Estimating the *Ex-ante* Recreational Loss of an Oil Spill

using Revealed and Stated Preference Data

Ana Faria Lopes, PostDoctoral Researcher

Centre for Ocean and Society, Christian-Albrechts-University Kiel, Neufeldstraße 10, 24118 Kiel, Germany; *ana.f.lopes@ae.uni-kiel.de*

John C. Whitehead, Professor

Department of Economics, Appalachian State University; *whiteheadjc@appstate.edu*

ABSTRACT

This paper combines data to estimate the *ex-ante* recreational impact of oil spills. Using a unique contingent behavior question, our application highlights the gains in combining stated and revealed preferences. We consider both a reduction of the available choice set and of perceived site quality. We show that omitting perceived site quality leads to low welfare losses while omitting alternative specific constants leads to high welfare losses. Overall, we find recreational losses due to potential oil spills in Norway ranging from 347 and 524 NOK (32 to 49 US dollars) per person across the four oil spill scenarios. (Q51)
1. Introduction

Coastal areas, especially those near heavy oil tanker traffic or oil rigs, are under increasing pressure due to economic activity offshore. One of these threats is the increased risk of oil spill accidents, whose consequences imply both use and non-use value losses. A handful of studies estimate the losses due to oil spill accidents in terms of non-use (Carson et al., 2003; e.g., Bishop et al., 2017) or use values (e.g., Winkler and Gordon, 2013). Where the losses or gains in terms of recreational value are likely to be substantial (e.g., Alvarez et al., 2014), revealed preference (RP) methods are typically used, namely the travel cost method.

Two different approaches have been used to retrieve aggregate welfare change estimates in the context of a multi-site model. The first approach is to calculate the number of lost recreational trips and multiply them by the value of a beach trip (English et al., 2018; Glasgow and Train, 2018). The second approach is to estimate the change in welfare per trip due to the presence of an oil spill and multiply it by the number of total trips (Hausman, Leonard and McFadden, 1995; Alvarez et al., 2014). Both of these approaches focus on the impact of an oil spill ex-post.

Neither of these approaches work if the research contemplates the ex-ante impact of an oil spill. Estimating changes in use values prior to an oil spill happening is informative to the decision-making process prior to motivate the implementation of preventive or adaptive measures to oil spill accidents (Laurans et al., 2013). Parsons (2008) is the only existing study to estimate the ex-ante impact of an oil spill. He assumes that the recreational loss from an oil spill is the result of site closure, as previously available recreational sites are now soiled and closed. However, this approach implicitly assumes that individuals would incur no loss if the beach is not closed. Yet, in the event of no beach closure, preferences towards oil spill avoidance might lead individuals to opt-out from engaging in beach recreation altogether or decide instead to engage in a completely different
recreational activity. This type of behavior can be elicited using contingent behavior (CB) questions (i.e. stated behavior) by asking individuals how they would change their behavior given a hypothetical scenario on overall site quality (e.g. Landry et al., 2012), attributes (e.g., Adamowicz et al., 1997), travel cost (e.g., Azevedo, Herriges and Kling, 2003), or access to sites (e.g., Grijalva et al., 2002).

Given the dual potential of observed and stated behavior to estimate the recreational impact of an oil spill, one can combine the two types of data. RP-SP data are complementary in regards to their strengths and weaknesses (Whitehead et al., 2008). RP data is grounded on the individuals’ observed choices, whereas SP data is criticized due to its hypothetical nature (Scott, 1965). SP data can be collected with an experimental design to introduce variation in attribute levels while modeling RP data is challenging due to the multicollinearity in the attribute data. In recognition of their duality, the number of applications of joint RP-SP data grew, especially after Adamowicz et al. (1994). The combination of the RP-SP data generally results in a better fit of the models and the possibility to estimate welfare losses from attributes that would not be possible from using datasets separately. However, there may be differences across RP-SP datasets that should be accounted for, for example, if the respondents are more uncertain when facing SP scenarios (Whitehead and Lew, 2020) or exhibit attribute non-attendance (Hindsley et al., 2022).

This paper aims at combining RP-SP data to estimate the recreational impact of four hypothetical oil spills in Norway. The focus on losses from different potential oil spill scenarios is similar to Egan et al. (2022) but our paper analyzes discrete choices rather than counts. While we focus on oil spill impacts, our discussion applies to any future threat that changes the quality, travel cost, and/or available choice set of goods and services. Previous RP-SP studies have considered the ex-ante impact of wind farms (Landry et al., 2012), water flow (Loomis, 1997), or forest fires (Simões,
Barata and Cruz, 2013) on site quality, but no study to date has considered a simultaneous change in the choice set and site quality. Our application is the first to do so by considering: a reduction in the available choice set, and a reduction in perceived beach site quality. We estimate welfare losses ranging from 347 and 524 NOK per choice occasion and per person across the four oil spill scenarios. We find that combining RP-SP data has several advantages in terms of welfare analysis: omitting perceived site quality when using RP data leads to low welfare losses while omitting alternative specific constants when using SP data leads to high welfare losses. We also investigate how robust our RP-SP model is given different underlying preferences assumptions and different practices of combining RP and SP data.

In previous CB surveys, individuals are given a limited choice set in the CB question: the option to visit one or more recreational sites within the same study area (e.g. Truong, Adamowicz and Boxall, 2018), the option to “stay at home” (Yi and Herriges, 2017), or the option of postponing the trip (Parsons and Stefanova 2011). Yet, when faced with an oil spill, an individual might opt for an activity or site that is not available in the CB question (e.g. a park, hike, forest). Stafford (2018) shows the importance of considering appropriate outside or opt-out options to obtain unbiased welfare estimates. We build on existing efforts by expanding the individual’s choice set including other recreational sites and refer to this as “multinomial” CB data. This paper is the first to jointly estimate RP site selection data and multinomial CB data in a Random Utility Model framework.

The remainder of this paper is structured as follows. Section 2 discusses the overall motivation of the paper and the chosen approach. Section 3 presents the case study and survey data. Section 4 reports the estimation results and welfare estimates. Section 5 provides a sensitivity analysis. Section 6 concludes.
2. Methods

Any losses arising due to an oil spill accident entail both use and non-use value losses. If non-use value losses comprise the majority of the welfare loss at stake, researchers should use SP methods. Both the contingent valuation method (e.g., Carson *et al.*, 2003; Loureiro and Loomis, 2013) and choice experiments (e.g., Casey, Kahn and Rivas, 2008; Tuhkanen *et al.*, 2016) have been used to estimate the non-use value losses due to an oil spill.

