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Developed coastlines provide a variety of recreation opportunities to coastal residents and visitors but are also the first line of defense for oceanfront development against chronic hazards like erosion and sea level rise. In the Pacific Northwest of the United States, oceanfront homes also face an additional severe but very low frequency acute hazard: a Cascadia Subduction Zone earthquake and tsunami. These chronic and acute coastal hazards pose a challenge for policymakers because they often create conflicting interests. This dissertation is composed of two essays on issues of acute and chronic coastal risk in Oregon. The first essay investigates the impact of information shocks about tsunami risk on coastal residents' risk perceptions, as capitalized into property prices. We use revealed preference methods to examine the coastal Oregon housing market response to three sets of tsunami risk signals: two exogenous events, a hazard planning change, and the addition of visual cues of tsunami risk in residential neighborhoods. The potential housing market impacts identified in these analyses suggest that risk signals about a high severity but low frequency acute hazard can be salient to coastal residents. These findings suggest that Oregon policymakers and emergency managers may be able to use risk signals to induce individuals to pay attention to and prepare more for a Cascadia Subduction Zone event. In the second essay, we develop a combined revealed and stated preference survey and collect survey data from Oregon households. We use this data to estimate stated preference models and measure Oregon residents' willingness to pay for coastal erosion management conditional on differences in shoreline armoring policy for private oceanfront landowners. Results are suggestive of significant welfare gains stemming from a coastal management plan that would provide funding

for sediment management to preserve safe recreation access on developed Oregon beaches. We do not find evidence of a significant difference between how much Oregon residents are willing to pay for a policy scenario where the existing shoreline armoring policy (Goal 18) is relaxed to allow more armoring of private property and a policy scenario where the existing armoring policy is maintained in its current form. Overall, these two essays contribute new information about Oregon residents' perceptions and preferences regarding acute and chronic coastal risk. These findings can help inform policies in both emergency and resource management.

Valuing Coastal Risk with Revealed and Stated Preference Methods

by
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I understand that my dissertation will become part of the permanent collection of Oregon State University libraries. My signature below authorizes release of my dissertation to any reader upon request.

Amila Hadziomerspahic, Author

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1 Introduction

Sea level rise, changing storm patterns and severity, and increases in development are exposing coastal communities to increased hazard risks. Developed coastlines experience multiple chronic hazards, such as erosion and flooding, with different frequency and intensity. In the United States (U.S.), coastal erosion causes approximately $500 million dollars per year in property damages and loss of land (U.S. Climate Resilience Toolkit, 2021). The impacts of erosion on developed coastlines and oceanfront property will likely increase with sea level rise (SLR) (Institute for Water Resources, 2022; Sweet et al., 2022). An intermediate SLR scenario of 0.9 m by 2100 would place a projected 4.2 million people at risk of inundation in the continental U.S. (Hauer et al., 2016). Since developed beaches tend to be more vulnerable to the effects of erosion, rising sea levels also have the potential to decrease safe recreation access. Developed beaches may therefore require active management in the future to preserve oceanfront development and safe recreation access due to increasing erosion and SLR.

In the Pacific Northwest of the U.S., there is an additional severe but very low frequency acute hazard: a Cascadia Subduction Zone (CSZ) earthquake and tsunami. There is a 7% to 15% chance of a major earthquake (up to 9.2 in magnitude) occurring in the next 50 years along the CSZ fault off of the Pacific Northwest coast (OSSPAC, 2013). In Oregon, economic losses could be more than $30 billion – almost one-fifth of Oregon's gross state product – and fatalities due to the combined earthquake and tsunami could be more than ten thousand (OSSPAC, 2013). Coastal communities in the tsunami zone are especially vulnerable. They will experience the strongest earthquake motions due to their proximity to the fault, be subject to multiple tsunami inundations, and account for the majority of expected fatalities (OSSPAC 2013; Schulz 2015b). Tens of thousands of Oregon residents who live within the tsunami inundation zone will be instantly displaced. It may take 3 to 6 months to restore electricity, 1 to 3 years to restore drinking water, and up to 3 years to restore healthcare facilities on the coast (OSSPAC, 2013). Oregon's resilience to a magnitude 9.0 CSZ earthquake is low compared to compared to countries, like Japan and Chile, that regularly experience earthquakes (OSSPAC, 2013). The last CSZ earthquake and tsunami occurred in 1700 so Oregon has not experienced a major earthquake and tsunami in recent history. The low frequency of occurrence may lead to a lack of public salience about earthquake and tsunami risk, which contributes to low resilience to this acute hazard.

In this dissertation, I address the issues of acute and chronic coastal risk and management in Oregon using revealed and stated preference methods. Chapter 2 focuses on the acute hazard of the CSZ earthquake and tsunami. I investigate the impact of information shocks about tsunami risk on coastal residents' risk perceptions using revealed preference methods. Chapter 3 focuses on the chronic hazards of coastal erosion and SLR. I use stated preference methods to explore public support for coastal erosion management policies in Oregon.

Estimating risk perceptions related to natural disasters is critical to understanding behavioral responses of individuals and adaptive capacity of communities. In Chapter 2 of this dissertation, I study the coastal Oregon housing market response to three sets of tsunami risk signals: two exogenous events, a hazard planning change, and the addition of visual cues of tsunami risk in residential neighborhoods. For the first analysis, results suggest that a property inside the primary tsunami inundation zone sells for 6.5% to 8.5% less than a property outside of the zone after the 2011 Tohoku earthquake and tsunami in Japan, with this discount decaying within 2.5 years. For the second analysis, I find evidence that the release of new tsunami inundation and evacuation maps in 2013 was capitalized into home values in only the most vulnerable new inundation zone. Results for the third analysis suggest houses near roadway blue lines denoting entrance into the tsunami inundation zone may be selling for 8% less compared to houses farther away from the lines. The potential housing market impacts identified in these analyses suggest that risk signals – i.e., information shocks – about a high severity but low frequency hazard can be salient to coastal residents and may be useful policy tools to increase resilience to acute coastal hazards.

Oregon's coastal residents are also exposed to chronic hazards like erosion and SLR. Chronic coastal hazards – and policies to manage them – will impact not only Oregon's coastal residents but also beach recreators and non-recreators who value Oregon's developed beaches. In Chapter 3 of this dissertation, I evaluate the welfare effects of coastal erosion management policies focused on maintaining safe recreation access on developed Oregon Coast beaches. My hypothetical coastal management plan pairs Oregon's existing shoreline armoring policy (Goal

18) with a fund to manage sediment and preserve safe recreation access on eroding developed beaches. I develop a combined revealed and stated preference survey and collect survey data from Oregon households. To account for both users and non-users of developed Oregon Coast beaches, the survey collected both beach recreation trip counts and contingent valuation data about respondents' willingness to pay for the hypothetical coastal management plan. I also describe the modeling framework that motivated the survey design and how it will be used to decompose willingness to pay into use and non-use values for coastal erosion management. In this chapter, I use stated preference data from the survey to estimate contingent valuation models and measure Oregonians' willingness to pay for coastal erosion management conditional on differences in shoreline armoring policy for private landowners. I find that the economic value of coastal erosion management policies that affect safe recreation access on developed beaches is high – between $296 and $342 per household per year. I do not find evidence of a statistically significant difference between Oregonians' willingness to pay for a coastal management plan that relaxes existing armoring restrictions under Goal 18 and a plan that maintains Goal 18's armoring restrictions in their current form.

Chapter 4 concludes this dissertation by summarizing key findings and discussing their policy implications.

2 Tsunami risk and information shocks: Evidence from the Oregon housing market

2.1 Introduction

Severe but low frequency events pose a unique challenge for hazard planning. The connection between risk perception about catastrophic events and preparedness action is still much disputed (Wachinger et al., 2013). The risk of a catastrophic natural disaster must be salient to the people it will impact to translate into personal preparedness. If the risk is either not salient to individuals or does not translate into behavior change, it may fall on policymakers to correct the market failure to internalize risk and increase resilience.

The Pacific Northwest of the United States (U.S.) is facing such a challenge. There is a 7% to 15% chance for a major earthquake (up to 9.2 in magnitude) to occur in the next 50 years along the Cascadia Subduction Zone (CSZ) (OSSPAC, 2013). In Oregon, preparedness for such a large seismic event is low. A recent study estimated that economic losses could be more than $30 billion – almost one-fifth of Oregon's gross state product – and fatalities due to the combined earthquake and tsunami could be more than ten thousand (OSSPAC, 2013). Coastal communities in the tsunami zone are especially vulnerable since they will experience the strongest earthquake motions due to their proximity to the fault, will be subject to multiple tsunami inundations, and will account for the majority of expected fatalities (OSSPAC, 2013; Schulz, 2015b).

Individual Oregonians can increase their resilience by retrofitting their homes, purchasing earthquake and flood insurance, or moving away from high-risk areas such as the tsunami inundation zone. Whether individuals will take action to prepare themselves depends in part on their beliefs about the risk of a Cascadia earthquake and tsunami occurring in their lifetimes. If Oregonians' subjective risk perceptions underestimate the *objective* probability of a Cascadia event – if the risk is not salient – then they will likely underprepare themselves. This gap between subjective risk perceptions and objective risk is plausible given that Oregon has not experienced a major earthquake and tsunami in recent history – the last CSZ earthquake and tsunami occurred in 1700 – and has low resilience compared to countries, like Japan and Chile, that regularly experience earthquakes (OSSPAC, 2013). The lack of recent earthquakes has led Oregon to also be less prepared and more vulnerable than its neighboring states of California and Washington

(Totten, 2019). This motivates an important question about tsunami risk perceptions: Can new information about the risk of a tsunami from a Cascadia earthquake change people's risk perceptions and narrow the gap between subjective and objective risk? Here, I investigate whether risk discounts are present in coastal Oregon housing markets following exogenous information shocks about tsunami risk. I study the housing market's response to three sets of risk signals: 1) two exogenous events – the March 11, 2011 Tohoku (Japan) earthquake and tsunami and the July 20, 2015 New Yorker article "The Really Big One"; 2) a hazard planning change – the release of new official tsunami evacuation maps in 2013 by the Oregon Department of Geology and Mineral Industries (DOGAMI); and 3) visual cues of tsunami risk – the Tsunami Blue Line project, which has installed signage denoting the upper limit of the tsunami inundation zone in communities along the coast since 2016.

Using a dataset of residential property transactions for the Oregon coast (Zillow, 2020), I estimate the treatment effects of these tsunami risk signals in a series of hedonic pricing models using difference-in-differences (DID) and triple differences (DDD) research designs. First, I use information from the northern Oregon coast housing market to estimate the impact of two exogenous events that represent "pure" or "distant" information shocks in that there is no actual disaster event or that the disaster event is distant and there is little associated local damage. An increased volume of Google searches suggest that these events were salient to Oregonians and may be a mechanism by which individuals update perceptions of risk related to the potential for a major Cascadia event. I differentiate risk using a regulatory tsunami hazard line as the treatment boundary since the entire coastline is likely to face similar impacts from an earthquake. Results suggest that a property inside the regulatory tsunami inundation zone sells for 6.5% to 8.5% less than a property outside of the zone after the Tohoku earthquake and tsunami. This result is robust to several alternative specifications, including the Oaxaca-Blinder estimator, four post-matching estimators, and an event study specification. I find that the effect is short-lived as property prices inside the inundation zone quickly return to baseline levels within 2.5 years of the Tohoku event. A back-of-the-envelope calculation suggests that this tsunami risk discount had an average capitalization effect of $6.1 to $28.7 million dollars in the northern Oregon housing market during its short-lived duration. Since tsunami damage would likely be covered by flood insurance

(OSSPAC, 2018), I also compare this capitalization effect to capitalized flood insurance premiums. I find that the capitalized value of flood insurance premiums ($6,695 to $16,120) may be up to four times smaller than the average capitalization per home of the tsunami risk discount ($19,964 to $27,642).

I then use housing information from the entire Oregon coast to estimate the impact of a 2013 update of official tsunami inundation and evacuation maps based on a new series of modeled inundation maps for five CSZ tsunami size scenarios (i.e., S, M, L, XL, XXL) (DOGAMI, n.d.-a). The largest of this series – the XXL scenario representing the potential inundation from a worst-case CSZ tsunami – became the inundation line for official tsunami evacuation brochures and signage, supplanting the original and more conservative inundation line that was established in 1995 through Oregon Senate Bill 379. This hazard planning change represents a tsunami risk signal – and a "pure" information shock – about houses that were not in the original 1995 SB 379 evacuation zone but found themselves inside one of the new 2013 inundation zones. I find the estimates are not statistically significant for the XXL, XL, L or M tsunami inundation zones. The DID and Oaxaca-Blinder estimators for the smallest inundation zone (SM) suggest that homes that were not in the original tsunami inundation zone but *are* now in a zone vulnerable to inundation from even a small tsunami sell for 16% to 27% less after the map update. This risk discount appears to persist as it does not have a statistically significant decay effect.

Lastly, Oregon's Tsunami Blue Line project has installed thermoplastic blue lines across roadways indicating the upper extent of the 2013 XXL tsunami inundation zone in several coastal communities since its launch in 2016 (Office of Emergency Management, 2016). The blue lines are visual cues of tsunami risk and their installation represents a risk signal and a "pure" information shock to properties near those blue lines. To determine whether this project resulted in a risk discount for homes near the blue lines and inside the tsunami evacuation zone, I estimate the effect of the blue lines on property prices, with properties differentiated by proximity to the blue lines and – for a DDD approach – by the XXL tsunami inundation zone. Results from my preferred standard two-way fixed effects (TWFE) DID model are suggestive of an 8% risk discount for properties that are within 1000' of a blue line. The DDD results for this model are not statistically significant, suggesting homebuyers may attend to the visual cue but not the risk signal given by the tsunami inundation zone. However, since the blue lines were installed at different times, there is variation in treatment timing. Several recent studies have pointed out problems with interpreting the results of the standard TWFE DID regression when the treatment effect is heterogeneous over time (Borusyak et al., 2021; de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun & Abraham, 2020). To explore this further I first assess the robustness of the TWFE estimator to heterogeneous treatment effects using the measure proposed by de Chaisemartin and D'Haultfœuille (2020) and find that treatment effect heterogeneity could be a concern for this analysis. I then estimate two new estimators that are valid in the presence of treatment effect heterogeneity (Callaway & Sant'Anna, 2020; de Chaisemartin & D'Haultfœuille, 2020). Using de Chaisemartin's and D'Haultfœuille's (2020) approach, I find a large, negative, but not statistically significant effect. The data for this analysis is too sparse to estimate most of Callaway's and Sant'Anna's (2020) group average treatment effects and therefore their overall average treatment effect. The group average treatment effects I am able to estimate are negative but not statistically significant. Although the treatment effects generated by these two methods have the same sign as the TWFE DID estimate, their magnitudes and significance are likely impacted by the small sample size of this analysis. The study area is composed of small, rural communities so I do not have the statistical power to precisely estimate these new estimators and verify the results from the TWFE regression.

This work contributes to the hedonic literature on hazard risk and the impacts of information on subjective risk perceptions. This paper is one of few studies that attempts to measure the effects of "pure" or "distant" information shocks in that either there is no actual disaster event, as in the case of the 2015 New Yorker article, 2013 evacuation map change, and the Tsunami Blue Line project, or that the disaster event is distant and there is little associated local damage, as in the case of the 2011 Tohoku earthquake and tsunami (Atreya & Ferreira, 2015; Brookshire et al., 1985; Gibson & Mullins, 2020; Gu et al., 2018; Hallstrom & Smith, 2005; Nakanishi, 2017; Parton & Dundas, 2020). To my knowledge, this paper is the first to investigate the tsunami risk discount in property values disentangled from the earthquake risk discount. Previous studies have explored either the combined earthquake and tsunami risk (Nakanishi, 2017) or the earthquake risk alone (Beron et al., 1997; Brookshire et al., 1985; Gu et al., 2018; Naoi et al., 2009). This study's results also contribute to the literature on risk salience (Kask & Maani, 1992; Nakanishi, 2017) and the link between risk perception and preparedness action (Wachinger et al., 2013) by investigating the impact of new hazard information on the gap between subjective risk and objective risk and how narrowing this gap may change self-protective behavior in the housing market.

My results have important risk communication and policy implications for the U.S. Pacific Northwest. Research shows that Oregon is chronically under-prepared for a Cascadia earthquake and tsunami (OSSPAC, 2013). Policymakers and emergency managers may need to communicate risk more effectively to increase risk salience and induce individual decision-makers to take appropriate preparedness actions. Some recent policy changes have even done the opposite. Oregon House Bill 3309, passed and signed in June 2019 with nearly unanimous bipartisan support, overturns a nearly 25-year-old law prohibiting new schools, hospitals, jails, police stations, and fire stations from being built in the tsunami inundation zone (Oregonian, 2019). Efforts such as this run counter to Oregon's dual policy challenge of increasing risk salience and preparedness actions. The potential risk discounts identified here suggest that at least three types of tsunami risk signals – exogenous events, hazard planning changes, and visual cues – may be salient to coastal residents. These results suggest that "pure" or "distant" information shocks can shift homebuyers' subjective risk perceptions to better match the objective risks of the Cascadia event. Thus, policies and other "pure" information shocks may be able to successfully communicate the risk of a Cascadia event and induce individuals to take preparedness actions. Given Oregon's current and chronic under-preparedness for a Cascadia event, additional policies – or risk signals – are needed to help mitigate hazard risk.

This paper proceeds as follows. Section 2.2 reviews the hedonic literature on risk and hazards, along with empirical strategies to investigate price differentials across hazard zones and the persistence of risk premium changes. Section 2.3 describes the study areas and their policy and news backgrounds. Section 2.4 describes the data and Section 2.5 defines my empirical approach and discusses identification strategies for all three analyses. Section 2.6 presents results for all three analyses. Section 2.7 concludes by providing a summary of my findings, next steps to identify these risk signals, and implications for resilience planning and policy.

2.2 Hazard risk and housing markets

The property attribute of interest in this paper is subjective tsunami risk and I use hedonic pricing models to test whether three different types of tsunami risk signals capitalize into coastal Oregon property values. Rosen's (1974) seminal paper was the first to show that regressing observed product prices on their attributes can reveal buyers' marginal willingness-to-pay (MWTP) for individual attributes of a differentiated product.[1] Modern hedonic property models typically rely on the foundational assumptions that the total supply of housing is fixed and implicit marginal prices represent market equilibria (Hanley et al., 2007).

Previous literature has used hazard events or regulatory hazard delineation to identify the impact of risk on housing prices. Early research by Brookshire et al. (1985) found significant discounting of housing prices in zones with high earthquake risk in California following the passing of an earthquake risk disclosure law in 1974. The majority of hedonic earthquake risk studies since have examined the impacts of specific earthquake events (Beron et al., 1997; Gu et al., 2018; Naoi et al., 2009). Other studies that investigate earthquake risk impacts without the occurrence of a local seismic event have nonetheless focused on locations like California and Japan where earthquakes have occurred in recent memory (Brookshire et al., 1985; Nakanishi, 2017). Hedonic models have also been used to measure risk premiums for natural hazards like floods (Atreya et al., 2013; Kousky, 2010), hurricanes (Bakkensen et al., 2019; Bin & Landry, 2013; Gibson & Mullins, 2020; Hallstrom & Smith, 2005), wildfires (McCoy & Walsh, 2018), and coastal storm surge (Dundas, 2017; Qiu & Gopalakrishnan, 2018), as well as man-made sources of risk like proximity to fuel pipelines (Hansen et al., 2006), hazardous waste sites (McCluskey & Rausser, 2001), and nuclear power plants (Tanaka & Zabel, 2018).

[1] Kuminoff and Pope (2014) point out that the parameters estimated by panel models such as difference-in-differences are not necessarily theoretically equivalent to the parameters (MWTP) identified by the reduced-form (first-stage) hedonic model. Rosen's (1974) model considers market equilibrium, not the equilibrating process that would follow an exogenous change in product attributes. If we are willing to make the assumption that the gradient of the price function is constant over the duration of the study period, then we can interpret the panel model coefficients as MWTP values (Kuminoff & Pope, 2014). This is a strong assumption for studies with large shocks – such as shocks to amenities, preferences, income, or information – and for study periods that span potentially large changes in house and neighborhood attributes – such as the eight-year duration of the first analysis (2009-2017). In these cases, the equilibrium hedonic price function can shift after the shock in which case hedonic model estimates will conflate MWTP at a point in time with changes in the price function. Therefore, I interpret the coefficient estimates from my hedonic approach as capitalization effects, not MWTP, because they describe how the change in the attribute of interest was capitalized into housing prices over time.

Difference-in-differences (DID) approaches have been used to show that disaster events can increase house (or land) price differentials across hazard zones (Atreya et al., 2013; Bakkensen et al., 2019; Bin & Landry, 2013; Gibson & Mullins, 2020; McCoy & Walsh, 2018; Nakanishi, 2017; Naoi et al., 2009; Tanaka & Zabel, 2018). The quasi-experimental DID approach uses a recent disaster as an exogenous information change to separate properties into a treatment group that experiences the disaster event and a control group that does not. The idea behind this approach is that the disaster event provides new information that causes a change in the level of subjective risk that may capitalize into house prices.[2] Temporal variation in the attribute of interest is used to difference out time-invariant omitted variables that would otherwise confound identification. The DID approach allows us to isolate contemporaneous effects, such as macroeconomic shocks or housing supply changes, and measure only the effect attributable to the exogenous risk signal. Triple differences (DDD) has also been used to recover effects of risk on property prices (Bakkensen et al., 2019; Muehlenbachs et al., 2015; Qiu & Gopalakrishnan, 2018). These approaches typically exploit an additional treatment (control) group that is more (less) sensitive to the treatment.

Information available to housing market participants can change due to a catastrophic event, media coverage, or new laws (Bakkensen et al., 2019; Bin & Landry, 2013; Brookshire et al., 1985; Gibson & Mullins, 2020; Gu et al., 2018; Hallstrom & Smith, 2005; Kask & Maani, 1992; Kousky, 2010; McCluskey & Rausser, 2001; McCoy & Walsh, 2018; Parton & Dundas, 2020; Qiu & Gopalakrishnan, 2018; Tanaka & Zabel, 2018). Kask and Maani (1992) were the first to show that consumers' subjective probabilities may under or overestimate objective probabilities, biasing hedonic prices under conditions of uncertainty. Under the uncertainty of a hazardous event occurring, hedonic prices are based on consumers' subjective probability which they define as a function of the objective probability, the consumer's expenditures on self-protection (e.g., insurance) and information level (an exogenous variable). The effect of increased information on

[2] Banzhaf (2021) shows that DID capitalization effects can recover bounds on nonmarginal welfare measures even when the hedonic price function shifts, e.g., after an information shock. He shows that DID hedonic studies can identify the direct effect of treatment if they allow for changes in the hedonic price function over time. This direct effect can be interpreted as a movement along the *ex post* hedonic price function and also as a lower bound on the welfare effect of a nonmarginal change in the attribute of interest (i.e., a lower bound on Hicksian equivalent surplus). For decreases in the attribute of interest, the capitalization effect can identify an upper bound on welfare loss (in absolute values). I do not estimate Banzhaf's (2021) flexible DID approach in this paper and leave that as an extension for future work.

behavior depends on the gap between objective risk and the consumer's initial subjective risk (Kask & Maani, 1992). For example, above-average objective risk and a lower initial subjective probability will lead to increasing subjective probability as information increases.[3]

New information can lead individuals to update their subjective perceptions of risk and, in turn, risk premiums may be identified in a hedonic model. However, few studies have attempted to measure the effects of a "pure" information shock – when there is no actual disaster event – on property prices (Brookshire et al., 1985; Gibson & Mullins, 2020; Nakanishi, 2017; Parton & Dundas, 2020). For example, Gibson and Mullins (2020) use DID to look at housing market responses to two "pure" flood risk signals in New York – the passing of the Biggert-Waters Flood Insurance Reform Act (which increased flood insurance premiums) and new floodplain maps produced by the Federal Emergency Management Agency (FEMA) – as well as housing market responses to an actual disaster event – Hurricane Sandy. The release of the new floodplain maps, which had not been updated in 30 years, was accompanied by prominent press coverage and presented New Yorkers with three decades worth of updated information about climate change in a single event. Hurricane Sandy and the Biggert-Waters Act, similarly, acted as exogenous information shocks about flood risk. Gibson and Mullins (2020) find that all three flood risk signals decreased the sales prices of impacted properties by 3% to 11% (depending on the risk signal).

Furthermore, salience of risk may capitalize into property prices only temporarily after a disaster event. Other studies have found that the change in risk premium due to a disaster event may disappear rapidly over the course of a couple of years if additional disaster events do not occur (Atreya et al., 2013; Bin & Landry, 2013; Hansen et al., 2006; Kousky, 2010; McCluskey & Rausser, 2001; McCoy & Walsh, 2018; Tanaka & Zabel, 2018). Leveraging multiple storm events in North Carolina, Bin and Landry (2013) find risk premiums between 6.0% and 20.2% following

[3] Kask and Maani (1992) use expected utility theory to show that new information about hazards can cause the difference between subjective and objective risk to change, e.g., to decrease, as claimed here. Other research has found that conventional expected utility theory is insensitive to rare catastrophic events (Chichilnisky, 2009). Under this assumption, using expected utility theory would fail to economically value the impact of catastrophic – major impacts but low probability – risk. Nakanishi (2017) uses Chichilnisky's (2009) generalized expected utility framework that is sensitive to rare catastrophic events and a matching DID design to investigate the change in land prices in Japan in response to a government report on the expected damage of rare catastrophic earthquakes and tsunamis. The usefulness of this generalized expected utility framework is that it allows for the observed statistically significant change in the price function even if households understand the government report, i.e., even when the risk is salient. Essentially, a significant estimation result is justified without requiring the subjective risk distribution to differ from the objective risk distribution (Nakanishi, 2017).

major flooding events for properties inside the 100-year flood zone. This risk premium decreases over time without new flood events and disappears 5-6 years after the last recorded event. This decay of risk premium suggests that people's risk perceptions change with the prevalence of disaster events. Without new information, individuals' subjective probabilities will diminish. Hansen et al. (2006) investigate the effects of distance from a fuel pipeline on property prices in Bellingham, WA before and after a major pipeline accident in 1999. They find a large risk discount following the accident and that, for a given distance from the pipeline, the effect of the explosion decays over time.

Hansen et al. (2006) point out three reasons why the effect of an event on subjective risk perceptions may decrease over time. First, the informational effect of the event will diminish as new people move into the area. Second, individuals who were exposed to the event may experience decay of their active recall of the event. Their passive recall of the event may be intact, such that they can recall the event if prompted, but for the event to influence property prices, homebuyers must be thinking about the risk when making purchasing decisions. Lastly, in addition to providing information, a disaster event focuses attention on the hazard risk and can cause the subjective risk to increase beyond the level of objective risk. However, as media coverage decreases and people's attention turns to more recent events, this attention-focusing effect of the event will diminish over time.

A related explanation for the observed decay in risk premium is availability bias (Atreya et al., 2013; Bin & Landry, 2013; Gallagher, 2014; Kousky, 2010; McCoy & Walsh, 2018; Tanaka & Zabel, 2018). The availability heuristic posits that individuals' subjective probability of an event occurring depends on how recent or memorable that event was (Tversky & Kahneman, 1973). Availability bias implies that a decision maker's subjective risk perception depends on the availability of information and/or recall of events related to the hazard in question. The low frequency of disaster events suggests that individuals without recent experience with natural hazards have limited information and ability to recall similar events. Thus, availability bias would suggest that these individuals have low subjective risk perceptions. For example, Gallagher (2014) uses an event study framework to estimate the effect of large regional floods on insurance uptake rates and finds strong evidence of an immediate increase in the fraction of homeowners with flood insurance policies in communities hit by the flood. The insurance uptake rate steadily declines until, after nine years, the effect of the flood is no longer statistically distinguishable in uptake rates. Gallagher (2014) also finds that this insurance uptake spike-and-decay pattern repeats if a community is hit by another flood, suggesting that the occurrence of new flood events is relatively important in forming flood risk beliefs. Without new information, individuals' subjective probabilities will diminish.

However, even when the natural hazard risk is salient, it may not translate into behavior. In their review of prior research on natural hazard risk perception and behavior, Wachinger et al. (2013) find that the link between risk perception and preparedness action can be weak even when individuals understand the risk.[4] Wachinger et al. (2013) also find that the main factors responsible for determining risk perception are direct experience of a natural hazard, trust in scientific experts and authorities, and confidence in protective measures. Secondary but significant factors include media coverage, a form of indirect experience, and home ownership, which stimulates concern when the homeowner perceives a vulnerability or has personal experience. They note that the indirect experience provided by mass media influences risk perception but only when the respondents lack direct experience.

2.3 Study area and background

Oregon is a geologic mirror image of northern Japan, where the March 11, 2011 magnitude 9.0 Tohoku earthquake and tsunami caused widespread damage. The resulting tsunami surges also caused millions of dollars of damages to parts of the Oregon coast (Jung, 2011). The majority of damage in Oregon was concentrated in the port of Brookings where the waves destroyed docks, resulting in $7 million in damage (Tobias, 2012). Longer-term effects of the tsunami included multiple cleanup efforts as debris from Japan slowly made its way to Oregon shores.

[4] Wachinger et al. (2013) offer possible reasons for this weak relationship even when individuals understand the risk. First, residents of an area facing natural hazard risk may choose to accept the risk if their perceived benefits outweigh the potential impacts, e.g., in this study, distance to the coast serves as both a proxy for coastal amenities and increased risk to homeowners. The second reason is due to the effect of trust in government and/or structural measures. Individuals are less likely to prepare themselves when they trust these measures to protect them than when they have little trust in the government authority or the effectiveness of existing measures. Essentially, they transfer responsibility for action to someone else, e.g., state or local government. Third, there may be confusion or ignorance about the appropriate preparedness action to take or individuals may have little capacity or few resources to help themselves.

Oregon is due to experience a major subduction zone earthquake of a similar magnitude to the Tohoku event. The probability of a Cascadia Subduction Zone (CSZ) earthquake occurring in the next 50 years is 7% to 15% for a great earthquake between 8.7 and 9.2 magnitude and approximately 37% for a very large earthquake between 8.0 and 8.6 magnitude (OSSPAC, 2013). Unlike Japan, Oregon's resilience to a magnitude 9.0 Cascadia earthquake is low. Coastal communities in the tsunami zone are especially vulnerable since they will experience the strongest earthquake motions due to their proximity to the fault and will then be subject to multiple tsunami inundations for up to 24 hours after the earthquake (OSSPAC, 2013). Residents who live within the tsunami inundation zone may be displaced instantly. It may take 3 to 6 months to restore electricity, 1 to 3 years to restore drinking water, and up to 3 years to restore healthcare facilities on the coast (OSSPAC, 2013).

In their 2013 report, the Oregon Seismic Safety Policy Advisory Commission (OSSPAC) (2013) separated Oregon into four impact zones based on the expected pattern of damage for a 9.0 Cascadia earthquake and tsunami scenario (Figure 2.1). They predict that damage will be the most extreme in the tsunami (inundation) zone and heavy throughout the coastal zone. The coastal zone, which encompasses most of the coastal county population centers, is expected to experience severe damages from shaking, liquefaction, and landslides. Throughout the coastal zone, single-family homes and other wood frame structures will shift off foundations if unsecured. In some areas of the coast, even well-built wooden structures may be heavily damaged and in need of replacement. However, in the tsunami (inundation) zone, the damage will be nearly complete. The tsunami will not only further damage buildings, roads, and utilities but it will also "obliterate nearly all wood frame buildings" (OSSPAC, 2013, p. 49). This difference in outcomes of residential buildings inside versus outside the tsunami inundation zone suggests that there is a distinct difference between earthquake and tsunami risk for coastal residents. Similarly, the tsunami zone will also experience a higher proportion of fatalities. Approximately 4% of permanent residents in the seven coastal counties live in the tsunami inundation zone (as defined by the 1995 SB 379 regulatory tsunami line) (Wood, 2007). However, half of the fatalities of a 9.0 magnitude Cascadia event are expected to be due to the tsunami (OSSPAC, 2013).

Even though the entire coastline would experience similar impacts from an earthquake, coastal homes outside of the tsunami inundation zone may survive the Cascadia earthquake but those inside of the zone will likely not. In this paper, I differentiate risk using tsunami inundation lines from maps produced by the Oregon Department of Geology and Mineral Industries (DOGAMI) as the treatment boundaries. Senate Bill 379 established the original tsunami inundation zone in Oregon in 1995. This line, also known as “SB 379,” represents the best estimate of tsunami inundation from a typical or most likely Cascadia earthquake in 1995 (DOGAMI, n.d.-b). The 1995 SB 379 line was the regulatory tsunami inundation line for Oregon until 2019 and limited the construction of certain critical and essential facilities inside the inundation line (DOGAMI, n.d.-b). Official tsunami evacuation brochures and signage used the SB 379 line until 2013 when DOGAMI released a new series of tsunami inundation maps for a Cascadia earthquake.[5] The 2013 tsunami inundation map series TIM Plate 1 was derived using systematic, Oregon-coast-wide models of tsunami inundation for five scenarios – XXL, XL, L, M, and SM – that represent the full range of severity of past and expected tsunamis (DOGAMI, n.d.-a). The largest scenario of this series – the XXL scenario – became the one used by DOGAMI to represent the “maximum local source” inundation level in their official tsunami evacuation maps and signage (DOGAMI, n.d.-a). Thus, the XXL scenario has represented the tsunami evacuation line for the public at large since 2013. The release of the evacuation maps in 2013 also confronted homeowners who were outside of the 1995 SB 379 evacuation zone but inside the 2013 XXL evacuation zone with new and up-to-date information about tsunami risk. Thus, this change in hazard planning also acts as a “pure” information shock about those houses.

The July 20, 2015 New Yorker article “The Really Big One” by Kathryn Schulz (2015a) brought national media attention to the predicted Cascadia event and to Oregon’s low level of resilience and preparation for it. This article went viral in the summer of 2015 (Fletcher & Lovejoy,

[5] Oregon does not have a tsunami inundation zone disclosure requirement in its statutory “Seller’s Property Disclosure Statement” (defined in ORS 105.464) (Property Rights, 2019). Oregon House Bill 2140, effective 2018, added the first and only seismic disclosure, which requires property sellers to tell buyers whether the home was built before 1974, and if so, whether the house has been bolted to its foundation (House Bill 2140, 2017). The “Seller’s Property Disclosure Statement” also requires sellers to state whether the property is in a designated slide or other geologic hazard zone. However, this requirement does not distinguish between tsunami hazard zones and all other geologic hazard zones. The only tsunami-specific disclosure requirement in Oregon is Tillamook County’s Ordinance 84 which was revised in 2019 to require short term rental properties located within a tsunami inundation zone to post a DOGAMI Tsunami Evacuation Brochure in a visible location as close as possible to the main entrance of the short term rental (Tillamook County Short Term Rental Ordinance, 2017).

2018; Lacitis, 2015; Marum, 2016). It also prompted preparedness actions such as the selling out of emergency preparedness kits (Lacitis, 2015; Lovejoy, 2018), earned its author a Pulitzer (Marum, 2016), and motivated a book addressing risk perception, preparedness, and communication (Fletcher & Lovejoy, 2018). In a chapter of this book, Crowe (2018) compares media coverage of the CSZ before and after Schulz' article. She finds that before Schulz' article the 3 largest spikes in U.S. newspaper coverage occurred after the 2001 Nisqually earthquake in WA, the 2004 Indian ocean earthquake and tsunami, and the 2011 Tohoku earthquake and tsunami (Crowe, 2018). The Tohoku earthquake and tsunami had the most media coverage to date that connects the CSZ to another natural disaster. Within 3 months of "The Really Big One", 33 unique newspaper articles were published that referenced both Schulz' article and the CSZ. Journalists reported on increased individual actions following the article, including spikes in earthquake survival kit sales and home earthquake retrofitting, and group actions including public forums, events, and roundtables on earthquake preparedness. Essentially, "The Really Big One" both communicated the risk of the Cascadia earthquake and tsunami and spurred the public to prepare for it (Lovejoy, 2018).

Google search intensity spikes are also in line with Crowe's (2018) findings of spikes in media coverage following Schulz' 2015 New Yorker article and the 2011 Tohoku earthquake and tsunami. Figure 2.2(a) graphs the Google searches in Oregon for the terms "Oregon earthquake", "Cascadia subduction zone", and "Earthquake prediction" between 2004 and 2017. Search popularity is measured as a percentage of search interest relative to the highest point on the chart for Oregon web users (searches originating from Oregon addresses) between 2004 and 2017 (*Google Trends*, n.d.). The number of searches peaked in July 2015 reflecting the viral popularity of the New Yorker article. The Tohoku earthquake and tsunami in March 2011 represents the second highest peak in searches and was 75% as popular as the New Yorker article. However, the search intensity for "Oregon earthquake" at its peak after the 2015 New Yorker article is only 40% of the search intensity for "Oregon tsunami" at its peak during the 2011 Tohoku event (see Figure 2.2(b)).

Combined, the increase in internet searches for information on an Oregon earthquake/tsunami and media coverage on the CSZ immediately after these two events suggests that they acted as information shocks to Oregon residents. The Tohoku 2011 earthquake and tsunami could have increased Oregonians' information levels about the Cascadia event due to its similarity to the predicted Cascadia event and the fact that its impacts were felt on the Oregon coast. The 2015 New Yorker article also likely impacted Oregonians' information levels and risk perceptions about the Cascadia event through its viral status and detailed explanation and illustration of the objective risk.

Oregon has implemented several policies designed to make the public more aware of and prepared for the Cascadia earthquake and tsunami. The Tsunami Blue Line project launched in February 2016 and provided communities along the Oregon coast with funds and materials to install thermoplastic blue lines and signs directly on roadways marking the entrance to the tsunami evacuation zone (Office of Emergency Management, 2016). The blue lines and "Leaving Tsunami Zone" signs were installed on the 2013 XXL tsunami inundation and evacuation line at various times since 2016 through the present day. Most blue lines are approximately 12" wide and have "Leaving Tsunami Zone" signs next to them, as seen in Figure 2.3(a), though some only have the "Leaving Tsunami Zone" sign without an accompanying blue line, as seen in Figure 2.3(b). Thus, the blue lines present distinct visual markers of entry/exit into the tsunami inundation and evacuation zone. The coastal communities that had blue lines installed were Bay City, Cannon Beach, Coos Bay, Florence, Gold Beach, Lincoln City, Manzanita, Nehalem, Newport, Reedsport, Seaside, and Yachats as well as some unincorporated areas of Lincoln County. Each of these communities managed the installation of their own blue lines except for unincorporated communities whose blue lines were installed by their county's public works department. The blue lines and signs were installed on roads generally as close as possible to the 2013 XXL tsunami line (S. Absher & A. Rizzo, personal communication, December 3, 2021).

The siting of the blue lines within each community was driven primarily by evacuation concerns. For example, the city of Seaside's Emergency Preparedness Committee identified the best locations for pedestrians to be able to see and follow five established evacuation routes (City of Seaside, 2019). They concluded that thermoplastic road markers should be placed at evacuation decision points, e.g., street intersections. In their Tsunami Evacuation Facilities Improvement Plan (TEFIP) the city of Waldport (Lincoln County) proposed locations for additional blue lines and tsunami signage, suggesting that blue lines could be used to indicate arrival at higher ground along major evacuation routes and that routes should be prioritized for signage based on traffic and need (City of Waldport, 2019). The TEFIP of the city of Netarts (Tillamook County) recommended that blue lines be placed in heavily trafficked areas that would present highly visible locations and in areas where additional clarity is needed about the direction of high ground during an evacuation (Tillamook County, 2019). Sarah Absher, the director of Tillamook County Department of Community Development, noted that topography, road conditions, and the presence of existing signage also informed where tsunami signage was located (S. Absher & A. Rizzo, personal communication, December 3, 2021).[6] Some local governments (e.g., Tillamook County) also held community meetings to elicit feedback and input about tsunami wayfinding efforts.[7] In addition to a statewide press release (Office of Emergency Management, 2016) and flyers announcing the new blue lines, several community news agencies also reported on their local blue lines following installation (Fontaine, 2016; Kustura, 2016; Sheeler, 2018).

The first analysis in this paper focuses on the three northernmost counties of Clatsop, Tillamook, and Lincoln because the North Oregon coast is expected to experience the most concentrated tsunami exposure (OSSPAC, 2013).[8] Since the Tohoku earthquake/tsunami and New Yorker article are both "pure" or "distant" information shocks, I chose to focus on the region of Oregon that is likely to be the most sensitive to such shocks. The northern coast counties have the highest percentages of tsunami-prone land that is zoned as urban (Wood, 2007). While 95% of the

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[6] For example, a blue line may be effective in locations where heading inland leads evacuees to higher elevations so the blue line exists to let evacuees know how far they have to go to be outside of the tsunami inundation zone. However, in communities like Rockaway Beach or Cape Meares (Tillamook County) the topography is such that running inland does not necessarily result in moving to higher elevations so evacuation routes need to zigzag people through streets and neighborhoods to keep them out of low-lying areas. In these cases blue lines are less effective than signage that points evacuees in which direction to go next. Another factor in deciding where to install blue lines was the condition of the road and the likelihood that the road would be maintained. In cases where existing road conditions were poor or road maintenance was infrequent, communities installed signs rather than blue lines. Local governments also had to follow existing AASHTO (American Association of State Highway and Transportation Officials) road signage guidelines so that tsunami signs were not in conflict with existing signage (S. Absher & A. Rizzo, personal communication, December 3, 2021).

[7] These community meetings were attended by a variety of stakeholders including community residents, second home owners, realtors, business owners, short term rental management companies, utility districts, and local emergency management personnel like the fire district chief and the county sheriff (S. Absher & A. Rizzo, personal communication, December 3, 2021).

[8] To measure only the "pure" information effect due to the Tohoku earthquake and tsunami and not the effect of damages from the tsunami, Curry County (the southernmost county in Oregon) was intentionally excluded from the potential study area because the port of Bookings experienced much higher damage than any other coastal community in Oregon. With this limitation, the costs to the Oregon coast are then primarily the indirect cleanup costs of debris from Japan and not direct infrastructure damage. According to local newspapers, the majority of damage occurred in southern Oregon and northern California (Jung, 2011; Tobias, 2012).

land in Oregon's tsunami inundation zone is classified as undeveloped, the tsunami zones in Clatsop, Lincoln, and Tillamook counties have 48, 34, and 21%, respectively zoned as urban (Wood, 2007). The northern coast cities contain the highest number of public venues and dependent-population facilities like schools and hospitals in the tsunami inundation zone. These cities also have the highest percentages of their employees in the tsunami inundation zone (Wood, 2007). In 2018, the population of these counties was: 39,200 in Clatsop, 26,395 in Tillamook, and 48,210 in Lincoln (Secretary of State, n.d.-b). All three of these counties are rural with the largest city – Newport, the county seat of Lincoln County – having a population of 10,125 in 2018 (Secretary of State, n.d.-a). Population and housing are concentrated primarily in the small incorporated and unincorporated coastal towns of these counties. Clatsop County has five incorporated towns, Tillamook County has seven, and Lincoln County has six. As of 2007, approximately 36% of residents in the tsunami inundation zone lived in rural, unincorporated areas of the seven coastal counties, primarily in the unincorporated towns of the three northern counties (Wood, 2007).

Oregon's Office of Economic Analysis (OEA) groups the three counties together as a regional economy. It is reasonable to consider these counties as a single housing market given their separation from Oregon's population centers in the Willamette Valley, their connection via Highway 101, and their similar economies and industries. These three counties span approximately 150 miles in the north-south direction. While it is unlikely that someone would commute over three hours from Yachats (the southernmost town) to Astoria (the northernmost town) for work, it is plausible that people would commute half that distance.[9] Figure 2.4 shows a map of the three northern counties (green hatching) and the boundaries of the seven coastal counties (black). The map also illustrates the clustering of and connections between population centers on the coast, the

[9] According to the 2009-2013 American Community Survey's 5-year Commuting Flows, 201 residents of Clatsop county commuted to Tillamook county for work, compared to 15,513 residents who worked and lived in Clatsop county (U.S. Census Bureau, 2013). Of Lincoln County residents, 18,312 worked in the same county and 102 commuted to Tillamook County for work. For Tillamook County, 9,182 residents worked in the same county, 365 residents commuted to Clatsop County, and 185 commuted to Lincoln County. Between 2009 and 2013, Clatsop and Lincoln were the counties with largest commuting flows from Tillamook County. This suggests that only 2-3% of residents in Tillamook County commute to the other two counties and even less than that commute from those two counties to Tillamook. While this result does not suggest significant commuting between these counties, there is even less commuting to other adjacent counties (excluding commuting to the metro areas of Portland, Salem, and Eugene). Given the 150-mile span of these counties and the commuting patterns between Tillamook County and its adjacent coastal counties, these three counties could plausibly be grouped into one fairly diffuse housing market.

lack of population along the Oregon Coast Range, and the separation from the urban centers in the adjacent Willamette Valley counties.

The second and third analyses have more narrowly defined sample spaces that contain a limited number of treated observations, necessitating an expansion to include housing data from all seven coastal counties. For example, I have tsunami blue line data for only eleven coastal communities and some of these communities (e.g., Cannon Beach) received as few as three blue lines. These blue lines were installed at times between 2016 and 2019, which results in a short post-installation time range and therefore few property sales after installation for the DID model in the third analysis. This extension assumes that the entire Oregon Coast can be treated as a single housing market, as in Dundas and Lewis (2020). Under this assumption, the three northern coast counties comprise a sub-market of this larger housing market.

2.4 Data

Property sales data were obtained using Zillow's ZTRAX database from 2009 through 2018 (Zillow, 2020). These data were cleaned to remove all non-residential transactions and transactions missing key structural variables (e.g., bedrooms, age). In each year, transactions with prices in the bottom one percent were removed because they may reflect non-arms-length transactions (e.g., intra-family transfer). Transactions in the top one percent in each year were also removed to reduce the influence of outliers in the analyses. Houses that sold more than five times between 2009 and 2018 were dropped because of potential unobservables driving their frequent resale. Potential multi-family dwellings – properties with more than eight bedrooms or six bathrooms – were dropped from the sample. Finally, transactions that took place less than one year since the previous sale were removed since they often reflected either the same transaction recorded at multiple points through the sale process or a house purchased to be flipped and re-sold. Remaining transactions contain only arms-length, single-family residential sales that reflect the valuations of potential homeowners.[10] Some of the key structural covariates from the Zillow data include the effective

[10] The Zillow ZTRAX data does not have reliable second home indicators so identifying second home ownership is not possible at this time. Second homes and vacation rentals constitute a large share of housing in the northern counties due to the dominance of the tourism sector on the Oregon coast. According to the 2019 *Clatsop County Housing Strategies Report* (Appendix A, 2019) the estimated vacancy rate of ownership housing is very high, especially in beachside communities. They also find that in several beachside communities short-term rentals have outpaced the addition of new units; an estimated 58% of new houses built in the county since 2010 are used as short-term rentals (*Clatsop County Housing Strategies Report*, Appendix A, 2019). Second

age of the house (2018 – remodel year), indoor square footage, total acreage, number of bedrooms, number of bathrooms, and whether the house has a garage.

Neighborhood and location amenity data for each parcel in the study area are collected from several state and federal sources. Much of the data comes from the Emergency Preparedness Data Collection, the public version of a dataset compiled by Oregon's Preparedness Framework Implementation Team (Prep-FIT) for the Oregon Incident Response Information System (OR-IRIS). This dataset is a collection of existing and purpose-built GIS datasets combined to help understand the setting of a potential emergency response incident (Preparedness Framework Implementation Team (Prep-FIT), n.d.). Sources of the OR-IRIS data include state agencies such as the Oregon Department of Transportation (ODOT) and federal agencies such as USGS. This data includes location information for airports, fire stations, hospitals, wastewater treatment plants, beach access points, highways and roads, railroads, rivers and other waterbodies, the ocean shoreline, and cities. Distance to the nearest central business district is measured as the distance to the center of the nearest town (incorporated or unincorporated). Distances to the nearest hospital, law enforcement station, fire station, and wastewater treatment plant were included since proximity to one of these facilities may serve as a proxy for a "safety" amenity.

Location information on state and federal protected areas (public lands) came from the USGS Protected Areas Database of the United States (PAD-US). Federal public lands include conservation areas, national forests, national historic sites, national monuments, national parks, national recreation areas, national wildlife refuges, wilderness areas, and recreation or resource management areas. State public lands include only state forests, state parks, and wildlife management areas. Elevation data was collected in 10m-by-10m pixels from DOGAMI. GIS software was used to calculate the elevation of each property and the distance from each property to the nearest location amenity.[11] For oceanfront properties, additional data on shoreline armoring and armoring eligibility is included. Shoreline armoring is a private option to protect oceanfront

homeowners who do not live on the Oregon coast and directly face the risk of a Cascadia tsunami may have different risk perceptions and preferences than permanent residents of the Oregon coast. Accounting for second home ownership is therefore important for accurately estimating residents' risk perceptions.

[11] All distances are Euclidian. Euclidian distances may underestimate true distances in these rural counties. Also, Euclidian and travel distances may capture different amenities. For example, I would expect that as travel distance to the nearest beach access point increases, property values decrease since beach access is an amenity. However, Euclidian distance to a beach access point may primarily capture the visual disamenity of congestion at popular beach access points.

properties from erosion and storm surges by installing hardened shoreline protection structures.[12] Armoring eligibility and the existence of shoreline protective structures represent safety amenities for oceanfront properties. Oceanfront parcels were identified using the Oregon Department of Land Conservation & Development's inventory of oceanfront parcels and their armoring eligibility.

Several studies have used changes in the number of insurance policies following a disaster event as a measure of changing subjective perceptions about the expectation of a future disaster (Atreya et al., 2013; Gallagher, 2014). This study omits insurance information due to a lack of parcel-level earthquake and flood insurance data.[13] Finer-scale fixed effects, however, should be able to capture some of the unobservable heterogeneity due in part to earthquake insurance uptake differences between neighborhoods. Parcels are assigned to a Census block group, areas that generally contain between 600 and 3,000 people, to be used for these neighborhood-level spatial fixed effects. The block group is the smallest geographical unit above the block level that is uniquely identified and therefore represents the smallest neighborhood unit data available.

Earthquake insurance, however, only covers damage from strong shaking but not water damage from a tsunami (OSSPAC, 2018). Tsunami damage is typically covered by flood insurance (OSSPAC, 2018). FEMA's National Flood Insurance Program (NFIP) requires the purchase of flood insurance for mortgages in the 100-year floodplain – also known as Special Flood Hazard Areas (SFHA) – that are managed by federally regulated lenders.[14] Mortgage lenders must also inform homebuyers if the property is in the SFHA. On the Oregon coast, the SFHA floodplain line is similar but not identical to the tsunami inundation lines (OSSPAC, 2018). For example, for the first analysis, only 3% of properties outside the SB 379 tsunami inundation zone are inside a SFHA; however, 36% of properties inside the SB 379 inundation zone are also inside a SFHA

[12] Oregon's Statewide Planning Goal 18 designates which parcels are eligible to install shoreline armoring (Department of Land Conservation & Development, n.d.-a, p. 18). To limit shoreline armoring and resulting beach erosion and loss of beach access Goal 18 limits shoreline armoring to parcels where development existed prior to 1977.

[13] Most homeowner insurance policies in Oregon do not cover earthquake damage though many homeowners insurance providers offer standalone earthquake coverage and earthquake insurance is widely available through the state of Oregon (Division of Financial Regulation, n.d.). As of 2017 approximately 14.8% of Oregonians with residential homeowners insurance also have earthquake insurance (Cheng, 2018). This is comparable to other Pacific Coast states with high earthquake risks, e.g., Washington's uptake rate of 11.3% and California's uptake rate of 15.1%. Earthquake insurance data is only available at the county level and the variation in insurance uptake between the coastal counties is too low for the county-level information to be useful.

[14] Neither the NFIP nor the state of Oregon require the purchase of flood insurance for mortgages in tsunami inundation zones. Therefore, Oregon homeowners do not have insurance requirements specific to being inside the tsunami inundation zone.

(Table 2.1). These homes in both the tsunami inundation zone and in the SFHA likely have flood insurance. Therefore, even without fine-scale flood insurance policy data, it may be possible to use presence in a SFHA to roughly proxy for flood insurance ownership inside the tsunami inundation zone. This SFHA indicator will underestimate the amount of flood insurance policies because, while most homes inside the SFHA have flood insurance, some homes outside the SFHA may also have flood insurance but will not be picked up by the SFHA indicator.

For the first analysis, the sample space of transactions was limited to those properties within 1 mile of the original tsunami inundation zone (SB 379). This removes non-coastal properties on the eastern side of the county from the sample. Non-coastal properties likely have different amenity sets than coastal properties so their removal from the sample better controls for omitted neighborhood and location amenities. A distance of 1 mile from the SB 379 line captures all of the towns in the three counties and does not extend into large rural or forest parcels on the eastern sides of the counties.[15] The temporal extent of the first analysis is 2009 to 2017 so that each event – the 2011 earthquake and the 2015 article – is bracketed by two years of property sales data before and after the event. The Zillow data spans the years 2009 to 2017 and contains 15,627 transactions.[16]

The tsunami inundation zones that define the treatment group in the first analysis include the 1995 SB 379 line and the largest of the 2013 TIM scenarios (XXL). Table 2.1 compares the descriptive statistics of houses inside and outside the 1995 SB 379 tsunami inundation zone to illustrate differences between the treatment and control groups for the sample used in the first analysis. Approximately 27% of the transactions between 2009 and 2017 were inside the SB 379 inundation zone. The houses inside and outside the SB 379 zone are similar in terms of effective age, total acreage, number of bedrooms and bathrooms, and whether they have a fireplace or external structures (e.g., garage, patio, fencing). Houses inside the inundation zone on average sell for $16,000 more which likely reflects the shorter distances to likely amenities such as the ocean, rivers, public lands, and schools and the greater distances to likely disamenities such as highways.

[15] Distance to the SB 379 tsunami inundation zone was chosen instead of distance to the shoreline only because the ocean shoreline data does not extend into the Columbia River on the northern boundary of the three-county area and the SB 379 data does extend into the Columbia.

[16] Table A1 in Appendix 6.3 presents summary statistics for the sample used in the first analysis, i.e., for 2009-2017 property sales that occur within 1 mile of the 1995 SB 379 line in the three northern counties.

Houses outside of the inundation zone have larger indoor square footage and total acreage which may be due to the higher density of houses inside the inundation zone. Approximately 99% of the houses inside the SB 379 inundation zone are also in the 2013 XXL scenario inundation zone. The XXL scenario of the 2013 TIM series was in use for official tsunami evacuation maps during the 2015 New Yorker article. Approximately 49% of the transactions between 2009 and 2017 were in this inundation zone.[17] The change in tsunami inundation and evacuation maps between the two events of interest presents a model specification problem that is addressed in section 2.5.1. See Appendix 6.2 for figure comparisons of the 2013 TIM and 1995 SB 379 tsunami inundation scenarios for the city of Tillamook.

The last column of Table 2.1 presents the standardized difference in means for the structural and location covariates. Several key explanatory variables such as elevation (1.51) and distance to the ocean shoreline (0.55) have large absolute standardized differences (in parentheses). Some researchers have suggested that an absolute standardized difference of 0.25 or more indicates that covariates are imbalanced between groups (Stuart, 2010). This suggests that the treated and control groups are considerably imbalanced and that covariate balancing, e.g., matching or weighting, may be useful or necessary for identification.

For the second analysis, the sample space of transactions is limited to those properties that were outside of the original 1995 SB 379 tsunami evacuation zone. The 2013 update of tsunami inundation and evacuation maps represents an exogenous risk signal to houses that were outside of the original 1995 SB 379 inundation zone but with the hazard planning change found themselves inside one of the new 2013 inundation zones. As such, each of the five 2013 tsunami inundation zones is used as the treatment boundary for a separate sample where the sample is restricted to a narrow band of properties within 1 mile of the treatment boundary given by the XXL, XL, L, M, or SM inundation line. Table 2.2 compares the samples of the resulting five different sample spaces and lists the number of transactions inside and outside the given inundation zone for each sample. This table illustrates the data limitations of this analysis even after extending the sample space to all seven coastal counties, as can be seen by the small number of treated observations (81) available for the SM inundation line treatment boundary sample. The time range for this analysis is from

[17] See Table A1 in Appendix 6.3.

