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Dispersion-induced status quo bias in pivot-designed choice experiment

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ABSTRACT

Two drivers of the status quo bias are explored via a split sample survey with a pivot-designed Discrete Choice Experiment interviewing recreational fishers in Italy. The propensity to favor the status quo over the hypothetical alternatives given an individual reference point, which we call "dispersion" is examined and modelled accordingly. The framing of the status quo alternative, as a driver of bias, is explored as reference point or opt-out option. Our findings demonstrate that when the status quo is characterized as an individual-specific reference, the status quo bias increases with the magnitude of the dispersion, leading to potentially biased willingness-to-pay estimates. Conversely, when the status quo is characterized as an opt-out option, the effect of the dispersion diminishes, improving the robustness of the estimates. Our results underscore the importance of carefully designing choice scenarios to manage status quo bias. In order to improve the accuracy of welfare estimates, a novel random utility model that tests and accounts for the effects of dispersion is proposed. The performance of this model is numerically tested in a series of Monte Carlo experiments.

1. Introduction

One of the main challenges in using stated preference methods is defining realistic choice set scenarios. In discrete choice experiments (DCEs), a common strategy is to include a characterization of the existing status quo (SQ) as one of the alternatives in the choice card. Two strategies summarize the general approaches: (i) including a SQ that is the same across individuals, as one of the alternatives,[1] or (ii) using an alternative, already experienced or chosen by the respondent, known as the reference point and normally individual-specific as the opt-out alternative. Both approaches generally frame choice card alternatives around SQ real-world alternatives (Haghani et al., 2021; Hensher, 2010).

Using a fixed SQ alternative representing a current scenario is common practice, especially when defining an individual-specific reference point is challenging or unfeasible. This is often the case in environmental economics, where DCEs are usually designed to assess the welfare implications of public policy changes that inherently involve hypothetical alternatives (Mariel et al., 2021). On the other hand, referencing or pivoting alternatives around an individual-specific situation is an effective way to enhance the realism when

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[1] Such current situation could be also defined as "choosing not to choose" remaining in the same situation as before the choice. In this case the SQ alternative is often referred to as "opt-out alternative".

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an experienced alternative does exist (Hensher, 2010). This approach is especially relevant in transportation research, particularly in studies of travel mode choice (e.g., Guevara and Hess, 2019; Rose et al., 2008; Train, 2001).

However, the inclusion of a SQ alternative has been observed to push respondents in favor of such an option, introducing the "SQ effect" or "SQ bias" (e.g., Scarpa et al., 2007; Hess and Rose, 2009; Train, 2001).[2] This behavioral tendency is well documented and aligns with broader principles of prospect theory (Kahneman and Tversky, 2013) and is rooted in the idea that people naturally prefer what is familiar or already experienced compared to unknown alternatives. This effect could potentially bias welfare estimates if not appropriately addressed (Hess and Rose, 2009).

In this paper, we conduct a split sample survey where a pivot-designed DCE permits to examine two potential SQ biases. First, we focus on the effect of what we call choice card *dispersion* from the reference point. This dispersion is defined as the average variation between the attribute levels of the individual-specific reference point and those of the hypothetical alternatives. The dispersion indicates, on average, the magnitude of differences between the attribute levels of hypothetical alternatives and those of the individual-specific SQ alternative. Oehlmann et al. (2017) report that as this dispersion increases the likelihood of choosing the SQ alternative rises. Intuitively, the greater is the dispersion less credible the respondent may find hypothetical alternatives, leading to a higher probability of selecting the SQ alternative. We then examine the framing of the SQ alternative either as individual-specific reference alternative or as an opt-out option. These two SQ biases can impact willingness to pay estimates and therefore a novel random utility model is proposed to mitigate them.

Our case study relies on primary data collected in Italy during 2023 to investigate recreational fishers' preferences for different fishing trips for the bluefin tuna. Each hypothetical fishing trip was constructed using variations from an individual specific reference point, that was directly stated by the respondent. Respondents were given 12 choices between alternative fishing trips, split into two groups of six. The only difference between groups was how the SQ alternative was characterized: either the SQ was described as "your usual trip" with individual-specific attribute levels, or as "not to go fishing" option.

In our empirical application, we find evidence of dispersion-induced SQ bias only when the SQ alternative is characterized as an individual-specific reference point, while such additional effect disappears when the SQ is framed as an opt-out. At the same time, willingness to pay estimates appear more stable across different model specifications when a "not to go fishing" option is available. To appreciate the noise, introduced by SQ biases, we conduct a Monte Carlo simulation to compare different models' performance when a "dispersion-induced SQ effect" is present in the data generation process. We demonstrate that individual-specific reference SQ alternative induces a systematic bias which can be modelled via our novel approach.

The contribution that we bring is then twofold. First, we highlight that when designing DCE cards if hypothetical alternatives are too dispersed (different) compared to the individual's reference point estimates suffer from severe SQ bias. In this sense, we confirm the findings in Oehlmann et al. (2017). However, unlike previous studies, where econometric approaches for identifying and addressing the SQ bias were developed under more general conditions (Hess et al., 2008; Hess and Rose, 2009; Scarpa et al., 2005, 2007), we propose a novel econometric specification to mitigate this bias. Second, by comparing different characterizations of the SQ alternative, we show that using an opt-out rather than an individual-specific reference point may reduce the dispersion-induced bias. This way, we also contribute to the growing body of research on the behavioral drivers of SQ bias (Boxall et al., 2009; Ceren Ahi et al., 2023; Penn and Hu, 2021; Rolfe and Windle, 2012; Adamowicz et al., 2011; Scott and Witt, 2020).

The rest of the paper is organized as follows. In the next section, we first review the literature on the SQ bias in DCEs. In the third section, we present the econometric framework, treating classical ways to account for SQ bias and introducing our extension to consider explicitly the dispersion in the choice card. In the fourth section, we describe the empirical application and its results. Subsequently, we conduct a Monte Carlo simulation to compare the different models analyzed. In the last section we discussed the findings, suggesting potential strategies to address dispersion-induced SQ bias.

2. The SQ effect

The first use of the term SQ-effect can be traced back to the seminal paper of Samuelson and Zeckhauser (1988), who defined it as the tendency to "disproportionately stick with the status quo" that is, "doing nothing or maintaining one's current or previous decision". Subsequent studies across disciplines, including economics, psychology, and decision theory, have tried to explain this inclination (Beshears et al., 2008; Thaler and Sunstein, 2009; Dhar and Simonson, 2003; Tversky and Shafir, 1992; Hartman et al., 1991; Kahneman et al., 1991). These works have identified factors contributing to the SQ effect, such as loss aversion (Kahneman et al., 1991) the inclination to favor inaction over action (Ritov and Baron, 1994) uncertainty about one's own preferences (Dhar and Simonson, 2003) and inherent complexities in decision-making processes (Beshears et al., 2008; Thaler and Sunstein, 2009). These tendencies have been substantiated through choice experiments, reinforcing the notion that the SQ bias is a pervasive aspect of human decision-making across a range of contexts (Oehlmann et al., 2017; Hess and Rose, 2009; Scarpa et al., 2005).

However, in DCE, this source of bias must be reconciled with the necessity of making the hypothetical scenario as credible as possible (Mariel et al., 2021). Simply excluding this type of alternative from the choice set would undermine the incentive compatibility of the DCE (Penn and Hu, 2021). Consequently, two research strands have emerged: one focuses on developing suitable econometric tools to address it (Cantillo et al., 2007; Hess et al., 2008; Scarpa et al., 2005, 2007), while the other seeks to understand its behavioral sources to tackle it in the DCE design (Boxall et al., 2009; Ceren Ahi et al., 2023; Oehlmann et al., 2017; Penn and Hu,

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[2] In the remainder of the paper, we use the expressions SQ effect and SQ bias interchangeably.

2021; Rolfe and Windle, 2012; Adamowicz et al., 2011; Scott and Witt, 2020).

Within the first strand, DCE data is typically analyzed in the Random Utility Maximization (RUM) framework. Typically, to account for SQ bias, researchers specify an alternative specific constant (ASC) for the SQ or the alternatives, aiming to account for the effect and produce unbiased estimates. However, despite these adjustments, SQ bias remains potentially problematic, as for example it could shape different substitution patterns between SQ and hypothetical alternatives or introduce a pattern of serial correlation among repeated choices of the same individual (Train, 2001). This necessitates the utilization of more advanced models, such as nested logit or error component logit (Walker, 2001), offering greater flexibility in accommodating distinct substitution patterns or correlation along repeated choices (refer to Hess and Rose, 2009 for a comprehensive overview). The error component logit model has shown better performance in addressing econometric challenges arising from SQ bias (Scarpa et al., 2005, 2007), yet additional aspects are not addressed. Cantillo et al. (2007) propose a model to account for potential inertial mechanisms of respondents toward the SQ alternative, demonstrating how choosing the SQ at time *t* increases the probability of repeating the same choice at time *t+1* in a nonlinear fashion, thereby introducing a bias in the parameters of interest. Hess et al. (2008) explore asymmetrical evaluations of positive and negative variations from an individual-specific reference alternative. They present a model that separately estimates parameters for these variations in a travel mode choice application, showing its superior fit compared to traditional models.