Alternatively, this paper focuses on estimating recreational (use) value losses from an oil spill. In such a context, RP methods are more appropriate. Both the travel cost method (e.g., English *et al.*, 2018) and the hedonic pricing method (e.g., Cano-Urbina, Clapp and Willardsen, 2019) have been applied to estimate the use-value losses due to an oil spill. While the former tends to focus on impacts on local recreation, the latter has estimated the losses arising from oil spills in the housing market (Winkler and Gordon, 2013), fish prices (Domínguez Alvarez and Loureiro, 2013), or wages (Aldy, 2014).

Out of the methods presented, the Travel Cost Method is the most appropriate in our context due to its exclusive focus on recreation. To infer how to estimate the recreational impact of an oil spill on recreation, we identified prior studies that do so in different contexts. These studies follow two different approaches to estimate the aggregate recreational loss due to an oil spill.

One approach is to calculate the value of a trip, usually in terms of consumer surplus, and multiply it by the number of lost trips due to the oil spill, thus yielding aggregate losses. Bonnieux and Rainelli (2003), Stratus Consulting Inc. (2010) and English et al. (2018) follow this approach. For example, English et al. (2018) estimate the value of a lost user day to be $37.23 and multiply this
value by the estimate of lost user days (10 million), which equals $379 million in aggregate losses due to the Deepwater Horizon oil spill in 2010.¹

An alternative approach is to calculate the welfare loss per trip or choice occasion due to an oil spill. The aggregate loss due to the spill is estimated by multiplying the loss per trip by the total number of trips or choice occasions (Hausman, Leonard and McFadden, 1995; e.g., Alvarez et al., 2014; Whitehead et al., 2018). Whitehead et al. (2018) estimate a change in consumer surplus due to the oil spill of $43 and multiply it by 4.82 million households, which yields $207 million in aggregate damages due to the Deepwater Horizon oil spill.

However, the two approaches require some form of historical trip data after an oil spill has occurred. Estimating the loss of an oil spill before it has occurred may be useful in the sense of motivating the creation or improvement of prevention measures, by identifying trade-offs or informing cost-benefit analyses (Laurans et al., 2013). Out of the studies identified, Parsons (2008) and Egan et al. (2022) are the only studies to estimate the ex-ante impact of an oil spill, but Egan et al. (2022) uses count data. Parsons (2008) uses discrete data and estimates the loss per trip due to a hypothetical oil spill in South Padre Island (US) by assuming that an oil spill would reduce welfare due to the closure of soiled beaches. Using RP data, the author uses the discrete choice model and estimates compensating surplus by reducing the choice set available to recreationists.

If we follow this approach, then it suffices to apply a discrete choice model to visitation data. In a Random Utility Model (RUM), for individual $i$, let $M_i$ denote income, $C_{ij}^0$ represent travel cost associated with getting to beach $j$ and $Q_{jt}^0$ denote quality of site $j$. The utility of going to site $j$ at choice occasion $t$ is given by:

$$U_{ijt} = V_{ijt}(M_i - C_{ij}^0, Q_{jt}^0) + \frac{1}{\sigma} \varepsilon_{ijt},$$

[1]
where \( \sigma \) is the scale parameter, which is usually normalized to one.

With the above framework in place, we can calculate the compensating surplus (CS) given various scenarios of beach closure. Let \( J \) represent the choice set before the oil spill, and \( J_1 \) the reduced choice set after the oil spill. To estimate the loss, we calculate the CS per choice occasion as derived by Small and Rosen (1981):

\[
E(CS) = \frac{1}{\beta_1} \left\{ \ln \left[ \sum_{j \in J_1} e^{V_{ij}t(M_i - C_{ij}^0, Q_{jt}^0)} \right] - \ln \left[ \sum_{j \in J} e^{V_{ij}t(M_i - C_{ij}^0, Q_{jt}^0)} \right] \right\}, \tag{2}
\]

where \( \beta_1 \) denotes the marginal utility of money. This estimate of CS is a special case that assumes the indirect utility to be linear in parameters. See Hanemann (1984) and Haab and McConnell (2002).

In this approach, a scenario that does not imply beach closure yields a CS estimate of zero. This approach implicitly assumes that a recreationist would not change their behavior if the beach is not closed.

However, recreationists may have preferences towards oil spill avoidance. Such changes might occur even if their preferred or last visited beach remains open and not soiled. For example, recreationists might prefer either to stay at home, or to visit another beach and/or recreational site. In fact, there was evidence in the Deepwater Horizon oil spill that “individual perceptions of or uncertainty about conditions in the Gulf altered the (…) recreation behavior” of recreationists, “even in areas where the oil never actually made it to local beaches” (English et al., 2018). English et al (2018) account for this by calibrating alternative specific constants using data during and after the oil spill. Glasgow and Train (2018) also propose the idea of welfare losses arising because recreationists “anticipated that the sites would or might be degraded.” We interpret this as evidence.
of a reduction in the individuals’ perceived quality of the beach sites, even if the beach is not actually soiled.

When considering the *ex-ante* impact of an oil spill, the researcher cannot capture behavior due to changes in perceptions with RP data alone. Instead, such behavior can be elicited using SP data. Whereas in the RP data there is no variation in the perceived site quality, in the SP scenario we assume that beach sites near the hypothetical oil spill suffer a drop in perceived site quality from $Q_j^0$ to $Q_j^1$.

Hence, welfare losses due to an oil spill are given by: 1) a reduction in the choice set, and 2) a reduction in perceived site quality. The resulting CS per choice occasion due to an oil spill is given by:

$$E(CS) = \frac{1}{\beta_1} \left[ \ln \left( \sum_{j \epsilon J_1} e^{V_{ij}(M_i - C_i^0, Q_j^1)} \right) - \ln \left( \sum_{j \epsilon J_0} e^{V_{ij}(M_i - C_i^0, Q_j^0)} \right) \right].$$

Estimating CS *ex-ante* is only possible with SP data. The combination of RP-SP data has several advantages compared with the use of SP data alone. One of the advantages is attenuation of hypothetical bias stemming from the SP data source (Whitehead and Lew, 2020). SP data is particularly vulnerable to hypothetical bias due to the unfamiliarity associated with the hypothetical scenarios (Whitehead *et al.*, 2008). Thus SP data may be calibrated to match actual market shares from RP data (e.g., Revelt and Train, 1998). On the other hand, the hypothetical nature of SP data enables the creation of different policy scenarios to be considered, which is not possible with RP data alone (Whitehead *et al.*, 2008). RP data frequently suffer from lack of variation or high multicollinearity, while SP data introduces greater variation in the levels of the attributes.

When combining RP and SP data, the majority of studies combine count rather than discrete data. For example, Egan *et al.* (2022) is a recent study combining RP-SP count data to analyze
recreational value losses due to potential oil spills. When it comes to discrete data, two types of SP data may be combined with RP data: discrete choice experiments (e.g., Whitehead and Lew, 2020) or contingent behavior (e.g., Zimmer, Boxall and Adamowicz, 2012).