2011 to 2015 so that the 2013 evacuation map change is bracketed by two years of property sales data before and after the event.[18]

The third analysis restricts the sample space to a small neighborhood of properties around newly installed blue lines and the 2013 XXL inundation line. The preferred model restricts treated observations to be within 1000' of the blue line and control observations to be within 2500'. The temporal extent of the sample is 2014 to 2018 so that each blue line has at most two years of property sales before and after its installation since the blue lines were installed at different times between 2016 and 2019.[19] Table A4 in Appendix 6.3 compares the descriptive statistics of houses inside and outside the blue line neighborhood given by a 1000' radius to illustrate differences between the treatment and control groups for the sample used in the preferred model. This table shows that the standardized differences in means for this sample space are small in comparison to the sample spaces of the first and second analyses. This suggests that the narrow sample space definition successfully restricts neighborhoods to be more homogenous and thus may help deal with time-invariant and time-varying unobservables that may be correlated with either proximity to the blue lines or the 2013 XXL line.

A database of blue line locations and installation dates does not exist at the state or county levels. Thus, information about when and where the blue lines were installed was gathered by contacting individual city and county emergency managers, public works departments, and planning departments along the Oregon coast. Emails and phone conversations were used to compile a list of approximate blue line locations and installation month and year.[20] Some locations were given as being in the vicinity of street intersections or nearby landmarks so I approximate the

[18] Table A2 in Appendix 6.3 presents summary statistics for the sample used in Model 1 of the second analysis, i.e., for 2011-2015 property sales that are outside the 1995 SB 379 line and are within 1 mile of the 2013 XXL line in the seven coastal counties. This is the largest sample space in the second analysis and encompasses the other four sample spaces. Table A3 in Appendix 6.3 compares the descriptive statistics of houses inside and outside the 2013 SM tsunami inundation zone to illustrate differences between the treatment and control groups for the sample used in Model 5. This is the smallest sample space and has the largest standardized differences in means. Descriptive statistics for the remaining samples used in this analysis are not presented here but are available upon request.

[19] For blue lines installed in 2018 less than one year of property sales is available post-installation. For blue lines installed in 2019, there are no post-installation property sales. This is due to a lack of updates to ZTRAX housing transactions after 2018 for most Oregon counties (as of June 2021).

[20] For some blue lines, no timing information other than the year of installation was available. This ambiguity of installation dates further reduces the post-installation time range for the DID and DDD models. Timing and location information is currently incomplete for several towns that are known to have blue lines installed, usually due to multiple blue line installation periods or uncertainty about whether some blue lines were installed. Due to the potential non-randomness of this missing data, these towns were not included in the dataset analyzed in this paper.

location of the blue line based on the location of the 2013 XXL tsunami inundation line and this firsthand information.

2.5 Methodology

Previous literature has measured the impact of risk on housing prices using hazard events and/or regulatory hazard delineation (see section 2.2). For example, Hallstrom and Smith (2005) used a hedonic price function within a simple, two outcome expected utility model to demonstrate how an information shock provided by a nearby hurricane can change individuals' subjective probability of a coastal storm causing damage and how this change in subjective risk impacts property values. This model can be modified for the case of tsunami risk to show that if the information shocks increased individuals' subjective probability of a Cascadia earthquake and tsunami, the change in subjective risk should then decrease the hedonic price function. See Appendix 6.1 for the modified expected utility model and result.

In all three analyses, I make the standard simplifying assumption and take the housing market equilibrium as given with hedonic prices as equilibrium outcomes. I follow the first-stage hedonic approach to estimate the marginal capitalization effects of the tsunami risk signals. An identification issue for coastal risk studies is the difficulty of distinguishing amenity and risk effects. Because the distance to the coast serves as both a proxy for coastal amenities and for increased risk of damage (Hallstrom & Smith, 2005), new information in the form of an exogenous event or shock is needed to disentangle coastal amenities from the tsunami risk disamenity.

2.5.1 First analysis: 2011 Tohoku earthquake and tsunami and 2015 New Yorker article

Two exogenous information shocks are used to distinguish between the effect of coastal amenities and the increased subjective risk of tsunami inundation. I use a difference-in-differences (DID) hedonic model to difference out time-invariant omitted variables and contemporaneous effects such as macroeconomic shocks. There is a complication with defining the treatment group (inside the tsunami inundation zone) and control group (outside of the inundation zone) because the tsunami inundation maps changed in 2013 from the SB 379 line to the new DOGAMI series. This motivates three model specifications. For the first specification (Model I), I consider only the Tohoku earthquake event and the 1995 SB 379 tsunami line as the boundary between the treatment and control groups. The time range for this specification is from 2009 to 2013 (before the DOGAMI tsunami inundation maps change). The model specification is:

$$ln(price_{ict}) = \boldsymbol{X}'_{it}\beta_1 + \beta_2 sb379_i + \beta_3 tohoku_t + \delta_1 sb379_i * tohoku_t$$
$$+quarter_t + blckgrp_c * year_t + \varepsilon_{ict} \quad (1)$$

where $price_{ict}$ is the sale price (in constant 2019 dollars) of house i with structural and location characteristics $\boldsymbol{X}$ in Census block group c at time t. The log transformation of $price_{ict}$ was chosen as the dependent variable in all models because taking the log of $price$ narrows its range and can make estimates less sensitive to extreme values. The treatment variable $sb379_i$ indicates whether the house is in the tsunami inundation zone given by the 1995 SB 379 scenario. The event variable $tohoku_t$ indicates that the sale happened after 3/11/2011 (the post-Tohoku period).[21] The parameter of interest is δ_1, the marginal effect of the Tohoku 2011 earthquake and tsunami on property values inside the tsunami inundation zone given by the 1995 SB 379 scenario. The structural characteristics in $\boldsymbol{X}_{it}$ include quadratic terms for the non-binary variables to better account for their expected diminishing effect on property prices (e.g., Atreya et al., 2013; Bin & Landry, 2013). I also follow previous hedonic studies and take log transformations of the distance variables (originally in feet) in $\boldsymbol{X}_{it}$ to abstract from unit issues (Atreya et al., 2013; Bin & Landry, 2013). The temporal fixed effects $quarter_t$ were included to capture any seasonal (90-day) heterogeneity or shocks that affect all property sales. The Census block group spatial fixed effects $blckgrp_c$ are interacted with the annual fixed effects $year_t$ in $blckgrp_c * year_t$ to capture how these neighborhoods are changing over time. These spatial-temporal fixed effects soak up annual changes at the neighborhood level such as storm surges and allow neighborhoods to flexibly differ in their recoveries from the subprime mortgage crisis and Great Recession.[22]

Model II considers the New Yorker event and the largest scenario (XXL) of the new 2013 tsunami zones as the boundary between treatment and control groups. The time range for this specification is 2013 – 2017. While the SB 379 is most comparable to the M and L scenarios by

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[21] The $tohoku_t$ event variable is defined as between 3/11/2011 and 7/20/2015 (the post-Tohoku period and pre-New Yorker article period). Since the time range for Model I is from 2009 to 2013, the $tohoku_t$ variable equals 1 for all sales during this time that occur after the Tohoku earthquake and tsunami on 3/11/2011. The $tohoku_t$ variable definition is discussed further in the Model III specification section.

[22] The appropriate scale at which Great Recession recovery is capitalized may be at shorter time scales, i.e., at the $blckgrp_c * quarter_t$ scale. This fixed effect is tested as a robustness check.

area, the XXL scenario was chosen as the treatment for Model II because it is the most extreme scenario. I expect that households willing to pay a risk premium to avoid tsunami inundation will likely choose to locate outside the entire region of potential tsunami inundation. The XXL scenario is also the scenario used by DOGAMI to create their tsunami evacuation maps, making it the most salient scenario for the public at large. The model specification for Model II is:

$$ln(price_{ict}) = \boldsymbol{X}'_{it}\beta_1 + \beta_2 xxl2013_i + \beta_3 article_t + \delta_1 xxl2013_i * article_t + quarter_t + blckgrp_c * year_t + \varepsilon_{ict} \quad (2)$$

The treatment variable $xxl2013_i$ indicates whether the house is in the tsunami inundation zone given by the 2013 XXL scenario. The event variable $article_t$ indicates the sale happened after 7/20/2015 (the post-New Yorker article period). The parameter of interest is δ_1, the marginal effect of the 2015 New Yorker article on property values inside the tsunami inundation zone given by the 2013 XXL scenario.

Model III incorporates the New Yorker article event into Model I and keeps the 1995 SB 379 tsunami line as the treatment boundary. Since the 2013 tsunami inundation maps are only two years old and the 1995 map had been in circulation for 20 years by the New Yorker article's publication, there could be a lag in the public's knowledge and acceptance of the new tsunami boundaries. This specification assumes an information lag and that homebuyers place more importance on the long-standing SB 379 line when choosing where to locate. The time range for this specification is 2009 to 2017. The DID model specification for Model III is:

$$ln(price_{ict}) = \boldsymbol{X}'_{it}\beta_1 + \beta_2 sb379_i + \beta_3 tohoku_t + \beta_4 article_t + \delta_1 sb379_i * tohoku_t + \delta_2 sb379_i * article_t + quarter_t + blckgrp_c * year_t + \varepsilon_{ict} \quad (3)$$

The implicit assumption in the definition of the $tohoku_t$ variable here is that the impact of the 2011 Tohoku earthquake/tsunami on property values decreases over time and disappears by the New Yorker article in 2015. This assumption follows previous findings that risk premiums decay over time and may disappear if additional disaster events do not occur (Atreya et al., 2013; Bin & Landry, 2013; Hansen et al., 2006; Kousky, 2010; McCluskey & Rausser, 2001; McCoy & Walsh, 2018). The parameters of interest are δ_1 and δ_2, the marginal effects of the 2011 earthquake/tsunami and 2015 article on property values inside the tsunami inundation zone given by the 1995 SB 379 scenario.

Consistent estimation of these treatment effects requires the parallel trends assumption. The parallel trends assumption requires that absent the two information shocks, the difference in unobserved property price drivers between properties inside the tsunami inundation zone and outside the tsunami inundation zone would have remained constant. I assess the validity of this assumption in Figure 2.5, which plots residual housing prices inside and outside of the treatment inundation line – SB 379 or 2013 XXL, depending on the model – for the three northern counties. To account for observable differences across houses, I first regress log sale prices on structural attributes, location covariates, and fixed effects for quarter and Census block group by year. I then aggregate the residuals to the group (treated or control) and month level and plot these residuals over time using local polynomial regressions. Figure 2.5(a) plots the housing price trends inside and outside of the 1995 SB 379 tsunami inundation zone for Model I's time range – March 2011 to March 2013. Adjusted prices of the treated group before the 2011 Tohoku earthquake and tsunami exhibit a similar trend as those of the control group. Following the 2011 Tohoku event, residual prices for the treated group initially drop but then recover to nearly pre-treatment levels by 2013.[23] Figure 2.5(b) plots the housing price trends inside and outside of the 2013 XXL tsunami inundation zone for Model II's time range – July 2013 to July 2017. Before the 2015 New Yorker article, the treated group exhibits a similar trend as the control group. However, residual prices for the treated group appear to increase following the 2015 article event.

Following the estimation of the DID regressions, I test whether the resulting risk discounts decay over time. However, the literature on how to measure these decay effects is not standardized and a variety of methods exist that attempt to measure the decay effect. I use a method similar to the one used by Bin and Landry (2013). This method uses only data after the event and regresses log sale prices on the treatment variable, a count of months between the event and the month of sale ($monthpost_t$), and the interaction between the two. For example, the specification for the SB 379 tsunami inundation zone is:

[23] Following the 2011 Tohoku event, residual prices for the control group initially increase but then recover to nearly pre-treatment levels by 2013. This unexpected increase in control group residual prices could be suggestive of a substitution effect between groups in coastal communities. For example, if residents prioritize remaining in or near their coastal community over moving to another – potentially distant – community, then the information shock of the 2011 Tohoku event may decrease demand for parcels inside the tsunami inundation zone (treatment group) and increase demand for parcels outside the zone (control group).

$$ln(price_{ict}) = \boldsymbol{X}'_{it}\beta_1 + \beta_2 sb379_i + \beta_3 monthpost_t + \beta_4 sb379_i * f(monthpost_t) + quarter_t + blckgrp_c * year_t + \varepsilon_{ict} \quad (4)$$

Different specifications are used for $f(monthpost_t)$ transformation including linear, log, square root, and ratio specifications, i.e., $monthpost_t$, $\ln(monthpost_t)$, $\sqrt{monthpost_t}$, $\frac{monthpost_t - 1}{monthpost_t}$. The parameter of interest is β_4, the coefficient on the interaction between the $f(monthpost_t)$ transformation and the treatment variable. A positive and statistically significant coefficient suggests that the risk premium is decaying over time (Bin & Landry, 2013).

As a robustness check, I run a Oaxaca-Blinder regression (Blinder, 1973; Oaxaca, 1973). The Oaxaca-Blinder regression decomposes the difference in average outcomes into a component that is explained by group differences in the predictors and a part that remains unexplained by these differences. This second component is called the unexplained component and can be interpreted as the average treatment effect on the treated (ATET), much like the DID estimator (Fortin et al., 2010; Słoczyński, 2015). In the Oaxaca-Blinder regression weights are used to generate exact covariate balance between treated and control groups (Kline, 2011). The Oaxaca-Blinder estimator is "doubly robust" in that it is consistent if either the model for the potential outcomes or the model for the propensity score is correct (Kline, 2011). The Oaxaca-Blinder estimator is also easily implemented in unbalanced designs with few treated units and many controls (Kline, 2011) and has been used previously in a coastal hedonic setting (Dundas, 2017). Practically, I compute the two-fold decomposition using the coefficients from a pooled model over both groups (treated and control) as the reference coefficients (Jann, 2008). The treated group is those houses inside the given inundation zone after the event, i.e., the treated group is represented by the DID interaction term. Thus, the Oaxaca-Blinder estimator can be computed for Models I and II but not for Model III since Model III contains two events and therefore two treated groups. As an alternative to the DID specification and as another robustness check, I also specify event study designs for the models with only one event of interest (Models I and II). The event study design extends the standard DID by replacing the single "post event" indicator with binary lead and lag variables that indicate whether the given observation occurred a given number of quarters away from the event of interest.

An important identification concern is the covariate imbalance found for several key explanatory variables. Estimating average treatment effects using ordinary linear regression methods becomes more challenging when there is considerable imbalance in covariates between the treatment and control groups. Matching and weighting methods were developed to estimate average treatment effects under weaker assumptions by avoiding distributional and functional form assumptions (Imbens, 2004). Matching methods can also be used to preprocess data to improve causal inference (Ho et al., 2007). Methods that combine matching (to preprocess the data) and regressions are more robust against misspecification of the regression function than regressions alone (Imbens, 2004).

To improve covariate balance and potentially increase robustness against model misspecification I pre-process the data using four matching/weighting methods – nearest neighbor propensity score matching (PSM), nearest neighbor Mahalanobis (NNM) distance matching, coarsened exact matching (CEM), and entropy balancing (EB) as robustness checks. Although they are popular matching methods, both PSM and NNM are also members of a class of methods known as "Equal Percent Bias Reducing" (EPBR), which have been shown to not guarantee imbalance reduction for any given data set and to rely on a set of strict and unverifiable assumptions about the data generating process (Iacus et al., 2011, 2012). Iacus et al. (2011) introduce a new class of matching methods that have many attractive properties and require fewer assumptions. In one of these methods, CEM, each variable is coarsened so that similar values are grouped into a stratum and assigned the same value. Then, an exact matching algorithm is applied to the coarsened data so that control units within each stratum are weighted to equal the number of treated units in that stratum. Strata without at least one treated and one control unit are discarded. The remaining units with their original uncoarsened variable values form the matched data set. Entropy balancing is a weighting method (Hainmueller, 2012) that, like CEM, specifies constraints on covariate balance before the preprocessing adjustment. Entropy balancing is designed to improve balance on all covariate moments by directly incorporating covariate balance into the weight function applied to the data. This method directly adjusts the unit weights of the control group to match the moments of the treatment group while also keeping the control weights as close as possible to the base weights. Unlike CEM, entropy balancing does not discard treated units.

While there are various guidelines for selecting variables for matching, there is a consensus that only those covariates anticipated to influence both treatment and the outcome variable should be included (Brown & Atal, 2019; Caliendo & Kopeinig, 2008). The explanatory variables that likely influence treatment (tsunami inundation zone) assignment are elevation and distance to the ocean. I also match on the event(s) of interest to distinguish potential matches between pre and post event.[24] To further anchor the matched observations in time, I match on the year the property was sold (Muehlenbachs et al., 2015). For the PSM and NNM matching methods, I use a *k*-nearest neighbor matching (*k=1*) algorithm with replacement. Matching with replacement is recommended when there are few comparable control observations, as here (Caliendo & Kopeinig, 2008). For the CEM method, I use the default Sturges binning algorithm to coarsen the data. The EB method does not discard units, unlike the other three methods, and instead generates weights to be used in the DID regressions.[25]

Lastly, I perform four sets of falsification tests. In the first and second sets of tests I shift the date of the 2011 Tohoku earthquake/tsunami in Models I and III to one year before the true event and to one year after the true event, respectively, as in Atreya and Ferreira (2015). In the third and fourth sets of tests, I follow Bakkensen et al. (2019) and randomize treatment exposure in both the spatial (randomly assign sales to either the control or treatment group in all three models) and temporal (randomly assign sales to either pre- or post-event in Models I and II) dimensions.

2.5.2 Second analysis: 2013 change in tsunami evacuation maps

The second analysis uses residential housing sales data before and after the 2013 tsunami inundation and evacuation map change to measure its impact on coastal Oregon property values. Since there are five 2013 inundation zones in the TIM Plate 1 map series, I need to specify five different models to capture all relevant event and treatment combinations. Model 1 uses the XXL line as the treatment boundary, Model 2 uses the XL line, Model 3 uses the L line, Model 4 uses the M line, and Model 5 uses the SM line. The sample is comprised of properties outside of the 1995 SB 379 evacuation zone and restricted to a narrow 1-mile band of properties around the

[24] NNM allows for exact matching the event variable.
[25] The other three matching methods can also generate weights to be used in the DID regressions.

treatment boundary given by the XXL, XL, L, M, or SM inundation line, depending on the model. Thus, the control group consists of properties that are not in either (1995 or 2013) evacuation zone and the treatment group consists of properties that were not in the 1995 SB 379 evacuation zone but following the map change are in the XXL, XL, L, M, or SM inundation zone. The DID specification is:

$$ln(price_{ict}) = \boldsymbol{X}'_{it}\beta_1 + \beta_2 tsu2013_i + \beta_3 newmaps_t + \delta_1 tsu2013_i * newmaps_t + quarter_t + blckgrp_c * year_t + \varepsilon_{ict} \qquad (5)$$

where the treatment variable $tsu2013_i$ indicates whether the house is in the tsunami inundation and evacuation zone given by one of the five 2013 inundation zones. The event variable $newmaps_t$ indicates that the sale happened after the 2013 map change (10/2/2013 and later).[26] The time range for this specification is 2011 to 2015 so that the 2013 evacuation map change is bracketed by two years of property sales data before and after the event as well as to avoid contamination from the two events studied in the first analysis. The parameter of interest is δ_1, the marginal effect of the 2013 map change on property values outside of the original 1995 SB 379 inundation zone and inside a new 2013 inundation zone. This analysis uses the same temporal and spatial-temporal fixed effects as the first analysis.[27] The structural characteristics in $\boldsymbol{X}_{it}$ now also contain the distance from the property to the 2013 XXL tsunami inundation zone (for properties that are inside that zone). This variable is a proxy for distance to safety with safety represented as being outside of the entire region of potential tsunami inundation.

I assess the validity of the parallel trends assumption as in the first analysis. Figure A3 in Appendix 6.4 plots residual housing prices inside and outside of the treatment inundation line – XXL, XL, L, M, or SM – for the seven coastal counties. The takeaway from these plots is that before the 2013 map change only Model 1 (XXL line) and Model 5 (SM line) have treated and control groups that exhibit parallel pre-trends. However, counterintuitively, in Model 1 the residual

[26] DOGAMI released updated tsunami inundation maps by county throughout 2013. An October 2nd, 2013 news release by DOGAMI states that inundation maps had been released for the entire coast, suggesting that this date could be considered as the date of completion for the map change (DOGAMI, 2013).

[27] Covariate imbalance is an identification concern for several models in this analysis, e.g., Model 5 has large standardized differences in means for several key explanatory variables (see Table A3 in Appendix 6.3). Models 1 and 2 have less covariate imbalance than Models 3 through 5. However, the number of observations for Models 3, 4, and 5 (see Table 2.2) is too small for the matching methods to be able to produce useful matched samples. Thus, I forego matching or weighting for the models in this analysis.

prices for the treated group appear to increase following the 2013 map change. In fact, Model 5 is the only model where the residual prices for the treated group appear to drop following the 2013 map change, as expected.

As a robustness check, I estimate a pooled model with all five 2013 tsunami inundation zones as treatments in a single model. This model uses the sample space of Model 1 (XXL line) because it encompasses the samples of the other four models. Similar to the first analysis, I also run Oaxaca-Blinder regressions, specify event study designs, and perform the four sets of falsification tests for all five models. Lastly, I test whether the risk discounts from the DID regressions decay over time using the method of Bin and Landry (2013).

2.5.3 Third analysis: Tsunami Blue Line project

The third analysis measures the impact of the Tsunami Blue Line project on coastal Oregon property values using residential housing sales data before and after the installation of the blue lines. Starting in 2016 the Tsunami Blue Line project installed thermoplastic blue line signs on the 2013 XXL tsunami inundation and evacuation line. Properties are differentiated by proximity to blue lines and by whether they are inside the 2013 XXL tsunami inundation and evacuation zone. The sample is restricted to a circular neighborhood of properties around the blue lines, signifying that those properties are adjacent to a blue line. Circular neighborhoods are the result of defining proximity to a blue line using a single distance, i.e., a distance radius will trace out a circular neighborhood or buffer around that blue line. This also restricts the sample to small neighborhoods around the 2013 XXL line. In practice I use two different types of distances to define the circular treatment and control buffers: Euclidian distances, which measure the straight-line distance between each blue line and transaction, and road network distances, which measure the shortest path between each blue line and transaction along the road network. Figure 2.6 shows a taxlot map with example treatment and control groups around a blue line (small blue squares) in Lincoln City, OR. The treatment group is given by those property sales (small black circles) inside the neighborhood around the blue line (red circular buffer). The corresponding control group is those property sales outside of the blue line neighborhood (red circular buffer) but inside a slightly larger neighborhood surrounding it (green circular buffer). The 2013 XXL inundation and evacuation line (thick purple line) separates houses that are more sensitive to the blue line treatment – houses inside the inundation zone – from those that are less sensitive to the treatment. One identification issue is how to deal with overlapping neighborhoods for blue lines that are in close proximity to each other. For example, Figure 2.6 shows that the control group (green circular buffer) encompasses two other blue lines.[28] This impacts how I define the treatment indicator.

Two new binary indicators are needed for the DID and DDD models: treatment and event. The treatment variable indicates whether the house is in the neighborhood around the blue line, which is complicated by the potential for multiple blue line neighborhoods to overlap a transaction.[29] The event variable indicates that the sale happened after the blue line was installed, which is also complicated by the problem of "which blue line?" To generate these indicators and deal with the overlap issue I focus on the timing of treatment instead of on spatial controls. The key idea is that "earliest supersedes nearest." If a transaction lies within a given buffer distance of two different blue lines and one of the blue lines is installed before the transaction and the other is installed after the transaction, I use the first installed blue line as the reference point, not the nearest blue line. In case there is a tie for earliest – multiple blue lines were installed at the same time – then the nearest blue line is chosen. To create the "treatment" variable, I consider all possible cases of buffer overlap. The key question is how should we treat transactions that fall in one blue line's "treatment" buffer and another blue line's "control" buffer? There are nine total cases that can occur when a treatment buffer and control buffer overlap for a transaction. Appendix 6.4 illustrates all nine cases and explains how treatment and event status were defined. Essentially, if multiple blue lines fall within a given radius (buffer distance) of the transaction in question, one blue line is chosen as the appropriate reference point. Then, the values of the treatment and event indicators are determined by whether the transaction is within the given radius of that blue line and whether the sale occurred after the blue line was installed, respectively.

I test a variety of neighborhood sizes around the blue lines, i.e., the radii for the treatment and control buffers. I run 100 models by varying the treatment buffer radius between 500' and

[28] The treatment group (red circular buffer) also encompasses another blue line. This blue line and a blue line on the edge of the control group (green circular buffer) are not displayed in Figure 2.6 for visual clarity. They are included in the analysis, however.

[29] Since the siting of the blue lines within each community was driven primarily by evacuation concerns, treatment assignment – whether a house is inside the neighborhood around the blue line – is not completely random. The explanatory variables that likely influence evacuation routes and therefore treatment assignment are elevation, distance to the ocean, distance to the nearest highway or interstate, and distance to the nearest major road. After conditioning on these covariates, treatment assignment is plausibly conditionally independent of potential outcomes.

3000' and the control buffer radius between 1000' and 8000'.[30] Each model is defined by the treatment buffer size and control buffer size combination that determines its sample space. Models 1 through 50 use Euclidian distances to define the treatment and control buffers and Models 51 through 100 use road network distances. I hypothesize that this effect will probably be highly localized so smaller buffer sizes are more likely to show a treatment effect. The DID specification for all 100 models is:

$$ln(price_{ict}) = \boldsymbol{X'_{it}}\beta_1 + \beta_2 blueline_i + \beta_3 installation_t + \delta_1 blueline_i * installation_t + quarter_t + city_c * year_t + \varepsilon_{ict} \tag{6}$$

where the treatment variable $blueline_i$ indicates whether the house is in the neighborhood around the blue line. The event variable $installation_t$ indicates that the sale happened after the blue line was installed. Since the blue lines were installed at different times between 2016 and 2019, the timing of the event variable is different between blue lines. The parameter of interest is δ_1, the marginal effect of proximity to the blue lines on property values.

The DDD specification adds the variable $xxl2013_i$, which indicates whether the house is inside the 2013 XXL inundation zone:

$$\begin{aligned} ln(price_{ict}) &= \boldsymbol{X'_{it}}\beta_1 + \beta_2 blueline_i + \beta_3 installation_t + \beta_4 xxl2013_i \\ &+\delta_1 blueline_i * installation_t + \delta_2 blueline_i * xxl2013_i + \delta_3 xxl2013_i * installation_t \\ &+\delta_4 blueline_i * installation_t * xxl2013_i + quarter_t + city_c * year_t + \epsilon_{ict} \end{aligned} \tag{7}$$

The parameter of interest is δ_4, the marginal effect of proximity to the blue lines on property values for properties inside the 2013 XXL tsunami inundation and evacuation zone.

This analysis faces an identification challenge: variation in treatment timing. Specifically, this is a staggered adoption design: units are treated at different times and once units are treated, they remain treated in the following periods. The canonical DID setup has two time periods and two groups: no units are treated in the first period and then some units become treated in the second period (the treated group) while other units remain untreated (the control group). This model is often estimated with the standard two-way fixed effects (TWFE) regression, as in equation (6).

[30] I test 100 models to determine the likely spatial extent of this effect. However, I do not believe that there are 100 possible valid models for this analysis. Thus, while I do apply multiple hypothesis testing corrections, I do not apply them to all 100 models. Section 6.3 elaborates on the 100 models tested and the hypothesis testing corrections performed.

Several recent studies have found that under treatment effect heterogeneity the TWFE estimator recovers a weighted average of underlying treatment effect parameters (Borusyak et al., 2021; de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun & Abraham, 2020).[31] The problem is that some of these weights can be negative, suggesting that the TWFE estimator can be opposite in sign from the true average treatment effects. Furthermore, these weights are sensitive to the size of each group, the timing of treatment, and the total number of time periods (Callaway & Sant'Anna, 2020). Sun and Abraham (2020) show that the standard event study estimator suffers from a similar problem – it is contaminated by treatment effects from other periods. Some of these studies have proposed measures to assess these weights and how robust the TWFE estimator is to heterogeneous treatment effects (de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun & Abraham, 2020).[32] I calculate the measure proposed by de Chaisemartin and D'Haultfœuille (2020) to assess the robustness of the TWFE estimator to heterogeneous treatment effects.

de Chaisemartin and D'Haultfœuille (2020) also propose a new DID estimator that estimates the treatment effect in the groups that switch treatment, at the time when they switch. This estimator is valid in staggered adoption designs and when the treatment effect is heterogeneous over time. Callaway and Sant'Anna (2020) develop another framework for DID setups with multiple time periods and variation in treatment timing that is valid in the presence of treatment effect heterogeneity. Their framework is based on estimating group-time average treatment effects, which are the average treatment effect for units that are members of a particular group g at a particular time t where a "group" is defined by the time when units are first treated. The group-time average treatment effects can be aggregated into group average treatment effects, which are the average effect of participating in the treatment for units in group g. These group average treatment effects can be averaged into an overall aggregate measure: the "average effect

[31] Baker et al. (2021) use simulations to show that DID estimates are unbiased in settings where there is a single treatment period, i.e., the canonical 2x2 DID setup, even when there are dynamic treatment effects. Due to this result, I did not use the new DID estimators that are valid in the presence of treatment effect heterogeneity in the first and second analyses.

[32] The Goodman-Bacon (2021) decomposition theorem states that the DID TWFE estimator is a weighted average of all possible 2x2 DID estimators with weights depending on group sizes and variances. It decomposes the TWFE estimator into weighted averages of the individual 2x2 DID estimators and can thus be used as a diagnostic tool to assess the weights on the 2x2 DID estimators that comprise the TWFE estimator. I do not use the Goodman-Bacon (2021) decomposition because the Zillow property sales used in this paper are repeated cross-sectional data and the Stata package that implements this diagnostic (bacondecomp) requires panel data.

of participating in the treatment experienced by all units that ever participated in the treatment" whose interpretation is like the average treatment effect on the treated (ATET) in the TWFE DID setup (Callaway & Sant'Anna, 2020). I estimate both of these new estimators (Callaway & Sant'Anna, 2020; de Chaisemartin & D'Haultfœuille, 2020).

2.6 Results

2.6.1 First analysis: 2011 Tohoku earthquake and tsunami and 2015 New Yorker article

Table 2.3 reports selected estimation results of the key coefficients for Models I through III in the first analysis.[33] The difference-in-differences (DID) coefficients are statistically significant (at the 5% significance level) for the 2011 Tohoku earthquake and tsunami in both Models I and III. The DID estimator for the 2015 New Yorker article is not statistically significant in either Model II or III. According to the coefficient estimate from Model I, a property inside the SB 379 tsunami inundation zone has a risk discount of 8.5% following the Tohoku event.[34] The coefficient estimate from Model III implies a slightly smaller risk discount of 6.5%. Taken together, these results imply that a property inside the tsunami inundation zone sells for 6.5% to 8.5% less than a property outside of the zone after the Tohoku event.

The Tohoku event is statistically significant in Model I (at the 5% significance level). Properties sold after the Tohoku earthquake/tsunami sold for 9.0% more according to Model I. The New Yorker article event is not statistically significant in either Model II or III. The coefficients on these event variables capture the temporal effect for properties both inside and outside the tsunami inundation zone. This result indicates that the average real value for all properties increased over time by approximately 9.0% between the Tohoku earthquake and the New Yorker article but did not appreciably increase after the New Yorker article. The coefficients on the SB 379 tsunami inundation zone treatment variable in Models I and III implies that houses inside the SB 379 zone have a price premium of 6.4 to 6.9% (at the 10% significance level). This

[33] Table A9 of Appendix 6.7 reports the full estimation results with all coefficients.

[34] All percentage effects of the dummy variable coefficients are calculated according to Halvorsen and Palmquist (1980) as $percenteffect = 100(e^{dummycoeff} - 1)$.

suggests that the SB 379 zone treatment variable may be capturing the value of unobserved coastal amenities. The coefficient on $xxl2013$ is not statistically significant.

As expected, house prices increase with elevation and with proximity to the ocean. These results are statistically significant (at the 1% or 5% level) and signify the importance of coastal view amenities. I interact these two variables for oceanfront homes in $elevation\ x\ \ln(ocean)\ x\ oceanfront$ to create a proxy for ocean view. This proxy appears to have a positive and statistically significant effect (at the 1% level) on property prices in all models. For oceanfront homes, as elevation increases and (log) distance to the ocean shoreline increases (implying increasing beach width), sales prices increase. While this interaction term has the expected sign, it does not fully capture the view amenity for oceanfront homes.[35]

Following the finding of a statistically significant risk discount for the 2011 Tohoku earthquake and tsunami, I test whether this risk discount decays over time. I find that three out of the four transformations of the $f(monthpost_t)$ variable in equation (4) had a positive and statistically significant interaction with treatment, which is suggestive of a decay effect (at the 5% or 10% significance level).[36] Figure 2.7 plots the significant results as in Bin and Landry (2013) using the coefficients on the treatment variable and on the interaction term between treatment and the $f(monthpost_t)$ transformation. This figure suggests that the risk premium decays between 10 months and 30 months after the Tohoku event. Thus, the overall result for this analysis suggests that a property inside the SB 379 tsunami inundation zone sells for 6.5% to 8.5% less than a property outside of the zone after the Tohoku event but property prices inside the inundation zone quickly return to baseline levels within 2.5 years of the Tohoku event.

Table 2.4 reports the results from the Oaxaca-Blinder decompositions. Recall that, like the DID estimator, the unexplained component of the decomposition can be interpreted as the average treatment effect on the treated (ATET) (Fortin et al., 2010; Słoczyński, 2015). Thus, the Oaxaca-Blinder estimator suggests that there is an 8.5% risk discount for properties inside of the SB 379 inundation zone after the Tohoku event (at the 5% significance level). The Oaxaca-Blinder estimator for the article event is not statistically significant for Model II.

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[35] Further attempts to disentangle coastal amenities from tsunami risk involve using GIS viewshed tools and fine-scale digital surface models of the ocean shoreline to calculate the view amenity for oceanfront homes. See section 7 for further details.

[36] These results are not presented here but are available upon request.

Table 2.5 presents results from the event study regression for Models I and II. The lead variables represent quarters prior to the event of interest and the lag variables represent quarters after the event, e.g., the $lag1$ variable represents the first quarter after the event. As is standard, the first lead is omitted as a baseline. The first quarter lag is statistically significant but subsequent lag variables are not. This suggests there is a risk discount of 13.1% one quarter after the Tohoku earthquake and tsunami but that this effect decays rapidly after the first quarter. This event study estimator is slightly larger in magnitude than the full data OLS results and decays more rapidly. However, the key outcome is that the risk discounts are in the same direction and relative magnitude. This short-lived response supports the idea that the Tohoku event acted as a pure/distant information shock that does not persist. For Model II, the statistically significant results for the post-event lag variables are conflicting. The variable for the quarter during which the event of interest occurs ($lag0$) is positive and two quarters later the second lag variable is negative. Thus, the event study results are inconclusive about the direction of the risk discount, which is complementary to the full data OLS results that suggest a null result for Model II.

Appendix 6.6 presents the covariate balance results for the PSM, NNM, CEM and EB matching/weighting methods. The two matching methods (PSM and NNM) that improved covariate balance for the key variables that likely influence treatment also dropped approximately 90% of the control observations and the matching method (CEM) that does not drop most of the control observations also does not appreciably improve covariate balance. EB, a pure weighting method, improved covariate balance for the key matching variables but effectively "dropped" many control observations by assigning very small weights to them. Due to these concerns the matched samples are not used to replace the original unmatched data. Instead, I run the three primary models using the matched data from all four matching methods and report these results in comparison to the full, unmatched data results.

Table 2.6 reports selected estimation results of the key coefficients for Models I through III using the matched data. After PSM, the DID estimators are still statistically significant (at the 5% significance level) for the 2011 Tohoku earthquake and tsunami in both Models I and III. The coefficient estimates suggest that a property inside the SB 379 tsunami inundation zone has a risk discount of 10.0-11.5% following the Tohoku event. After NNM, the DID estimator for the

Tohoku event is suggestive of an 11.6% risk discount (at the 5% significance level) for Model I but is no longer statistically significant for Model III. After CEM, the DID estimator for the Tohoku event is suggestive of an 8.8% risk discount (at the 10% significance level) for Model III but is no longer statistically significant for Model I. After EB, the DID estimators are no longer statistically significant for either Model I or III. The DID estimator for the 2015 New Yorker article is not statistically significant in either Model II or III for any of the four methods. One issue with matching is that there are few good controls with respect to the two key matching variables – elevation and distance to the ocean – since assignment to the tsunami inundation zone is highly dependent on both variables. Thus, all four matching/weighting methods assign high weights to few observations and low weights to many observations, effectively "dropping" many control observations. This increases standard errors and confidence intervals for the resulting post-matching DID coefficients. However, the post-matching estimators all have similar magnitudes to the full data OLS results and the Oaxaca-Blinder results. Since the post-matching results are consistent with the full data results, albeit with larger standard errors, matching may not be important in this context.

The results of the four sets of falsification tests are presented in Table A10 of Appendix 6.7. In all four tests the DID estimates for Model I are smaller in magnitude compared to the main full data estimate of 8.5% and are not statistically significant. The DID estimates for Model III and the 2011 Tohoku event are also smaller in magnitude than the main estimate of 6.5% and are not statistically significant in all tests (the fourth test does not apply to Model III). The 2015 New Yorker article event is still not statistically significant in either Model II or III in all four tests. These falsification tests lend additional support to a causal interpretation of the estimated risk discounts.

Figure 2.8 summarizes the results for the first analysis. It plots the average treatment effect on the treated (ATET) estimates with 95% confidence intervals for Models I and II.[37] For each model, the full data estimator is on the left. The next four points represent the estimators after the

[37] See Figures A5(a) and A5(b) in Appendix 6.7 for plots of the ATETs for Model III's Tohoku event and New Yorker article event, respectively. These results generally corroborate the results in Figures 2.8(a) and 2.8(b). For the Tohoku event, all of the ATETs are negative and most (except the post-NNM estimator) are similar in magnitude to the full data estimate. For the New Yorker article event, most of the ATETs including the full data estimate are not statistically significant.

data was processed with the four matching methods (PSM, NNM, CEM, and EB). "OB" represents the Oaxaca-Blinder estimator. The final six estimators represent the full data estimator under different sample space assumptions. The sample space is changed from within 1 mile of the tsunami inundation line to ½ mile and also to 2 miles to compare the effects of decreasing and increasing the sample area, respectively. Similarly, I decrease the time range from 2 years around the event of interest to 1 year around the event. Finally, I try extending the sample space to the entire seven counties. Figure 2.8(a) plots the ATETs for Model I. The takeaway from this plot is that the full data result is robust to the matching estimators, the Oaxaca-Blinder estimator, and to varying the sample space: all of the ATETs for the 2011 Tohoku earthquake and tsunami have the expected negative sign and approximately same magnitude as the coefficient from the full data results. Figure 2.8(b) plots the ATETs for Model II and shows that the full data's *null* result is robust to the matching estimators, the Oaxaca-Blinder estimator, and to varying the sample space: the ATETs for the 2015 New Yorker article are not statistically significant for any of the presented models.

Lastly, I use two back-of-the-envelope calculations to quantify the monetary impact of the Tohoku earthquake and tsunami risk discount and to compare it to capitalized flood insurance premiums. I first calculate the possible range for the average capitalization effect using the estimated 10-30 month duration of the 6.5-8.5% risk discount (see Figure 2.7). The lower (upper) bound of the average capitalization effect per home is the Model III (Model I) risk discount multiplied by the average sale price in the SB 379 tsunami inundation zone for the homes that sold in the 10 months (30 months) after the Tohoku earthquake and tsunami. The risk discount translates to an average capitalization per home in the range of $19,964-$27,642.[38] This is multiplied by the number of homes that sold inside the SB 379 tsunami inundation zone after the Tohoku event before the risk discount decayed (for each risk discount duration) to get an average capitalization effect of $6.1-$28.7 million dollars.[39] I also compare the Tohoku earthquake and tsunami risk

[38] For the lower bound average capitalization effect calculation, the average sale price of homes that sold in the 10 months after the Tohoku earthquake and tsunami was $305,729 and the risk discount was 6.53%. For the upper bound calculation, the average sale price of homes that sold in the 30 months after the Tohoku earthquake and tsunami was $324,997 and the risk discount was 8.51%.

[39] The number of sales that occurred in the SB 379 tsunami inundation zone in the 10 months (30 months) after the Tohoku earthquake and tsunami was 306 sales (1,039 sales).

discount to the present value (PV) of flood insurance premiums in the three northern counties since tsunami damage is typically covered by flood insurance (OSSPAC, 2018). According to the National Flood Insurance Program (2022), the average flood insurance premium (in 2022) for the three northern counties was $806.[40] The range of PVs for this flood insurance premium was calculated using three different payment duration values: 11 years – the median tenure of owner-occupied homes in the U.S. in 2011 (U.S. Census Bureau, 2020), 30 years – the most common mortgage term in the U.S., and in perpetuity.[41] The PV of insurance premiums for these payment durations are, respectively, $6,695, $12,390, and $16,120.[42] For the average home in the SB 379 tsunami inundation zone, these capitalized insurance values translate to 2.1%, 3.8%, and 5.0% of the sale price, respectively.[43]

2.6.2 Second analysis: 2013 change in tsunami evacuation maps

For the second analysis, the DID coefficients for the XXL, XL, L or M tsunami inundation zones are not statistically significant (Models 1-4). The DID coefficient for the smallest inundation zone is negative, large, and statistically significant at the 5% level, implying that a property inside the 2013 SM tsunami inundation zone has a risk discount of 26.9% following the 2013 map change. These results are summarized in Figure 2.9, which plots the full data DID estimators with 95% confidence intervals for Models 1 through 5.[44] I also test whether the risk discount for the SM tsunami inundation zone decays over time and find that none of the four transformations of the $f(monthpost_t)$ variable in equation (4) had a statistically significant interaction with treatment. This suggests that the risk discount does not have a statistically significant decay effect.

[40] The average insurance premium for the three counties of Clatsop, Tillamook, and Lincoln was calculated using the number of NFIP policies in force as of January 31, 2022 (18,592 policies) and the sum of the total premiums (and federal policy fees) for these policies ($16,881,940) (National Flood Insurance Program, 2022). This is the average insurance premium across all flood zones and occupancy types in the three counties. This average insurance premium reflects then number of policies in force in 2022 and may thus be different from the average insurance premium from 2011-2013 (the duration of the Tohoku earthquake and tsunami risk discount).

[41] The median tenure of owner-occupied homes is assumed to be 11 years according to the 2011 American Community Survey's 1-Year Estimates Detailed Tables (U.S. Census Bureau, 2020). Thus, this PV calculation assumes that households remain in their homes for 11 years, which likely gives a lower bound on the length of time flood insurance premiums are paid. The assumption that households pay flood insurance premiums in perpetuity provides an upper bound on this length of time.

[42] The PV of flood insurance premiums is calculated as $PV = \sum_{t=1}^{n} \$806/(1+r)^t$ where the discount rate (r) is assumed to be 5% and the payment duration (n) takes three different possible values: 11, 30, and ∞).

[43] The PV of flood insurance premiums are divided by the average sale price of homes in the SB 379 tsunami inundation zone ($323,072) (see Table 2.1).

[44] Table A11 of Appendix 6.7 reports the full estimation results with all coefficients.

The combined model with all five 2013 tsunami inundation zones supports the main DID results: the only statistically significant DID coefficient is that of the smallest inundation zone.[45] This model implies that a property inside the 2013 SM inundation zone has a risk discount of 21.3% following the 2013 map change (at the 10% significance level). A robustness check with the Oaxaca-Blinder decomposition is not statistically significant for the XXL, XL, L or M tsunami inundation zones (Models 1-4).[46] However, the Oaxaca-Blinder estimator is marginally significant for Model 5 and suggestive of a 15.8% risk discount for properties inside the 2013 SM tsunami inundation zone following the 2013 map change. I ran event study regressions for Model 5, the only model that had significant full data results, but there were too few treated observations in some quarters to precisely estimate treatment effects in an event study framework.[47] The results of the four sets of falsification tests are presented in Table A14 of Appendix 6.7. In all four tests the DID estimates for Model 5, the primary model of interest, are smaller in magnitude compared to the main estimate of 26.9% and are not statistically significant.[48] This result supports the causal interpretation of the risk discount found in Model 5. Combined, the OLS and Oaxaca-Blinder results suggest that properties inside the SM inundation zone sold for 16-27% less after the 2013 map change.[49]

2.6.3 Third analysis: Tsunami Blue Line project

The first step in this analysis required testing neighborhood sizes around the blue lines by running 100 models that vary the treatment buffer and control buffer radii. Figure 2.10 summarizes the results of these tests. It plots the average treatment effect on the treated (ATET) estimates for the DID models with 95% confidence intervals for Models 1 through 100 where each model is defined by the treatment buffer size and control buffer size combination that determines its sample space.

[45] Table A12 of Appendix 6.7 reports the combined model results.
[46] Table A13 of Appendix 6.7 reports the Oaxaca-Blinder results.
[47] These results are not presented here but are available upon request.
[48] There are two unexpected and statistically significant results of the falsification tests. First, the DID estimates for Models 1 and 2 are marginally statistically significant in the first test (shifting the date of the 2013 map change to one year before the true event, i.e., October 2012). Since some counties received updated tsunami maps in early 2013 these two models may be picking up the treatment effect due to these early-adopting counties. Second, the DID estimates for Model 3 are statistically significant but positive in the third test (randomly assigning sales to either the control or treatment group). This result is counterintuitive and likely an artifact of the randomization.
[49] I do not quantify the monetary impact of the estimated risk discount or compare it to capitalized flood insurance premiums in this analysis.

The 95% confidence intervals – and the p-values used for hypothesis testing – were generated using subcluster wild bootstrapping, an extension of the wild cluster bootstrap. Each municipality that installed blue lines was given a set of blue lines from the state and chose themselves where to install these blue lines, meaning that the treatment assignment mechanism is clustered by municipality. This suggests using cluster-robust standard errors. However, there are only 8 to 15 municipalities (this varies by model), which is less than the recommended 40 to 50 clusters (Angrist & Pischke, 2009). With too few clusters, the cluster-robust variance matrix estimate will be downward-biased, leading to over-rejection of the null hypothesis (A. C. Cameron & Miller, 2015). Bootstrapping diagnostics suggested that subcluster wild bootstrapping – clustering on both municipality and year – performed better than ordinary wild cluster bootstrapping on municipality alone. Furthermore, whereas the ordinary wild cluster bootstrap fails when cluster sizes vary, as is the case here, the subcluster wild bootstrapping method has been shown to perform well when the number of clusters is small and when cluster sizes vary (MacKinnon & Webb, 2018).

Models 1 through 50 (Figures A6(a) and A6(b) in Appendix 6.7) use Euclidian distances and Models 51 through 100 (Figures 2.10(a) and 2.10(b)) use road network distances to define the treatment and control buffers. The models that use road network distances tend to have treatment effects that agree more with each other within a given treatment buffer compared to the models that use Euclidian distances, which possibly suggests that the road network distance models are more consistently picking up the effect of proximity to a blue line. This makes intuitive sense since the blue lines are placed on roads that homeowners drive on regularly to and from their properties. So, using the road network to measure distances between properties and blue lines likely aligns better with how homeowners are perceiving these distances. Therefore, I focus on the results road network models (Figures 2.10(a) and 2.10(b)).

Figure 2.10(a) shows the estimates for the 500’, 1000’, and 1500’ treatment buffers defined using road network distances. The first nine model estimates in this figure are for the 500’ treatment buffer with the control buffer expanding from 1000’ to 5000’. The next nine estimates are for the 1000’ treatment buffer with the control buffer expanding from 1500’ to 5500’. The last nine estimates are for the 1500’ treatment buffer with the control buffer expanding from 2000’ to 6000’. Figure 2.10(a) suggests that the 500’ treatment buffer is too small – there are not enough observations to identify the treatment effect. The 1000’ treatment buffer models all have negative effects, with several treatment effects having statistical significance. The 1500’ treatment buffer does not have any significant treatment effects. In fact, the treatment effect appears to go to zero. Figure 2.10(b) shows the estimates for the 2000’, 2500’, and 3000’ treatment buffers. Combined, these two figures suggest that when the treatment buffer is 1500’ or larger the treatment effect goes to zero. The most significant results tend to be for smaller treatment buffers, specifically the 1000’ treatment buffer, and these results are more significant for smaller control buffers, which is when the sets of treatment and control buffer observations are the most comparable or balanced. As hypothesized, the treatment effect of the blue lines is extremely localized. Thus, I narrow the spatial extent choice to the 1000’ treatment buffer.

Within this treatment buffer, I am simultaneously testing nine control buffers (Models 60 through 68) so I have to account for this multiple hypothesis testing.[50] I use the Simes correction to generate q-values (adjusted p-values) for these nine models because it has several desirable features: it is not as conservative as the traditional Bonferroni correction, it is a step-up method, and it allows for non-negative correlation between the p-values (Newson, 2010). Step-up methods start with a single-step method (like the Bonferroni correction) but then improve upon single-step methods by possibly rejecting further hypotheses in subsequent steps (Romano et al., 2010). The q-value generated by the Simes procedure for Models 62 and 63 is 0.089.[51] This is the minimum proportion of false positive results (the false discovery rate) when the test is significant, i.e., 8.9% of significant results will result in a false positive.

Following these tests, I choose one model to continue the analysis with: Model 62.[52] It has a 1000’ treatment buffer and a 2500’ control buffer. Table 2.7 reports selected DID and DDD estimation results of the key coefficients for Model 62.[53] The DID estimator suggests that there is an 8.0% risk discount for properties that are within 1000’ of a blue line (at the 5% significance

[50] I could apply multiple hypothesis testing procedures to a larger subset of models but, as expected, the adjusted p-values are very high.
[51] The full set of q-values is not reported here but is available upon request.
[52] Once this model is selected, subsequent p-values are generated using the subcluster wild bootstrapping procedure and are not corrected for multiple testing procedures.
[53] Table A15 of Appendix 6.7 reports the full estimation results with all coefficients.

level, uncorrected).[54] The DDD estimator is not statistically significant, however. These results suggest homebuyers attend to the visual cues but do not differentiate the signal according to the classification of tsunami inundation risk. The treatment and event variables are not statistically significant in either the DID or DDD model. The sensitivity variable for the 2013 XXL tsunami inundation zone is statistically significant (at the 10% level) in the DDD model, suggesting that houses inside the 2013 XXL inundation zone sell for 14.6% more than houses outside of it. This variable may be capturing the value of unobserved coastal amenities. In both the DID and DDD models house prices increase with proximity to the ocean (at the 1% significance level). However, elevation and the ocean view proxy $elevation\ x\ \ln(ocean)\ x\ oceanfront$ are no longer statistically significant in either the DID or DDD model.[55]

Next, I calculate the measure proposed by de Chaisemartin and D'Haultfœuille (2020) to assess the robustness of the TWFE estimator to heterogeneous treatment effects. This robustness measure is the ratio of the TWFE estimator to the standard deviation of the weights attached to the TWFE regression (de Chaisemartin & D'Haultfœuille, 2020). If this ratio is very large, the TWFE estimator and the ATET can only be of opposite signs under a very large and implausible amount of treatment effect heterogeneity. However, if many weights are negative, and if the robustness measure is not very large (close to 0), the TWFE estimator and the ATET can be of opposite signs even under a small and plausible amount of treatment effect heterogeneity. The calculated robustness measure (0.0103) for Model 62 suggests that treatment effect heterogeneity could be a serious concern for the validity of the TWFE estimator.

Following this result, I estimate two new estimators that are valid in the presence of treatment effect heterogeneity. I first compute the new DID estimator by de Chaisemartin and D'Haultfœuille (2020) that estimates the treatment effect in the groups that switch treatment, at the time when they switch. I find a large, negative but not statistically significant effect ($DID_M = -0.392, SE = 0.664$). I then run a new estimator developed by Callaway and Sant'Anna (2020)

[54] A Oaxaca-Blinder decomposition yields similar results to the DID estimation. The Oaxaca-Blinder estimator suggests that there is an 8.3% risk discount for properties that are within 1000' of a blue line (at the 10% significance level, uncorrected). These results are not presented here because the p-values were not generated with subcluster wild bootstrapping or corrected with multiple hypothesis testing procedures. Furthermore, the validity of the Oaxaca-Blinder decomposition is uncertain in staggered adoption designs. The Oaxaca-Blinder decomposition results are available upon request.

[55] I do not quantify the monetary impact of the estimated risk discount or compare it to capitalized flood insurance premiums in this analysis.

whose interpretation is similar to the ATET in the TWFE DID setup. However, the data for Model 62 is too sparse to be able to estimate most of their group-time average treatment effects. Out of seven groups, I can calculate group average treatment effects (GATET) for only two groups and, while negative, these group average treatment effects are not statistically significant ($GATET_1 = -0.317, SE_1 = -0.195$ and $GATET_2 = -0.020, SE_2 = 0.161$). There are also too many missing group average treatment effects to calculate an overall treatment effect that could be compared to the TWFE DID estimator. The treatment effects generated by these new methods have the same sign as the TWFE estimate but the magnitudes and significance are likely impacted by the small sample size in this rural location.

2.7 Discussion and Conclusion

The Pacific Northwest is facing a severe but low frequency threat: the Cascadia Subduction Zone (CSZ) earthquake and tsunami. In Oregon, resilience to such a large seismic event is low and coastal communities in the tsunami inundation zone are especially vulnerable. They will account for the majority of expected fatalities and those who survive will be instantly displaced (OSSPAC, 2013; Schulz, 2015b). Whether individual Oregonians will take action to prepare themselves for a CSZ event depends on how salient the risk is. Since Oregon has not experienced a Cascadia earthquake and tsunami in recent history, Oregonians' subjective risk perceptions may underestimate the objective probability of a Cascadia event. This study asks whether new information about the risk of a Cascadia earthquake and tsunami can narrow the gap between subjective and objective risk.

The results for the first analysis on exogenous events suggest that a property inside the SB 379 tsunami inundation zone sells for 6.5% to 8.5% less than a property outside of the zone after the 2011 Tohoku earthquake and tsunami. However, this risk discount is short-lived and properties inside the SB 379 inundation zone return to baseline levels within 2.5 years of the Tohoku event. The DID estimator for the 2015 New Yorker article is not statistically significant in either Model II or III. The 2011 Tohoku earthquake and tsunami treatment effect is robust to the Oaxaca-Blinder estimator, matching estimators, and an event study specification. This decay of the Tohoku event risk discount has several potential explanations. For example, the informational effect of the Tohoku event will diminish when new people move into the area and the attention-focusing effect of the event will diminish as media coverage decreases. A related explanation is availability bias. Under this explanation, an individual's subjective risk perception depends on the availability of information about and/or recall of events related to a predicted Cascadia event. The low frequency of such events suggests that, before an event like the 2011 Tohoku earthquake and tsunami, individual Oregonians would have low subjective risk perceptions about the risk of a Cascadia event occurring in their lifetimes. Thus, the 2011 Tohoku earthquake and tsunami would have acted as a source of new information, increasing subjective risk perceptions. However, this effect diminishes over time as recall of the Tohoku event declines. The risk discount due to the Tohoku event is also shorter-lived than risk discounts found in other studies that used local disaster events – such as floods or hurricanes – as information shocks (Atreya et al., 2013; Bin & Landry, 2013; Hansen et al., 2006; Kousky, 2010; McCluskey & Rausser, 2001; McCoy & Walsh, 2018). These results suggests that a "distant" information shock can shift homebuyers' subjective risk perceptions to better match the objective risks of the Cascadia event. However, these distant information shocks may not be as persistent as local information shocks.

Two back-of-the-envelope calculations place these results into perspective for policy. I find that Tohoku earthquake and tsunami risk discount had an average capitalization effect of \$6.1 to \$28.7 million dollars in the northern Oregon housing market before it decayed. The average capitalization per home of this transient risk signal (\$19,964 to \$27,642) is larger than the likely capitalized value of flood insurance premiums (\$6,695 to \$16,120). Put into percentage terms, the present value of flood insurance premiums is likely between 2.1% and 5.0% for the average home in the SB 379 tsunami inundation zone. This capitalized insurance value is up to four times smaller than the Tohoku earthquake and tsunami risk discount of 6.5% to 8.5%. Previous work has found closer agreement between capitalized insurance premiums and sales price differentials, e.g., Bin et al. (2008) found that capitalized values of flood insurance are consistent with sale price differentials associated with being in a floodplain for coastal housing markets in North Carolina. However, it is plausible that a tsunami risk discount would be larger than the flood insurance premium in the coastal Oregon housing market since a tsunami is a catastrophic but low frequency event that would effectively generate much more severe damage than even the worst case flooding scenario.

