Accounting for Outside Options in Discrete Choice Models: An Application to Commercial Fishing Effort

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Abstract

Discrete choice models often feature a generic outside option that combines all alternatives other than those of particular interest to the researcher, which allows overall demand for the alternatives of interest to be captured. I demonstrate that combining diverse alternatives into a single outside option can result in distorted parameter estimates and misleading predictions. To evaluate the practical importance of how outside options are treated, I use data on the Florida spiny lobster and stone crab fisheries to compare a discrete choice model that explicitly accounts for individuals' ability to target both species with one that includes stone crab alternatives in the generic outside option. I find that parameter estimates and predictions for the lobster fishery depend heavily upon whether stone crab alternatives are explicitly accounted for. In addition, I conduct a series of Monte Carlo experiments, which demonstrate that the sign and magnitude of differences in predictions between models are complex functions of the characteristics of the empirical environment. Together, these results highlight the importance of carefully considering the composition of outside options when estimating discrete choice models and making predictions based on the estimates. (JEL Q22, C23, and C25)

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I Introduction

Discrete choice models have become widely used to study individuals' selection among finite sets of alternatives. These models provide a tractable framework in which to estimate the drivers of agents' decision-making and allow researchers to make behavioral and welfare predictions. Often, the researcher is interested in choices over a subset of all possible alternatives, due to the aim of the study or due to data availability. However, if the researcher is interested in whether agents choose *any* of the alternatives of interests, then the choice set must include all possible alternatives, not just those of particular interest. This is typically accomplished by combining all other alternatives into a generic outside option.[1] This is the case in analyses spanning many areas of study, such as product demand (Berry, Levinsohn and Pakes (1995)), energy conservation (Cameron (1985) and Allcott and Wozny (2014)), recreational demand (Morey, Rowe and Watson (1993)), and commercial fishing effort (Smith (2002) and Smith and Wilen (2003)).

In this paper, I demonstrate that it is important to carefully consider the composition of agents' outside options when estimating discrete choice models and making predictions based on the estimates. When alternatives are combined to form a single outside option, they are implicitly treated as identical alternatives, regardless of their similarity to one another or their similarity to other explicitly-modeled alternatives. This can result in misleading parameter estimates and predictions for a number of reasons. First, outside options are made observationally equivalent by homogenizing alternative-specific characteristics. Second, variables are constrained to have identical effects on all alternatives contained in the outside option. In addition to biasing parameter estimates, these issues can distort estimated substitution patterns between choices that are explicitly modeled, particularly when some of the alternatives contained in the outside option share commonalities with modeled alternatives. Third, the set of policy predictions that the researcher is able to consider is limited to those policies that affect only explicitly-modeled alternatives.

I examine the quantitative effects of these issues in the context of a discrete choice model

[1] For example, in estimating the determinants of demand for differentiated automobile models, not purchasing an automobile must be included in the choice set in order to allow overall demand for automobiles to depend on variables such as prices.

of commercial fishing effort. In particular, I consider a case in which the researcher is primarily interested in understanding the location decisions of individuals harvesting spiny lobster. In addition to fishing for lobster, these individuals may fish for stone crab in each location or engage in a number of other activities, such as leisure time. I consider two characterizations of this choice set. In the first, which I label the "true" model for expositional purposes, I explicitly model each species-location alternative and combine all other possible activities into a generic outside option. In the second, which I label the "naive" model, I combine stone crab alternatives with all other activities to form the outside option. In doing so, values of alternative-specific variables, such as revenues and costs, are implicitly set to zero for stone crab alternatives, and other variables, such as weather, are constrained to affect stone crab fishing in the same manner that they affect all other outside options. Given that stone crab fishing is almost certainly more similar to lobster fishing than to non-fishery activities, these restrictions are likely to produce biased parameter estimates and misleading policy predictions.

I find that estimated marginal effects differ substantially in magnitude, and the predicted effects of policy changes differ both in magnitude and, in some cases, the direction of the effect. Specifically, using the fitted models, I simulate the effect of marine reserve establishment (area closures) – an increasingly popular regulatory tool – on total effort and the distribution of effort in the lobster fishery. When areas are closed to the lobster fishery only, both models necessarily predict a decrease in overall lobster fishing effort. However, the magnitude of the predicted response differs between models by more than 50%. When areas are closed to both fisheries, in some instances, the true model predicts an increase in overall lobster fishing, a result that is ruled out by the naive model. The increase in lobster fishing effort arises because individuals that had chosen to fish for stone crab in a now-closed area find it optimal to fish for lobster in a still-open area. The propensity to target another species in response to area closures cannot be captured by the naive model. As a result, the naive model predicts a decrease in lobster trips.

To determine the extent to which the empirical results depend on the specifics of the lobster and stone crab fisheries, I conduct a series of Monte Carlo experiments. In these experiments, I vary characteristics of the choice environment and evaluate how variations affect estimates and predictions of the misspecified model. This exercise reveals that both the sign and magnitude of biases in estimates and predictions are heavily dependent on the choice environment. While the relationships between characteristics and biases are, to a large extent, predictable, it is not straightforward to forecast the sign and magnitude of biases when a number of characteristics interact. The empirical results and Monte Carlo analysis suggest that researchers and practitioners should explicitly model choices closely related to the decisions of interest whenever possible and, when not possible, carefully consider the potential biases introduced by pooling such choices with other outside options.

The treatment of outside options is not an issue that is limited to discrete choice applications. In demand analysis, the researcher is typically interested in modeling demand for a subset of goods and must decide how to treat all remaining goods. Several strategies have been developed to cope with this issue. If the goods of interest enter preferences through a weakly separable function, demand for these goods may be modeled conditional on total expenditures allocated to these goods. Alternatively, if the prices of goods not modeled vary proportionally across observations, the researcher may aggregate these goods into a single Hicksian composite good and model demand for the goods of interest as a function of own prices, the composite good price, and income. In most empirical applications, including the one studied here, the researcher will find the assumptions required to apply these methods too restrictive. A third option, the incomplete demand system approach, developed in Epstein (1982) and further analyzed in LaFrance and Hanemann (1989) and von Haefen (2002), involves modeling demand for the goods of interest as a function of own prices, all remaining goods prices, and income. A number of studies have used this approach to estimate recreation demand models.[2]

Although appealing, the incomplete demand system approach is not well-suited for discrete choice applications. When a good is not consumed, it is not the observed market price for the good that influences choices, but the price that would drive demand to zero (the "virtual" price),

[2] See, e.g., Eom and Larson (2006) and Phaneuf, Carbone and Herriges (2009).

which is not observed by the researcher. A correctly specified incomplete demand system includes observed prices for goods that are consumed and virtual prices for goods that are not consumed. Although methods have been developed to consistently estimate *complete* demand systems when corner solutions are present, no feasible method currently exists for incomplete demand systems (von Haefen, 2010). Moreover, von Haefen (2010) demonstrates that biases in welfare estimates can be quite large when observed market prices are used in place of virtual prices in an incomplete demand system with corner solutions. Because discrete choice problems necessarily involve a multitude of corner solutions, these biases are likely to be exacerbated in such settings.

This study contributes to a broader literature on choice set formation in discrete choice models that is devoted to understanding the implications of theoretical assumptions and practical concessions made by analysts when defining choice sets. An important theoretical decision faced by the analyst is determining the appropriate scope of individuals' choice sets, which requires identifying reasonable and relevant substitutes. Jones and Lupi (1999) investigate the consequences of expanding the choice set to include additional "reasonable" substitute activities. For a number of reasons, such as lack of information or prohibitive cost, the set of alternatives considered by an individual when making a choice may not consist of the universe of available alternatives. This observation has lead a number of researchers to investigate the consequences of including "irrelevant" alternatives in the choice set. Peters, Adamowicz and Boxall (1995), Adamowicz et al. (1997), and Hicks and Strand (2000) consider the case in which individuals may not be familiar with all alternatives and use survey data to identify and remove unfamiliar alternatives from individuals' choice sets. Parsons and Hauber (1998) and Banzhaf and Smith (2007) study choice in settings with a spatial dimension and consider the case in which alternatives beyond a certain distance cease to be considered by the individual. Carson and Louviere (2014), however, argues that truncating the choice set creates selection bias if the consideration set formation process is endogenous and urges analysts to model the process by which consideration sets are formed together with the choice process. A number of papers follow this approach. Based on Manski (1977), Haab and Hicks (1997), von Haefen (2008), Hicks and Schnier (2010), Li, Adamowicz and Swait (2015), and Thiene, Swait and Scarpa (2017) consider the case in which these processes are separable and independent. Horowitz and Louviere (1995) and Swait (2001) consider the case in which utility determines both consideration sets and choices. Rather than assume irrelevant (in this case unfamiliar) alternatives are excluded from the consideration set, Parsons, Massey and Tomasi (1999) allows for different utility functions for alternatives deemed familiar and unfamiliar. My paper is most similar to Jones and Lupi (1999) in that I consider the treatment of "reasonable" alternatives. A key distinction is that I evaluate the consequences of pooling such alternatives into a composite outside option rather than omitting them from the choice set entirely.

An important practical issue faced by the analyst is the computational burden associated with large choice sets. One solution to this problem is to aggregate sets of alternatives. Parsons and Needelman (1992), Feather (1994), and Kaoru, Smith and Liu (1995) caution against this practice and suggest that, when aggregation is necessary, the estimation adjust for the number of alternatives that have been aggregated and for the level of heterogeneity among aggregated alternatives. Lupi and Feather (1998) and Parsons, Plantinga and Boyle (2000) investigate the use of "partial" aggregation whereby alternatives assumed to be less relevant are aggregated in a meaningful way while alternatives assumed to be more relevant remain disaggregated. Based on McFadden (1978), Parsons and Kealy (1992) argue that when the choice set must be reduced for computational reasons, it is better to draw a random subset of all possible alternatives than to aggregate alternatives into fewer distinct choices. My paper contributes to this literature by characterizing the biases in parameter estimates and predictions of a particular form of aggregation that is common in practice: combining diverse alternatives into a generic outside option.