While DCEs introduce generic alternatives but allow for greater variation across attribute levels, CB alternatives include actual recreational sites but introduce fewer changes in attribute levels. In CB questions, individuals are presented with a scenario featuring changes in site quality, travel cost and/or choice sets. Individuals are then asked to either anticipate how many trips they expect to make to each of the available sites (Jeon and Herriges, 2010; Zimmer, Boxall and Adamowicz, 2012; Yi and Herriges, 2017; Truong, Adamowicz and Boxall, 2018), or answer how their behavior would change relative to some past visit (Loomis, 1997; Boxall, Englin and Adamowicz, 2003; Parsons and Stefanova, 2011). Given our interest to mimic actual choices, we opt for combining RP data with CB, wherein we elicit behavior relative to the last recreational visit.

**Econometric Approach and Scale differences**

In order to combine RP-SP data, one could assume the utility function in Equation 1 to hold in both datasets. This should be reflected by the equality of parameters of the travel cost $C_{ij}$ and environmental quality $Q_j$ that define the indirect utility function, $V_{ijt}$, across RP and SP data. Then one could stack the data and jointly estimate Equation 1 in a “naïve” way.

We call it “naïve” because this approach ignores potential scale differences across RP-SP datasets. As illustrated by Swait and Louviere (1993), SP and RP data may appear to lead to different parameters of the utility function due to scale differences. That is, the scale parameter $\sigma$ in Equation 1 is dataset-specific, $\sigma^{RP} \neq \sigma^{SP}$. Scale differences across RP-SP datasets may be due to various factors: “random noise” (Hensher and Bradley, 1993), rank order or fatigue effects (Bradley and
Daly, 1994), choice uncertainty (Lundhede *et al.*, 2009) or different “effect of unobserved factors \(\ldots\) between revealed and stated preferences” (Morikawa, 1994). To be able to compare and jointly estimate SP and RP data, differences in scale should first be accounted for (Swait and Louviere, 1993).

We can modify Equation 1 for the case of RP-SP data. We can combine datasets if we expect that the underlying preferences are the same, that is, the indirect utility functions are the same in both datasets (Swait and Louviere, 1993). For the RP data, the utility of alternative \(j\) for individual \(i\) is represented by:

\[
U_{ij}^{RP} = V_{ij}(M_i - C_{ij}, Q) + \frac{1}{\sigma_{RP}} \varepsilon_{ij}. \tag{4}
\]

In SP data, we introduce variation in the environmental quality. Hence, the utility function is expressed as:

\[
U_{ij}^{SP} = V_{ij}(M_i - C_{ij}, Q_j) + \frac{1}{\sigma_{SP}} \varepsilon_{ij}. \tag{5}
\]

Let the indirect utility function be linear in parameters as follows:

\[
V_{ij} = \beta_0 + \beta_1 C_{ij} + \beta_2 Q_j, \tag{6}
\]

where \(\beta_0, \beta_1\) and \(\beta_2\) are the parameters of interest which are the same in both the RP-SP dataset.

When jointly estimating RP-SP data, we can estimate the relative scale parameter so long as one of the scale parameters is fixed (Adamowicz *et al.*, 1997). By fixing the RP scale parameter \(\sigma^{RP}\) to one, the resulting parameters are estimated relative to the RP data (e.g. Adamowicz *et al.*, 1997; Cheng and Lupi, 2016). The estimated scale parameter for SP data varies across studies: between 0.05 and 0.22 in Jeon and Herriges (2017), 0.13 in Adamowicz *et al.* (1997), between 0.12 and 0.45 in von Haefen and Phaneuf (2008), 0.59 in Truong *et al.* (2018), 0.62 in Cheng and Lupi (2016),
between 0.61 and 0.70 in Haener et al. (2001), and 0.76 in Whitehead and Lew (2020). In many of these studies, the relative scale parameter is lower and statistically different from one, which implies greater variance in the SP data (Whitehead et al., 2008).

Several econometric models allow the estimation of the scale parameter when jointly estimating RP-SP data. One approach is to use the “nested logit trick” (Hensher and Bradley, 1993; Hensher, Rose and Greene, 2008). In such models, the RP-SP data are in different branches, and we retrieve the scale parameter through the dissimilarity parameter across branches. In the last decade, other approaches have been applied, such as the conditional logit model with scale (Haener, Boxall and Adamowicz, 2001; Cheng and Lupi, 2016; Truong, Adamowicz and Boxall, 2018), the latent class model (Jeon and Herriges, 2017), the generalized mixed logit model (Cha and Melstrom, 2018), or the Error Component Mixed Logit model (Abildtrup, Olsen and Stenger, 2015). However, the choice of statistical model seems to have modest to negligible effects on welfare estimates (Whitehead and Lew, 2020).

The mixed logit models allows for preference heterogeneity, while the conditional logit model does not. The mixed logit model also allows for efficient estimation when data is comprised of repeated choices per individual rather than single choices (Revelt and Train, 1998). When only single choices are available, we apply the conditional logit model by assuming the indirect utility function in Equation 6. When repeated choices are available, we apply the mixed logit model by following up on Equation 6 as follows:

\[ V_{ijt} = ASC_{ij} + \beta_1 C_{ij} + \beta_2 Q_{jt} \]  \[ 7 \]

We allow coefficients to be randomly distributed. We assume that the travel cost (TC) coefficient follows a log-normal distribution. By assuming a lognormal distribution, we ensure that the
distribution of the change in welfare estimate does not have infinite moments (Daly, Hess and Train, 2012). The coefficient associated with travel cost is expressed as follows:

\[ \beta_{1i} = -\exp(\tilde{\beta}_1 + \sigma_1 \cdot \varepsilon_{1i}), \]  

where \( \tilde{\beta}_1 \) is the mean of the log-normal distribution, \( \sigma_1 \) is the standard deviation of \( \beta_1 \), and \( \varepsilon_{1i} \) follows a normal distribution across individuals. The parameters to be estimated are \( \tilde{\beta}_1 \) and \( \sigma_1 \).

We assume that the parameter associated with environmental quality (Q) follows a normal distribution. The random coefficient associated with environmental quality (Q) is expressed as follows:

\[ \beta_{2i} = \tilde{\beta}_2 + \sigma_2 \cdot \varepsilon_{2i}, \]  

where \( \varepsilon_{2i} \) is individual-specific and follows a normal distribution. The parameters to be estimated are \( \tilde{\beta}_2 \) and \( \sigma_2 \).