For the second analysis on regulatory map changes, the DID estimators are statistically significant for the 2013 SM tsunami inundation zone but not for the M, L, XL, or XXL zones. The coefficient estimate from Model 5 implies that a property inside the SM inundation zone has a risk discount of 26.9% following the 2013 map change. This risk discount does not have a statistically significant decay effect. The SM inundation zone result is robust to the Oaxaca-Blinder estimator, which suggests a more conservative risk discount of 15.8%. These results suggest that only properties in the most *vulnerable* inundation zone see a risk discount following the 2013 map update. These are homes that were not in the original 1995 tsunami inundation zone but are in the smallest 2013 inundation zone and therefore *all* of the new inundation zones, making them the most vulnerable to a Cascadia event tsunami. This result also suggests that a "pure" information shock can shift homebuyers' subjective risk perceptions to better match the objective risks of the Cascadia event.

For the third analysis on local visual risk cues, results from the TWFE DID regression using a 1000' treatment buffer and a 2500' control buffer are suggestive of an 8.0% risk discount for properties that are within 1000' of a blue line. According to my estimate of the robustness measure from de Chaisemartin and D'Haultfœuille (2020), a potential concern for this analysis is that the DID result could be invalidated by the presence of treatment effect heterogeneity. However, the sample composed of small, rural communities limits my ability to verify the results from the TWFE regression with the newly developed estimators that account for treatment effect heterogeneity. The recent estimators developed by de Chaisemartin and D'Haultfœuille (2020) and Callaway and Sant'Anna (2020) partition the data into groups and estimate treatment effects for each group. Thus, precise estimation of both new estimators requires a sufficient number of observations within each group and a large number of observations overall. When I run these new estimators, they are suggestive of a negative effect of proximity to a blue line but, again, are not able to be estimated precisely. The DDD results from the third analysis are not statistically significant, suggesting that people are not sensitive to whether they are inside the tsunami inundation zone. Homeowners may not perceive a difference in risk if they're immediately across the inundation zone, e.g., they may think the water will reach their property even if they are outside of the inundation zone since the zone is a modeled result and cannot be perfectly predictive. This result suggests that people may attend to the visual cue given by the blue lines but not to the actual hazard delineation given by the tsunami inundation zone.

Many of the limitations of these three analyses are due to limited observations or covariates. In the first analysis, the positive coefficients on the SB 379 tsunami inundation zone treatment variable suggest that it is capturing the value of unobserved coastal amenities. One promising attempt to disentangle coastal amenities from tsunami risk involves using GIS viewshed tools and fine-scale digital surface models of the ocean shoreline to calculate the view amenity for oceanfront homes (Bin et al., 2008; Dundas, 2017). There may also be unobservable factors that influence the price trend for oceanfront properties. More data may be needed to fully account for the unobserved coastal amenities driving location choice and potentially confounding results. Similarly, for the second analysis, an ocean view covariate for oceanfront homes may help this analysis better disentangle coastal amenities from tsunami risk. There are two potential concerns with second analysis' primary SM inundation zone result. First, for there are only 81 property transactions that fall into the treatment group, i.e., were not in the SB 379 zone but are in the 2013 SM zone, for Model 5. My inability to pick up a statistically significant decay effect for the SM zone risk discount may also be due to the small number of treated transactions. Another concern is the substantial covariate imbalance for this sample (see Table A3 of Appendix 6.3). However, the small sample size for this model precluded using any of the four matching methods to preprocess the data as a robustness check. Given some challenges with the third analysis, I see potential for extension of this work in the future. Increasing the sample size by acquiring more recent housing transactions may enable implementation of the de Chaisemartin and D'Haultfœuille (2020) and Callaway and Sant'Anna (2020) estimators. Then, since pre-tests based on the group-time average treatment effects of Callaway and Sant'Anna (2020) are valid even if there is variation in treatment timing, implementing this estimator would enable parallel pre-trends tests for the third analysis.

The potential risk discounts identified in this paper indicate that at least three types of tsunami risk signals – exogenous events, hazard planning changes, and visual cues – may be salient to coastal residents. These results suggest that "pure" or "distant" information shocks about tsunami risk may shift homebuyers' subjective risk perceptions to better match the objective risks of the Cascadia event, meaning that a salient risk signal may be able to successfully induce individuals to take preparedness actions. And given that Oregon is currently and chronically under-prepared for a Cascadia earthquake and tsunami, policymakers and emergency managers face the dual policy challenge of increasing risk salience and preparedness action. This paper's findings suggest that Oregon policymakers may be able to use risk signals to induce individuals to pay attention to and prepare more for a Cascadia event. These "pure" risk signals – or policies – would act as a source of new information, increasing Oregonians' subjective risk perceptions. However, the effect of these information shocks on risk perceptions would likely disappear over time, as found in the first analysis, and may disappear more rapidly than the effects of local disaster events. Thus, regular (e.g., annual) risk signals may be necessary to prompt individuals to continue adjusting their subjective risk perceptions. For example, existing annual events like the Great Oregon ShakeOut earthquake drill that occurs each October could be publicized more widely and intensively before they happen (Office of Emergency Management, 2019b). Existing home preparedness programs such as 2 Weeks Ready could be regularly promoted with bursts of media coverage on local and social media (Office of Emergency Management, 2019a). Programs like the Tsunami Blue Line Project that implement visual cues of risk may also be effective at adjusting risk perceptions. These visual cues act as a regular risk reminder every time people pass by them. However, the drawback of these types of policies is that they have highly localized effects and that, while individuals may attend to the visual cue, they may not attend to the actual hazard, as found in the third analysis.

However, even if these risk signals are able to decrease the gap between subjective risk perceptions and the objective risk of a Cascadia event, they may not necessarily lead to increased individual preparedness actions. Wachinger et al. (2013) offer possible explanation for a weak relationship between risk perception and preparedness action even when individuals understand the risk, i.e., when the risk is salient. First, residents of an area facing natural hazard risk may choose to accept the risk if their perceived benefits outweigh the potential impacts, e.g., in this study, distance to the coast serves as both a proxy for coastal amenities and increased risk to homeowners. The second reason is due to the effect of trust in government and/or structural measures. Individuals are less likely to prepare themselves when they trust these measures to protect them than when they have little trust in the government authority or the effectiveness of existing measures. Essentially, they transfer responsibility for action to someone else, e.g., state or local government. Third, there may be confusion or ignorance about the appropriate preparedness action to take or individuals may have little capacity or few resources to help themselves. These are all factors that Oregon policymakers and emergency managers may want to consider when developing policies and other risk signals to deal with the dual policy challenge of increasing risk salience and preparedness action for a Cascadia earthquake and tsunami.

2.8 List of Figures

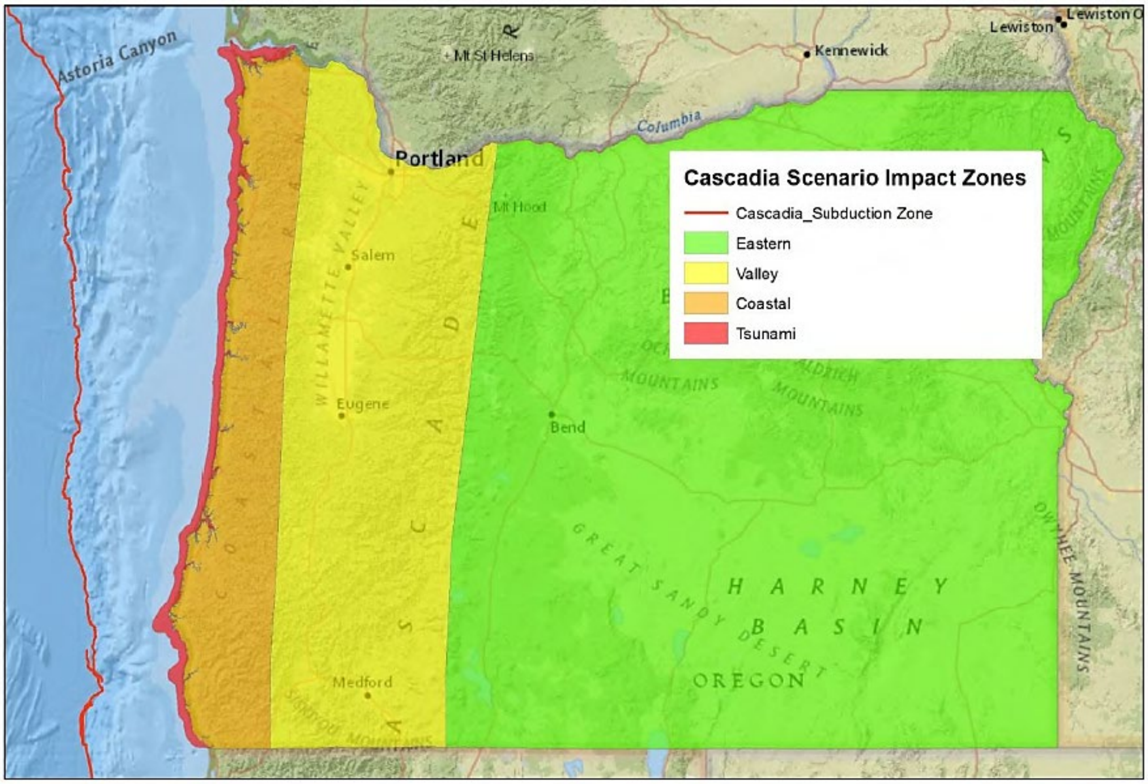
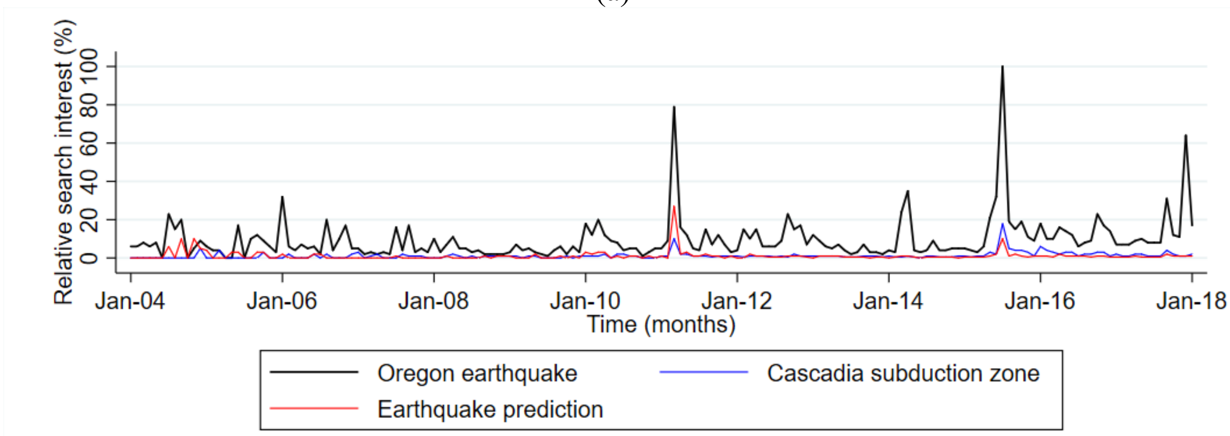


Figure 2.1. Impact zones for the magnitude 9.0 Cascadia earthquake scenario

Note: Damage will be extreme in the Tsunami zone, heavy in the Coastal zone, moderate in the Valley zone, and light in the Eastern zone. From Figure 1.5 of the "Oregon Resilience Plan" (OSSPAC, 2013).

(a)



(b)

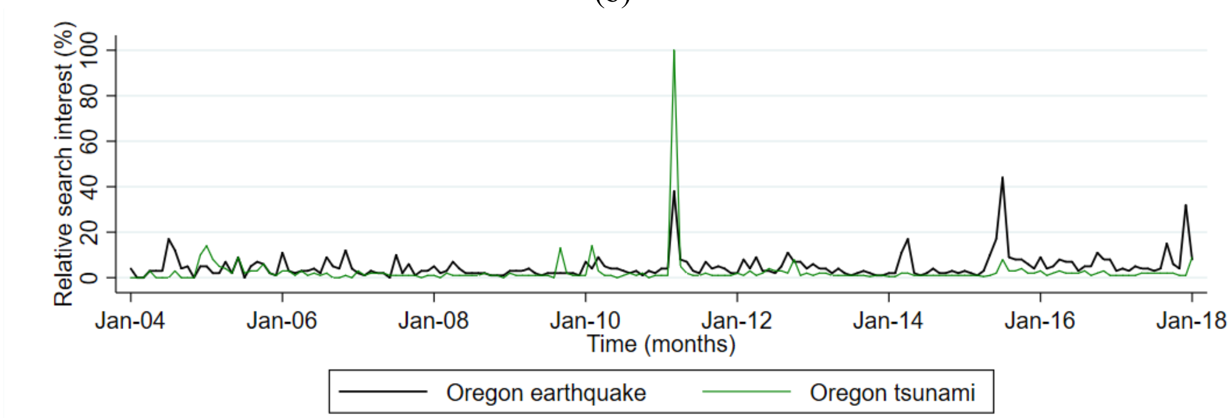


Figure 2.2. Google searches between 1/1/04 and 1/1/18 in Oregon as measured by search interest

Note: Search interest is relative to the highest point on the chart for the given region and time range. Panel (a) shows search interest for terms "Oregon earthquake", "Cascadia subduction zone", and "Earthquake prediction". Panel (b) shows search interest for terms "Oregon earthquake" and "Oregon tsunami". The term "Oregon tsunami" is omitted from (a) due to an order of magnitude spike in search intensity for "Oregon tsunami" during the Tohoku event relative to the other three terms over the time range.

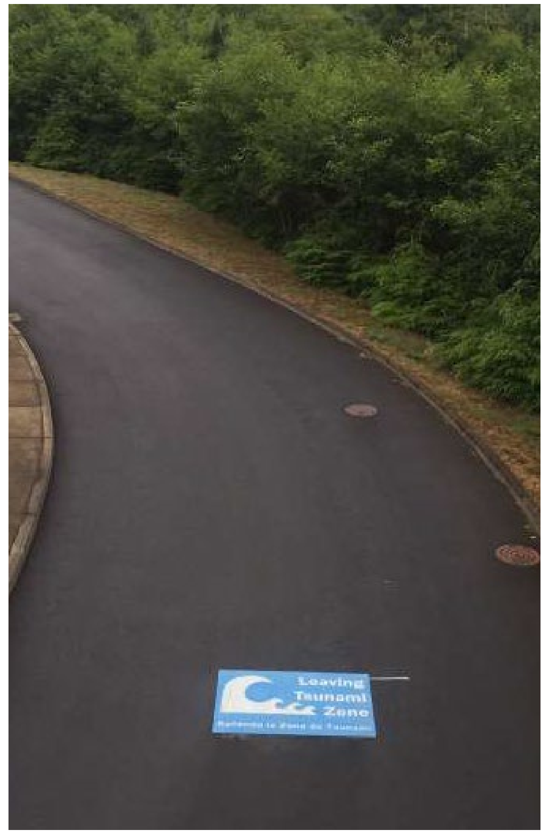
(a)  (b) 

Figure 2.3. Examples of tsunami blue line signage

Note: Tsunami blue line signage in (a) Newport, OR (Courtesy, City of Newport and Mike Eastman) and (b) Seaside, OR (Courtesy, City of Seaside).

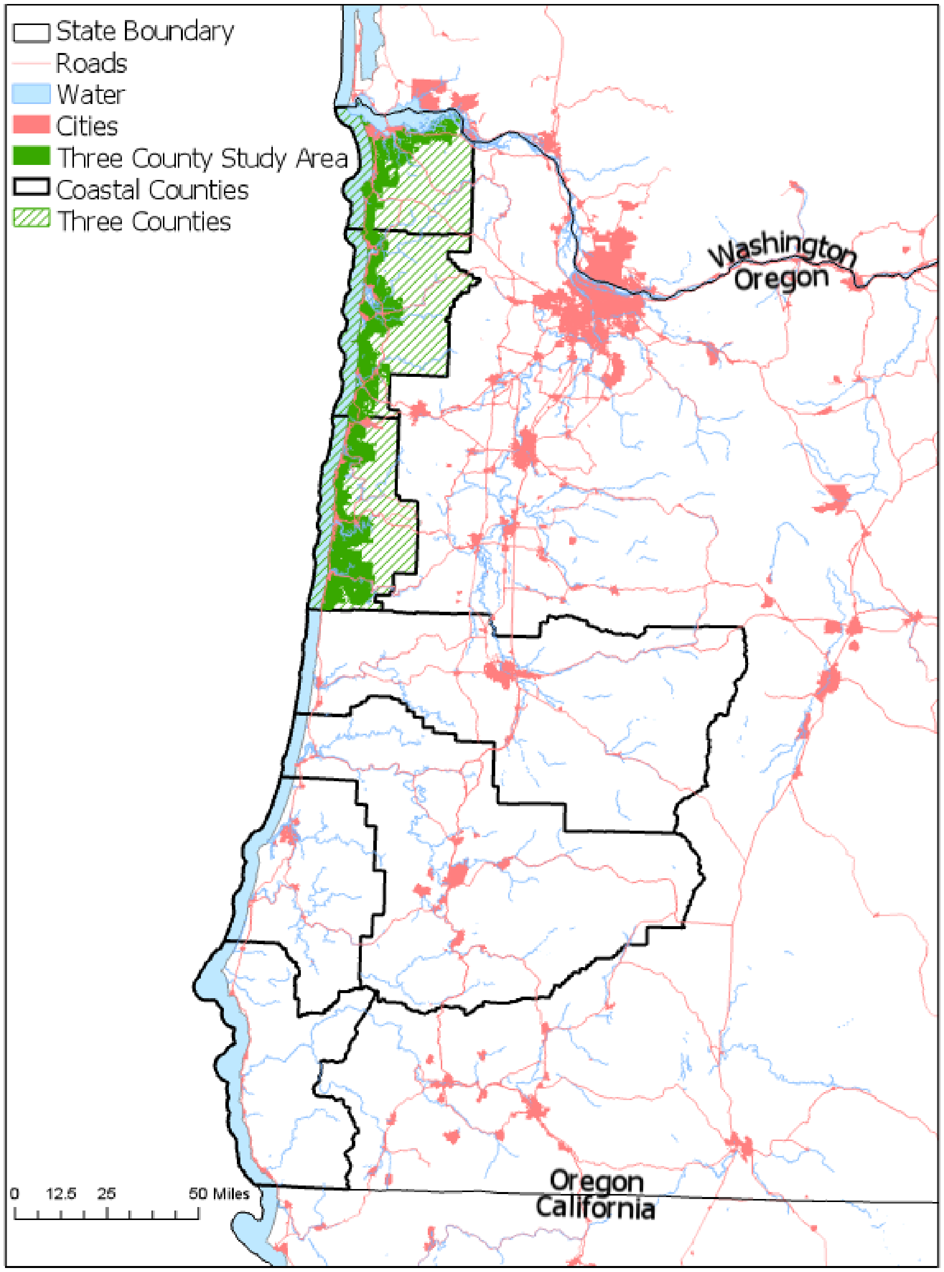
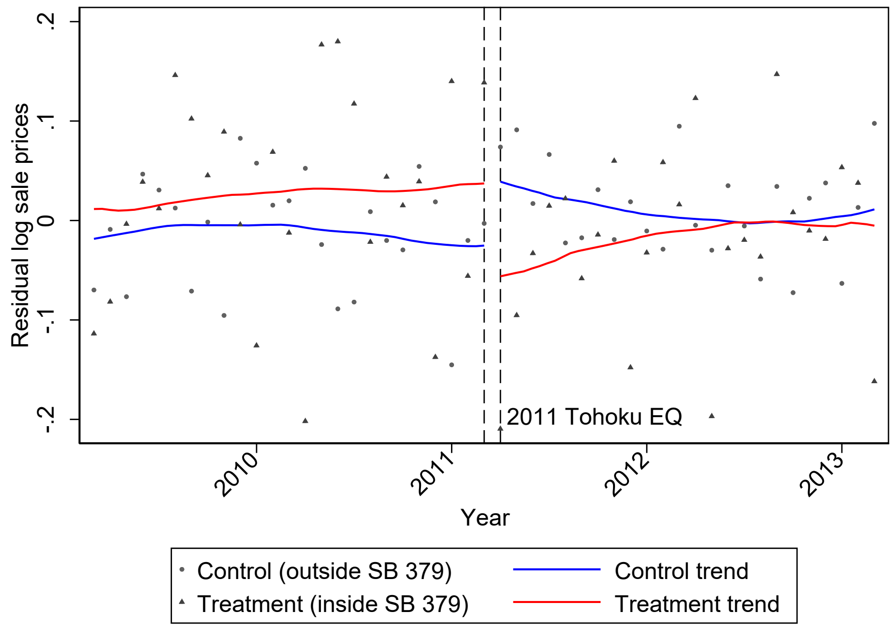
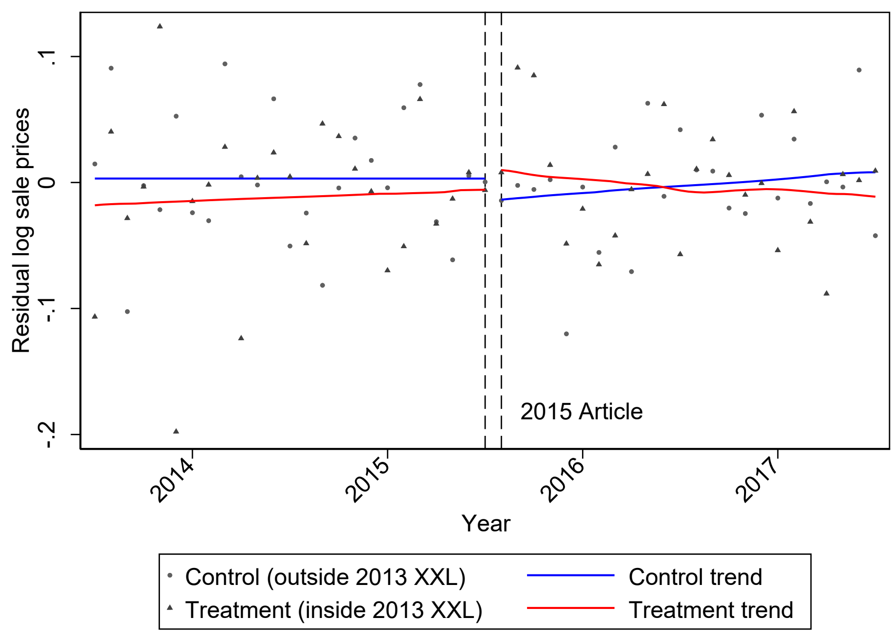


Figure 2.4. GIS data for three county study area and the seven coastal counties

Note: GIS data for the three county study area is shown in green hatching and for the seven coastal counties is given by the black border. Coastal counties from north to south (unlabeled): Clatsop, Tillamook, Lincoln, Lane, Douglas, Coos, and Curry. The study area for the first analysis (solid green) is defined to be within 1 mile of the tsunami inundation zone given by the 1995 SB 379 line.



(a)



(b)

Figure 2.5. Housing price trends inside and outside of the treatment inundation line for the first analysis

Note: Plot of residual (log) sale prices net of structural attributes, location covariates, and fixed effects aggregated by month with local polynomial trend lines for the three counties. (a) For Model I's time range and the SB 379 treatment inundation line. (b) For Model II's time range for the 2013 XXL treatment inundation line.

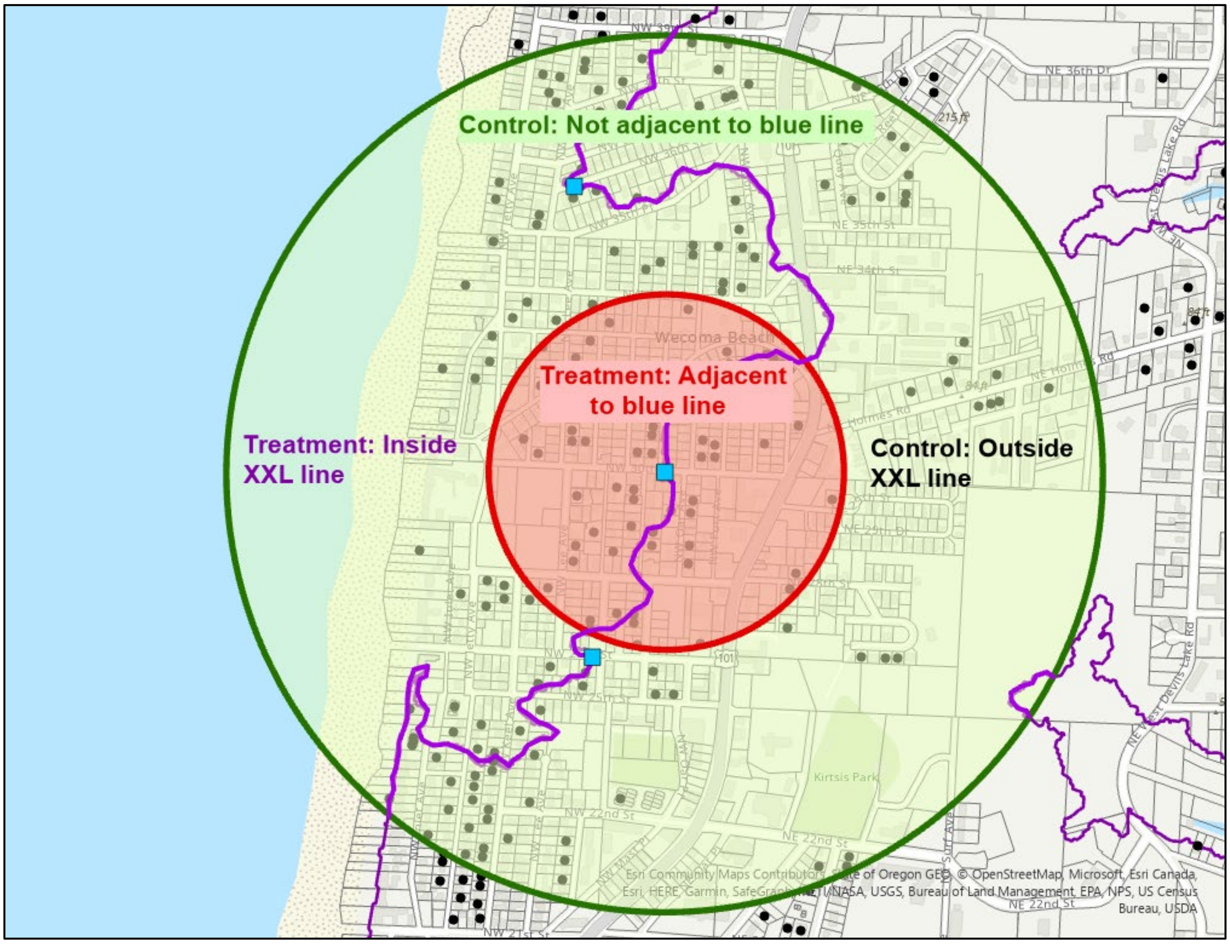


Figure 2.6. Example treatment and control groups around a blue line

Note: Taxlot map with a treatment and control group around a blue line (small blue squares) in Lincoln City, OR. The treatment group (red circular buffer labeled "Treatment: Adjacent to blue line") and control group (green circular buffer outside of the red circular buffer labeled "Control: Not adjacent to blue line") represent whether property sales (small black circles) are adjacent to the blue line or not, respectively. The 2013 XXL inundation and evacuation line (thick purple line) separates houses that are more sensitive to the treatment (area labeled "Treatment: Inside XXL line," mostly to the left of the purple line bisecting the green circular buffer) from those that are less sensitive to the treatment (area labeled "Control: Outside XXL line", mostly to the right of the purple line bisecting the green circular buffer).

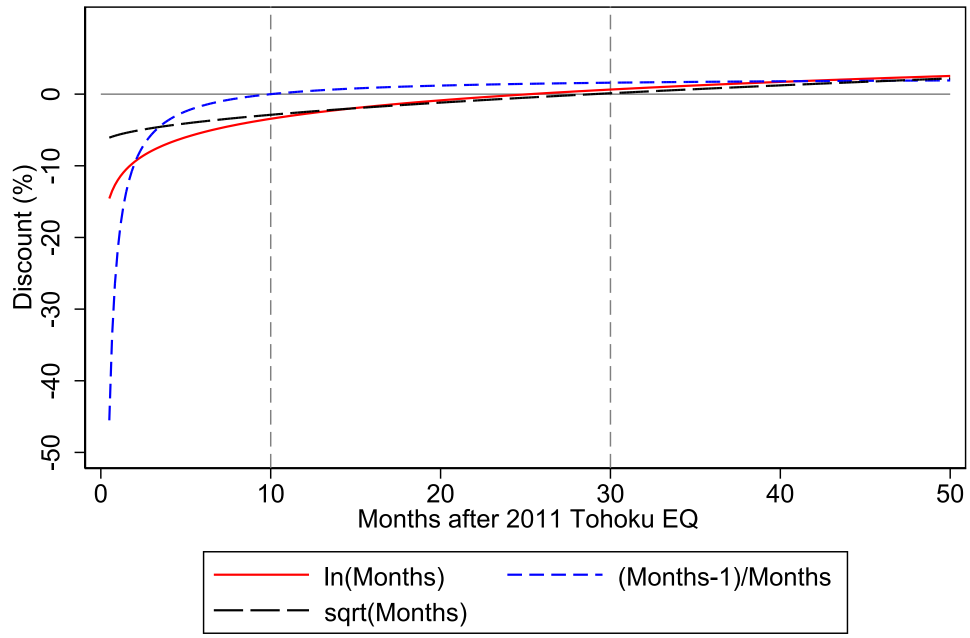
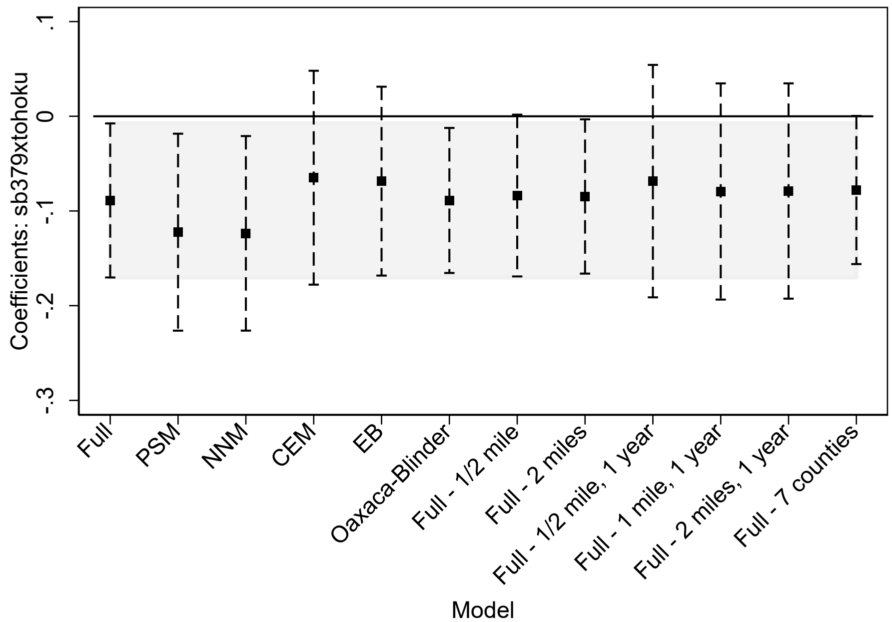
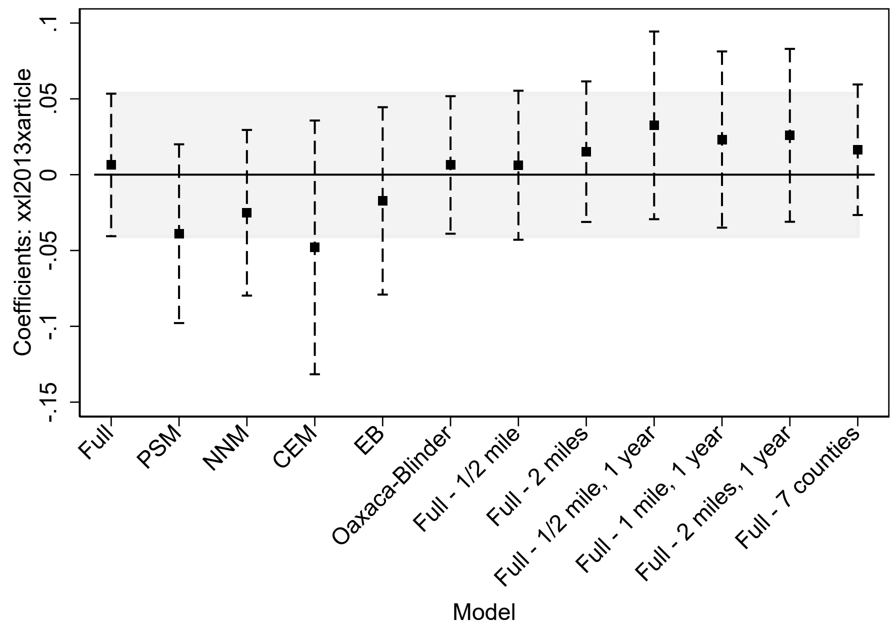


Figure 2.7. Decay effects of tsunami risk over time after the Tohoku earthquake and tsunami

Note: Plot of coefficients from equation (4) as in Bin and Landry (2013).



(a)



(b)

Figure 2.8. Average treatment effect on the treated with 95% confidence intervals for Models I and II of the first analysis

Note: The full data estimator is on the left. The next four points represent the estimators after the data was processed with the four matching methods (PSM, NNM, CEM, and EB). OB represents the Oaxaca-Blinder estimator. The final six estimators represent the full data estimator under different sample space assumptions. (a) For Model I. (b) For Model II.

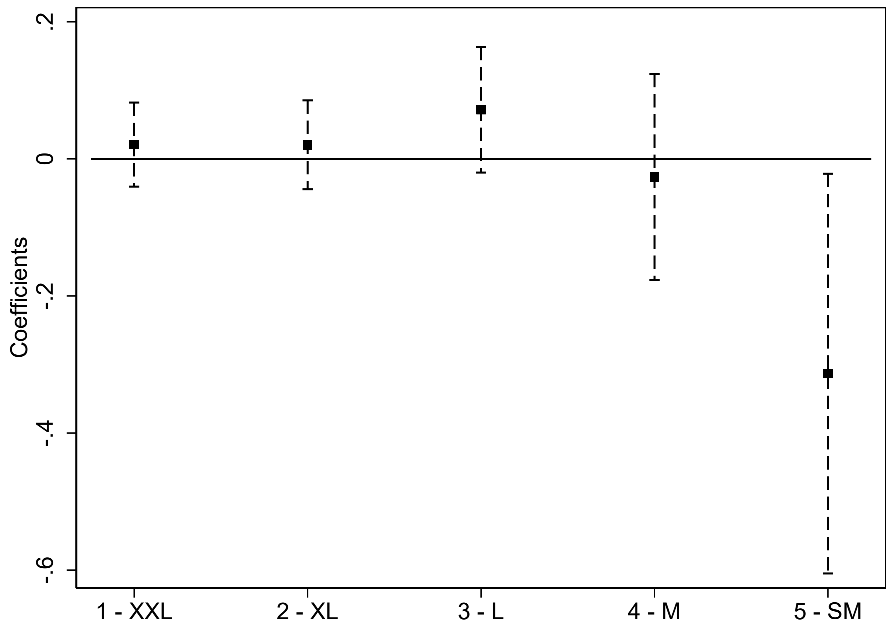
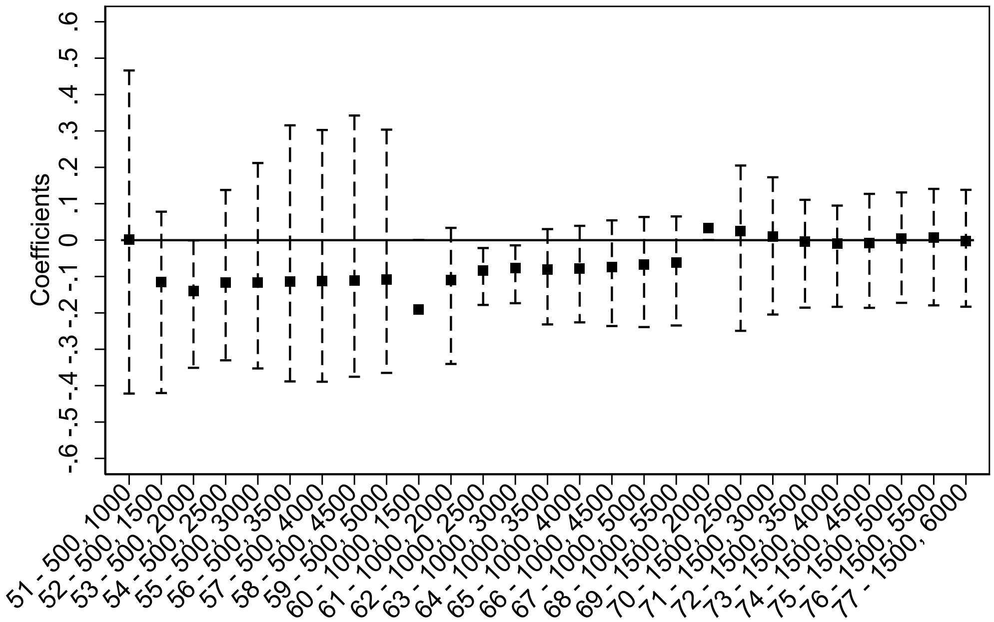
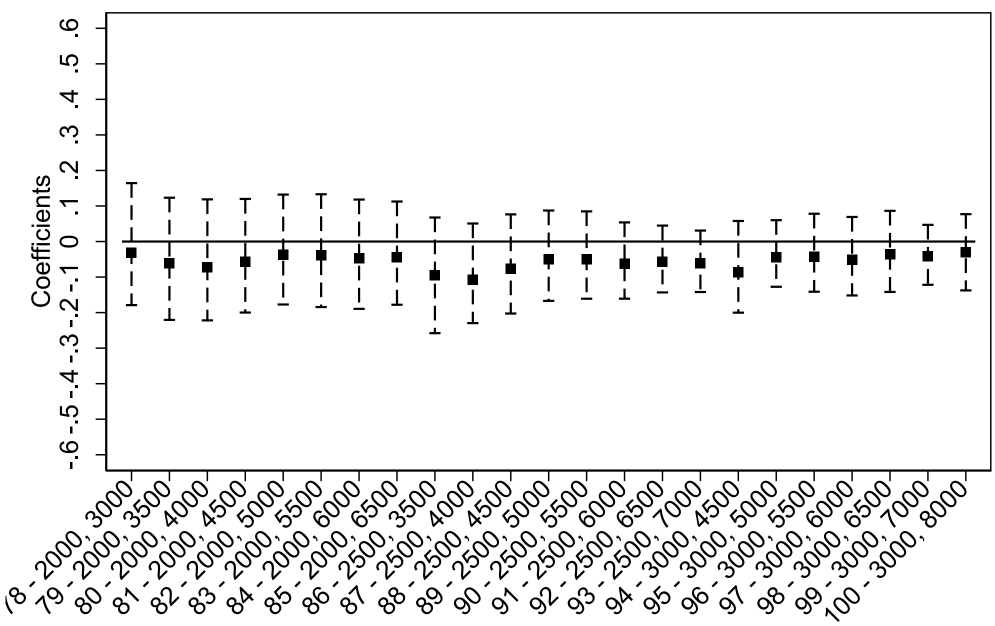


Figure 2.9. Average treatment effect on the treated with 95% confidence intervals for the second analysis



(a)



(b)

Figure 2.10. Average treatment effect on the treated with 95% confidence intervals for the third analysis

Note: Road network distances define the treatment and control buffers. For each ATET, the model number is followed by the size of the treatment buffer (ft) and the size of the control buffer (ft), e.g., Model 51 has a 500’ treatment buffer and 1000’ control buffer. (a) For Models 51-77. (b) For Models 78-100. Note: confidence intervals that are out of bounds are suppressed, e.g., for Model 60.

2.9 List of Tables

Table 2.1. Variable Definitions and Descriptive Statistics, by SB 379, First Analysis Sample, 2009-2017

	Outside SB 379 zone		Inside SB 379 zone		
	Mean	Std dev	Mean	Std dev	Std diff in means
Event					
Sold after 2011 Tohoku EQ (tohoku=1)	0.81	(0.39)	0.81	(0.39)	-
Sold after 2015 article (article=1)	0.33	(0.47)	0.32	(0.47)	-
Treatment					
Inside 1995 SB 379 tsunami zone (sb379=1)	0	(0)	1	(0)	-
Inside 2013 XXL tsunami zone (xxl2013=1)	0.31	(0.46)	0.99	(0.09)	-
Inside 2013 XL tsunami zone (xl2013=1)	0.28	(0.45)	0.99	(0.10)	-
Inside 2013 L tsunami zone (l2013=1)	0.12	(0.33)	0.96	(0.20)	-
Inside 2013 M tsunami zone (m2013=1)	0.04	(0.20)	0.82	(0.38)	-
Inside 2013 SM tsunami zone (sm2013=1)	0.01	(0.09)	0.47	(0.50)	-
Structural					
Sale price (2019 constant dollars)	306,745.77	(163,480.12)	323,071.60	(186,908.93)	-0.09
Bedrooms	2.89	(0.92)	2.68	(0.93)	0.23
Bathrooms	2.06	(0.78)	1.90	(0.75)	0.22
Indoor square footage	1,744.24	(715.21)	1,505.16	(645.45)	0.35
Total acreage (equal to indoor area if apartment)	0.42	(2.13)	0.33	(2.28)	0.04
Effective age of property (2018 - remodel year)	35.97	(25.54)	36.43	(24.46)	-0.02
Heating (=1)	0.95	(0.22)	0.91	(0.29)	0.17
Fireplace (=1)	0.66	(0.47)	0.61	(0.49)	0.09
Garage (=1)	0.77	(0.42)	0.69	(0.46)	0.18
Carport (=1)	0.04	(0.20)	0.03	(0.18)	0.04
Deck (=1)	0.11	(0.31)	0.16	(0.36)	-0.14
Patio (=1)	0.17	(0.38)	0.20	(0.40)	-0.07
Fencing (=1)	0.14	(0.35)	0.18	(0.38)	-0.10
Goal 18 eligible (=1)	0.02	(0.13)	0.10	(0.30)	-0.35
Has shoreline armoring (=1)	0.00	(0.05)	0.04	(0.20)	-0.28
Location					

Table 2.1. Variable Definitions and Descriptive Statistics, by SB 379, First Analysis Sample, 2009-2017 (Continued)

	Outside SB 379 zone		Inside SB 379 zone		
	Mean	Std dev	Mean	Std dev	Std diff in means
Special Flood Hazard Area (SFHA) (=1)	0.03	(0.16)	0.36	(0.48)	-0.94
Elevation (ft)	97.42	(70.54)	20.95	(11.02)	1.51
Slope (angular degrees of slope)	2.72	(4.82)	1.74	(2.38)	0.26
Distance to nearest beach access point (ft)	4,348.03	(6,943.63)	2,075.03	(4,633.56)	0.39
Distance to ocean shoreline (ft)	16,402.69	(23,311.22)	5,926.15	(13,706.17)	0.55
Oceanfront (=1)	0.03	(0.16)	0.11	(0.32)	-0.35
Distance to nearest water body (lake, pond, bay) (ft)	6,977.92	(7,673.00)	6,437.03	(9,694.99)	0.06
Distance to nearest river (ft)	8,155.13	(8,038.36)	4,987.01	(7,363.52)	0.41
Distance to nearest state park or public land (ft)	25,889.50	(26,449.02)	21,853.60	(24,369.87)	0.16
Distance to nearest national park or public land (ft)	17,547.64	(16,187.60)	20,618.42	(18,961.51)	-0.17
Distance to nearest highway or interstate (ft)	2,735.67	(4,070.97)	4,346.39	(6,942.60)	-0.28
Distance to nearest major road (ft)	3,173.23	(5,045.23)	5,383.81	(8,321.11)	-0.32
Distance to nearest railroad (ft)	68,837.11	(60,557.73)	83,561.70	(51,105.73)	-0.26
Distance to nearest airport (ft)	32,312.90	(19,089.39)	26,215.34	(19,586.41)	0.32
Distance to nearest k-12 school (ft)	14,668.42	(15,629.87)	12,327.99	(10,823.89)	0.17
Distance to nearest central business district (city) (ft)	11,027.20	(10,671.49)	9,171.75	(8,882.89)	0.19
Distance to nearest wastewater treatment plant (ft)	15,651.49	(11,137.14)	11,604.52	(9,447.23)	0.39
Distance to nearest fire station (ft)	5,992.65	(4,597.47)	6,141.79	(5,116.56)	-0.03
Distance to nearest law enforcement station (ft)	30,593.44	(35,657.69)	34,384.59	(44,793.06)	-0.09
Distance to nearest hospital (ft)	45,555.14	(42,443.18)	54,716.99	(45,225.25)	-0.21
Observations	11,467		4,160		

Table 2.2. Second Analysis Samples, 2011-2015

Sample	Model	Total observations	Outside inundation zone	Inside inundation zone
Within 1 mile of the XXL inundation zone	1	8,010	5,855	2,155
Within 1 mile of the XL inundation zone	2	7,790	5,829	1,961
Within 1 mile of the L inundation zone	3	6,593	5,698	895
Within 1 mile of the M inundation zone	4	5,842	5,527	315
Within 1 mile of the SM inundation zone	5	5,429	5,348	81

Table 2.3. Difference-in-differences selected results for the first analysis, full data

Variables	Model I Coefficient/SE	Model II Coefficient/SE	Model III Coefficient/SE
Event			
Sold after 2011 Tohoku EQ (tohoku=1)	.0858**		.0631
	(.0426)		(.0390)
Sold after 2015 article (article=1)		.0136	.0026
		(.0236)	(.0200)
Treatment			
Inside 1995 SB 379 tsunami zone (sb379=1)	.0620*		.0671**
	(.0333)		(.0308)
Inside 2013 XXL tsunami zone (xxl2013=1)		-.0073	
		(.0222)	
Diff-in-Diff			
SB 379 zone (sb379) x sold after 2011 Tohoku EQ (tohoku)	-.0889**		-.0675**
	(.0415)		(.0340)
2013 XXL zone (xxl2013) x sold after 2015 article (article)		.0064	
		(.02397)	
SB 379 zone (sb379) x sold after 2015 article (article)			.0269
			(.02441)
Location			
Elevation (ft)	5.7e-04***	2.6e-04**	4.6e-04***
	(1.7e-04)	(1.3e-04)	(9.8e-05)
Log distance to ocean shoreline	-.0835***	-.0746***	-.0786***
	(.0115)	(.0059)	(.0055)
Elevation (ft) x Log distance to ocean shoreline x on oceanfront (=1)	3.9e-04***	2.7e-04***	3.2e-04***
	(7.7e-05)	(7.4e-05)	(5.3e-05)
Observations	5890	9160	15627
Adj. R-squared	0.376	0.441	0.411

* p<0.10, ** p<0.05, *** p<0.01

Table 2.4. Oaxaca-Blinder results for the first analysis, full data

	Model I Coefficient/SE	Model II Coefficient/ SE
Overall Differential		
Treated group	12.457***	12.537***
	(.0239)	(.0118)
Control group	12.451***	12.492***
	(.0086)	(.0074)
Difference	.0063	.0449***
	(.0254)	(.0139)
Decomposition		
Explained	.0952**	.0385*
	(.0386)	(.0231)
Unexplained	-.0889**	.0064
	(.0391)	(.0231)
Observations	5890	9160

* p<0.10, ** p<0.05, *** p<0.01

Table 2.5. Event study results for the first analysis, full data

	Model I		Model II	
	Coefficient	SE	Coefficient	SE
lead8	-.0581	(.1246)	-.0357	(.0537)
lead7	.0244	(.0663)	-.0325	(.0574)
lead6	.1344**	(.0622)	-.0113	(.0440)
lead5	.0899	(.0630)	-.0269	(.0404)
lead4	.0142	(.0599)	.0079	(.0381)
lead3	.0634	(.0602)	.0237	(.0399)
lead2	.0824	(.0603)	-.0006	(.0361)
lag0	.0609	(.0707)	.0603*	(.0318)
lag1	-.1399**	(.0682)	.0534	(.0364)
lag2	-.0212	(.0606)	-.0671*	(.0386)
lag3	-.0675	(.0659)	.0127	(.0353)
lag4	.0284	(.0632)	.0008	(.0354)
lag5	-.0372	(.0551)	.0007	(.0381)
lag6	.0267	(.0577)	-.0657	(.0429)
lag7	-.0056	(.0625)	-.0570	(.0395)
lag8	.0890	(.1266)	-.0134	(.0667)
Observations	5890		9160	
Adj. R-squared	0.375		0.441	

* p<0.10, ** p<0.05, *** p<0.01

Table 2.6. Difference-in-differences selected results, matched data

Matching method and Diff-in-Diff estimators	Model I Coefficient/SE	Model II Coefficient/SE	Model III Coefficient/SE
Nearest neighbor propensity score (PSM)			
SB 379 zone (sb379) x sold after 2011 Tohoku EQ (tohoku)	-.1224** (.0530)		-.1056** (.0426)
2013 XXL zone (xxl2013) x sold after 2015 article (article)		-.0389 (.0301)	
SB 379 zone (sb379) x sold after 2015 article (article)			.0459 (.0297)
Nearest neighbor Mahalanobis (NNM)			
SB 379 zone (sb379) x sold after 2011 Tohoku EQ (tohoku)	-.1236** (.0524)		-.0165 (.0415)
2013 XXL zone (xxl2013) x sold after 2015 article (article)		-.0251 (.0279)	
SB 379 zone (sb379) x sold after 2015 article (article)			6.7e-04 (.0293)
Coarsened exact matching (CEM)			
SB 379 zone (sb379) x sold after 2011 Tohoku EQ (tohoku)	-.0649 (.0576)		-.0923* (.0508)
2013 XXL zone (xxl2013) x sold after 2015 article (article)		-.0480 (.0427)	
SB 379 zone (sb379) x sold after 2015 article (article)			.0371 (.0355)
Entropy balancing (EB)			
SB 379 zone (sb379) x sold after 2011 Tohoku EQ (tohoku)	-.0685 (.0509)		-.0393 (.0410)
2013 XXL zone (xxl2013) x sold after 2015 article (article)		-.0173 (.0315)	
SB 379 zone (sb379) x sold after 2015 article (article)			-.0086 (.0291)

* p<0.10, ** p<0.05, *** p<0.01

Table 2.7. Difference-in-differences and triple differences results for the third analysis, Model 62

	DID Coefficient	p-value	DDD Coefficient	p-value
Treatment				
Blue line treatment buffer (treatment362=1)	.0218	.4658	.0398	.2532
Event				
Sold after first blue line installed (event362=1)	.0185	.8296	.1012	.7396
Sensitivity				
Inside 2013 XXL tsunami zone (xxl2013=1)			.1365*	.0800
Diff-in-Diff				
Blue line treatment buffer (treatment362) x sold after first blue line installed (event362)	-.0834**	.0254	-.0832	.4731
Blue line treatment buffer (treatment362) x 2013 XXL zone (xxl2013)			-.0623	.3290
2013 XXL zone (xxl2013) x sold after first blue line installed (event362)			-.2488	.1507
Triple Difference				
Blue line treatment buffer x 2013 XXL zone x sold after first blue line installed			-.0117	.9404
Location				
Elevation (ft)	5.9e-04	.2038	.0011	.1197
Elevation (ft) x Log distance to ocean shoreline x on oceanfront (=1)	2.9e-04	.2527	2.8e-04	.2660
Log distance to ocean shoreline	-.0799***	.0081	-.0747***	.0088
Observations	1334		1334	
Adj. R-squared	0.491		0.496	

* p<0.10, ** p<0.05, *** p<0.01

3 Economic valuation of coastal erosion management on the Oregon Coast using stated preference data

3.1 Introduction

Developed beaches – beaches in coastal towns with buildings and other structures behind the beach – provide a variety of recreation opportunities by facilitating access to coastal amenities as well as nearby restaurants, shops, and lodging facilities. Developed beaches are also the first line of defense for oceanfront residential and commercial development against hazards like coastal erosion, storm surge, and high tides. These chronic coastal hazards are driven by winter storms, currents, winds, rain, runoff, and elevated water levels caused by seasonal variations (e.g., El Niño–Southern Oscillation) and sea level rise (SLR). Rising sea levels will also likely increase the impacts of coastal erosion and other chronic coastal hazards (Institute for Water Resources, 2022; Sweet et al., 2022). Developed beaches tend to be more vulnerable to the effects of erosion than undeveloped (or natural) beaches because coastal development is fixed in place. For example, erosion may cause the shoreline of an undeveloped beach to move inland but the beach can preserve width (and access) because it is not confined by development behind the beach. On a developed beach, however, the beach gets narrower as it erodes because structures behind the beach fix the shoreline in place. As the beach loses width, beach access for recreation will decrease and oceanfront properties will become more vulnerable to erosion and inundation. Thus, developed beaches may require active management to preserve safe recreation access in the future.

This motivates the following research questions. First, what is the economic value of coastal erosion management policies that affect safe recreation access on developed beaches? Second, how do coastal management policies affect use and non-use values for developed beaches? Since coastal management policies impact both beach recreators and non-recreators, in our second question we ask how the welfare effects of coastal erosion management differ between these two groups of people. To answer these questions we develop a combined revealed and stated preference survey and collect primary survey data from Oregon households. To understand how Oregon residents value coastal management policies that impact recreation, it is also necessary to consider the existing policy context. Thus, we are also interested in measuring the welfare effects of changing (relaxing) Oregon's existing coastal management policy, Statewide Planning Goal 18.

Our overarching objective is therefore to use combined revealed and stated preference methods to measure Oregonians' willingness to pay for coastal erosion management policies that preserve safe recreation access on developed Oregon beaches and relax or maintain coastal armoring restrictions under Statewide Planning Goal 18.

In this chapter we answer the first research question but also explain how the survey data will be used to answer the second research question. Our focus is on describing the survey instrument and answering the first research question using the stated preference data from the survey. The survey gathers information on recreation use of developed beaches (revealed preference data), knowledge of coastal processes and management, attitudes about coastal issues, and contingent valuation referendum votes (stated preference data) for coastal erosion management policies using two distinct sample frames – an address-based probability sample and an opt-in online panel from Qualtrics.[56] We use the stated preference data from the Qualtrics online panel sample to estimate contingent valuation models and measure Oregonians' total economic value (willingness to pay) for erosion management conditional on differences in coastal armoring policy for private landowners. We also address one potential response anomaly in our survey data: attribute non-attendance, which occurs when respondents ignore one or more attributes in the valuation question. We use respondents' stated attendance information to modify our primary models and allow "attending" and "non-attending" respondents to have different outcomes.

Also included in this chapter is an outline of the combined revealed and stated preference modeling framework that motivated large portions of our survey design. This modeling framework has been used in the context of water quality valuation in South Korea (Eom & Larson, 2006), Iowa (Egan, 2011), and Taiwan (Huang et al., 2016) as well as for valuing coastal erosion management plans in North Carolina (Landry et al., 2020). This framework will allow us to answer our second research question by enabling a decomposition of willingness to pay into use and non-use values for coastal erosion management policies that affect safe recreation access. The results of this combined model are not estimated in this chapter.

[56] As a secondary question we are also interested in comparing the results of a probability sampling method – address-based mailing – with a non-probability sampling method – an opt-in Qualtrics online panel. This question is not addressed in this chapter.

The two erosion management strategies that have generally been employed along sandy coastlines in the U.S. are shoreline armoring and beach nourishment (Gopalakrishnan et al., 2018; Landry et al., 2003, 2020). Given potentially conflicting interests between beach recreators and oceanfront landowners in Oregon, one objective of this research is to analyze Oregonians' public support for these two management options. Shoreline armoring can protect oceanfront properties but cannot preserve safe recreation access in response to coastal erosion and SLR on Oregon's developed beaches. A sediment management plan (e.g., beach nourishment) could preserve safety and access. However, of these two management strategies, only shoreline armoring has been used in Oregon. Thus, it is critical to understand what economic value Oregon residents place on potential sediment management plans within the existing Statewide Planning Goal 18 shoreline armoring policy context. We are also interested in estimating the economic value of relaxing Goal 18 to allow more shoreline armoring given recent interest in this possibility.