This paper also contributes to the literature using discrete choice models to better understand and manage commercial fisheries, beginning with Bockstael and Opaluch (1983). Over the years, researchers have extended and adapted the multinomial logit model of Bockstael and Opaluch (1983) to study a number of aspects of harvester behavior, including location and participation decisions,[3]

[3]See, e.g., Eales and Wilen (1986), Mistiaen and Strand (2000), Smith (2005), Hicks and Schnier (2006), Hicks and Schnier (2008), Haynie, Hicks and Schnier (2009), Haynie and Layton (2010), Abbott and Wilen (2010), Abbott and Wilen (2011), Smith, Zhang and Coleman (2008), Smith (2002), Smith and Wilen (2003), Kahui and Alexander (2008), and Smith, Sanchirico and Wilen (2009).

temporal dependency,[4] and complex forms of individual heterogeneity.[5] The species target decision, however, has remained a largely overlooked aspect of behavior.[6] I explore the importance of this dimension of choice and show that omitting it from the model can lead to misleading predictions of the effects of commonly-considered policies, such as the establishment of marine reserves.

The following section lays out a discrete choice framework that typifies models common in the commercial fishing literature, making clear the distinction between a model that explicitly formulates the species target decision and one that includes secondary species in the outside option. In Section III, I estimate these models using data from the Florida spiny lobster and stone crab fisheries, and I discuss the important differences in the estimates and policy implications of the two models. Section IV presents the results of the Monte Carlo experiments, and the final section discusses the overall implications of my findings.

II Discrete Choice Models of Fishing Effort

In this section, I describe how participation, species target, and location decisions may be modeled in a three- and two-dimensional nested logit framework. While much of the derivation of these models will be familiar to the reader, this facilitates a comparison of models and lays the foundation for the empirical application and Monte Carlo experiments.

II.A Three-Dimensional Nested Logit

Consider an environment where, on each choice occasion ($t = 1, 2, ..., T$), individuals ($i = 1, 2, ..., I$) make a participation decision, $p \in \{0, 1\}$, a species target decision, $s \in \{0, 1, 2, ..., S\}$, and a location decision, $j \in \{0, 1, 2, ..., J\}$, where 0 denotes non-participation at each stage of the decision process. Define an alternative, psj, as an outcome arising from a particular combination of decisions. If individuals are able to target each species in each location, the choice set consists of $S \times J + 1$ alternatives. Each alternative provides the individual with some amount of utility, U_{psjit}, which may

[4]See, e.g., Hicks and Schnier (2006), Hicks and Schnier (2008), and Smith (2005).

[5]See, e.g., Mistiaen and Strand (2000), Smith (2005), and Haynie, Hicks and Schnier (2009).

[6]Although a few studies have incorporated this dimension of choice – see, e.g., Bockstael and Opaluch (1983), Holland and Sutinen (2000), Curtis and Hicks (2000), and Zhang and Smith (2011) – to my knowledge, none have examined how doing so affects estimates and policy predictions, which is the focus of my paper.

depend on the characteristics of the alternative, individual, or choice occasion. A random utility model specifies the utility associated with each alternative-individual-choice occasion as consisting of a deterministic component, which is known by the researcher up to some parameters, and an additive random component, which is not known by the researcher:

$$U_{psjit} = \widetilde{V}_{psjit} + \tilde{\epsilon}_{psjit}. \tag{1}$$

On each choice occasion, individuals select the alternative that provides the greatest utility among all possible choices. Although utility is not fully observed by the researcher, she can make probabilistic statements about the decision maker's choice. The functional form that these choice probabilities take depends on assumptions regarding the joint probability distribution of the random components.

In the fisheries literature, the most common formulations for discrete choice probabilities are multinomial logit[7], nested logit[8], mixed logit[9], and discrete choice dynamic programming[10]. Multinomial logit, which arises under the assumption that the error terms, $\tilde{\epsilon}_{psjit}$, are independently and identically Gumbel distributed, is the simplest and easiest to estimate. However, this formulation imposes that relative choice probabilities remain invariant to changes in the choice set. This restriction – the well-known and often inappropriate assumption of independence of irrelevant alternatives (IIA) – is particularly problematic in a fisheries context where the researcher hopes to uncover *how* relative probabilities change as policies alter the choice set. Although mixed logit and discrete choice dynamic programming offer a number of advantages, including relaxing the IIA assumption, the complexities of these models are unnecessary to convey the point of this paper. Instead, I adopt a nested logit framework, which is intuitively appealing, partially relaxes the IIA assumption, is computationally efficient, and, for these reasons, is a popular choice among fisheries economists.

The nested logit model permits a specific type of correlation structure in unobserved utility by allowing researcher-specified groups and sub-groups of alternatives to share common error compo-

[7]See, e.g., Bockstael and Opaluch (1983), Eales and Wilen (1986), Abbott and Wilen (2010), and Abbott and Wilen (2011).

[8]See, e.g., Morey, Rowe and Watson (1993), Holland and Sutinen (2000), Smith (2002), Smith and Wilen (2003), and Kahui and Alexander (2008).

[9]See, e.g., Mistiaen and Strand (2000), Smith (2005), and Haynie, Hicks and Schnier (2009).

[10]See, e.g., Hicks and Schnier (2006), and Hicks and Schnier (2008).

nents. When the choice set is multi-dimensional, it is natural to group alternatives that share a common element along one or more dimensions. To make assumptions clear, rewrite (1) as

$$\begin{aligned} U_{psj} &= V_p + V_s + V_j + V_{ps} + V_{pj} + V_{sj} + V_{psj} \\ &\quad + \epsilon_p + \epsilon_s + \epsilon_j + \epsilon_{ps} + \epsilon_{pj} + \epsilon_{sj} + \epsilon_{psj}, \end{aligned} \tag{2}$$

where the composite deterministic and error terms have been decomposed into portions that vary over each dimension and combination of dimensions of choice, and I have dropped individual- and time-subscripts to simplify notation. In this setting, when $p = 0$ the choice set consists of a single alternative, so (2) reduces to

$$U_{psj} = V_p + V_{ps} + V_{pj} + V_{psj} + \epsilon_p + \epsilon_{ps} + \epsilon_{pj} + \epsilon_{psj}. \tag{2'}$$

While, in reality, all of the error components in (2′) may have positive variance, the nested logit model is only able to accommodate a subset of these possibilities. To meet these requirements, I assume that $var(\epsilon_{pj}) = 0$, which is equivalent to assuming that there are no unobserved location-specific, species-invariant “shocks” to utility. In addition, I make the following assumptions:

- Error components, ϵ_p, ϵ_{ps}, and ϵ_{psj}, are independent for all p, s, and j;
- Composite errors, $\epsilon_p + \epsilon_{ps} + \epsilon_{psj}$, are i.i.d. Gumbel with scale parameter $\lambda > 0$, which I normalize to 1;
- Composite sub-nest errors, $\epsilon_{ps} + \epsilon_{psj}$, are i.i.d. Gumbel with scale parameter $\lambda_p \in [0, 1]$; and
- Idiosyncratic errors, ϵ_{psj}, are i.i.d. Gumbel with scale parameter $\lambda_{ps} \in [0, \lambda_p]$.

Under these assumptions, marginal and conditional choice probabilities are given by

$$Pr(p) = \frac{exp\{V_p + \lambda_p I_p\}}{\sum\limits_{p' \in P} exp\{V_{p'} + \lambda_{p'} I_{p'}\}}, \tag{3}$$

$$Pr(s \in S_p | p) = \frac{exp\{(V_{ps} + \lambda_{ps} I_{ps})/\lambda_p\}}{\sum\limits_{s' \in S_p} exp\{(V_{ps'} + \lambda_{ps'} I_{ps'})/\lambda_p\}}, \quad \text{and} \tag{4}$$

$$Pr(j \in J_{ps} | ps) = \frac{exp\{(V_{psj} + V_{pj})/\lambda_{ps}\}}{\sum\limits_{j' \in J_{ps}} exp\{(V_{psj'} + V_{pj'})/\lambda_{ps}\}}, \tag{5}$$

where the "inclusive value" or "log-sum" for nest p is given by $I_p = \ln \sum_{s \in S_p} exp\{(V_{ps} + \lambda_{ps} I_{ps})/\lambda_p\}$, and the inclusive value for sub-nest ps is given by $I_{ps} = \ln \sum_{j \in J_{ps}} exp\{(V_{psj} + V_{pj})/\lambda_{ps}\}$. Assuming decisions are independent across individuals and time, the likelihood function is given by

$$\mathscr{L}(\boldsymbol{\beta}, \boldsymbol{\lambda}) = \prod_{i \in I} \prod_{t \in T} \prod_{psj \in C_{it}} [Pr_{it}(j \in J_{ps}|ps) * Pr_{it}(s \in S_p|p) * Pr_{it}(p)]^{y_{psjit}}, \tag{6}$$

where $\boldsymbol{\beta}$ denotes the vector of parameters associated with the deterministic component of utility $(\widetilde{V})$, $\boldsymbol{\lambda}$ denotes the vector of scale parameters associated with the random component of utility $(\tilde{\epsilon})$, and $y_{psjit} = 1$ if fisherman i chose alternative $j \in J_{ps}$ on choice occasion t and zero otherwise. Under fairly general conditions, the values of $\boldsymbol{\beta}$ and $\boldsymbol{\lambda}$ that maximize this function are consistent and efficient estimates of the true parameters (Brownstone and Small, 1989).