3. Data

The study site concerns the Jæren beaches on the south-western coast of Norway (illustrated in Figure 1). Annual visitation to the Jæren beaches is estimated to be at least 600,000 visits (Sveen, 2018). Oil spills are a constant threat due to heavy marine traffic along the coast, as oil tankers navigate as close as three kilometers from the coast. Since 2011, the Norwegian Maritime Authority has recorded a total of 132 cargo ship accidents in the jurisdiction of the Rogaland county. The most alarming ship accident occurred in February 2017 when the ship “Tide Carrier” was grounded just two kilometers away from the Jæren beaches but no oil was spilled. The environmental damage would have been considerable if the oil aboard the ship had spilled (around 600 m\(^3\) of heavy oil and around 300 m\(^3\) of diesel oil).
To collect visitation data, we conducted an original household survey during October to November in 2018. The survey instrument was based on previous surveys (Parsons, Massey and Tomasi, 1999; Hicks and Strand, 2000; Yeh, Haab and Sohngen, 2006; e.g. Bin et al., 2007; Lew and Larson, 2008; Chen, 2013; Leggett et al., 2014; Bujosa et al., 2015; Matthews, Scarpa and Marsh, 2018). Pre-testing included two pilot surveys in early 2017 and 2018, expert feedback from valuation experts and stakeholders, and focus groups in early 2018 as well as individual interviews in September 2018 to test the survey instrument. A national survey company sampled residents in the Rogaland county from their national web-panel. The dataset comprises 984 respondents, 647 of which had visited the study site in the previous four months and were kept for data analysis. The response rate was 25.9%. More details about the survey design process, survey implementation, and data description is available in Lopes and Mariel (2021). Descriptive statistics and a comparison with county statistics are available in Appendix Table A4.

The survey is organized into four sections. In the first two sections of the survey, we collect RP visitation data. In the third section we elicit the individuals’ preferences towards oil spill aversion using a CB question. In the final section, we ask individuals to report their household and individual characteristics.

**Revealed Preference Data**

In the RP sections of the survey, individuals report two types of visitation data: all their beach visits during the summer season, and their last visited beach during the same season. The summer season consists of 123 days, from May to August. The last visited beach constitutes a single choice and it is less prone to recall bias. In total, the 647 sampled individuals visited beaches a total of 5985 times during the summer season. The number of beach visits during the summer season ranges from 1 visit (96 individuals) to 123 visits (3 individuals). The mean number of beach visits is 9.25 (median
is 5 visits). When modeling the last visited beach, we assume individuals face the 26 beach alternatives (shown in Figure 1 below).

We construct the travel cost $C_{ij}$ for individual $i$ associated with beach $j$ as follows:

$$C_{ij} = 2 \ast (0.79 \ast d_{ij} + 0.33 \ast w_i \ast t_{ij})$$

[10]

Travel cost is calculated per person and per visit. We measure the one-way driving distances ($d_{ij}$) and times ($t_{ij}$) from the reported postal codes of each individual to each beach using the Google Maps API tool. Cost per kilometer of driving a diesel car is assumed to be 0.79 NOK per kilometer (0.053 liters per kilometer times a price of 15.04 per liter of diesel in August 2018). We assume the opportunity cost of time to be 33% of the individual’s hourly wage rate ($w_i$). We input annual mean net income (376,897 NOK) for individuals with missing income data. Mean travel cost for the sample is 175 NOK (median=144).

**Stated Preference Data**

In the SP section of the survey, individuals are randomly assigned to one oil spill size: either a small, medium, large, or very large oil spill, and asked how their site choice would change. The hypothetical oil spill occurs due to a ship grounding south of the Jæren beaches. The company DNV-GL simulated four oil spill dispersion scenarios given the quantity of oil spilled in each scenario, local ocean currents and the origin of the oil spill, which was chosen given local marine traffic intensity. Figure 1 illustrates the four oil spill sizes as well as the oil dispersion in each case.

[[Insert Figure 1 here]]

The design of the CB question builds upon previous CB surveys, such as Loomis (1997), Boxall et al. (2003) and Parsons and Stefanova (2011). We build on previous studies by expanding the opt-
out options: studies have only considered either not to engage in the recreational activity (e.g., Adamowicz et al., 1997) or to stay at home (e.g., Adamowicz, Louviere and Williams, 1994). We designed this CB question to mimic a real-life recreational choice, by giving individuals not only the options of visiting another beach in the study area or stay at home, but also the options to engage in other recreational activities or a different recreational site.

An example of the CB question for a large oil spill is in Figure 2. The elicited behavior is relative to the respondent’s last beach visit, hence visiting the same site is not always an option in the contingent behavior question if the previously visited beach is now closed.

Each oil spill was described in terms of the kilometers of coastline soiled, time of beach closure, and time required for the ecosystem to recover from the oil spill (i.e., underlined text in Figure 2). Recreational impacts increase with oil spill size, as described in Table 1. In the case of a small oil spill, the choice set includes 29 alternatives: the initial 26 beach sites and 3 opt-outs (i.e., Go to another beach outside Jaeren, Visit another recreational site, or Stay at home / Do something else). For larger oil spill sizes, we assume the choice set of beach alternatives decreases.

The response to the CB question comprises the single choice in SP data. Table 2 summarizes the number of responses in the CB data. Following up on the discussion in Section 2, if the loss of recreational values is exclusively due to beach closure, we would expect all respondents to visit the same beach in the case of a small oil spill, which is not the case. As expected, the number of individuals who choose to visit the Jaeren beaches (i.e., Visit the same beach, or Go to another Jaeren beach) decreases as the oil spill size increases. In parallel, the number of people choosing to stay at home increases with the oil spill size.
Almost half of the respondents (43%) chose one of our two opt-outs in the contingent behavior question (i.e., Go to another beach outside Jæren, or Visit another recreational site). These respondents are then asked to write the name of a specific recreational site they would visit in a follow-up (open-ended) question. This type of question is an example of Lupi et al. (2020, p. 309)’s recommendations to gauge how wide the choice set is. Respondents indicated 58 different recreational sites, such as mountains, forests and lakes (which jointly serve as alternative 29), or other local beaches (which serve as alternative 28). These alternative recreational sites are illustrated in the “Substitute Sites” dots in Figure 1, and comprise our multinomial SP data.

We calculate individual and site-specific travel costs using Equation 10 for these different recreational sites. These travel costs are slightly lower than the travel costs to visit the Jæren beaches: the average TC to visit another beach outside Jæren (alternative 28) is 92 NOK, and to visit another recreational site (alternative 29) is 114 NOK.