Within the second strand, the research has focused on understanding which aspects of the DCE design could mitigate or exacerbate SQ bias. The likelihood of opting for the SQ alternative has been observed to be positively influenced by factors such as the choice complexity (Adamowicz et al., 2011; Boxall et al., 2009) or the number of alternatives (Rolfe and Windle, 2012). Other aspects of the choice architecture have been found to have an influence on the SQ bias, such as the number of attributes for the alternatives (Ceren Ahi et al., 2023) or setting the SQ as a default option (Penn and Hu, 2021). An important contribution shedding light on the impact of experimental design aspects on SQ/reference bias is the work of Oehlmann et al. (2017). They conduct a DCE using multiple split samples with experimental variations in the design, in order to investigate how different design features influence the SQ effect. They demonstrate how the SQ bias is alleviated by a greater number of available hypothetical alternatives and exacerbated by an increased number of repeated choices and greater variability (dispersion) in hypothetical alternatives with respect to the SQ. Building on this last aspect, we note that this relationship between levels of variation from the SQ and the magnitude of the SQ bias can pose challenges for accurate parameter estimation. In the next section, we show how and when.

3. Econometric framework

The starting point of our methodological framework is the random utility model, upon which the utility of the choice maker *n* for the alternative *i* can be decomposed into a deterministic and observable component and a random and unobservable component. Formally:

$$U_{ni} = V_{ni} + \varepsilon_{ni} \qquad \text{Eq. (1)}$$

The indirect utility V_{ni} is assumed to be completely determined by the observable attribute $\boldsymbol{X}_i$ of the alternative *i*. In most applications, including this one, V_{ni} is also a linear function of the observables, so that:

$$V_{ni} = \boldsymbol{\beta X_i} \qquad \text{Eq. (2)}$$

Where $\boldsymbol{\beta}$ is the vector of parameters to be estimated. The random error ε_{ni} is assumed to follow the classical Gumbel distribution with scale σ so that the choice probability gets the convenient logit form.

$$P_{ni} = \frac{\exp\left(\boldsymbol{\beta X_i}/_{\sigma}\right)}{\sum_{j=1}^{J} \exp\left(\boldsymbol{\beta X_j}/_{\sigma}\right)} \qquad \text{Eq. (3)}$$

Equation (2) represents the basic form of indirect utility, and our modeling framework starts from there. For simplicity and consistency with most DCE applications, we consider a setting with three alternatives, one of which is the SQ alternative. In pivot-designed DCE, the hypothetical alternatives are built using variations from attributes' levels of an individual specific reference point that generally serves as the SQ alternative itself. We formalize the SQ effect simply as an additional fixed effect in the utility of the SQ alternative. This fixed effect can easily be modelled using an alternative specific constant (ASC). The utilities of the three alternatives can be written as:

$$U_{n,1} = \boldsymbol{\beta}\left(\boldsymbol{X_{ref}} \cdot \boldsymbol{Var_1}\right) + \varepsilon_{n1}$$

$$U_{n,2} = \boldsymbol{\beta}\left(\boldsymbol{X_{ref}} \cdot \boldsymbol{Var_2}\right) + \varepsilon_{n2}$$

$$U_{n,SQ} = \begin{cases} \boldsymbol{\beta X_{ref}} + ASC_{ref} + \varepsilon_{n,SQ}, \textit{in treatment } 1 \\ ASC_{opt-out} + \varepsilon_{n,SQ}, \textit{in treatment } 2 \end{cases} \qquad \text{Eq (4)}$$

Where $\boldsymbol{Var_1}$ and $\boldsymbol{Var_2}$ are the vectors representing the attribute-level deviations of the two hypothetical alternatives from the individual-specific reference scenario $\boldsymbol{X_{ref}}$, as defined by the experimental design. A well-known limitation of this specification is the independence of irrelevant alternatives assumption (see Train, 2001, chapter 2). In other words, this specification assumes that if one alternative is not available for the respondent, its choice probability will be distributed proportionally over the remaining alternatives. This is a huge limitation in this context because it does not allow for different substitution patterns among hypothetical and SQ alternatives. One way to relax this assumption is to decompose the error term of the SQ alternative into a random and zero-centered normal distributed term and a residual unobserved Gumbel distributed term (Walker, 2001). The error term of the reference alternative takes the form $\varepsilon_{n,SQ} = \rho_{n,SQ} + \widetilde{\varepsilon}_{n,SQ}$, where $\rho_{n,SQ} \sim N(0, \omega^2)$ and $\widetilde{\varepsilon}_{n,SQ}$ is still Gumbel distributed with scale parameter σ.[3] The utility of the SQ alternative becomes then:

$$U_{n,SQ} = \begin{cases} \boldsymbol{\beta X_{ref}} + ASC_{ref} + \rho_{n,SQ} + \widetilde{\varepsilon}_{n,SQ}, \textit{in treatment } 1 \\ ASC_{opt-out} + \rho_{n,SQ} + \widetilde{\varepsilon}_{n,SQ}, \textit{in treatment } 2 \end{cases} \qquad \text{Eq (5)}$$

This error component (EC) specification, besides allowing for different correlation structures among the error terms of hypothetical alternatives and the SQ alternative, has also a more comprehensive economic interpretation. In fact, now the SQ effect for the individual n is composed by the sum $ASC_{ref/opt-out} + \rho_{n,SQ}$ so that it can be considered as an individual specific and normally distributed random variable, centered on $ASC_{ref/opt-out}$ and with variance ω^2. Scarpa et al. (2005) report that the error component specification consistently achieves more accurate results than a conditional logit with an ASC for the SQ alternative.

3.1. Levels of variations from the reference point

In previous sections, we argued that the levels of variations (dispersion) of attribute levels of the hypothetical alternatives from the individual-specific reference point can have an additional effect on the SQ bias. In our study, as in most applications, the range of attribute variations is determined by the experimental design. A better understanding of how these variations affect estimates can help researchers improve the design of pivot experiments. We characterize a synthetic measure of the level of variations in the choice card t in a pivot design with J alternatives described by K attributes. To do so, we introduce a function of the level of variations from the reference point that we call "dispersion" and is defined as:

$$D_t = \frac{1}{K}\sum_{k=1}^{K}\frac{1}{J}\sum_{j\in J\ /\{ref\}}\left|\widehat{Pvar}_{tjk}\right| \qquad \text{Eq (6)}$$

Where $\widehat{Pvar}_{tjk}$ represents the percentage variation of the j_{-th} alternative's and k_{-th} attribute from the reference point. We use the simple difference for attributes that can take a value of zero. To accommodate for the different scales of attributes, we perform a standard normalization. To treat positive and negative variations equivalently, we compute the absolute value of the resulting measure.[4]

The dispersion D_t indicates, on average, the extent of the differences between hypothetical alternatives and SQ alternative in choice card t. The utility of the SQ alternative can now be redefined to account for dispersion-induced SQ bias:

$$U_{t,n,SQ} = \boldsymbol{\beta X_{t,SQ}} + ASC_{t,SQ} + \rho_{t,n,SQ} + \lambda^* D_t + \widetilde{\varepsilon}_{t,n,SQ} \qquad \text{Eq (7)}$$

Where λ is the parameter that measures the marginal (dis)utility of the dispersion indicator.[5] If such parameter is significantly positive, we say that the dispersion increases the SQ effect. A potential issue with the specification in equation (7) is that it considers the effect of dispersion as linear. While this might be the case, from a behavioral standpoint, it is more plausible that the propensity to favor the SQ due to dispersion is triggered only beyond a certain threshold. In other words, this tendency arises only when respondents perceive the hypothetical alternatives as significantly dispersed with respect to their reference point. To account for this, we say that the choice card t is "dispersed" if the measure D_t exceeds a certain threshold τ that is defined *a priori* by the researcher. So, we define an indicator function such that:

$$I_t = \begin{cases} 1, \textit{if } D_t > \tau \\ 0, \textit{otherwise} \end{cases}$$

We treat the indicator I_t as an additive term for the SQ effect, so that the utility of the SQ alternative at time t can be written as:

$$U_{t,n,SQ} = \boldsymbol{\beta X_{t,SQ}} + ASC_{t,SQ} + \rho_{t,n,SQ} + \lambda^* I_t + \widetilde{\varepsilon}_{t,n,SQ} \qquad \text{Eq (9)}$$

Finally, the log-likelihood function of the model is:

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[3] We are assuming homoscedasticity to avoid too many subscripts that may confuse the reader, but the same framework could be extended to account for heteroscedastic simply specifying different scales.

