stated preference (SP) surveys are widely used to elicit preference toward alternative regulations, including wildlife management (e.g., Cornicelli et al. 2011), fishery management (e.g., Aas, Haider, and Hunt. 2000; Cha and Melstrom 2018; Knoche and Lupi 2016; Lew and Larson 2012; Murphy et al. 2019; Oh et al. 2005), energy regulations (e.g., Sælen and Kallbekken 2011), land use policies (e.g., Johnston et al. 2003), and health care regulations (e.g., Zweifel, Telser, and Vaterlaus 2006). Unfortunately, the outcomes arising from alternative regulations are often perceived, especially by the general public, with a high degree of uncertainty. For example, in wildlife and fishery management, the outcomes of policies (e.g., changes in wildlife or fish abundance) under consideration are often unclear, owing to the absence of accurate and precise information on, and the complexity of, the underlying environmental system. The stated preference literature contains no consensus concerning whether the expected outcomes should be presented in addition to the alternative regulations. Regarding fishery regulations, some stated preference studies include expected or projected outcomes in the choice questions (e.g., Aas, Haider, and Hunt. 2000; Oh et al. 2005) while others do not (e.g., Cha and Melstrom 2018; Lew and Larson 2012; Murphy et al. 2019).

Recent studies attempt to clarify the benefits of providing information on outcomes. Some studies suggest that respondents adjust the scenario (i.e., specific option in a choice occasion) based on their subjective beliefs; if so, providing outcome information potentially overcomes this tendency to adjust (Flores and Strong 2007; Cameron et al. 2011). Another perspective is offered by Johnston et al. (2013), who suggested that the common practice of omitting some of the final ecosystem services (which are usually perceived as outcomes) might cause respondents to speculate about the effects of intermediate factors, "leading to biased welfare estimates." Johnston et al. (2013) accordingly developed a structural model to infer respondents' speculations when SP studies omit some outcome variables (i.e., final services). Investigating in an experimental setting, Meldrum et al. (2020) suggest that presenting management outcomes and associated uncertainty is important in the sense that respondents would attend to the choice tasks more closely.

We develop a systematic method to quantitatively assess the impact of outcome variables on respondents' choices among alternative regulations. To illustrate the underlying mechanism, we first provide a conceptual framework to guide the empirical analyses. Our framework builds on the random utility model and assumes that respondents' utility is driven by regulatory attributes, outcomes affected by the regulations, and respondents' endowment (or wealth). We offer a randomized experimental design in which a treatment group is provided estimates of projected outcomes, and outline two different approaches to assess the influence of outcome provision on the choice-making process. The first approach compares regulation coefficients of models including only regulatory variables in both treatment and control groups (i.e., outcome variables are omitted in the treatment group estimates), which reflects the fishery managers' aspect of the problem since outcomes are not within the set of variables that can be directly managed. The second approach imputes projected outcome variables for the control group and conducts the estimation as if the respondents knew (i.e., making very close speculations) these variables when making choices. The second approach corresponds to the angler's view, where both the regulations and the outcomes impact their utility.

We employ recreational fishery regulations for Tautog (*Tautoga onitis*) as an exemplary application. The SP survey focuses on the Long Island Sound regional component of the coastwide Tautog population, which is overfished and calls for more restrictive fishery regulations. The survey was conducted in 2019, before the fall fishing season.[1] To assess behavioral changes (i.e., fishing effort and compliance rate) under alternative scenarios, the survey asked anglers to answer choice questions offering alternative regulatory scenarios. To assist anglers' choice-making process and to investigate the influence of outcomes on anglers' choices, we also provided projected outcomes in each scenario in an experimental setting. Anglers in the treatment group are presented with choice questions that included both regulations and projected outcomes based on simulations of how the regulations would affect population dynamics[2] (Kasper et al. 2020), while anglers in the control group considered choice questions that included only the alternative regulations.

Our study provides several innovations. Firstly, our study develops an integrated approach to testing the effect of projected outcomes in stated preference studies that compare alternative regulations. A conceptual framework is built to support empirical estimates of the effect of the outcomes, and two empirical approaches to estimating the effect are explicated. Secondly, we use multidimensional outcome indicators that are based on a model of the natural system, rather than representing only uncertainty in specific outcome indicators (e.g., Wielgus et al. 2009; Meldrum et al. 2020). Also, unlike many of the previous studies that provide outcomes (i.e., specifying the outcome levels systematically within an experimental design procedure), the outcome indicators in this study are loosely tied with the regulatory attributes, providing more precise information for the choice making process (also minimizing the chance to provide unrealistic options) while avoiding multicollinearity in estimation. Finally, we allow the scale parameters (i.e., the standard deviation of the error term representing the random part of utility) to differ between control and treatment groups.

Our conceptual framework differs from that presented by Johnston et al. (2013) in two respects. Firstly, Johnston et al. (2013) investigated the effect of omitting information on final ecosystem services that affect respondents' utility, where the baseline questions present ecosystem services that are partly intermediate and generate the tested final services. These final services may not be clearly distinct from ecosystem services that generate them, so that the final service might be perceived as the reflection of other services and its additional utility may not be correctly counted when omitted in the value elicitation questions. In our paper, we investigate fishing-experience-related outcomes that are generated via a complex and prolonged ecological process influenced by the multidimensional regulatory changes presented to respondents. The survey clarifies the mechanism and timeframe of this outcome-generation-process and that the outcomes in the question do not cover all the aspects of the regulatory effects. Second, Johnston et al. (2013) focused on recovering respondents' perceptions about the outcomes and attempted thereby to infer the impacts of these perceptions. We present an experimental design for a clean test of outcome influence on respondents' utility and choice patterns.

Results from the recreational fishery application show that respondents are more likely to choose the status quo option and are more conservative in paying for alternative management in the absence of outcome information. However, when the two groups have different random utility dispersion (measured by the scale parameter), these choice pattern differences become insignificant, while the control group is dispersed in a much wider range in the utility space than the treatment group. These results are in line with the reasoning of Haab, Huang, and Whitehead (1999) in their comments on Cummings et al. (1997). An intuitive explanation is that without outcomes, anglers are showing more conservative choice patterns, which reflects the in-depth uncertainty in their utility as a function of regulatory attributes. Further investigations into the choice pattern heterogeneity with latent class models indicate that when not provided with the outcome, a larger share of respondents falls into the status quo (the SQ class comprising individuals who mostly choose SQ), while all latent classes show slightly less dislike toward the SQ option. The results are consistent with the interpretation that the provision of outcome is helpful for respondents who are unable or unwilling to figure out the outcome to make informed choices, while respondents with well-informed priors or close speculations might act as if they knew the outcome variables that could or would have been provided based on modeling from the regulations that were presented.

The paper proceeds as follows. Section II introduces the conceptual framework guiding the analysis into the influence of outcome provision. Section III presents the design of the survey application. Section IV shows the results of the analyses. Section V discusses the implications of the results. Section VI gives concluding remarks.

2. The Conceptual Framework

Assuming respondents' utility involves the regulatory attributes ($\mathbf{X}$), regulation-driven environmental outcomes ($\mathbf{O}$) that directly affect the respondents (e.g., in the fishery example, the future fishing experience), and the outside good represented by a monetary measure (W), the utility function can be represented by:[3]

$$U(\mathbf{X}, \mathbf{O}, W) = v(\mathbf{X}, \mathbf{O}, W) + e. \quad [1]$$

Equation [1] implements the familiar random utility model (RUM), where $v(\mathbf{X}, \mathbf{O}, W)$ represents the empirically measurable component of the utility and e represents the unobservable error term, which is assumed to be independently and identically distributed according to the Type-I extreme value distribution. The expected outcomes are assumed to be a function of the regulatory attributes, so that $\mathbf{O} = f(\mathbf{X})$ represents the natural, ecological production function on which anglers' activity operates under the proposed regulations.