Prior studies that have estimated economic values for coastal management policies have focused on East Coast states like Florida (Shivlani et al., 2003), Georgia (Landry et al., 2003), South Carolina (Pompe & Rinehart, 1995), North Carolina (Gopalakrishnan et al., 2016, 2018; Landry et al., 2020; Whitehead et al., 2008), Delaware (Parsons & Powell, 2001), New Jersey (Dundas, 2017; Silberman et al., 1992), Connecticut (Johnston et al., 2018), and New Hampshire and Maine (Huang et al., 2007; Lindsay et al., 1992). To our knowledge, there are only two studies on the West Coast that have estimated economic values for coastal management policies and both have used revealed preference methods, a study in California by Pendleton et al. (2012) and another in Oregon using a hedonic approach (Dundas & Lewis, 2020). Revealed preference methods can be used when we have data on decisions made under policies that have actually occurred. However, when we're interested in investigating policy options that have not been used before in this area – like sediment management in Oregon – we need to use stated preference methods. To the best of our knowledge, there have been no prior stated preference studies on how Oregon residents value coastal management policies in response to erosion and SLR by exploring management options outside the range of historical options.

Economic values from research on the East Coast are not readily transferable to Oregon for two reasons. First, the policy landscape differs between the two coasts. On the East Coast, beach nourishment and shoreline armoring are regularly-used approaches to coastal erosion control on sandy beaches (Gopalakrishnan et al., 2018; Landry et al., 2003, 2020). In Oregon, however, beach nourishment has not been used to provide coastal protection or recreation opportunities (Elko et al., 2021; Program for the Study of Developed Shorelines, n.d.). Second, individuals' preferences for beach attributes also likely differ between the two coasts given differences in coastal geomorphology, e.g., the proportion of rocky shores versus sandy beaches or gravel beaches. Previous research has focused on beach width as a key beach attribute that recreators and property owners value (Gopalakrishnan et al., 2011; Landry et al., 2003, 2020; Parsons et al., 2013; Pompe & Rinehart, 1995; Shivlani et al., 2003; Silberman et al., 1992; Whitehead et al., 2008). In the Pacific Northwest the variation in widths between beaches is large and many beaches have large widths (Institute for Water Resources, 2022; Ruggiero et al., 2013). Rather than beach width, safe recreation access may be a more salient attribute to Oregon beach recreators (C. Plybon, personal communication, January 13, 2021). Thus, in this chapter we focus on how proposed coastal management plans would change the beach attribute of safe recreation access.

We find that the economic value of coastal erosion management policies that impact safe recreation access on developed beaches is high – between $296 and $342 per household per year. We apply these estimates to all Oregon households to find an aggregate economic welfare estimate of approximately $490 to $560 million annually for coastal management that preserves safe recreation access. We do not find evidence of a statistically significant difference between Oregon residents' willingness to pay for a coastal management plan where the Goal 18 armoring policy is relaxed to allow more armoring and a plan where Goal 18 is maintained in its current form. However, we find that when the shoreline armoring scenario the respondent was shown (i.e., relaxing or maintaining Goal 18) is interacted with the respondent's beliefs about shoreline armoring, the Goal 18 scenario often did impact how they voted on the proposed coastal management plan. We also find that respondents who believe that Goal 18 will likely be maintained in its current form, respondents who believe that vulnerable properties should be allowed to install armoring, and respondents who said they were aware that safe recreation access on developed beaches may decrease as erosion increases were all more likely to support the proposed coastal management plan.

This chapter proceeds as follows. Section 3.2 provides background information on coastal hazards and management in Oregon. Section 3.3 reviews stated preference methods and best practices. Section 3.4 describes the survey design and the survey data used in this analysis. Section 3.5 defines our empirical approach in this chapter as well as the proposed combined revealed and stated preference modeling framework that motivated our survey design. Section 3.6 presents results and Section 3.7 concludes by providing a summary of our findings and next steps.

3.2 Coastal hazards and management in Oregon

Coastal erosion and rising sea levels along the Oregon Coast will impact oceanfront landowners, beach recreators, and non-recreators who value Oregon's developed beaches. Rising sea levels not only have the potential to increase the vulnerability of developed Oregon beaches and private oceanfront property to erosion but will also likely decrease safe recreation access to these beaches. Scenarios of future SLR range from 0.2 m to 1.8 m by 2100 for the Pacific Northwest (Oregon to Washington) U.S. coastline (Sweet et al., 2022). Under an intermediate SLR scenario of 0.9 m by 2100, population projections suggest that over 12,000 people on the Oregon Coast would likely be at risk of inundation (Hauer et al., 2016). In Tillamook and Lincoln Counties on the northern Oregon Coast, where 10 out of 16 of the developed beaches in the study are located, the majority of the coastline has been eroding on average; the average net rate of short-term (1960s through 2002) shoreline change was negative in six out of seven littoral cells (Ruggiero et al., 2013).[57] Many of the beaches on the northern Oregon Coast are in a degraded state, and therefore exposed to future storm-induced flooding and erosion, due to not having fully recovered from the 1997-98 El Nino and 1998-99 severe winter (Ruggiero et al., 2013). Ruggiero et al. (2013) also find that a greater percentage of the Pacific Northwest shoreline has become erosional over time.[58] Due to increasing erosion and SLR, Oregon's developed beaches may therefore require active management in the future to preserve oceanfront development and safe recreation access.

Erosion poses a challenge for coastal management because it creates conflicting interests between beach recreators and oceanfront landowners. Both groups of Oregon residents have

[57] A littoral cell represents a self-contained coastal unit with little to no sediment transport occurring between cells. The net short-term shoreline change was negative – indicating erosion – in 10 of the 18 littoral cells in Oregon (Ruggiero et al., 2013).
[58] While 36% of the PNW shoreline eroded in the long-term (1860s through 2002), 44% of the shoreline eroded in the short-term (1967 to 2002 in Oregon and 1986 to 2002 in Washington) (Ruggiero et al., 2013).

property rights associated with developed beaches, the former through the Beach Bill and the latter through Statewide Planning Goal 18. In 1967, the Oregon Legislature passed legislation commonly known as the Beach Bill that gave Oregonians a permanent public easement to access and recreate on all beaches seaward of the existing line of vegetation, regardless of ownership (Department of Land Conservation & Development, n.d.-b). Discussions with Charlie Plybon, the Oregon Policy Manager for the Surfrider Foundation, suggested that safe access is a key concern for people making recreation trips to the Oregon Coast (C. Plybon, personal communication, January 13, 2021). Safe recreation access implies that beaches are sufficiently wide so that recreators can avoid common safety hazards such as sneaker waves (waves that surge high up on the beach often without warning) and rip currents (strong currents that can carry even the strongest swimmers away from shore). For example, for much of the U.S. West Coast, sneaker waves result in more fatal accidents than all other weather hazards combined (NOAA National Weather Service, n.d.).

Prior studies have also found that recreators value beach width and coastal management policies that lead to wider beaches (Landry et al., 2003, 2020; Parsons et al., 2013; Pendleton et al., 2012; Shivlani et al., 2003; Silberman et al., 1992; Whitehead et al., 2008). Therefore, Oregon beach recreators are likely to prefer coastal management options that preserve beach width and safe access. One such management option is beach nourishment, a type of sediment management where sand is dredged from another location and spread on a beach to increase beach width and combat erosion. By increasing beach width, beach nourishment can also help prevent damage to oceanfront development. However, in Oregon there have been no federal or state efforts to manage sediment (nourish beaches) specifically to provide coastal protection or recreation opportunities (Elko et al., 2021; Program for the Study of Developed Shorelines, n.d.).

Oregon land use policy allows only one option for oceanfront landowners to protect infrastructure behind developed beaches from erosion: shoreline armoring. Shoreline armoring involves the construction of seawalls, riprap revetments, and other hard structures to protect oceanfront properties from erosion and storm surges. Oregon’s Statewide Planning Goal 18 designates which parcels are eligible to install shoreline armoring (Department of Land Conservation & Development, n.d.-a). Goal 18 was originally implemented in 1977 to restrict armoring of private property to conserve and protect Oregon's beaches in their natural state for all beach users. Goal 18 restricts armoring eligibility to land parcels where development existed prior to January 1st, 1977. All properties developed since that date are not eligible to install armoring. Approximately 50% of Oregon's 9,050 oceanfront parcels are eligible for armoring and, as of 2015, approximately 1,000 of these eligible parcels have installed armoring (Beasley & Dundas, 2021). If Goal 18 is maintained, projections suggest that another 300 eligible parcels will install shoreline armoring in the next 30 years.[59]

However, because the option to armor is not available to every oceanfront property owner and given concerns about erosion and rising sea levels, some coastal landowners have expressed interest in relaxing Goal 18 to allow more oceanfront landowners to armor the shoreline in front of their homes. For example, homeowners along Tillamook County's Rockaway Beach and Lincoln County's Gleneden Beach recently received exceptions to construct armoring structures despite not being eligible under Goal 18 (Foden-Vencil, 2022). If Goal 18 is relaxed, the projected number of parcels that will install armoring in the next 30 years increases to 550 parcels, including many that are not currently eligible. While relaxing Goal 18 would allow more private landowners to protect their properties from erosion, the armoring structures may take up space on the public recreation easement and further reduce the width of these beaches. By disrupting the natural flow of sediment, shoreline armoring can lead to erosion of the natural shoreline in front of the armoring and also in neighboring regions of the coastline (NOAA Office of Ocean and Coastal Resource Management, 2010).

Both shoreline armoring and beach nourishment are commonly used management strategies in the U.S. (Gopalakrishnan et al., 2018; Landry et al., 2003, 2020). However, these two management strategies differ in their ability to protect oceanfront properties and preserve safe recreation access. Therefore, beach recreators and oceanfront landowners in Oregon may have conflicting preferences for shoreline armoring and sediment management on developed beaches.

3.3 Stated preference methods

[59] These projections were calculated using the data and method described in Beasley and Dundas (2021).

3.3.1 Contingent valuation method

Stated preference (SP) methods use survey information on how respondents say they would behave in hypothetical situations to estimate measures of economic value, e.g., willingness-to-pay (WTP) for environmental goods and services. In comparison, revealed preference (RP) methods use data revealed in related market transactions to estimate measures of economic value, e.g., the travel time and money costs borne while taking a trip to a destination for the purpose of outdoor recreation.

Two common types of SP methods are the discrete choice contingent valuation method (CVM) and the discrete choice experiment (DCE). DCE surveys ask respondents to choose between a set of hypothetical alternatives where each alternative is defined by a set of attributes with potentially different levels. CVM surveys ask respondents about their WTP for a change in the quantity, quality, or probability to be valued, e.g., a change in environmental quality. The CVM's valuation question is often framed as a hypothetical referendum that asks respondents whether they would vote for a hypothetical government program with a specified cost. When framed as such, the valuation question is a binary, take-it-or-leave-it discrete choice question over a single issue. There are several different ways of stating the valuation question within the CVM, including, open-ended, payment card, dichotomous (single binary) choice, and double-bounded dichotomous choice. In the open-ended format, the respondent is asked to state the maximum amount they would be willing to pay for a proposed change. If the survey is incentive compatible, their response provides a direct estimate of their WTP. For an environmental CVM survey that asks the respondent about their WTP for a program or scenario that improves or protects the environmental good or service, the WTP can be formally defined as the compensating variation (CV), i.e., the decrease in income that leaves the person indifferent between the baseline and improved level of the environmental good. In the payment card approach, the respondent is either shown a card containing a range of prices (or bids) and selects the price corresponding to their maximum WTP or answers "yes" or "no" to several possible bids on the card.

In the single-bounded dichotomous choice (SBDC) format, respondents are asked whether they would pay a specified amount of money for a change in environmental quality. This is a "yes" or "no" closed-ended (dichotomous) question about their WTP for this change. The question varies the presented environmental quality change and bid amount across individuals. This format minimizes the cognitive burden of answering a CVM question by asking respondents to respond to a single posted price as they would in a real life setting. The SBDC format has become the most widely used elicitation method in CVM surveys due its property of being incentive compatible in many settings (Phaneuf & Requate, 2016, p. 577). Vossler et al. (2012) provide a proof for Carson and Groves' (2007) argument that an SBDC format is incentive compatible when it meets four conditions: (i) survey participants care about the outcome; (ii) the policy administrator can enforce payments by voters; (iii) the elicitation involves a yes or no vote on a single policy; and (iv) the probability that the proposed policy is implemented is weakly monotonically increasing with the proportion of yes votes. Thus, a binary choice CVM elicitation format can be incentive compatible if it is sufficiently similar to a single issue voting referendum.

However, the SBDC format is statistically inefficient (Landry, 2017). Since it only collects respondents' WTP information for one price point, it requires a large number of responses. Hanemann et al. (1991) developed the double-bounded dichotomous choice (DBDC) approach as an extension of the SBDC approach that is asymptotically more efficient. The DBDC format is more efficient because it collects WTP information at two price points for each respondent. After the respondent replies to the initial bid (B^1), they are asked to reply to a follow-up dichotomous question with a different bid (B^2). If the respondent answered "yes" to the initial question, the follow-up bid is higher. And if the respondent answered "no" to the initial question, the follow-up bid is lower. Thus, the DBDC approach collects more information about the respondents' WTP and potentially either bounds their WTP or lowers (raises) the upper (lower) bound. Empirically, however, WTP estimates from SBDC and DBDC questions often differ (Landry, 2017; Phaneuf & Requate, 2016, p. 588). One possible explanation is that the follow-up question is not incentive compatible because a sequence of binary choices, as in the DBDC format, may lead to strategic answers (Carson & Groves, 2007; Phaneuf & Requate, 2016, p. 594). For example, the respondent may believe that: the second price signals that the agency overseeing the program is willing to bargain over the price, the actual cost will be some type of weighted average of the two prices, the second price signals that the quantity has changed to match the changed price, or that the actual cost to the respondent is uncertain (Carson & Groves, 2007). Scheufele and Bennett (2012) investigated the effects of repeated binary choices on choice behavior (in DCEs) and found evidence of strategic response. Their results suggest that respondents are more cost sensitive (and thus have a lower WTP) if they were shown the same bundle at a lower cost in the previous choice task, but not if they were shown the same bundle at a higher cost in the previous choice task.

Multiple studies have used SBDC (Banerjee et al., 2016; Loomis & Santiago, 2013; Shivlani et al., 2003) and DBDC (Alberini et al., 2004; Logar & van den Bergh, 2014; Oh et al., 2008) CVM methods to measure economic values for coastal erosion management or beach recreation improvements. Banerjee et al. (2016) use an SBDC approach in an *ex post* evaluation of coastal infrastructure projects that combined armoring and beach nourishment at three different beaches in Barbados. Tourists, residents, and businesses were all surveyed using separate CV surveys, with the only difference between the tourist and resident surveys being the payment vehicle. The tourist survey results showed that beach width and sandy beaches were the most important beach characteristics for tourists. Sandy beaches were the most important beach characteristic for residents although residents' motivations for paying or not paying to maintain the beaches were more complex (Banerjee et al., 2016). Loomis and Santiago (2013) estimate beach visitors' WTP for increasing a particular beach attribute one level from its worst level. They use both SBDC CVM and DCE methods to compare the incremental values of increasing a particular beach attribute from its worst level to an improved level. The SBDC surveys consisted of three independent WTP questions, one for each set of attribute levels. Their four beach attributes are wave height, absence of trash, crowding, and water clarity. They find that improving water clarity and eliminating trash are the two attributes that have a statistically significant impact on the value of beach visits in Puerto Rico (Loomis & Santiago, 2013). Shivlani et al. (2003) use an SBDC approach to measure the WTP for beach nourishment that would widen beaches for improved recreational access or that would enhance nesting habitat for turtles on three South Florida beaches threatened by coastal erosion. Each respondent was asked questions about only recreational opportunities or resource protection, i.e., this was a split-sample design in which respondents asked the turtle nesting habitat question weren't asked the recreational access question. Results suggest there are two types of beach goers: occasional or seasonal visitors and repeat visitors. They find that beach visitors value enhancing habitat: respondents increased their

WTP over the WTP for improved beach recreational access when informed that beach nourishment would also increase sea turtle nesting habitat (Shivlani et al., 2003).

Using a DBDC approach, Alberini et al. (2004) estimate WTP for a public program in the island of S. Erasmo in the Lagoon of Venice that would reduce coastal erosion (via beach nourishment) and improve infrastructure. They find that visitors and potential visitors to S. Erasmo have statistically higher mean WTP for this program than non-visitors (Alberini et al., 2004). Logar and van den Bergh (2014) examine the WTP of beach visitors in Croatia for beach maintenance aimed at preventing erosion. They conduct a DBDC CVM survey of two beaches – one with an existing market (a beach with entrance fees) and one with no market (the nearest free beach). They find that WTP estimates for preventing erosion are higher for the free beach than for the paid beach and that these WTP estimates are slightly higher than the current entrance fee at the paid beach (Logar & van den Bergh, 2014). Oh et al. (2008) use a DBDC approach to determine non-resident visitors' WTP for public beach access to three popular South Carolina beaches. They estimate that mean WTP for a beach visitor was $6.60 per day for additional beach access points and parking (Oh et al., 2008). Coastal SP research has also frequently used beach width as the environmental quality measure that is changed by the proposed coastal management policy (Landry et al., 2003, 2020; Parsons et al., 2013; Shivlani et al., 2003; Silberman et al., 1992; Whitehead et al., 2008). Gopalakrishnan et al. (2016) compile a fairly comprehensive review of SP, RP, and combined RP/SP studies of coastal erosion management.

3.3.2 Combined contingent valuation and recreation demand model

SP data can also be combined with RP data to estimate economic values. The benefit of this approach is that is that the strengths and weakness of RP and SP methods complement each other (Phaneuf & Requate, 2016, p. 609). A strength of RP methods is that they use data on decisions that actually occurred, while a weakness of SP methods is that decisions are made in hypothetical situations. Likewise, a strength of SP methods is that the researcher is able to exploit variation outside of the range that has occurred historically, while a weakness of RP methods is that they rely on naturally occurring variation in observational data. Some combined RP/SP frameworks also allow for a decomposition of WTP into use and non-use values as well as tests of common preference assumptions for RP methods (i.e., the weak complementarity assumption and the Willig (1978) condition).

Eom and Larson (2006) develop an empirical framework to estimate WTP for environmental quality changes by combining revealed (recreation demand) and stated (CVM) data. They start from a utility maximization problem in which the individual is choosing recreational trip frequency. They specify trip demand empirically using a semi-log functional form, substitute it into the utility function, and integrate the resulting indirect utility function to recover a quasi-expenditure function. This quasi-expenditure function depends on travel cost, environmental quality, and a constant of integration term that can incorporate potential non-use value. Using this quasi-expenditure function they derive a CV measure for total value (WTP) and decompose this into use and non-use values. By allowing for a decomposition of WTP into use and non-use components, this framework also enables them to test the weak complementarity assumption of zero non-use value. The empirical approach combines CVM and trip frequency data to jointly estimate the WTP and recreation demand equations. Eom and Larson (2006) apply their framework to estimate use and non-use values for improvements in water quality for the Man Kyoung River basin in South Korea. Egan (2011) apply the Eom and Larson (2006) model to water quality for the eight lakes in Iowa. They employ a “between” experimental survey design by dividing the state of Iowa into eight zones and asking all households in each zone a CV question about improving water quality in the “focus lake” of their zone.

Huang et al. (2016) extend the Eom and Larson (2006) framework by deriving WTP functions and their use and non-use components for six commonly used empirical recreation demand models and three specifications for the constant of integration. They also show that the Willig (1978) condition – a necessary condition for using a Marshallian welfare measure (the change in the area under the Marshallian demand curve) to approximate a Hicksian welfare measure (CV) for a quality change – can be tested as a parameter restriction in this framework.[60] They also derive the weak complementarity and Willig conditions for each of their derived WTP

[60] Willig (1978) presents three versions of this condition. Huang et al. (2016) use the condition that is most recognized in the literature. This version of the Willig condition requires that the relative slopes of indifference curves in the price and quality space are independent of income.

functions. They apply their extended framework to estimate the WTP for maintaining water quality in the Danshui River System in Northern Taiwan.

Landry et al. (2020) build on the models of Eom and Larson (2006) and Huang et al. (2016) to estimate WTP for coastal erosion management in North Carolina among beach recreators and non-recreators. They expand this framework by incorporating a "between" research design where the sample was split between three types of beach erosion management strategies: beach replenishment (nourishment), shoreline armoring in conjunction with beach replenishment, and shoreline retreat. They also vary the environmental impacts of erosion management in their between experimental design so that some respondents are presented with a scenario with minimal environmental impacts and other respondents are presented with negative environmental impacts. The environmental quality that is changed in the CVM question is beach width. Thus, their experimental design allows them to estimate how use and non-use values are affected by beach width, erosion management strategy, and negative environmental impacts of the erosion management intervention.

3.3.3 Stated preference best practices

A rich literature on stated preference best practices has emerged since the original comprehensive set of guidelines in the Arrow et al. (1993) NOAA Blue Ribbon panel report on contingent valuation following the 1989 Exxon Valdez oil spill (Carson & Groves, 2007; Johnston et al., 2017; Kling et al., 2012). A key development since the NOAA panel is the emphasis on incentive compatibility and consequentiality as necessary conditions for truthful reporting in SP surveys. Several conditions are necessary to elicit truthful responses from survey respondents, including consequentiality and incentive compatibility (Carson & Groves, 2007; Kling et al., 2012). A survey is consequential if a respondent believes that the answers they provide will influence decisions related to the outcomes of the policy and if the respondent cares about the outcomes of the policy (Carson & Groves, 2007). A survey is incentive compatible if a truthful response to the question asked constitutes an optimal strategy for the respondent (Carson & Groves, 2007).

If these conditions hold, then SP methods have several desirable features. A key advantage of SP methods is that they allow the researcher to ask about environmental quality values that are outside of the range that has occurred historically, i.e., the researcher can elicit WTP for environmental quality changes that may occur under future conditions (Phaneuf & Requate, 2016, p. 593). SP methods also allow the researcher to present different provision prices to different respondents and control the institutional setting in which the respondent makes decisions without relying on existing variation. RP methods require observational data and therefore face identification challenges such as dealing with confounders. In contrast to RP methods, SP methods do not require observing actual behavior and therefore face fewer identification challenges (Phaneuf & Requate, 2016, p. 573). Lastly, SP methods are also the only available approach to estimate non-use values where no market transactions are available (Johnston et al., 2017).

A key disadvantage of SP methods is that they rely on answers given in hypothetical scenarios. It is in part due to this hypothetical nature that debates about the validity of SP methods persist. Validity in this context is the extent to which an SP method provides unbiased estimates of the true underlying value under consideration (Phaneuf & Requate, 2016, p. 596). Kling et al. (2012) review the 20 years of SP research since the Exxon Valdez spill and discuss the four validity concepts that have since become standard in the SP literature. Criterion validity consider how SP estimates compare to other measures that are considered to be suitable proxies for the true underlying value (Kling et al., 2012). Construct validity is the extent to which predictions from SP studies are consistent with prior expectation such as those informed by economic theory and previous empirical studies (Johnston et al., 2017; Kling et al., 2012). Convergent validity is a special case of construct validity that refers to how well a SP estimate correlates with an RP estimate of the same economic value (Johnston et al., 2017; Kling et al., 2012). Content validity relates to the appropriateness of the methods in the design and implementation of the survey. It essentially depends on how well a SP study adheres to current best study design practices (Johnston et al., 2017; Kling et al., 2012).

Hypothetical bias is a key concern of stated preference studies. According to the meta-analysis on hypothetical bias by Penn and Hu (2018), the three primary techniques to deal with hypothetical bias – cheap talk, certainty follow-up questions, and consequentiality – all significantly mitigate the magnitude of hypothetical bias. Cheap talk is an *ex ante* mitigation technique that informs respondents of hypothetical bias and/or reminds them to answer the valuation question as if it were a real and binding purchase. Certainty follow-up questions are an *ex post* method that asks the respondent immediately following the valuation question about how confident they were in their previous response. Consequentiality includes payment consequentiality and policy consequentiality. Consequentiality can be applied *ex ante* through a consequentiality script and *ex post* by asking respondents about their consequentiality perceptions in follow-up questions (Penn & Hu, 2018). Penn and Hu (2019) use a meta-analysis to investigate the effectiveness of cheap talk scripts. They find that cheap talk is more effective in studies where hypothetical bias is more extensive and less effective in cases where hypothetical bias is smaller. They recommend using cheap talk with a budget/substitute reminder, which reminds the respondent that they have a limited budget, in conjunction with other hypothetical bias mitigation techniques, specifically in the context of a DCE about a public good (Penn & Hu, 2019).

Schläpfer and Fischhoff (2012) incorporate measures of task familiarity and context in a meta-analysis regression to predict the extent of hypothetical bias. They find that hypothetical bias is smaller when the tasks are familiar and the context is meaningful. They recommend conducting SP studies only when the good and the context can be made familiar and meaningful to respondents. Needham et al. (2018) test how providing information about the attributes of an environmental public good – new coastal (estuarine) wetlands – impacts respondents' knowledge and valuation of that good. They gauge respondents' *ex ante* knowledge about the public good's attributes, exogenously vary the amount of information about these attributes that is presented to respondents, and measure their post-valuation knowledge about these attributes. They find that giving respondents more information caused significant but incomplete learning. Learning additional information about the attributes does not significantly affect valuations, holding *ex ante* knowledge fixed, however. They find that *ex ante* levels of information do affect valuation: *ex ante* more knowledgeable respondents valued the good less than *ex ante* less knowledgeable respondents. Needham et al. (2018) also find evidence of fatigue: as respondents are given increasing amounts of new information, their marginal learning rates decrease.

Several other types of bias may be present in survey responses. Inattention bias occurs when respondents are inattentive and do not answer questions carefully. "Trap" questions – questions with an obvious answer that are designed to "trap" inattentive respondents into incorrectly answering – are often used in surveys to identify inattention bias in respondents. The convention in prior studies has been to drop inattentive respondents from the sample (Malone & Lusk, 2019). Malone and Lusk (2019) test the effects of trap questions and of providing inattentive respondents the opportunity to review their incorrect response. They find that respondents who miss trap questions and do not revise their responses when prompted have significantly different preferences compared to respondents who correctly answer trap questions. Inattentive respondents who revised their answers to trap questions were still different from respondents who responded correctly and also different from persistently inattentive respondents. Malone and Lusk (2019) suggest that the answers of "untrapped" participants are more consistent with a thoughtful response.

Another concern and task when designing a survey instrument is choosing the duration and timing of payments. Egan et al. (2015) make three arguments for matching the duration of a CVM survey's payments with the duration of the proposed benefits. Since environmental benefits are often long lasting, this often implies using an ongoing annual stream of payments instead of a lump sum payment. First, using ongoing annual payments spares survey respondents from performing complicated present value calculations. Second, the authors compare WTP estimates from CVM surveys with one-time and ongoing annual payments to WTP estimates from a travel cost analysis. They find that estimates from surveys with ongoing annual payments better match annual travel cost consumer surplus estimates (Egan et al., 2015). Third, they argue that large one-time payments may push the limits of respondents' mental accounting budget constraints more than if they were faced with ongoing annual payments.

An issue that is relevant in our survey that is not relevant in most CVM surveys is that of attribute non-attendance (ANA). ANA occurs when respondents ignore one or more attributes in a DCE. Our survey mimics a text DCE because we have two attributes – in addition to cost – that we vary across surveys. If ANA is present but not accounted for, welfare estimates will be biased (Lew & Whitehead, 2020). Lew and Whitehead (2020) review the ANA literature and find consistent evidence of some level of ANA behavior in a variety of DCE applications. There are two primary types of approaches used to deal with ANA: inferred models and stated models. Inferred ANA models use flexible econometric models to infer ANA behavior directly from the discrete choice data. Stated ANA approaches ask respondents follow-up questions in the survey about which attributes they ignored or considered when answering the choice experiment questions. This yields self-reported information on ANA behavior which can be used to condition the utility function. For example, in the "attribute elimination model" introduced by Hensher et al. (2005) each parameter is conditioned on whether a respondent stated they attended to that attribute or not. Specifically, Hensher et al. (2005) construct the probabilities so that the parameters associated with ignored attributes are set to zero in the likelihood function, which assumes that respondents have zero marginal utility for the attributes they ignore.

Hess and Hensher (2010) introduce the "ANA validation model" as a response to the attribute elimination model. They note that respondents who stated they ignored an attribute may have actually just placed a lower importance on it, which suggests they may actually have a non-zero marginal utility for this attribute. Thus, rather than setting the coefficients in the non-attending group to zero as in the attribute elimination model, they estimate separate coefficients in this group and the attending group. If respondents ignored the attributes they stated they ignored, the associated coefficients in the non-attending group should be zero (Hess & Hensher, 2010). They find that the coefficient estimates for the non-attending group are nonzero and statistically significant, contrary to the assumption of the attribute elimination model. They also find significant differences in the marginal utility coefficients between self-reported attending and non-attending respondents, with the coefficients associated with non-attending respondents generally having lower magnitudes (Hess & Hensher, 2010). Finally, they compare the ANA validation models with inferred ANA models that condition on inferred ignored attributes and find that the inferred models outperform the stated models in terms of model fit. Scarpa et al. (2013) also use the ANA validation model to test the validity of ANA statements in surveys. They reject a likelihood ratio test that imposed the null of identical parameters across the attending and non-attending groups, which lends support to the idea that ANA statements can be informative. They also compare how well stated ANA frequencies agree with ANA frequencies inferred using inferred ANA models. They obtain mixed results with none of the inferred models being a clear winner in terms of agreeing with respondents' ANA responses.

While stated ANA approaches have produced evidence of differences in behavior between self-reported attending and non-attending respondents, stated ANA approaches have several key drawbacks. First, collecting additional data on ANA increases survey length and cost (Scarpa et al., 2013). Second, question framing, respondent recall (and inattention), and potential strategic behavior may impact the accuracy and validity of self-reported ANA behavior (Lew & Whitehead, 2020; Scarpa et al., 2013). In fact, multiple studies have found that stated ANA information may not be a good indicator of actual ANA behavior (Lew & Whitehead, 2020).

Current best practices recommend using probability sampling methods like address-based mailing to identify respondents for the survey (Johnston et al., 2017). Probability sampling involves a random selection process such that each individual in the study population has a known nonzero probability of being selected into the sample (Champ, 2017). In contrast, in a non-probability sample not everyone in the population has a known and equal probability of being selected (Champ, 2017). In recent years, non-probability online samples have become a popular alternative to address-based mailing. Non-probability online samples are often composed of panels of individuals who have opted-in to take multiple surveys (Champ, 2017). The advantage of panel surveys is that the researcher is guaranteed a given number of completed surveys and only pays for those responses, often resulting in a lower cost per complete than address-based mailing (Champ, 2017). To enable generalizing the results of a panel survey to the population of interest, researchers often request the panel company uses quota sampling, in which a predetermined sample size of individuals is recruited to match the demographic distribution of the study population (Champ, 2017; Johnston et al., 2017). However, non-probability samples such as opt-in online panels may be subject to unknown selection biases that may not be corrected by balancing on demographic quotas (Johnston et al., 2017). Recent research by Sandstrom et al. (2021) comparing CVM valuation results from the opt-in online panels MTurk and Qualtrics to an address-based mailing sample has found statistically significant differences in parameter and WTP estimates.

3.4 Survey design and data

3.4.1 Survey design

The objective of our survey is to estimate Oregonians' WTP for coastal erosion management policies that impact safe recreation access. Since these coastal management plans would impact both users and non-users of Oregon's developed beaches, one key aim of this research is to be able to decompose survey respondents' WTP for coastal management into use and non-use values using the combined RP/SP framework of Eom and Larson (2006), Huang et al. (2016), and Landry et al. (2020). Thus, the survey accounts for users and non-users of developed Oregon Coast beaches by collecting both RP data on recreation trip counts and SP data using a DBDC CVM format. The latter CVM data is used in this chapter to estimate SBDC and DBDC models and measure the total value (WTP) for coastal erosion management. Our eventual goal is to use the data to jointly estimate the demand for beach recreation trips and the WTP for coastal erosion management using the aforementioned combined RP/SP framework.

Another aim of this research is to investigate demographic and response differences between a probability sample and a non-probability sample. Specifically, our goal is to compare the results of an address-based sample – a traditionally preferred but cost prohibitive probability sample – with an opt-in online panel sample from Qualtrics – a less costly non-probability sampling alternative that may be subject to potential selection biases. In this chapter we describe how the two samples were identified and recruited but report only the results from the Qualtrics panel sample.[61]

The sample frame for the Qualtrics panel sample is the set of Oregon residents that Qualtrics has recruited to take online surveys. Participants were identified, recruited, and compensated by Qualtrics to participate in our survey. One advantage of the quota-based Qualtrics panel sample is that we were guaranteed a specific number of high-quality completed surveys from respondents with demographics matching Oregon's general population. We purchased 1,800 responses meeting quotas that match the gender and age percentages of the 2016-2020 American Community Survey's 5-year estimates for Oregon (U.S. Census Bureau, 2020). Since our survey is similar to a text DCE in that it has multiple attributes, the (minimum) sample size was determined using Orme's Rule of Thumb for DCEs (de Bekker-Grob et al., 2015).

The sample frame for the address-based sample is all Oregon residents with home addresses. In our addressed-based sample, residents of Oregon were identified through a

[61] At the time this chapter was written, the Qualtrics panel sample data collection was nearing completion whereas the address-based sample data collection was still in progress.

randomized address list obtained from Dynata, a market research firm, by OSU's Survey Research Center. The number and type of contacts was chosen according to Dillman's (2007) guidelines for mail surveys.[62] Participants were contacted four times during the recruitment process. We purchased address records with names to enable us to personalize these contacts and establish trust (Dillman, 2007, p. 20). Personalizing mailings has also been found to increase response rates (Dillman, 2007, p. 152). First, a pre-letter describing the survey and the next contact was sent via the US Postal Service to 11,000 households who reside in the state of Oregon.[63] In the following week, a letter from OSU's Survey Research Center with information about how to find the online survey (URL) and a personal access code was mailed to all in the list. A postcard follow-up was mailed approximately one week later. A final letter to encourage completion was mailed two weeks later to those that had not yet responded to the survey.

In both sample frames the survey itself was administered online through Qualtrics. The only difference between survey versions for the address-based sample and the Qualtrics panel sample was the inclusion of three quota-related questions at the beginning of the survey in the Qualtrics panel version. In the first question of the Qualtrics panel survey version, respondents were asked to provide their ZIP code to verify they are located in Oregon. The next two questions in the Qualtrics panel version were the age and gender demographic questions that were moved from the demographics section of the survey to enable specifying age and gender quotas for that sample. The next question – and the first question in the address-based survey version – introduces the survey, provides information about confidentiality, and asks the respondent to provide consent before proceeding. The rest of the survey is identical between sample versions.

Two attributes are varied across respondents in addition to the presented bid. The first attribute is the shoreline armoring scenario that each respondent is shown. We use a split-sample or "between" experimental design by assigning half of the sample to a scenario where the Goal 18 shoreline armoring policy is relaxed to allow more armoring (Relax) and the other half of the sample to a scenario where the Goal 18 policy remains as is (Maintain). Given coastal landowners' interest in relaxing the current shoreline armoring policy, this sample split allows us to estimate

[62] The updated edition of these guidelines allows for more flexible contact mailing strategies (Dillman et al., 2014, p. 373).
[63] The OSU's Survey Research Center's prior mail survey experience in Oregon suggested an undeliverable rate of 11-12%. Our goal was successfully reaching 10,000 Oregon households so we chose to mail approximately 11,000 households.

the economic value of relaxing the current shoreline armoring policy to all Oregonians. The second attribute that we vary is the change in safe recreation access that is presented in the CVM question. We define our measure of safe recreation access as the number of daylight hours per day that people can safely access the beach and engage in recreation activities, referred to in the survey as “safe hours.” The “safe hours” measure was developed with help from coastal scientists. See section 3.4.2 for a more detailed definition and data sources of “safe hours.”

The survey instrument was designed according to the best practices outlined in Johnston et al. (2017). See Appendix 6.8 for the Qualtrics panel version of the survey instrument. The order of the survey is as follows. We begin the survey with a brief explanation about the Beach Bill to remind respondents that they have a permanent easement to access and recreate on all beaches in the state. Aside from this information, this page also includes a photograph and an easy, non-threatening question about their familiarity with the Beach Bill. Throughout the survey, we break up large sections of text with relevant questions about the provided information as suggested in Champ (2017). These auxiliary “attention-check” and familiarity questions are used to both partition the flow of long sections of text into pages and engage respondents as they read presented information. Content is separated into pages to facilitate taking the survey on mobile devices by minimizing scrolling. To further break up blocks of text and engage respondents, we include colored photographs or maps on most pages. Photograph presentation has been found to influence valuation of environmental goods (Labao et al., 2008). Labao et al. (2008) compare how colored versus black and white photographs affect respondents’ WTP for an enhanced Philippine Eagle Conservation program. They find that WTP values for surveys using colored photographs were significantly higher than WTP values for surveys using black and white photographs. Labao et al. (2008) argue that the colored photographs may have improved respondents’ ability to digest and understand information.

We then introduce the first part of the survey, which collects RP data on the respondent’s recreation trips to developed Oregon beaches from April 2021 to March 2022. The survey was fielded starting in April 2022 so this time span represents the most recent 12-month period in which respondents could have recreated. We split the RP questions into two sections. In the first section we ask respondents about day trips they have taken to developed Oregon Coast beaches for outdoor recreation. In the second section we ask them about their short overnight trips (defined as 3 nights or less). Collecting RP data on only day trips would implicitly exclude residents of the eastern part of the state who, due to their distance from the Oregon Coast, do not make day trips to the beach. Thus, we ask about both day trips and short overnight trips to capture beach recreation behavior for all Oregonians. Data on day trips and short overnight trips can be combined as in Parsons et al. (2013). Parsons et al. (2013) collect day trip and short overnight trip (also defined as 3 nights or less) data on beach use in Delaware to value changes in beach width on a per-trip basis. They combine day trip and short overnight trip data by classifying each respondent as taking either primarily day trips or primarily short overnight trips and then aggregate their trips, e.g., a person who took six day trips and one short overnight trip would be classified as a day tripper taking seven trips.

Before asking any recreation trip questions, we first define the difference between a “developed” and an “undeveloped” beach. These descriptions are accompanied by side-by-side photos of a developed and an undeveloped Oregon beach. Both day and short overnight RP sections also ask about auxiliary recreation behavior, e.g., how many people they typically go to the beach with and how many hours (for day trips) or nights (for short overnight trips) they typically spend at the beach. This auxiliary information can be incorporated into the recreation demand model of the combined RP/SP framework.

In both RP sections we show respondents a map of the Oregon Coast labeled with 16 developed beaches and ask them if they took a recreation trip to a developed Oregon Coast beach in the previous 12 months. These 16 beaches were chosen with expert feedback because they are the most popular developed beaches on the Oregon Coast (C. Plybon, personal communication, January 13, 2021). Respondents are then presented with a list of these 16 developed beaches and are asked to select all of the beaches they visited in the previous 12 months or answer with “Not sure.” Answering “Not sure” after answering that they did take recreation trips suggests that the respondent is a recreator but is “forgetful” about where they recreated. Collecting this information allows us to identify “forgetful recreators” and compare their responses to those of non-recreators and non-forgetful recreators. These “forgetful” respondents are asked a follow-up question about how many total trips they took to developed beaches in the previous 12 months before exiting the

RP section. This question is used to verify that they did in fact take recreation trips to developed Oregon Coast beaches.

Respondents that selected at least one of the 16 developed beaches are asked, for each beach that was selected, how many trips they took to that beach in each of the four seasons between April 2021 and March 2022. We chose to ask about seasonal trip behavior because both recreation behavior and daylight safe hours – our safe recreation access measure – vary by season. Doing so effectively provides us with a panel dataset of trip counts but increases the recall burden for respondents. To ease recall for frequent visitors we provide trip counts or ranges as well as frequency estimates in parentheses, e.g., “6-8 trips (about every other week)” and “24-40 trips (two to three times a week).” Respondents are also shown a zoomed-in map of the location of that beach on the coast to help with recall. We defined each season as a 3-month period with Spring starting in April 2021, the first month we collect RP data for.

If a person responds “Not sure” to having taken trips in any season for a given beach, they are then asked a follow-up question about how many *total* trips they have taken to that beach in the previous 12 months. The total number of trips taken can be used to verify their answers to the per-season questions for that beach since it should be at least as large as the sum of their seasonal trip counts. This question may also help respondents recall how many trips they’ve taken to that beach in a particular season. Respondents are allowed to go back to previous questions and modify their answers to the per-season questions. Respondents who answer “Not sure” to the per-season questions are “forgetful” about *when* they recreated and can therefore be identified as “forgetful recreators” like respondents who are forgetful about *where* they recreated. These respondents are allowed to also answer “Not sure” to the “Not sure” follow-up question about the total number of trips taken to that beach. If they do so, they are then asked a second follow-up question to verify that they did in fact recreate on that beach in the previous 12 months.

The multiple “Not sure” follow-up questions in the RP sections allow us to verify respondents’ trip counts, ascertain whether they recreated when/where they stated they did, and label them as “forgetful” to enable comparisons with non-recreators and non-forgetful recreators. Allowing respondents to answer with “Not sure” to the RP questions also increases RP data accuracy since respondents are not forced to provide a number if they are not certain about how many trips they took. After the two RP sections respondents are asked several more auxiliary recreation behavior questions including whether they visit nearby undeveloped beaches on a typical trip to a developed beach and what type of beach they prefer to visit. This information can help us gauge the substitutability of developed beaches for undeveloped beaches in Oregon.

The second part of the survey provides the necessary background information before we can present the hypothetical coastal management plan and valuation questions. Large sections of text are again broken up with colored photographs and an "attention-check" question on the bottom of each page. We first tell respondents that recreation opportunities on developed Oregon Coast beaches may be impacted by erosion. We then describe safety hazards that they may encounter while recreating on Oregon Coast beaches and define our "safe hours" measure. We tell respondents that safety hazards impact the number of "safe hours" and describe (using text and photos) how the same beach would look like at a "safe" hour versus an "unsafe" hour. Next we state that erosion on developed beaches is likely to increase with rising sea levels in the future, which will potentially decrease beach safe hours, reduce safely accessible beach areas, and increase the risks of safety hazards. We then provide background information about the policy setting by describing shoreline armoring, Goal 18, and sediment management. We balance the amount of background information and survey length. Following the recommendation by Schläpfer and Fischhoff (2012), we attempt to include enough information about the good and the context to make both familiar and meaningful to respondents so that hypothetical bias is reduced. However, we also attempt to keep this information concise so as to avoid fatigue, as was found to be an issue by Needham et al. (2018).

The third part of the survey collects SP data on the respondent's WTP for the hypothetical coastal management plan. We begin the SP section by clarifying what we are asking the respondents to choose between. We tell them that the State of Oregon is considering future coastal management policies and show them *two* options. The first option would increase their household's annual state income taxes to implement a new coastal management plan, which is then described. The second option is to do nothing: there would be no new coastal management plan and no increase to their household's annual state income taxes. The proposed coastal management plan in the first option does two things. First, it creates an "Oregon Public Beach Fund" to manage sediment on eroding developed beaches. We tell respondents that this fund would be overseen by Oregon State Parks and used to address erosion and preserve access and safe hours for recreation. Since all beaches in Oregon are public and managed by Oregon State Parks (under the Oregon Parks and Recreation Department), tasking them with implementing the policy helps improve policy consequentiality. The second part of the proposed coastal management plan depends on whether the respondent was randomly sorted into the "Relax" or "Maintain" Goal 18 sample. Respondents in "Relax" are told that the proposed plan will relax armoring restrictions under Goal 18 to address erosion issues on oceanfront parcels, meaning that all oceanfront homeowners would become eligible to install armoring when their property becomes vulnerable to erosion. Respondents in "Maintain" are told that the proposed plan will maintain current armoring restrictions under Goal 18 and that the amount of properties eligible for armoring would not change.

We ask "verification check" questions about shoreline armoring at two points in the survey. Responses to these two questions can be used to "verify" that the respondent's vote in the CVM question is consistent with their beliefs about shoreline armoring. Specifically, their beliefs about shoreline armoring should help predict whether they vote "yes" for the policy, depending on which shoreline armoring scenario they are shown. The first question follows the description of Goal 18: "Do you believe it is likely that Goal 18's armoring policy will be maintained in its current form for the foreseeable future?" The second question follows the two proposed policy options: "How much do you agree or disagree with the following statement? All properties that are vulnerable to erosion should be able to install shoreline armoring."

After the policy description we include a consequentiality statement and cheap talk with a budget reminder (Penn & Hu, 2019). To mimic the probability that the proposed plan is implemented is weakly monotonically increasing with the proportion of yes votes (Vossler et al., 2012), we include the last sentence in the following consequentiality statement: "Your opinion matters. You will be asked to vote on a new proposed coastal management plan. Your vote may help inform the State of Oregon about what plan to put on a state ballot measure in an upcoming election. The plan would be implemented if chosen by a majority of Oregon voters." We then present the initial CVM question. We ask respondents to vote on a state ballot measure that would increase their household's annual state income taxes per year for the next 30 years to implement the coastal management plan and re-iterate what this ballot initiative would do. Oregon voters are accustomed to voting on state ballot measures that increase state income taxes. Thus, framing our referendum question as a state ballot measure promotes policy consequentiality and using state income taxes as the payment vehicle promotes payment consequentiality. Ongoing annual payments are used instead of a lump sum payments as recommended by Egan et al. (2015).

The environmental quality change that is presented in the CVM question is the percent loss of safe hours that is prevented by the proposed coastal management plan. For example, a respondent may be told that the ballot initiative would: "Increase funding for sediment management to prevent a 20 % loss of safe hours for recreation at developed beaches at the highest risk of erosion." The range of values shown to respondents for the prevented percent loss of safe hours was 10%, 20%, 30%, and 40%. This range is based on the projected percent loss of safe hours for the Oregon Coast in 2050 (see section 3.4.2 for details on how these values were calculated). The stream of payments was chosen to be 30 years to match the stream of benefits from the sediment management policy that would prevent the specified percent loss of safe hours in 2050. Bid levels were informed by recent environmental surveys in Oregon (Lewis et al., 2019; Nguyen et al., 2022) and developed for this survey based on a range of realistic estimates tested in focus groups and pre-test surveys. The nine bids were \$10, \$25, \$50, \$75, \$100, \$150, \$200, \$300, and \$400. The environmental quality change was framed as a "prevented" percent loss of safe hours because we found this to be a more realistic outcome of a sediment management strategy compared to an "increase in safe hours," especially in the scenario where Goal 18 is relaxed and subsequent increased armoring leads to narrower beaches. We use a percent change in safe hours instead of a level change because the number (level) of daylight safe hours varies across developed Oregon Coast beaches, e.g., a 30% loss of safe hours over the next 30 years could represent a loss of two (2) safe hours on one beach but on another beach may represent a loss of four (4) safe hours.

After voting "yes" or "no" on the initial CVM question, respondents are asked a certainty follow-up question, i.e., how certain they are of their vote. Following this, respondents are asked the follow-up (DBDC) CVM question with a different bid and another certainty-follow up question about their vote on the second CVM question. We attempted to minimize hypothetical bias by using all three primary techniques – cheap talk, certainty follow-up questions, and consequentiality – that were found by Penn and Hu (2018) to significantly mitigate the magnitude of hypothetical bias.

The survey includes a number of follow-up questions after the valuation questions that are designed to measure the extent of response anomalies and to enhance the validity of the study, as best practices recommend (Johnston et al., 2017). Behavioral or "response" anomalies occur when respondents do not reveal their true preferences (Johnston et al., 2017). We ask debriefing questions to identify response anomalies and other motivations for value elicitation responses. Examples of response anomalies we attempt to identify include scenario rejection and protest responses. Scenario rejection occurs when respondents do not believe something about the scenario and choose the status quo alternative or refuse to make any choice at all. For example, a respondent may vote "no" on the ballot initiative because they "do not believe that a policy to preserve safe hours would be paired with relaxing Goal 18 in a state ballot measure." Protest responses occur when respondents state a zero valuation for a good (i.e., vote "no") even though their true valuation is greater than zero, e.g., because they "do not trust the Oregon State government to protect Oregon beaches" or they "do not believe it is the state government's responsibility to fund a coastal management plan." In addition to capturing possible reasons for scenario rejection and protest responses, the debriefing section also includes questions to capture why respondents may have voted "no" while revealing their preferences truthfully. The debriefing section also collects information on attitudes and demographics to allow the assessment of content and construct validity, i.e., whether demand differs in expected ways across different groups of people.

We include an ANA question in the debriefing section to collect stated ANA information. This question asks respondents to select how important each factor was in influencing their vote and shows them the three ballot initiative outcomes whose attributes vary between survey respondents: increasing funding for sediment management to prevent a [10, 20, 30, 40] % loss of safe hours, [relaxing, maintaining] Oregon's Goal 18 shoreline armoring policy, and the increase to their household's annual state income taxes per year for the next 30 years. Respondents are presented with a 5-point Likert scale including very important (1), moderately important (2), neutral (3), slightly important (4), and not at all important (5) as well as a not sure (6) option. We then ask all respondents who voted “no” on at least one CVM question the extent to which they agree or disagree (on a 5-point Likert scale) with a series of statements describing potential reasons why they may have voted “no.”

All respondents are then asked the following to question to gauge how consequential they found the survey to be: “How much do you agree or disagree with the following statement? The results of this survey will influence Oregon state agencies and policymakers as they make their decisions about future coastal management plans for developed beaches.” We then ask all respondents the extent to which they agree or disagree (on a 5-point Likert scale) with a series of statements meant to identify potential response anomalies. This list also includes a “trap” question designed to identify inattentive respondents and potential bots. This question asks respondents to “Please select Strongly Disagree here.” Attitudinal questions about erosion risk, sea level rise, and climate change are asked after these de-briefing questions. Dillman et al. (2014, p. 231) recommend placing sensitive or potentially objectionable questions near the end of the survey after the respondent has had the opportunity to become engaged with the survey and has established trust with the surveyor.

Demographic questions are placed at the end of the survey, following standard survey practice (Champ, 2017), to enable adjusting for systematic sample selection. Systematic sample selection is not a primary concern in the Qualtrics panel sample because Qualtrics was able to specify respondent age and gender quotas to match the demographic distribution of Oregon residents. Thus, since this chapter’s focus is analyzing only the Qualtrics panel sample data, we do not use a sample selection correction method here.[64] In addition to standard demographic questions about education, employment, etc. we also identify respondents who own property on the coast. Respondents are asked whether their primary residence is a coastal property (within one mile of the coast) and whether they own a second home that is a coastal property. We also ask all respondents if they moved between April 2021 and March 2022 and, if yes, when and to which ZIP code they moved. Moving information will allow us to calculate more accurate travel costs in the recreation demand model of the combined RP/SP framework. The final section of the survey

[64] Analysis of the address-based sample will require systematic sample selection adjustments, as in Cameron and DeShazo (2013).

includes questions about engagement and social media use.[65] Respondents are asked if and how they have engaged with coastal advocacy groups like the Surfrider Foundation or the Oregon Shores Conservation Coalition. Respondents are also asked whether they post to social media about their trips to the Oregon Coast. The final question is a comment box that allows respondents leave the research team comments about anything we overlooked. This question can also be used to identify likely bot responses.

Prior to implementing the survey, we conducted three online focus groups and a pre-test. The pre-testing of SP surveys before final implementation is a central component of content validity (Johnston et al., 2017). Focus group participants were recruited and compensated by InsightsNow, a Corvallis-based behavioral research firm, using a screener questionnaire provided by the research team. We conducted three online sessions with four to five participants in each session. We reviewed all sections of the survey except for the debriefing questions. The "safe hours" and "prevented percent loss of safe hours" measures were discussed extensively in focus groups and revised until we received positive feedback that they were clear and easy to understand. We also focused discussions on other key background information such as the developed beach definition, the policy description, and the CVM question. A pre-test was conducted using the first 180 respondents (10%) of the Qualtrics panel sample, in increments of 30 respondents. Pre-test responses were analyzed every 30-respondent increment so that multiple survey adjustments could be made, if needed.

3.4.2 Data

The "safe hours" and "prevented percent loss of safe hours" measures were defined prior to survey implementation as follows. Daylight hours were identified using data on sunrise and sunset times for Newport, OR for each day of the year (Global Monitoring Laboratory, n.d.). Newport is located at approximately the midpoint between the northernmost and southernmost latitudes of Oregon and thus its sunrise and sunset times represent average times for the Oregon Coast. Newport is also one of the developed beach trip locations we ask about in the survey. Daylight hours are designated as "safe" if a minimum beach width is met during daylight hours. Beach width is defined as the

[65] This data is not analyzed here.

distance between total water level (TWL) elevation (i.e., the maximum elevation that water reaches on the shore) and back shore feature elevation (e.g., the elevation of the dune or cliff toe).[66] Beach width is calculated at multiple points for each beach to ensure a 1 km resolution between the north and south endpoints of a given beach. We use three different minimum beach width measures – 10 m, 15 m, and 20 m – in an attempt to capture different perceptions about what constitutes as "safe" to access and recreate on. The number of daylight safe hours are counted for each day of the year, for each of the three minimum beach width definitions, and at each of the 16 developed beaches. This number is then aggregated by season to get the present-day seasonal average of safe hours at each location for each minimum beach width definition.

To determine the plausible range of values for the percent loss of safe hours that is prevented by the proposed coastal management plan, we also calculate seasonal average safe hours 30 years into the future. Seasonal average safe hours are calculated for the year 2050 using three different sea level rise scenarios corresponding to 0.5 m, 1.0 m, and 1.5 m of sea level rise by 2100. These present day and projected seasonal average safe hours are used to determine the percent change (loss) in safe hours:

$$pctchange = 100 * \left(\frac{safehours_{present} - safehours_{future}}{safehours_{present}}\right) \tag{1}$$

The Qualtrics panel sample used in this analysis contained 1,831 valid responses. Qualtrics collected 3,324 total responses for the online panel but labeled 1,493 of them as invalid for a variety of reasons. For example, 180 respondents were labeled as "speeders." These respondents completed the survey in less than the median time it took non-recreators to complete. The most frequent reason a response was labeled as invalid is if the respondent failed the "trap" question "Please select Strongly Disagree here." A total of 734 respondents did not answer this question correctly. We follow prior convention and drop these inattentive respondents from the sample (Malone & Lusk, 2019). It is also possible that some of these "speeders" and "inattentive

[66] TWLs were calculated for the historical period at the county scale using deep water wave data (wave height, period, and direction) from GOW2 wave hindcast nodes (Perez et al., 2017) and water level data from NOAA tide gauges at Port Orford, South Beach, Garibaldi, and Astoria. Wave and water level data were input into nearshore surrogate models (SWAN lookup tables combined with empirical Stockdon run up formulas) following the methodology presented in Serafin et al. (2019) to extract TWLs at 1000m resolution. This process was repeated for each site after adding three regional SLR projections for 2050 representing intermediate likelihoods for low, moderate, and high global mean SLR scenarios to the water level inputs (Sweet et al., 2017).

respondents" were actually bots. As an additional quality check, we reviewed all comments left in the comment box in the final question and removed likely bot responses, e.g., nonsensical responses and copy-pasted question text from the survey instrument.

Table 3.1 reports the descriptive statistics of this sample. Demographic explanatory variables include household income, age, household size, the number of children in the household, and a series of binary indicator variables. Approximately 51% of the sample is female, 86% of the sample is white, and average respondent age is 48 years old. The midpoint household income is $58,000 with 32% of the sample employed full-time, 21% retired, and 3% full-time students. Approximately 21% of the sample has a high school diploma (only) and 30% of the sample has a bachelor's degree or higher. The average household size is 2.64 and 28% of respondents report being single. Approximately 40% of the sample identifies as extremely, moderately, or slightly liberal. Coastal residents – respondents whose primary residence is within one mile of the coast – comprise 6.8% of the sample and another 2.1% of the sample owns a coastal second home. The only RP data we use in this chapter is whether the respondent recreated on a developed Oregon Coast beach between April 2021 and March 2022. Any respondent who stated they took a day trip and/or a short overnight trip was labeled as a "recreator," which was approximately 74% of respondents. Selected debriefing question responses are also included in Table 3.1. Approximately 64% of respondents agreed with the consequentiality statement "The results of this survey will influence Oregon state agencies and policymakers as they make their decisions about future coastal management plans for developed beaches." Less than 11% of respondents disagreed with the consequentiality statement (the remaining respondents were either neutral or not sure). Respondents generally believed that the risk of erosion 30 years from now will be greater (83% agreed) and that Oregon's climate is changing (90% agreed).