[4] The measure we propose can be readily adapted by researchers to accommodate hot-coded categorical variables, which are commonly used in DCEs. One possible approach is to replace the term $\left|\widehat{Pvar}_{jk}\right|$ with a Gower distance metric (Gower, 1971). The Gower distance (Gower, 1971) is a metric specifically designed to handle mixed data types, such as continuous and categorical variables. It standardizes the contribution of each attribute to a score between 0 and 1, reflecting similarity or dissimilarity, and allows the researcher to assign different weights to different attributes. The overall distance is obtained by averaging these standardized scores. We thank an anonymous reviewer for noting this.

[5] Of course, the researcher could define λ as a function of socio demographic variables characterizing it as a random variable in estimation.

$$LL\ (\beta,\rho,\tau,\lambda\ ) = \int_{\rho} \sum_{n=1}^{N} \sum_{j=1}^{J} y_{nj} \left[V_{ni} - \ln \left(\sum_{k=1}^{J} V_{nk} \right) \right] \quad f(\rho)\ \ \mathrm{d}(\rho) \qquad \text{Eq (10)}$$

Where y_n is an indicator function that takes on value 1 if the alternative j is chosen by the respondent n. This integral does not take a closed form, so the parameters must be estimated via simulated maximum likelihood (McFadden and Train, 2000), that has been carried out in the R environment using the Apollo package (Hess and Palma, 2019). In the estimation routine, we employed 5 000 Halton draws.

4. Empirical application

4.1. Design and data collection

The DCE questionnaire was administered to a cohort of Italian bluefin tuna recreational fishers as part of a wider research project on the economic value of recreational fisheries. Bluefin tuna fishing is a difficult and expensive practice that requires a high level of experience and skills among recreational fishers. Preparatory analysis was therefore conducted with 5 semi-structured interviews organized with marine biologists from the Italian National Research Council to investigate fishing practices and six semi-structured interviews with experienced fishermen.

The questionnaire was piloted with 28 fishermen recruited during a fishing competition in August 2022. At the end of this process, the questionnaire was consolidated and organized into four sections, each collecting the following information: (i) respondent's fishing habits, including details to construct the individual specific reference point, (ii) DCE questions, (iii) follow-up questions to check the respondent's attention, potential protest responses, and attribute non-attendance, (iv) sociodemographic information.

Each choice card includes three different fishing trips that differ in the chance of catching tuna, the number of fish of other species caught, and the total cost of the trip. To define the hypothetical scenarios, we asked respondents to imagine that it was the last day of the open season to fish bluefin tuna. Here is the full description of the hypothetical scenario.

“Imagine that today is the last open day of the bluefin tuna fishing season. You will be presented with twelve choice situations representing bluefin tuna trips with different characteristics. You simply have to choose your favorite. Consider that the characteristics not presented (weather, crew members, equipment) are identical among the alternative trips. Below is a brief explanation of each trip's features. We kindly ask you to pay close attention to this description.

- ***Probability of catching a bluefin tuna:*** *This is the probability that the crew hooks and catches a medium-sized bluefin tuna (*45 kg*) during the trip. You will find the probability expressed as a percentage.*
- ***Number of other species caught:*** *Number of other fish such as flatfish, stingrays, swordfish, or similar species, and retained during the trip;*
- ***Trip cost:*** *Individual expenses that each crew member incurs for a trip. The expense does not refer exclusively to the costs of the single trip, but the costs of the durable equipment are also included. Consider that the change in these costs depends on the taxation imposed on the equipment needed for fishing (hooks, fuel, bait, port fees, etc.). The variation in this cost, therefore, does NOT depend on the quality and quantity of the equipment.”*

Then, each respondent received 12 choice situations framed as: 6 choice cards presented the SQ alternative as an individual-specific typical trip (Fig. 1a), and 6 choice cards with the SQ alternative characterized as a “not to go fishing” option (Fig. 1b). The software automatically calculated the individual-specific reference alternative using information collected in the first section of the questionnaire. Specifically, for each respondent the reference probability of catching a bluefin tuna was estimated as the ratio between successful fishing trips (where a Tuna was caught) and total fishing trips. This estimated probability of catching bluefin tuna was then presented to the respondent, who could confirm or adjust it based on their expectations for the upcoming fishing trip. For example, a respondent who reported going on 10 bluefin tuna fishing trips and catching a tuna on 4 of them would be shown a 40% probability. They were then asked: *“Given that you caught at least one Bluefin Tuna on 40% of your trips last year, what do you think are the chances of catching one on your next trip?“*. The reference level for the attribute “Number of other species caught” was determined by directly asking the respondent to provide the average number of other fish species caught in a single trip along the previous season. Lastly, the reference trip cost was calculated using various factors, including fuel and bait costs, the type of boat owned, fixed port expenses, and the typical number of crew members per trip.

The two blocks of treatment were randomly presented to respondents. The possible levels of variation for each attribute are reported in Table 1a. We adopted a D-efficient design (Ferrini and Scarpa, 2007), treating variations from the reference as discrete levels to define the hypothetical alternatives. The levels presented to each respondent were generated by applying the experimental design's variations to their individual-specific reference alternative.[6] For each of the two blocks of treatments, the hypothetical alternatives shown to respondents were identical. The only variation between treatments was the framing of the SQ alternative as either individual-specific reference point or opt-out.

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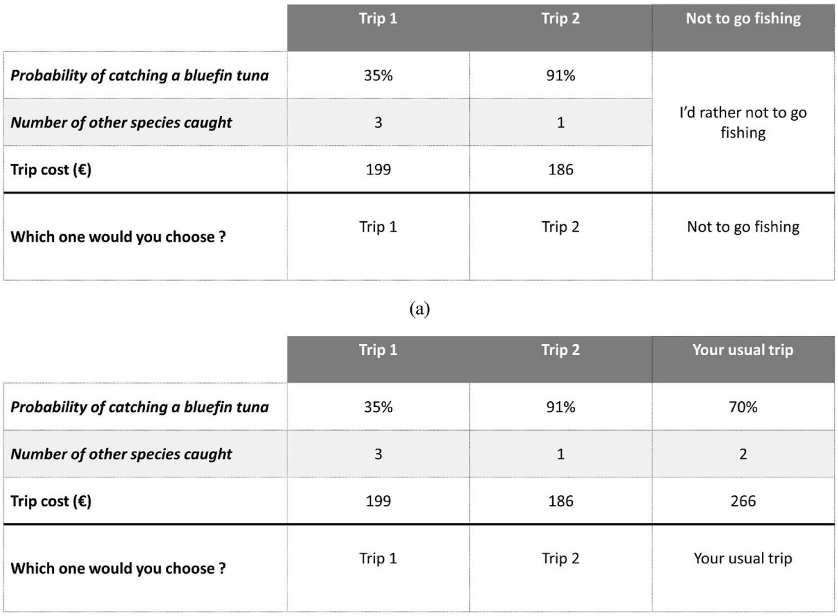
[6] To ensure the probabilities remained within a valid range, we capped any values exceeding 1 for respondents with a high reference probability of catching a bluefin tuna. In other words, if the variation resulted in a probability greater than 1, it was truncated to 1. Similarly, for the attribute “Number of other fish captured,” any negative values resulting from the variation were set to 0, as it is impossible to catch a negative number of fish.



	Trip 1	Trip 2	Not to go fishing
Probability of catching a bluefin tuna	35%	91%	I'd rather not to go fishing
Number of other species caught	3	1	
Trip cost (€)	199	186	
Which one would you choose ?	Trip 1	Trip 2	Not to go fishing

(a)

	Trip 1	Trip 2	Your usual trip
Probability of catching a bluefin tuna	35%	91%	70%
Number of other species caught	3	1	2
Trip cost (€)	199	186	266
Which one would you choose ?	Trip 1	Trip 2	Your usual trip

(b)

Fig. 1. Example of a choice cards. The alternatives' levels are defined as variation from the individual specific reference point ("your usual trip"). Fig. 1a shows an example of inclusion of the reference (treatment 1), while Fig. 1b shows an example of inclusion of the opt-out alternative (treatment 2).

Table 1a
Levels of variations from the baseline included in the experimental design.