We assume RUMs take a linear functional form, which leads to the following equation system:

$$U = \alpha_0 + \boldsymbol{\alpha}\mathbf{X} + \boldsymbol{\beta}\mathbf{O} + \gamma W + e, \quad [2a]$$

$$\mathbf{O} = f(\mathbf{X}). \quad [2b]$$

Based on equations [2a] and [2b], we have,

$$U = \alpha_0 + \boldsymbol{\alpha}\mathbf{X} + \boldsymbol{\beta} f(\mathbf{X}) + \gamma W + e, \quad [2c]$$

where α_0 represents the intercept (or alternative specific constant), $\boldsymbol{\alpha}$ is a $(1 \times I)$ vector representing the regulation coefficients, $\boldsymbol{\beta}$ is a $(1 \times J)$ vector representing the outcome coefficients. J and I denote the total number of outcome variables and regulation attributes, respectively. In contingent valuation or discrete choice experiment applications, utility is usually specified as a linear function representing the first order approximation to the underlying function. A linear approximation of equation [2c] would give

$$U = \alpha_0 + (\boldsymbol{\alpha} + \boldsymbol{\beta\theta})\mathbf{X} + \gamma W + e, \quad [2d]$$

where $\boldsymbol{\theta}$ is a $(J \times I)$ matrix representing the parameters defining the linear approximation of the outcome function (i.e., $\mathbf{O} = f(\mathbf{X}) = \boldsymbol{\theta}\mathbf{X}$). Equation [2d] represents the parsimonious linear utility model where no outcomes are directly included.

Based on a multinomial logit model (MNL), the loglikelihood function of an individual choosing the kth alternative in choice question t is:

$$L_{\mathrm{kt}} = \frac{e^{\alpha_0 + \sum_{\mathrm{I}} \alpha_{\mathrm{i}} X_{\mathrm{ki}} + \sum_{\mathrm{J}} \beta_{\mathrm{j}} O_{\mathrm{kj}}}}{\sum_{\mathrm{n}=1}^{3} e^{\alpha_0 + \sum_{\mathrm{I}} \alpha_{\mathrm{i}} X_{\mathrm{ni}} + \sum_{\mathrm{J}} \beta_{\mathrm{j}} O_{\mathrm{nj}}}}, \quad [3]$$

where n denotes the alternative choices and k denotes the option selected in each choice occasion, j indexes the projected outcome variables, and i indexes the regulation attributes. The αs and βs are the estimated utility function coefficients in equation [2a] and [2d].

To estimate the impact of omitting outcome variables, we design a randomized experiment in which two groups differ in whether outcomes are presented in choice question scenarios. Suppose the choice questions involve both regulatory attributes and outcomes variables in a general case. Corresponding to equation [3], the estimation process omitting outcome variables O would estimate the loglikelihood function:

$$L_{\mathrm{kt}} = \frac{e^{\alpha_0 + \sum_{\mathrm{I}} (\alpha_{\mathrm{i}} + \beta_{\mathrm{j}} \theta_{\mathrm{ji}}) X_{\mathrm{ki}}}}{\sum_{\mathrm{n}}^{3} e^{\alpha_0 + \sum_{\mathrm{I}} (\alpha_{\mathrm{i}} + \beta_{\mathrm{j}} \theta_{\mathrm{ji}}) X_{\mathrm{ni}}}}. \quad [4]$$

Based on equations [3], estimates with all variables (denoting regulatory coefficients with $\hat{\alpha}^{\mathrm{T}}{}_{\mathrm{i}}$ in this case) would give $\hat{\alpha}^{\mathrm{T}}{}_{\mathrm{i}} = \alpha_{\mathrm{i}}$. Based on equation [4][4], estimates without outcome variables (denoting regulatory coefficients with $\hat{\alpha}^{\mathrm{C}}{}_{\mathrm{i}}$) give $\hat{\alpha}^{\mathrm{C}}{}_{\mathrm{i}} = \alpha_{\mathrm{i}} + \beta_{\mathrm{j}}\theta_{\mathrm{ji}}$. If outcomes impact respondents' utility in a manner that is, at least partially, separate from the regulations themselves, then it is obvious that $\hat{\alpha}^{\mathrm{C}}{}_{\mathrm{i}} \neq \hat{\alpha}^{\mathrm{T}}{}_{\mathrm{i}}$.

To estimate $\hat{\alpha}^{\mathrm{T}}{}_{\mathrm{i}}$, a proper way is needed to generate the regulatory outcome variables in the survey based on available scientific data and models. Applying a projection without uncertainty, as specified in equation [2b], is problematic for two reasons[5]. First, since typical choice question designs only result in a certain number of regulatory scenarios, multicollinearity will likely cause identification challenges if the definite relation between outcomes and the regulatory attributes is applied and both groups of variables are added in the regression. Second, biological models underlying the projected outcomes usually offer a range of predictions instead of a point prediction. To simplify survey design, we use a random draw from the lower bound, the mean, and the upper bound of the biological outcome prediction[6] to generate the outcome variables that will be presented to respondents. The outcome variables are thus expressed as:

$$O_{\mathrm{j}} = \sum\{I(floor[Runiform(0,3)] = l) \cdot O_{\mathrm{l}}\}, \qquad [5]$$

where $I(\cdot)$ is the indicator function, $floor(\cdot)$ is a floor function which takes only the integer part of the value, $Runiform(\cdot)$ is a random number generator drawing numbers from a uniform distribution, and l is the indicator of lower bound ($l = 0$), mean ($l = 1$), or upper bound ($l = 2$) of the outcome predictions.

Estimates for the treatment group including all attributes, both regulations X and outcomes O, give us $\hat{\alpha}^{\mathrm{T}}{}_{\mathrm{i}}$, while estimates for the control group, for whom outcome variables were not provided, give us $\hat{\alpha}^{\mathrm{C}}{}_{\mathrm{i}}$. The final dataset enables us to compare the estimated choice parameters between the two groups and quantify the benefits of providing projected outcomes and their impacts on respondents' choice-making process. If we find $\hat{\alpha}^{\mathrm{C}}{}_{\mathrm{i}} \neq \hat{\alpha}^{\mathrm{T}}{}_{\mathrm{i}}$, the regulatory outcomes impact respondents' utility (i.e., $\boldsymbol{\beta}$ isn't a null vector and $f(\mathbf{X})$ does not define a null relation)[7]. A more interesting question is to what extent the projected outcome variables help with the choice-making process. Because the respondents would speculate on the regulatory outcomes if they are not provided, the projected outcome variables might only help slightly when the speculations are close to the projected scenarios.

Two alternative ways are proposed to infer benefits from the provision of outcome variables[8]. The first approach (approach I, reflecting the managers' view) constructs a treatment group coefficient vector $\hat{\alpha}^{\mathrm{T}'}{}_{\mathrm{i}}$ that corresponds to $\hat{\alpha}^{\mathrm{C}}{}_{\mathrm{i}}$, where $\hat{\alpha}^{\mathrm{T}'}{}_{\mathrm{i}}$ is the estimated regulation coefficients when the outcome variables are omitted in the model of responses from the treatment group (even though this group received the outcome attributes). This approach focuses on the correspondence of $\alpha_{\mathrm{i}} + \beta_{\mathrm{j}}\theta_{\mathrm{ji}}$, which combines the outcome effects (e.g., β_{j}) and regulation effects (α_{i}) into the regulation coefficient estimates; $\hat{\alpha}^{\mathrm{T}'}{}_{\mathrm{i}}$ represents the regulation coefficients for the treatment group when estimation omits the outcome variables, so that these coefficients may be compared to those estimated from the control group in vector $\hat{\alpha}^{\mathrm{C}}{}_{\mathrm{i}}$). The null hypothesis, $H_0{:}\,\hat{\alpha}^{\mathrm{T}'}{}_{\mathrm{i}} = \hat{\alpha}^{\mathrm{C}}{}_{\mathrm{i}}$, can be interpreted as stating that the control group can subjectively project the outcomes well enough in the absence of outcome attributes and thus make choices in the same way as the treated respondents do. In other words, failing to reject the null hypothesis would imply that the absence of outcome attributes does not lead to major deviations in respondents' choice over regulatory packages.