II.B Two-Dimensional Nested Logit

If decisions to fish for secondary species are unobserved or otherwise ignored, these decisions are classified, at least implicitly, as non-participation. The choice set is reduced to $J + 1$ alternatives, and it becomes redundant to index alternatives by both p and s. Using p to denote the (modified) participation dimension, ($2'$) collapses to

$$U_{pj} = V_p + V_{pj} + \epsilon_p + \epsilon_{pj}. \tag{$2''$}$$

To be consistent with the nested logit model, the error components in ($2''$) are assumed to meet the following conditions:

- Error components, ϵ_p and ϵ_{pj}, are independent for all p and j;
- Composite errors, $\epsilon_p + \epsilon_{pj}$, are i.i.d. Gumbel with scale parameter $\lambda > 0$, which I normalize to 1; and
- Idiosyncratic errors, ϵ_{pj}, are i.i.d. Gumbel with scale parameter $\lambda_p \in [0, 1]$.

Under these assumptions, marginal and conditional nested logit choice probabilities are given by

$$Pr(p) = \frac{exp\{V_p + \lambda_p I_p\}}{\sum\limits_{p' \in P} exp\{V_{p'} + \lambda_{p'} I_{p'}\}} \tag{7}$$

and

$$Pr(j \in J_p|p) = \frac{exp\{V_{pj}/\lambda_p\}}{\sum\limits_{j' \in J_p} exp\{V_{pj'}/\lambda_p\}}, \tag{8}$$

where $I_p = \ln \sum_{j \in J_p} exp\{V_{pj}/\lambda_p\}$. Assuming decisions are independent across individuals and time, the likelihood function is given by

$$\mathscr{L}(\boldsymbol{\beta}, \boldsymbol{\lambda}) = \prod_{i \in I} \prod_{t \in T} \prod_{pj \in C_{it}} [Pr_{it}(j \in J_p|p) * Pr_{it}(p)]^{y_{pjit}}, \tag{9}$$

where $y_{pjit} = 1$ if fisherman i chose alternative $j \in J_p$ on choice occasion t and zero otherwise.

It is important to note that, although more flexible, the three-dimensional model does not nest the two-dimensional model. When decisions to fish for secondary species are classified as non-participation, the data and model are modified significantly. First, the size of the choice set is reduced considerably. This not only affects estimation, but limits the set of policies the researcher is able to analyze to those that affect the primary species only. Second, the characteristics that describe secondary species alternatives are implicitly replaced with the characteristics that describe non-fishery participation. For example, values of location-specific explanatory variables, such as revenues and costs, are set to zero for secondary species. Third, the parameters in ($2''$) are implicitly constrained to be equal for non-participation and stone crab alternatives. For example, variables such as weather conditions are constrained to affect utilities associated with stone crab fishing in the same way that they affect utilities associated with not fishing. Importantly, these issues are not specific to the nested logit model and will present problems in other discrete choice formulations as well. Thus, it is clear that the two-dimensional model is misspecified when individuals are able to target more than one species. The question that remains is whether these restrictions have sizable effects on parameter estimates and policy forecasts.

III Empirical Application

To test the implications of the model misspecification described in the previous section, I estimate the three- and two-dimensional nested logit models described in sections II.A and II.B, respectively, on data from the Florida spiny lobster and stone crab fisheries.

III.A Industry Characteristics

Spiny lobsters (*panulirus argus*) are warm-water clawless lobsters found in the western Atlantic waters from North Carolina to Brazil. Commercial trap fishermen in southern Florida are typically responsible for $60 - 80\%$ of annual U.S. spiny lobster landings. I focus my analysis on this group of fishermen. Many of these fishermen also participate in the Florida stone crab fishery. Like lobsters, stone crabs are abundant in southern Florida, which enables fishermen to target either species from the same port. Because stone crabs are also harvested by trap, fishermen use much of the same capital and labor – e.g. vessel, hydraulic trap puller, and crew – to harvest both species, which enables fishermen to easily switch between fisheries on a daily basis. Still, local habitats are distinct, and fishermen use different traps to harvest each species. So, while fishermen can target either species on a given day, they do not typically target both species on the same day. Although the lobster and stone crab fisheries are subject to a number of regulations, such as size limits, there are no individual, location-specific, or fishery-wide quotas in either fishery. Fishermen are permitted to fish for either species as often as they wish and wherever they wish, provided they hold the appropriate permits, the season is open, and the location is not closed to fishing.[11]

Since 1978, the Florida Fish and Wildlife Conservation Commission (FWC) has required dealers (buyers) to fill out a Marine Fisheries Trip Ticket for each commercial purchase of marine life. Among other things, trip tickets record the dealer's and seller's unique license numbers, the date of the trip, the quantity and unit price of each species sold, and the location of the trip, classified as one of eighteen statistical areas spanning the coast of Florida. The FWC has provided me with all trip ticket records from 1996 through 2007 that record any amount of lobster sold as well as all remaining trip tickets associated with this group of fishermen. These data allow me to observe when fishermen participate, what they catch, and where they fish. Using these data, I determine the primary species targeted by each fisherman on each day, and I extract lobster trap fishermen from the universe of individuals that ever sold lobster. Details on this process are provided in appendix

[11] The lobster fishery is open from August 6 until March 31, and the stone crab fishery is open from October 15 until May 15. I study the decisions made by lobster trap fishermen during the lobster season. Hence, stone crab alternatives are not included in the choice set between August 6 and October 14.

Table 1: Sample Size and Distribution of Effort

	Lobster		Stone Crab	
	Number	Percentage	Number	Percentage
Trips taken				
Area 1	45,852	4.29	15,557	1.46
Area 2	15,992	1.50	1,253	0.12
Area 3	4,788	0.45	8,963	0.84
Area 4	37,840	3.54	4,010	0.38
Area 5	82,326	7.71	23,510	2.20
Total	186,798	17.50	53,293	4.99
Fishermen in sample	840			
Open season days	2,859			
Total choice occasions	1,067,636			

NOTE.— Not all fishermen participate in the lobster fishery every season, so the number of choice occasions is smaller than the product of the number of fishermen in the sample and the number of open season days. See appendix A for details on how the sample is constructed.

A. To reduce computing requirements, I focus my analysis on the five southernmost statistical areas in Florida, which account for roughly 95% of all lobster and stone crab landings, and I drop all trip tickets associated with fishermen ever observed to fish outside these areas. A map of statistical areas is provided in appendix B.

Table 1 describes the sample of commercial lobster trap fishermen and the distribution of effort across species and locations. The sample includes 840 commercial lobster trap fishermen and 2,859 open season days. Commercial lobster trap fishermen participate in the lobster fishery 17.5% of the time and the stone crab fishery 5% of the time. There are large differences in the number of trips made to each location and in the spatial distribution of effort between species. For example, Area 5 is the most visited location in both fisheries, accounting for approximately 44% of all lobster trips and stone crab trips. However, while Area 3 ranks third in total stone crab trips, it ranks last in total lobster trips. This heterogeneity is a source of the interesting differences in policy forecasts between the two- and three-dimensional models that I present below.

Since treatment of the outside option is the focus of this paper, a discussion of its composition in this application is warranted. An obvious concern is whether the outside option includes fishing for species other than lobster and stone crab, which would introduce the type of misspecification that I seek to avoid. Fortunately, I observe every fishing trip made by lobster fishermen and the composition of each trip. All trips can be reasonably classified as lobster or stone crab trips (see appendix A), which strongly suggests that the outside option for these individuals does not include fishing for other species in any meaningful way. Still, the outside option may include other substitute activities, such as recreational fishing or other forms of employment. Unfortunately, I do not have information on non-commercial fishing activities and so cannot test or control for this possibility, which is a caveat of the empirical analysis. Despite this caveat, we can still learn about the importance of the composition of the outside option by comparing models that explicitly include stone crab fishing with models that do not.

III.B Model

To aid discussion, I slightly modify notation in the empirical application. On each open season day, fishermen make a participation decision, $p \in \{no, fish\}$, a species target decision, $s \in \{neither, lobster, crab\}$, and a location decision, $j \in \{0, 1, 2, 3, 4, 5\}$, where *no*, *neither*, and 0 reflect non-participation at each stage of the decision process. In this application, individuals are able to target both species in all locations, so the choice set consists of eleven alternatives.

I assume that the utility that fisherman i receives from selecting alternative psj on day t is a linear function of an alternative-specific constant, alternative-invariant and alternative-specific variables, and a component that is unobserved by the researcher:

$$U_{psjit} = \alpha_{psj} + \mathbf{X_{it}} \boldsymbol{\beta_p} + \mathbf{Z_{psjit}} \gamma + \tilde{\epsilon}_{psjit}. \tag{10}$$

Alternative-invariant variables, $\mathbf{X}$, consist of (i) temporal variables, which include a set of fishing season indicators, a set of month indicators, and indicators for Saturday and Sunday to capture weekend work preferences; and (ii) weather variables, which include indicators for moderate ($15-20$ *miles per hour*) and high wind speeds (20+ *mph*). I allow alternative-invariant variables to affect utilities associated with participation differently than utilities associated with non-participation, and I normalize $\boldsymbol{\beta_{no}}$ to zero. Alternative-specific variables, $\mathbf{Z}$, include (i) location-specific costs, for which I use distance from port (measured in *miles*) as a proxy; and (ii) location- and species-specific expected daily revenue (measured in $\$s/trap$). I set values of $\mathbf{Z}$ to zero for non-participation.