Attribute	Levels (in terms of variation from the baseline)
Probability of catching an average-sized Bluefin Tuna	• -50% • -25% • 0 • +25% • +50%
Number of other fish captured	• -1 • 0 • +1 • +2
Cost of the trip	• -75% • -50% • -25% • 0 • +25% • +50% • +75%

The experiment design comprised 36 choice cards, 18 with SQ as reference point and 18 with an opt-out option, that were split into 3 blocks. Table 1b presents the experimental design for both treatments, showing the coded levels of variation for each attribute alongside the predicted dispersion for each choice card.[7]

Respondents were provided with contextual explanations directly within the survey to ensure the credibility of the hypothetical variations presented in the choice tasks. Changes in the probability of catching bluefin tuna were attributed to potential regulatory adjustments in catch quotas and access rights, such as redistributions between commercial and recreational fishing sectors as well as seasonal patterns in Bluefin Tuna migrations—situations that recreational fishers in Italy are familiar with. Variations in trip cost were explained as stemming from exogenous changes in navigation conditions (e.g., needing to travel further offshore), fluctuations in fuel prices, and potential increases in port fees or equipment costs due to new safety or environmental standards. Changes in the number of

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[7] Please note that the actual level of dispersion may differ slightly from these predicted values, due to the capping of the probability of catching a tuna at one and the number of other fish at zero.

Table 1b
Full experimental design for one treatment. For each choice card, the predicted level of dispersion is reported.

Alternative 1			Alternative 2			Predicted Dispersion
Probability	Other fish	Trip cost	Probability	Other fish	Trip cost	
0.2	−1	−0.5	0	1	0.25	0.7
0	1	0.25	0.2	−1	−0.5	0.7
0.4	2	0.75	0.4	−1	−0.75	1.44
−0.2	0	−0.25	−0.4	0	0	0.51
−0.4	0	0	−0.2	0	−0.25	0.51
−0.2	1	−0.25	−0.4	0	−0.25	0.57
0	2	0.5	0	−1	−0.5	0.81
−0.4	0	0.25	−0.2	1	−0.5	0.71
−0.4	0	0	−0.2	1	0.25	0.6
0	2	−0.75	0	−1	0.5	0.89
0	−1	−0.5	0	2	0.5	0.81
0.2	2	−0.75	0.2	−1	0.75	1.21
0.4	−1	−0.75	0.4	2	0.75	1.44
−0.4	0	0	−0.2	1	−0.25	0.54
0.4	−1	0.75	0.4	2	−0.75	1.44
−0.2	1	−0.25	−0.4	0	0	0.54
0.2	−1	0.5	0.2	2	−0.75	1.13
−0.2	1	−0.5	−0.4	0	0	0.62

other fish caught were described as a result of shifting fishing locations, variations in baiting strategies, or seasonal fluctuations in species availability—realistic factors that fishers regularly encounter. Participants to the survey were recruited by the register of the Italian Federation of Sport Fishing and Underwater Activities FIPSAS (*Federazione Italiana Pesca Sportiva ed Attività Subacquee*), the main recreational fishing association and the only one recognized by Italian authorities. The federation sponsored the data collection by distributing the questionnaire to Italian bluefin tuna fishermen via email. After the first invitation, three reminders were sent within a month of each other. The invitation to participate contained a link redirecting to the survey that was coded and managed using the software of the company Surveyengine (https://surveyengine.com/). The field period was from October 2022 to May 2023.

4.2. Descriptive statistics

The recruitment campaign gained 278 responses. After excluding incomplete responses, protest responses,[8] declarants of attribute non-attendance, and inactive fishers for at least 3 years, the final sample included 202 fishers, resulting in 2 424 choice observations (1 212 for each treatment). In Table 2, we report the sociodemographic characteristics of the sample along with specific features on the fishing behavior and stated features of their usual fishing trip.

In line with recreational fishery literature (see, for example, Arlinghaus et al., 2021), our sample is almost entirely composed of men (93%), with an average age of around 50. The level of education was rather heterogeneous and in line with the Italian population of the same age (www.istat.it), with approximately 62% of respondents not holding a bachelor's or a master's degree, 22% of whom did not finish high school. Bluefin tuna recreational fishers are confirmed to be very active and prone to spending a significant amount of money on their activity. While catching a bluefin tuna is challenging, as evidenced by the 38% of respondents who failed to do so during the previous year, fishers embark on an average of 20 bluefin tuna fishing trips. This activity is evenly distributed between the open and closed seasons, during which catching and keeping a bluefin tuna is not allowed. It is worth noting a discrepancy between reported outcomes and subjective beliefs. While the average number of bluefin tuna caught per respondent in the previous year was 1.48, and 38% of respondents did not catch any, the average self-reported probability of catching a tuna on a given trip is approximately 50%. This mismatch may reflect a systematic overestimation of success probability, potentially rooted in overconfidence or motivated beliefs. Importantly, our design intentionally leverages each respondent's stated perceptions to define their individual-specific reference trip. As such, these beliefs, although potentially biased, are integral to how respondents evaluate hypothetical alternatives, in line with the behavioral foundation of the SQ effect explored in this study.

Bluefin tuna fishing is rarely an individual activity; in fact, only four respondents (2% of the sample) declared to go out on a trip alone, while most of the participants (93% of the sample) declared to go fishing in groups from 2 to 4 people, with an average of people per crew of 3.07. Regarding individual expenses, the average cost for one trip is relatively high at 361 €. This cost is primarily associated with significant annual expenditures for boat maintenance (3 624 €), the depreciation of specific durable equipment like rods, tuna fighting belts, and fishing reels (5 185 €), as well as current expenses for a single trip, including gasoline and bait (326 €). The average number of catches for other species stands at 1.28.

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[8] Protest responses were identified through a follow-up question posed to respondents who consistently selected the status quo option. Those who indicated reasons "*I don't think recreational fishing regulation should change*" or "*These scenarios are not realistic*" were classified as protest responses and excluded from the final sample.

Table 2
Sociodemographic and other characteristics of the sample.

Variable	Mean	Median	Std. dev.
Age	46.6	46	12.95
Gender (Male)	0.93	NA	NA
Years of education	14	13	3.42
Annual fishing trips	58.97	60	17.87
Annual Tuna Trip (season open)	10.93	5	15.13
Annual Tuna trip (season closed)	9.8	5	14.46
Captured a Tuna last year (Yes = 1)	0.62	NA	NA
Number of Tuna Caught last year	1.48	0	0.48
Boat owner (Yes = 1)	0.87	NA	NA
Crew members	3.07	3	0.86
For each tuna trip …			
Number of fish of other species caught	1.28	1	0.97
Stated probability of catching a tuna	0.49	0.5	0.26
Cost (€)	361	292	222
People in the crew	3.1	3	0.86
Expenses for …			
perishable goods for a tuna trip (gasoline, baits, etc.) (€)	326	250	286
Durable equipment (€)	5 185	5 000	3 699
Yearly boat maintenance (if boat owners) (€)	3 624	3 000	3 798

4.3. Estimation results

For each of the two treatments, we estimated four models: (i) a simple multinomial logit with an ASC for the SQ alternative as in eq (4) that we call MNL-ASC; (ii) an error component logit as in eq (5), that we call EC; (iii) an error component logit with an additional parameter to capture additional dispersion-induced SQ bias in a linear fashion as in eq (7), that we call “EC-D linear”; (iv) a model as the latter but with a nonlinear specification of the effect of the dispersion on the SQ bias, that we call “EC-D threshold”. We use the multinomial logit model with an ASC for the SQ option and the error component logit model as benchmarks, representing the simplest and the most widely used approaches to address SQ bias. We also evaluate the performance of our proposed model specification against an alternative approach commonly used to account for the SQ effect, namely a multinomial logit model that captures asymmetric preferences around the reference point, as proposed by Hess et al. (2008). Estimation results for this model are reported in Appendix B.

Table 3 provides the results of (simulated) maximum likelihood estimations in the willingness-to-pay space for these four models. In brackets we report the heteroscedasticity robust T-ratios computed using the sandwich estimator. According to our setting, the WTP for a bluefin tuna is the compensating variation between being certain of not catching the tuna (probability 0) and being certain of catching it (probability 1). In the “EC-D threshold” model, we defined a choice card as dispersed if D in equation (6) exceeded a threshold of $\tau = 1.3$. Such value signifies that, on average, the attributes of the hypothetical alternatives vary by 1.3 standard deviations from the individual-specific reference point. To choose the size of such a parameter, we ran the model multiple times using different magnitudes of the threshold parameter; we then chose the value that provided the highest log-likelihood value (see Appendix A).