The second approach (approach II, reflecting anglers' view) constructs a control group coefficient vector $\hat{\alpha}^{\mathrm{C}'}{}_{\mathrm{i}}$ that corresponds to $\hat{\alpha}^{\mathrm{T}}{}_{\mathrm{i}}$ (estimated with outcomes), where the control group analysis incorporates imputed regulatory outcomes (from biological model - equation [5]; i.e., the analysis is conducted as if the respondents were making decisions with these projected outcomes). Approach II investigates the effect of outcome provision (or absence of outcome) with an assumption that respondents make speculations on outcomes that are close to the projected outcomes, and their choices are based on these speculations. When discrepancies between treated and control estimates are detected (i.e., the null hypothesis is rejected) in both approaches, approach II and approach I give similar implications that the absence of outcome attributes does lead to deviations in respondent's choices. However, it is not clear from both approaches whether the deviations are from the differences between outcome expectations and provided outcomes (the expectation channel), or from the differences in choices based on these outcomes (the choice channel). The meaning of approach 2 stands out when the estimates in approach 1 give statistically insignificant discrepancies (i.e., the null hypothesis is not rejected) between the treated and controls: the deviations from the expectation channel and the choice channel could cancel out in a statistical sense after aggregation in approach 1 (thus the null effect might be a coincident), while approach 2 helps to check whether the null effect of outcome provision is real[9].

3. The Survey Design and Distribution

The Survey

We conduct a stated preference survey on Long Island Sound Tautog to illustrate the proposed method. The primary goal of the underlying project is to evaluate the biological and economic impacts of alternative recreational fishery management practices. The overall goal of the survey is to assess the degree to which effort and compliance of CT and NY anglers[10] would change under different management scenarios, based on anglers' perception of how the regulatory packages would affect the stock and the quality of their fishing. We focus on Tautog (*Tautoga onitis*) fishing in Long Island Sound (LIS), for which we developed a population dynamics model that predicts the spawning stock biomass under different regulations, assuming that fishing effort and compliance do not change (Kasper et al. 2020). These projections are then added to the survey to assist respondents' choices.

The first section of the survey provides background information on different management strategies and elicits basic information about anglers' recreational fishing behaviors (Appendix A provides a complete version of a sample survey). The next section presents a series of choice questions (five in total for each survey) to identify anglers' preference toward different alternative management scenarios. In the final section, the survey includes demographic questions to enable rich interpretations of survey data. We have prepared multiple versions of the survey using statistical design software Ngene to increase design efficiency.[11] The sets of choice questions in each survey are randomly varied and ordered in online survey presentation software Qualtrics.

Sample choice questions are presented in Figure 1 (i.e., on the left is the treated version). The choice questions are designed to elicit anglers' preference toward different regulations. Respondents choose one of three scenarios in each choice question. Each choice scenario comprised: 1) a set of fishing regulations which include season length, daily bag limit, and retention size (length) limit; 2) an enforcement indicator (how many officers are dedicated to Tautog regulation enforcement); and 3) an associated cost increase, the cost needed to implement (manage and enforce) the regulations. Detailed definition and levels of choice option attributes can be found in Table 1. Each choice question was followed by questions regarding how angling habits would change following the suggested changes in the chosen management scenario.

Most anglers are familiar with a minimum size limit, which requires anglers to release fish below the minimum length. When fish abundance is low, managers often increase the minimum length limit. At present, there is a minimum length limit for Tautog of 16" in LIS. Another type of size limit is called the harvest slot limit, setting both a minimum size and a maximum size between which fish may be kept. Slot limits can increase the abundance of large female fish (Kasper et al. 2020), which produce more offspring than small ones (LaPlante and Schultz 2007; see also Barneche et al. 2018).

Due to the novelty of certain alternative strategies and the uncertainty of the population trajectory itself, one major concern in the survey design is the extent to which anglers understand the impact on the stock and future fishing opportunities of different regulatory approaches (e.g., a slot limit versus a minimum size limit). Since we don't know whether respondents can accurately project the impact of different regulatory scenarios, we varied choice questions to examine this issue by randomly assigning respondents to control and treatment groups. In the treatment group, choice questions included descriptions of regulatory outcomes (i.e., future fishing conditions including number of fish caught, keepers caught, and lunkers caught) based on projections of the population dynamics model. As suggested in the conceptual framework, these projected outcomes incorporate uncertainty from the population model, which is based on the Long Island Sound stock assessment model used for managing Tautog[12] with modifications following other recent analyses (Kasper et al. 2020). The final outcome entries in the question are randomly picked from the lower-bound, mean, or the upper-bound prediction (equation [5]), which for this survey comprised the lower bound of the 95% confidence interval, the mean, and the upper bound of the 95% confidence interval, respectively. In the control group, choice question scenarios presented the regulations and the cost attributes but omitted the outcomes.

Survey Preparation and Distribution

We invested extensive effort in survey development to establish clarity for respondents and a correspondence between the understanding of the questions by both respondents and researchers (cf., Johnston et al. 1995). The team discussed 20 separately dated revisions of the survey and conducted multiple focus groups in 2018 and 2019. The survey was also informally evaluated by undergraduates in fishery clubs. Near the end of the revision process, we distributed the survey to project partners, regional experts in fisheries science, and personnel with the New York Department of Environmental Conservation (NY-DEC), and the Connecticut Department of Energy and Environmental Protection (CT-DEEP). The focus groups and reviewers helped to ensure that the material in the survey was interpreted by respondents in a way that would yield an accurate reflection of their experience and opinions. Our final results in this paper are not reviewed or subject to approval from these organizations and reflect independent research and opinions of the authors.

The target population for the survey comprises recreational fishers for Tautog in LIS. The sampling frame for CT includes the 2018 registry of anglers holding marine fishing licenses, and the sampling frame for NY includes the 2018 registry of anglers holding marine fishing licenses who resided in eight counties that are adjacent to or proximate to LIS (Bronx, Kings, Nassau, Putnam, Queens, Richmond, Suffolk, and Westchester Counties, Appendix Figure B1). The registry for each state included angler e-mail addresses or street addresses or both.

We distributed invitations to access the surveys by e-mail and surface mail. Invitations were delivered to all e-mail addresses in the sampling frame, and via postcard (Figure B2) to randomly selected subsamples of 10,000 street addresses in that portion of the sampling frames for which we had no e-mails. Each invitation included the web link to access the online survey. We used a single weblink for all online surveys in each state. Upon accessing the survey online, respondents entered identifying information with which their responses were tagged.

Respondents were invited to contact the team if they preferred to receive a printed copy of the survey by surface mail rather than completing it online.

The method for distributing invitations differed by state. The team had direct access to CT angler contact information, courtesy of CT-DEEP. We issued e-mail invitations and provided the vendor who printed our postcards with the subsample of street addresses for CT anglers. CT respondents received individualized authorization codes in their invitations. In contrast, we did not have direct access to NY angler contact information. NY-DEC personnel distributed email invitations to NY anglers and provided the vendor who printed postcards with the subsample of street addresses of NY anglers. With this method of dissemination, it was not possible to generate individualized authorization codes. Instead, respondents were asked to enter their e-mail address upon accessing the survey.

E-mail addresses in the CT portion of our sample received three reminder invitations on the 4th, 11th, and 32nd day after the initial e-mail. No reminders were sent to postcard recipients or the NY e-mail addresses. The survey was closed after 39 days.

In total, we sent invitations to access the survey to more than 125,000 registered marine anglers (Table B1). The potential respondent pool is undoubtedly much smaller than the total registered anglers, considering that anglers who fish for Tautog are a subset of the marine angler community.

4. Results

Survey Responses

The overall response rate to survey invitations, defined as the proportion of delivered invitations that yielded at least partial completion of a survey, was 2.5%. The response rate to e-mailed invitations was higher than to mailed invitations, and CT invitees responded (i.e., overall response rate 5.3%) at about three times the rate as NY invitees (check Appendix Table B1 for details). Although the general nominal response rate appears low, the true response rate should be considerably higher since the targeted population is the proportion of marine fishing license holders who actually fish for Tautog[13]. According to Marine Recreational Information Program (MRIP) survey data in Long Island Sound, about 25% of marine fishing trips taking place in the first half of the fall season (wave 5 in MRIP) target Tautog. A follow-up survey conducted by the authors shows that 40% out of about 2000 CT marine fishing license holders report themselves as Tautog fishers[14]. When the actual share of anglers who fish for Tautog is between 25% and 40%, the adjusted response rate for CT anglers would be around 13.25% to 21.2%, and the general adjusted response rate, including NY, might be 6.25% to 10% (i.e., we have no additional information relating to the NY population of anglers who target Tautog). Although the aforementioned information helps to alleviate concerns about the low response rates, we recognize that the response rates could be improved. We believe future efforts to survey anglers should be encouraged to adopt approaches that potentially yield better response rates.