When contemplating how to measure site quality, a more direct approach would be to include oil spill size dummies for a small, medium, large and very large oil spill. However, it is not possible to identify the oil spill dummies due to them being confounded by the reduction in the choice set. Instead, we introduce a variable ($Q_j$) that represents the proximity of each beach to the oil spill. We calculate the Euclidian distance from the oil spill to each of the beaches or recreational sites given the oil spill scenario. In the case of inland recreational sites or when selecting the stay-at-home option, proximity was set equal to the maximum distance calculated to any inshore recreational site (125 kilometers). The average proximity from each beach site to the oil spill is 33 kilometers (median=22). The inclusion of the proximity variable is only possible due to the availability of SP data.
4. Results

We present the results for three scenarios: using only SP data, using only RP data, and combining RP and SP data. In the RP-SP mixed logit model, we combine the SP question with the RP data from the last visited beach (i.e., single choice) since the elicited behavior in the SP question is relative to the respondent’s last beach visit. Our RP-SP sample is comprised of 1294 choice occasions among 647 individuals. The former includes 647 beach visits (RP data) and 647 responses to the CB question (SP data).

We also present an alternative econometric strategy that makes use of the repeated choices in the RP data, following the spirit of English et al. (2018). Prior to estimating the RP-SP model, we use the reported number of all beach visits during the summer season to estimate a mixed logit model using RP data. We obtain the ASCs estimates for the 26 beach alternatives since the ASCs yield more information about the average quality of each site, and then fix these values in the RP-SP model that uses the last visited beach. The ASC parameters should be more credible when estimated by analyzing repeated choices rather than a single choice.

We use the full set of ASCs, travel cost and proximity to explain site choice as specified in Equation 7. We chose to omit observed site attributes to explain site choice (e.g., parking) since: 1) the coefficients associated with the ASCs already capture the average utility of observable and unobservable characteristics of each site; 2) including the full set of ASCs in detriment of observed site attributes allows us to control for unobserved attributes, thus avoiding problems such as endogeneity (Murdock, 2006; Klaiber and von Haefen, 2018); and 3) our aim is to calculate site closure welfare estimates rather than changes in attribute levels, for which we only need ASCs.
The 26 Jæren beaches are coded from 1 to 26 (ASC1 is fixed at zero). The utility of staying at home relative to ASC1 is captured by ASC27, the utility of going to another beach is captured by ASC28, and the utility of going to another recreational site is captured by ASC29.

The novelty of our paper is through the inclusion of the Proximity variable and ASC28 and ASC29, which is possible through the inclusion of SP data. Moreover, the stay-at-home option (ASC27) is also seldom included in discrete data choice models applied to recreation.¹²

Preferences for proximity are expected to exhibit diminishing marginal utility. That is, a marginal increase in proximity (i.e. by one kilometer) is expected to generate greater utility when the individual had previously visited a beach near the oil spill, in contrast to a beach farther away. We account for this with the squared root of proximity that allows for diminishing marginal utility.

We apply the conditional logit model when using the SP data or RP data, and the mixed logit model when using the RP-SP data. The results for the SP, RP, and RP-SP data are summarized in Table 3. The fourth model is the alternative econometric specification that fixes the ASC parameters to those obtained with the repeated RP data. The estimated parameters are in utility space.

[[Insert Table 3 here]]

There are considerable gains in combining RP and SP data in our context. We can estimate a mixed logit model rather than a conditional logit model since we have two choice occasions per individual. This allows for preference heterogeneity in the travel cost and proximity variables. The travel cost parameter obtained with the RP-SP model is similar in magnitude to that obtained using RP data alone. The specification we use as the baseline is the one wherein we fix the ASCs to those obtained using repeated RP data (fourth model in Table 3). This alternative model performs slightly worse in terms of fit (i.e., AIC or BIC) compared with the RP-SP without fixed ASCs, but it is a plausible
model from a theoretical point of view and it converges successfully unlike the RP-SP model without fixed ASCs.

Across all models, the coefficient associated with the travel cost variable is negative and statistically significant, thus as expected respondents exhibit negative price sensitivity to beach visits. Individuals also exhibit preference heterogeneity regarding the travel cost variable, since the standard deviation ($\sigma_1$) is statistically significant. As specified in Equation 12, the coefficient associated with the travel cost variable when using RP or RP-SP data is assumed to be negative log-normally distributed in the mixed logit models, hence the travel cost variable may be transformed as $-\exp(-4.033) = -0.018$. The resulting parameter is approximately six times higher than the coefficient obtained when using the SP data ($\beta_1 = -0.002$).

The coefficients associated with staying at home (ASC27), visiting another beach outside Jæren (ASC28) or going to another recreational site (ASC29) are negative and statistically significant in the RP-SP models ($\overline{ASC}_j < 0, j = \{27,28,29\}$). This implies that staying at home or visiting a different recreational site generate disutility relative to visiting the first beach. Staying at home brings the highest disutility ($\overline{ASC}_{27} = -7.96$).

As expected, visitors are averse towards recreating near the coast after an oil spill has occurred. The coefficient associated with proximity of recreational sites to the oil spill (in kilometers) is positive and statistically significant ($\beta_{PROX} = 0.72$). This means that respondents get utility by visiting beaches away from the oil spill. The standard deviation of the proximity distribution is statistically significant, hence respondents exhibit preference heterogeneity regarding the proximity variable.

As discussed in Section 2, if the scale differs across the two datasets, the estimated coefficients from the SP data are not directly comparable with the coefficients from the RP or RP-SP data. We
find that the estimated scale parameter (0.744) is statistically different from one (t-statistic of 

\[(0.744 - 1)/0.08 = -3.2\].

**Welfare Estimates**

Because the chosen specification includes the non-linear transformation of the proximity variable, 

the marginal WTP per kilometer is given by 

\[-\frac{\beta_2 i}{2\sqrt{\text{PROX}}} \cdot e^{\beta_1 i} \cdot \frac{1}{2\sqrt{\text{PROX}}}\]  

At the average proximity of 33 kilometers, the mean marginal WTP is 6.19 NOK per person to get 1 kilometer farther away from the oil spill (median=2.93). When proximity is close to zero, marginal WTP per kilometer ranges between -85.2, and 665 NOK. When considering recreational sites farther away from the spill (100 km away), marginal WTP ranges from -8.52 to 66.5 NOK. The negative WTP may be explained by the phenomenon of solidarity visits. Loureiro et al. (2006) reported a “solidarity effect” in the case of the Prestige oil spill, as some visitors specifically choose to visit the site soiled by the oil spill given their interest and desire to experience the Prestige oil spill by themselves.

In Table 4, we report the CS estimates when using SP data, RP data and RP-SP data. When using RP-SP estimates, we obtain a CS of -347 NOK for small, -479 NOK for medium, -520 NOK for large, and -524 NOK to avoid a very large oil spill. When considering the RP-SP model without fixed ASCs, then CS estimates are around 70% higher.

[[Insert Table 4 here]]

Throughout Section 2, we argue that the combination of RP-SP data allows for the estimation of perceived quality of beaches. As seen in Table 4, welfare change estimates when excluding proximity (RP data) range from gains of 20 NOK to losses of 123 NOK, while using RP-SP data
and estimating the proximity parameter leads to losses from 347 to 524 NOK per person and per visit.