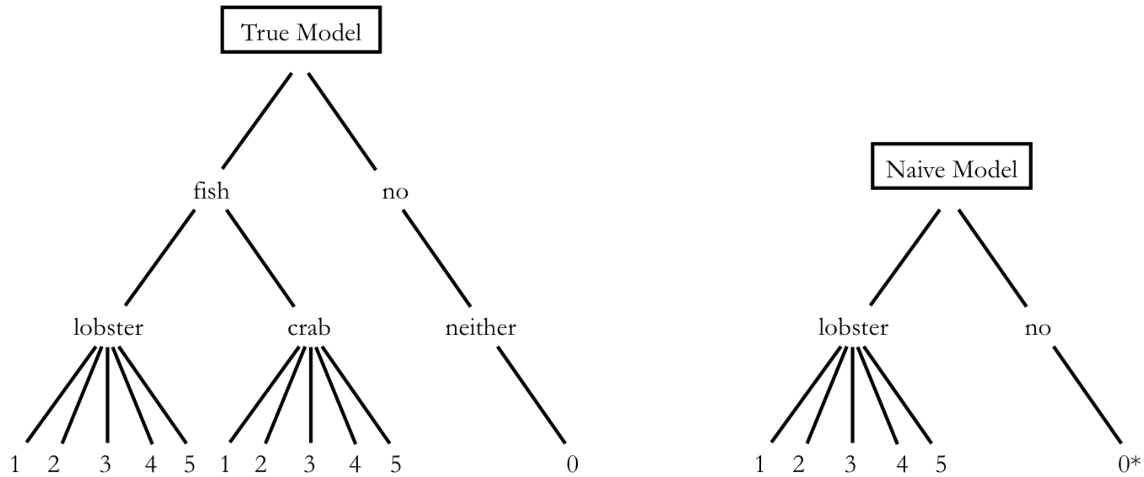

Figure 1: Nested Logit Decision Trees

Details on how weather, distance, and revenue variables are constructed are provided in appendix C. I assume unobserved components, $\tilde{\epsilon}_{psjit}$, meet the requirements laid out in section II.A, and I normalize λ to one. For expositional purposes, I refer to this model as the "true" model. The structure of this model is illustrated in Figure 1.

Table 2 presents summary statistics on key variables. To generate these statistics, I first calculate participation rates for each open season day in the sample. I then take a weighted average over all days sharing the same characteristic, weighting daily values by the number of fishermen participating that day.[12] These statistics illustrate that far fewer fishermen participate in either fishery on windy days and on weekends, particularly Sundays. Visitations to each fishing location are also higher when expected revenues are above average.

I compare the three-dimensional model to one that omits the species target decision. In particular, I classify decisions to fish for stone crab as non-participation, drop stone crab alternatives from the choice set, and collapse the choice structure to two dimensions, $p \in \{no, lobster\}$ and $j \in \{0, 1, 2, 3, 4, 5\}$. Implicitly, this restricts utilities associated with non-participation and utilities associated with stone crab fishing to be equal, which is accomplished by making non-participation

[12]When calculating statistics for the stone crab fishery, I do not include days prior to the opening of the stone crab season, which necessarily have zero participation. As a result, the average participation rate shown in Table 2 (7.08%) is larger than the percentage of choice occasions on which individuals chose to fish for stone crab shown in Table 1 (4.99%).

Table 2: Summary Statistics of Daily Participation Rates

Variable		Lobster Mean (%)	Lobster Standard Deviation	Stone Crab Mean (%)	Stone Crab Standard Deviation
		Panel A: Participation-Specific Variables			
All days		17.50	13.05	7.08	4.49
Weekdays		19.26	13.40	7.76	4.54
Saturdays		15.91	12.00	6.49	3.99
Sundays		9.98	8.64	4.12	3.24
Wind speed, low		21.41	13.20	8.44	4.37
Wind speed, moderate		12.36	9.78	5.92	4.03
Wind speed, high		6.94	6.76	3.84	3.29
		Panel B: Location-Specific Variables			
Area 1:	Revenue, low	3.60	3.10	1.93	1.38
	Revenue, high	5.60	3.95	2.27	1.62
Area 2:	Revenue, low	1.37	1.11	0.11	0.21
	Revenue, high	1.53	1.52	0.20	0.36
Area 3:	Revenue, low	0.43	0.75	1.12	0.96
	Revenue, high	0.48	0.67	1.46	1.04
Area 4:	Revenue, low	2.77	2.78	0.50	0.48
	Revenue, high	4.83	3.61	0.74	0.82
Area 5:	Revenue, low	7.15	5.38	2.85	2.01
	Revenue, high	11.93	6.41	4.15	2.70

and stone crab alternatives observationally equivalent – i.e. setting $\mathbf{Z}$ to zero for stone crab alternatives – and constraining the parameters of (10) to be equal for non-participation and stone crab alternatives. The structure of this "naive" model is also illustrated in Figure 1.

State Dependence

In addition to day t characteristics, fishermen's past choices may influence their present choices. For example, there may be costs to switching between participation and non-participation or between fisheries. To investigate whether state dependence mitigates or exacerbates differences between models, I estimate the following augmeted version of (10)

$$U_{psjit} = \alpha_{psj} + \mathbf{X_{it}}\boldsymbol{\beta_p} + \mathbf{Z_{psjit}}\boldsymbol{\gamma} + \delta_{ps} y_{psit-1} + \tilde{\epsilon}_{psjit}, \tag{10'}$$

where $y_{psit-1} = 1$ if fisherman i chose an alternative in subset J_{ps} on choice occasion $t-1$ and zero otherwise, and δ_{ps} captures the effect of the previous day's choice on utility. Thus, utility depends

Table 3: Estimates of Parameters and Marginal Effects

Variable	True Model Estimate	True Model Standard Error	Naive Model Estimate	Naive Model Standard Error	Percent Difference
	Panel A: Coefficients				
Wind speed, moderate (0,1)	-0.545	0.0005	-0.487	0.0005	
Wind speed, high (0,1)	-1.321	0.0017	-1.269	0.0019	
Saturday (0,1)	-0.280	0.0007	-0.273	0.0008	
Sunday (0,1)	-0.841	0.0009	-0.821	0.0010	
Distance (*miles*)	-0.019	0.0000	-0.010	0.0000	
Revenue (\$/*trap*)	0.012	0.0000	0.001	0.0000	
	Panel B: Marginal Effects				
Wind speed, moderate (0,1)	-7.540	0.3050	-9.058	0.4397	20.14
Wind speed, high (0,1)	-15.726	0.4744	-19.011	0.6631	20.89
Saturday (0,1)	-4.018	0.3827	-5.352	0.5333	33.20
Sunday (0,1)	-11.066	0.3959	-13.980	0.5418	26.33
Distance (*miles*)	-0.585	0.0429	-0.206	0.0141	-64.76
Revenue (\$/*trap*)	0.365	0.0572	0.024	0.0160	-93.30
	Panel C: Dissimilarity Parameters				
λ_{fish}	0.463	0.0033			
$\lambda_{lobster}$	0.359	0.0001	0.189	0.0001	-47.36
λ_{crab}	0.448	0.0002			

NOTE.— I cluster standard errors at the calendar date level, and I calculate standard errors of marginal effects using the delta method.

on the previous day's choice at the species-level (but not the location-level), and the effect of this choice is allowed to differ across species.[13]

III.C RESULTS

Table 3 presents estimates of parameters and marginal effects. The signs of coefficient estimates (Panel A) are the same in both models and are consistent with the summary statistics shown in Table 2. Fishermen are less likely to participate on windy days and on weekends, less likely to choose alternatives that are far from port, and more likely to choose alternatives with greater expected revenue.

To compare magnitudes of effects across models, I present marginal effects in Panel B. I evaluate marginal effects for a weekday in November 1998 with low wind speed (< 15 *mph*), mean alternative-

[13]In addition to this model, I have evaluated a number of alternative specifications of state dependence. Results are qualitatively similar and are available upon request.

specific revenues, and modal location-specific distances.[14] In these calculations, I sum probabilities over all lobster alternatives, so marginal effects report changes in the probability of fishing for lobster in any location. For example, the true model suggests that fishermen are 11% less likely to fish for lobster on Sundays, and the naive model suggests that fishermen are 14% less likely. Marginal effects for alternative-specific variables report changes in probabilities associated with a one-unit increase in the variable across *all* lobster alternatives. For example, the true model suggests that fishermen are 0.6% less likely to fish for lobster when all lobster locations become 1 mile farther from port, and the naive model suggests that fishermen are 0.2% less likely. To aid comparison across models, the last column in Table 3 reports the percent difference in the absolute value of marginal effects across models. These differences are large, particularly for distance and revenue.

Panel C reports estimates of scale parameters, or "dissimilarity parameters", which describe the variances of error components.[15] Formulas for these variances are given in Table 4, where $\lambda_p \in [0,1]$ and $\lambda_{ps} \in [0, \lambda_p]$.[16] In this context, participation is implied when referring to lobster or stone crab alternatives, so for simplicity I refer to λ_{ps} as λ_s when discussing scale parameters of the true model. In both models, $\lambda_{lobster}$ describes the relative importance of idiosyncratic shocks to utilities associated with lobster fishing. As $\lambda_{lobster}$ approaches 1, the variances of error components common to all lobster alternatives approach 0, and the correlation among lobster utilities approaches 0.[17] Conversely, as $\lambda_{lobster}$ approaches 0, the variance of the idiosyncratic component approaches 0, and the correlation among lobster utilities becomes relatively large. Generally, the larger is the correlation, the greater is the substitutability and the propensity to select another lobster alternative when one is removed from the choice set. Although estimates of $\lambda_{lobster}$ are quite small in both

[14]In particular, I set lobster revenues to 4.25, 7.03, 4.43, 4.2, and 3.14 dollars per trap and stone crab revenues to 3.24, 4.66, 2.66, 2.47, and 2.77 dollars per trap for Areas 1, 2, 3, 4, and 5, respectively. I set distances to reflect the distances that an individual from the modal zip code, 33050, must travel to reach each area. These distances are 0, 57, 25, 37, and 3 miles for Areas 1, 2, 3, 4, and 5, respectively.