About 70% of respondents completed at least the choice question part of the survey, and almost two-thirds of respondents completed the entire survey. 19 invitees requested paper surveys, and, of these, 17 returned completed surveys.

Demographics of respondents and summary data on their fishing habits are provided in Appendix C. Respondents were predominantly male, predominantly white, and most of them had completed an undergraduate college degree. While fishing enthusiasts or professionals appeared to be relatively more likely to complete the survey, statistics (Figure C1) regarding Tautog fishing avidity show that our final sample doesn't primarily capture a specific group of avid Tautog anglers[15]. Moreover, based on the by-group statistics presented in Table C2, the experimental design leads to a well-balanced sample (i.e., no significant difference between the respondents in the treatment group and the control group).

Baseline Model

We analyzed choice question responses to compare respondents' preference to different regulation attributes. A standard conditional logistic regression is employed as the baseline model to provide a preliminary view of the average effects of the regulatory attributes. This baseline model omits outcomes to reflect the fishery managers' aspect of the problem as outcomes are not directly managed. There was relatively strong support for slot limits, and roughly comparable support for status quo management (Column 1 in Table 2), compared to a more restrictive minimum size limit, which was omitted in the regression and serves as the reference-level management practice. Respondents were not in favor of a total moratorium on fishing. As expected, the cost attribute had a negative effect; all else being equal, respondents were not inclined to pay more for Tautog fishing.

Table 2 Column 2 presents a specification including outcomes only for the treatment group, which allows us to test whether outcomes are significant drivers of anglers' choices (i.e., whether $\beta_{\mathrm{kj}} = 0$). The results show that two out of three outcome-associated coefficients β_{kj} are significant at a 10% level, and with these outcomes, all regulatory coefficients become insignificant. A log-likelihood ratio test (LR chi-squared=7.33, p-value=.062) suggests that omitting all of these outcome variables, as a group, imposes a significant restriction on the model and hence the outcome variables significantly influence the respondents' decision process in the treatment group (i.e., rejecting the null hypothesis $\beta_{\mathrm{j}} = 0$ or $\hat{\alpha}^{\mathrm{C}}{}_{\mathrm{i}} = \hat{\alpha}^{\mathrm{T}}{}_{\mathrm{i}}$).

The Treatment Effect – Influence of Outcome Variables on Preference

Based on the conceptual framework, we implement two alternative methods to construct a comparison between the treatment group and the control group. Approach I omits the outcome variables from the choice model estimates of the treatment group dataset, so that the outcome effects are mixed up with the regulatory attribute effects and the resulting coefficients ($\widehat{\boldsymbol{\alpha}}^{\mathbf{T}'}$) are directly comparable to control group coefficients ($\widehat{\boldsymbol{\alpha}}^{\mathbf{C}}$). Approach II imputes outcomes for the control group, which follows the same principle that generates outcome variables in the treatment group (i.e., actually presented in the survey). To get the estimates while mitigating random factors from the imputation, we conducted simulations consist of a large number of repetitions, where each repetition imputes the control outcomes per regulation bundle with a random draw from the outcome distribution of the corresponding regulation bundle in the treatment group. Appendix E shows simulated distributions of selected coefficients and corresponding standard errors (S.E.). The coefficients to be reported in the Tables are the average of the coefficients over all repetitions. Since the standard errors are distributed over a very narrow region (as shown in Appendix E) and the variation in S.E. only minimally affect most of the interested statistical tests, we also report the average S.E. and related test results (with certain exceptions when the test is sensitive to S.E.). Note that the comparison ($\widehat{\boldsymbol{\alpha}}^{\mathbf{T}}$ versus $\widehat{\boldsymbol{\alpha}}^{\mathbf{C}'}$) in Approach II is built on a conjecture that respondents can speculate upon outcomes in a manner that converges to the biological modeling of outcomes used in the treatment group's survey and make choices based on these speculations. The estimates test both the validity of this conjecture and the assertation that the choice behaviors are different.

The results are shown in Table 3. Based on the t-test results from Approach I, we find that control group respondents, without the assistance of outcome variables, are more likely to choose

SQ scenarios and less inclined to pay more for alternative management scenarios. In addition, control group respondents have similar preference patterns among the alternative management strategies (i.e., Slot limit narrow, Slot limit wide, Moratorium, and a more restrictive minimum size limit, which is the omitted size limit in the model). However, it is unknown whether the differences are driven by a certain group of individuals or a general behavioral pattern. This issue will be addressed later through a latent class model analysis. Column 3, 4, and 5 in Table 3 show the simulation results from Approach II, which imputes outcomes for the control group according to the model projection of the same policy bundle in the treatment group. Consistent with the results from Approach I, Approach II generates a significantly (marginally) higher preference on SQ and a lower magnitude of the coefficient on cost for the control group, compared with the treatment group.

Due to the possibility of different random utility dispersion between groups, we adopt an alternative assumption that allows the scale parameter to differ between control and treatment groups. Note that since the scale parameters always present in the likelihood function as the denominator of the coefficient, the identified coefficients in Table 3 are the ratios between the utility parameters and the scale parameters, which are normalized to one in both groups. Thus, the scale parameters cannot be identified from the estimates in Table 3 (Cameron et. al. 2002). To identify group-specific scale parameters and compare coefficients in different treatment groups, we add interaction terms to indicate differences between the two groups. Normalizing the scale parameter for the control group to 1, σ is used to denote the scale for the treatment group. Taking the likelihood maximization in Approach I as an example, the log-likelihood function becomes:

$$LL = \sum_{Control} \ln\left(\frac{e^{\hat{\alpha}_0^{\mathbf{C}}+\hat{\alpha}^{\mathbf{C}}\cdot \mathbf{x_k}}}{\sum_{\mathrm{n}}^{3} e^{\hat{\alpha}_0^{\mathbf{C}}+\hat{\alpha}^{\mathbf{C}}\cdot \mathbf{x_n}}}\right) + \sum_{Treated} \ln\left(\frac{e^{\frac{\hat{\alpha}_0^{\mathbf{C}}+\left(\hat{\alpha}^{\mathbf{C}}+\Delta\hat{\alpha}\right)\cdot \mathbf{x_k}}{\sigma}}}{\sum_{\mathrm{n}}^{3} e^{\frac{\hat{\alpha}_0^{\mathbf{C}}+\left(\hat{\alpha}^{\mathbf{C}}+\Delta\hat{\alpha}\right)\cdot \mathbf{x_n}}{\sigma}}}\right). \quad [6]$$

Results assuming different scale parameters are presented in Table 4. The differences in preference parameters between control and treatment groups in Table 3 become largely insignificant in Table 4 (i.e., joint LR test for all interactions: chi-squared=1.33, p-value=0.2492 for approach I, and chi-squared=1.28, p-value=.2572 for approach II). Nonetheless, the scale parameters in the treated group are lower than the control group (normalized to 1) across both methods. Statistical tests show that, under Approach II, the treatment-specific scale parameter (on average σ = 0.328) is significantly different from one (at .1 significance level) 716 times out of 1000 repetitions. Under Approach I, the treatment-specific scale parameter is not significantly different from one (σ = 0.407, chi-squared = 2.02, p-value = 0.1554). Since the scale parameter represents the standard deviation of random utility in our model, this suggests that dispersion in the estimated utility of the control group (i.e., when outcomes are not provided) is much wider than that of the treatment group. The greater dispersion in the utility of choices in the control group may be partly reflected in the higher frequency of status quo choices.

Notice that estimates in Table 3 are based on the assumption that treatment and control groups hold the same random utility dispersion, while the coefficients come from separated regressions for the two groups. In contrast, the estimates in Table 4 are based on the assumption that the dispersion of random utility differs, and the coefficients come from a pooled regression. We find that when investigated separately assuming the same random utility dispersion, the control group is more likely to show conservative choice behaviors (i.e., higher preference for status quo and low-cost options). However, when investigated in a pooled model allowing for different utility dispersion, anglers in the control group show much higher dispersion in their utility distribution while the intergroup differences in individual coefficients become mostly insignificant (consistent with findings in Haab, Huang, and Whitehead 1999). These results suggest that when outcome variables are not provided, anglers exhibit more conservative choice patterns, reflecting uncertainty in their utility as a function of regulatory attributes[16].