In Section 2, we also argue that grounding SP outcomes on actual choices is one of the advantages of combining RP-SP data. Indeed, estimated losses using the SP data alone are around 2 to 3 times higher (between 734 and 1463 NOK) than estimated losses using the combined RP-SP data (between 347 and 524 NOK). Adamowicz et al. (1994) and Hindsley et al. (2022) also find that estimated welfare measures are higher by several orders of magnitude using SP data compared with RP or RP-SP data.

5. Discussion

The analysis above shows the advantages of combining RP and SP datasets. Because we aim at mimicking real-life decisions, we choose to ask a contingent behaviour question rather than choice experiments, which would have given more statistical information in the form of repeated choice occasions. At the same time, we try to minimize the cognitive burden and recall bias by asking for the respondents’ last beach trip, and then complement this by asking for respondents to report all beach trips during the summer season, as per Lupi et al. (2020)’s recommendations. To capture the ASCs for all possible substitutes to beaches, we would have to ask respondents to report recreational trips to up to hundreds of recreational sites (e.g. beaches, forests, mountains, etc), which would yield little statistical information about the beach sites of interest and increase burden for respondents. Instead, we bundle these alternatives in ASC28 and ASC29.

We choose to analyze respondents’ last visited beach, as well as the contingent behavior question that uses this last visited beach as a reference. We then compare the two models (RP and SP) and combine the data (RP-SP). In our context, there are significant gains from combining datasets
because neither RP nor SP data alone can provide a complete set of relevant parameters. This also results in large differences in welfare estimates due to the inability to account for all structural parameters when we estimate RP and SP data separately. When we estimate SP data with a single choice occasion, we are unable to estimate a full set of ASCs for beach sites. When we estimate RP data with lack of variation in environmental quality, we are unable to estimate the proximity variable as well as ASCs for staying at home (ASC27) nor going to another recreational site (ASC28 and ASC29). The omission of the proximity variable when modeling RP data is problematic.

Combination of different datasets presumes that the underlying preferences are the same across datasets (i.e., the indirect utility function in Equation 7). To check whether datasets can be combined, one can use the Likelihood ratio test as proposed by Swait and Louviere (1993). We cannot use this test because the RP and SP models are too different (e.g., the joint model can account for standard deviations of ASC27, ASC28, ASC29 and Proximity variable) and the joint RP-SP model largely outperforms the separate estimation of RP and SP models. Instead, we can see in Table 3 that the travel cost variable was substantially different across datasets even after accounting for scale. If the common parameters are still different after scale is accounted for, it begs the question of whether the datasets should be combined in the first place.

In fact, it is quite common in RP-SP combinations for the travel cost coefficient to differ in RP and SP datasets. This applies for both discrete data (e.g., Zimmer, Boxall and Adamowicz, 2012) and count data (e.g., Kipperberg et al., 2019) combinations. To test for differences, a common practice is to include an interaction term between travel cost and either RP or SP data and then test for statistical significance (e.g., Loomis, 1997; Zimmer, Boxall and Adamowicz, 2012). Less common approaches include using a latent class model to separate by internally consistent and inconsistent
groups (Jeon and Herriges, 2017), or using a scale constructed as the ratio of the travel cost variables in RP and SP models (von Haefen and Phaneuf, 2008). Many of these approaches find that it is only the travel cost that differs across datasets, while other parameters are not statistically different. When the interaction term between travel cost and a dummy representing either RP or SP is statistically significant, many authors conclude that there might be hypothetical bias in SP data rather than preferences differing (Zimmer, Boxall and Adamowicz, 2012; Simões, Barata and Cruz, 2013; Kipperberg et al., 2019; Hindsley et al., 2022).

Moreover, the strategies employed when combining discrete RP-SP data vary across papers. As mentioned in Section 2, many econometric models are available to combine SP and RP data that can account for different aspects of decision-making, such as preference heterogeneity, scale heterogeneity or allowing for correlated parameters. It is unclear if allowing for more flexible models will change our conclusions.

In this section we perform a sensitivity analysis to investigate different underlying preferences across datasets. The results from various specifications are reported in Appendix Table A2. Similar to the literature, we test for differences in underlying preferences by adding interaction terms to the baseline model. We estimate two models: one including only an interaction term between travel cost and a dummy for the SP scenario, and another one including interactions between all possible variables and the dummy for the SP scenario. These models are reported in the first two columns of Appendix Table A2. When looking at the measures of fit, we conclude that the model with the best fit is the one with the SP dummy interacted with travel cost. The interaction term between travel cost and SP dummy is positive and statistically significant ($\hat{\beta}_{TC*SP} = 0.014$). Moreover, the impact on welfare estimates of both these alternative specifications is significant. The estimated welfare losses are lower by a magnitude of 3 to 4 times (see Appendix Table A3). Hence, we conclude that
only the travel cost variable seems to differ across datasets besides what would be already allowed by the scale parameter. As other authors pointed out, this can be evidence of hypothetical bias, rather than differences in underlying preferences.

We also explore the results when using other approaches. von Haefen and Phaneuf (2008) suggest using RP parameters and filling in missing parameters from SP data to estimate welfare measures. The authors transfer SP parameters to RP-space by fixing the scale parameter as the ratio of the travel cost parameters in SP and RP data. Accordingly, in our dataset we fix the scale parameter to be \( \bar{\sigma}_{SP} = -\frac{-0.002}{-0.015} = 0.13 \). This fixed scale is five times lower than the freely estimated scale parameter (\( \hat{\sigma}_{SP} = 0.744 \)). The resulting estimated welfare losses are nearly identical to those from the baseline model. These are reported in Appendix Table A3. Alternatively, von Haefen and Phaneuf (2008) suggest filling-in missing parameters in SP data with scale-adjusted RP parameters. The estimated welfare losses are almost two times higher than those estimated with the parameters from the jointly estimated data.

We also use a latent class model to separate by internally consistent and inconsistent groups as suggested by Jeon and Herriges (2017). The estimation results are reported in Appendix Table A2. Class 1 encompasses 30% of respondents who are consistent in the RP and SP datasets, while class 2 includes the remaining 70% wherein coefficients are allowed to differ in RP and SP scenarios. Not only does travel cost seem to differ, but also ASCs differ in RP and SP datasets. The latent class model also suggests that respondents in the inconsistent class are indifferent towards the proximity to the oil spill variable. The welfare estimates for the consistent group are smaller than the baseline model (see Appendix Table A3). As for the inconsistent group, welfare estimates are very high for large oil spill sizes, but zero for a small oil spill.
Rather than focusing on testing for equality in the underlying preferences, the strategies employed when combining discrete RP-SP data vary significantly across papers to account for various aspects of decision-making. As mentioned in Section 2, many econometric models are available to combine SP and RP data accounting for preference heterogeneity, scale heterogeneity or allowing for correlated parameters. It is of interest to know how allowing for more flexible models may change our conclusions.