[15]Scale parameters are identified if the associated decision node contains more than one option. For example, because the decision to not participate reduces the choice set to a single option, in the naive model, we cannot distinguish the variance of the common shock (ϵ_{no}) from the variance of the idiosyncratic shock ($\epsilon_{no,0}$), and λ_{no} is not identified. The same is true of λ_{no} and $\lambda_{neither}$ in the true model.

[16]Kling and Herriges (1995), Herriges and Kling (1996), and Gil-Molto and Hole (2004) provide conditions under which $\lambda_p > 1$ and $\lambda_{ps} > \lambda_p$ are consistent with utility maximizing behavior.

[17]In the extreme case where $\lambda_{lobster} = 1$ in the naive model, only idiosyncratic components remain, and nested logit reduces to multinomial logit. The same is true in the true model when $\lambda_{fish} = \lambda_{lobster} = \lambda_{crab} = 1$.

Table 4: Error Variances

Model	Dimension of Choice		
	Participation (p)	Species Target (s)	Location (j)
Three-Dimensional	$var(\epsilon_p) = \frac{\pi^2}{6}\left[1 - \lambda_p^2\right]$	$var(\epsilon_{ps}) = \frac{\pi^2}{6}\left[\lambda_p^2 - \lambda_{ps}^2\right]$	$var(\epsilon_{psj}) = \frac{\pi^2}{6}\lambda_{ps}^2$
Two-Dimensional	$var(\epsilon_p) = \frac{\pi^2}{6}\left[1 - \lambda_p^2\right]$		$var(\epsilon_{pj}) = \frac{\pi^2}{6}\lambda_p^2$

NOTE.— Formulas for the variances of error components are based on a normalization of λ to 1.

models, the estimate is much smaller in the naive model, suggesting that lobster alternatives are characterized as better substitutes for one another in this model.

Two additional scale parameters are estimated in the true model: λ_{crab}, which has an interpretation analogous to $\lambda_{lobster}$, and λ_{fish}, which describes the relative importance of shocks common to *both* lobster and stone crab alternatives. As λ_{fish} approaches 1, the variance of the error component common to both species approaches 0, and the correlation in utilities *across* species approaches 0. The estimated value of λ_{fish} is quite small, suggesting that shocks common to *all* fishing alternatives are relatively large. Given that the naive model constrains correlation between lobster and stone crab utilities to zero, this result suggests that the naive model may be a particularly poor characterization of the choice environment studied here.

To determine whether differences in estimates produce meaningful differences in policy forecasts, I conduct several simulations. I simulate the effect of closing each of the five fishing locations to the lobster fishery only ("partial" closure) and to both fisheries ("complete" closure). Table 5 presents predicted changes in the number of lobster trips made to each location in response to area closures. Because stone crab alternatives are not explicitly modeled in the naive model, partial and complete closures are indistinguishable. Consequently, I present one set of forecasts for the naive model. When an alternative is removed from the choice set, the probability of selecting each of the remaining alternatives increases. Hence, both models predict an increase in the number of lobster trips made to each non-closed area. However, predicted magnitudes of these responses differ across models. For example, on almost 50,000 occasions, individuals choose to fish for lobster in Area 1. When this area is closed to the lobster fishery only, the true model forecasts that on almost 30,000

Table 5: Marine Reserve Simulations

	Predicted Change in the Number of Lobster Trips					
Simulation	Area 1	Area 2	Area 3	Area 4	Area 5	Total
Close Area 1						
True Model, Partial	-47,434	7,947	1,794	1,592	16,395	-19,706
True Model, Complete	-47,434	9,890	2,221	2,041	20,357	-12,924
Naive Model	-48,123	11,759	2,669	2,149	23,099	-8,446
Close Area 2						
True Model, Partial	6,536	-16,956	629	220	2,735	-6,837
True Model, Complete	6,844	-16,956	659	234	2,885	-6,334
Naive Model	9,200	-17,047	891	298	3,822	-2,836
Close Area 3						
True Model, Partial	1,221	497	-4,929	144	1,306	-1,763
True Model, Complete	2,563	1,057	-4,929	361	2,879	1,931
Naive Model	1,675	677	-5,019	186	1,752	-728
Close Area 4						
True Model, Partial	1,814	222	206	-35,375	15,537	-17,596
True Model, Complete	2,001	247	228	-35,375	17,241	-15,658
Naive Model	2,571	304	296	-34,840	23,601	-8,069
Close Area 5						
True Model, Partial	23,487	3,382	2,431	14,145	-82,194	-38,750
True Model, Complete	28,890	4,163	3,006	18,137	-82,194	-27,999
Naive Model	35,742	4,972	3,737	20,552	-81,769	-16,767
Predicted Trips						
True Model	47,434	16,956	4,929	35,375	82,194	186,889
Naive Model	48,123	17,047	5,019	34,840	81,769	186,798
Observed Trips	45,852	15,992	4,788	37,840	82,326	186,798

of these occasions, individuals will choose to fish for lobster in another location. Hence, closing Area 1 leads to a decrease of almost 20,000 lobster trips or roughly a 42% reduction in overall lobster effort. In contrast, the naive model forecasts a decrease of fewer than 9,000 lobster trips or roughly an 18% reduction in effort. Results are similar in each area closure simulation: the naive model predicts a decline in the number of lobster trips that is $21 - 27$ percentage points smaller than the decline predicted by the true model. These differences are substantial and are due, in large part, to differences in the estimated value of $\lambda_{lobster}$.

As more alternatives are removed from the choice set, the probability of selecting each of the remaining alternatives gets larger. Hence, for the true model, predicted increases in the number of lobster trips made to each non-closed area are larger under complete closures. Differences in predictions from partial and complete closures are particularly striking when Area 3 is closed. When this area is closed to the lobster fishery only, the true model necessarily predicts a decrease in total lobster trips. When this area is closed to both fisheries, however, the true model predicts an overall *increase* in total lobster trips. This reversal in sign is a consequence of the initial distribution of effort across species and locations. Area 3 is the least popular lobster destination and the third most popular stone crab destination (see Table 1). This, combined with the fact that lobster fishing is seen as a better substitute for stone fishing than non-participation ($\hat{\lambda}_{fish} << 1$), means that much of the effort displaced from the stone crab fishery as a result of the area closure is reallocated to lobster fishing in other areas. Because the naive model does not explicitly consider stone crab alternatives, it cannot capture this behavior and is constrained to predict only a decrease in overall lobster effort.

State Dependence

Tables 6 and 7 provide results, analogous to those provided in Tables 3 and 5, from the models with state dependence. I evaluate marginal effects under two scenarios. In Panel B of Table 6, I set the value of initial conditions and states to zero for all alternatives. This scenario approximates a situation in which individuals have no history with alternatives; their decisions depend only on the independent variables on day t. This scenario allows for a direct comparison of the marginal effects of these independent variables with those of the main models, shown in Table 3. In Panel C, I set the value of initial conditions to zero and the value of species-specific states to the sample "average" for each species-level group of alternatives (lobster, stone crab, and neither). I define the sample average for a particular group of alternatives as the fraction of times that species was selected in the entire sample. In this scenario, the marginal effects represent the effects of independent variables for a typical fisherman on a typical day at a point far enough into the season that the effects of initial conditions have dissipated. For brevity, I only report coefficient and marginal effects estimates of $\delta_{lobster}$. Results indicate that there is significant correlation between today's choice and yesterday's choice. The true model with state dependence suggests that fishermen are 10.5% more likely to go lobster fishing if they went lobster fishing yesterday, compared to a situation in which

Table 6: Estimates of Parameters and Marginal Effects of Models with State Dependence

Variable	True Model Estimate	True Model Standard Error	Naive Model Estimate	Naive Model Standard Error	Percent Difference
	Panel A: Coefficients				
Wind speed, moderate (0,1)	-0.506	0.0004	-0.458	0.0005	
Wind speed, high (0,1)	-1.232	0.0015	-1.195	0.0017	
Saturday (0,1)	-0.350	0.0006	-0.331	0.0006	
Sunday (0,1)	-0.877	0.0008	-0.848	0.0009	
Distance (*miles*)	-0.014	0.0000	-0.007	0.0000	
Revenue (\$/*trap*)	0.004	0.0000	-0.000	0.0000	
State Dependence $(0-1)$	0.345	0.0008	0.731	0.6917	
	Panel B: Marginal Effects with State Dependence = 0				
Wind speed, moderate (0,1)	-8.384	0.3376	-8.803	3.5043	5.00
Wind speed, high (0,1)	-18.749	0.5466	-18.833	9.0490	0.45
Saturday (0,1)	-5.839	0.4021	-6.543	2.4890	12.05
Sunday (0,1)	-14.033	0.4600	-14.725	6.5710	4.93
Distance (*miles*)	-0.421	0.0217	-0.147	0.0490	-65.09
Revenue (\$/*trap*)	0.118	0.0302	-0.003	0.0108	*sign change*
State Dependence $(0-1)$	10.521	0.4003	15.526	12.6575	47.57
	Panel C: Marginal Effects with State Dependence > 0				
Wind speed, moderate (0,1)	-6.639	0.2844	-6.828	0.3437	2.84
Wind speed, high (0,1)	-13.360	0.4332	-14.047	0.5120	5.14
Saturday (0,1)	-4.754	0.3306	-5.121	0.3830	7.72
Sunday (0,1)	-10.477	0.3780	-11.158	0.4390	6.50
Distance (*miles*)	-0.300	0.0140	-0.118	0.0105	-60.56
Revenue (\$/*trap*)	0.084	0.0213	-0.003	0.0087	*sign change*
State Dependence $(0-1)$	7.484	0.3913	12.480	14.2026	66.75
	Panel D: Dissimilarity Parameters				
λ_{fish}	0.594	0.0017			
$\lambda_{lobster}$	0.263	0.0001	0.132	0.0001	-49.72
λ_{crab}	0.314	0.0001			

they have no history with any of the alternatives. The estimated effect is much larger in the naive model. Parameter and marginal effects estimates for the remaining variables are qualitatively similar across models with and without state dependence. However, there are some notable differences. First, the effect of revenue in the naive model is now negative (although small and statistically insignificant). Second, differences in the magnitudes of marginal effects of participation-specific variables are smaller. Third, estimated marginal effects of revenue and distance are smaller in magnitude in both models, suggesting that unobservables play a relatively larger role in selection among alternatives within species. Finally, estimates of $\lambda_{lobster}$ and λ_{crab} are smaller in magnitude