Further Investigation with Latent Class Models

As suggested by conditional logit estimates, the control group is more likely to choose SQ scenarios and less inclined to pay more for alternative management scenarios. However, the mechanism behind this phenomenon is not clear and cannot be recovered from the conditional logit model. The latent class models help to check whether the differences between the treatment and control groups are driven by a fixed share of individuals in the control group who do not figure out the regulatory outcomes and hence are more likely to choose SQ, or by changes in the shares of individuals with a specific choice pattern. Specifically, this analysis compares the class shares of the SQ-choosing groups and the choice equation coefficients between the treatment and control latent classes.

Table 5 presents the information criteria including Schwartz's Bayesian Information Criterion (BIC; Schwarz 1978) and Bozdogan's Criterion (CAIC; Bozdogan 1987). The criteria suggest four latent classes as the most appropriate specification for both the treatment and control groups. The choice patterns of four latent classes are similar in the treatment and control groups (Table 6; details of membership equations are provided in Appendix D). For example, class 1 is named "Slot Supporter" since the clear feature of this class in both treatment and control groups is that they support both slot limits. Therefore, the classes are named the same in both groups. The only dissimilarity in choice pattern between the groups is in class 4, wherein the treatment group version features the preference on option A (i.e., alternative management presented on the left-hand side), whereas the control group version features additional preferences on lower costs, the wide slot limit, higher daily possession limits, and higher enforcement levels.

Comparing the treatment group and control group estimates, the latent class models do show the consistency of the class components to some extent. The latent class logit model allows for heterogeneity in preferences, such that a particular class of respondents is a group within which members have preferences (can choice patterns) that are more similar to each other than to the preferences of respondents in a different class. (Class membership is estimated using characteristics of individual respondents, as we present in Appendix D.) The class-specific estimates tell us that providing outcomes generally does not change the choice pattern of the first three classes, as shown by the signs, significance, and relative magnitude of regulation coefficients in each class.

However, the latent class models do present certain differences across the treatment and control group. Compared to the treatment group, the SQ class in the control group features a more sizable SQ coefficient, and the Slot Supporter and Minimum Size classes feature a slightly lower level of distaste on SQ. The choice pattern changes for the reference class are quite noticeable: the reference class in the treatment group primarily displays a preference toward choosing the alternative option A, while the reference class in the control group dislikes higher annual cost and SQ, and prefers the wide slot, higher daily possession limit, more enforcers, together with alternative option A. Moreover, the control group consists of a larger share of the SQ class and a larger share of the Reference class. These results show that the absence of outcome information in the control group leads to a higher tendency to choose the SQ scenarios both because of the increase in the SQ-choosing class shares and the increase in the preference for the SQ option across classes. A similar story applies to the treatment-control difference in annual cost.

5. Discussion

Our primary motivation is to assess the impact of presenting outcome variables on the respondents' choice-making process and estimation of preferences for policy attributes like regulations on recreational fishing. Respondents might not be able or willing to develop subjective beliefs about the outcomes. Assuming the same scale parameters across treatment groups, results from our application show that respondents who are not provided with the outcome variables choose the status quo option more frequently. This result likely arises because those with little information about the outcome of an alternative option may react with caution about the unknown future, leading such respondents to be more likely to choose the status quo as a hedge against such uncertainty. Such results may be consistent with the discussion of status quo bias (e.g., Kahneman, Knetsch, and Thaler 1991; Samuelson and Zeckhauser 1988; Boxall, Adamowicz, and Moon 2009) or nonparticipation[17] (e.g., von Haefen, Massey, and Adamowicz 2005; Chen, Swallow, and Yue 2020). However, if regulations are well understood by the respondents, the propensity to choose the SQ might be considerably lower when outcome variables are not provided.

Allowing for differential utility dispersion, we find higher uncertainty in anglers' utilities if provided with no outcome attributes. The relatively conservative choice behaviors in the control group are somewhat weakened in these results. While we cannot claim which set of assumptions (i.e., assuming different scales in a pooled model, or assuming the same scale in separated models) is strictly preferred in every aspect, either set provides one aspect of the story[18]. Aggregately, provided with no outcome, respondents show a higher tendency to choose SQ and higher uncertainty of their utility as a function of regulations. The linkage between lower utility dispersion and presentation of outcome is quite similar to previous findings that presentation of outcome would improve model fit (Meldrum et al. 2020), and that it overcomes the effect of respondents adjusting the scenario based on their subjective beliefs (Flores and Strong 2007; Cameron, Deshazo, and Johnson 2011).

In theory, the proposed Approach I (i.e., omitting outcome to construct the estimates for comparison) is preferred to Approach II (i.e., imputing outcome to construct the comparison) in this empirical context, due to the concern on the instability caused by outcome imputation (i.e., although mitigated with simulations, imputed outcome levels still carry randomness into the bootstrapped standard errors) in Approach II. Also, since approach I does show discrepancy between the treatment and control group estimates, the application does not involve the situation where approach II is set up to provide critical cross-check (i.e., when approach I shows no discrepancy). However, our results suggest Approach II does provide some useful implications in this context. For example, assuming different treatment-specific scale parameters, Approach II still shows a statistically significant (at .1 level) difference in anglers' preference for SQ (Column 4 Table 4).

6. Concluding Remarks

This study proposes a systematic approach to quantitatively investigate the impact of projected outcome variables on respondents' choices among alternative regulations in SP studies. In an application to recreational Tautog fishery regulations in Long Island Sound, we find a representative respondent, if not provided with projected outcome variables, is more likely to choose the SQ option and prefers lower costs. An alternative analysis allowing for differential utility dispersion shows that these conservative choice behaviors are likely to reflect the in-depth higher uncertainty in anglers' utilities if provided with no outcome. Further investigation considering heterogeneity (i.e., latent class models) suggests that the control group (i.e., respondents receiving no information on outcome variables) includes more respondents in the class distinguished by a high propensity to choose SQ, while some other classes also show a slightly higher tendency to choose SQ. The results suggest the provision of outcome attributes might be useful for respondents who feel unable to develop subjective beliefs or who have limited confidence in their subjective beliefs (projections) of outcomes, while other respondents do have subjective beliefs about outcomes that are close to scientific projections and make choices across regulations effectively with such beliefs.

Our results suggest that the provision of outcome variables can be potentially important in stated preference studies regarding alternative fishery regulations, or at least in cases similar to our application where regulation outcomes carry considerable uncertainty and unfamiliarity. While our method can be used in different applications, results may vary and depend on the empirical contexts. The provision of outcome information is helpful even if only a small proportion of participants are not familiar with the regulations and have limited experience with the natural resource to develop subjective estimates of the outcome variables, since speculating about the outcome takes both effort and time (statistics in Appendix Table C2 shows our control group takes longer to finish the survey) which then complicates the choice-making process and may drastically reduce the quality of response data. In contrast, the provision of projected outcomes is not likely to be costly, since the simulated outcomes are needed for policy making (or a careful survey design) in many cases. One argument against provision of outcome information would be that the exact outcome entries in the choice scenarios may not reflect the real outcome and hence may bias the inference derived from the study. However, scientific projections are less likely to incorporate more randomness or higher bias than heuristic speculations with limited information. Note that our conclusions are based on a choice experiment with alternative scenarios where the proposed regulations are more strict compared to the status quo, since more regulations are needed to avoid the collapse of Tautog population. It could be interesting to apply our method and check whether the presence of outcome variables have a similar effect when the alternative scenarios contain regulations that are more lax compared to the status. Unfortunately, our empirical context limits our ability to fully uncover the relationship between the relative stringency of alternative regulation scenarios and the role of outcome variable in the decision-making process.