As a first test, one could not allow for any preference heterogeneity. We run a conditional logit model which results in worse fit than out mixed logit baseline model when we consider both the AIC (5539.46 > 5367.62), and BIC (5566.3 > 5403.4) (see Appendix Table A2). Welfare measures computed with the estimates from the conditional logit model are lower than the baseline mixed logit model.

Accounting for scale heterogeneity is a relatively recent practice. Scale heterogeneity entails the existence of a scale-adjusting term $\varphi_i$, which is individual specific (Hess and Rose, 2012). For example, lack of experience by some respondents when confronted by SP scenarios might increase uncertainty and result in larger variation of the scale-adjusting term in SP data for this group of respondents (Hensher, 2012). Ignoring differences in scale heterogeneity may also lead to erroneous conclusions of differing parameters in SP and RP data.

There are several options of account for scale heterogeneity. One option is to estimate a mixed logit model with all coefficients specified as randomly distributed as well as correlated: Hess and Train (2017) point out that if all utility coefficients are assumed to be randomly distributed, scale heterogeneity is captured and entangled in the standard errors of the estimated parameters from a mixed logit model. Specifying random parameters as correlated in a mixed logit model would
prevent scale heterogeneity from being picked by the parameters of interest (Mariel and Meyerhoff, 2018).

We show the estimation results of these two models in the last two columns of Appendix Table A2. The mixed logit model with correlated parameters outperforms the baseline mixed logit when we consider the AIC (5337.82 < 5367.62) and BIC (5378.08 < 5403.4). Estimated welfare losses are nearly identical to the baseline mixed logit model.

6. Conclusions

In this paper we combine revealed preference and stated preference data to estimate the recreational impact of four hypothetical oil spills on the Jæren beaches in Norway. We make two contributions to the state-of-the-art of RP-SP data combinations. We estimate the recreational impact of an oil spill ex-ante, while the overwhelming majority of literature focuses on the ex-post impact. We also propose the design of a contingent behavior question that expands the recreationist’s choice set and more faithfully mimics real-life recreational choices.

We present separately estimated SP and RP data models and jointly estimated RP-SP models. We find that the scale parameter differs between data sources. We conclude that the data is compatible with joint estimation, but the travel cost variable is likely to enter the utility functions in SP and RP data differently. We find that the welfare loss from an oil spill is 347 and 524 NOK (32 to 49 US dollars) per person per beach visit over the small to very large oil spill range.

Our application considers two simultaneous changes due to an oil spill: a reduction in the available choice set available to recreationists and a reduction in the perceived beach site quality. The inclusion of beach site quality through the variation present in the SP data leads to the calculation of welfare loss estimates (RP-SP data) which are higher than the estimates when excluding the
proximity variable (RP data). In the SP data the travel cost coefficient is eight orders of magnitude different than the travel cost coefficient from the RP data. This leads to a stated preference WTP estimate that is much higher than the WTP estimate estimated from the RP data. Jointly estimating the RP-SP data calibrates the welfare estimates.

We further show that resulting welfare estimates are robust to preference and scale heterogeneity assumptions. A fundamental question raised by this work is whether the underlying preferences differ across RP and SP datasets. There is evidence that only the travel cost parameter enters the utility function differently in SP and RP, which may also be indicative of hypothetical bias. Future research should target whether underlying preferences differ, provided that all relevant parameters are accounted for (e.g. scale heterogeneity).

Common practice in contingent behavior recreational choice modeling is to include a general opt-out option (i.e., not to engage in the recreational activity or to stay at home). The second contribution of our work is to provide an alternative design of a contingent behavior question that expands the choice set in the CB question. Our motivation was to mimic real-life recreational choices by including not only the option to stay at home, but also to recreate at other sites, such as forests, mountains or lakes. When modeling recreational choices, excluding the alternatives to engage in other recreational sites is a reduction in the available choice set. The high proportion of respondents (43%) choosing to visit other recreational sites in the CB question suggests that local lakes, mountains and hikes are relevant recreational substitutes to beach recreation and their omission is problematic.

Our study has limitations. We exploit a simple CB question which requires some researcher decisions when combining the revealed and stated preference data. Our SP and RP data are comprised of a single choice task, which complicates the identification of relevant parameters.
While the evidence suggests that the revealed and stated preference data are compatible, future combinations of RP-SP data should include sensitivity analysis around these decisions.

Another limitation of our work, but also of recreation choice models in general, pertains to the way outside options are modeled. We separate outside options into three categories: stay-at-home (ASC27), going to another beach (ASC28), and going to another recreational site (ASC29). ASC28 captures the average value of the attributes of other local beaches, and ASC29 captures the average value of the attributes of 58 different recreational sites, such as mountains, forests, and lakes. Given their heterogeneity (local beaches, or mountains, lakes, and forests), ASC28 and ASC29 are an imperfect measure of the average value of the corresponding attributes. Papers such as Stafford (2018) caution against aggregating outside options, but there does not seem to be a consensus in the literature on the balance between feasible survey designs and including all relevant alternatives. No prior data was available on the universe of available alternatives to Jæren beaches. Eliciting more information on these sites would have been useful but survey length constraints precluded it. However, we can argue that, as Stafford (2018) pointed out, our approach is an improvement over aggregating alternatives 27, 28 and 29 into a single outside option.

Acknowledgments:

This research was funded by the Norwegian Research Council in the context of the COAST-BENEFIT project (project number 255777). We thank participants at a Brown Bag Seminar at UNC Greensboro, the 2019 AERE Summer Conference and the 2020 EAERE Conference. The corresponding author would like to thank Gorm Kipperberg, Maria Loureiro, Thomas Lundhede
and Eija Pouta for their insightful comments, as well as John Rolfe for his input at the survey design stage.
References


Parsons, G.R. (2008) Monetary Values and Restoration Equivalents for Lost Recreational Services on the Gulf Coast of Texas Due to Oil Spills and Other Environmental Disruptions. Final Report Submitted to The Coastal Response Research Center.