Figures 3.1 and 3.2 plot response frequencies for the two "verification check" questions. Figure 3.1 plots responses for the "Do you believe it is likely that Goal 18's armoring policy will be maintained in its current form for the foreseeable future?" question. The most frequent response was "Somewhat likely" followed by "Neither likely nor unlikely," suggesting that, while most respondents believed Goal 18 would likely continue as is, many respondents were either not sure about or indifferent to Goal 18's future. Responses to this question were used to create

$g18\_future\_likely$, a variable indicating that the respondent thought it was very likely or somewhat likely that Goal 18 will continue as is. Figure 3.2 plots responses for the “How much do you agree or disagree with the following statement? All properties that are vulnerable to erosion should be able to install shoreline armoring.” question. The most frequent response was “Somewhat agree” followed by “Strongly agree,” suggesting that many Oregonians believe that vulnerable properties should be allowed to armor. Responses to this question were used to create $armoring\_allow\_agree$, a variable indicating that the respondent either strongly agrees or somewhat agrees with the aforementioned statement.

We check that voting behavior confirms to prior expectations by plotting initial CVM question bid occurrences versus follow-up CVM question bid occurrences for respondents who changed their votes from “yes” to “no” (and vice versa). Figure 3.3(a) plots the count of bid occurrences for the initial and follow-up CVM question for respondents who voted “yes” on the initial question then “no” on the follow-up question. We expect that, for respondents who initially voted “yes,” the probability of a “no” response to the follow-up bid should increase monotonically as the bid increases. Figure 3.3(a) shows that bid occurrences increase as bids increase for respondents voting yes-no, as we would expect. Figure 3.3(b) plots the count of bid occurrences for the initial and follow-up CVM question for respondents who voted “no” on the initial question then “yes” on the follow-up question. We expect that, for respondents who initially voted “no,” the probability of a “yes” response to the follow-up bid should decrease monotonically as the bid increases. Figure 3.3(b) shows that bid occurrences decrease as bids increase for respondents voting no-yes, as we would expect.

In this chapter we focus on one response anomaly: ANA. Table 3.1 presents the different stated ANA variable definitions created from responses to the ANA question in the debriefing section as well as the proportion of respondents that stated they did not attend to each attribute’s outcome under each definition. We created a binary “ANA likely” indicator for each attribute that is equal to 1 if the respondent likely did not attend to that attribute’s outcome. We specify two definitions of this indicator, following prior concerns about differentiating between attribute non-attendance and attribute indifference (Hess & Hensher, 2010). The first definition is conservative and assumes that respondents attended to the attribute only if they answered that its outcome was "very important" or "moderately important" in influencing their vote. If a respondent chose neutral, slightly important, not at all important, or not sure, we assume that they likely did not attend to that attribute. The three ANA variables with this strict definition are ana_likely_cost, $ana\_likely\_safehours$, and ana_likely_goal18. The second definition differentiates between attention and indifference. Respondents who assigned "neutral" importance are believed to attending to but indifferent to that attribute. Similarly, respondents who answered the outcome was "slightly important" are believed to be attending but largely indifferent to that attribute. Thus, in this alternative and less strict definition, we assume that the respondent likely did not attend to that attribute only if they chose not at all important or not sure. The three ANA variables with this less strict definition are $ana\_likely\_cost\_3$, $ana\_likely\_safehours\_3$, and $ana\_likely\_goal18\_3$.

Figure 3.4 plots the response frequencies for the ANA question for the three ballot initiative outcomes by the shoreline armoring policy scenario that the respondent was assigned to (Maintain or Relax). We expect *a priori* that stated ANA response frequencies do not differ between the "Relax" and "Maintain" shoreline armoring scenarios for the cost outcome. However, since the proposed coastal management plan in the ballot initiative is composed of two parts – the Oregon Public Beach Fund and the Goal 18 policy change to relax/maintain shoreline armoring restrictions – we would expect the stated ANA response frequencies for the safe hours outcome and the Goal 18 outcome to differ between the "Relax" and "Maintain" shoreline armoring scenarios. Figure 3.4(a) plots response frequencies for the cost outcome, i.e., "the increase to your household's annual state income taxes per year for the next 30 years." As expected, the distribution of responses is similar between the "Relax" and "Maintain" scenarios suggesting that respondents in both scenarios placed similar importance on the bid presented to them. Figure 3.4(b) plots response frequencies for the safe hours outcome, i.e., "increasing funding for sediment management to prevent a [10, 20, 30, 40] % loss of safe hours for recreation at developed beaches at the highest risk of erosion." Our *a priori* expectation is that respondents would place higher importance on the safe hours outcome in the "Maintain" scenario because safe hours is the only attribute that changes in that scenario (since Goal 18 is maintained as is). However, the distribution of responses is similar between the "Relax" and "Maintain" scenarios, which suggests that respondents in both scenarios placed similar importance on the safe hours outcome. Figure 3.4(c) plots response frequencies for the Goal 18 outcome, i.e., "[relaxing, maintaining] Oregon's Goal 18 shoreline armoring policy so that all oceanfront property owners become eligible to armor the shoreline in front of their homes." Our *a priori* expectation is that respondents would place higher importance on the Goal 18 outcome in the "Relax" scenario because the Goal 18 policy is changed in this scenario but unchanged in the "Maintain" scenario. As expected, more respondents said the Goal 18 outcome was "very important" in the "Relax" scenario compared to the "Maintain" scenario. More respondents said they were "not sure" how the Goal 18 outcome influenced their vote in the "Maintain" scenario, which is what we would expect since the Goal 18 outcome does not actually change in this scenario.

3.5 Methodology

3.5.1 Contingent valuation method

To estimate Oregonians' WTP for coastal erosion management policies that affect safe access on developed Oregon beaches, we use both a single-bounded dichotomous choice (SBDC) and a double-bounded dichotomous choice (DBDC) CVM format in the survey. Due to the follow-up question's incentive compatibility problem in DBDC formats (Carson & Groves, 2007; Phaneuf & Requate, 2016, p. 594), the DBDC models are used for robustness rather than for primary inference.

Hanemann (1984) formally worked out the utility function approach for analyzing dichotomous choice data from a random utility maximization (RUM) perspective. The utility approach assumes that the dichotomous choice represents a comparison between two utility levels. In the RUM model, the utility function consists of a component that is observable to the researcher and a component that is unknown to the researcher but known to the decision-maker. This assumes that the utility function is additively separable in deterministic and stochastic preferences. In the dichotomous choice CVM context, let the subscript $j = 1$ denote the counterfactual scenario and subscript $j = 0$ denote the status quo. Then, the utility available to person i from the two alternatives $j = 1, 0$ is:

$$V_{i1}(y_i - B_i, q_1, \boldsymbol{Z_i}, \varepsilon_{i1}) = v_{i1}(y_i - B_i, q_1, \boldsymbol{Z_i}) - \varepsilon_{i1} \tag{2}$$

$$V_{i0}(y_i, q_0, \boldsymbol{Z_i}, \varepsilon_{i0}) = v_{i0}(y_i, q_0, \boldsymbol{Z_i}) - \varepsilon_{i0} \tag{3}$$

where B_i is the bid amount presented to person i in the survey, y_i is their income, and q_j is a measure of safe hours. We specify the $\boldsymbol{Z_i}$ vector to contain the shoreline armoring scenario the respondent was randomly shown ("Relax" or "Maintain"), individual or household characteristics other than income (e.g., age), and the respondent's answers to familiarity and preference questions in the survey (e.g., familiarity with beach nourishment). Here $v_{ij}(.)$ is a parametric specification for the observable component of utility and ε_{ij} is a random variable with known distribution (e.g., normal or extreme value) that represents the unobservable component of utility. It is assumed the respondent will answer "yes" to the dichotomous choice if and only if $V_{i1}(.) \geq V_{i0}(.)$. The condition for person i giving a "yes" answer can be expressed as a utility difference:

$$v_{i1}(y_i - B_i, q_1, \boldsymbol{Z_i}) - \varepsilon_{i1} \geq v_{i0}(y_i, q_0, \boldsymbol{Z_i}) - \varepsilon_{i0} \tag{4}$$

$$\varepsilon_{i0} - \varepsilon_{i1} \leq v_{i1}(y_i - B_i, q_1, \boldsymbol{Z_i}) - v_{i0}(y_i, q_0, \boldsymbol{Z_i}) \tag{5}$$

$$\varepsilon_i \leq v_{i1}(y_i - B_i, q_1, \boldsymbol{Z_i}) - v_{i0}(y_i, q_0, \boldsymbol{Z_i}) \tag{6}$$

where $\varepsilon_i = \varepsilon_{i0} - \varepsilon_{i1}$ is symmetric with a mean of zero.[67] Thus, the probability of observing a "yes" answer is:

$$\Pr(yes_i) = \Pr[\varepsilon_i \leq v_{i1}(.) - v_{i0}(.)] \tag{7}$$

After a functional form is chosen for $v_{ij}(.)$, estimation of the included parameters can proceed via maximum likelihood. A common approach is to assume a linear specification for $v_{ij}(.)$:

$$v_{i1} = \alpha_1 + \beta_1(y_i - B_i) + \gamma_1 q_1 + \boldsymbol{Z_i'\eta_1} \tag{8}$$

$$v_{i0} = \alpha_0 + \beta_0 y_i + \gamma_0 q_0 + \boldsymbol{Z_i'\eta_0} \tag{9}$$

where $\boldsymbol{Z_i'\eta_j} = \sum_{k=1}^{m} \eta_{jk} Z_{ik}$ (for a set of m variables) and the parameters to be estimated are $\alpha, \beta, \gamma, \boldsymbol{\eta}$. A common assumption is that the marginal utility of income is constant between the alternative states so that $\beta_1 = \beta_0$ and the utility difference becomes:

$$v_{i1}(.) - v_{i0}(.) = \boldsymbol{Z_i'\eta} + \gamma\Delta q - \beta B_i \tag{10}$$

where $\boldsymbol{\eta} = \boldsymbol{\eta_1} - \boldsymbol{\eta_0}$, $\gamma = \gamma_1 - \gamma_0$, and $\Delta q = q_1 - q_0$. Then the probability of responding "yes" becomes:

[67] The random terms can be expressed as a single term ε_i since differences in the random components between the status quo and counterfactual scenario cannot be identified (Haab & McConnell, 2002, p. 26).

$$\Pr(yes_i) = \Pr[\varepsilon_i \leq \boldsymbol{Z_i'\eta} + \gamma\Delta q - \beta B_i] \tag{11}$$

If we suppose that ε_{i0} and ε_{i1} are each independent normal, then ε_i is normally distributed: $\varepsilon_i \sim N(0, \sigma^2)$. This can be converted to a standard normal variable by letting $\theta = {}^{\varepsilon}/_{\sigma}$. Then, $\theta \sim N(0,1)$ and:

$$\Pr[\varepsilon_i \leq \boldsymbol{Z_i'\eta} + \gamma\Delta q - \beta B_i] = \Pr\left[\theta \leq \frac{\boldsymbol{Z_i'\eta} + \gamma\Delta q - \beta B_i}{\sigma}\right] = \Phi\left(\frac{\boldsymbol{Z_i'\eta} + \gamma\Delta q - \beta B_i}{\sigma}\right) \tag{12}$$

where $\Phi(.)$ is the cumulative standard normal distribution. This is the binary probit model (adapted from Haab and McConnell (2002) and Phaneuf and Requate (2016)). Note that the parameters are divided by an unknown variance, meaning that the parameters can only be estimated up to a scalar multiple.

The likelihood function is constructed by matching the "yes" and "no" answers to their probability expressions, for a given distribution of the unobserved component of utility. Suppose the sample size is N and let $I_i = 1$ if respondent i answers "yes". Then the likelihood function is:

$$L(\boldsymbol{\eta}, \gamma, \beta | \boldsymbol{y}, \boldsymbol{Z}, \boldsymbol{B}) = \prod_{i=1}^{N} \left[\Phi\left(\frac{\boldsymbol{Z_i'\eta} + \gamma\Delta q - \beta B_i}{\sigma}\right)\right]^{I_i} \left[1 - \Phi\left(\frac{\boldsymbol{Z_i'\eta} + \gamma\Delta q - \beta B_i}{\sigma}\right)\right]^{1-I_i} \tag{13}$$

and the log likelihood function is:

$$\ln L(\boldsymbol{\eta}, \gamma, \beta | \boldsymbol{y}, \boldsymbol{Z}, \boldsymbol{B}) = \sum_{i=1}^{N} I_i \ln\left[\Phi\left(\frac{\boldsymbol{Z_i'\eta} + \gamma\Delta q - \beta B_i}{\sigma}\right)\right] + (1 - I_i)\ln\left[1 - \Phi\left(\frac{\boldsymbol{Z_i'\eta} + \gamma\Delta q - \beta B_i}{\sigma}\right)\right] \tag{14}$$

Following maximum likelihood estimation of the parameters, we can solve for WTP. By definition, WTP is the compensating variation (CV) in the case of an improvement in q. Recall that CV is the decrease in income necessary to leave the person indifferent between the baseline and improved levels of q. CV for respondent i is defined by:

$$\boldsymbol{Z_i'\eta_1} + \gamma_1 q_1 + \beta_1(y_i - CV_i) = \boldsymbol{Z_i'\eta_0} + \gamma_0 q_0 + \beta_0 y_i \tag{15}$$

$$CV_i = \frac{\boldsymbol{Z_i'\eta} + \gamma\Delta q}{\beta} + \frac{\varepsilon_i}{\beta} \tag{16}$$

The mean WTP for respondent i is then $E[CV_i|\beta, \gamma, \boldsymbol{\eta}, \boldsymbol{Z_i}] = {}^{(\boldsymbol{Z_i'\eta} + \gamma\Delta q)}/_{\beta}$. The mean (total) WTP for the entire sample is therefore $E[CV|\beta, \gamma, \boldsymbol{\eta}, \boldsymbol{Z}] = {}^{(\boldsymbol{Z'\eta} + \gamma\Delta q)}/_{\beta}$ where each parameter estimate is multiplied by the mean value (or value of interest) of that explanatory variable. For a linear utility function and a symmetric, mean zero error representing random (unobservable) preferences, this is also equal to the median WTP (Phaneuf & Requate, 2016, p. 585).

In the DBDC approach, each respondent is asked a follow-up dichotomous question. This produces four possible response sequences based on the different bids (B^1, B^2). A "yes" response to the initial bid followed by a "no" response to the follow-up bid (yes-no) suggests that $B^1 \leq CV < B^2$. A no-yes response suggests that $B^1 > CV \geq B^2$. For yes-yes and no-no responses, the follow-up bid raises the lower bound $(CV \geq B^2)$ or lowers the upper bound $(CV < B^2)$, respectively. To construct the likelihood function for the DBDC model it is first necessary to derive the probability of observing each of the two-bid response sequences. To illustrate this simply using a notation similar to that of Hanemann et al. (1991), we follow the approach developed by Cameron & James (1987). This approach interprets the respondent's answer as a lower or upper bound on their WTP. Then the approach assumes a specification for the WTP function and estimates WTP using the bounds implied by the respondents' answers. To see this, first assume a general econometric model of the form:

$$CV_i^k = \mu_i^k + \varepsilon_i^k \tag{17}$$

where CV_i^k represent the WTP of respondent i, $k = 1, 2$ represents the initial and follow-up answers to the CVM question, and μ^k is the mean for response k. The means μ_i^1 and μ_i^2 depend on the individual covariates $\boldsymbol{Z_i'\eta^k} + \gamma^k \Delta q$ as before although the parameters and random terms are allowed to differ between a respondent's first and second answer.[68] Using this formulation, the probability respondent i answers "yes" to the initial bid and "no" to the follow-up bid is:

$$\Pr(yes_i, no_i) = \Pr[CV_i^1 \geq B^1, CV_i^2 < B^2] \tag{18}$$

$$\Pr(yes_i, no_i) = \Pr[\mu_i^1 + \varepsilon_i^1 \geq B^1, \mu_i^2 + \varepsilon_i^2 < B^2] \tag{19}$$

After constructing the remaining three response sequences, respondent i's contribution to the likelihood function becomes:

$$L_i(\mu|B) = \Pr[\mu_i^1 + \varepsilon_i^1 \geq B^1, \mu_i^2 + \varepsilon_i^2 < B^2]^{YN} \times \Pr[\mu_i^1 + \varepsilon_i^1 > B^1, \mu_i^2 + \varepsilon_i^2 \geq B^2]^{YY}$$

[68] Note that if we assume the responses to the initial and follow-up bid are based on the same underlying preference structure – i.e., if the respondent does not strategically answer the follow-up question – then the specification simplifies to the case where $CV_i^1 = CV_i^2$, $\boldsymbol{\eta^1} = \boldsymbol{\eta^2}$, and $\gamma^1 = \gamma^2$ but with additional bounds on CV_i provided by the two bids B^1 and B^2.

$$\times \Pr[\mu_i^1 + \varepsilon_i^1 < B^1, \mu_i^2 + \varepsilon_i^2 < B^2]^{NN} \times \Pr[\mu_i^1 + \varepsilon_i^1 < B^1, \mu_i^2 + \varepsilon_i^2 > B^2]^{NY} \tag{20}$$

where the binary-valued indicator $YN = 1$ for a yes-no answer (and 0 otherwise), $YY = 1$ for a yes-yes answer, $NN = 1$ for a no-no answer, and $NY = 1$ for a no-yes answer. If the random terms are assumed to be jointly normally distributed with means of zero and variances σ_k^2, then CV_i^1 and CV_i^2 have a bivariate normal distribution with means μ_i^1 and μ_i^2, variances σ_1^2 and σ_2^2, and correlation coefficient $\rho = {}^{\sigma_{12}}\Big/_{\sqrt{\sigma_1^2 + \sigma_2^2}}$ where σ_{12} is the covariance between ε_1 and ε_2. This is the bivariate probit model introduced by Cameron and Quiggin (1994) (and adapted here from Haab and McConnell (2002)). The response scenario terms in the likelihood function become:

$$\Pr[\mu_i^1 + \varepsilon_i^1 \geq B^1, \mu_i^2 + \varepsilon_i^2 < B^2] = \Phi_{\varepsilon 1 \varepsilon 2}\left(-\frac{B^1 - \mu_i^1}{\sigma_1}, \frac{B^2 - \mu_i^2}{\sigma_2}, -\rho\right) \tag{21}$$

$$Pr[\mu_i^1 + \varepsilon_i^1 > B^1, \mu_i^2 + \varepsilon_i^2 \geq B^2] = \Phi_{\varepsilon 1 \varepsilon 2}\left(-\frac{B^1 - \mu_i^1}{\sigma_1}, -\frac{B^2 - \mu_i^2}{\sigma_2}, \rho\right) \tag{22}$$

$$\Pr[\mu_i^1 + \varepsilon_i^1 < B^1, \mu_i^2 + \varepsilon_i^2 < B^2] = \Phi_{\varepsilon 1 \varepsilon 2}\left(\frac{B^1 - \mu_i^1}{\sigma_1}, \frac{B^2 - \mu_i^2}{\sigma_2}, \rho\right) \tag{23}$$

$$\Pr[\mu_i^1 + \varepsilon_i^1 < B^1, \mu_i^2 + \varepsilon_i^2 > B^2] = \Phi_{\varepsilon 1 \varepsilon 2}\left(\frac{B^1 - \mu_i^1}{\sigma_1}, -\frac{B^2 - \mu_i^2}{\sigma_2}, -\rho\right) \tag{24}$$

where $\Phi_{\varepsilon 1 \varepsilon 2}(.)$ is the standard bivariate normal cumulative distribution function with zero means, unit variances, and correlation coefficient ρ. Define the indicator $y_{1i} = 1$ if the response to the initial question is "yes" (and 0 otherwise) and $y_{2i} = 1$ if the response to the follow-up question is "yes." Also define the multipliers $d_{1i} = 2y_{1i} - 1$, and $d_{2i} = 2y_{2i} - 1$. Then respondent i's contribution to the likelihood function simplifies to:

$$L_i(\mu|B) = \Phi_{\varepsilon 1 \varepsilon 2}\left(d_{1i}\left(\frac{B^1 - \mu_i^1}{\sigma_1}\right), d_{2i}\left(\frac{B^2 - \mu_i^2}{\sigma_2}\right), d_{1i}d_{2i}\rho\right) \tag{25}$$

The parameters of the bivariate probit model can be estimated via maximum likelihood.

In this chapter we use the simplest specification for the change in safe hours (Δq) between the counterfactual (q_0) and status quo (q_1) scenarios. This specification takes Δq to be the percent loss of safe hours that is prevented by the proposed coastal management plan, as shown to the respondents. A more complex specification would convert this prevented percent loss into a level change – a change in the number of safe hours per day – using present-day safe hours data for the

16 developed beaches of interest. To illustrate how this conversion to level changes could be done in future work, we provide an example using the simplest aggregation of safe hours data. We aggregate the seasonal average safe hours across seasons and across developed beaches to get a single measure of average annual safe hours for the entire Oregon Coast. For the median minimum beach width value of 15 m, the present-day annual average safe hours are 10.6 hours per day. Survey respondents are told that the counterfactual scenario with the coastal management plan will prevent a 10% to 40% loss of safe hours. Thus, we use these percent loss values as the total percent change in safe hours in the status quo scenario. For the counterfactual scenario, we assume the coastal management plan prevents the *entire* loss of safe hours that would occur under the status quo.[69] Thus, the counterfactual scenario has a 0% loss of safe hours and the present-day annual average safe hours of 10.6 hours per day is also the counterfactual annual average safe hours (q_1). We use this value and the four values of prevented percent loss of safe hours in the survey to calculate what the future annual average safe hours would be under the status quo in which these losses are not prevented (q_0). The difference between the future annual average safe hours under the counterfactual scenario and the status quo is the Δq in levels. Table 3.2 presents the future annual average safe hours and the Δq values in levels using this aggregation method, as an example.

While not implemented in this chapter, we will also have the ability to refine the spatial scale of this specification of Δq – and our welfare estimate – by aggregating only the safe hours at the beaches that our respondents have actually visited or would be likely to visit. In this specification, the prevented percent loss is converted into levels using the respondents' revealed preference data. For recreators, we select the three developed beaches that they visited most frequently between April 2021 and March 2022. For non-recreators, we select the three developed beaches that are in closest proximity to their home ZIP code. We aggregate the seasonal average safe hours across seasons and across each respondent's three selected beaches for the median minimum beach width of 15 m to get a single measure of present-day average annual safe hours that is unique to each respondent. Under the counterfactual scenario with 0% loss, this is also each respondent's counterfactual annual average safe hours. We use this value and the prevented

[69] Otherwise, we lack a reference point for the entire loss for converting Δq from a percent change to a level change.

percent loss of safe hours shown to the respondent to calculate what the future annual average safe hours would be under the status quo. This is a measure of the status quo future average safe hours that is the most relevant to each respondent. The difference between the future annual average safe hours under the counterfactual scenario and the status quo is again the Δq in levels except that this value is now unique to each respondent and is refined to the coastal region they actually visit or may possibly visit.

In the base versions of both the SBDC and DBDC models we initially assume that all explanatory variables enter linearly. The utility difference (i.e., the model specification – see equations 7 through 12) for the base SBDC model (Model 1) is:

$$v_{i1}(.) - v_{i0}(.) = \beta bid1_i + \gamma deltasafehours_i + \eta_1 relax_i + \eta_2 g18\_future\_likely_i$$
$$+\eta_3 armoring\_allow\_agree_i + \eta_4 hh\_income_i + \boldsymbol{Z_i'\eta} \tag{26}$$

where $bid1$ is the initial bid, $deltasafehours$ is the prevented percent loss of safe hours (i.e., Δq), $relax$ indicates that the respondent was shown the "Relax" Goal 18 scenario, and the remaining variables including those in $\boldsymbol{Z_i'\eta}$ are explanatory demographic and familiarity variables. The prevented percent loss of safe hours variable $deltasafehours$ is treated as continuous in this chapter.[70]

In two alternative SBDC specifications we also use the indicator for which Goal 18 scenario the respondent was shown ($relax$) with the respondent's perceptions about Goal 18 and shoreline armoring. In one of these models (Model 2), $relax$ is interacted with $g18\_future\_likely$, a variable indicating that the respondent thought it was very likely or somewhat likely that Goal 18 will be maintained in its current form for the foreseeable future. This specification considers the interaction between respondents' perceptions about Goal 18's future and the Goal 18 future we present them with. The utility difference for Model 2 is:

$$v_{i1}(.) - v_{i0}(.) = \beta bid1_i + \gamma deltasafehours_i + \eta_1 relax_i * g18\_future\_likely_i$$
$$+\eta_3 armoring\_allow\_agree_i + \eta_4 hh\_income_i + \boldsymbol{Z_i'\eta} \tag{27}$$

In the other model (Model 3), $relax$ is interacted with $armoring\_allow\_agree$, a variable indicating that the respondent either strongly agrees or somewhat agrees with the statement that

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[70] A categorical specification of Δq would allow for non-linear effects. However, in this chapter we focus on the simplest specification of Δq, i.e., a continuous prevented percent loss of safe hours.

"All properties that are vulnerable to erosion should be able to install shoreline armoring." This specification looks at how a respondent's preferences about shoreline armoring eligibility affect how they vote in the "Maintain" vs "Relax" Goal 18 scenarios. The utility difference for Model 3 is:

$$v_{i1}(.) - v_{i0}(.) = \beta bid1_i + \gamma deltasafehours_i + \eta_1 relax_i * armoring\_allow\_agree_i \\ + \eta_2 g18\_future\_likely_i + \eta_4 hh\_income_i + \boldsymbol{Z_i'\eta} \tag{28}$$

We use a stated ANA approach to deal with attribute non-attendance. Specifically, we use the "ANA validation model" as in Hess and Hensher (2010) and Scarpa et al. (2013). ANA validation models estimate separate preference parameters for those stating they ignore the attribute from those who stated they attend to it. To do so we interact the "ANA likely" variables with their respective attribute – the initial bid ($bid1$), the prevented loss of safe hours ($deltasafehours$), and whether the respondent was shown the "Relax" Goal 18 scenario ($relax$). For example, the utility difference for Model 3 using the ANA validation approach with the stricter definition of ANA is:

$$v_{i1}(.) - v_{i0}(.) = \beta bid1_i * ana\_likely\_cost_i + \gamma deltasafehours_i * ana\_likely\_safehours_i \\ + \eta_1 relax_i * ana\_likely\_goal18_i * armoring\_allow\_agree_i \\ + \eta_2 g18\_future\_likely_i + \eta_4 hh\_income_i + \boldsymbol{Z_i'\eta} \tag{29}$$

where $\boldsymbol{Z_i'\eta}$ includes remaining explanatory demographic and familiarity variables.

3.5.2 Combined contingent valuation and recreation demand model

While we do not implement the combined CVM and recreation demand model in this chapter, we present the model specification here as it motivated how the survey was designed. This model builds on the frameworks of Eom and Larson (2006), Huang et al. (2016), and Landry et al. (2020). Our objective is to use this framework to jointly estimate the demand for beach recreation trips and the WTP for coastal management to investigate how the resulting use and non-use values are affected by shoreline armoring policies and safe hours.

The starting point for this framework is the constrained utility maximization problem for choosing recreational trip frequency:

$$\max_{X,z} u(X, z, q) \;\; s.t. \, y = pX + z \tag{30}$$

where X is recreational trip frequency with price p, y is household income, z is a numeraire good, and q is the environmental quality variable of interest. Solving this problem yields the Marshallian demand for recreation trips $X = X(p, q, y)$. The next step requires specifying the functional form for recreation demand. The dependent variable, X_i, is the count of beach trips per person per season, which are collected in the revealed preference section of the survey. Survey data is augmented with data on travel costs and site characteristics to estimate a count model of recreation demand for the 16 developed beaches. Travel costs include the roundtrip time costs and auto costs. Time is the round-trip travel time and depends on the road distance and travel time from a person's primary residence to the beach as well as on their income (i.e., wage is the opportunity cost of time spent recreating). Auto costs contain fuel and vehicle depreciation. We use three definitions of the trip counts. The first and second definitions are the count of day trips per season and the count of short overnight trips per season, respectively. The third definition aggregates day trip and short overnight trip counts into a single count of trips per season. In this specification of X_i we also classify each recreator in our sample as taking either primarily day trips or short overnight trips, as in Parsons et al. (2013).

Prior studies that used the Eom and Larson (2006) framework have primarily specified a single-site continuous model of recreation demand. Eom and Larson (2006) have recreational data for six sites in the Man Kyoung River basin. They use a semi-log specification for a single-site recreation demand model with this six-site system where trip data is based on the number of trips to the site most frequently visited by a respondent. This choice was made to have variation in the water quality variable among respondents since the water quality variable is an annual average of biochemical oxygen demand. Egan (2011) have recreational data for eight lakes in Iowa. They also use a single-site semi-log recreation demand model where trip data is based on the number of trips to the focus lake of their zone. Huang et al. (2016) have recreational data for a single recreation area on the Danshui River in Taipei City, Taiwan. They employ six different commonly-specified single-site recreation demand models. Landry et al. (2020) have recreation data for 20+ North Carolina beaches. Unlike the other studies that use the Eom and Larson (2006) framework, however, they model the individual demand equation as an incomplete demand system. They also decide that a continuous model of trip demand (e.g., semi-log) is not applicable since nearly 35%

of respondents reported no beach trips. Instead, they fit a Poisson log-normal and a negative binomial model to the trip demand data. They jointly estimate the Poisson log-normal demand model with the probit CVM model to recover use and non-use values.

Since the count of trips per season is a non-negative integer, our goal is to specify a count data model for trip demand, as in Landry et al. (2020). For example, we can initially specify the most common count model, the Poisson model. This model assumes that the distribution of trips to a single site is described by the random variable X_i, where X_i is distributed Poisson with mean and variance equal to λ_i. An individual's probability of making x_i trips to a site in a given season in the Poisson model is given by:

$$\Pr(X_i = x_i) = \frac{e^{-\lambda_i}\lambda_i^{x_i}}{x_i!}\,, x_i = 0, 1, 2, \dots \tag{31}$$

where λ_i is specified as the expected demand equation for trips. The most common specification for expected demand in count data models is a semi-log form so that the expected value is positive (Parsons, 2017). Thus, the expected number of trips taken by person i is given by:

$$E(X_i) = \lambda_i = e^{\boldsymbol{Z}_i'\boldsymbol{\alpha} + \beta p_i + \gamma \ln(y_i) + \delta q} \tag{32}$$

where p_i is the price or travel cost of the individual to reach the site, y_i is income, q is our environmental quality variable of interest (seasonal average of safe hours), and $\boldsymbol{Z}_i$ includes other individual and site covariates believed to influence the number of trips taken in a season. Individual characteristics include age, family size, ownership of a coastal second home, etc. Site characteristics include beach width, beach length, and the number of access points. The environmental quality variable of interest (q) is measurable at the sites that trip count data is collected for, i.e., the information about q in the recreation demand model does not come from the CVM question but rather from the measured site characteristics. The q in our recreation demand model is the present-day seasonal average daylight safe hours at each beach. Note that this environmental quality variable must have variation across beaches and be included in both the trip demand model and the CVM model. These parameters are estimated via maximum likelihood.

For the following illustration of the Eom and Larson (2006) framework, we use a semi-log demand function similar to Huang et al. (2016):[71]

$$\ln(X) = \boldsymbol{Z'\alpha} + \beta p + \gamma \ln(y) + \delta q + \varepsilon \tag{33}$$

Using the duality theorem, this Marshallian demand function can be integrated back to derive the quasi-expenditure function (Hausman, 1981). First, the trip demand is substituted into the utility function to produce the indirect utility function $V(p,q,y) = u(X(p,q,y), z(p,q,y), q)$.[72] By the duality theorem, the inverse of indirect utility with respect to income (y) is the minimum expenditure function:

$$E(p,q,u) = \min_{X,z}\{pX + z : u(X,z,q) = u\} \tag{34}$$

By Shephard's Lemma, the price slope of the expenditure function is Hicksian demand $X^h(p,q,u)$, which is equal to Marshallian demand $X(p,q,y)$ when money income y is replaced by the expenditure function $E(p,q,u)$:

$$\frac{\partial E(p,q,u)}{\partial p} = X^h(p,q,u) = X\big(p,q,E(p,q,u)\big) \tag{35}$$

Using the semi-log specification of recreation demand X, this ordinary differential equation can be solved to find the quasi-expenditure function $I(p,q,c(q,u))$ (as shown in Hausman (1981)):

$$I\big(p,q,c(q,u)\big) = \left(\frac{1-\gamma}{\beta} e^{\boldsymbol{Z'}\boldsymbol{\alpha}+\beta p+\delta q+\varepsilon} + (1-\gamma)c\right)^{\frac{1}{1-\gamma}} \tag{36}$$

where $c(q,u)$ is the constant of integration. In general, $c(q,u)$ depends on environmental quality q (Huang et al., 2016). As in Huang et al. (2016) and Landry et al. (2020), we will examine several alternative specifications for c. In the first specification, c is assumed to be constant: $c = u$. This specification assumes that the constant of integration is independent of q, which implies weak complementarity (no non-use value). The second specification of c is exponential:

$$c = f(u,q) = ue^{-\sum_i \phi_i W_i q} \tag{37}$$

[71] We also drop the i subscript in the following equations for visual clarity.
[72] Suppressing dependence on the individual and site covariates in $\boldsymbol{Z}$.

where $\boldsymbol{W}$ is a set of variables including individual and site characteristics that influence the marginal utility of q. The variable sets $\boldsymbol{Z}$ and $\boldsymbol{W}$ can contain the same variables (Huang et al., 2016). Landry et al. (2020) specify the $\boldsymbol{W}$ vector to contain information from the CVM question including which erosion management strategy the respondent was shown and whether there are negative environmental impacts of the strategy. Similar to Landry et al. (2020), we employ a "between" experimental design by assigning half of the sample to a scenario where the Goal 18 policy is relaxed to allow more shoreline armoring ("Relax) and the other half of the sample to a scenario where the Goal 18 policy remains as is (Maintain). Thus, $\boldsymbol{W}$ will include the same site and individual characteristics as $\boldsymbol{Z}$ as well as stated preference information such as whether the respondent was shown the "Relax" Goal 18 scenario ($relax$) and their beliefs about the future of Goal 18 ($g18\_future\_likely$). Lastly, the third specification of c is linear:

$$c = f(u, q) = u - \sum_{i} \tau_i W_i q \tag{38}$$

Once a specification for $c(q, u)$ is chosen, the indirect utility function $V(p, q, y)$ can be re-derived from the quasi-expenditure function $I(p, q, c(q, u))$ by the duality theorem, i.e., by switching u (embedded in c) to V and switching I to y. For example, for the exponential specification of c:

$$V = e^{\phi q}\left(\frac{1}{1-\gamma} y^{1-\gamma} - \frac{1}{\beta} e^{\boldsymbol{Z}'\boldsymbol{\alpha}+\beta p+\delta q+\varepsilon}\right) = e^{\phi q} y^{1-\gamma}\left(\frac{1}{1-\gamma} - \frac{1}{\beta}\frac{X}{y}\right) \tag{39}$$

where $\phi = \sum_i \phi_i W_i$. The WTP for an incremental increase in environmental quality (q) from q_0 to q_1 can be derived by setting the indirect utility function for q_1 equal to the indirect utility function for q_0:

$$V(p, q_1, y - WTP, \boldsymbol{Z}, \varepsilon) = V(p, q_0, y, \boldsymbol{Z}, \varepsilon) \tag{40}$$

The total WTP for an increase from q_0 to q_1 is the CV, which is the reduction in income necessary to make the utility of a higher quality (q_1) equal to the utility of the original quality (q_0). Thus, solving this equality for WTP gives the change in quasi-expenditure:

$$WTP = I(p, q_0, \boldsymbol{Z}, u_0, \varepsilon) - I(p, q_1, \boldsymbol{Z}, u_1, \varepsilon) \tag{41}$$

In our setting, q_0, the environmental quality *before* the quality change proposed in the CVM question, is the *future* seasonal average safe hours under the status quo scenario without the proposed management plan. Likewise, q_1, the environmental quality after the quality change proposed in the CVM question, is the future seasonal average safe hours under the counterfactual scenario with the management plan. As in section 3.5.1, we assume the coastal management plan prevents the *entire* loss of safe hours that would occur under the status quo. Therefore, the counterfactual future seasonal average safe hours (q_1) is also the present-day actual seasonal average safe hours that can be measured at the developed beaches of interest. This formulation of q_0 and q_1 is different from the original formulation in Eom and Larson (2006) because our survey frames the CVM question as "preventing a future loss" in environmental quality rather than "improving current" environmental quality, meaning that q_1 is both the current environmental quality and the future environmental quality with the coastal management plan whereas q_0 is the predicted future environmental quality without the coastal management plan.

To illustrate how to solve for WTP using the equality in equation (40) we employ the exponential specification of c so that this equality becomes:

$$e^{\phi q}\left(\frac{1}{1-\gamma}(y-WTP)^{1-\gamma}-\frac{1}{\beta}e^{\boldsymbol{Z}'\boldsymbol{\alpha}+\beta p+\delta q+\varepsilon}\right)=e^{\phi q}\left(\frac{1}{1-\gamma}y^{1-\gamma}-\frac{1}{\beta}e^{\boldsymbol{Z}'\boldsymbol{\alpha}+\beta p+\delta q+\varepsilon}\right) \quad (42)$$

Thus, the WTP for the exponential specification of c is:

$$WTP=y-y\left[e^{-\phi(q_1-q_0)}+\frac{1-\gamma}{\beta}\left(\frac{X_1}{y}-e^{-\phi(q_1-q_0)}*\frac{X_0}{y}\right)\right]^{\frac{1}{1-\gamma}} \quad (43)$$

The WTP functions for the constant and linear specifications of c are, respectively:

$$WTP=y-y\left[1+\frac{1-\gamma}{\beta}\left(\frac{X_1}{y}-\frac{X_0}{y}\right)\right]^{\frac{1}{1-\gamma}} \quad (44)$$

$$WTP=y-y\left[1+\frac{1-\gamma}{\beta}\left(\frac{X_1}{y}-\frac{X_0}{y}\right)-(1-\gamma)\tau y^{\gamma-1}(q_1-q_0)\right]^{\frac{1}{1-\gamma}} \quad (45)$$

where $\tau=\sum_i \tau_i W_i$ in equation (45). The constant specification of c assumes that q enters the utility function solely through the demand for trips X. The exponential and linear specifications allow q to enter the utility function through ϕ and τ, respectively, independent of the consumption of X. Revealed preference information enters the WTP function through X_1 and X_0, which are the true trip demand values evaluated at q_1 and q_0, respectively. Stated preference information from the survey enters via q_0, which is the future seasonal average safe hours under the status quo without the proposed coastal management plan, i.e., the number of safe hours that would result from the percent loss of safe hours that would not be prevented in the status quo scenario.

After deriving the WTP function for a given specification of trip demand (X) and the constant of integration (c), we create the log-likelihood function for the system of equations given by X and WTP. Each respondent makes a joint decision about how many trips to take (X) and what their WTP response is to the initial CVM question (no, yes). Let $Pr(X, no)$ and $Pr(X, yes)$ represent the joint distributions of the trip count and binary response variable for a "no" and "yes" vote, respectively. Then, the likelihood function for all respondents' joint decisions is:

$$L = \prod_{k \in no} Pr(X, no) \prod_{l \in yes} Pr(X, yes) \tag{46}$$

where the first term is the product across all respondents (k) who voted "no" and the second term is the product across all respondents (l) who answered "yes." The joint distribution $Pr(X, no)$ can be written as the product of the conditional distribution of a no CVM response depending on having taken X trips, $Pr(no|X)$, and the marginal distribution of trips $\omega(X)$. The joint distribution $Pr(X, yes)$ can be decomposed in a similar manner.

The conditional probability functions for "yes" and "no" votes can be derived using the chosen functional form for WTP and an assumption about how WTP is distributed. To see this, we start with a latent variable model for WTP:

$$WTP^* = WTP(q, X) + \nu \tag{47}$$

where $WTP(q, X)$ represents the chosen functional form for WTP and ν is normally distributed with mean zero and variance σ_ν^2.[73] As before, for an initial bid of B^1, the unconditional probability of a "no" vote is $Pr(WTP^* < B^1)$, which gives the probit probability $\Pr(no) = \Phi\left({(B^1 - WTP(q, X))}/{\sigma_\nu}\right)$. Likewise, the unconditional probability of a "yes" vote, $\Pr(WTP^* > B^1)$, gives the probit probability $\Pr(no) = \Phi\left({(WTP(q, X) - B^1)}/{\sigma_\nu}\right)$. To derive the conditional probabilities $\Pr(WTP^* > B^1|X)$ and $\Pr(WTP^* < B^1|X)$ we need to make an assumption about the marginal distribution of trips, $\omega(X)$.[74] Eom and Larson (2006) derive the conditional

[73] Both Eom and Larson (2006) and Landry et al. (2020) also assumed that WTP^* is normally distributed.
[74] Our eventual goal is to assume that X is Poisson distributed, as in equation 32.

probabilities assuming that X is normally distributed and Landry et al. (2020) derive the conditional probabilities for the case where X follows a Poisson-log normal distribution. After making an assumption about $\omega(X)$ and deriving the conditional probabilities, the likelihood function can be rewritten as:

$$L = \prod_{k \in no} \omega(X) Pr(WTP^* < B^1|X) \prod_{l \in yes} \omega(X) Pr(WTP^* > B^1|X) \tag{48}$$

This can be transformed into a log-likelihood function for the joint decisions, which is used to jointly estimate the demand and WTP equations to recover all parameter values. After estimating parameters, the total WTP can be decomposed into use and non-use value components, as in Huang et al. (2016).[75]

3.6 Results and Discussion

We estimate three primary SBDC models, both with and without the ANA validation model incorporated. Table 3.3 reports estimation results for the three primary SBDC models without the ANA validation model. Model 1 is the base SBDC model where all explanatory variables enter the probit model linearly, Model 2 interacts $relax$ with $g18\_future\_likely$, and Model 3 interacts $relax$ with $armoring\_allow\_agree$. Both Models 2 and 3 specify the interaction term but leave out the main effects. This is a variation of a cell means model in which the intercept is the mean for the omitted cell, i.e., $relax = 0$ and $g18\_future\_likely = 0$ for Model 2 and $relax = 0$ and $armoring\_allow\_agree = 0$ for Model 3. Therefore, the interaction coefficients give the difference between each of the cell means and the mean for the omitted cell. In this way we are able to investigate how respondents' beliefs about armoring and the armoring scenario they are shown impact their voting behavior.

All parameter estimates for the three CVM question attributes are consistent with prior expectations. The coefficient on cost ($bid1$) is statistically significant (at the 1% significance level) and negative in all three models suggesting that as the cost of the proposed coastal management plan increases, respondents are less likely to vote "yes." The law of demand states that the price of a good or service is negatively correlated with its demand (*ceteris paribus*). Thus, the negative

[75] Huang et al. (2016) derive the use and non-use value components for several specifications of trip demand (X) and the constant of integration (c).

relationship between the cost of the coastal management plan and respondents' WTP for it conforms to theory. The coefficient on safe hours ($deltasafehours$) is positive and marginally statistically significant in Model 1 (p-value of 0.1048). It is positive and statistically significant at the 10% significance level in Models 2 and 3. This suggests that respondents are more likely to vote "yes" as the percent loss of safe hours that is prevented by the coastal management plan increases. As anticipated, support for a coastal management plan increases as its predicted preservation of safe hours increases.

The coefficient on the Goal 18 armoring policy scenario ($relax$) is not statistically significant on its own in Model 1. However, beliefs about armoring – $g18\_future\_likely$ and $armoring\_allow\_agree$ – are highly statistically significant (at the 1% significance level). Respondents who believe that Goal 18 will likely be maintained in its current form and respondents who believe that vulnerable properties should be able to install armoring are more likely to vote "yes" to the coastal management plan, therefore indicating a higher WTP. This further motivates interacting these "verification check" variables with the armoring scenario in Models 2 and 3 to more closely investigate these relationships.

When $relax$ is interacted with $g18\_future\_likely$ in Model 2, the coefficient on $(relax = 0) * (g18\_future\_likely = 1)$ is positive and statistically significant (at the 5% level). This suggests that respondents in the "Maintain" scenario ($relax = 0$) who believe that Goal 18's armoring policy will be maintained in its current form ($g18\_future\_likely = 1$) are more likely to vote "yes" than respondents in the "Maintain" scenario who *do not* believe that Goal 18 will be maintained as is. This result aligns with our prior expectations. Individuals who believe that Goal 18 will be maintained and are presented with a future "Maintain" scenario are more likely to accept this scenario than individuals whose beliefs about Goal 18's future *conflict* with the future scenario that is presented to them. We would expect the group that is skeptical about the future scenario presented to them in the CVM question to be more likely to reject that scenario by voting "no" on the coastal management plan.

The coefficient on $(relax = 1) * (g18\_future\_likely = 0)$ is negative but not statistically significant. This suggests that, for respondents who believe that Goal 18 will likely not continue as is, whether they are presented with a future scenario where Goal 18 is relaxed

$(relax = 1)$ or maintained $(maintain = 0)$ does not have a significant effect on how they vote on the coastal management plan. Our prior expectation is a positive coefficient. This case is similar to the previous case. We would expect that individuals who believe that Goal 18 will likely not continue as is (i.e., who believe that Goal 18 will be relaxed) and are presented with a future "Relax" scenario are more likely to accept this scenario than individuals in the "Maintain" scenario who *do not* believe that Goal 18 will be maintained as is (i.e., whose beliefs about Goal 18's future conflict with the future scenario presented to them). Thus, this result is counter to our prior expectations.

The coefficient on $(relax = 1) * (g18\_future\_likely = 1)$ is positive and statistically significant (at the 5% level). This suggests that respondents in the "Relax" scenario who believe that Goal 18's armoring policy will be maintained in its current form are more likely to vote "yes" than respondents in the "Maintain" scenario who do not believe that Goal 18 will be maintained as is. We do not have a prior expectation for the sign on this coefficient. For both groups of respondents, the future Goal 18 scenario presented to them conflicts with their beliefs about Goal 18's future. The sign on this coefficient therefore does not have an intuitive behavioral interpretation since both groups of respondents would be skeptical about the future scenario presented to them in the CVM question.

A respondent whose belief about Goal 18's future conflicts with the future Goal 18 scenario in the CVM question may not necessarily reject that scenario. They may alternatively choose to ignore the Goal 18 scenario and not let it affect how they vote. In this case, $g18\_future\_likely$ may not necessarily be a good predictor of voting behavior. A better predictor may be whether the respondent believes that properties vulnerable to erosion should be allowed to install shoreline armoring. This motivates the interaction between $relax$ and $armoring\_allow\_agree$ in Model 3. The coefficient on $(relax = 0) * (allow\_armoring\_agree = 1)$ is positive and statistically significant (at the 1% level). This suggests that respondents in the "Maintain" scenario $(relax = 0)$ who believe that vulnerable properties should be allowed to armor $(armoring\_allow\_agree = 1)$ are more likely to vote "yes" than respondents in the "Maintain" scenario who *do not* believe that vulnerable properties should be allowed to armor. This aligns with our prior expectations. Individuals who believe armoring should be allowed are more likely to vote "yes" for the status quo that allows some armoring ("Maintain") than individuals who do not believe armoring should be allowed.

The coefficient on $(relax = 1) * (armoring\_allow\_agree = 0)$ is negative and statistically significant (at the 5% level). This suggests that respondents who do not believe vulnerable properties should be allowed to armor and are presented with a future scenario where Goal 18 is relaxed to allow more armoring are less likely to vote "yes" than respondents who are similarly against allowing armoring but are presented with a future scenario where Goal 18 is maintained. This result is consistent with our prior expectations. Individuals who are against allowing vulnerable properties to armor are less likely to vote "yes" for a coastal management plan that "relaxes" Goal 18 compared to a plan that "maintains" Goal 18.

The coefficient on $(relax = 1) * (armoring\_allow\_agree = 1)$ is positive and statistically significant (at the 1% level). This suggests that respondents in the "Relax" scenario who believe that vulnerable properties should be allowed to armor are more likely to vote "yes" than respondents in the "Maintain" scenario who *do not* believe that vulnerable properties should be allowed to armor. This result aligns with our prior expectations and is the most intuitive result from this set of interactions. Individuals who support relaxing Goal 18 to allow vulnerable properties to armor are more likely to support a coastal management plan that "Relaxes" Goal 18 than individuals who are against allowing armoring and are presented with the status quo ("Maintain").

All other significant variables have the expected signs in all three primary SBDC models. Respondents who are older are less likely to vote "yes," indicating a lower WTP for the coastal management plan (at the 1% level). One potential explanation for this result is that older respondents may be less willing to support a policy whose benefits they may not receive, i.e., the proposed coastal management plan requires funding for 30 years to prevent a loss of safe hours over that time. Whether a respondent has a bachelor's degree or higher is statistically significant and positive (at the 10% level), suggesting that respondents with college degrees are more likely to vote "yes" for the coastal management plan. Respondents who identified as liberal were more likely to vote "yes" to the coastal management plan (at the 1% significance level). Contrary to our expectations, having a primary residence or second home on the coast did not have a statistically significant effect on how respondents voted. Beliefs about armoring – $g18\_future\_likely$ and $armoring\_allow\_agree$ – are highly statistically significant (at the 1% level) in all models. Respondents who said they were familiar with shoreline armoring were less likely to vote "yes" for the coastal management plan (at the 5% level). Thus, respondent familiarity with shoreline armoring appears to imply less support for a plan that either maintains or relaxes the existing Goal 18 armoring policy. However, familiarity with beach nourishment (sediment management) did not affect voting behavior. Respondents who said they were aware that the number of safe hours may decrease as erosion on developed beaches increases were more likely to vote "yes" for the coastal management plan (at the 1% level).

Table 3.4 reports estimation results for the three primary SBDC models using the ANA validation model and the stricter ANA definition, e.g., ana_likely_cost. As before, Model 1A is the base model where all explanatory variables enter the probit model linearly, Model 2A interacts $relax$ with $g18\_future\_likely$, and Model 3A interacts $relax$ with $armoring\_allow\_agree$. The interaction effects for attending respondents are the interactions in which the ANA-likely variable equals zero, e.g., $(ana\_likely\_cost = 0) * bid1$. We interpret the interaction coefficients for attending respondents only. If respondents ignored the attributes they stated they ignored, then the interaction coefficients for non-attending respondents should be zero (Hess & Hensher, 2010). However, like Hess and Hensher (2010), we find that the coefficient estimates for the non-attending group are often nonzero and statistically significant. This suggests that respondents who stated they ignored an attribute may have actually just placed a lower importance on it, i.e., these respondents are better labeled as "not fully attending" rather than "non-attending." Since the ANA-likely definition used in Models 1A-3A is the strictest (most conservative) definition, it is likely that many respondents who were labeled as "not attending" to an attribute were actually partially attending to it. Therefore, these "not fully attending" respondents may actually have non-zero marginal utility for the attributes they stated they ignored.

Applying the ANA validation model improved model fit for all three primary SBDC models. All parameter estimates for the three CVM question attributes in Models 1A-3A are consistent with prior expectations for attending respondents and consistent with estimates from Models 1-3. The coefficient on cost for attending respondents $(ana\_likely\_cost = 0) * bid1$ is negative and statistically significant in all three models (at the 1% significance level). The coefficient on safe hours for attending respondents $(ana\_likely\_safehours = 0) * deltasafehours$ is positive, as in Models 1-3, but is now highly statistically significant in Models 1A-3A (at the 1% level). The coefficient on the Goal 18 armoring policy scenario $(relax = 1) * (ana\_likely\_goal18 = 0)$ is not statistically significant on its own in Model 1A, as in Model 1. Beliefs about armoring – $g18\_future\_likely$ and $armoring\_allow\_agree$ – are still statistically significant in Model 1A (at the 10% and 1% levels, respectively).

The interaction coefficients between $relax$, ana_likely_goal18, and $g18\_future\_likely$ in Model 2A have the same signs as the corresponding interaction coefficients in Model 2 but with different levels of significance. The coefficient on $(relax = 0) * (ana\_likely\_goal18 = 0) * (g18\_future\_likely = 1)$ is still positive but is no longer statistically significant. Our prior expectation is a significant positive coefficient – as was found in Model 2. The coefficient on $(relax = 1) * (ana\_likely\_goal18 = 0) * (g18\_future\_likely = 0)$ is still negative but is now statistically significant at the 10% level. This suggests that (attending) respondents who are presented with a future "Relax" scenario and believe that Goal 18 will likely not continue in its current form are *less* likely to vote "yes" than (attending) respondents with the same Goal 18 beliefs in the "Maintain" scenario. Our prior expectation is a positive coefficient, i.e., that the former group of respondents would be *more* likely to vote "yes." Thus, this result is still counter to our prior expectations. The coefficient on $(relax = 1) * (ana\_likely\_goal18 = 0) * (g18\_future\_likely = 1)$ is still positive but is no longer statistically significant. We do not have a prior expectation for the sign on this coefficient since it does not have an intuitive behavioral interpretation.

The interaction coefficients between $relax$, ana_likely_goal18, and $armoring\_allow\_agree$ in Model 3A have the same signs and statistical significance as the corresponding interaction coefficients in Model 3. The coefficient on $(relax = 0) * (ana\_likely\_goal18 = 0) * (armoring\_allow\_agree = 1)$ is still positive and statistically significant (at the 1% level). The coefficient on $(relax = 1) * (ana\_likely\_goal18 = 0) * (armoring\_allow\_agree = 0)$ is still negative and statistically significant (at the 5% level). The coefficient on $(relax = 1) * (ana\_likely\_goal18 = 0) * (armoring\_allow\_agree = 1)$ is still positive and statistically significant (at the 1% level). All three results align with our prior expectations.

The other significant variables in Models 1A-3A have the expected signs, as in Models 1-3. Familiarity with shoreline armoring and awareness that safe hours may decrease now have lower statistical significance. Whether a respondent has a bachelor's degree or higher is no longer statistically significant. Results from Models 2A and 3A suggest that respondents who own a second home on the coast are more likely to vote "yes" for the coastal management plan (at the 10% level).

Table 3.5 reports estimation results for the three primary SBDC models using the ANA validation model and the less strict ANA definition, e.g., $ana\_likely\_cost\_3$. As before, Model 1B is the base model where all explanatory variables enter the probit model linearly, Model 2B interacts $relax$ with $g18\_future\_likely$, and Model 3B interacts $relax$ with $armoring\_allow\_agree$. The interaction effects for attending respondents are once again the interactions in which the ANA-likely variable equals zero, e.g., $(ana\_likely\_cost\_3 = 0) * bid1$. We only interpret the interaction coefficients for attending respondents, as in Models 1A-3A. Note that the coefficient estimates on cost ($bid1$) and safe hours ($deltasafehours$) for the non-attending respondents are nonzero and statistically significant. Recall that this suggests that respondents who stated they ignored an attribute may have actually partially attended to that attribute. These "not fully attending" respondents may therefore have non-zero marginal utility for the attributes they stated they ignored. The less strict definition assumes that the respondent likely did not attend to an attribute only if they chose "not at all important" or "not sure" in response to the statement "Please select how important each factor was in influencing your vote." However, even with this less conservative definition of ANA, there appear to be "not fully attending" respondents labeled as "non-attending."

Applying the ANA validation model with this less strict ANA definition improved model fit in Models 1B-3B compared to Models 1-3. However, the ANA validation models with the stricter ANA definition (Models 1A-3A) improved model fit more than the ANA validation models with the less strict ANA definition (Models 1B-3B). The coefficients on the three CVM question attribute – $bid1$, $deltasafehours$, and $relax$ – are again consistent with prior expectations for attending respondents and consistent with estimates from Models 1-3. The coefficients on $(ana\_likely\_cost\_3 = 0) * bid1$ and $(ana\_likely\_safehours\_3 = 0) * deltasafehours$ have the expected signs and are statistically significant (at the 1% and 5% levels, respectively). The coefficient on the Goal 18 scenario $(relax = 1) * (ana\_likely\_goal18\_3 = 0)$ is not statistically significant on its own, as in Models 1 and 1A. Beliefs about armoring – $g18\_future\_likely$ and $armoring\_allow\_agree$ – are still statistically significant (at the 1% level).