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Tables

Table 1. Choice Question Attributes and Statistics

Variable	Specification and Description (available in the question)	Levels	Mean	Standard Deviation
Regulatory Attributes				
Size limit	Current size limit: min 16 inches ("), no max limit	0,1	0.357	0.479
	Increased min size limit: min 17", no max	0,1	0.182	0.386
	Slot limit narrow: min 16", max 19"	0,1	0.278	0.448
	Slot limit wide: min 16", max 21.5"	0,1	0.182	0.386
Daily possession limit	Can keep as many as # fish per day	1,3,4	2.825	1.032
Fall season start	Earlier season: starting Oct 5th instead of Oct 10th, no change in other seasons	0,1	0.285	0.452
Fall season length	Shorter season: 40 days in the fall season instead of 50	0,1	0.334	0.471
Enforcement level	Number of enforcement agents checking Tautog regulations in CT	16, 41,67,90	41.026	28.491
Outcome Variables (only for the treated)				
Tautog caught	An average angler will catch #% as many fish as what they would catch if current management remains	87-125	104.007	6.910
Keepers caught	An average angler will catch #% as many as what they would catch if current management remains	56-149	93.097	16.404
Lunkers caught	An average angler will catch # fish longer than 23" out of every 10000 caught	0-93	31.657	29.584
Cost Variable				
Cost	Cost of the license and stamp fees $ per year	0, 32, 45, 70, 85	50.357	21.931
N(options) = 31355				

Note: Statistics involves all scenarios/options presented. Demographic statistics are presented in Appendix Table C1 and C2. Current limit size is always bundled with daily possession limit 3, fall season start at Oct 10th, 16 enforcement officers, 100% tautog caught and keepers caught, and 8 lunkers caught, and this bundle is referred to as Status Quo or SQ in the text and analysis. There are 2251 moratorium scenarios with most attributes level as 0. The moratorium scenarios are not counted in the table to avoid confusion in the statistics of choice question design.

Table 2. Conditional Logit Results

Scenario Attributes	Pooled sample baseline	Treatment Group including outcome
Moratorium	-1.134***	-0.515
	(0.0896)	(0.516)
Slot limit narrow (16" to 19")	0.232***	-0.107
	(0.0457)	(0.140)
Slot limit wide (16" to 21.5")	0.321***	0.0436
	(0.0513)	(0.126)
ASC – Option A	0.189***	0.166***
	(0.0282)	(0.0384)
Status Quo (16”)	0.274***	-0.0161
	(0.0555)	(0.116)
Daily possession limit	0.0317**	0.0509
	(0.0114)	(0.0363)
Season reduced by 10 days	0.0636*	0.00701
	(0.0283)	(0.0422)
Season earlier by 6 days	0.0411	0.0533
	(0.0294)	(0.0498)
Number of Enforcers	0.00132+	0.00101
	(0.000720)	(0.000997)
Annual Cost	-0.0188***	-0.0173***
	(0.000774)	(0.00114)
Tautog Caught in five years		-0.00257
(relative to SQ management)		(0.00466)
Keepers Caught in five years		0.00458+
(relative to SQ management)		(0.00252)
Lunkers Caught in five years		0.00348+
(relative to SQ management)		(0.00181)
Log(likelihood)	-11647.395	-5918.100
N (choice scenarios)	33606	16839
N (individuals)	2461	1227

Note: Standard errors are in parentheses, while symbols denote significance levels: +, *, **, *** indicate p<0.1, p<0.05, p<0.01, p<0.001, respectively.

Table 3. Estimates from the Treated and Controls with the Same Scale (Separated Models)

Scenario Attributes	Treated[I] ($\hat{\alpha}^{T\prime}$)	Control ($\hat{\alpha}^{C}$)	T-test ($\Delta_I = 0$)[II]	Treated ($\hat{\alpha}^{T}$)	Control ($\hat{\alpha}^{C\prime}$)[IV]	T-test ($\Delta_{II} = 0$)[II]
Method		Approach I			Approach II	
Moratorium	-1.112***	-1.137***	.15147	-0.515	-1.365*	1.1166
	(0.127)[I]	(0.128)		(0.516)	(0.558)	
Slot limit narrow	0.191**	0.266***	-.86962	-0.107	0.245	-1.5416
(16" to 19")	(0.0639)	(0.0658)		(0.140)	(0.181)	
Slot limit wide	0.318***	0.334***	-.1692+	0.0436	0.342*	-1.6193
(16" to 21.5")	(0.0721)	(0.0735)		(0.126)	(0.135)	
Status Quo (above 16”)	0.148+	0.408***	-2.1693*	-0.0161	0.414***	-2.9594**
	(0.0769)	(0.0810)		(0.116)	(0.0875)	
Daily possession limit	0.0215	0.0459**	-.90476	0.0509	0.0571	-0.1172
	(0.0159)	(0.0166)		(0.0363)	(0.0387)	
Season reduced by 10	0.0208	0.102*	-1.4638	0.00701	0.0970*	-1.4451
days	(0.0393)	(0.0417)		(0.0422)	(0.0458)	
Season earlier by 6 days	0.0191	0.0453	-.47032	0.0533	0.0532	0.0009
	(0.0416)	(0.0427)		(0.0498)	(0.0519)	
Number of Enforcers	0.00112	0.00187+	-.5362	0.00101	0.00189+	-0.6041
	(0.000994)	(0.00105)		(0.000997)	(0.00106)	
Annual Cost	-0.0169***	-0.0208***	2.3318*	-0.0173***	-0.0209***	2.1466*
	(0.00108)	(0.00113)		(0.00114)	(0.00121)	
ASC – Option A	0.164***	0.230***	-.95136	0.166***	0.233***	-1.1506
	(0.0383)	(0.0419)		(0.0384)	(0.0434)	
Tautog Caught in 5 years				-0.00257	0.00245	-0.7013
relative to SQ				(0.00466)	(0.00542)	
Keepers Caught in 5 years				0.00458+	-0.00021	1.4306
relative to SQ				(0.00252)	(0.00221)	
Lunkers Caught in 5 years				0.00348+	0.00024	1.0329
relative to SQ				(0.00181)	(0.00255)	
Log(likelihood)	-5921.766	-5706.193		-5918.100	--	
N (choice scenarios)	16839	16767		16839	16400[III]	
N (individuals)	1227	1234		1227	1234	

[I] Standard errors are in parentheses, while symbols denote significance levels: +, *, **, *** indicate p<0.1, p<0.05, p<0.01, p<0.001, respectively. [II] The numbers represent t-statistics, the signs show whether the differences ($\Delta_I = \hat{\alpha}^{T\prime} - \hat{\alpha}^{C}$ or $\Delta_{II} = \hat{\alpha}^{T} - \hat{\alpha}^{C\prime}$) are above or below zero, and symbols denote significance levels of the difference: +, *, **, *** indicate p<0.1, p<0.05, p<0.01, p<0.001, respectively. [III] The number of observations is smaller than the original control group N since outcomes are imputed based on corresponding regulation bundle in the treatment group (following equation [5]), while a few regulation bundles in the control group are not present in the treatment group. This is caused by the difference in response across groups rather than the design (treatment and control groups were balanced in the surveys sent out). Some regulatory bundles (i.e., options) presented in the control group data do not receive responses in the treatment group, and thus the projected outcomes from the random number generation process in Qualtrics are not recorded. [IV] The coefficients reported are averages based on a simulation consists of 1000 repetitions. The reported standard errors are the average standard errors, since the standard errors only change within a very narrow region in the simulation (shown in Appendix E) and the variation does not qualitatively affect the T-tests.

Table 4. Difference in Treated and Controls Assuming Different Scale Parameters[I]

Scenario Attributes	Main Effects ($\hat{\alpha}^{C}$)	Interactions with Treatment ($\Delta\hat{\alpha}^{C}$)[II]	Main Effects ($\hat{\alpha}^{C\prime}$)	Interaction with Treatment ($\Delta\hat{\alpha}^{C\prime}$)[II]
Method	Approach I		Approach II[IV]	
Moratorium	-1.137***	-0.428	-1.020*	-0.341
	(0.128)	(0.538)	(0.445)	(0.5057)
Slot limit narrow	0.266***	0.00254	0.0667	-0.0132
(16" to 19")	(0.0658)	(0.141)	(0.136)	(0.1340)
Slot limit wide	0.334***	0.113	0.197+	0.0840
(16" to 21.5")	(0.0735)	(0.180)	(0.114)	(0.1700)
Status Quo (above 16")	0.408***	-0.200	0.387***	-0.242+
	(0.0810)	(0.133)	(0.0869)	(0.1340)
Daily possession limit	0.0459**	-0.0156	0.0597+	-0.0168
	(0.0166)	(0.0296)	(0.0321)	(0.0290)
Season reduced by 10 days	0.102*	-0.0727	0.0984*	-0.0777
	(0.0417)	(0.0696)	(0.0445)	(0.0676)
Season earlier by 6 days	0.0453	-0.0184	0.0617	-0.0188
	(0.0427)	(0.0729)	(0.0491)	(0.0712)
Number of Enforcers	0.00187+	-0.000299	0.00183+	-0.0004
	(0.00105)	(0.00179)	(0.00106)	(0.00173)
Annual Cost	-0.0208***	-0.00292	-0.0210***	-0.0017
	(0.00113)	(0.00704)	(0.00119)	(0.00662)
ASC – Option A	0.230***		0.230***	
	(0.0419)		(0.0439)	
Tautog Caught in 5 years			0.00004	
relative to SQ			(0.00415)	
Keepers Caught in 5 years			0.00161	
relative to SQ			(0.00186)	
Lunkers Caught in 5 years			0.00262	
relative to SQ			(0.00179)	
σ	1	0.407 (0.417)	1	0.328[V] Percent $H_0(\sigma = 1)$ Rejected: 71.6%
Log(likelihood)	-11627.958		-11504.074	
N (choice scenarios)	33606		33329[III]	
N (individuals)	2461		2461	