Table 7: Marine Reserve Simulations of Models with State Dependence

	Predicted Change in the Number of Lobster Trips					
Simulation	Area 1	Area 2	Area 3	Area 4	Area 5	Total
Close Area 1						
True Model, Patrial	-45,624	9,973	1,739	1,327	19,509	-13,077
True Model, Complete	-45,624	10,809	1,881	1,459	21,125	-10,350
Naive Model	-46,653	12,502	2,595	1,988	24,394	-5,175
Close Area 2						
True Model, Patrial	7,862	-15,631	556	154	2,785	-4,273
True Model, Complete	7,967	-15,631	564	154	2,819	-4,127
Naive Model	9,829	-16,215	852	257	3,793	-1,485
Close Area 3						
True Model, Patrial	1,205	460	-3,995	94	1,267	-969
True Model, Complete	1,660	642	-3,995	142	1,824	271
Naive Model	1,668	641	-4,227	159	1,680	-78
Close Area 4						
True Model, Patrial	1,626	180	161	-33,339	18,725	-12,647
True Model, Complete	1,654	184	166	-33,339	19,306	-12,029
Naive Model	2,439	250	250	-33,472	24,915	-5,618
Close Area 5						
True Model, Patrial	31,276	3,461	2,461	16,642	-82,705	-28,867
True Model, Complete	33,564	3,677	2,641	18,411	-82,705	-24,412
Naive Model	37,980	4,874	3,567	21,201	-80,106	-12,483
Predicted Trips						
True Model	45,624	15,631	3,995	33,339	82,705	181,294
Naive Model	46,653	16,215	4,227	33,472	80,106	180,672
Observed Trips	45,852	15,992	4,788	37,840	82,326	186,798

both models, suggesting that alternatives within species are viewed as closer substitutes.

Policy simulations are shown in Table 7. They display the same pattern of behavior as those of the main models.[18] The naive model predicts much smaller decreases in lobster effort than the true model when Areas 1, 2, 4, and 5 are closed, and the true model predicts an increase in lobster effort when Area 3 is closed to both fisheries, although the magnitude is smaller. A notable difference between the predictions in Tables 5 and 7 is the magnitude of responses to area closures. Overall,

[18]Predictions are made using simulated rather than observed choices to calculate states, which I generate with the following iterative procedure. First, I calculate individual- and alternative-specific choice probabilities for the first day of each lobster season. I use a uniform(0,1) random number generator to select an alternative for each individual based on the model's predicted choice probabilities. Using this simulated choice, I construct individual- and alternative-specific states for the second day of each lobster season. Using these state variables, I calculate individual- and alternative-specific choice probabilities for the second day. I continue in this fashion until choice probabilities have been calculated for each day in each season. I repeat this exercise 100 times for each model (and each policy simulation) and report average predictions.

there is much higher substitution among lobster alternatives such that the impact of area closures on total lobster effort is, on average, 30% smaller for the true model and 44% smaller for the naive model. These results suggest that state dependence is an important dimension of the choice problem that should be carefully considered. Yet, the inclusion of state dependence does not affect the main conclusion that parameter estimates and policy predictions for the lobster fishery depend heavily upon whether stone crab alternatives are combined with fishermen's outside option.

Tables 5 and 7 illustrate that "true" and "naive" models lead to very different policy forecasts when applied to the Florida spiny lobster and stone crab fisheries. The naive model substantially under-predicts the decline in total lobster trips taken when areas are closed to the lobster fishery only, and when areas are closed to both fisheries, predicted changes in total lobster trips can differ on sign. These results demonstrate that the model misspecification addressed in this paper is of practical importance in the Florida spiny lobster fishery. An important unanswered question, however, is whether one should expect similar biases in other settings. I address this question next.

IV Monte Carlo Experiments

To evaluate the sensitivity of results to the empirical environment, I conduct a number of Monte Carlo experiments. In each experiment, I generate data on attributes and choices that are consistent with the three-dimensional nested logit model described in section II.A. Next, I estimate the misspecified two-dimensional nested logit described in section II.B on the generated data. Lastly, I simulate the effect of area closures and compare predictions of the misspecified model with predictions of the true model. In these experiments, I vary aspects of the empirical environment and evaluate the extent to which these variations influence the results. This exercise demonstrates that it is difficult to make general statements about the effect of model misspecification on estimates and policy forecasts. Under some conditions, the misspecified model generates predictions that differ only slightly from the true model. Under other conditions, differences in predictions are extremely large. Furthermore, the empirical setting affects whether the misspecified model over- or under-predicts policy responses. In this section, I discuss and present results from a subset of these experiments that illustrate these findings.

In these experiments, I consider a simplified version of the empirical model described in section III.B. On each choice occasion, individuals select one of five alternatives: fish for lobster in location 1 or 2; fish for stone crab in location 1 or 2; or fish for neither species, which I label non-participation. Decisions are independent across individuals and time. Indexing alternatives by participation (p), species (s), and location (j), the utility that individual i receives from selecting alternative psj on choice occasion t is a function of an alternative-specific constant, wind speed, alternative-specific revenue, and an alternative-specific component that is unobserved by the researcher:

$$U_{psjit} = \alpha_{psj} + W_{it}\beta_p^W + R_{psjit}\beta^R + \tilde{\epsilon}_{psjit}. \tag{11}$$

Following the empirical application in section III, I allow wind speed to affect utilities associated with participation differently than utilities associated with non-participation, and I normalize β_{no}^W to 0. Unobserved components, $\tilde{\epsilon}_{psjit}$, meet the requirements laid out in section II.A, and I normalize λ to 1. In each experiment, I set values for $\boldsymbol{\alpha}$, $\boldsymbol{\beta}$, and $\boldsymbol{\lambda}$ and draw 10,000 values of W and R_{psj}. This process is discussed in detail below. For each draw, I calculate choice probabilities according to (3), (4), and (5) and select an alternative using draws from a uniform(0,1) random number generator. I estimate the misspecified two-dimensional model, (9), on the generated data of choices and attributes and replicate each experiment 500 times.

In the experiments presented here, I vary the data generating process to evaluate the sensitivity of the results to three features of the empirical environment. The first feature that I study is the relative importance of the stone crab fishery. Presumably, the model misspecification issue studied here becomes less problematic as the frequency with which lobster fishermen participate in the stone crab fishery decreases. The second feature that I study is the degree of spatial heterogeneity across species. In particular, I vary the degree to which lobsters are favored in one location and stone crabs in the other. This type of heterogeneity appears to be a key driver in forecast differences in the empirical application. The third feature that I study is the degree of correlation in revenues among fishing alternatives. Presumably, as fishing utilities become more correlated, misclassifying stone crab trips as non-participation becomes more problematic. Analysis of the first and second feature requires varying $\boldsymbol{\alpha}$, and analysis of the third requires varying the data generating process for R_{psj}. The remaining parameters and data generating processes are held fixed throughout all experiments. In particular, I set β^W_{fish} to -0.1 and β^R to 0.1, I set $\lambda_{fish} = \lambda_{lobster} = \lambda_{crab} = 0.5$, and, using the empirical application as guidance, I draw values of W_{it} independently from a Weibull$(2.1, 0.15)$ distribution.

Given realizations of W and R_{psj} and chosen values of $\boldsymbol{\beta}$ and $\boldsymbol{\lambda}$, the values assigned to alternative-specific constants, α_{psj}, determine the distribution of choices across alternatives, which in turn determine the relative importance of the stone crab fishery and the degree of spatial heterogeneity. Let $N_{lobster}$, N_{crab}, and $N_{neither}$ denote the fraction of times individuals choose to fish for lobster, fish for stone crab, and fish for neither species, respectively. Similarly, let N_{sj} denote the fraction of times individuals choose to fish for species s in location j. In the baseline specification, I choose alternative-specific constants such that $N_{neither} = 0.5$ and $N_{lobster1} = N_{lobster2} = N_{crab1} = N_{crab2} = 0.125$.[19] To evaluate the relative importance of the stone crab fishery, in one set of simulations I vary the values of alternative-specific constants such that $N_{lobster1}$ and $N_{lobster2}$ remain fixed at baseline levels while $N_{neither}$ and N_{crab} vary, subject to $N_{crab1} = N_{crab2}$, so as not to introduce spatial heterogeneity at this stage. At one extreme, utilities associated with stone crab fishing are so low that individuals rarely select these alternatives ($N_{neither} \approx 0.75$ and $N_{crab} \approx 0$). At the other extreme, utilities are so large that individuals rarely choose non-participation ($N_{neither} \approx 0$ and $N_{crab} \approx 0.75$).

To evaluate the effect of spatial heterogeneity, in a second set of simulations I vary the values of alternative-specific constants such that $N_{lobster}$, N_{crab}, and $N_{neither}$ remain fixed at baseline levels while each N_{sj} varies, subject to $N_{lobster1} = N_{crab2}$ and $N_{lobster2} = N_{crab1}$. At one extreme, individuals rarely fish for lobster in location 1 and rarely fish for stone crab in location 2 ($N_{lobster1} = N_{crab2} \approx 0$ and $N_{lobster2} = N_{crab1} \approx 0.25$). At the other, individuals rarely fish for lobster in location 2 and rarely fish for stone crab in location 1 ($N_{lobster1} = N_{crab2} \approx 0.25$ and $N_{lobster2} = N_{crab1} \approx 0$). Table 8 provides the ranges of choice outcomes that are considered in each set of simulations.