The interaction coefficients between $relax$, $ana\_likely\_goal18\_3$, and $g18\_future\_likely$ in Model 2B have the same signs and approximately the same statistical significance as the corresponding interaction coefficients in Model 2. The coefficient on $(relax = 0) * (ana\_likely\_goal18\_3 = 0) * (g18\_future\_likely = 1)$ is still positive and statistically significant (now at the 10% level), which conforms to prior expectations. The coefficient on $(relax = 1) * (ana\_likely\_goal18\_3 = 0) * (g18\_future\_likely = 0)$ is negative but not statistically significant, as in Model 2. Since our prior expectation is a positive coefficient, this result is again counter to our expectations. The coefficient on $(relax = 1) * (ana\_likely\_goal18\_3 = 0) * (g18\_future\_likely = 1)$ is still positive and statistically significant (now at the 10% level). We do not have a prior expectation for the sign on this coefficient.

The interaction coefficients between $relax$, $ana\_likely\_goal18\_3$, and $armoring\_allow\_agree$ in Model 3B have the same signs and approximately the same statistical significance as the corresponding interaction coefficients in Model 3. The coefficient on $(relax = 0) * (ana\_likely\_goal18\_3 = 0) * (armoring\_allow\_agree = 1)$ is still positive and statistically significant (at the 1% level). The coefficient on $(relax = 1) * (ana\_likely\_goal18\_3 = 0) * (armoring\_allow\_agree = 0)$ is still negative but is now only marginally significant (p-value of 0.1016). The coefficient on $(relax = 1) * (ana\_likely\_goal18 = 0) * (armoring\_allow\_agree = 1)$ is still positive and statistically significant (at the 1% level). All three results align with our prior expectations. The other significant variables in Models 1B-3B have the expected signs, as in Models 1-3. Results from Models 1B-3B suggest that respondents who own a second home on the coast are more likely to vote "yes" for the coastal management plan (at the 10% level), which is similar to results from Models 2A and 3A.

Table 3.6 reports estimation results for the DBDC model. The DBDC model is a bivariate probit model where each individual probit is specified so that all explanatory variables enter linearly, as in the base SBDC model, Model 1. The coefficients on cost – ($bid1$) for the initial vote and ($bid2$) for the follow-up vote – are statistically significant (at the 1% significance level) and negative, as expected. The coefficient on safe hours ($deltasafehours$) is positive for the initial vote and negative for the follow-up vote but neither coefficient is statistically significant. Therefore, the base DBDC model suggests that the prevented percent loss of safe hours does not have a significant effect on how respondents vote on the coastal management plan. This result is not consistent with the SBDC model results, which had positive and mostly statistically significant coefficients on safe hours. The coefficient on the Goal 18 scenario ($relax$) is not statistically significant on its own in the DBDC model, as in all of the SBDC models.

The other significant variables have the same signs as in the SBDC models. Respondent age, whether they hold a bachelor's degree or higher, and whether they identify as liberal are still statistically significant demographic variables. Respondents who believe that Goal 18 will likely be maintained in its current form and respondents who believe that vulnerable properties should be able to install armoring are more likely to vote "yes" to the coastal management plan (at the 1% level). While the safe hours attribute ($deltasafehours$) is not statistically significant in the DBDC model, respondents who said they were aware that the number of safe hours may decrease as erosion on developed beaches increases are more likely to vote "yes" to the coastal management plan (at the 1% level).

The correlation coefficient (ρ) is positive and statistically significant (at the 1% level). A significance test on ρ is a test of independent probits versus a bivariate probit specification (Haab & McConnell, 2002, p. 136). A LR test rejects the null hypothesis H_0: $\rho = 0$ at the 1% significance level. This positive and statistically significant correlation coefficient suggests that using the bivariate probit model is more appropriate than using independent probit models for jointly analyzing the initial and follow-up CVM question responses. It also suggests that there is a positive correlation between responses to the initial CVM question and the follow-up CVM question.

Table 3.7 reports mean/median WTP estimates for the base SBDC and DBDC models where all explanatory variables enter linearly. For each model, mean WTP was calculated separately for the "Relax" and "Maintain" scenarios to compare WTP across the sample split. Confidence intervals (95%) for WTP estimates are calculated using both the Krinsky and Robb (1986) method and the Delta method, for comparison.[76] The Delta method yields symmetric confidence intervals. However, Haab and McConnell (2002, p. 110) recommend simulating the confidence interval for WTP using the Krinsky and Robb (1986) method, which allows the confidence interval to be non-symmetric.[77] According to Model 1, the base SBDC model, mean WTP for the coastal management plan where armoring restrictions under Goal 18 are relaxed is \$296 per household per year. In comparison, mean WTP for the coastal management plan where current armoring restrictions under Goal 18 are maintained is \$342 per household per year. Both estimates are statistically significant at the 5% level. The mean WTP estimate for the "Relax" scenario is inside the 95% confidence interval of the mean WTP estimate for the "Maintain" scenario, and vice versa. This large overlap of mean WTP distributions suggests that there is not a statistically significant difference between mean WTP for the two future Goal 18 scenarios.

The mean WTP estimates from the DBDC model are lower for both Goal 18 scenarios and both CVM questions (initial and follow-up). According to the initial vote for the "Relax" scenario, the mean WTP for the coastal management plan where Goal 18 is relaxed is \$181 per household per year. According to the follow-up vote, the mean WTP is \$188 per household per year. The WTP estimates from the initial and follow-up CVM questions are very similar for the "Relax" scenario. According to the initial vote for the "Maintain" scenario, the mean WTP for the coastal management plan where Goal 18 is maintained is \$191 per household per year. According to the follow-up vote, the mean WTP is \$156 per household per year. All WTP estimates from the DBDC model are statistically significant at the 5% level. The mean WTP estimates from the initial and follow-up questions in the "Relax" scenario are within each other's 95% confidence interval,

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[76] For simplicity, mean WTP and confidence intervals based on the Krinsky and Robb (1986) method were calculated using the user-created "wtpcikr" package in Stata (Jeanty, 2007). The Delta method was used primarily to verify the "wtpcikr" confidence intervals.

[77] Since mean/median WTP measures are non-linear functions of estimated parameters, they will likely not be normally distributed even when the parameters are (Haab & McConnell, 2002, p. 110). In this case, the distribution of the WTP measure will be asymmetric.

suggesting that there is not a statistically significant difference between these two WTP estimates. However, in the “Maintain” scenario, the mean WTP estimate from the follow-up question is outside of the 95% confidence interval of the mean WTP estimate from the initial question. The significantly smaller WTP estimate from the follow-up question in the “Maintain” scenario is an expected result. Previous studies have found that the WTP function for the follow-up CVM question implies a smaller WTP for the environmental good (Phaneuf & Requate, 2016, p. 588). As in the SBDC models, the mean WTP estimates for the “Relax” scenario are inside the 95% confidence intervals of the mean WTP estimates for the “Maintain” scenario and vice versa. Thus, the DBDC model also suggests that there is not a statistically significant difference between mean WTP for the two future Goal 18 scenarios. However, for both future Goal 18 scenarios, the mean WTP estimates from the DBDC model are outside of the 95% confidence interval of the mean WTP estimate from the SBDC model, and vice versa. This suggests that the mean WTP estimates from the DBDC model are significantly smaller than the mean WTP estimates from the SBDC model, regardless of the future Goal 18 scenario.

3.7 Conclusion

Coastal erosion causes approximately $500 million dollars per year in property damages and loss of land in the U.S. (U.S. Climate Resilience Toolkit, 2021). Rising sea levels will also likely increase the impacts of coastal erosion (Institute for Water Resources, 2022; Sweet et al., 2022). In the continental U.S., an intermediate SLR scenario of 0.9 m by 2100 would place land projected to house 4.2 million people at risk of inundation (Hauer et al., 2016). In Oregon, this SLR scenario would place over 12,000 people at risk of inundation (Hauer et al., 2016). Coastal erosion and rising sea levels impact not only the people who live along the Oregon Coast but also beach recreators and non-recreators who value Oregon’s developed beaches. Since developed beaches tend to be more vulnerable to the effects of erosion, rising sea levels have the potential to increase the vulnerability of oceanfront property to erosion but also decrease safe recreation access to these beaches. Therefore, developed beaches may require active management in the future to preserve safe recreation access for Oregon residents.

This chapter estimates the welfare effects of coastal erosion management policies focused on maintaining safe recreation access on developed Oregon Coast beaches. Our hypothetical coastal management plan pairs Oregon's existing shoreline armoring policy (Goal 18) with a fund that would manage sediment on eroding developed beaches. We use SP survey data from Oregon households to estimate WTP for coastal management policies that have not been implemented before in Oregon in response to erosion and SLR. We use CVM questions to determine respondents' WTP for the proposed coastal management plan. We also use a sample split that allows us to estimate the economic value of relaxing the current shoreline armoring policy.

Results from the primary SBDC model suggest that Oregon residents have a mean WTP of $296 to $342 per household per year for the proposed coastal management plan, regardless of changes to the existing Goal 18 armoring policy. We find a large overlap of the WTP distributions for a policy scenario where Goal 18 is relaxed to allow more armoring and a scenario where Goal 18 is maintained in its current form, which suggests that there is not a statistically significant difference between how much Oregon residents are willing to pay for relaxing and maintaining current armoring restrictions under Goal 18. Results from a robustness check using information from both the initial and follow-up CVM questions in a DBDC model are suggestive of lower WTP for coastal management, under both armoring policy scenarios. This model estimates a mean WTP of $156 to $191 per household per year for the proposed coastal management plan. These mean WTP estimates are outside of the 95% confidence interval of the mean WTP estimate from the SBDC model, which suggests that the DBDC model produces significantly smaller mean WTP estimates. However, like the SBDC model, the DBDC models do not find evidence of a statistically significant difference between WTP estimates for a plan that "relaxes" Goal 18 and a plan that "maintains" Goal 18. Given concerns about the incentive incompatibility of the follow-up question, however, the DBDC results are not used for primary inference.

We find significant welfare gains arising from a coastal management plan that would provide funding to preserve safe recreation access on developed Oregon beaches via sediment management, regardless of changes to the existing shoreline armoring policy, according to the results from the primary SBDC model. Applying our mean WTP values to all Oregon households (1.643 million, from the U.S. Census Bureau (2021)) produces an aggregate economic welfare estimate of approximately $490 to $560 million per year for coastal management that preserves safe recreation access.

We find that respondents are more likely to support the proposed coastal management plan as the percent loss of safe hours that is prevented by the plan increases, as expected. Respondents who said they were aware that the number of safe hours may decrease as erosion on developed beaches increases are also more likely to support the proposed coastal management plan. As the cost of the proposed coastal management plan increases, respondents are less likely to support it, as is anticipated by economic theory. Other factors that influenced respondents' stated WTP included their age, whether they hold a bachelor's degree or higher, whether they identify as liberal, and whether they own a second home on the coast. Individuals' beliefs about shoreline armoring influenced their WTP for coastal management. Respondents who believe that Goal 18 will likely be maintained in its current form and respondents who believe that vulnerable properties should be able to install armoring are more likely to support the proposed coastal management plan. The Goal 18 armoring policy scenario that a respondent was shown ("Relax" or "Maintain") did not appear to influence their stated WTP when considered on its own. However, when this information is interacted with the respondent's armoring beliefs, the future Goal 18 scenario they were presented with often did impact their voting behavior in predictable ways. For example, respondents in the "Relax" scenario who believe that vulnerable properties should be allowed to armor were more likely to support the coastal management plan than respondents in the "Maintain" scenario who do not believe that vulnerable properties should be allowed to armor.

Results from the three primary SBDC models using the ANA validation approach – for both definitions of "non-attendance" – generally corroborate the results from the models that don't correct for stated ANA. The key factors that influenced respondents' stated WTP – e.g., the cost of the plan, the percent loss of safe hours that is prevented, and respondents' armoring beliefs – all had the same signs and usually similar statistical significance between models. Applying the ANA validation model does improve model fit for all three primary SBDC models. Results also suggest that respondents who stated they ignored an attribute may have actually partially attended to that attribute. These "not fully attending" respondents may have non-zero marginal utility for the attributes they stated they ignored.

There are several questions that we intend to address in future work using this survey data. One question is how the welfare effects of coastal erosion management policies differ between beach recreators and non-recreators. We plan to use the combined RP/SP framework developed by Eom and Larson (2006) that motivated our survey design. This framework will allow us to decompose WTP estimates into use and non-use values for coastal erosion management that affects safe recreation access on developed Oregon beaches. The combined RP/SP framework can also be used to verify the economic values estimated by the primary SBDC model and to further investigate whether there is a statistically significant welfare effect of relaxing the current Goal 18 shoreline armoring policy. Another objective is to investigate demographic and response differences between our non-probability sample – an opt-in Qualtrics online panel – and our probability sample – the traditionally preferred address-based sample – once data collection for both sample frames is completed. We also intend to use the survey data to measure the extent of response anomalies such as scenario rejection and protest responses, to investigate the impacts of engagement and social media use on WTP for coastal management, and to examine differences between "forgetful" recreators, non-recreators, and non-forgetful recreators.

3.8 List of Figures

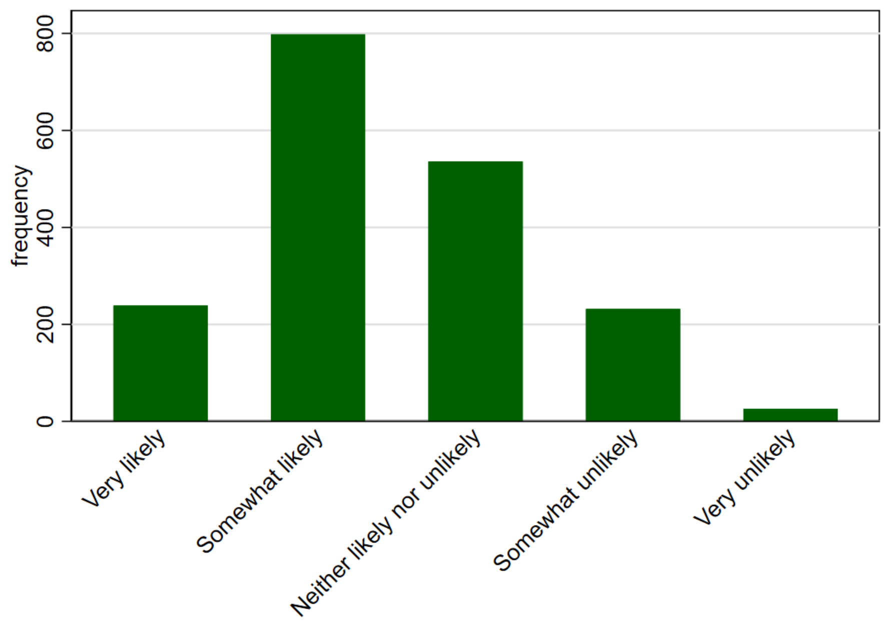


Figure 3.1. Response frequencies for the question "Do you believe it is likely that Goal 18's armoring policy will be maintained in its current form for the foreseeable future?"

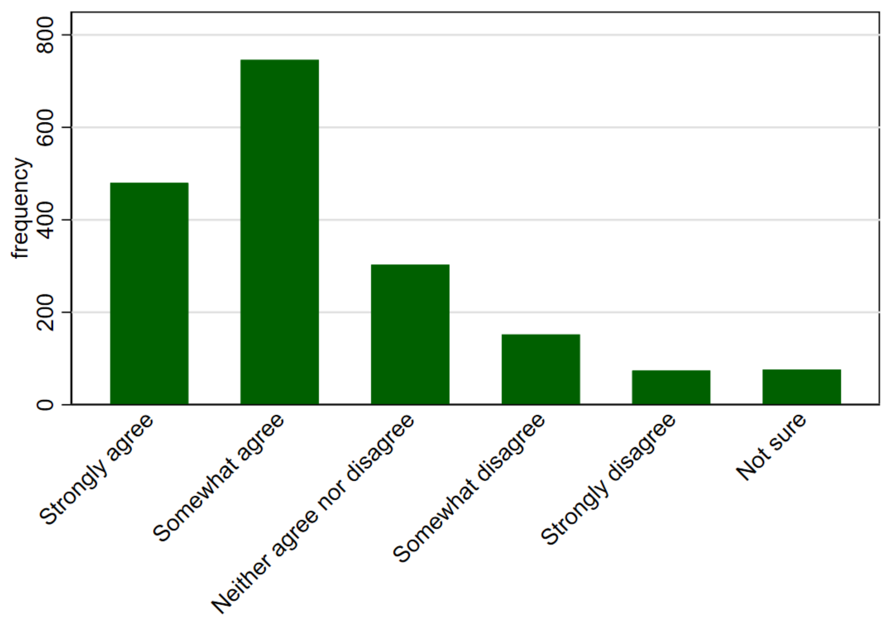
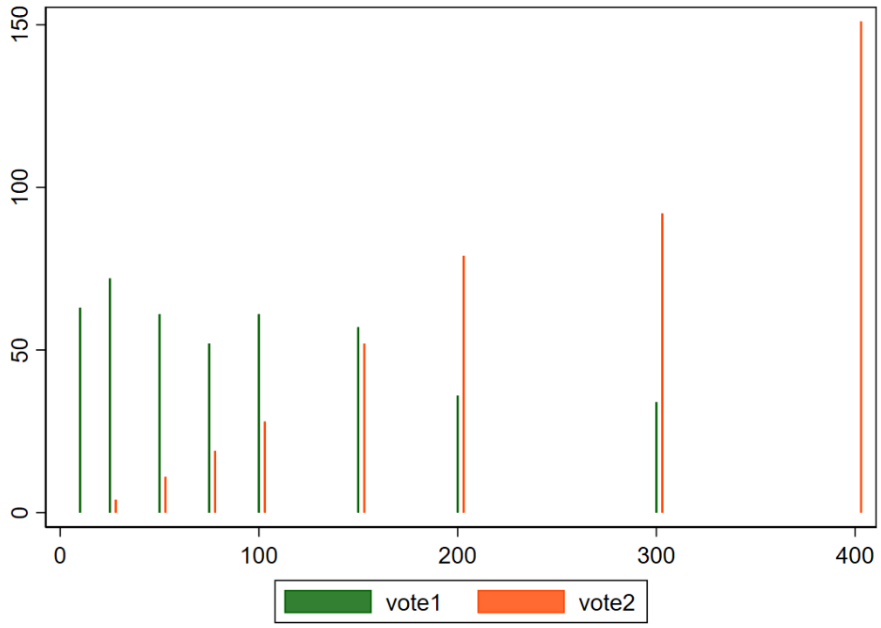
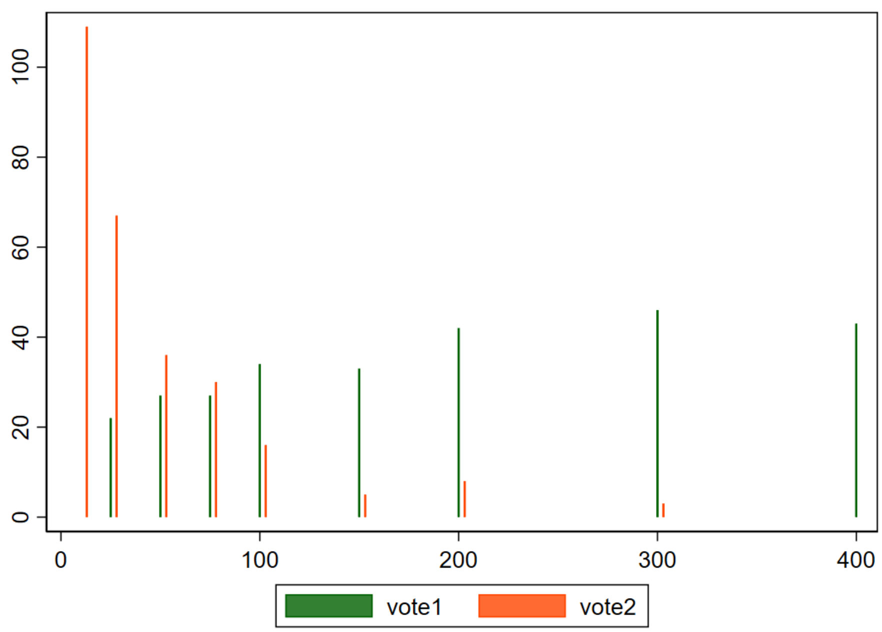


Figure 3.2. Response frequencies for the question “How much do you agree or disagree with the following statement? All properties that are vulnerable to erosion should be able to install shoreline armoring.”



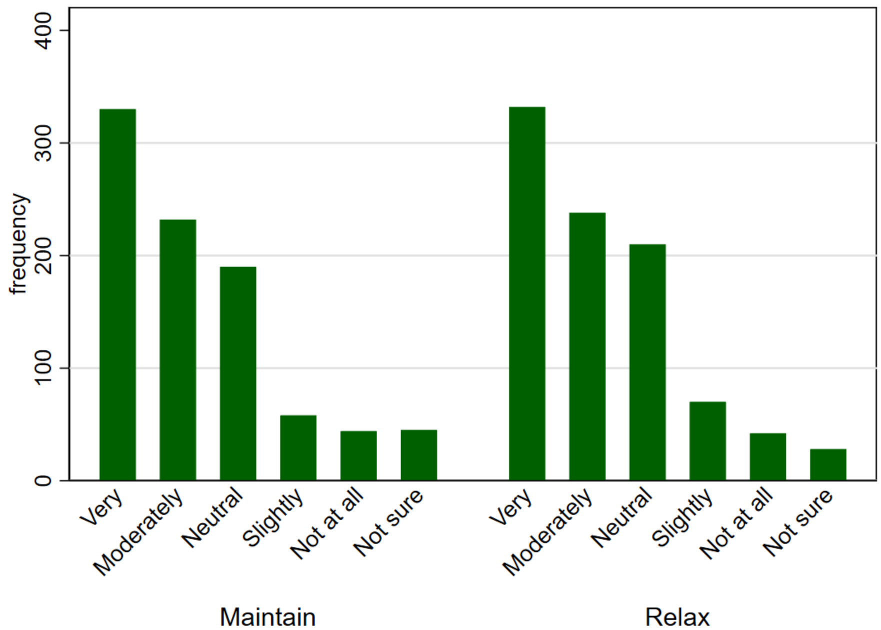
(a)



(b)

Figure 3.3. Count of bid occurrences for the initial and follow-up CVM questions

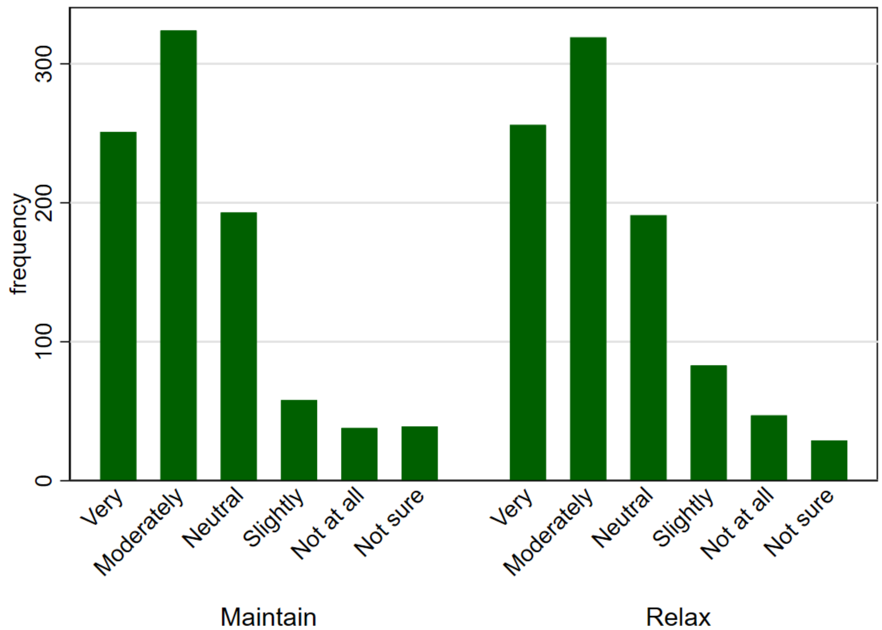
Note: Plot of initial CVM question bid occurrences (vote1) versus follow-up CVM question bid occurrences (vote2). (a) For respondents voting "yes" on the initial question then "no" on the follow-up question. (b) For respondents voting "no" on the initial question then "yes" on the follow-up question.



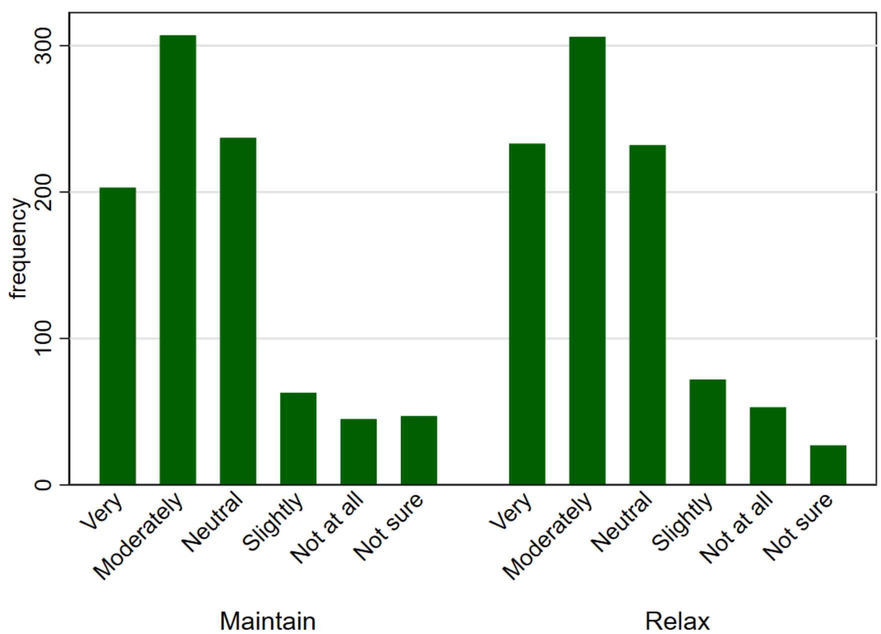
(a)

Figure 3.4. Responses to ANA questions for the three ballot initiative outcomes by shoreline armoring scenario (Maintain or Relax)

Note: Plot of response frequencies for the question "We would like to understand how the outcomes of the ballot initiative influenced your vote. Please select how important each factor was in influencing your vote." (a) The increase to your household's annual state income taxes per year for the next 30 years. (b) Increasing funding for sediment management to prevent a [10, 20, 30, 40] % loss of safe hours for recreation at developed beaches at the highest risk of erosion. (c) [Relaxing, Maintaining] Oregon's Goal 18 shoreline armoring policy so that all oceanfront property owners become eligible to armor the shoreline in front of their homes.



(b)



(c)

Figure 3.4. Responses to ANA questions for the three ballot initiative outcomes by shoreline armoring scenario (Maintain or Relax) (Continued)

3.9 List of Tables

Table 3.1. Variable Definitions and Descriptive Statistics, Qualtrics Panel Sample

Variables	Definitions	Mean	Std Dev
Attributes			
bid1	Initial bid	146.759	(124.301)
bid2	Follow-up bid	170.808	(141.937)
deltasafehours	Percent loss of safe hours that is prevented by the coastal management plan	24.855	(11.180)
relax	In 'Relax' Goal 18 scenario (=1)	0.506	(0.500)
Demographics			
hh_income	Midpoint household income in $10,000s	57.744	(42.047)
age	Age (midpoint of range)	47.538	(17.607)
female	Female (=1)	0.512	(0.500)
race_white	Race: White (=1)	0.859	(0.348)
educ_hs	Education: High School Diploma / GED (=1)	0.212	(0.409)
educ_college_plus	Education: Bachelor's degree or higher (=1)	0.303	(0.460)
marital_single	Marital/partner status: Single (=1)	0.282	(0.450)
hh_size	Size of household	2.642	(1.446)
hh_children	Number of children in household	0.540	(1.000)
employ_fulltime	Employment situation: Employed, working full time (=1)	0.321	(0.467)
employ_retired	Employment situation: Retired (=1)	0.205	(0.404)
employ_student	Employment situation: Full-time student (=1)	0.031	(0.173)
politics_liberal	Politics: Extremely, moderately, or slightly liberal (=1)	0.404	(0.491)
beachwchild_no	Visit beaches with children: No (=1)	0.484	(0.500)
coastalprimary	Primary residence is coastal (=1)	0.068	(0.251)
coastalsecondhome	Second home is coastal (=1)	0.021	(0.145)
Preferences and Familiarity			
recreator	Took day and/or short overnight trips (=1)	0.744	(0.437)
armoring_familiar	Familiar with shoreline armoring (=1)	0.267	(0.442)
nourishment_familiar	Familiar with beach nourishment (=1)	0.196	(0.397)
safehours_aware	Aware that safe hours may decrease (=1)	0.341	(0.474)
g18_future_likely	Goal 18 maintained in future: Very likely or Somewhat likely (=1)	0.566	(0.496)
armoring_allow_agree	Allow vulnerable properties to armor: Strongly agree or Somewhat agree (=1)	0.699	(0.459)
Stated ANA			
ana_likely_cost	Importance of tax increase on vote: Neutral, Slightly, Not at all, Not sure (=1)	0.382	(0.486)
ana_likely_cost_3	Importance of tax increase on vote: Not at all, Not sure (=1)	0.093	(0.291)
ana_likely_safehours	Importance of preserving safe hours loss on vote: Neutral, Slightly, Not at all, Not sure (=1)	0.372	(0.483)
ana_likely_safehours_3	Importance of preserving safe hours loss on vote: Not at all, Not sure (=1)	0.085	(0.279)
ana_likely_goal18	Importance of Goal 18 policy on vote: Neutral, Slightly, Not at all, Not sure (=1)	0.427	(0.495)
ana_likely_goal18_3	Importance of Goal 18 policy on vote: Not at all, Not sure (=1)	0.097	(0.296)

Table 3.1. Variable Definitions and Descriptive Statistics, Qualtrics Panel Sample (Continued)

Variables	Definitions	Mean	Std Dev
Debriefing			
consequential_agree	Consequentiality: Results will influence Oregon policymakers: Strongly agree or Somewhat agree (=1)	0.636	(0.481)
futureerosion_greater	Risk of erosion 30 years from now will be: Much greater or Somewhat greater (=1)	0.828	(0.378)
climatechange	Believe that Oregon's climate is changing (=1)	0.895	(0.307)

Table 3.2. Future annual average safe hours and Δq in levels

Percent loss of safe hours	Annual average safe hours in the status quo future	Δq in levels
10%	9.618	0.962
20%	8.816	1.763
30%	8.138	2.441
40%	7.557	3.023

Table 3.3. SBDC results for the primary models, without the ANA validation model

	Model 1 Coefficient	SE	p-value	Model 2 Coefficient	SE	p-value	Model 3 Coefficient	SE	p-value
Attributes									
bid1	-.0014***	(3.1e-04)	1.6e-05	-.0014***	(2.9e-04)	1.9e-06	-.0014***	(3.1e-04)	7.4e-06
deltasafehours	.0055	(.0034)	.1048	.0055*	(.0033)	.095	.0056*	(.0034)	.0968
relax	-.0612	(.071)	.3888						
(relax=0)*(g18_future_likely=0)				0	(0)	.			
(relax=0)*(g18_future_likely=1)				.2804**	(.1134)	.0134			
(relax=1)*(g18_future_likely=0)				-.0968	(.1197)	.4191			
(relax=1)*(g18_future_likely=1)				.2452**	(.1079)	.023			
(relax=0)*(armoring_allow_agree=0)							0	(0)	.
(relax=0)*(armoring_allow_agree=1)							.5241***	(.1133)	3.8e-06
(relax=1)*(armoring_allow_agree=0)							-.2579**	(.1238)	.0372
(relax=1)*(armoring_allow_agree=1)							.5547***	(.1069)	2.1e-07
Demographics									
hh_income	-9.7e-04	(9.8e-04)	.3221	-9.7e-04	(9.6e-04)	.3141	-9.5e-04	(9.6e-04)	.3209
age	-.0087***	(.0025)	5.2e-04	-.0087***	(.0022)	7.2e-05	-.0087***	(.0023)	1.7e-04
race_white	.1084	(.1028)	.2921	.1082	(.105)	.3025	.1089	(.1063)	.306
educ_college_plus	.1481*	(.0841)	.0784	.1486*	(.0856)	.0824	.1462*	(.0843)	.0828
politics_liberal	.4138***	(.0771)	7.9e-08	.4137***	(.0765)	6.4e-08	.4111***	(.075)	4.2e-08
hh_size	.0248	(.027)	.3581	.0244	(.0286)	.3939	.0239	(.029)	.4094
coastalprimary	.0948	(.1493)	.5257	.0933	(.1562)	.5505	.086	(.1548)	.5785
coastalsecondhome	.479	(.2974)	.1073	.4803	(.2947)	.1031	.4787*	(.2801)	.0874
Preferences and Familiarity									
recreator	.0474	(.0885)	.5922	.0494	(.088)	.5748	.0534	(.0909)	.5571
g18_future_likely	.3119***	(.0751)	3.3e-05				.3164***	(.0793)	6.6e-05
armoring_allow_agree	.6648***	(.0795)	0	.6661***	(.079)	0			
armoring_familiar	-.2035**	(.0902)	.024	-.2037**	(.0923)	.0273	-.2027**	(.0917)	.0271
nourishment_familiar	-.0294	(.1049)	.7791	-.0315	(.1005)	.754	-.0372	(.0993)	.708
safehours_aware	.2669***	(.0832)	.0013	.2681***	(.0854)	.0017	.2681***	(.0866)	.002
constant	-.3015	(.2316)	.1929	-.282	(.2374)	.2348	-.2135	(.2341)	.3619
Observations	1348			1348			1348		
Log likelihood	-813.782			-813.694			-812.076		

Table 3.3. SBDC results for the primary models, without the ANA validation model (Continued)

	Model 1			Model 2			Model 3		
	Coefficient	SE	p-value	Coefficient	SE	p-value	Coefficient	SE	p-value
Pseudo-R-squared	0.110			0.110			0.111		

* p<0.10, ** p<0.05, *** p<0.01

Table 3.4. SBDC results for the primary models, for the ANA validation model and strict ANA definition

	Model 1A Coefficient	SE	p-value	Model 2A Coefficient	SE	p-value	Model 3A Coefficient	SE	p-value
Attributes									
(ana_likely_cost=0)*bid1	-.0021***	(3.7e-04)	1.9e-08	-.0021***	(3.5e-04)	1.1e-09	-.0021***	(3.8e-04)	1.7e-08
(ana_likely_cost=1)*bid1	-5.7e-04	(4.1e-04)	.1698	-6.2e-04	(4.0e-04)	.1216	-5.6e-04	(4.0e-04)	.1607
(ana_likely_safehours=0)*deltasafehours	.0141***	(.0039)	3.3e-04	.0142***	(.0038)	1.7e-04	.0145***	(.0038)	1.6e-04
(ana_likely_safehours=1)*deltasafehours	-.0153***	(.0046)	8.4e-04	-.0155***	(.0047)	9.3e-04	-.0149***	(.0044)	8.0e-04
(relax=0)*(ana_likely_goal18=0)	0	(0)	.						
(relax=0)*(ana_likely_goal18=1)	-.5353***	(.1135)	2.4e-06						
(relax=1)*(ana_likely_goal18=0)	-.1158	(.0959)	.2273						
(relax=1)*(ana_likely_goal18=1)	-.4986***	(.1116)	8.0e-06						
(relax=0)*(ana_likely_goal18=0)*(g18_future_likely=0)				0	(0)	.			
(relax=0)*(ana_likely_goal18=0)*(g18_future_likely=1)				.0484	(.1553)	.7554			
(relax=0)*(ana_likely_goal18=1)*(g18_future_likely=0)				-.5757***	(.1819)	.0015			
(relax=0)*(ana_likely_goal18=1)*(g18_future_likely=1)				-.4711**	(.1862)	.0114			
(relax=1)*(ana_likely_goal18=0)*(g18_future_likely=0)				-.3329*	(.1859)	.0734			
(relax=1)*(ana_likely_goal18=0)*(g18_future_likely=1)				.0621	(.1587)	.6958			
(relax=1)*(ana_likely_goal18=1)*(g18_future_likely=0)				-.4484**	(.1968)	.0227			
(relax=1)*(ana_likely_goal18=1)*(g18_future_likely=1)				-.5291***	(.184)	.004			
(relax=0)*(ana_likely_goal18=0)*(armoring_allow_agree=0)							0	(0)	.
(relax=0)*(ana_likely_goal18=0)*(armoring_allow_agree=1)							.5616***	(.1508)	2.0e-04
(relax=0)*(ana_likely_goal18=1)*(armoring_allow_agree=0)							-.3549*	(.2009)	.0772
(relax=0)*(ana_likely_goal18=1)*(armoring_allow_agree=1)							-.0818	(.1654)	.6206
(relax=1)*(ana_likely_goal18=0)*(armoring_allow_agree=0)							-.4441**	(.1934)	.0217
(relax=1)*(ana_likely_goal18=0)*(armoring_allow_agree=1)							.5411***	(.1451)	1.9e-04

Table 3.4. SBDC results for the primary models, for the ANA validation model and strict ANA definition (Continued)

	Model 1A Coefficient	SE	p-value	Model 2A Coefficient	SE	p-value	Model 3A Coefficient	SE	p-value
(relax=1)*(ana_likely_goal18=1)*(armoring_allow_agree=0)							-.3162*	(.1808)	.0803
(relax=1)*(ana_likely_goal18=1)*(armoring_allow_agree=1)							-.0556	(.1712)	.7455
Demographics									
hh_income	-5.3e-04	(.0011)	.6327	-5.0e-04	(.0011)	.6545	-7.5e-04	(.001)	.4735
age	-.0117***	(.0027)	1.7e-05	-.0116***	(.0024)	9.3e-07	-.0113***	(.0026)	1.4e-05
race_white	.1053	(.1071)	.3258	.1059	(.1093)	.3328	.0951	(.1143)	.4056
educ_college_plus	.0772	(.0906)	.3941	.0778	(.0932)	.4038	.0899	(.0939)	.3384
politics_liberal	.3522***	(.0775)	5.5e-06	.3493***	(.0846)	3.6e-05	.3657***	(.0782)	2.9e-06
hh_size	.0123	(.0309)	.6905	.0093	(.0296)	.7523	.0128	(.0298)	.6682
coastalprimary	.146	(.1708)	.3927	.137	(.1668)	.4113	.1369	(.1769)	.439
coastalsecondhome	.4959	(.3023)	.1009	.4948*	(.2688)	.0657	.5088*	(.3031)	.0933
Preferences and Familiarity									
recreator	.0284	(.0974)	.7705	.0402	(.0982)	.6825	.0349	(.0978)	.7208
g18_future_likely	.1388*	(.0832)	.095				.1402*	(.0835)	.0932
armoring_allow_agree	.5266***	(.0846)	4.9e-10	.5319***	(.0887)	2.0e-09			
armoring_familiar	-.1697*	(.0944)	.0723	-.1654	(.1024)	.1061	-.1596	(.1085)	.1412
nourishment_familiar	-.0958	(.105)	.3611	-.1008	(.1072)	.3469	-.1132	(.1096)	.3015
safehours_aware	.1699*	(.0953)	.0745	.1682*	(.0959)	.0795	.1689*	(.0922)	.0668
constant	.432	(.2659)	.1042	.4848*	(.2753)	.0782	.3927	(.2786)	.1588
Observations	1348			1348			1348		
Log likelihood	-731.092			-728.401			-724.955		
Pseudo-R-squared	0.200			0.203			0.207		

* p<0.10, ** p<0.05, *** p<0.01

Table 3.5. SBDC results for primary models, for the ANA validation model and less strict ANA definition

	Model 1B Coefficient	SE	p-value	Model 2B Coefficient	SE	p-value	Model 3B Coefficient	SE	p-value
Attributes									
(ana_likely_cost_3=0)*bid1	-.0013***	(3.1e-04)	1.7e-05	-.0013***	(3.2e-04)	2.7e-05	-.0014***	(3.0e-04)	5.1e-06
(ana_likely_cost_3=1)*bid1	-.0033***	(.001)	.0013	-.0034***	(.0011)	.002	-.0034***	(9.8e-04)	6.4e-04
(ana_likely_safehours_3=0)*deltasafehours	.0075**	(.0035)	.0333	.0073**	(.0037)	.0459	.0079**	(.0035)	.0234
(ana_likely_safehours_3=1)*deltasafehours	-.0374***	(.0114)	.001	-.0374***	(.0143)	.009	-.037***	(.0133)	.0053
(relax=0)*(ana_likely_goal18_3=0)	0	(0)	.						
(relax=0)*(ana_likely_goal18_3=1)	-.1074	(.2284)	.6383						
(relax=1)*(ana_likely_goal18_3=0)	-.0522	(.0802)	.5146						
(relax=1)*(ana_likely_goal18_3=1)	-.3307	(.2629)	.2085						
(relax=0)*(ana_likely_goal18_3=0)*(g18_future_likely=0)				0	(0)	.			
(relax=0)*(ana_likely_goal18_3=0)*(g18_future_likely=1)				.2074*	(.1188)	.0808			
(relax=0)*(ana_likely_goal18_3=1)*(g18_future_likely=0)				-.1753	(.2911)	.5469			
(relax=0)*(ana_likely_goal18_3=1)*(g18_future_likely=1)				.1898	(.404)	.6385			
(relax=1)*(ana_likely_goal18_3=0)*(g18_future_likely=0)				-.1372	(.1269)	.2794			
(relax=1)*(ana_likely_goal18_3=0)*(g18_future_likely=1)				.214*	(.1165)	.0662			
(relax=1)*(ana_likely_goal18_3=1)*(g18_future_likely=0)				-.1296	(.4237)	.7597			
(relax=1)*(ana_likely_goal18_3=1)*(g18_future_likely=1)				-.3042	(.3257)	.3504			
(relax=0)*(ana_likely_goal18_3=0)*(armoring_allow_agree=0)							0	(0)	.
(relax=0)*(ana_likely_goal18_3=0)*(armoring_allow_agree=1)							.5489***	(.1227)	7.6e-06
(relax=0)*(ana_likely_goal18_3=1)*(armoring_allow_agree=0)							.2276	(.3414)	.505
(relax=0)*(ana_likely_goal18_3=1)*(armor							.1748	(.305)	.5665

Table 3.5. SBDC results for primary models, for the ANA validation model and less strict ANA definition (Continued)

	Model 1B			Model 2B			Model 3B		
	Coefficient	SE	p-value	Coefficient	SE	p-value	Coefficient	SE	p-value
ing_allow_agree=1)									
(relax=1)*(ana_likely_goal18_3=0)*(armoring_allow_agree=0)							-.242	(.1478)	.1016
(relax=1)*(ana_likely_goal18_3=0)*(armoring_allow_agree=1)							.5782***	(.1149)	4.9e-07
(relax=1)*(ana_likely_goal18_3=1)*(armoring_allow_agree=0)							-.2682	(.3397)	.4297
(relax=1)*(ana_likely_goal18_3=1)*(armoring_allow_agree=1)							.1185	(.3946)	.764
Demographics									
hh_income	-7.4e-04	(.001)	.4783	-7.0e-04	(9.5e-04)	.4584	-6.9e-04	(.001)	.5002
age	-.0079***	(.0026)	.0022	-.0081***	(.0026)	.0018	-.0079***	(.0024)	9.3e-04
race_white	.1011	(.1115)	.3642	.1023	(.1259)	.4168	.097	(.112)	.3868
educ_college_plus	.1273	(.0847)	.1328	.1278	(.0936)	.1722	.1254	(.0964)	.1933
politics_liberal	.3767***	(.0848)	8.9e-06	.3767***	(.0765)	8.4e-07	.3713***	(.0815)	5.2e-06
hh_size	.021	(.0284)	.4585	.0195	(.032)	.5426	.017	(.0285)	.5508
coastalprimary	.0847	(.144)	.5566	.0826	(.1628)	.612	.0899	(.1614)	.5776
coastalsecondhome	.739**	(.3266)	.0237	.7362**	(.3229)	.0226	.7533**	(.3041)	.0132
Preferences and Familiarity									
recreator	.0726	(.0922)	.431	.0737	(.0921)	.4231	.075	(.0899)	.404
g18_future_likely	.2702***	(.0777)	5.0e-04				.2715***	(.0758)	3.4e-04
armoring_allow_agree	.6396***	(.0811)	3.1e-15	.6439***	(.0844)	2.4e-14			
armoring_familiar	-.1761*	(.1003)	.0789	-.1833*	(.0965)	.0574	-.1741*	(.1024)	.089
nourishment_familiar	-.0204	(.1067)	.8487	-.0208	(.1077)	.8468	-.0338	(.107)	.7523
safehours_aware	.2321**	(.0912)	.0109	.2372**	(.0933)	.011	.227**	(.0895)	.0112
constant	-.2564	(.2564)	.3173	-.2047	(.2637)	.4376	-.1893	(.2365)	.4236
Observations	1348			1348			1348		
Log likelihood	-780.162			-779.150			-776.982		
Pseudo-R-squared	0.146			0.147			0.150		

* p<0.10, ** p<0.05, *** p<0.01

Table 3.6. DBDC results for the base model

	Initial vote (vote1): Coefficient	SE	p-value	Follow-up vote (vote2): Coefficient	SE	p-value
Attributes						
bid1 or bid2	-.0044***	(3.5e-04)	0	-.0025***	(3.4e-04)	1.6e-13
deltasafehours	.0038	(.0036)	.2894	-.0013	(.0032)	.682
relax	-.0428	(.0818)	.6009	.0803	(.0699)	.2507
Demographics						
hh_income	-8.3e-04	(.001)	.4273	2.5e-04	(.0011)	.8096
age	-.0092***	(.0026)	4.4e-04	-.0056**	(.0025)	.0258
race_white	.0999	(.1217)	.4116	.0786	(.122)	.5191
educ_college_plus	.1956**	(.0956)	.0408	.1981**	(.0865)	.022
politics_liberal	.3906***	(.0855)	4.9e-06	.1865**	(.0747)	.0125
hh_size	.0127	(.0335)	.7036	.0602*	(.032)	.0599
coastalprimary	.1039	(.1699)	.5407	.2018	(.1498)	.1781
coastalsecondhome	.2913	(.3212)	.3645	.6942**	(.3119)	.026
Preferences and Familiarity						
recreator	.0072	(.1016)	.9436	-.0273	(.0894)	.7599
g18_future_likely	.3235***	(.0858)	1.6e-04	.2114***	(.0767)	.0058
armoring_allow_agree	.654***	(.0872)	6.3e-14	.3962***	(.0821)	1.4e-06
armoring_familiar	-.191*	(.1009)	.0585	-.0377	(.0913)	.6797
nourishment_familiar	-.0647	(.1114)	.5613	-.0564	(.1054)	.593
safehours_aware	.306***	(.0954)	.0013	.2669***	(.0935)	.0043
constant	.1947	(.2487)	.4335	-.1594	(.2348)	.4972
Observations	1228					
Log likelihood	-1474.961					
rho	0.612***					
LR test of rho=0	71.210					

* p<0.10, ** p<0.05, *** p<0.01

Table 3.7. WTP estimates

Goal 18 scenario	Mean/Median WTP	95% C.I. (Krinsky and Robb method)	95% C.I. (Delta method)
SBDC Model 1:			
Relax	$296.34	($213.82, $445.05)	($195.34, $397.35)
Maintain	$341.63	($261.00, $496.00)	($239.62, $443.63)
DBDC Model:			
Relax			
Initial vote:	$181.10	($154.94, $210.95)	($153.34, $208.87)
Follow-up vote:	$187.79	($150.71, $229.07)	($150.38, $225.21)
Maintain:			
Initial vote:	$190.82	($166.31, $218.60)	($165.00, $216.64)
Follow-up vote:	$155.78	($112.49, $197.04)	($114.52, $197.05)

4 Conclusion

Developed coastlines provide a variety of amenities to both coastal residents and beach recreators but are also exposed to multiple chronic hazards like erosion and sea level rise (SLR). Chronic coastal hazards pose a challenge for policymakers because they often create conflicting interests between oceanfront landowners and beach recreators. Coastlines in Oregon are also exposed to a severe but low frequency acute hazard: the Cascadia Subduction Zone (CSZ) earthquake and tsunami. Given Oregon's current and chronic under-preparedness for a CSZ earthquake and tsunami, policymakers and emergency managers face the dual policy challenge of increasing public risk salience and preparedness action. My research in this dissertation explores issues of chronic and acute coastal risk management in Oregon using both revealed and stated preference non-market valuation techniques.

Chapter 2 asks the question: Can new information about the risk of a tsunami from a CSZ earthquake change people's risk perceptions? This chapter uses revealed preference methods to investigate whether risk discounts are present in coastal Oregon housing markets following exogenous information shocks about tsunami risk. I study the housing market's response to three sets of risk signals: two exogenous events, a hazard planning change, and the addition of visual cues of tsunami risk in residential neighborhoods. Results for the first analysis suggest that a property inside the primary tsunami inundation zone sells for 6.5% to 8.5% less than a property outside of the zone after the 2011 Tohoku earthquake and tsunami in Japan. However, this risk discount is short-lived and properties inside the primary tsunami inundation zone return to baseline levels within 2.5 years of the Tohoku event. A back-of-the-envelope calculation suggests that this tsunami risk discount had an average capitalization effect of \$6.1 to \$28.7 million dollars in the northern Oregon housing market during its short-lived duration. Results from the second analysis suggest that homes that were not in the original (primary) tsunami inundation zone but are now in a zone vulnerable to inundation from even a small CSZ tsunami sell for 16% to 27% less after the map update. In the third analysis I find evidence of an 8% risk discount for houses that are within 1000' of a roadway blue line denoting entrance into the tsunami inundation zone. This chapter's findings suggest that Oregon policymakers may be able to use risk signals to induce individuals to pay attention to and prepare more for a CSZ event. However, the effect of these signals on risk perceptions would likely disappear over time, as found in the first analysis. Programs that implement visual cues of risk may also be effective at adjusting risk perceptions, as found in the third analysis. These visual cues act as a regular risk reminder every time people pass by them but have the drawback of having highly localized effects.

Chapter 3 asks the question: what is the economic value of coastal erosion management policies that impact safe recreation access on developed beaches? To answer this question I develop a combined revealed and stated preference survey and collect primary survey data from Oregon households. I then use this data to estimate stated preference models and measure Oregon residents' willingness to pay (WTP) for a proposed coastal erosion management policy that affects safe recreation access on developed beaches. I also use a sample split to estimate the economic value of relaxing the current Goal 18 shoreline armoring policy to allow more armoring of private property. I do not find evidence of a statistically significant difference between Oregon residents' willingness to pay for a coastal management plan where Goal 18 is relaxed and a plan where Goal 18 is maintained as is. However, when interacted with the respondent's beliefs about shoreline armoring, I find that the shoreline armoring scenario the respondent was presented with (i.e., relaxing or maintaining Goal 18) often did impact how they voted on the proposed coastal management plan. Results from the primary model suggest that Oregon residents have a mean WTP of $296 to $342 per household per year for the proposed coastal management plan, regardless of changes to the Goal 18 shoreline armoring policy. Applying these estimates to all Oregon households produces an aggregate economic welfare estimate of approximately $490 to $560 million annually for coastal management that preserves safe recreation access.

To my knowledge, my dissertation is the first to investigate tsunami risk perceptions in Oregon using revealed preference methods and the first to investigate preferences for coastal erosion management in Oregon using stated preference methods. Thus, this research contributes new information about Oregon residents' perceptions and preferences regarding coastal risk, which could be useful to policy makers in both emergency and resource management. These findings can help inform policy to increase the Oregon Coast's resilience to chronic and acute hazards.

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6 Appendix

6.1 Expected utility model modified from Hallstrom and Smith (2005) for Chapter 2

Using the expected utility framework, a person's willingness to pay for a risk reduction captures the value of risk reduction (conditional on their previous actions to reduce risk) (Hanley et al., 2007). A simple, two outcome expected utility model, modified from Hallstrom and Smith (2005), demonstrates this in the case of an earthquake and tsunami risk. Assume a person's utility is given by the expected value of their utility of wealth (income). Indirect utility $V(.)$ is defined over annual income minus any hazard insurance (m) and the vector of housing attributes. This vector is decomposed into h, the housing and site attributes that are *not* related to the coastal amenities or risks, and r, the site attribute that relates to both the earthquake/tsunami risk and coastal amenities (such as distance to the shoreline). The household's subjective probability for an earthquake and tsunami at a given location (measured by distance r), with a specific information set (I), and state-contingent utility $U_T(.)$ is given by $p(r, I)$. Their subjective probability of no earthquake and tsunami is $(1 - p(r, I))$. Information, I, can change due to preparedness programs, media coverage, or the occurrence of earthquakes or tsunamis. In this two-outcome scenario, a homeowner's expected utility is given by

$$E(V) = p(r,I)U_T\big(r,h,m - R\big(r,h,i_0,p(r,I)\big) - L(r,h,i_0)\big)$$
$$+\big(1 - p(r,I)\big)U_{NT}\Big(r,h,m - R\big(r,h,i_0,p(r,I)\big)\Big), \qquad (A.1)$$

where $R(.)$ is the annual hedonic price function, i_0 is the insurance rate per dollar of coverage, and L is the monetary loss due to the earthquake and tsunami, net of any insurance coverage. The state where the earthquake and tsunami occurs is labeled (T) and the state where no earthquake occurs is (NT). Individuals maximize their expected utility by selecting a house with attributes h and r conditional on their income (m), information (I), insurance rates (i_0), and the exogenous price function for these site attributes $(R(.))$. Assuming that this hedonic price function is the outcome of housing market equilibrium, we can differentiate it with respect to an attribute of choice to find the implicit marginal price (marginal capitalization effect) for that attribute. However, Hallstrom and Smith (2005) showed that it is difficult to disentangle and interpret estimates for the marginal effect of r $(R_r = \frac{\partial R}{\partial r})$ because distance (r) serves as a proxy for both coastal amenities and risks of tsunami damage. They then show that observing the response of housing prices to an exogenous information shock ($R_I = \frac{\partial R}{\partial I}$), instead, has the potential to reduce confounding multiple influences on the marginal effect. Intuitively, a change in information changes the individual's perceived probability of an earthquake/tsunami $p(r, I)$. This probability change (p_I) is converted into a monetary tradeoff via the implicit price function. So, with an exogenous information shock (∂I), the marginal price from the hedonic isolates the *ex ante* marginal capitalization effect of the information-induced change in subjective risk

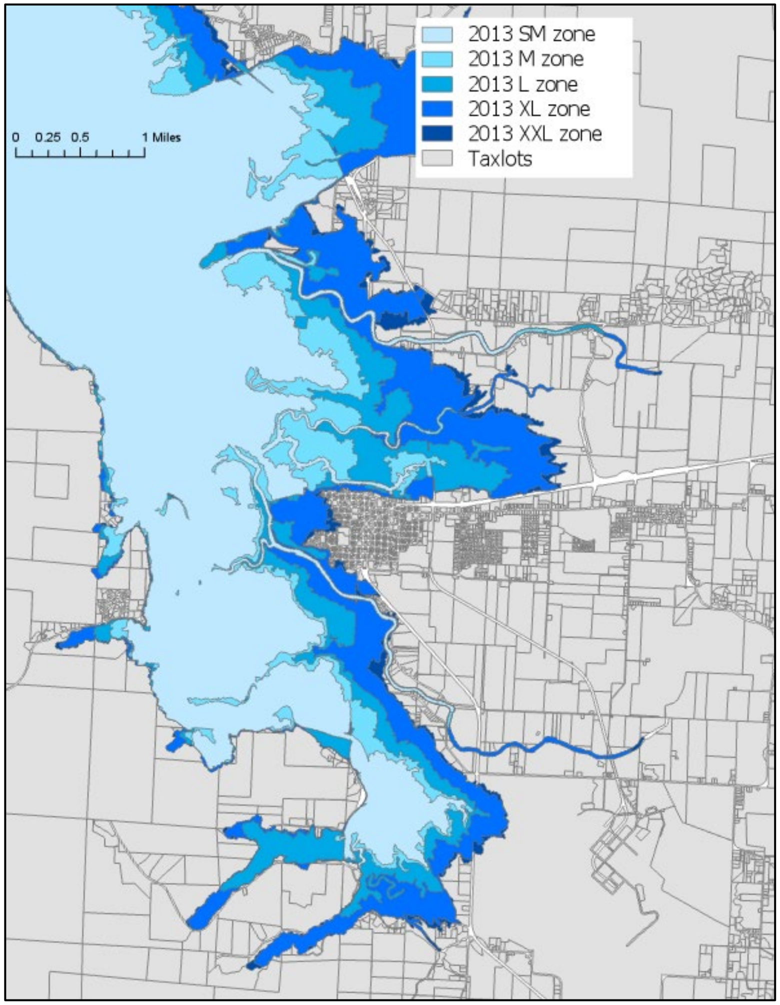
$$R_I = \frac{\partial R}{\partial I} = \frac{p_I(U_T - U_{NT})}{pU_{Tm} + (1-p)U_{NTm}}, \qquad (A.2)$$

where $\frac{U_T - U_{NT}}{pU_{Tm} + (1-p)U_{NTm}}$ is the "incremental option price" for a unit risk reduction in the hazard (T) and p_I is the change in the perceived probability of an earthquake and tsunami due to the information shock I.[78] Under my hypothesis that the tsunami risk signals – i.e., information shocks – impacted Oregonians' risk perceptions about the Cascadia earthquake and tsunami, the sign of the *ex ante* marginal capitalization effect (R_I) is expected to be negative for all information shocks. I expect that each information shock (I) increased individual's perceived probability of an earthquake/tsunami $p(r, I)$. The change in perceived risk (p_I) should then decrease the hedonic price function ($R(r, h, i_0, p(r, I))$).