Note: [I] The underlying model for each regression (approach I or II) are from a single model with interactions and different scale parameters across different treatment groups, unlike Table 3. Standard errors are in parentheses, while symbols denote significance levels: +, *, **, *** indicate p<0.1, p<0.05, p<0.01, p<0.001, respectively. [II] Numbers represent coefficients of the interaction between the regulatory attributes and the treatment dummy, denoting the differences between the treatment and control group. Symbols show whether the differences ($\Delta\hat{\alpha}^{C}$ or $\Delta\hat{\alpha}^{C\prime}$) are statistically different from zero: +, *, **, *** indicate p<0.1, p<0.05, p<0.01, p<0.001, respectively. [III] The number of observations is smaller since outcomes are imputed based on the corresponding policy combinations in the treatment group, while a few policy combinations in the control group do not present in the treatment group. Like suggested in the notes of Table 3, this isn't caused by the survey design. [IV] The coefficients reported for approach II are averages based on a simulation consists of 1000 repetitions. The reported standard errors are the average standard errors, since the standard errors only change within a very narrow region in the simulation (shown in Appendix E). [V] As the test for $H_0(\sigma = 1)$ is sensitive to standard error variations in σ, we do not report the average standard error. Instead, we report the percentage of σ being significantly (at .1 significance level) different from 1 across all repetitions.

Table 5. Statistics on Latent Class Models of Different Latent Classes

Classes	Log(likelihood)	No. of parameters	AIC	BIC	CAIC
Panel A - Treatment Group					
2	-4856.37	29	9770.744	9919.002	9948.002
3	-4713.28	48	9522.563	9767.955	9815.955
4	-4612.32	67	9358.634	**9701.16**	**9768.16**
5	-4567.15	86	9306.304	9745.964	9831.964
6	-4504.3	105	9218.595	9755.389	9860.389
7	-4461.07	124	9170.132	9804.06	9928.06
Panel B - Control Group					
2	-4650.06	29	9358.109	9506.531	9535.531
3	-4506.13	48	9108.256	9353.921	9401.921
4	-4423.3	67	8980.593	**9323.5**	**9390.5**
5	-4367.22	86	8906.43	9346.58	9432.58
6	-4334.08	105	8878.157	9415.549	9520.549
7	-4312.31	124	8872.616	9507.25	9631.25

Table 6. Comparison of Four-segment Latent Class Logit Models[I]

Panel A: Treatment Group (Choice Eq.)	Class 1 (Slot Supporter)	Class 2 (Mini Size)	Class 3 (SQ)	Class 4 (Reference)
Annual Cost	-0.0327***	-0.0168***	-0.0392***	-0.000268
	(0.00364)	(0.00389)	(0.00488)	(0.0122)
Moratorium	-1.488***	-3.246***	-1.497***	2.150
	(0.376)	(0.495)	(0.418)	(2.174)
Slot limit narrow	0.408*	0.208	-1.049***	1.258
(16" to 19")	(0.202)	(0.301)	(0.236)	(1.348)
Slot limit wide	0.819**	0.378	-0.181	-0.258
(16" to 21.5")	(0.255)	(0.265)	(0.231)	(0.559)
ASC – Option A	0.0494	-0.935***	0.615**	2.260***
	(0.163)	(0.167)	(0.192)	(0.470)
Status Quo (above 16”)	-1.234***	-2.611***	1.759***	-0.837
	(0.227)	(0.336)	(0.310)	(0.832)
Daily possession limit	0.507***	-0.453***	0.159*	0.0202
	(0.0817)	(0.0699)	(0.0742)	(0.136)
Season reduced by 10 days	-0.307*	0.124	0.416*	-0.0603
	(0.126)	(0.107)	(0.204)	(0.264)
Season earlier by 6 days	0.122	0.197	-0.407*	0.00323
	(0.130)	(0.134)	(0.187)	(0.263)
Number of Enforcers	-0.00268	0.000462	-0.000853	0.000921
	(0.00300)	(0.00397)	(0.00388)	(0.00747)
Class share[II]	0.271	0.231	0.365	0.133
Panel B: Control Group (Choice Eq.)	**Class 1 (Slot Supporter)**	**Class 2 (Mini Size)**	**Class 3 (SQ)**	**Class 4 (Reference)**
Annual Cost	-0.0630***	-0.0192***	-0.0229***	-0.0178**
	(0.0106)	(0.00510)	(0.00647)	(0.00554)
Moratorium	-3.331***	-2.699***	-0.0822	-0.999
	(0.473)	(0.673)	(0.491)	(0.856)
Slot limit narrow	0.565*	-0.173	-0.712**	0.225
(16" to 19")	(0.272)	(0.371)	(0.246)	(0.435)
Slot limit wide	0.918***	0.289	-0.543	0.880**
(16" to 21.5")	(0.272)	(0.260)	(0.301)	(0.276)
ASC – Option A	0.135	-0.806***	0.311	1.212***
	(0.178)	(0.182)	(0.216)	(0.221)
Status Quo (above 16”)	-0.854*	-2.497***	2.158***	-1.548**
	(0.332)	(0.442)	(0.333)	(0.523)
Daily possession limit	0.289**	-0.303***	0.390***	0.142*
	(0.0933)	(0.0712)	(0.105)	(0.0626)
Season reduced by 10 days	-0.0667	0.224	0.552*	-0.0519
	(0.193)	(0.119)	(0.221)	(0.112)
Season earlier by 6 days	-0.156	0.263*	-0.187	-0.00696
	(0.149)	(0.119)	(0.204)	(0.126)
Number of Enforcers	-0.00243	0.00609	-0.00460	0.0112*
	(0.00434)	(0.00432)	(0.00435)	(0.00458)
Class share[II]	0.233	0.190	0.376	0.201

Note: [I] Treatment group estimation is based on 5613 choices from 1227 survey respondents. Control group estimation is based on 5589 choices from 1234 survey respondents. Standard errors are in parentheses. *, **, *** indicate $p<0.1$, $p<0.05$, $p<0.01$, respectively. [II] Class shares are mean predicted class membership probabilities based on the membership equations (in Appendix D).

Figure Titles

Figure 1. Sample Choice Questions – Treated (left) versus Control (right)

Endnotes

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[1] The fall season is when most (about 70% according to the 2016 stock assessment) recreational fishing effort toward Tautog takes place.

[2] Note that the projected outcomes here do not consider how anglers' behavioral changes would affect population dynamics. Taking anglers' behavioral changes into the assessment of population dynamics is a broad goal of the underlying research project.

[3] In our context, both the regulations and outcomes enter the utility model. Previous literature (e.g., Johnston and Duke, 2007) shows that policy attributes can affect preference even after controlling for outcomes. The outcomes O in the application refer to the outcomes that directly affect the participants through the biological response of the fishery. The effects coming from regulation changes are much broader than the direct consequences on future fishing experience – for examples, besides changes in non-use values, regulation changes may impose an immediate reduction in harvest per trip, and can influence the general ecosystem and long-term economic benefits from maintaining a specific fishery. Also, increase in enforcement effort can generate intrusions on an angler's day rather than only influencing catch rates or distribution of fish size.

[4] Note that equations [4] could take different forms if the relationship between the regulations and outcomes are approximated differently (i.e., not with a linear specification). But these changes would not affect the ability of this framework to illustrate the main story of this study.