We first look at the MNL model estimated on the data characterizing the SQ alternative as an individual-specific reference point. The first column of Table 3 shows a willingness to pay of 216 € for catching with certainty one bluefin tuna and 30 € for a fish of a different species. Both values are statistically significant. The ASC for the SQ alternative is notably positive, providing evidence of a discernible SQ effect. The error component logit yields willingness-to-pay estimates strikingly similar to the previous ones, with the same level of statistical significance. The ASC remains significant and positive. The coefficient of the normally distributed random component suggests heterogeneity in the level of the SQ effect among respondents. However, the introduction of this error component does not lead to a significant improvement in model fit ($LR_1 = 1.52$, p-value = 0.22).[9] Then, we shift our focus to the two models accounting for dispersion in hypothetical alternatives.

Both models show a significant improvement in the model fit with respect to the MNL specification (respectively $LR_2 = 7.32$ and $LR_3 = 9.32$) and consistently, the parameter λ is positive and highly significant, indicating that individuals tend to favor the SQ alternative more frequently when variations in attributes among hypothetical alternatives are more pronounced. The “EC-D linear” model estimates an average willingness to pay for catching and keeping a Bluefin tuna with certainty of 361 €, a value far from the one in the EC model (220 €). The “EC-D threshold” model provides a value that is a bit closer to the previous estimate but still markedly different (318 €), while the willingness to pay for another fish remains stable across the different specifications. The substantial disparity in willingness to pay estimates for bluefin tuna with respect to the first two models suggests the potential for bias. This is particularly important for our research question. It suggests that when a “dispersion-induced” additional SQ effect is present, standard

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[9] LR_1 refers to a log-likelihood ratio test with 1 degree of freedom.

Table 3
Estimation results in willingness to pay space for the models presented in the methods section (Heteroscedasticity robust t-ratios in parentheses).

	“Your usual trip” option				“Not to go fishing” option			
	MNL-ASC	EC	EC – D linear	EC-D threshold	MNL-ASC	EC	EC – D linear	EC-D threshold
WTP Tuna (€)	216.01 (3.41)	220.35 (3.43)	361.36 (4.13)	318.36 (4.04)	282.46 (8.25)	281.07 (8.35)	281.61 (8.24)	281.20 (8.33)
WTP OF (€)	30.78 (4.78)	31.06 (4.81)	32.81 (5.05)	30.13 (4.63)	19.61 (5.12)	19.72 (5.09)	19.71 (5.06)	19.81 (5.12)
ASC ref	1.31 (13.87)	1.31 (13.05)	0.69 (2.76)	1.20 (11.29)	—	—	—	—
ASC Opt-out	—	—	—	—	−1.57 (7.21)	−1.66 (7.25)	−1.55 (4.22)	−1.65 (7.02)
Cost	−0.009 (16.37)	−0.009 (16.29)	−0.008 (16.35)	−0.009 (16.17)	−0.011 (20.09)	−0.011 (19.38)	−0.011 (18.67)	−0.011 (19.06)
Random parameters' standard deviations:								
ASC ref	—	0.35 (2.27)	0.33 (2.16)	0.35 (2.32)	—	—	—	—
ASC Opt-out	—	—	—	—	—	0.54 (3.26)	0.54 (3.27)	0.55 (3.27)
Dispersion-induced SQ effect:								
λ		—	0.74 (2.42)	0.57 (2.62)	—	—	−0.13 (0.35)	−0.06 (0.19)
Number of parameters	4	5	6	7	4	5	6	7
loglikelihood	−896.73	−895.97	−893.07	−892.07	−690.39	−688.69	−688.59	−688.64
AIC	1801.46	1801.94	1798.15	1798.14	1 388.79	1 387.39	1 389.18	1 391.28
BIC	1821.86	1827.44	1828.75	1833.84	1 409.19	1 412.89	1 419.78	1 426.98
Share of the SQ	41%				25%			

econometric techniques, such as including an ASC or using the error component logit model, could fail to provide unbiased estimators of the parameters of interest.

However, even when we consider directly the dispersion impact, alternative specifications of such effect provide different estimates, signaling that the importance to carefully design and test choice card for dispersion effects. In our application, the difference in willingness to pay estimates between EC-D linear and EC-D threshold model is around 11% demonstrating that a misspecification of such an effect could have significant policy implications. At the same time, in our empirical application, we cannot claim that one specification clearly outperforms the other. The information criteria (AIC and BIC) provide inconsistent rankings of models, but we claim that this specification could be considered in future applications where a dispersion effect could be at play.

A graphical illustration of the relationship between SQ bias and dispersion is provided in Fig. 2, where we plot the dispersion-induced SQ effect against the dispersion of the choice card. On the y-axis, the “additional SQ effect” is calculated as the difference between the observed probability of choosing the SQ alternative (i.e., its share in the data) and the probability predicted by a simple multinomial logit with an ASC for the SQ[10]. Although this exercise is purely illustrative, it serves as an exploratory tool to visualize the portion of the SQ effect that is not captured by the fixed ASC and how it varies with dispersion. In Fig. 2, the blue line shows an OLS fit to the data points. The figure shows a reasonable linear trend between dispersion and the unexplained propensity to choose the SQ, which is consistent with the results of our EC-D linear specification. While the figure does not suggest the presence of a clear threshold triggering the dispersion-induced SQ effect, it remains consistent with the behavioral rationale underlying the threshold specification. From a behavioral perspective, it is plausible that the effect of dispersion on SQ propensity does not increase gradually but is instead activated once the perceived deviation from the reference point becomes sufficiently large. For this reason, we argue that the EC-D threshold model represents a useful alternative specification to be tested both in this study and in future applications.

Finally, it is noteworthy that separately identifying the ASC from the parameter λ may be more difficult in the linear specification, as the parameter λ may capture some idiosyncratic SQ effect that is not dispersion dependent. Instead, separating λ from ASC may be easier when the effect of dispersion is introduced after a specific threshold. This hypothesis is further supported by the change in the magnitude of the parameter λ between the two models, suggesting that in the linear specification, λ is conflating the dispersion effect with the spurious SQ effect. The Monte Carlo simulation will shed more light on this hypothesis.

The right half of Table 3 reports the estimate of the same four models applied to choices characterizing the SQ alternative as a “not-to-go-fishing” option (see Fig. 1b). In this case, the willingness to pay estimates are much more stable across the four specifications. With respect to the treatment including a reference alternative, the MNL estimates a higher willingness to pay for catching a bluefin tuna with certainty (282.46 €), with associated t ratios notably increased, and a lower willingness to pay to catch a fish of another species (19.61 €). As expected, the ASC for the SQ alternative is now negative, suggesting that, on average, staying at home is viewed as less preferable for recreational tuna fishers. Adding a normally distributed error component does not change the estimates but improves the model fit (LR1 = 3.4, p-value = 0.06). The estimate of the standard deviation of the error component is highly significant, indicating heterogeneity in the magnitude of the preference for opting out. The estimated normal distribution has a mean of −1.66 and a standard deviation of 0.54. This implies a heterogeneous yet almost exclusively negative distribution of preferences for opting out. Accounting for dispersed choices does not lead to notable changes in the willingness to pay estimates. These remain stable at around

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[10] The parameters of such multinomial logit are taken as the arithmetic averages of the estimates from the four models reported in Table 3.

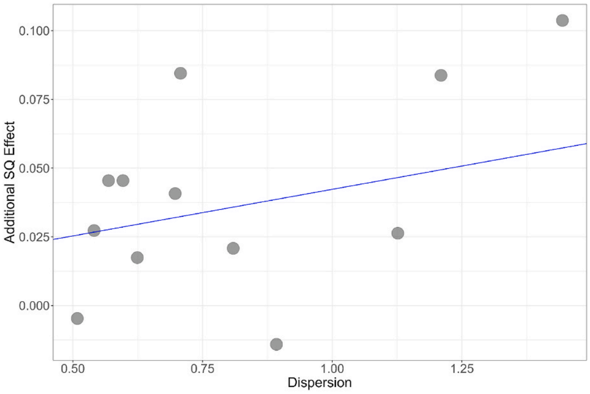

Fig. 2. Additional SQ effect as a function of choice card dispersion. The additional SQ effect represents the probability of choosing the SQ alternative, adjusted subtracting the predicted probability from a simple multinomial logit specification. The “true parameters” used for this adjustment are averaged from the four models estimated in left part of Table 3. The blue line represents an OLS fit using all observations. (For interpretation of the references to colour in this figure legend, the reader is referred to the Web version of this article.)