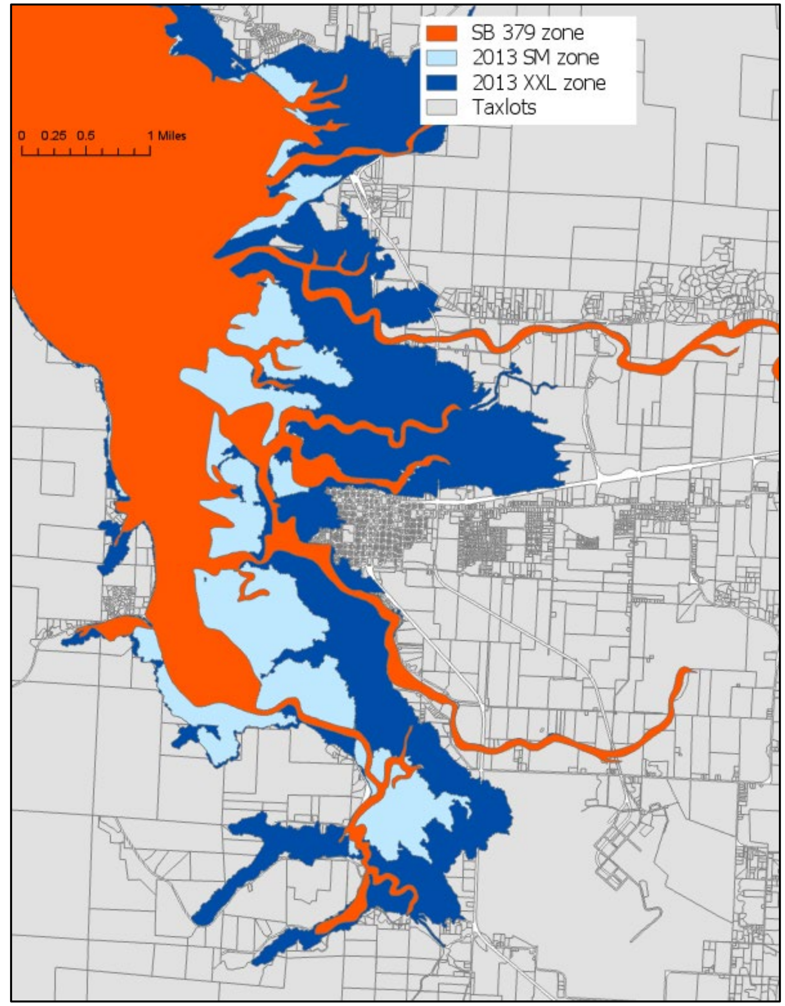
[78] Note that what I am calling the "incremental option price," i.e., the maximum payment that an individual would make under uncertainty to reduce the probability of the earthquake and tsunami state, is the term that converts the change in probability into monetary terms. Also note that calling this term "incremental option price" is no longer technically correct since we are not able to interpret the marginal effects of the hedonic price function as MWTP. For conciseness, I keep its original label here.

6.2 Tsunami inundation zone scenario comparison for Chapter 2

Figure A1(a) presents the five 2013 tsunami inundation scenarios for the town of Tillamook, the Tillamook County seat of 4,935 people (Secretary of State, n.d.-a). The five scenarios are known as the SM, M, L, XL, and XXL tsunami inundation scenarios. Figure A1(b) compares the SM and XXL 2013 scenarios (blue) to the 1995 SB 379 (orange) scenario for Tillamook. The differences between the two map series reflect the differences in scientific information and modeling effort between 1995 and 2013. Figure A2 maps the Census block groups for this same area in Tillamook to illustrate the approximate scale of a Census block group for this sample.



(a)



(b)

Figure A1. Tsunami inundation scenarios for the city of Tillamook, Tillamook County

Note: (a) Tsunami inundation zones given by the five 2013 tsunami scenarios: SM, M, L, XL, XXL. (b) Comparison of tsunami inundation zones between the 1995 SB 379 line (orange) and the SM and XXL 2013 scenarios (blue).

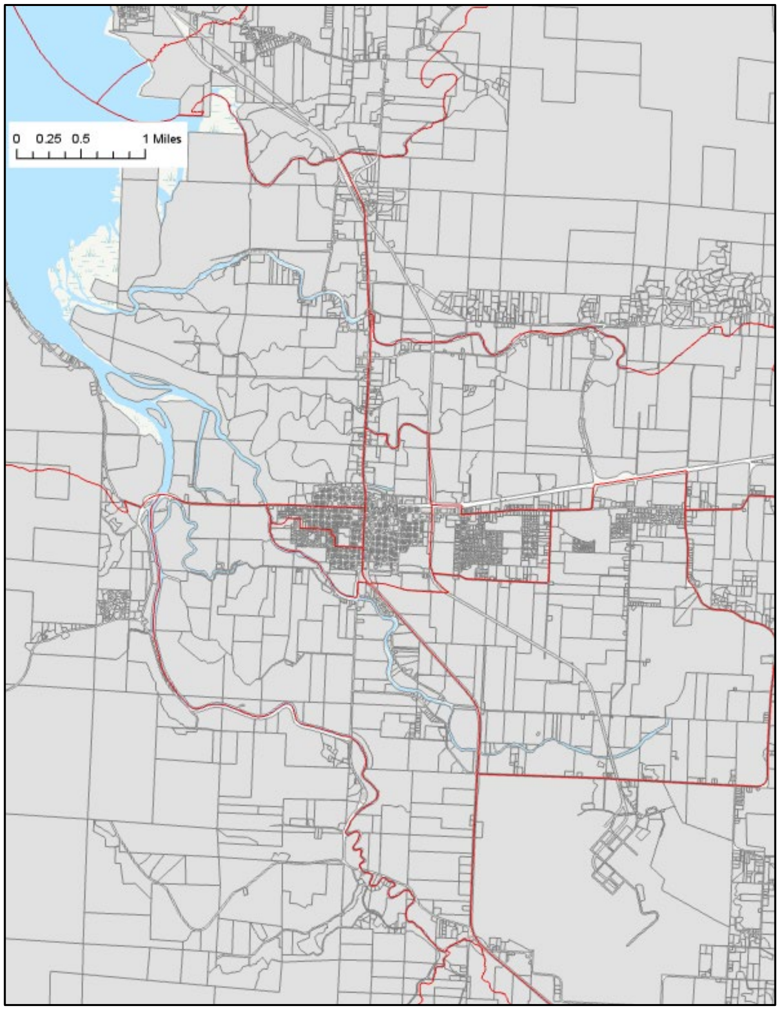


Figure A2. Approximate scale of Census block groups in the city of Tillamook (red)

6.3 Additional summary statistics for Chapter 2

Table A1. Variable Definitions and Descriptive Statistics, First Analysis Sample, 2009-2017

Variables	Mean	Std Dev	Min	Max
Event				
Sold after 2011 Tohoku EQ (tohoku=1)	0.81	(0.39)	0	1
Sold after 2015 article (article=1)	0.33	(0.47)	0	1
Treatment				
Inside 1995 SB 379 tsunami zone (sb379=1)	0.27	(0.44)	0	1
Inside 2013 XXL tsunami zone (xxl2013=1)	0.49	(0.50)	0	1
Inside 2013 XL tsunami zone (xl2013=1)	0.47	(0.50)	0	1
Inside 2013 L tsunami zone (l2013=1)	0.34	(0.48)	0	1
Inside 2013 M tsunami zone (m2013=1)	0.25	(0.43)	0	1
Inside 2013 SM tsunami zone (sm2013=1)	0.13	(0.34)	0	1
Structural				
Sale price (2019 constant dollars)	311,091.80	(170,179.49)	31,393	1,003,509
Bedrooms	2.83	(0.93)	1	8
Bathrooms	2.02	(0.77)	.5	6
Indoor square footage	1,680.60	(705.27)	208	7,265
Total acreage (equal to indoor area if apartment)	0.40	(2.17)	.0057	115
Effective age of property (2018 - remodel year)	36.09	(25.25)	0	137
Heating (=1)	0.94	(0.24)	0	1
Fireplace (=1)	0.65	(0.48)	0	1
Garage (=1)	0.75	(0.43)	0	1
Carport (=1)	0.04	(0.19)	0	1
Deck (=1)	0.12	(0.33)	0	1
Patio (=1)	0.18	(0.38)	0	1
Fencing (=1)	0.15	(0.36)	0	1
Goal 18 eligible (=1)	0.04	(0.19)	0	1
Has shoreline armoring (=1)	0.01	(0.11)	0	1
Location				
Special Flood Hazard Area (SFHA) (=1)	0.12	(0.32)	0	1
Elevation (ft)	77.06	(69.47)	0	685
Slope (angular degrees of slope)	2.46	(4.33)	0	32
Distance to nearest beach access point (ft)	3,742.94	(6,488.61)	0	58,260
Distance to ocean shoreline (ft)	13,613.77	(21,683.77)	0	171,886
Oceanfront (=1)	0.05	(0.22)	0	1
Distance to nearest water body (lake, pond, bay) (ft)	6,833.93	(8,262.88)	0	54,308
Distance to nearest river (ft)	7,311.76	(7,987.83)	0	42,105
Distance to nearest state park or public land (ft)	24,815.12	(25,972.40)	0	97,127
Distance to nearest national park or public land (ft)	18,365.10	(17,023.94)	0	74,910
Distance to nearest highway or interstate (ft)	3,164.46	(5,049.39)	0	36,871
Distance to nearest major road (ft)	3,761.70	(6,169.40)	0	36,909
Distance to nearest railroad (ft)	72,756.88	(58,552.91)	21	174,281
Distance to nearest airport (ft)	30,689.69	(19,410.33)	163	83,958
Distance to nearest k-12 school (ft)	14,045.38	(14,543.35)	102	70,987
Distance to nearest central business district (city) (ft)	10,533.27	(10,258.51)	0	71,539
Distance to nearest wastewater treatment plant (ft)	14,574.16	(10,861.35)	44	78,773
Distance to nearest fire station (ft)	6,032.35	(4,741.50)	.85	33,221
Distance to nearest law enforcement station (ft)	31,602.66	(38,338.10)	108	160,319
Distance to nearest hospital (ft)	47,994.08	(43,389.19)	229	167,748

Table A2. Variable Definitions and Descriptive Statistics, Second Analysis Sample, Model 1, 2011-2015

Variables	Mean	Std Dev	Min	Max
Event				
Sold after 2013 map change (after 10/2/13) (newmaps=1)	0.59	(0.49)	0	1
Treatment				
Inside 2013 XXL tsunami zone (xxl2013=1)	0.27	(0.44)	0	1
Inside 2013 XL tsunami zone (xl2013=1)	0.24	(0.43)	0	1
Inside 2013 L tsunami zone (l2013=1)	0.11	(0.31)	0	1
Inside 2013 M tsunami zone (m2013=1)	0.04	(0.19)	0	1
Inside 2013 SM tsunami zone (sm2013=1)	0.01	(0.10)	0	1
Structural				
Sale price (2019 constant dollars)	296,220.40	(163,439.01)	31,540	1,003,509
Bedrooms	2.87	(0.89)	1	8
Bathrooms	2.01	(0.74)	.5	6
Indoor square footage	1,658.07	(714.75)	96	6,577
Total acreage (equal to indoor area if apartment)	0.51	(1.95)	.0023	112
Effective age of property (2018 - remodel year)	35.98	(24.94)	0	137
Heating (=1)	0.77	(0.42)	0	1
Fireplace (=1)	0.57	(0.49)	0	1
Garage (=1)	0.71	(0.45)	0	1
Carport (=1)	0.03	(0.18)	0	1
Deck (=1)	0.09	(0.29)	0	1
Patio (=1)	0.18	(0.38)	0	1
Fencing (=1)	0.12	(0.33)	0	1
Goal 18 eligible (=1)	0.02	(0.13)	0	1
Has shoreline armoring (=1)	0.00	(0.05)	0	1
Location				
Special Flood Hazard Area (SFHA) (=1)	0.03	(0.17)	0	1
Elevation (ft)	99.83	(82.15)	0	1,146
Slope (angular degrees of slope)	1.85	(4.26)	0	32
Distance to nearest beach access point (ft)	5,065.92	(8,094.82)	0	74,110
Distance to ocean shoreline (ft)	16,628.09	(20,257.40)	0	137,602
Oceanfront (=1)	0.03	(0.16)	0	1
Distance to nearest water body (lake, pond, bay) (ft)	6,878.71	(7,469.41)	0	60,075
Distance to nearest river (ft)	7,264.15	(7,481.09)	0	42,105
Distance to nearest state park or public land (ft)	23,041.32	(25,780.63)	0	116,124
Distance to nearest national park or public land (ft)	14,391.50	(14,826.84)	0	74,910
Distance to nearest highway or interstate (ft)	3,468.93	(5,347.09)	0	63,013
Distance to nearest major road (ft)	2,805.27	(4,675.05)	0	36,683
Distance to nearest railroad (ft)	85,412.48	(106,850.86)	0	394,958
Distance to nearest airport (ft)	29,597.25	(20,233.50)	474	121,345
Distance to nearest k-12 school (ft)	13,697.00	(15,220.24)	152	99,992
Distance to nearest central business district (city) (ft)	10,798.03	(11,022.24)	0	99,593
Distance to nearest wastewater treatment plant (ft)	16,868.88	(21,145.99)	220	166,371
Distance to nearest fire station (ft)	6,385.07	(5,420.26)	3.4	62,965
Distance to nearest law enforcement station (ft)	25,640.00	(32,459.64)	157	160,319
Distance to nearest hospital (ft)	47,723.01	(48,161.14)	229	176,429

Table A3. Variable Definitions and Descriptive Statistics, by SM2013, Second Analysis Sample, Model 5, 2011-2015

	Outside SM2013 inundation zone		Inside SM2013 inundation zone		
	Mean	Std Dev	Mean	Std Dev	Standardized diff. in means
Event					
Sold after 2013 map change (after 10/2/13) (newmaps=1)	0.59	(0.49)	0.53	(0.50)	-
Treatment					
Inside 2013 XXL tsunami zone (xxl2013=1)	0.00	(0.00)	1.00	(0.00)	-
Inside 2013 XL tsunami zone (xl2013=1)	0.00	(0.00)	1.00	(0.00)	-
Inside 2013 L tsunami zone (l2013=1)	0.00	(0.00)	1.00	(0.00)	-
Inside 2013 M tsunami zone (m2013=1)	0.00	(0.00)	1.00	(0.00)	-
Inside 2013 SM tsunami zone (sm2013=1)	0.00	(0.00)	1.00	(0.00)	-
Structural					
Sale price (2019 constant dollars)	295,066.23	(159,063.84)	231,780.57	(148,962.81)	0.41
Bedrooms	2.90	(0.88)	2.60	(0.96)	0.32
Bathrooms	2.01	(0.75)	1.63	(0.75)	0.51
Indoor square footage	1,675.28	(718.92)	1,400.46	(557.99)	0.43
Total acreage (equal to indoor area if apartment)	0.45	(1.40)	1.36	(5.21)	-0.24
Effective age of property (2018 - remodel year)	37.04	(25.55)	40.62	(25.24)	-0.14
Heating (=1)	0.77	(0.42)	0.81	(0.39)	-0.10
Fireplace (=1)	0.58	(0.49)	0.57	(0.50)	0.02
Garage (=1)	0.72	(0.45)	0.74	(0.44)	-0.05
Carport (=1)	0.04	(0.19)	0.01	(0.11)	0.16
Deck (=1)	0.09	(0.28)	0.16	(0.37)	-0.22
Patio (=1)	0.17	(0.38)	0.20	(0.40)	-0.07
Fencing (=1)	0.10	(0.31)	0.15	(0.36)	-0.13
Goal 18 eligible (=1)	0.01	(0.10)	0.06	(0.24)	-0.27
Has shoreline armoring (=1)	0.00	(0.02)	0.02	(0.16)	-0.22
Location					
Special Flood Hazard Area (SFHA) (=1)	0.01	(0.12)	0.33	(0.47)	-0.92
Elevation (ft)	121.66	(86.13)	16.40	(11.41)	1.71

Table A3. Variable Definitions and Descriptive Statistics, by SM2013, Second Analysis Sample, Model 5, 2011-2015 (Continued)

	Outside SM2013 inundation zone		Inside SM2013 inundation zone		
	Mean	Std Dev	Mean	Std Dev	Standardized diff. in means
Slope (angular degrees of slope)	2.04	(4.72)	1.71	(2.68)	0.08
Distance to nearest beach access point (ft)	5,224.44	(8,615.61)	5,212.64	(8,302.32)	0.00
Distance to ocean shoreline (ft)	18,437.87	(21,378.82)	24,432.62	(25,851.42)	-0.25
Oceanfront (=1)	0.02	(0.14)	0.14	(0.34)	-0.44
Distance to nearest water body (lake, pond, bay) (ft)	6,525.36	(6,387.23)	6,934.80	(7,488.63)	-0.06
Distance to nearest river (ft)	7,041.95	(7,623.14)	2,397.07	(4,759.82)	0.73
Distance to nearest state park or public land (ft)	21,838.01	(24,637.87)	28,713.47	(42,249.45)	-0.20
Distance to nearest national park or public land (ft)	12,909.37	(11,189.78)	21,961.15	(20,433.28)	-0.55
Distance to nearest highway or interstate (ft)	3,062.55	(4,486.45)	2,618.67	(3,738.27)	0.11
Distance to nearest major road (ft)	2,388.29	(4,137.68)	4,412.83	(4,579.18)	-0.46
Distance to nearest railroad (ft)	84,464.79	(110,408.42)	91,671.84	(130,852.13)	-0.06
Distance to nearest airport (ft)	29,363.18	(20,422.74)	29,765.47	(22,999.55)	-0.02
Distance to nearest k-12 school (ft)	12,305.84	(15,174.39)	13,800.28	(17,319.14)	-0.09
Distance to nearest central business district (city) (ft)	10,406.69	(11,050.82)	12,797.71	(16,355.74)	-0.17
Distance to nearest wastewater treatment plant (ft)	15,253.71	(16,461.63)	41,222.65	(57,833.19)	-0.61
Distance to nearest fire station (ft)	6,106.20	(5,134.15)	6,992.28	(7,554.86)	-0.14
Distance to nearest law enforcement station (ft)	23,176.83	(30,892.87)	19,219.65	(25,149.62)	0.14
Distance to nearest hospital (ft)	44,383.06	(48,983.38)	32,092.84	(30,190.89)	0.30
Observations	5348		81		

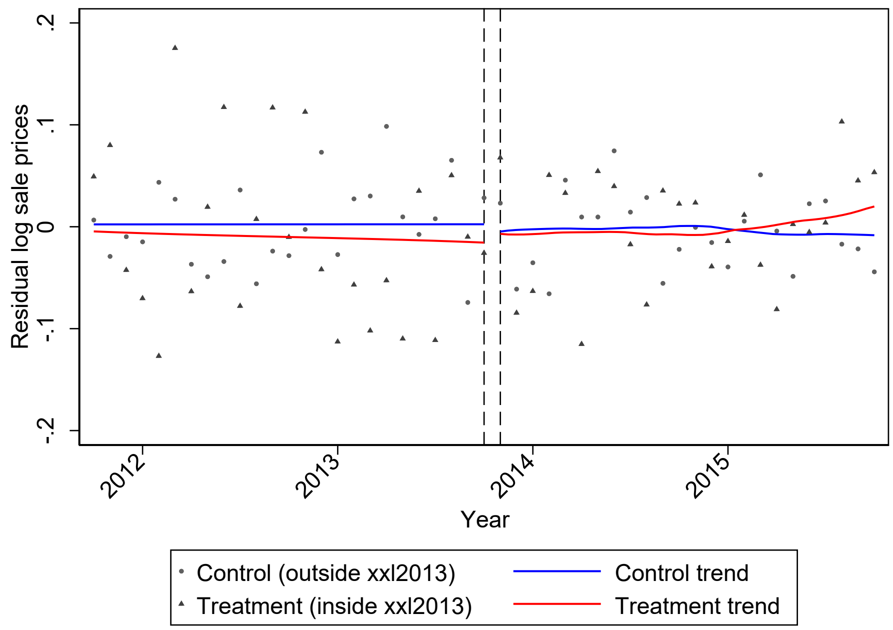
Table A4. Variable Definitions and Descriptive Statistics, by treatment, Third Analysis Sample, Model 62, 2014-2019

	Outside blue line neighborhood (>1000’)		Inside blue line neighborhood (≤1000’)		
	Mean	Std Dev	Mean	Std Dev	Standardized diff. in means
Event					
Sold after blue line was installed (installation=1)	0.15	(0.35)	0.12	(0.33)	-
Structural					
Sale price (2019 constant dollars)	314,429.10	(162,377.00)	309,337.13	(152,322.52)	0.03
Bedrooms	2.80	(1.00)	2.73	(0.97)	0.07
Bathrooms	1.97	(0.80)	2.04	(0.83)	-0.09
Indoor square footage	1,430.40	(740.11)	1,516.89	(684.84)	-0.12
Total acreage (equal to indoor area if apartment)	0.16	(0.28)	0.13	(0.12)	0.10
Effective age of property (2018 - remodel year)	42.96	(30.13)	44.09	(29.18)	-0.04
Heating (=1)	0.78	(0.42)	0.84	(0.37)	-0.15
Fireplace (=1)	0.60	(0.49)	0.65	(0.48)	-0.10
Garage (=1)	0.61	(0.49)	0.62	(0.49)	-0.01
Carport (=1)	0.05	(0.21)	0.03	(0.17)	0.08
Deck (=1)	0.06	(0.24)	0.08	(0.27)	-0.07
Patio (=1)	0.07	(0.25)	0.05	(0.21)	0.09
Fencing (=1)	0.13	(0.34)	0.11	(0.31)	0.09
Goal 18 eligible (=1)	0.04	(0.19)	0.04	(0.19)	0.00
Has shoreline armoring (=1)	0.00	(0.07)	0.01	(0.12)	-0.09
Location					
Special Flood Hazard Area (SFHA) (=1)	0.08	(0.27)	0.02	(0.15)	0.25
Elevation (ft)	78.54	(54.42)	72.78	(39.94)	0.12
Slope (angular degrees of slope)	1.26	(3.22)	1.17	(3.52)	0.03
Distance to nearest beach access point (ft)	1,753.50	(1,200.59)	1,567.82	(967.45)	0.17
Distance to ocean shoreline (ft)	7,004.29	(11,446.24)	5,277.00	(9,535.52)	0.16
Oceanfront (=1)	0.05	(0.21)	0.04	(0.20)	0.01
Distance to nearest water body (lake, pond, bay) (ft)	8,136.05	(10,203.83)	7,562.50	(8,366.73)	0.06
Distance to nearest river	8,247.52	(7,645.72)	9,927.78	(7,845.34)	-0.22

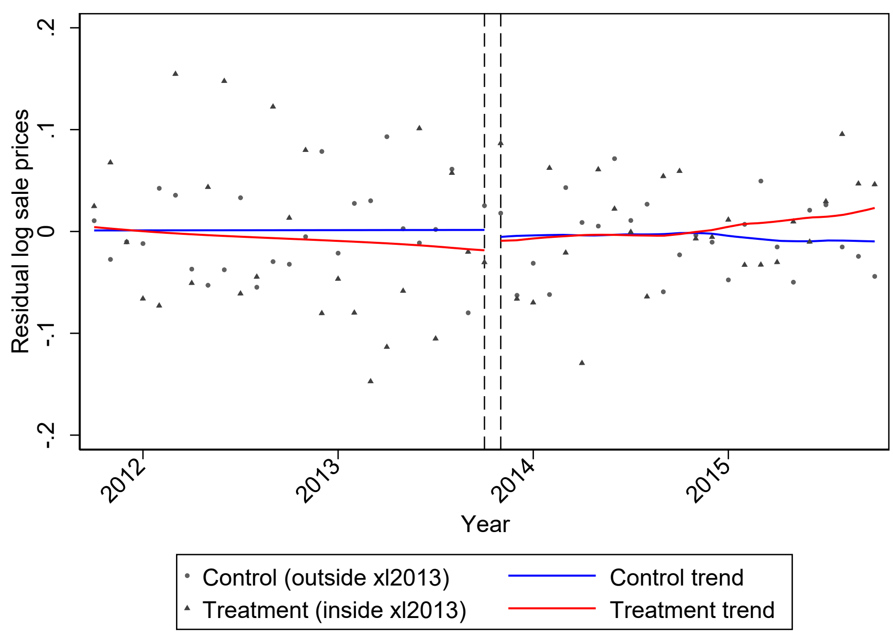
Table A4. Variable Definitions and Descriptive Statistics, by treatment, Third Analysis Sample, Model 62, 2014-2019 (Continued)

	Outside blue line neighborhood (>1000')		Inside blue line neighborhood (≤1000')		
	Mean	Std Dev	Mean	Std Dev	Standardized diff. in means
(ft)					
Distance to nearest state park or public land (ft)	39,778.11	(34,629.56)	45,823.91	(35,030.73)	-0.17
Distance to nearest national park or public land (ft)	9,977.76	(6,980.00)	11,159.64	(6,648.46)	-0.17
Distance to nearest highway or interstate (ft)	2,164.05	(2,831.09)	2,212.84	(2,349.62)	-0.02
Distance to nearest major road (ft)	983.77	(1,240.99)	1,076.13	(1,232.81)	-0.07
Distance to nearest railroad (ft)	102,717.54	(73,404.78)	116,566.90	(79,155.34)	-0.18
Distance to nearest airport (ft)	36,613.15	(18,433.78)	38,472.42	(18,765.22)	-0.10
Distance to nearest k-12 school (ft)	7,071.64	(7,165.77)	6,593.33	(6,142.44)	0.07
Distance to nearest central business district (city) (ft)	8,889.72	(6,145.68)	8,469.24	(5,351.54)	0.07
Distance to nearest wastewater treatment plant (ft)	15,418.32	(21,860.83)	20,407.66	(28,665.84)	-0.20
Distance to nearest fire station (ft)	4,308.63	(3,070.37)	4,416.41	(3,412.12)	-0.03
Distance to nearest law enforcement station (ft)	22,679.32	(39,096.02)	18,610.70	(33,196.20)	0.11
Distance to nearest hospital (ft)	35,715.25	(50,321.69)	29,175.45	(45,741.74)	0.14
Observations	822		512		

6.4 Price trends plots for the second analysis for Chapter 2



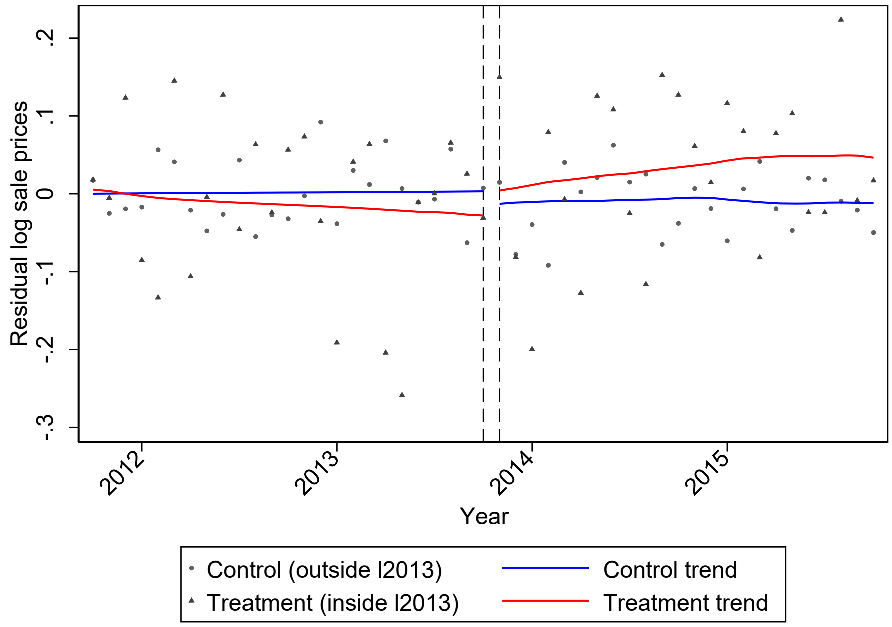
(a)



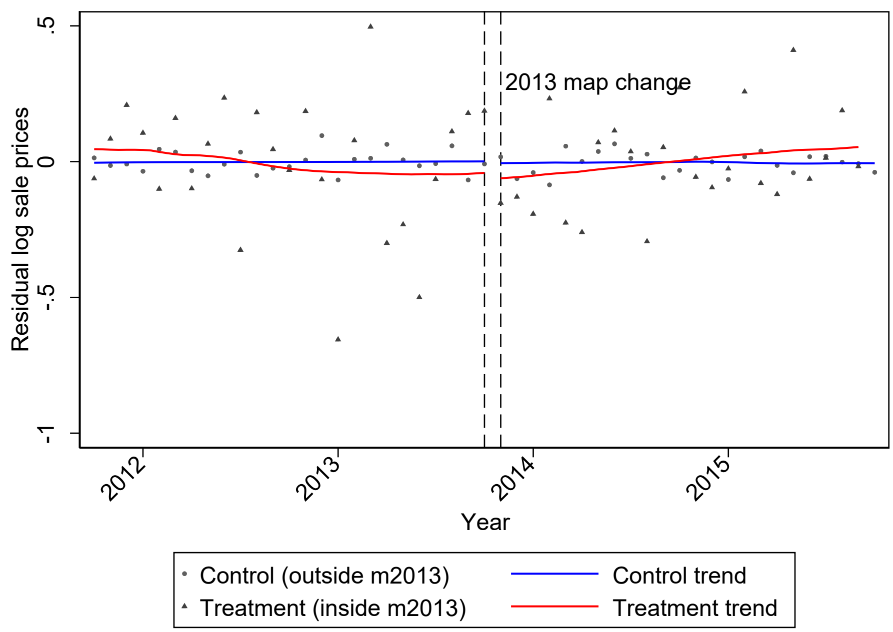
(b)

Figure A3. Housing price trends inside and outside of the treatment inundation line for the second analysis

Note: Plot of residual (log) sale prices net of structural attributes, location covariates, and fixed effects aggregated by month with local polynomial trend lines for the seven coastal counties. The time range is 2 years before and after the 2013 map change. Figures (a)-(e) present plots for Models 1 through 5 and treatment inundation lines XXL, XL, L, M, and SM, respectively.

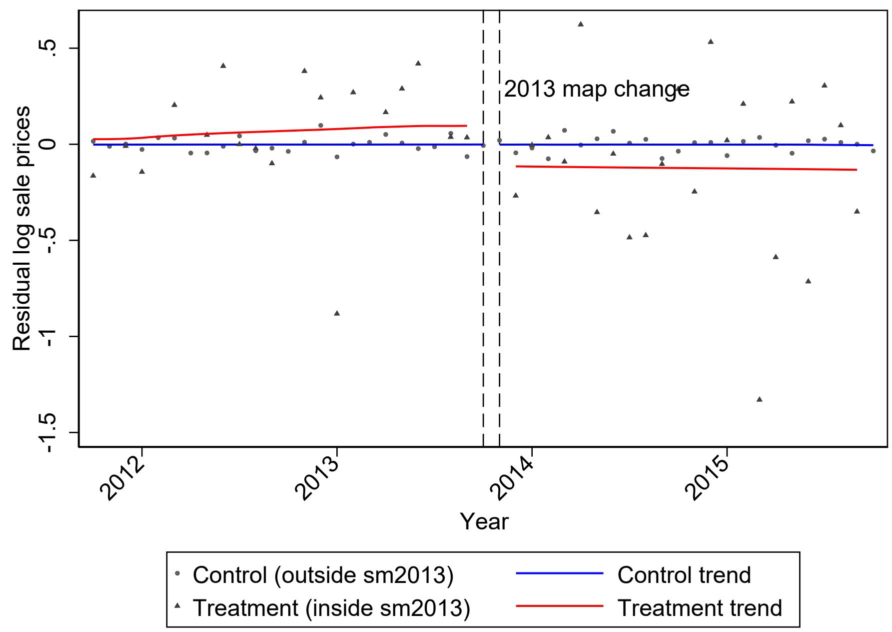


(c)



(d)

Figure A3. Housing price trends inside and outside of the treatment inundation line for the second analysis (Continued)



(e)

Figure A3. Housing price trends inside and outside of the treatment inundation line for the second analysis (Continued)

6.5 Tsunami blue line overlap cases for Chapter 2

Two binary indicators are needed for the DID and DDD regressions: treatment and event. Treatment defines whether the transaction is adjacent to a blue line, e.g., inside that blue line’s neighborhood (treatment buffer) versus not inside the blue line’s neighborhood (control buffer). Event defines whether the transaction occurs after the blue line was installed. This means that each transaction can fall into one of four categories: treatment post-installation, treatment pre-installation, control post-installation, and control pre-installation.

For the following explanations we will use the two diagrams in Figure A4. In both diagrams the small circular buffer (2000’) determines the treatment buffer and the large circular buffer (4000’) determines the control buffer. So, the “2017” blue line (blue square) falls in the treatment buffer and the “2018” blue line falls in the control buffer. The transaction (black point labeled “2016”) falls in both a treatment buffer of one blue line and a control buffer of another blue line. The diagram in Figure A4(a) is a more intuitive way of representing what’s happening. The transaction falls in both the treatment and control buffers of the blue lines but the buffers are centered on the blue lines. This is equivalent to the diagram on the right but not technically accurate. The diagram in Figure A4(b) is an accurate portrayal of how this is coded in Stata, i.e., the transaction has distance buffers around it that hold blue lines. For the sake of building intuition, I will use the diagram in Figure A4(a) to visualize the following overlap cases.

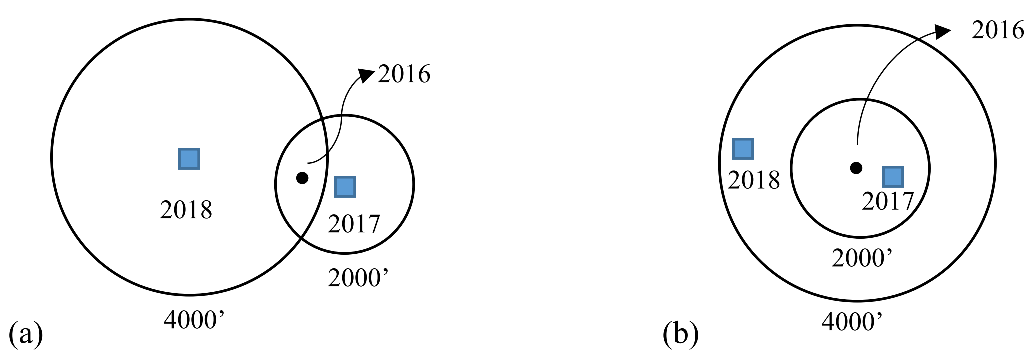


Figure A4. Diagram of circular treatment and control buffers around blue lines and transactions

Note: (a) Treatment and control buffers (circles labeled 2000’ and 4000’, respectively) are centered on their respective blue lines (blue squares labeled 2017 and 2018, respectively). (b) Treatment and control buffers are centered on the transaction (black point labeled 2016).

The central idea of treatment and event assignment is that “earliest supersedes nearest.” If a transaction lies within a given buffer distance of two different blue lines and one of the blue lines is installed before the transaction and the other is installed after the transaction, I use the first-installed blue line as the reference point, not the nearest blue line. In case there is a tie for earliest because multiple blue lines were installed at the same time, then the nearest blue line is chosen. Then, I determine whether the transaction occurred before or after this reference blue line was installed. This is used to create the “event” variable(s). To create “treatment” variable(s), I tried to consider all possible cases of buffer overlap. The key question is how should we treat transactions that fall in one blue line’s “treatment” buffer (e.g., 2000’ buffer) and another blue line’s “control” buffer (e.g., 4000’ buffer)? Which blue line should be chosen as the appropriate reference point? There are nine total unique cases that can occur when a treatment buffer and control buffer overlap for a transaction. I look at 11 cases but cases 1 and 2 are identical as are cases 3 and 4. Figures A5 through A15 present diagrams of these 11 overlap cases.

When the transaction occurs between the “treated” and “control” blue line installation dates, timing matters. In this case, “earliest supersedes nearest” and the first-installed blue line is the reference point. Cases 5, 6, 8, and 10 apply to this situation. When the transaction occurs before (after) both the “treated” and “control” blue lines are installed, timing “doesn’t matter” because the transaction is going to be labeled pre-installation (post-installation) regardless of which blue line is chosen as the reference point. In this case, *distance* determines whether the transaction is labeled as a treated or control, i.e., the “earliest supersedes nearest” principle is not applied in these cases because timing “doesn’t matter.” For example, if the transaction is in both the treated and control buffer and occurs pre-installation of both blue lines, the transaction is labeled as *treated* pre-installation, because the distance to the “treated” blue line is smaller and because it would be labeled pre-installation regardless. The remaining cases apply to this situation.

Case 1: Treated pre-installation and control pre-installation: the transaction is in the treated buffer before the blue line's installation and in the control buffer before the blue line's installation. Timing "doesn't matter" here because the transaction occurs before both the "treated" and "control" blue lines are installed. Since timing doesn't matter, distance determines whether it's treated or control. In this case, since it's in both, it's treated. So, the transaction should be used as treated pre-installation.

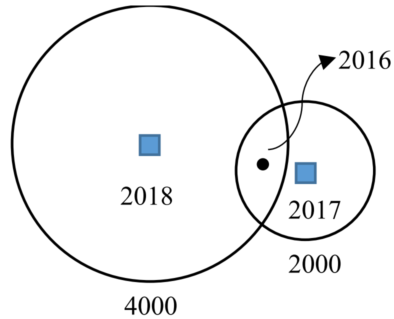


Figure A5. Diagram of overlap case 1

Case 2: Treated pre-installation and control pre-installation. The transaction should be used as treated pre-installation as in case 1.

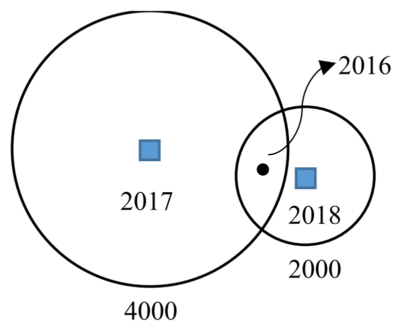


Figure A6. Diagram of overlap case 2

Case 3: Treated post-installation and control post-installation. Timing "doesn't matter" here because the transaction occurs after both the "treated" and "control" blue lines are installed. Since timing doesn't matter, distance determines whether it's treated or control. In this case, since it's in both, it's treated. So, the transaction should be used as treated post-installation.

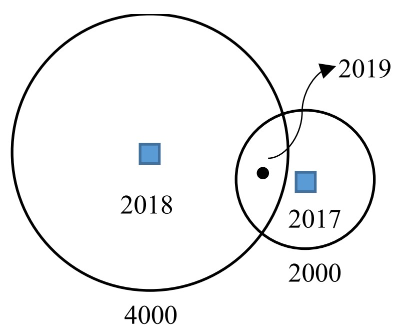


Figure A7. Diagram of overlap case 3

Case 4: Treated post-installation and control post-installation. The transaction should be used as treated pre-installation as in case 3.

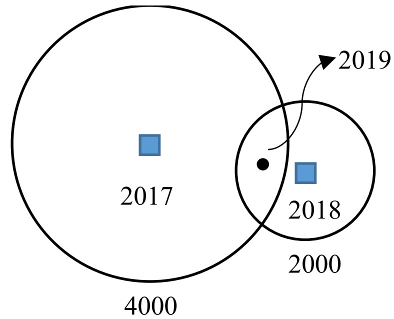


Figure A8. Diagram of overlap case 4

Case 5: Treated post-installation and control pre-installation. Now timing matters because the transaction occurs between the installation of the blue line whose control group it's in and the blue line whose treatment group it's in. "Earliest supersedes nearest" means that it's the blue line that's installed first that the event and treatment decision should be based on. So, since the transaction is post-installation of the treatment blue line, it should be used as treated post-installation.

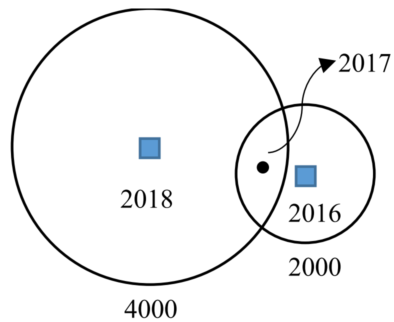


Figure A9. Diagram of overlap case 5

Case 6: Treated pre-installation and control post-installation. Timing matters because the transaction occurs between the installation of the blue line whose control group it's in and the blue line whose treatment group it's in. "Earliest supersedes nearest" means that it's the blue line that's installed first that the event and treatment decision should be based on. So, since the transaction is post-installation of the control blue line, it should be used as control post-installation.

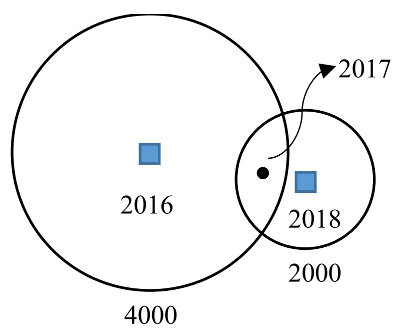


Figure A10. Diagram of overlap case 6

Case 7: Treated pre-installation and control is at installation (the transaction date and installation date of the blue line defining the control buffer is the same). When the transaction date is at the same time as the blue line installation date this is considered to be "pre-installation" because the blue line hasn't been in place long enough to affect the sale price of the property being sold at the same time. So, this is technically a "control pre-installation" situation. Thus, this is like case 2 and the transaction should be used as treated pre-installation.

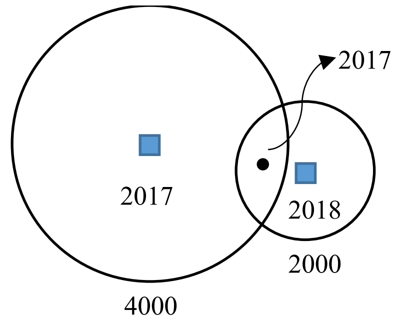


Figure A11. Diagram of overlap case 7

Case 8: Treated post-installation and control is at installation (the transaction date and installation date of the blue line defining the control buffer is the same). For the same reasons as in case 7, this is technically a "control pre-installation" situation. Thus, this is like case 5 and the transaction should be used as treated post-installation.

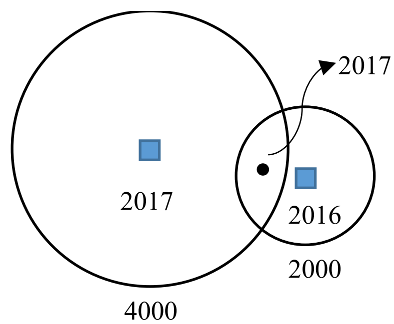


Figure A12. Diagram of overlap case 8

Case 9: Control pre-installation and treated is at installation (the transaction date and installation date of the blue line defining the treatment buffer is the same). For the same reasons as in case 7, this is technically a "treatment pre-installation" situation. Thus, this is like case 1 and the transaction should be used as treated pre-installation.

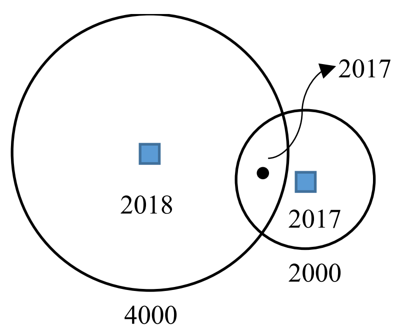


Figure A13. Diagram of overlap case 9

Case 10: Control post-installation and treated is at installation (the transaction date and installation date of the blue line defining the treatment buffer is the same). For the same reasons as in case 7, this is technically a “treatment pre-installation” situation. Thus, this is like case 6 and the transaction should be used as control post-installation.

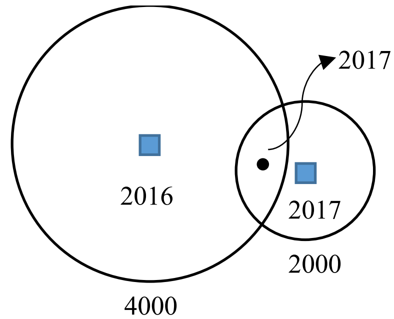


Figure A14. Diagram of overlap case 10

Case 11: Treated and control are at installation (the transaction date and installation dates of both blue lines are all the same). For the same reasons as in case 7, this is technically a “treatment pre-installation” and “control pre-installation” situation. Thus, this is like case 1 and the transaction should be used as a treated pre-installation.

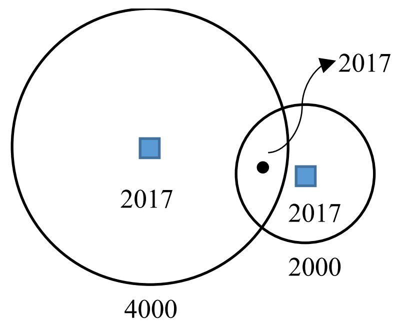


Figure A15. Diagram of overlap case 11

6.6 Matching results for the first analysis for Chapter 2

Tables A5, A6, A7, and A8 report the covariate balance results for the nearest neighbor propensity score matching (PSM), nearest neighbor Mahalanobis (NNM) distance matching, coarsened exact matching (CEM), and entropy balancing (EB) matching/weighting methods, respectively. For the PSM and NNM methods, the standardized difference in means for the matching variables is measured for all primary models before matching ("Raw") and after matching ("Matched"). For the CEM and EB methods, since they are primarily weighting methods, the standardized difference in means for the weighting variables before matching ("Raw") is compared to the *weighted* standardized difference in means after weighting ("Weighted").[79] The PSM method (Table A5) improved covariate balance for the key variables that likely influence treatment – elevation and distance to the ocean – in all models. However, the absolute standardized difference in means for the elevation variable in Model III did not decrease to below 0.25, the aforementioned rule of thumb indicating covariate balance (Stuart, 2010). Furthermore, approximately 87-92% of the control observations are dropped after matching, depending on the model. An additional drawback of propensity score matching was the inability to exactly match on event timing. The NNM method (Table A6) also improved covariate balance for the key matching variables but did not achieve covariate balance according to the rule of thumb for the elevation variable in Models I and III. Unlike PSM, NNM was able to exactly match on the events of interest. Similar to PSM, however, NNM dropped approximately 89-93% of the control observations. The CEM method (Table A7) improved covariate balance for the key matching variables but did not achieve covariate balance according to the rule of thumb for the elevation variable in Models I and III.[80] However, CEM was able to exactly match on the events of interest and on sale year according to the weighted standardized differences in means zero values. Unlike the PSM and NNM methods, the CEM method does not drop 90% of control observations. The EB method (Table A8) improved covariate balance for the key matching variables but did not achieve covariate balance according to the rule of thumb for the elevation variable in Models I and III.[81] Unlike the other three methods, however,

[79] CEM assigns a weight of "0" to some control and treatment observations so these observations are dropped in the weighted standardized difference in means calculation for the post-weighting sample. EB does not assign "0" weights and therefore does not drop any observations in the weighted standardized difference in means calculation.
[80] The "Weighted" columns of Table A7 report weighted standardized differences in means since CEM is primarily a weighting method and therefore drops few observations.
[81] The "Weighted" columns of Table A8 reports weighted standardized differences in means since EB is a weighting method and therefore doesn't drop observations.

the EB method is purely a weighting method and, as such, does not drop observations. However, an inspection of the weights generated by the CEM and EB methods shows that many observations are assigned very small weights, suggesting that these two methods also effectively "drop" many observations. In summary, while all four methods improved covariate balance for the key variables that likely influence treatment, there remains considerable imbalance in the elevation variable for Models I and III. Furthermore, the two matching methods (PSM and NNM) dropped approximately 90% of the control observations and the two predominantly weighting methods (CEM and EB) that do not appear to drop many/any of the observations did effectively drop many observations by assigning them very small weights.

Table A5. Propensity score matching standardized differences for the first analysis

	Model I		Model II		Model III	
Variables	Raw	Matched	Raw	Matched	Raw	Matched
Sold after 2011 Tohoku EQ (tohoku=1)	0.0463	-0.0038			0.0136	0.1306
Sold after 2015 article (article=1)			-0.0097	0.0041	-0.0032	-0.0067
Elevation (ft)	-1.5211	-0.1239	-1.7165	-0.1151	-1.5148	-0.2765
Log distance to ocean shoreline	-0.6606	0.0708	-0.7227	0.1190	-0.6743	0.1865
Sale year of the property	0.0368	0.0280	-0.0055	0.1225	0.0017	0.1181
Observations	5,890	1,932	9,160	4,996	15,627	5,088
Treatment	1,589	1,589	4,471	4,384	4,160	4,160
Control	4,301	343	4,689	612	11,467	928

Table A6. Nearest neighbor Mahalanobis matching standardized differences for the first analysis

	Model I		Model II		Model III	
Variables	Raw	Matched	Raw	Matched	Raw	Matched
Elevation (ft)	-1.5211	-0.3381	-1.7209	-0.0966	-1.5148	-0.3211
Log distance to ocean shoreline	-0.6606	-0.0218	-0.7361	-0.0315	-0.6743	-0.0205
Sale year of the property	0.0368	-0.0042	-0.0037	-0.0078	0.0017	0.0007
Observations	5,890	1,902	9,160	4,983	15,627	4,980
Treatment	1,589	1,589	4,471	4,471	4,160	4,160
Control	4,301	313	4,689	512	11,467	820

Exact matching on event (tohoku and/or article).