[5] The actual biological model does not follow the linear outcome function $O = f(X) = \theta X$, which is ultimately adopted as an approximation in the linear utility model; our outcome variables were generated from the biological model instead of Kasper et al. (2020). The linear approximation in the estimation doesn't affect its ability in illustrating the major claims in this paper, since the main tests do not rely on the exact functional form of f(X) in equation [2b]. For example, the interpretation of the test $\hat{\alpha}^{T\prime}{}_i = \hat{\alpha}^C{}_i$ doesn't really rely on whether $\hat{\alpha}^{T\prime}{}_i$ and $\hat{\alpha}^C{}_i$ can be written as a linear function of the utility parameters (i.e., $\alpha_i + \beta_j\theta_{ji}$). If the estimated $\alpha_i + \beta_j\theta_{ji}$ is statistically different between the treated and controls, the corresponding real utility expression ($\alpha X + \beta f(X)$) would also be different. If the estimated $\alpha_i + \beta_j\theta_{ji}$ is consistent across treatment groups, the corresponding utility expression are very likely to be consistent (at least very close). Also, concerning the test of $\hat{\alpha}^C{}_i = \hat{\alpha}^T{}_i$ or $\beta_j = 0$, if the linear relation β_j is not a null vector, the actual nonlinear relation is almost surely not a null relation.

[6] There might be concerns on that the random draw uncertainty behind the outcome projections might introduce noise and affect the conclusions. We think this effect would be trivial for several reasons. First, the outcomes, once projected, do not carry an uncertainty in individual choice options. Second, even if the anglers might think, based on prior knowledge or experience, the outcomes carry some sort of uncertainty, such perceived uncertainties are not really caused by the uncertainty underlying the random draws. Third, the real concern would be that one angler might find the projected outcomes are different across two very similar regulation scenarios in different questions, and thus may infer the uncertainty in the outcome random draws. We did restrict that one respondent won't get two questions with similar alternative regulation sets. We also strongly suggested, before each question, that respondents only compare the three options in the question (i.e., do not compare across questions). Moreover, since each participant will only answer four choice questions, the chance an angler can figure out the uncertainty in outcome projections is very low and hence the uncertainty in random draws is not likely to affect his decisions.

[7] Note that this can be easily tested and actually is tested in the results section.

[8] Note that the direct comparison of $\hat{\alpha}^C{}_i$ (control group estimation with regulatory variables) and $\hat{\alpha}^T{}_i$ (treatment group estimation with regulatory and outcome variables) doesn't provide much information to this objective, as explained above.

[9] Setting a constraint that controls' expected outcomes are very close to the treated (as approach 1 would predict in this case), approach 2 might show the choice patterns, incorporating these outcome variables, are quite different and hence will refute the "conclusion" in approach 1 that the absence of outcome has null effect. Here, the null effect conclusion would incorporate the zero effect from the expectation channel (i.e., controls have outcome

expectations that are very close to the projected outcomes) and from the choice channel (i.e., controls are chosen differently based on the expected outcomes than what the treated do based on projected outcomes). Approach 2 hence functions as a fail-safe mechanism. Also, if the null effects from both channels hold, approach 2 would give null effect as well (i.e., little discrepancies across all coefficients).

[10] Surveys to CT and NY anglers are designed slightly differently to reflect salient features of each state's fishery. For example, the status quo season starting dates are slightly different.

[11] D-efficiency is used to choose the final set of choice questions.

[12] See details at: https://www.asmfc.org/uploads/file/589e1d3f2016TautogAssessmentUpdate_Oct2016.pdf.

[13] Since there was little information to identify Tautog anglers beforehand, the survey was sent to marine fishing license holders, whether they fish for Tautog or not. As it is clear, in the invitation and the beginning part of the survey, that the survey is intended for Tautog anglers, anglers not targeting Tautog were very likely to be discouraged from taking the survey.

[14] This new survey was only sent to those who did not respond to the original survey. The ratio (40%) does not necessarily contradict the ratio based on trips from MRIP. An angler can take multiple trips but only one of these trips targets Tautog, and thus the share of anglers fishing for Tautog should be naturally higher than the share of fishing trips targeting Tautog. However, this ratio (40%) might be an overestimate since people may get an impression that the research team is studying Tautog (i.e., although the new survey wasn't formatted this way) and are likely to say "yes" even if they only marginally have the intention to fish Tautog. Also, this follow-up survey, conducted in late 2020, could not be sent to NY due to coordination issues sourced from the COVID pandemic.

[15] Although Tautog anglers (i.e., the target population) are relatively experienced anglers in general (since Tautog is a difficult fish to catch), it should be clarified that our final sample doesn't only capture a specific group of avid Tautog anglers. Survey statistics (Appendix Figure C1) show that a large proportion of the respondents (about 70%) fish Tautog less than 10 days per year, and 25% of them actually fish less than 2 days for Tautog. For comparison, the avid group of Tautog anglers (in the heavy distribution tail) could fish 30 to 80 days for Tautog per year, with the density generally evenly distributed across that range.

[16] Note that these discrepancies (i.e., scale difference or coefficients discrepancies) could reflect the differences in the way they make choices based on expected outcomes and given outcomes. And it's also possible that the discrepancies represent the deviations in the expected outcomes from the projected outcomes. While approach 2 mildly deals with these two different mechanisms (holding one as true and test another), a potential way to investigate further is to ask the control group their expected outcomes in the survey (we thank one of the reviewers for raising this point). Although it might increase the cognitive burden of answering a particular choice question or introduce certain framing effects, it would provide useful information and the issues could be offset by other means (e.g., make these expectation questions optional or reduce choice question numbers).

[17] Prior studies identify nonparticipation as occurring when, for example, control group respondents choose SQ across most of the questions to show their nonparticipation tendency or dissatisfaction about the lack of clarity regarding the outcomes.

[18] The separate models in Table 3 assumes the same scale but two (fully) different sets of coefficients for the treated and controls. While the pooled model in Table 4 with scale difference does have inter-group differences represented by the interactions, it has to involve a common alternative specific constant (ASC- Option A) for the scale parameters to be identifiable. Thus, moving from Table 3 to Table 4 actually involves a trade off between the scale difference and the ASC difference. Assuming the same ASC seems to be a weaker constraint (i.e., treated and controls have the same tendency to choose the left-hand side alternative option) than the common scale assumption, thus the different-scale model is generally preferred in practice. However, there's no guarantee that the common ASC assumption is theoretically sound and practically unrestrictive.

Imagine that changes to harvest restrictions, enforcement, and costs will be effective in 2020, and consider outcomes projected to occur in 5 years. Please tell us in each question **which of the management options you prefer,** comparing only the three options below. Each column (column Current, A, or B) presents one option. (Explanation of the attributes)

Choice Question: Vote at the bottom of the table

Current	A	B
	Harvest Restrictions:	
	Size limit	
Min 16 No Max	Min 16 Max 21.5	Min 17 No Max
	Daily possession	
3 per day	4 per day	1 per day
	Fall season	
50 days Starting Oct 10th.	50 days Starting Oct 5th.	40 days Starting Oct 10th.
	Enforcement (number of enforcement officers dedicated to Tautog)	
16	41	90
	Outcome in 5 years:	
	Tautog caught (compared to current management)	
100%	87% of current	118% of current
	Keepers caught (compared to current management)	
100%	72% of current	107% of current
	Lunkers caught (per 10,000 fish caught)	
8	57	8
	Cost (license and stamp fees)	
$32 per year	$45 per year	$70 per year

Which of these three options do you vote for?

- Current management
- Alternative Management A
- Alternative Management B

Imagine that changes to harvest restrictions, enforcement, and costs will be effective in 2020, and consider outcomes likely to occur in 5 years. Please tell us in each question **which of the management options you prefer,** comparing only the three options below. Each column (column Current, A, or B) presents one option. (Explanation of the attributes)

Choice Question: Vote at the bottom of the table

Current	A	B
	Harvest Restrictions:	
	Size limit	
Min 16 No Max	Min 16 Max 21.5	Min 16 Max 19
	Daily possession	
3 per day	1 per day	4 per day
	Fall season	
50 days Starting Oct 10th.	40 days Starting Oct 10th.	50 days Starting Oct 5th.
	Enforcement (number of enforcement officers dedicated to Tautog)	
16	16	41
	Cost (license and stamp fees)	
$32 per year	$85 per year	$32 per year

Which option do you vote for?

- Current management
- Alternative Management A
- Alternative Management B