281 € in both the EC-D linear and EC-D threshold specifications. Also, their likelihood measures are almost identical to that of the error component model (EC in Table 3). This result is a major finding of our research. Such results indicate that characterizing the SQ as an opt-out seems to have a positive impact on the ASC's ability to capture average inclinations toward that alternative, no matter the dispersion of the hypothetical alternatives from a reference point. Even if respondents, on average, express a dislike for opting out, this preference seems not to be influenced by the choice card dispersion as in the case where the SQ is characterized as an individual-specific reference point. This helps avoiding potential bias of this nature in the estimates. Additionally, the observation that the willingness to pay estimate is lower than the one in the best-fitting model in the treatment with reference alternatives (EC-D threshold, fourth column in Table 3) might suggest that forcing respondents' choices by not offering an opt-out option may render the results more susceptible to hypothetical bias. This implies that including an opt-out instead of a reference alternative would appear to be a more conservative way to manage SQ bias. We will discuss this result in detail in the last section of this work.

5. Monte Carlo simulation

In this section, we conduct a series of Monte Carlo experiments to find the finite sample properties of three different econometric models, namely the error component logit and the two extended models we propose to account for dispersion in a pivot design setting.

5.1. Simulation settings

In our simulation, we employ six different data generation processes (DGP), all incorporating an additional SQ effect based on the dispersion of hypothetical alternatives. The different DGPs differ in the specification of the dispersion effect (linear vs non-linear) and the experimental design adopted to generate the variation levels. The simulation is executed using custom-built R code. We conduct 5 000 simulations with a sample size of 500 individuals, each making 12 different choices, resulting in 6 000 observations. The steps of the data generation process are outlined below.

1) We randomly generate individual-specific reference attributes from a standard uniform distribution.
2) For each simulated individual, we generate levels of variation for the 12 choice tasks in the following three ways:
    a) We generate a random design (drawing the levels of variations from a uniform ranging from -1 to 1) that we then apply to each of the respondents so that each respondent faces the same level of variation with respect to their reference;
    b) Same as in the previous point, but constraining the design to avoid too dispersed choice card (i.e., where $D_t < \tau \quad \forall t$). The threshold parameter τ is kept fixed at the value in Table 4.
    c) We draw from a uniform distribution with extremes -1 and 1 the level of variations for each choice task (so there is no experimental design at all).
3) Compute the two hypothetical alternatives using the reference variation levels.
4) Compute the deterministic part of the utility using the parameter in Table 4.
5) Generate the unobserved stochastic component of the utility of each alternative by using random draws from the inverse cumulative distribution of a Gumbel distribution with scale 1.

Table 4
Parameters used for simulating the choice data.

	Mean	Standard deviation
β Tuna	3	0.3
β Other	1	0.1
β Cost	−1	0.1
Status quo ASC	1	0.3
λ	2	Fixed coefficient
τ	0.6	Fixed coefficient

6) Generate a respondent-specific ASC for the SQ alternative representing the idiosyncratic SQ effect. We do so, drawing from a normal distribution with mean and standard deviation reported in Table 4. This specification of the ASC also introduces a correlation between the two hypothetical alternatives.
7) For each choice occasion, we compute the dispersion D presented in eq (6). Then selecting a threshold τ (Table 4),[11] we compute the indicator I_t as in eq (8).
8) Generate the additional dispersion-induced SQ effect in two ways:
    a) In a linear fashion, as in eq (7)
    b) In a nonlinear fashion, as in eq (9)
9) Derive an indicator of choice from the alternative associated with the highest computed utility.
10) Proceed to estimate the parameters of three models using simulated data. Save the relevant estimation results (parameter estimates, t-values, log-likelihood at convergence, etc.). The three models estimated are an error component logit (EC) and two extended error components with a parameter (λ) to account for additional dispersion-induced SQ effect (EC-D linear and EC-D threshold).
11) Repeat this routine 5 000 times.

Then, we compute three synthetic measures for the Monte Carlo distribution of all the coefficients used in the DGP: the mean and standard deviation of the Monte Carlo distribution and the average relative absolute error defined as:

$$\overline{RAE} = \frac{1}{R}\sum_{r}^{R}\frac{|\widehat{\boldsymbol{\beta}_r} - \boldsymbol{\beta}_o|}{\boldsymbol{\beta}_o} \qquad \text{Eq (11)}$$

Where $\widehat{\boldsymbol{\beta}_r}$ is the vector of the coefficients estimate at iteration R and $\boldsymbol{\beta}_o$ is the correspondent true value in the DGP (Table 4). This measure gives a measure of the relative magnitude of the bias of the estimate.

5.2. Simulation results

Tables 5a, 5b, and 5c present the outcomes of simulations conducted over 5 000 iterations, each with a sample size of 6 000 choices made by 500 simulated individuals. The upper part of each table reports the results for a DGP based on the EC-D linear model, while the lower part presents results for the EC-D threshold model as the DGP. For each DGP specification, we report results for three different methods of constructing variations from the reference alternative (step 2 of the DGP). Finally, we present the results for the three estimated models.

In bold, we report the results of the simulations where the estimated model is correctly specified. In such cases, as expected, the mean of the distribution tends to align with the true value of the DGP, demonstrating the consistency of the estimators (Table 5a).

When the effect of dispersion is linear in the DGP, and the level of variations is randomly defined for each respondent with no design, both the EC and the EC-D threshold specifications provide estimates very close to the true values and with high level of precision (first and third column in the upper part of tables 5a-b-c). The correctly specified model (EC-D linear) provides, on average, estimates with a lower RAE (Average Relative Absolute Error) but a slightly higher standard deviation. Looking at the RAE of the models (first three columns in upper Table 5b), we observe nearly identical values for the three coefficients of the covariate of interest. However, a key difference between the “true” model and the other two is the ability to accurately recover the ASC for the SQ alternative. When the model is misspecified, the ASC seems to capture the additional effect of dispersion, completely in the case of an EC specification and partially in the case of an EC-D threshold specification.[12] In this case, when using a standard model as an error component specification, the effect of the dispersion will be confounded with the ASC parameter, but the welfare estimates will not suffer from bias.

Now, we shift our focus to the results obtained by fixing the same level of variations for every respondent to simulate the effect of an experimental design (central columns in upper Tables 5a and 5b). Defining the levels of variations from a random design appears to

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[11] The threshold parameter τ was not fixed to the empirically optimal value of 1.3 (as identified in Appendix A) but instead calibrated to ensure sufficient variation in dispersion above and below the threshold across simulated choice tasks.

[12] Since we draw variations from a uniform with extremes −1 and 1, it could be easily demonstrated that the average value of the dispersion is 0.5. The average effect of the dispersion, with $\lambda = 2$ will be an additional reference bias of 1 that is captured by the ASC.

Table 5a

Monte Carlo simulation results for 5 000 replications. Mean of the Monte Carlo distribution.

Dispersion effect = linear									
	No design			Design			Design no distant choices		
	EC	EC-D linear	EC-D thresh	EC	EC-D linear	EC-D thresh	EC	EC-D linear	EC-D thresh
β Tuna	2.91	**2.96**	2.93	2.87	**2.96**	2.91	2.96	**2.97**	NA
β Other	0.96	**0.99**	0.97	0.96	**0.99**	0.97	0.99	**0.99**	NA
β Cost	−0.97	**−0.99**	−0.97	−0.96	**−0.99**	−0.97	−0.99	**−0.99**	NA
ASC	1.95	**1**	1.88	1.95	**0.99**	1.88	1	**0.99**	NA
λ-	NA	**1.96**	0.43	NA	**1.98**	0.41	NA	**0.02**	NA
Dispersion effect = nonlinear									
	No design			Design			Design no distant choices		
	EC	EC-D linear	EC-D thresh	EC	EC-D linear	EC-D thresh	EC	EC-D linear	EC-D thresh
β Tuna	2.64	2.8	**2.96**	2.53	2.9	**2.97**	2.96	2.96	NA
β Other	0.88	0.93	**0.99**	0.84	0.95	**0.99**	0.98	0.99	NA
β Cost	−0.88	−0.94	**−0.99**	−0.84	−0.97	**−0.99**	−0.99	−0.99	NA
ASC	1.27	−0.6	**0.99**	2.03	−1.36	**1**	0.99	0.99	NA
λ-	NA	3.84	**1.97**	NA	7.28	**1.98**	NA	0.01	NA

Table 5b

Monte Carlo simulation results for 5 000 replications. Average Relative Absolute Error (RAE) of the Monte Carlo distribution.