For both sets of simulations described above, I consider two data generating processes for

[19] To achieve this distribution, I set $\alpha_0 = 0.53475$ and all other alternative-specific constants to 0.

Table 8: Monte Carlo Experiments – Distributions of Choice Outcomes Considered

	Base Case	Simulation 1		Simulation 2	
Alternative	Range	Restriction	Range	Restriction	Range
neither	.500	$.75 - crab$	$0 - .750$	$-fixed-$	.500
lobster	.250	$-fixed-$	.250	$-fixed-$	.250
*lobster*1	.125	$-fixed-$	.125	$.25 - lobster2$	$0 - .250$
*lobster*2	.125	$-fixed-$	.125	$.25 - lobster1$	$0 - .250$
crab	.250	$.75 - neither$	$0 - .750$	$-fixed-$	.250
*crab*1	.125	$crab/2$	$0 - .375$	$lobster2$	$0 - .250$
*crab*2	.125	$crab/2$	$0 - .375$	$lobster1$	$0 - .250$

NOTE.— In the base case, values for alternative-specific constants are chosen such that individuals choose neither species 50% of the time, lobster fishing 25% of the time, stone crab fishing 25% of the time, and trips are divided equally between Areas 1 and 2. In Simulation 1, I examine the importance of the secondary species by varying the distribution of choice outcomes between *neither* and *crab*, while holding *lobster* trips and the number of trips between areas equal. In Simulation 2, I examine the role of spatial heterogeneity by varying the distribution of choice outcomes between Areas 1 and 2.

revenues. In the first, I draw values of R_{psjit} for each fishing alternative independently from a Gamma$(4, 1)$ distribution. In the second, I draw five vectors of revenues. The first four are i.i.d. Gamma$(1, 1)$, and the fifth is i.i.d. Gamma$(3, 1)$. I add the fifth vector to each of the first four, generating four Gamma(4,1) vectors with a correlation of 0.75. In all specifications, revenues for non-participation are set to zero.

Figure 2 presents estimates of $\lambda_{lobster}$, β^R, and β^W_{fish}. True values are illustrated by solid lines and estimates by dashed lines. The importance of stone crab fishing to the choice environment (left column) has a large influence on the extent to which estimates from the misspecified model differ from true values. When utilities from stone crab fishing are very low such that few stone crab trips are made, few choice occasions are misclassified by the two-dimensional model, which minimizes the effect of misspecification and the difference between estimates and true values. When stone crab utilities become large, however, the number of misclassified choice occasions grows and so does the difference between estimates and true values. Heterogeneity across space (right column), on the other hand, has little influence on the extent to which estimates differ from true values. This is, perhaps, not surprising given that the number of misclassified choice occasions and the level of correlation between lobster and stone crab alternatives does not vary with this type of spatial heterogeneity.

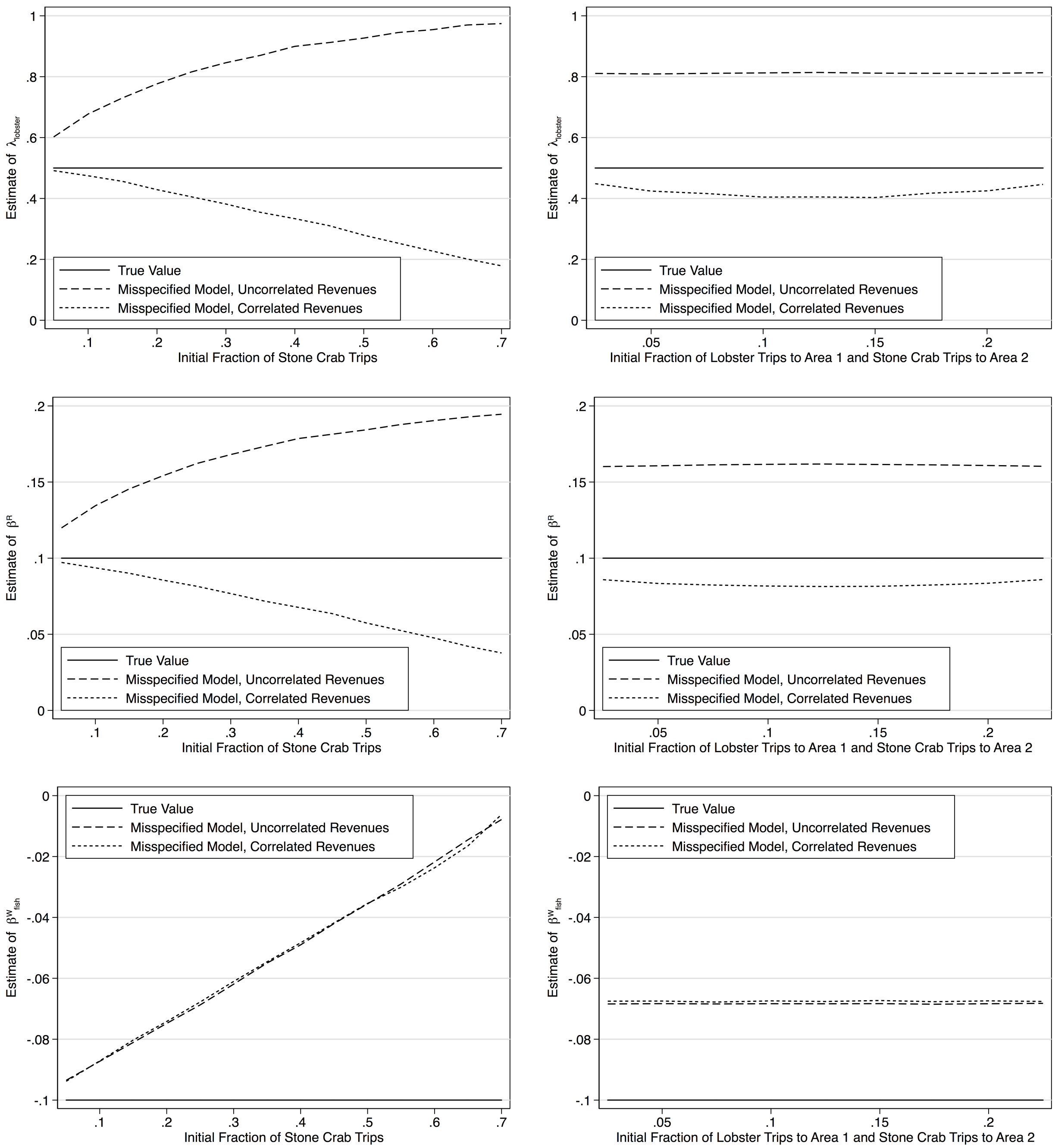

Figure 2: Monte Carlo Experiments – Parameter Estimates

Note.— Results from Simulation 1 are presented in the left column, and results from Simulation 2 are presented in the right column. I replicate each experiment 500 times and present mean results here. Solid lines illustrate true parameter values, long-dashed lines illustrate estimates from the misspecified model when revenues are uncorrelated, and short-dashed lines illustrate estimates when revenues are correlated.

When revenues are correlated (short-dashed lines), the misspecified model under-estimates $\lambda_{lobster}$, β^R, and the marginal effect of revenue (not shown). The reason for this is as follows. When revenues share a common component, on some occasions, revenues are high for all fishing alternatives, and individuals choose to fish for stone crab. In the misspecified model, these choices are classified as non-participation, making it appear as if the effect of revenues on utility is smaller than it actually is. This dampened effect of revenues applies to choice *among* lobster alternatives as well. To reconcile why individuals select the high-revenue lobster alternative as often as observed, the misspecified model apportions more of unobserved utility to the common component and less to the idiosyncratic component. Hence, $\lambda_{lobster}$ and β^R are under-estimated. The larger is the correlation in revenues across species, the larger will be the bias in estimates. In the empirical application, lobster and stone crab revenues are highly correlated, which may explain why $\lambda_{lobster}$ and β^R are under-estimated in that setting as well.

Figure 3 presents predicted percent changes in the number of lobster trips taken as a consequence of closing Area 2. What is clear from this figure is that the relationship between actual and predicted changes in lobster trips is heavily dependent on the empirical environment. For example, when revenues are uncorrelated (top row) and Area 2 is closed only to the lobster fishery (short-dashed lines), the misspecified model (solid lines) does a reasonably good job predicting the change in lobster trips, regardless of the initial percentage of stone crab trips (left) or the initial distribution of effort across locations (right). When Area 2 is closed to both fisheries (long-dashed lines), however, the misspecified model does poorly, particularly when the total number of stone crab trips or the number of stone crab trips taken to Area 2 is initially large. When revenues are correlated (bottom row), results change significantly. Interestingly, when the percentage of lobster trips to Area 1 and the percentage of stone crab trips to Area 2 are relatively low, the misspecified model under-predicts the decline in lobster trips. This mirrors the results in the empirical application when Areas 1, 2, 4, and 5 – areas that are more popular for lobster fishing than for stone crab fishing – are closed (see Table 5). However, when the percentage of lobster trips to Area 1 and the percentage of stone crab trips to Area 2 are relatively high, the misspecified model over-predicts the decline in lobster trips