### Table 1 – Oil Spill Attributes for each oil spill scenario (CB question)

<table>
<thead>
<tr>
<th>Oil Spill Size</th>
<th>Number of alternatives</th>
<th>Kilometers of coastline soiled</th>
<th>Time of beaches closure</th>
<th>Time required to recover from oil spill</th>
</tr>
</thead>
<tbody>
<tr>
<td>Small</td>
<td>29 = 26 beach + 3 opt-outs</td>
<td>5 km</td>
<td>0</td>
<td>6 months</td>
</tr>
<tr>
<td>Medium</td>
<td>19 = 16 beaches + 3 opt-outs</td>
<td>20 km</td>
<td>1 to 2 weeks</td>
<td>1 year</td>
</tr>
<tr>
<td>Large</td>
<td>8 = 5 beaches + 3 opt-outs</td>
<td>50 km</td>
<td>Some weeks</td>
<td>3 years</td>
</tr>
<tr>
<td>Very Large</td>
<td>5 = 2 beaches + 3 opt-outs</td>
<td>250 km</td>
<td>Several weeks</td>
<td>5 years</td>
</tr>
</tbody>
</table>

### Table 2 – Number of responses to CB question for each oil spill scenario

<table>
<thead>
<tr>
<th>Oil Spill Size</th>
<th>Visit the same beach</th>
<th>Go to another Jæren beach</th>
<th>Go to another beach outside Jæren</th>
<th>Visit another recreational site</th>
<th>Stay at home / Do something else</th>
</tr>
</thead>
<tbody>
<tr>
<td>Small</td>
<td>93</td>
<td>32</td>
<td>6</td>
<td>24</td>
<td>12</td>
</tr>
<tr>
<td>Medium</td>
<td>79</td>
<td>24</td>
<td>10</td>
<td>36</td>
<td>22</td>
</tr>
<tr>
<td>Large</td>
<td>9</td>
<td>23</td>
<td>17</td>
<td>76</td>
<td>26</td>
</tr>
<tr>
<td>Very Large</td>
<td>2</td>
<td>12</td>
<td>16</td>
<td>98</td>
<td>40</td>
</tr>
<tr>
<td>Total</td>
<td>183</td>
<td>91</td>
<td>49</td>
<td>234</td>
<td>100</td>
</tr>
</tbody>
</table>

### Table 3 – Selection of Estimation Results

<table>
<thead>
<tr>
<th></th>
<th>SP Data</th>
<th>RP Data</th>
<th>RP-SP Data</th>
<th>RP-SP Data</th>
</tr>
</thead>
<tbody>
<tr>
<td># of Observations</td>
<td>647</td>
<td>647</td>
<td>1294</td>
<td>1294</td>
</tr>
<tr>
<td># of RP Observations</td>
<td>0</td>
<td>647</td>
<td>647</td>
<td>647</td>
</tr>
<tr>
<td># of SP Observations</td>
<td>647</td>
<td>0</td>
<td>647</td>
<td>647</td>
</tr>
<tr>
<td>Model</td>
<td>Conditional Logit</td>
<td>Conditional Logit</td>
<td>Mixed Logit</td>
<td>Mixed Logit</td>
</tr>
<tr>
<td>Estimates</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Travel Cost</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
Table 4 – Mean Compensating surplus for oil spills of varying sizes

<table>
<thead>
<tr>
<th>CS Estimates</th>
<th>SP Data</th>
<th>RP Data</th>
<th>RP-SP Data (No Fixed ASCs)</th>
<th>RP-SP Data (Fixed ASCs)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Small</td>
<td>-734.37</td>
<td>0.00</td>
<td>-597.67</td>
<td>-346.70</td>
</tr>
<tr>
<td>Medium</td>
<td>-1119.28</td>
<td>19.94</td>
<td>-786.30</td>
<td>-479.12</td>
</tr>
<tr>
<td>Large</td>
<td>-1403.00</td>
<td>-66.61</td>
<td>-861.34</td>
<td>-520.33</td>
</tr>
<tr>
<td>Very Large</td>
<td>-1462.57</td>
<td>-123.17</td>
<td>-918.99</td>
<td>-524.41</td>
</tr>
</tbody>
</table>

Notes: Means of CS are shown. Values are in Norwegian kroner (NOK) per person and conditional on taking a beach trip (1 NOK = 0.11 US dollars). CS estimates for RP and RP-SP data are based on 10000 draws from the parameter distributions.
Figure 1 – Study site (left) and Oil Spill Illustration (right) for the four sizes considered

Figure 2 – Example of CB Question for large oil spill treatment (translation from Norwegian)
English et al. (2018) accounts for a demand-shift of recreation due to the oil spill by calibrating the alternative specific constants. Since we contemplate the impact of an oil spill *ex-ante*, we do not have data after the oil spill has happened and cannot use the same strategy.

The coefficient associated with travel cost is in the denominator of the site closure loss estimate. If the distribution of the travel cost variable allows values marginally close to zero, the resulting estimate in welfare loss converges to infinite. For an explanation, see Daly et al. (2012).

Norwegian marine traffic data for 2015 is available at [https://kystinfo.no/](https://kystinfo.no/).

This ship accident was mentioned in the survey to enhance consequentiality of the survey instrument, and 41% of the respondents indicated they had heard of this accident beforehand. More information about this ship grounding is available in Norwegian at [https://www.kystverket.no/](https://www.kystverket.no/).

The survey instrument in English and Norwegian is included in the Appendix B.

The sampling effort was dictated by budget constraints rather than power calculations.

The total number of invitees in the web-panel was 3793, of which 19 respondents were screened out and 965 respondents completed the questionnaire. The response rate is calculated as: \((19 + 965) / 3793 = 25.9\%\).

Values taken from Statistics Norway ([https://www.ssb.no/](https://www.ssb.no/))

As of 22/03/2022: USD 1 = NOK 8.72 (Source: Bloomberg)

The respondents who indicated that they would still visit a *Jæren* beach were not asked to name other recreational sites.

For every respondent with missing alternative recreational sites, we calculate their travel costs for the five most popular beaches (*Godalen, Vaulen, Skadbergsanden, Mollebukta, Sandvesanden*) and the five most popular recreational sites (*Dalsnuten, Stokkavatnet, Melsheia, Sørmarka, Preikestolen*). We then identify for each respondent the beach and the recreational site with the lowest travel cost.

Yi and Herriges (2017), Loomis (1997) and Parsons and Stefanova (2011) are notable exceptions which add the “stay at home” alternative in their discrete choice model. Count data applications of RP-SP include by default the option of not to recreate (i.e., count equal to zero).
Suppose that a large oil spill would occur. (…)

You have indicated earlier in the survey that your last visited beach along the Jæren coast in the summer of 2018 was [BEACH ID].

This oil spill would imply closing almost all of the Jæren beaches (…). A coastline of 50 kilometers would be affected, and it would take around three years for it to recover to the same state as before the oil spill.

Closure of the beaches is expected to take some weeks. You can still visit [BEACH ID] as previously.

Think about the last trip you took. What would you have done if the described oil spill had happened?

- [ ] Visit [BEACH ID] as previously
- [ ] Go to another Jæren beach
- [ ] Go to another beach outside Jæren
- [ ] Visit another recreational site (not a beach)
- [ ] Stay at home / Do something else