Dispersion effect = linear									
	No design			Design			Design no distant choices		
	EC	EC-D linear	EC-D thresh	EC	EC-D linear	EC-D thresh	EC	EC-D linear	EC-D thresh
β Tuna	0.05	**0.04**	0.05	0.06	**0.05**	0.06	0.05	**0.05**	NA
β Other	0.08	**0.07**	0.07	0.15	**0.1**	0.13	0.12	**0.12**	NA
β Cost	0.08	**0.07**	0.07	0.15	**0.1**	0.13	0.12	**0.12**	NA
ASC	0.95	**0.13**	0.88	0.95	**0.21**	0.88	0.07	**0.19**	NA
λ-	NA	**0.12**	0.8	NA	**0.21**	0.79	NA	**1**	NA
Dispersion effect = nonlinear									
	No design			Design			Design no distant choices		
	EC	EC-D linear	EC-D thresh	EC	EC-D linear	EC-D thresh	EC	EC-D linear	EC-D thresh
β Tuna	0.12	0.07	**0.04**	0.2	0.11	**0.04**	0.05	0.06	NA
β Other	0.12	0.08	**0.07**	0.43	0.29	**0.09**	0.12	0.12	NA
β Cost	0.12	0.08	**0.07**	0.43	0.3	**0.09**	0.12	0.12	NA
ASC	0.27	1.6	**0.07**	1.03	2.4	**0.09**	0.07	0.19	NA
λ-	NA	0.92	**0.05**	NA	2.66	**0.06**	NA	1	NA

introduce, on average, a higher level of bias in misspecified models (EC and EC–D threshold), even if the bias is not severe (lower than 5%). However, even if the Monte Carlo distribution of the error is centered upon a value that is close to the true one, on average, the error of the misspecified models starts to be non-negligible. In fact, the estimates of the parameters β_{other} and β_{cost} differ on average by 15% from the true value (RAE = 0.15).

Avoiding the introduction of choice tasks with dispersion greater than $\tau = 0.6$ (last two columns in upper Tables 5a and 5b) is beneficial for the consistency of the EC model, though it results in an estimator that is, on average, further from the true value compared to not using a design (greater RAE). At the same time, if the effect of the dispersion is linear in the DGP and there is no threshold effect, reducing the variability of the hypothetical alternatives results in less information to exploit in estimation, with a consequential increase in the RAE also of the true model (last three columns in upper Table 5b).

Looking at the lower half of Tables 5a and 5b, we observe the same metrics for a DGP that induces a nonlinear effect of dispersion on the SQ effect. In this scenario, when the model is misspecified, the estimators exhibit substantial bias from the true value. When the level of variation from the reference is randomly drawn from a uniform distribution, the mean of the Monte Carlo distribution of the estimates is far from the true value by around 12% using an error component specification and by 7% using the EC-D threshold model. When the levels of variations are fixed by a random design, such deviation increases to 17% in the error component specifications (fourth column in lower Table 5a). Regarding the ASC, the EC model still tends to capture the effect of λ when this is not specified, while the EC-D linear model shows a dramatic error in measuring this parameter, with a RAE of 92%, indicating a complete inability to recover the true parameter. This error worsens when specifying a fixed design for each respondent, reaching a RAE of 240% in the ASC parameter of the EC-D linear model. While these findings illustrate the potential advantage of the threshold specification under certain conditions of the DGP, they should be interpreted with caution, since in practice τ is unknown and must be estimated, which may reduce its relative performance.

Notably, and as expected, avoiding the introduction of excessively dispersed choices in the design allows the EC model to recover the correct parameters as effectively as the EC-D linear model, which is in line with part of the findings in Scarpa et al. (2005). If the researcher can identify the threshold at which hypothetical choices are perceived to be unrealistic because they are too dispersed from the reference, the data generation process collapses to an error component specification.

Table 5c reports the standard deviations of the Monte Carlo distributions for each estimated coefficient. When the DGP includes a linear dispersion effect, the standard deviations of the correctly specified model (EC-D linear) are generally equal to or slightly higher than those of the misspecified models, especially for parameters like λ and the ASC. This likely reflects the model's effort to separately identify two effects, the idiosyncratic SQ effect and the linear dispersion-induced one, which introduces some additional estimation variance. In contrast, when the simulated effect of dispersion is nonlinear, the EC-D threshold model (correctly specified) tends to show equal or substantially lower standard deviations for the key parameters, including λ and the ASC. One plausible explanation is that the threshold model treats the dispersion-induced effect as a discrete shift in behavior, which may allow for cleaner separation between sources of heterogeneity and lead to more stable estimates. This, however, relies again on a correct specification of the threshold parameter, which in applied settings may not be straightforward.

6. Concluding remarks

The study presents a split-sample DCE survey where randomly the respondents assess choice cards with individual-specific reference point and opt-out alternative. These two treatments differ in the characterization of the status quo (SQ) alternative and help to investigate the SQ bias induced by DCE design. Rather than aiming to identify the superior design, the paper explores how the SQ framings induce a dispersion respect to the hypothetical alternatives and influence choice behavior and welfare estimates. Within this framework, we propose a novel econometric model to accommodate the "dispersion" from the individual's reference point on the SQ effect and mitigate its impact on willingness to pay. As dispersion is known to influence the propensity to choose the SQ option (Oehlmann et al., 2017), we argue that it introduces a trade-off between making the choice task more realistic and increasing the potential for bias due to perceived implausibility of alternatives.

The paper contributes to exploring the severity of the SQ bias and the consequence of the researcher's choices regarding the experimental design. In our application, when the SQ is characterized as an individual-specific reference point (e.g. the average fish trip), bluefin tuna fishermen exhibit a preference for their typical trip over hypothetical alternatives. The propensity to favor the SQ becomes more pronounced as the alternative trips are more "dispersed." To identify such dispersion-induced SQ effect separately, we propose an extension of the error component model where the nonlinear effect of such dispersion can be captured. Through a Monte Carlo simulation, we demonstrate that not accounting for the design-induced dispersion could severely bias the willingness to pay estimates. If the effect of dispersion is linear, the bias can be quite effectively managed using traditional models like the error component logit. However, if the relationship between dispersion and the SQ effect is nonlinear, standard models may conflate the dispersion effect with the ASC, leading to more substantial bias. Our extended model helps disentangle these two effects and more accurately represents the behavioral mechanism when dispersion is present.

Conversely, when the SQ alternative is characterized as a "not going out" option, the propensity to choose such an alternative is, on average, lower than for the hypothetical ones. In this treatment, we do not find evidence of a dispersion-induced SQ effect, suggesting that the behavioral influence of dispersion is less pronounced when the individual-specific reference is not made explicit. However, we caution that this does not imply that dispersion has no effect in this setting—rather, its salience may be reduced. The greater stability of willingness to pay estimates across model specifications in the opt-out treatment should not be interpreted as a sign of superiority, but as a reflection of how a less personally anchored SQ alternative may interact differently with design-induced variation. From a behavioral standpoint, one possible explanation is that a fishing trip, which initially appears unrealistic because it deviates from the

Table 5c

Monte Carlo simulation results for 5 000 replications. Standard deviation of the Monte Carlo distribution of the estimated coefficients.

Dispersion effect = linear									
	No design			Design			Design no distant choices		
	EC	EC-D linear	EC-D thresh	EC	EC-D linear	EC-D thresh	EC	EC-D linear	EC-D thresh
β Tuna	0.14	**0.14**	0.14	0.21	**0.18**	0.20	0.19	**0.19**	NA
β Other	0.09	**0.09**	0.08	0.18	**0.13**	0.16	0.15	**0.15**	NA
β Cost	0.09	**0.09**	0.09	0.18	**0.13**	0.16	0.15	**0.15**	NA
ASC	0.10	**0.16**	0.10	0.12	**0.28**	0.12	0.08	**0.25**	NA
λ-	NA	**0.30**	0.09	NA	**0.56**	0.20	NA	**0.69**	NA
Dispersion effect = nonlinear									
	No design			Design			Design no distant choices		
	EC	EC-D linear	EC-D thresh	EC	EC-D linear	EC-D thresh	EC	EC-D linear	EC-D thresh
β Tuna	0.12	0.12	**0.14**	0.52	0.38	**0.16**	0.19	0.20	NA
β Other	0.07	0.08	**0.08**	0.52	0.36	**0.11**	0.15	0.16	NA
β Cost	0.07	0.08	**0.08**	0.51	0.37	**0.12**	0.15	0.16	NA
ASC	0.08	0.16	**0.08**	0.31	1.08	**0.12**	0.09	0.25	NA
λ-	NA	0.29	**0.12**	NA	2.19	**0.16**	NA	0.67	NA

typical trip, may become relatively more attractive when the only alternative is not to go fishing. Offering an opt-out may also enhance realism, as respondents always have the ability to reject all proposed options (Dhar and Simonson, 2003). On the other hand, visualizing one's own reference point may trigger an anchoring effect, causing respondents to evaluate hypothetical alternatives in direct comparison (Furnham and Boo, 2011).