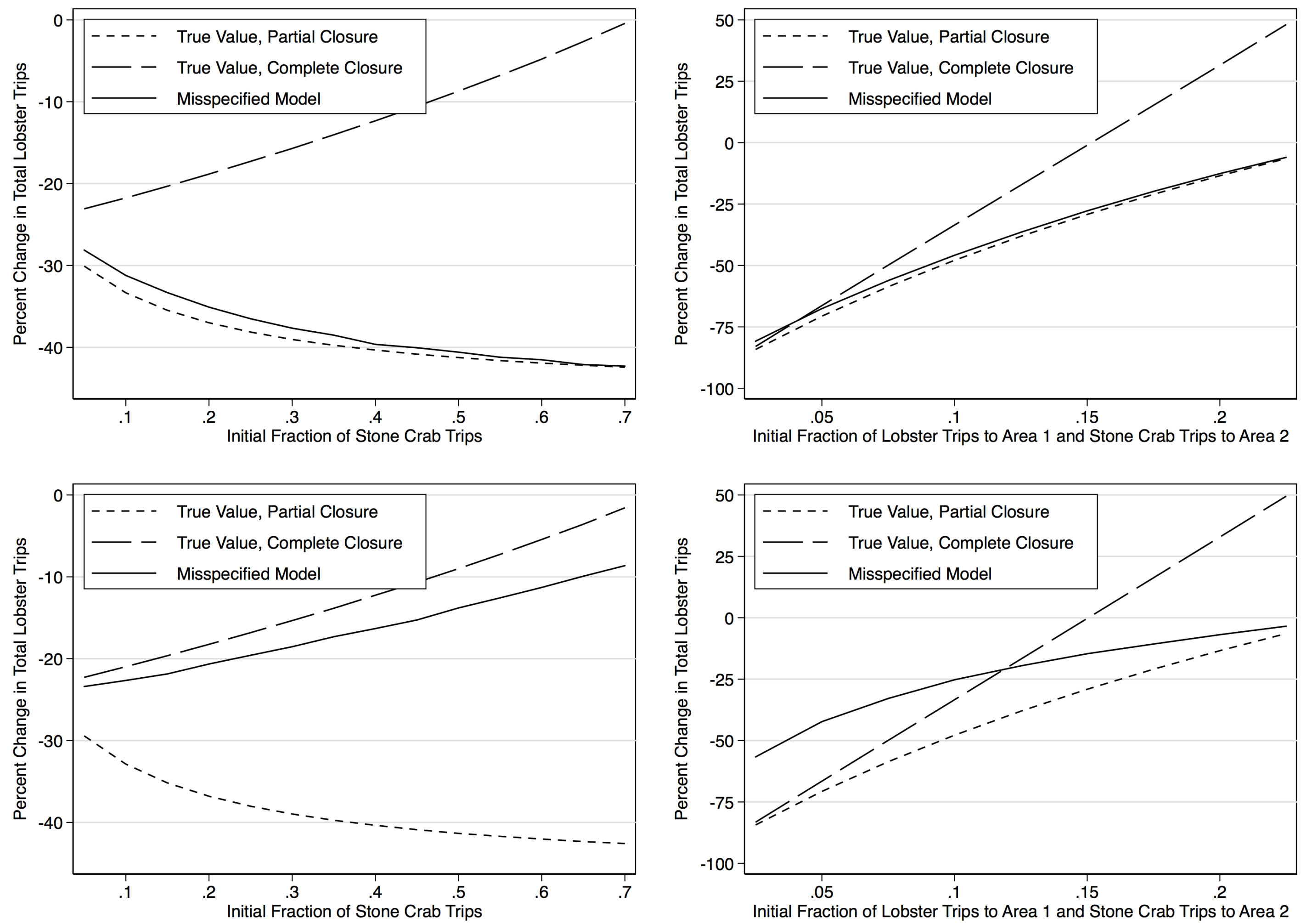

Figure 3: Monte Carlo Experiments – Area Closure Simulations

NOTE.— Results from Simulation 1 are presented in the left column, and results from Simulation 2 are presented in the right column. The top row presents results when revenues are uncorrelated, and the bottom row presents results when revenues are correlated. I replicate each experiment 500 times and present mean results here. Short-dashed lines illustrate the percent change in the total number of lobster trips made as a consequence of closing Area 2 to the lobster fishery only, long-dashed lines illustrate the change in trips when Area 2 is closed to both fisheries, and solid lines illustrate predictions from the misspecified model.

when Area 2 is closed to both fisheries. This mirrors the results in the empirical application when Area 3 – an area that is more popular for stone crab fishing than for lobster fishing – is closed.

In summary, Figures 2 and 3 demonstrate that both the direction and magnitude of the bias in estimates and predictions are heavily dependent on the empirical environment. While many of the results discussed here are intuitive, relationships between characteristics of the empirical environment – such as the level of spatial heterogeneity and correlation in revenues – are complex, and it is not straight-forward to predict how combinations of characteristics will affect estimates and predictions.

V Discussion

In this paper, I examine how pooling secondary species alternatives with non-fishery participation affects estimates and policy predictions of discrete choice models of participation and location decisions in a single commercial fishery. Both an empirical application and Monte Carlo experiments demonstrate that biases in estimates and predictions can be large. Moreover, the experiments demonstrate that both the direction and magnitude of biases are heavily dependent on the empirical environment. In some cases, the direction and magnitude of biases are relatively intuitive and predictable. For example, biases are relatively small when secondary species play a minor role in individuals' daily decisions and when secondary species share little in common with the primary species. Nevertheless, even in these cases, biases exist, and forecasting directions and magnitudes of biases becomes challenging as different aspects of the empirical environment – such as spatial heterogeneity – interact with these biases in nontrivial ways. As a result, it is not possible to make general statements about the signs of biases; under some conditions, the misspecified model over-predicts marginal effects and policy responses, and in others it under-predicts them. The evidence from this paper suggests that researchers should explicitly model as many alternatives that are relevant to the alternatives of interest as possible and proceed with caution in determining what can be safely relegated to a generic outside option.

Regarding the empirical application, I demonstrate that modeling the species target decision is quantitatively important in understanding how the growing use of spatial policies to manage fisheries will affect fishing pressure and the distribution of effort across space. Marine protected areas (MPAs), which typically restrict or prohibit commercial fishing, are an ever-growing tool for ecosystem management. An executive order signed by President Clinton in 2000 called for "strengthening and expanding the Nation's system of marine protected areas" (Executive Order 13158), and, according to the U.S. National Oceanic and Atmospheric Administration (NOAA), MPAs now cover more than 41% of U.S. marine waters. Supporters advocate that MPAs have the potential to improve ecosystems and even fisheries by creating safe havens for exploited areas to recover and repopulate non-protected areas. However, these favorable forecasts are often based on models that make unrealistic assumptions about the behavior of fishermen, and a number of studies have shown that benefits from MPAs decrease once assumptions about behavior are made more realistic.[20] This study adds to that literature.

A particularly striking finding is that, in some cases, closing an area to all fisheries can result in an *increase* in overall lobster fishing effort. Such a result is precluded from a model that includes fishing for secondary species in the outside option. When an area is closed only to lobster fishing, failing to model the species target decision results in significantly under-predicting the decrease in lobster fishing effort. Together, these results suggest that researchers and policy-makers should explicitly consider species choice when considering fisheries management policies, even when their primary interest lies with one particular species. This is particularly true in the present context. The Florida spiny lobster and stone crab fisheries are not subject to annual catch limits so no safety net exists to protect these fisheries from overfishing should the regulator miscalculate the behavioral response of fishermen.

Although not studied here, the species target decision is also important for understanding how regulatory policies will affect by-catch. In a retrospective study, Abbott and Haynie (2012) shows that the creation of two MPAs in the U.S. Eastern Bering Sea that were designed to reduce the by-catch of red king crab had the unintended consequence of increasing the by-catch of halibut as fishermen switched target species in response to the area closures. Had policy makers modeled

[20]See, e.g., Smith and Wilen (2003).

the species target decision, this switching behavior may have been foreseen and this unintended consequence avoided. Modeling the species target decision will become more important as climate change affects the distribution of species across space and fishermen adapt targeting behavior in response.

Although this paper focuses on commercial fisheries, the type of model misspecification studied here is not limited to this application. Closely related are models of recreational fishing effort. For example, Morey, Rowe and Watson (1993), Morey and Waldman (1998), and Abbott and Fenichel (2013) study participation and location decisions of recreational fishermen in the New England Atlantic salmon fishery, the Montana trout fishery, and the Great Lakes trout and salmon fisheries, respectively. A number of species are abundant in these waters, including bass, burbot, perch, pike, salmon, trout, walleye, and whitefish. Thus, it is quite conceivable that many of the individuals studied in these papers face a species target choice similar to that studied in this paper.

Another closely related branch of literature is the valuation of environmental resources using estimates of outdoor recreation demand, such as Lew and Larson (2008) and Kuriyama, Hanemann and Hilger (2010) for beaches and Egan et al. (2009) and von Haefen (2003) for lakes. In such contexts, as with the species choice in fisheries, there are likely to be several alternatives (for example, visiting a water park) that are both more closely substitutable with recreation demand at these sites than other outside options and whose demand is a function of similar variables, such as travel costs and weather.

Many other examples exist across the social sciences. For example, in their well-known analysis of the U.S. market for new automobiles, Berry, Levinsohn and Pakes (1995) note that individuals' outside option, as characterized in their model, is often simply purchasing a used car. In a study on doctors' choice among stent products to treat coronary artery disease, Grennan (2013) combines no treatment with all non-stent treatments, such as coronary artery bypass surgery, to form the outside option. Other examples can be found in health (e.g. Chernew et al. (2004) on insurance choice and Bonnet and Requillart (2011) on soft drink consumption), in energy (e.g. Allcott and Wozny (2014) and Huse and Lucinda (2014) on automobile purchases), and in housing (e.g. Bayer et al. (2016)

on neighborhood choice and Geyer and Sieg (2013) on public housing communities). In each case, the outside option includes alternatives of varying substitutability with the alternatives of interest. Evaluating the practical importance of relegating related alternatives to a generic outside option in other applications and further exploring these issues theoretically are potentially fruitful and useful avenues for further research.

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