Overall, our results suggest that researchers should consider the effect of dispersion around a reference point when designing alternative options, as this element can interact with the framing of the SQ alternative and potentially bias willingness to pay estimates. This issue may become more significant when the reference alternative is visible or explicitly presented.

An ideal approach for researchers would be to avoid including variations from the reference point that are too extreme, as these could trigger a potential nonlinear dispersion effect. However, this strategy presents two significant challenges. First, it is very difficult to know in advance what the critical threshold is before conducting the DCE. Second, incorporating wide variations may be necessary to capture a broad range of policy options and define the choke price necessary for estimating the full extent of the demand function.

Then, we suggest a more practical approach. In the initial phase of the DCE, the researcher should seek to approximate what respondents would find plausible regarding the attributes of the good in question, relative to their reference point. Qualitative methods, such as focus groups or structured and semi-structured interviews, which are commonly used in the DCE preliminary stage, should include a part focused on assessing the realism of the choice card based on respondents' past experiences. For example, in our application, we asked in the semi-structured interviews we conducted how much the cost of a bluefin tuna fishing trip could realistically vary compared to another. The obtained approximation should be considered in defining the experimental design. Then, a pilot study should be conducted following the best practices (Johnston et al., 2017), including in this exercise a test for potential framing effects — for example, whether responses differ depending on whether the SQ is framed as a personal reference or an opt-out. The results of the pilot study should be used to preliminary assess a potential effect of the dispersion of the choice card, and if this is the case to update the experimental design.

In the analysis stage, the paper proposes alternative econometric models to investigate the effect of dispersion and produce robust welfare measure for policy decision making. However, the proposed models accounting for dispersion will only correct the bias in willingness to pay estimates if the relationship between the SQ effect and choice card dispersion is accurately specified. While achieving this in practice can be difficult, finding the best-fitting model is the challenge that every econometrician is subjected to. In this regard, while in our empirical application we could not identify the functional form of the relationship between dispersion and the SQ effect, we claim that the alternative models could be tested in future applications and refined in advanced simulation analysis.

Finally, we acknowledge some limitations of the study. First, even though our main contribution is methodological, in our empirical application the scale of the sample size may slightly limit the precision of some estimates. Another limitation of our study is that we cannot assess the impact of including both characterizations of the SQ alternative in the choice card. A valuable avenue for future research could be to explore how the SQ bias and related willingness-to-pay estimates are affected when respondents are presented with both the reference and opt-out alternatives alongside the hypothetical options. Such designs could help further isolate the behavioral mechanisms behind status quo choices and determine to what extent dispersion effects persist when both the reference and the opt-out options are simultaneously available. Moreover, extending the Monte Carlo simulations to scenarios in which both types of SQ alternatives are included could offer additional insight into how different sources of bias interact, and how model performance varies under more complex but realistic conditions.

CRediT authorship contribution statement

Fabio Cevenini: Writing – original draft, Visualization, Methodology, Formal analysis, Data curation, Conceptualization. **Sandra Notaro:** Writing – review & editing, Supervision, Conceptualization. **Silvia Ferrini:** Writing – review & editing, Supervision, Conceptualization. **Fabio Grati:** Funding acquisition.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. different specifications of the dispersion threshold

In Table A1, we show model results for different specifications of the threshold parameter τ. The specification we chose was the one that provided the highest log-likelihood when applied to reference-treated choice tasks.

Table A1
maximum likelihood estimates in willingness to pay space for the "EC-D threshold" model for different values of τ.

	$\tau = 1.5$	$\tau = 1.4$	$\tau = 1.3$	$\tau = 1.2$	$\tau = 1.1$
WTP Tuna (€)	283.34 (3.87)	283.34 (3.87)	318.36 (4.04)	323.72 (4.06)	317.87 (3.99)
WTP OF (€)	30.3 (4.69)	30.3 (4.69)	30.13 (4.63)	31.66 (4.93)	33.51 (5.18)

(continued on next page)

Table A1 (*continued*)

	$\tau = 1.5$	$\tau = 1.4$	$\tau = 1.3$	$\tau = 1.2$	$\tau = 1.1$
ASC ref	1.24 (11.88)	1.24 (11.88)	1.20 (11.29)	1.20 (11.04)	1.21 (11.17)
Cost	−0.009 (16.25)	−0.009 (16.25)	−0.009 (16.17)	−0.009 (16.22)	−0.009 (16.30)
Random parameters' standard deviations:					
ASC	0.35 (2.31)	0.35 (2.31)	0.35 (2.32)	0.36 (2.39)	0.34 (2.19)
Dispersion-induced SQ effect:					
λ	0.46 (2.02)	0.46 (2.02)	0.57 (2.62)	0.56 (2.72)	0.45 (2.34)
Goodness of fit:					
loglikelihood	−894.31	−894.31	−892.07	−892.73	−893.7
	$\tau = 1$	$\tau = 0.9$	$\tau = 0.8$	$\tau = 0.7$	$\tau = 0.6$
WTP Tuna (€)	329.23 (4.01)	303.06 (3.87)	257.5 (3.35)	254.24 (3.35)	245.39 (3.74)
WTP OF (€)	34.22 (5.28)	34.69 (5.17)	31.58 (4.81)	31.04 (4.81)	31.32 (4.85)
ASC ref	1.19 (10.09)	1.22 (11.39)	1.25 (10.59)	1.23 (9.42)	1.18 (8.76)
Cost	−0.009 (16.30)	−0.009 (16.20)	−0.009 (16.39)	−0.009 (16.33)	−0.009 (16.17)
Random parameters' standard deviations:					
ASC	0.33 (2.12)	0.33 (2.12)	0.34 (2.22)	0.34 (2.25)	0.34 (2.25)
Dispersion-induced SQ effect:					
λ	0.49 (2.38)	0.38 (1.93)	0.15 (0.91)	0.13 (0.84)	0.18 (1.24)
Goodness of fit:					
loglikelihood	−893.31	−894.25	−895.58	−895.66	−895.32

Appendix B. testing for asymmetrical preferences

In this appendix, we test our model estimates against multinomial logit estimates where we allow for asymmetrical preferences around the reference point as in Hess et al. (2008). More formally, the utility function in equation (2) is specified as:

$$V_{ni} = \boldsymbol{\beta X_i} = \beta^{+}_{Tuna}\, max\left(Prob_{ref} - Prob_i, 0\right) + \beta^{-}_{Tuna}\, max\left(Prob_i - Prob_{ref}, 0\right) + \beta^{+}_{OF}\, max\left(OF_{ref} - OF_i, 0\right) + \beta^{-}_{OF}\, max\left(OF_i - OF_{ref}, 0\right) + \beta_{cost}\, Cost$$

Where β^{+}_{Tuna} represent the marginal utility of gains in the probability of catching and keeping a tuna while β^{-}_{Tuna} is the marginal (dis)utility of losses in the same probability. β^{+}_{OF} and β^{-}_{OF} have the same meaning but for the attribute "catching and keeping a fish of another species". In other words, the model allows the separate identification of the marginal utility for positive and negative deviations from the individual-specific reference point. Table B1 reports the estimation results in willingness to pay space, separately for the two specifications of the SQ alternative. Comparing the results with the ones reported in Table 3, we first note that the model fit does not significantly increase with respect to a simple Multinomial logit when the reference is included as the SQ ($LR_1 = 0.08$). When we use an opt-out specification of the SQ alternative, we observe a decrease in the likelihood of the model with respect to a multinomial logit, even with a more restricted specification (5 estimated parameters against 4).

Table B1
Estimation results of a multinomial logit allowing for asymmetrical preferences around the reference point as in Hess et al. (2008). The heteroskedastic robust T-ratios are reported in brackets.

	"Your usual trip" option	"Not to go fishing" option
Increasing WTP Tuna (€)	205.87 (1.72)	255.69 (1.90)
Decreasing WTP Tuna (€)	−218.62 (2.04)	−77.51 (0.79)
Increasing WTP OF (€)	28.29 (2.45)	40.12 (3.75)
Decreasing WTP OF (€)	−36.07 (1.48)	−28.39 (1.26)
ASC ref	1.28 (7.86)	—
ASC Opt-out	—	−2.74 (11.60)
Cost	0.009 (16.66)	0.01 (19.18)
Number of parameters	6	6
loglikelihood	−896.69	−729.71
AIC	1805.38	1 471.43

Data availability

The data and the code will be made available at https://github.com/cevefabio.

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