Table A7. Coarsened exact matching standardized differences for the first analysis

	Model I		Model II		Model III	
Variables	Raw	Weighted	Raw	Weighted	Raw	Weighted
Sold after 2011 Tohoku EQ (tohoku=1)	0.0463	0.0000			0.0136	-0.0000
Sold after 2015 article (article=1)			-0.0079	0.0000	-0.0032	0.0000
Elevation (ft)	-1.5211	-0.8376	-1.7209	0.0621	-1.5148	-0.6280
Log distance to ocean shoreline	-0.6606	-0.0386	-0.7361	-0.0283	-0.6743	-0.0136
Sale year of the property	0.0368	0.0000	-0.0037	0.0000	0.0017	-0.0000
Observations	5,890	3,447	9,160	5,771	15,627	9,202
Treatment	1,589	1,540	4,471	4,188	4,160	3,987
Control	4,301	1,907	4,689	1,583	11,467	5,215

Table A8. Entropy balancing standardized differences for the first analysis

	Model I		Model II		Model III	
Variables	Raw	Weighted	Raw	Weighted	Raw	Weighted
Sold after 2011 Tohoku EQ (tohoku=1)	0.0463	-0.0013			0.0136	-0.0006
Sold after 2015 article (article=1)			-0.0079	-0.0005	-0.0032	-0.0028
Elevation (ft)	-1.5211	-0.2852	-1.7209	-0.1697	-1.5148	-0.2699
Log distance to ocean shoreline	-0.6606	0.0042	-0.7361	-0.0021	-0.6743	0.0006
Sale year of the property	0.0368	-0.0005	-0.0037	0.0001	0.0017	-0.0005

6.7 Additional regression results and figures for Chapter 2

Table A9. Difference-in-differences results for the first analysis, full data

Variables	Model I Coefficient/SE	Model II Coefficient/SE	Model III Coefficient/SE
Event			
Sold after 2011 Tohoku EQ (tohoku=1)	.0858**		.0631
	(.0426)		(.0390)
Sold after 2015 article (article=1)		.0136	.0026
		(.0236)	(.0200)
Treatment			
Inside 1995 SB 379 tsunami zone (sb379=1)	.0620*		.0671**
	(.0333)		(.0308)
Inside 2013 XXL tsunami zone (xxl2013=1)		-.0073	
		(.0222)	
Diff-in-Diff			
SB 379 zone (sb379) x sold after 2011 Tohoku EQ (tohoku)	-.0889**		-.0675**
	(.0415)		(.0340)
2013 XXL zone (xxl2013) x sold after 2015 article (article)		.0064	
		(.0240)	
SB 379 zone (sb379) x sold after 2015 article (article)			.0269
			(.0244)
Structural			
Bedrooms	.1115***	.0323	.0592***
	(.0337)	(.0233)	(.0191)
Bedrooms squared	-.0189***	-.0083**	-.0117***
	(.0051)	(.0035)	(.0029)
Bathrooms	.1278***	.1688***	.1576***
	(.0403)	(.0344)	(.0253)
Bathrooms squared	-.0094	-.0184**	-.0165***
	(.0082)	(.0075)	(.0054)
Indoor square footage	3.7e-04***	5.0e-04***	4.5e-04***
	(4.5e-05)	(3.3e-05)	(2.7e-05)
Indoor square footage squared	-4.0e-08***	-5.5e-08***	-4.9e-08***
	(9.5e-09)	(7.1e-09)	(5.7e-09)
Total acreage (equal to indoor area if apartment)	.0160*	.0409***	.0274***
	(.0095)	(.0068)	(.0048)
Total acreage squared	-2.5e-05	-4.4e-04***	-1.4e-04***
	(8.8e-05)	(9.9e-05)	(5.3e-05)
Effective age of property (2018 - remodel year)	.0121***	.0105***	.0113***
	(.0012)	(9.1e-04)	(7.1e-04)
Effective age of property squared	-1.4e-04***	-1.3e-04***	-1.3e-04***
	(1.2e-05)	(8.8e-06)	(6.9e-06)
Heating (=1)	.1378***	.2823***	.2391***
	(.0374)	(.0255)	(.0208)
Fireplace (=1)	.1208***	.0877***	.1009***
	(.0171)	(.0120)	(.0097)
Garage (=1)	.0923***	.0510***	.0651***
	(.0186)	(.0132)	(.0105)
Goal 18 eligible (=1)	.0860	.0847**	.0788**
	(.0576)	(.0400)	(.0326)
Location			
Special Flood Hazard Area (SFHA) (=1)	-.0448	-.0377*	-.0397**

Table A9. Difference-in-differences results for the first analysis, full data (Continued)

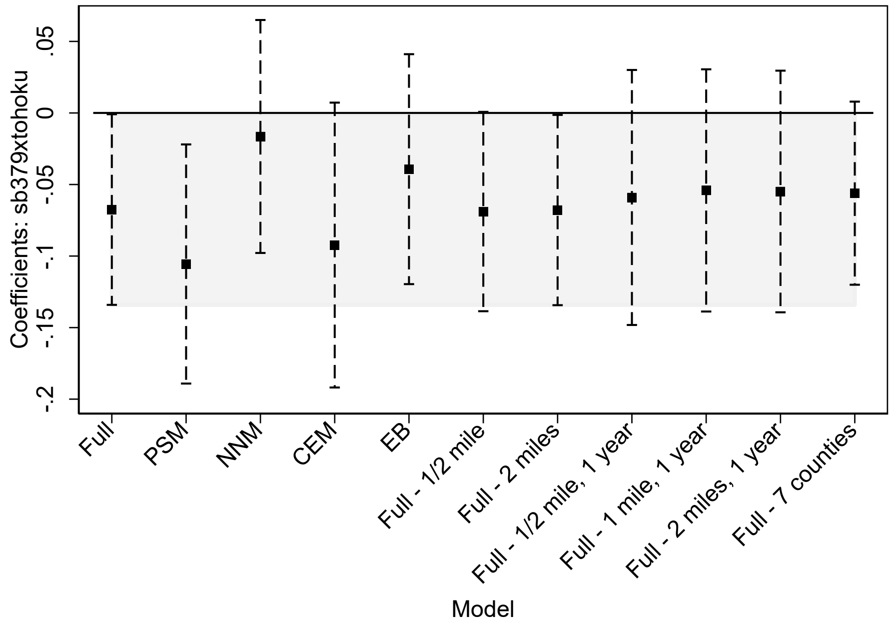
Variables	Model I Coefficient/SE	Model II Coefficient/SE	Model III Coefficient/SE
	(.0275)	(.0193)	(.0159)
Elevation (ft)	5.7e-04***	2.6e-04**	4.6e-04***
	(1.7e-04)	(1.3e-04)	(9.8e-05)
Log distance to nearest beach access point	-.0239**	-.0280***	-.0269***
	(.0093)	(.0057)	(.0050)
Log distance to ocean shoreline	-.0835***	-.0746***	-.0786***
	(.0115)	(.0059)	(.0055)
Elevation (ft) x Log distance to ocean shoreline x on oceanfront (=1)	3.9e-04***	2.7e-04***	3.2e-04***
	(7.7e-05)	(7.4e-05)	(5.3e-05)
Log distance to nearest river	-.0191***	-.0211***	-.0214***
	(.0056)	(.0039)	(.0032)
Log distance to nearest national park or public land	-.0374***	-.0336***	-.0344***
	(.0098)	(.0057)	(.0050)
Log distance to nearest highway or interstate	.0233***	.0137**	.0160***
	(.0077)	(.0054)	(.0044)
Log distance to nearest railroad	-.0185	-.0403***	-.0269***
	(.0167)	(.0117)	(.0100)
Log distance to nearest airport	.0434*	.0213	.03066**
	(.0223)	(.0160)	(.0128)
Log distance to nearest k-12 school	.0264*	.0305***	.0244***
	(.0149)	(.0104)	(.0084)
Log distance to nearest wastewater treatment plant	-.0230	-.0286***	-.0255***
	(.0145)	(.0107)	(.0085)
Log distance to nearest hospital	.0409	.0681***	.0587***
	(.0260)	(.0177)	(.0144)
Observations	5890	9160	15627
Adj. R-squared	0.376	0.441	0.411

* p<0.10, ** p<0.05, *** p<0.01

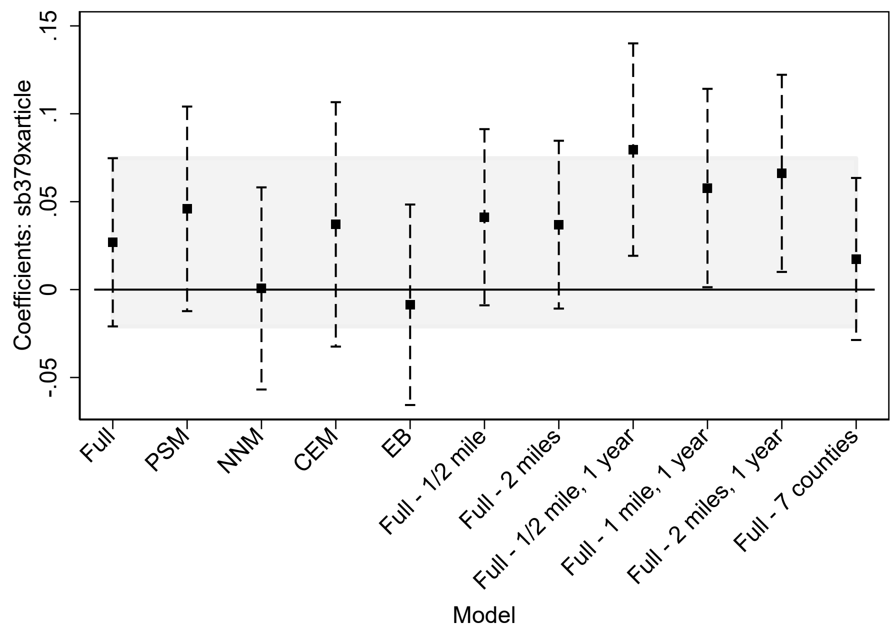
Table A10. DID falsification test results for the first analysis, full data

	Model I Coefficient/SE	Model II Coefficient/SE	Model III Coefficient/SE
Test #1			
SB 379 zone (sb379) x sold after 3/11/10 (falsetohoku)	-.0547		-.0507
	(.0463)		(.0431)
SB 379 zone (sb379) x sold after 2015 article (article)			.0169
			(.0237)
Test #2			
SB 379 zone (sb379) x sold after 3/11/12 (falsetohoku)	-.0153		-.0092
	(.0442)		(.0299)
SB 379 zone (sb379) x sold after 2015 article (article)			.0142
			(.0252)
Test #3			
Placebo treatment group (randomtreat) x sold after 2011 Tohoku EQ (tohoku)	.0199		.0211
	(.0270)		(.0216)
Placebo treatment group (randomtreat) x sold after 2015 article (article)		-.0134	5.1e-04
		(.0196)	(.0169)
Test #4			
SB 379 zone (sb379) x placebo event status (randomevent)	-.0154		
	(.0306)		
2013 XXL zone (xxl2013) x placebo event status (randomevent)		.0044	
		(.0193)	

* p<0.10, ** p<0.05, *** p<0.01



(a)



(b)

Figure A16. Average treatment effect on the treated estimates with 95% confidence intervals for Model III of the first analysis

Note: The full data estimator is on the left. The next four points represent the estimators after the data was processed with the four matching methods (PSM, NNM, CEM, and EB). OB represents the Oaxaca-Blinder estimator. The final six estimators represent the full data estimator under different sample space assumptions. (a) For Model III's Tohoku event estimator. (b) For Model III's article event estimator.

Table A11. Difference-in-differences results for the second analysis, full data

Variables	Model 1 Coefficient/SE	Model 2 Coefficient/SE	Model 3 Coefficient/SE	Model 4 Coefficient/SE	Model 5 Coefficient/SE
Event					
Sold after 2013 map change (after 10/2/13) (newmaps=1)	.0084	.0023	-.0226	-.0175	-.0090
	(.0360)	(.0365)	(.0394)	(.0420)	(.0438)
Treatment					
Inside 2013 XXL tsunami zone (xxl2013=1)	-.0305				
	(.0271)				
Inside 2013 XL tsunami zone (xl2013=1)		-.0093			
		(.0291)			
Inside 2013 L tsunami zone (l2013=1)			-5.9e-04		
			(.0433)		
Inside 2013 M tsunami zone (m2013=1)				.0654	
				(.0722)	
Inside 2013 SM tsunami zone (sm2013=1)					.2441*
					(.1256)
Diff-in-Diff					
2013 XXL zone (xxl2013) x sold after 2013 map change (newmaps)	.0209				
	(.0313)				
2013 XL zone (xl2013) x sold after 2013 map change (newmaps)		.0205			
		(.0331)			
2013 L zone (l2013) x sold after 2013 map change (newmaps)			.0717		
			(.0468)		
2013 M zone (m2013) x sold after 2013 map change (newmaps)				-.0265	
				(.0768)	
2013 SM zone (sm2013) x sold after 2013 map change (newmaps)					-.3133**
					(.1488)
Structural					
Bedrooms	.0613**	.0584**	.0591**	.0741**	.0732**
	(.0247)	(.0250)	(.0264)	(.0291)	(.0296)
Bedrooms squared	-.0097***	-.0091***	-.0096***	-.0117***	-.0115***
	(.0035)	(.0035)	(.0037)	(.0042)	(.0042)
Bathrooms	.2796***	.2717***	.2634***	.2723***	.2489***
	(.0363)	(.0369)	(.0400)	(.0430)	(.0425)
Bathrooms squared	-.0349***	-.0331***	-.0308***	-.0324***	-.0274***

Table A11. Difference-in-differences results for the second analysis, full data (Continued)

Variables	Model 1 Coefficient/SE	Model 2 Coefficient/SE	Model 3 Coefficient/SE	Model 4 Coefficient/SE	Model 5 Coefficient/SE
	(.0077)	(.0078)	(.0084)	(.0090)	(.0086)
Indoor square footage	2.8e-04***	2.9e-04***	3.0e-04***	3.2e-04***	3.3e-04***
	(3.6e-05)	(3.6e-05)	(3.9e-05)	(4.2e-05)	(4.4e-05)
Indoor square footage squared	-1.9e-08**	-2.1e-08***	-2.3e-08***	-2.5e-08***	-2.9e-08***
	(7.3e-09)	(7.4e-09)	(7.9e-09)	(8.3e-09)	(8.8e-09)
Total acreage (equal to indoor area if apartment)	.0357***	.0395***	.0376***	.0345***	.0698***
	(.0080)	(.0081)	(.0098)	(.0101)	(.0115)
Total acreage squared	-2.8e-04***	-3.1e-04***	-2.8e-04***	-2.3e-04***	-.0016***
	(8.0e-05)	(7.8e-05)	(8.4e-05)	(8.8e-05)	(4.9e-04)
Effective age of property (2018 - remodel year)	.0099***	.0100***	.0098***	.0105***	.0106***
	(.0010)	(.0010)	(.0011)	(.0012)	(.0013)
Effective age of property squared	-1.2e-04***	-1.2e-04***	-1.2e-04***	-1.3e-04***	-1.3e-04***
	(9.6e-06)	(9.7e-06)	(1.1e-05)	(1.1e-05)	(1.2e-05)
Heating (=1)	.1955***	.2146***	.2096***	.2345***	.2365***
	(.0321)	(.0321)	(.0343)	(.0384)	(.0390)
Fireplace (=1)	.1003***	.0952***	.0926***	.0712***	.07640***
	(.0143)	(.0144)	(.0155)	(.0163)	(.0168)
Garage (=1)	.0854***	.0785***	.0643***	.0697***	.0663***
	(.0150)	(.0152)	(.0165)	(.0177)	(.0185)
Carport (=1)	-.0693**	-.0740**	-.0924***	-.0804**	-.0840**
	(.0300)	(.0304)	(.0348)	(.0377)	(.0380)
Deck (=1)	-.0095	-.0117	-.0025	-.0046	.0043
	(.0217)	(.0219)	(.0244)	(.0261)	(.0268)
Patio (=1)	.0218	.0186	.0210	.0155	.0295
	(.0159)	(.0162)	(.0177)	(.0190)	(.0194)
Fencing (=1)	.0147	.0167	.0215	.0130	.0086
	(.0193)	(.0196)	(.0212)	(.0233)	(.0239)
Goal 18 eligible (=1)	.0909	.0905	.1475**	.1190	.0870
	(.0644)	(.0653)	(.0723)	(.0822)	(.0885)
Has shoreline armoring (=1)	.3308***	.3817***	.2773***	.2849**	.3365**
	(.0849)	(.0842)	(.0960)	(.1357)	(.1463)
Location					
Distance (ft) to 2013 XXL line if inside zone (=0 if outside of zone)	4.2e-05**	2.0e-05	1.1e-05	-2.4e-05	-1.6e-04*
	(1.9e-05)	(2.0e-05)	(2.5e-05)	(4.3e-05)	(8.7e-05)
Special Flood Hazard Area (SFHA) (=1)	-.0156	-.0130	-.0539	-.0241	.0320
	(.0397)	(.0406)	(.0456)	(.0575)	(.0582)
Elevation (ft)	6.5e-04***	6.7e-04***	6.9e-04***	6.4e-04***	5.8e-04***
	(1.1e-04)	(1.1e-04)	(1.1e-04)	(1.2e-04)	(1.2e-04)
Elevation (ft) x Log distance to ocean shoreline x on oceanfront (=1)	-.0031	-.0018	-.0019	4.5e-04	.0012
	(.0027)	(.0027)	(.0028)	(.0030)	(.0031)

Table A11. Difference-in-differences results for the second analysis, full data (Continued)

Variables	Model 1 Coefficient/SE	Model 2 Coefficient/SE	Model 3 Coefficient/SE	Model 4 Coefficient/SE	Model 5 Coefficient/SE
Slope (angular degrees of slope)	-.0150*	-.0130	-.0235***	-.0165*	-.0040
	(.0080)	(.0080)	(.0090)	(.0098)	(.0110)
Log distance to nearest beach access point	-.1084***	-.10587***	-.0909***	-.1003***	-.1231***
	(.0140)	(.0141)	(.0162)	(.0176)	(.0161)
Log distance to ocean shoreline	2.0e-04***	2.0e-04***	2.1e-04***	2.1e-04***	1.9e-04***
	(7.0e-05)	(7.1e-05)	(6.8e-05)	(6.2e-05)	(6.2e-05)
Log distance to nearest water body (lake, pond, bay)	-.0028	-.0050	8.8e-04	-3.6e-04	.0069
	(.0066)	(.0066)	(.0084)	(.0100)	(.0122)
Log distance to nearest river	-.0352***	-.0340***	-.0323***	-.0282***	-.0240**
	(.0056)	(.0057)	(.0069)	(.0083)	(.0095)
Log distance to nearest state park or public land	.0029	.0045	3.7e-05	.0098	.0207*
	(.0067)	(.0068)	(.0072)	(.0099)	(.0114)
Log distance to nearest national park or public land	-.0093	-.0063	-.0060	-.0132	-.0161*
	(.0073)	(.0077)	(.0085)	(.0089)	(.0089)
Log distance to nearest highway or interstate	.0247***	.0237***	.0307***	.0308***	.0289***
	(.0060)	(.0061)	(.0069)	(.0077)	(.0083)
Log distance to nearest major road	-4.2 e-04	5.4e-04	.0059	.0064	.0074
	(.0043)	(.0043)	(.0048)	(.0053)	(.0057)
Log distance to nearest railroad	-.0082	-.0043	-.0107	-.0126	-.0146
	(.0143)	(.0144)	(.0142)	(.0158)	(.0194)
Log distance to nearest airport	.0410*	.0390*	.0444*	.0174	-.0057
	(.0214)	(.0218)	(.0237)	(.0260)	(.0280)
Log distance to nearest k-12 school	.0042	.0055	.0094	.0262*	.0253*
	(.0121)	(.0122)	(.0131)	(.0140)	(.0147)
Log distance to nearest central business district (city)	.0186*	.0159	.0157	.0132	.0084
	(.0109)	(.0110)	(.0121)	(.0129)	(.0137)
Log distance to nearest wastewater treatment plant	-.0187	-.0242*	-.0304*	-.0458***	-.0600***
	(.0136)	(.0137)	(.0159)	(.0172)	(.0184)
Log distance to nearest fire station	-1.4e-04	.0025	.0060	3.9e-04	.0049
	(.0106)	(.0108)	(.0126)	(.0146)	(.0151)
Log distance to nearest law enforcement station	.0138	.0118	.0083	.0138	.0116

Table A11. Difference-in-differences results for the second analysis, full data (Continued)

Variables	Model 1 Coefficient/SE	Model 2 Coefficient/SE	Model 3 Coefficient/SE	Model 4 Coefficient/SE	Model 5 Coefficient/SE
	(.0141)	(.0144)	(.0156)	(.0168)	(.0175)
Log distance to nearest hospital	-.0175	-.0214	-.0106	-.0220	-.0397
	(.0183)	(.0186)	(.0209)	(.0234)	(.0249)
Observations	8010	7790	6593	5842	5429
Adj. R-squared	0.422	0.420	0.424	0.423	0.427

* p<0.10, ** p<0.05, *** p<0.01

Table A12. Difference-in-differences results for the second analysis, combined model, full data

	Coefficient	SE
Event		
Sold after 2013 map change (after 10/2/13) (newmaps=1)	.0077	(.0360)
Treatment		
Inside 2013 XXL tsunami zone (xxl2013=1)	-.0491	(.0541)
Inside 2013 XL tsunami zone (xl2013=1)	.0284	(.0582)
Inside 2013 L tsunami zone (l2013=1)	-.0047	(.0495)
Inside 2013 M tsunami zone (m2013=1)	.0144	(.0738)
Inside 2013 SM tsunami zone (sm2013=1)	.0562	(.0964)
Diff-in-Diff		
2013 XXL zone (xxl2013) x sold after 2013 map change (newmaps)	-.0477	(.0728)
2013 XL zone (xl2013) x sold after 2013 map change (newmaps)	.0559	(.0778)
2013 L zone (l2013) x sold after 2013 map change (newmaps)	.0903	(.0575)
2013 M zone (m2013) x sold after 2013 map change (newmaps)	-.0556	(.0890)
2013 SM zone (sm2013) x sold after 2013 map change (newmaps)	-.2393*	(.1343)
Structural		
Bedrooms	.0617**	(.0246)
Bedrooms squared	-.0097***	(.0035)
Bathrooms	.2796***	(.0363)
Bathrooms squared	-.0349***	(.0077)
Indoor square footage	2.8e-04***	(3.6e-05)
Indoor square footage squared	-1.9e-08**	(7.3e-09)
Total acreage (equal to indoor area if apartment)	.0365***	(.0081)
Total acreage squared	-2.8e-04***	(8.0e-05)
Effective age of property (2018 - remodel year)	.0098***	(.0010)
Effective age of property squared	-1.2e-04***	(9.6e-06)
Heating (=1)	.1949***	(.0321)
Fireplace (=1)	.1009***	(.0143)
Garage (=1)	.0871***	(.0149)
Carport (=1)	-.0699**	(.0302)
Deck (=1)	-.0098	(.0218)
Patio (=1)	.0220	(.0159)
Fencing (=1)	.0153	(.0193)
Goal 18 eligible (=1)	.0910	(.0638)
Has shoreline armoring (=1)	.3085***	(.0838)
Location		
Distance (ft) to 2013 XXL line if inside zone (=0 if outside of zone)	2.7e-05	(2.1e-05)
Special Flood Hazard Area (SFHA) (=1)	-.0134	(.0393)
Elevation (ft)	6.6e-04***	(1.1e-04)
Slope (angular degrees of slope)	-.0028	(.0026)
Log distance to nearest beach access point	-.0135*	(.0080)
Log distance to ocean shoreline	-.1096***	(.0140)
Elevation (ft) x Log distance to ocean shoreline x on oceanfront (=1)	2.0e-04***	(7.0e-05)
Log distance to nearest water body (lake, pond, bay)	-.0029	(.0067)
Log distance to nearest river	-.0348***	(.0056)
Log distance to nearest state park or public land	.0032	(.0067)
Log distance to nearest national park or public land	-.0091	(.0073)
Log distance to nearest highway or interstate	.0238***	(.0060)
Log distance to nearest major road	-1.0e-04	(.0043)
Log distance to nearest railroad	-.0067	(.0144)
Log distance to nearest airport	.0408*	(.0214)

Table A12. Difference-in-differences results for the second analysis, combined model, full data (Continued)

	Coefficient	SE
Log distance to nearest k-12 school	.0028	(.0121)
Log distance to nearest central business district (city)	.0168	(.0110)
Log distance to nearest wastewater treatment plant	-.0197	(.0136)
Log distance to nearest fire station	.0018	(.0106)
Log distance to nearest law enforcement station	.0134	(.0141)
Log distance to nearest hospital	-.0175	(.0183)
Observations	8010	
Adj. R-squared	0.423	

* p<0.10, ** p<0.05, *** p<0.01

Table A13. Oaxaca-Blinder results for the second analysis, full data

	Model 1 Coefficient/SE	Model 2 Coefficient/SE	Model 3 Coefficient/SE	Model 4 Coefficient/SE	Model 5 Coefficient/SE
Overall Differential					
Treated group	12.470***	12.484***	12.514***	12.374***	12.219***
	(.0178)	(.0188)	(.0276)	(.0509)	(.0985)
Control group	12.431***	12.435***	12.437***	12.439***	12.437***
	(.0073)	(.0073)	(.0076)	(.0079)	(.0081)
Difference	.0390**	.0500**	.0771***	-.0650	-.2184**
	(.0193)	(.0202)	(.0287)	(.0515)	(.0988)
Decomposition					
Explained	.0094	.0149	.0233	-.0247	-.0466
	(.0242)	(.0255)	(.0360)	(.0590)	(.1103)
Unexplained	.0296	.0350	.0539	-.0403	-.1718
	(.0249)	(.0261)	(.0357)	(.0597)	(.1047)
Observations	8010	7790	6593	5842	5429

* p<0.10, ** p<0.05, *** p<0.01

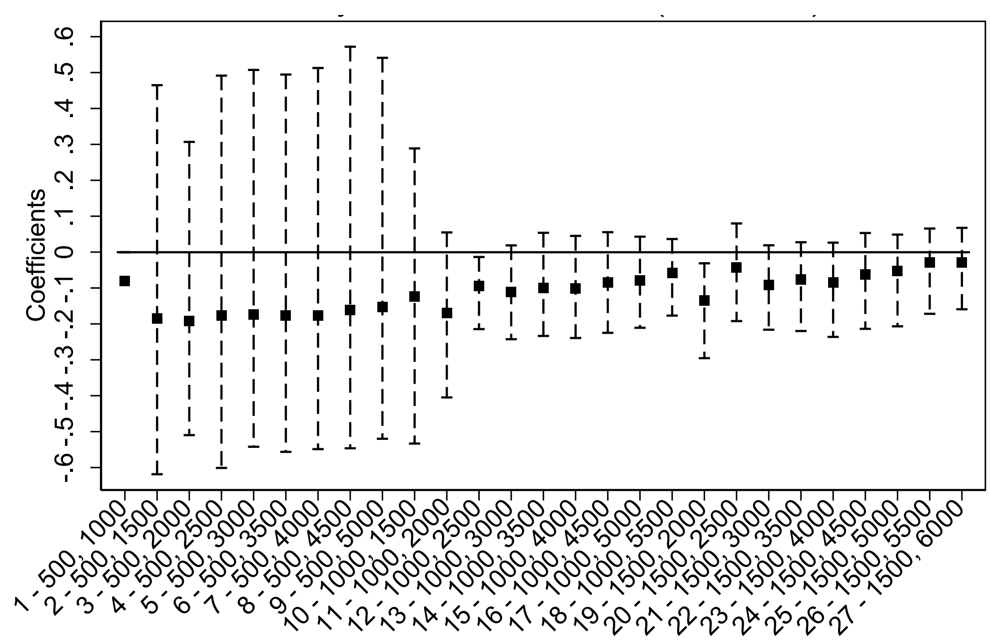
Table A14. DID falsification test results for the second analysis, full data

	Model 1 Coefficient/SE	Model 2 Coefficient/SE	Model 3 Coefficient/SE	Model 4 Coefficient/SE	Model 5 Coefficient/SE
Test #1					
2013 XXL zone (xxl2013) x sold after 10/2/12 (falsenewmaps)	-.0667*				
	(.0364)				
2013 XL zone (xl2013) x sold after 10/2/12 (falsenewmaps)		-.0728*			
		(.0393)			
2013 L zone (l2013) x sold after 10/2/12 (falsenewmaps)			-.0151		
			(.0555)		
2013 M zone (m2013) x sold after 10/2/12 (falsenewmaps)				-.0909	
				(.0809)	
2013 SM zone (sm2013) x sold after 10/2/12 (falsenewmaps)					-.0871
					(.1569)
Test #2					
2013 XXL zone (xxl2013) x sold after 10/2/14 (falsenewmaps)	.0288				
	(.0304)				
2013 XL zone (xl2013) x sold after 10/2/14 (falsenewmaps)		.0370			
		(.0313)			
2013 L zone (l2013) x sold after 10/2/14 (falsenewmaps)			.0889**		
			(.0432)		
2013 M zone (m2013) x sold after 10/2/14 (falsenewmaps)				.0627	
				(.0766)	
2013 SM zone (sm2013) x sold after 10/2/14 (falsenewmaps)					-.1252
					(.1559)
Test #3					
Placebo treatment group (randomtreat) x sold after 2013 map change (newmaps)	-.0197	.0197	.0132	.0419	.0052
	(.0229)	(.0230)	(.0253)	(.0269)	(.0281)
Test #4					
2013 XXL zone (xxl2013) x placebo event	.0248				

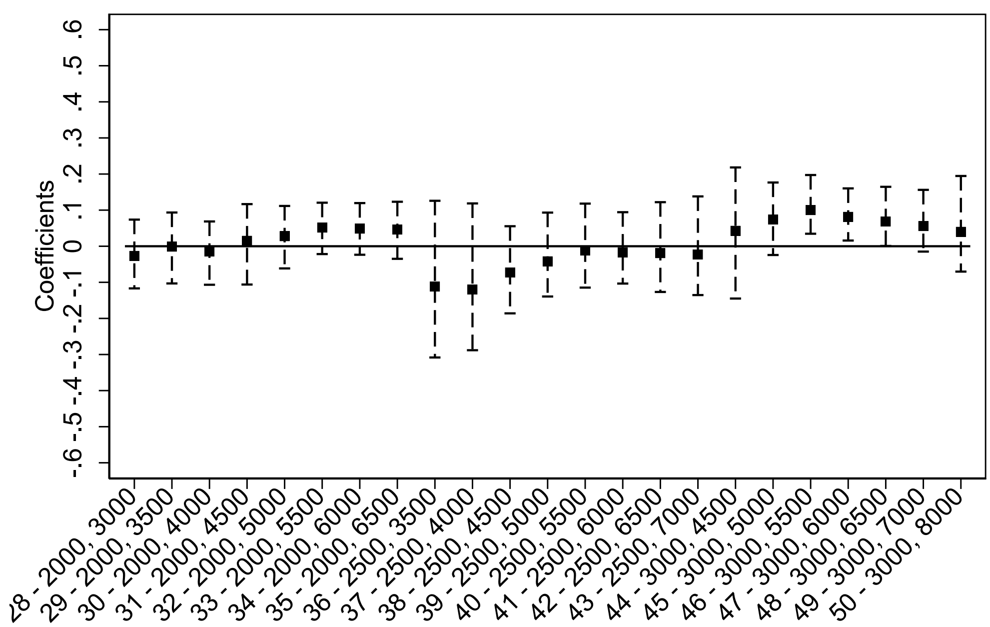
Table A14. DID falsification test results for the second analysis, full data (Continued)

	Model 1 Coefficient/SE	Model 2 Coefficient/SE	Model 3 Coefficient/SE	Model 4 Coefficient/SE	Model 5 Coefficient/SE
status (randomevent)					
	(.0251)				
2013 XL zone (xl2013) x placebo event status (randomevent)		.0300			
		(.0266)			
2013 L zone (l2013) x placebo event status (randomevent)			-.0025		
			(.0361)		
2013 M zone (m2013) x placebo event status (randomevent)				.0118	
				(.0647)	
2013 SM zone (sm2013) x placebo event status (randomevent)					.1576
					(.1298)

* p<0.10, ** p<0.05, *** p<0.01



(a)



(b)

Figure A17. Average treatment effect on the treated estimates with 95% confidence intervals for Models 1 through 50 of the third analysis

Note: Euclidian distances define the treatment and control buffers. For each ATET, the model number is followed by the size of the treatment buffer (ft) and the size of the control buffer (ft), e.g., Model 1 has a 500’ treatment buffer and 1000’ control buffer. (a) For Models 1-27. (b) For Models 28-50. Note: confidence intervals that are out of bounds are suppressed, e.g., for Model 1.

Table A15. Difference-in-differences and triple differences results for the third analysis, Model 62

	DID		DDD	
Variables	Coefficient	p-value	Coefficient	p-value
Treatment				
Blue line treatment buffer (treatment362=1)	.0218	.4658	.0398	.2532
Event				
Sold after first blue line installed (event362=1)	.0185	.8296	.1012	.7396
Sensitivity				
Inside 2013 XXL tsunami zone (xxl2013=1)			.1365*	.0800
Diff-in-Diff				
Blue line treatment buffer (treatment362) x sold after first blue line installed (event362)	-.0834**	.0254	-.0832	.4731
Blue line treatment buffer (treatment362) x 2013 XXL zone (xxl2013)			-.0623	.3290
2013 XXL zone (xxl2013) x sold after first blue line installed (event362)			-.2488	.1507
Triple Difference				
Blue line treatment buffer x 2013 XXL zone x sold after first blue line installed			-.0117	.9404
Structural				
Bedrooms	.0910	.5609	.0807	.5772
Bedrooms squared	-.0188	.3282	-.0173	.3037
Bathrooms	.1256*	.0669	.1241**	.0437
Bathrooms squared	-.0055	.7158	-.0045	.7533
Indoor square footage	4.4e-04**	.0168	4.4e-04**	.0166
Indoor square footage squared	-4.9e-08*	.0860	-5.1e-08*	.0681
Total acreage (equal to indoor area if apartment)	.0694	.7723	.1003	.6456
Total acreage squared	-.0104	.8428	-.0177	.7119
Effective age of property (2018 - remodel year)	-.0014	.6007	-.0019	.4708
Effective age of property squared	3.7e-06	.8702	7.7e-06	.7322
Heating (=1)	.2779**	.0113	.2892***	.0052
Fireplace (=1)	.0430	.3703	.0400	.4183
Garage (=1)	.0015	.9529	-.0011	.9603
Carport (=1)	-.0079	.8670	.0114	.8168
Deck (=1)	.0912	.1661	.0955	.1109
Patio (=1)	.0685	.4963	.0693	.4762
Fencing (=1)	.1049	.1486	.1041	.1461
Goal 18 eligible (=1)	-.0935	.4246	-.0892	.4680
Has shoreline armoring (=1)	.1540	.6131	.1998	.5912
Location				
Special Flood Hazard Area (SFHA) (=1)	-.0085	.8749	-.0266	.6057
Elevation (ft)	5.9e-04	.2038	.0011	.1197
Elevation (ft) x Log distance to ocean shoreline x on oceanfront (=1)	2.9e-04	.2527	2.8e-04	.2660
Slope (angular degrees of slope)	.0094	.4007	.0111	.3059
Log distance to nearest beach access point	-.0442	.1185	-.0429	.1068
Log distance to ocean shoreline	-.0799***	.0081	-.0747***	.0088
Log distance to nearest water body (lake, pond, bay)	-.0173	.4784	-.0159	.5071
Log distance to nearest river	.0167	.5020	.0201	.4495
Log distance to nearest state park or public land	.0356	.5106	.0443	.4588

Table A15. Difference-in-differences and triple differences results for the third analysis, Model 62 (Continued)

	DID		DDD	
Variables	Coefficient	p-value	Coefficient	p-value
Log distance to nearest national park or public land	-.0827	.1895	-.0799	.1892
Log distance to nearest highway or interstate	.0097	.7689	.0100	.7564
Log distance to nearest major road	-.0053	.6612	-.0064	.5865
Log distance to nearest railroad	-.1263	.1703	-.1240	.1558
Log distance to nearest airport	.0924	.5340	.0794	.5770
Log distance to nearest k-12 school	.0684	.4801	.0696	.4798
Log distance to nearest central business district (city)	.0134	.8239	.0085	.8788
Log distance to nearest wastewater treatment plant	.0393	.3820	.0425	.3642
Log distance to nearest fire station	.0216	.6341	.0220	.6354
Log distance to nearest law enforcement station	-.0104	.8095	-.0132	.7876
Log distance to nearest hospital	-.0150	.6977	-.0145	.67
Observations	1334		1334	
Adj. R-squared	0.491		0.496	

* p<0.10, ** p<0.05, *** p<0.01

6.8 Full survey text for Chapter 3

The Oregon Coast: A Survey about Coastal Recreation & Management Plans

Start of Block: Screener

Q2 Our research will be greatly improved if we can combine information collected in this survey with U.S. Census data about your local community. Please provide us with your 5-digit Postal/Zip code:

____

Page Break

Q4 What is your gender?

- Male
- Female
- Non-binary
- Prefer not to say

Page Break

Q6 What is your age?

- ○ Under 18
- ○ 18 – 24
- ○ 25 – 34
- ○ 35 – 44
- ○ 45 – 54
- ○ 55 – 64
- ○ 65 – 74
- ○ 75 – 84
- ○ 85 or older
- ○ Prefer not to say

Page Break

Q8 **The Oregon Coast:**
A Survey about Coastal Recreation & Management Plans
We would like to learn more about your personal views on future management plans for Oregon's coastline and beaches. This survey provides you with key information about Oregon's beaches and will ask you a series of questions related to your experiences and vision for the future.

Lincoln City, Oregon

You might not have previous experience with this topic, but the state's beaches are a resource freely accessible to you, an Oregon resident, and your participation in this survey is very important to us.

Participation in this survey is voluntary. If you decide to participate, you will be asked to read about Oregon beaches and answer a series of questions that will take about 20 minutes. If you run out of time completing the survey, you may leave and return later to complete it. Your previous answers will be saved if you use the same device and Internet browser when returning.

There are no known risks associated with participation in this survey. The benefits of this research study include providing Oregon state agencies and legislative bodies with information from the public that may help inform decisions about policies affecting Oregon's beaches.

Confidentiality of Records: Your individual responses to all questions in this survey will remain confidential. Any material linking you to your survey responses will not be released and will be destroyed at the end of the study.

Questions About this Survey? If you have any questions or concerns about this research project, please contact the principal investigator, Dr. Steven Dundas (dundas_survey@oregonstate.edu). If you have questions about your rights or welfare as a participant in this survey, please contact the Oregon State University Human Research Protection Program (HRPP) office at (541) 737-8008.

This research is sponsored by the National Oceanic and Atmospheric Administration (NOAA), a federal agency charged with managing the nation's coastal and marine resources.

Are you eligible and willing to be a participant in this study? By clicking Yes, you certify that:

- You are at least 18 years or older
- You currently live in Oregon
- You consent to have the information you provide used in this study

○ Yes

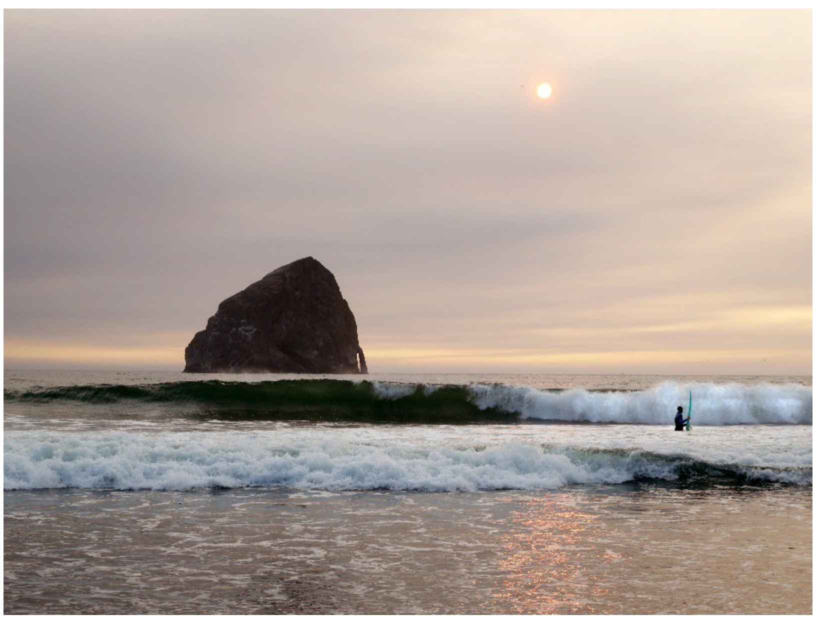
○ No

End of Block: Screener

Start of Block: Introduction

Q10 **Please start by reading some background information on Oregon's beaches.**

On June 7th, 1967, the Oregon Legislature passed the Beach Bill, which gave Oregonians a **permanent easement to access and recreate on all beaches in the state**. Unlimited access to the beach has made the Oregon Coast a source of recreation and enjoyment for residents and tourists alike.



Surfer in the water in Pacific City

Before today, were you aware that Oregon's 1967 Beach Bill guarantees permanent public access to coastal beaches in the state?

- ◯ Yes
- ◯ No
- ◯ Not sure

End of Block: Introduction

Start of Block: Background information about developed beaches

Q12 The first part of the survey focuses on recreation on **Oregon's developed beaches** – beaches in coastal towns with buildings and other structures behind the beach.

Undeveloped beaches, such as those found within Oregon's State Parks or U.S. Forest Service lands, are natural systems without significant coastal development. This type of beach is <u>not</u> the focus of this survey.

Both developed and undeveloped beaches can have amenities like parking, restrooms, and ramp access. However, developed beaches will have other services like restaurants, shops, and lodging facilities nearby.

Left: DEVELOPED BEACH: Nye Beach in Newport. Right: UNDEVELOPED BEACH: South Beach State Park in Newport.

Before today, have you ever visited developed and/or undeveloped beaches in Oregon?

- Yes, developed beaches only
- Yes, undeveloped beaches only
- Yes, both developed and undeveloped beaches
- No
- Not sure

End of Block: Background information about developed beaches

Start of Block: Revealed preference questions - day trips

Q14 Our research team is first interested in learning about **where you like to recreate on Oregon's developed beaches**.

Beach recreation can take many forms. There are several water-based activities like boating, surfing, & swimming and land-based activities like camping, walking on the beach, wildlife viewing, beachcombing, & photography.

RECREATION: Going for a walk, beachcombing, and photography are examples of beach recreation activities in Oregon. Pictured: Nye Beach in Newport.

Page Break

Q16 This first set of questions asks you about the **day trips for recreation** you have taken to **<u>developed</u>** Oregon Coast beaches (we will ask you about short overnight trips later).

We define a **day trip** as leaving your home and traveling to a developed beach for a single day visit (without spending the night) for the purpose of outdoor recreation and leisure.

If you made a single day trip that included visiting **multiple Oregon Coast beaches**, we would still consider that a single trip. If your **primary residence is near the Oregon Coast** and you visited a nearby developed beach for outdoor recreation (e.g., going for a walk), we would still consider that a day trip.

Above is a map with **<u>developed</u>** Oregon beaches labeled. In the last 12 months (**April 2021 to March 2022**), did you take **at least one (1) day trip to a <u>developed</u> Oregon Coast beach for any form of outdoor recreation?** (Please do not include trips taken for business or non-recreation purposes.)

- ○ Yes
- ○ No

Page Break

Q18 On a typical **day trip** to a **<u>developed</u>** Oregon Coast beach, how many people do you go to the beach with?

- ○ By myself
- ○ 1 person besides myself
- ○ 2-4 people besides myself
- ○ 5 or more people besides myself
- ○ Not sure

Q19 On a typical **day trip** to a **<u>developed</u>** Oregon Coast beach, how many hours do you spend at the beach?

- ◯ 1 hour
- ◯ 2-4 hours
- ◯ 5-7 hours
- ◯ 8-10 hours
- ◯ 11+ hours
- ◯ Not sure

Page Break

Q21
Much of Oregon experienced a record-breaking heat wave during the last week of June 2021. In most places temperatures peaked well above 100 degrees Fahrenheit between June 26th and June 28th.
Did you take at least one (1) **day trip** to visit **<u>developed</u>** Oregon Coast beaches for recreation because of the extreme heat during the last week of June 2021?

- ◯ Yes
- ◯ No
- ◯ Not sure

Page Break

Q23 You indicated you took **day trips** to visit **<u>developed</u>** Oregon beaches in the previous 12 months. Please review the map below and then answer the question below the map.

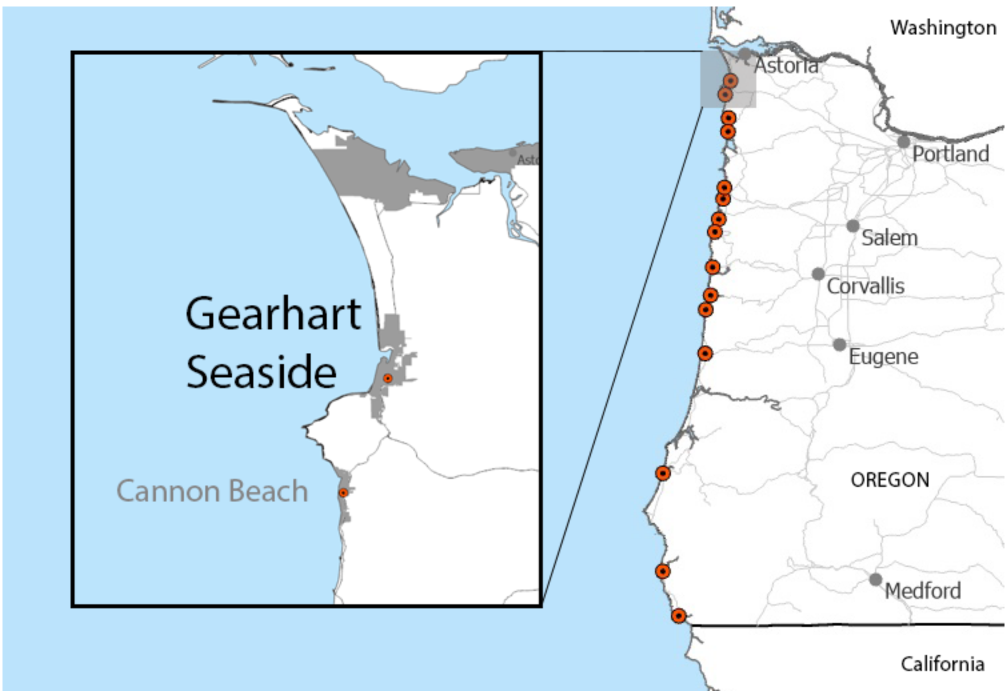
WASHINGTON
Gearhart/Seaside
Astoria
Cannon Beach
Manzanita
Rockaway Beach
Portland
Pendleton
La Grande
Pacific City
Neskowin
Lincoln City
Gleneden Beach
Salem
Newport
Corvallis
Waldport
Yachats
Florence
Eugene
Bend
OREGON
IDAHO
PACIFIC
OCEAN
Bandon
LEGEND
Developed Beach
Other Oregon Cities
Signed Highways
Gold Beach
Medford
Klamath Falls
Brookings
CALIFORNIA

Please select all **developed** Oregon Coast beaches you took **at least one (1) day trip** to for outdoor recreation between **April 2021 and March 2022**.

- ☐ Gearhart/Seaside
- ☐ Cannon Beach
- ☐ Manzanita
- ☐ Rockaway Beach
- ☐ Pacific City
- ☐ Neskowin
- ☐ Lincoln City north (D River to Roads End)
- ☐ Lincoln City south (Siletz Bay to Nelscott Beach)
- ☐ Gleneden Beach area (Lincoln Beach to Salishan)
- ☐ Newport (Nye and Agate Beaches)
- ☐ Waldport (Bayshore)
- ☐ Yachats
- ☐ Florence (Heceta Beach)
- ☐ Bandon
- ☐ Gold Beach
- ☐ Brookings
- ☐ ⊗None of the above
- ☐ ⊗Not sure

Page Break

Q25 In each season during the previous 12 months, how many **day trips** did you take to a **developed beach** in **Gearhart/Seaside**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any day trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q27
You indicated you are not sure how many **day trips** you took to Gearhart/Seaside in at least one season. Below are the responses you gave:
Spring: Apr - Jun 2021: ${Q25/ChoiceGroup/SelectedAnswers/10}

Summer: Jul - Sep 2021: ${Q25/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q25/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q25/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total day trips** did you take to a **developed beach** in **Gearhart/Seaside**? For frequent visitors, numbers are provided based on frequency in parentheses.

○ 1-3 trips (less than once a season)

○ 4-10 trips (about one to two times a season)

○ 12-24 trips (about one to two times a month)

○ 52-104 trips (about one to two times a week)

○ 156+ (at least three times a week)

○ Not sure

Page Break

Q29 You stated that you are not sure how often you took **day trips** to Gearhart/Seaside between April 2021 and March 2022.

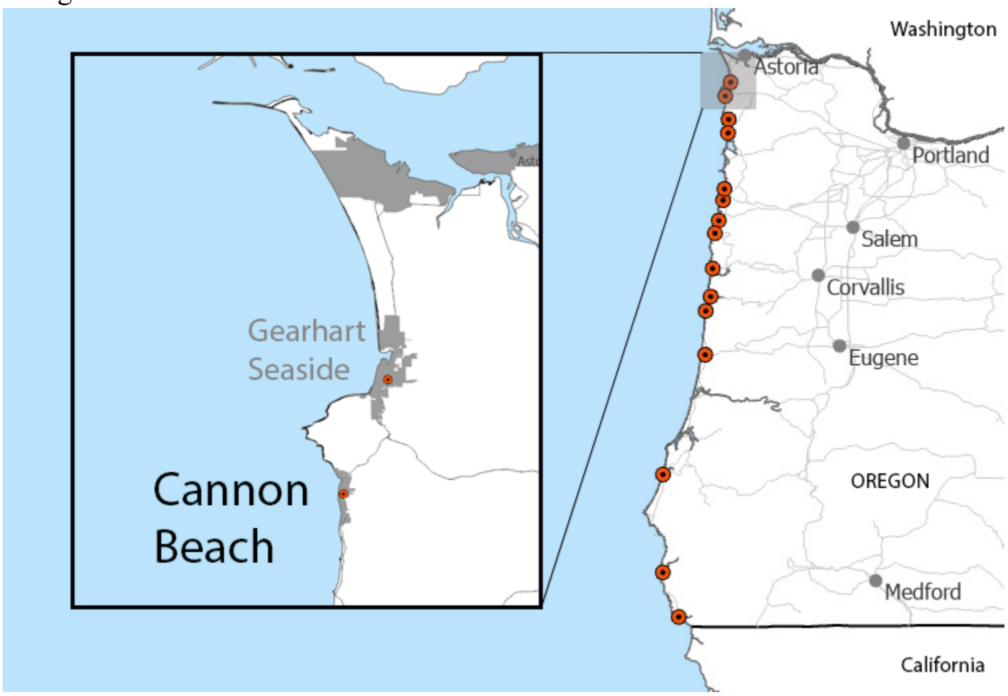
To the best of your recollection, during this 12-month period did you take a **day trip** to visit a **developed beach** in Gearhart/Seaside for outdoor recreation **more or less than once a season (a 3 month period)**?

○ More than once a season

○ Less than once a season

Page Break

Q31 In each season during the previous 12 months, how many **day trips** did you take to a **developed beach** in **Cannon Beach**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any day trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q33
You indicated you are not sure how many **day trips** you took to Cannon Beach in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q31/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q31/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q31/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q31/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total day trips** did you

take to a **developed beach** in <u>**Cannon Beach**</u>? For frequent visitors, numbers are provided based on frequency in parentheses.

- ◯ 1-3 trips (less than once a season)
- ◯ 4-10 trips (about one to two times a season)
- ◯ 12-24 trips (about one to two times a month)
- ◯ 52-104 trips (about one to two times a week)
- ◯ 156+ (at least three times a week)
- ◯ Not sure

Page Break

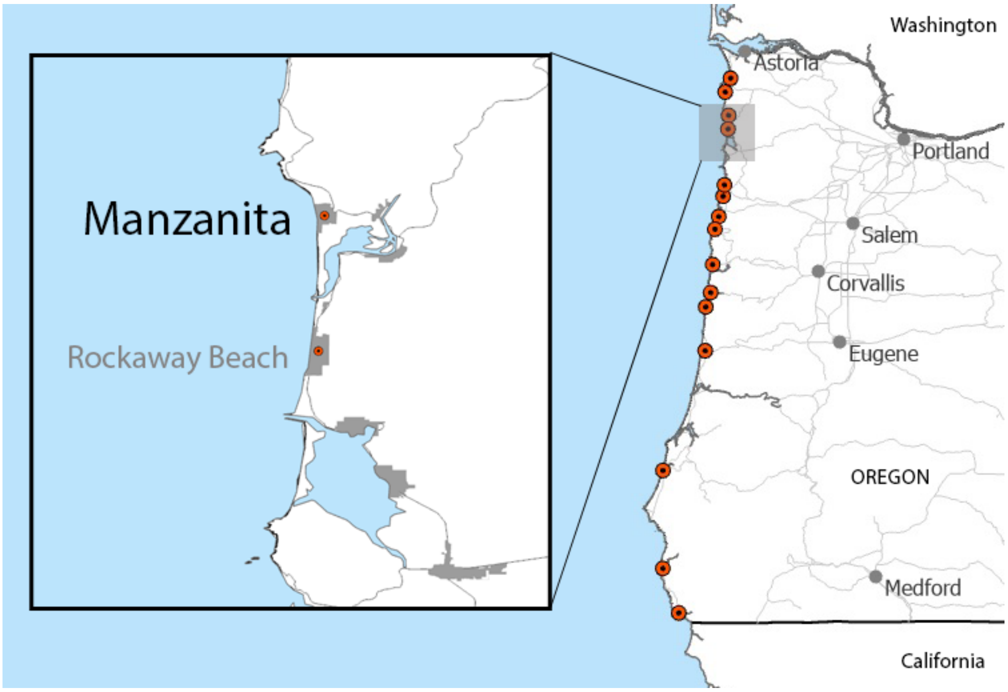
Q35 You stated that you are not sure how often you took **day trips** to Cannon Beach between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **day trip** to visit a **developed beach** in Cannon Beach for outdoor recreation **more or less than once a season (a 3 month period)**?

- ◯ More than once a season
- ◯ Less than once a season

Page Break

Q37 In each season during the previous 12 months, how many **day trips** did you take to a **developed beach** in <u>**Manzanita**</u>? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any day trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q39
You indicated you are not sure how many **day trips** you took to Manzanita in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q37/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q37/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q37/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q37/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total day trips** did you

take to a **developed beach** in **<u>Manzanita</u>**? For frequent visitors, numbers are provided based on frequency in parentheses.

- ◯ 1-3 trips (less than once a season)
- ◯ 4-10 trips (about one to two times a season)
- ◯ 12-24 trips (about one to two times a month)
- ◯ 52-104 trips (about one to two times a week)
- ◯ 156+ (at least three times a week)
- ◯ Not sure

Page Break

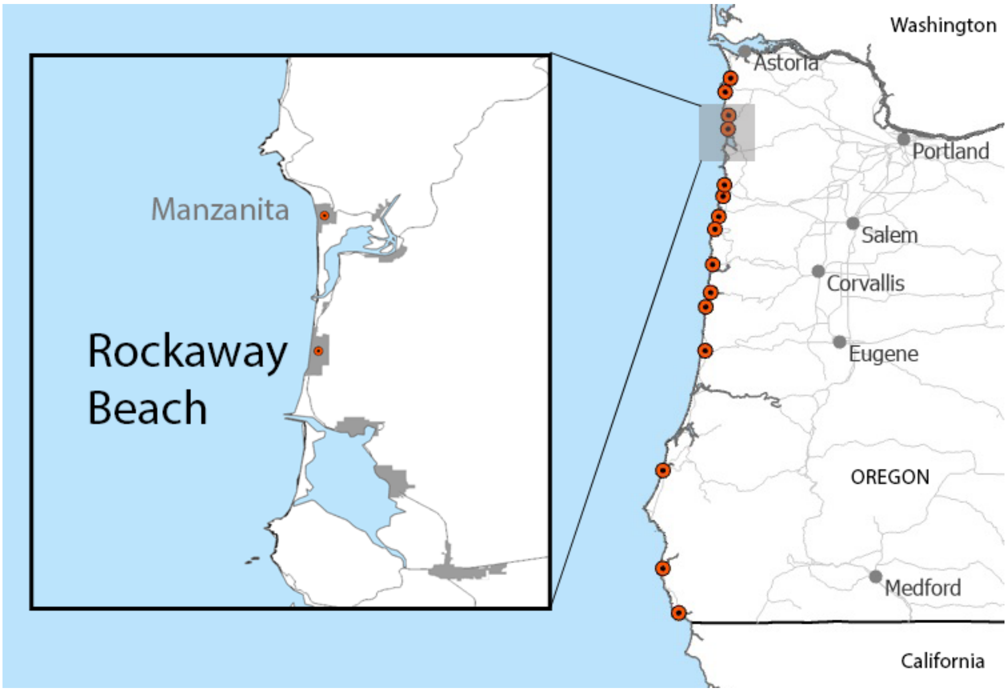
Q41 You stated that you are not sure how often you took **day trips** to Manzanita between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **day trip** to visit a **developed beach** in Manzanita for outdoor recreation **more or less than once a season (a 3 month period)**?

- ◯ More than once a season
- ◯ Less than once a season

Page Break

Q43 In each season during the previous 12 months, how many **day trips** did you take to a **developed beach** in **<u>Rockaway Beach</u>**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any day trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q45 You indicated you are not sure how many **day trips** you took to Rockaway Beach in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q43/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q43/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q43/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q43/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total day trips** did you

take to a **developed beach** in **Rockaway Beach**? For frequent visitors, numbers are provided based on frequency in parentheses.

- ◯ 1-3 trips (less than once a season)
- ◯ 4-10 trips (about one to two times a season)
- ◯ 12-24 trips (about one to two times a month)
- ◯ 52-104 trips (about one to two times a week)
- ◯ 156+ (at least three times a week)
- ◯ Not sure

Page Break

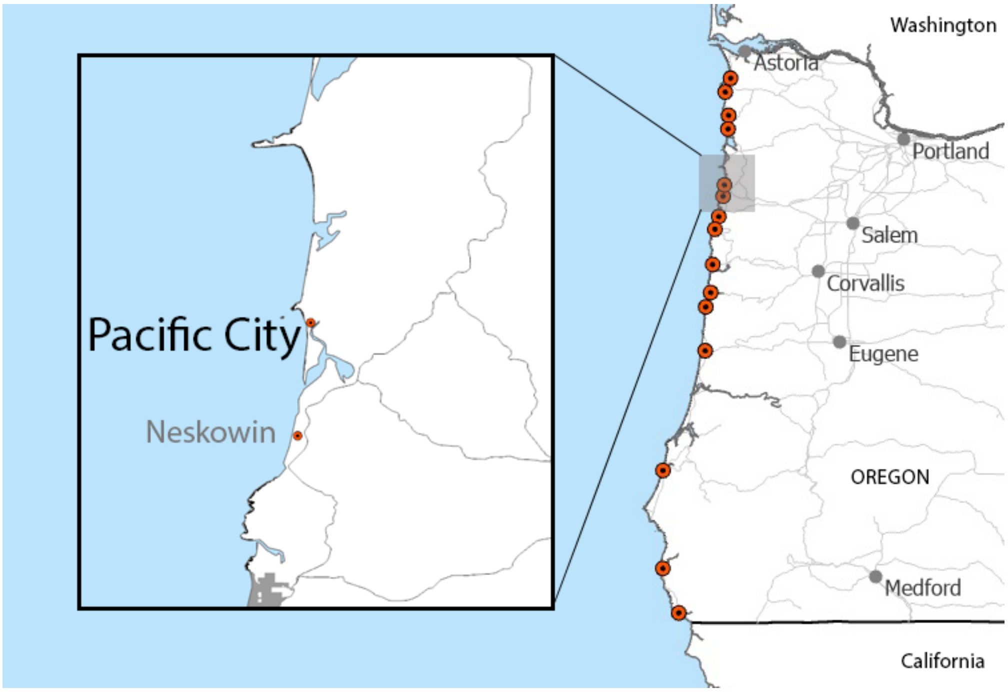
Q47 You stated that you are not sure how often you took **day trips** to Rockaway Beach between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **day trip** to visit a **developed beach** in **Rockaway Beach** for outdoor recreation **more or less than once a season (a 3 month period)**?

- ◯ More than once a season
- ◯ Less than once a season

Page Break

Q49 In each season during the previous 12 months, how many **day trips** did you take to a **developed beach** in **Pacific City**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any day trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q51 You indicated you are not sure how many **day trips** you took to Pacific City in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q49/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q49/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q49/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q49/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total day trips** did you

take to a **developed beach** in **<u>Pacific City</u>**? For frequent visitors, numbers are provided based on frequency in parentheses.

- ◯ 1-3 trips (less than once a season)
- ◯ 4-10 trips (about one to two times a season)
- ◯ 12-24 trips (about one to two times a month)
- ◯ 52-104 trips (about one to two times a week)
- ◯ 156+ (at least three times a week)
- ◯ Not sure

Page Break

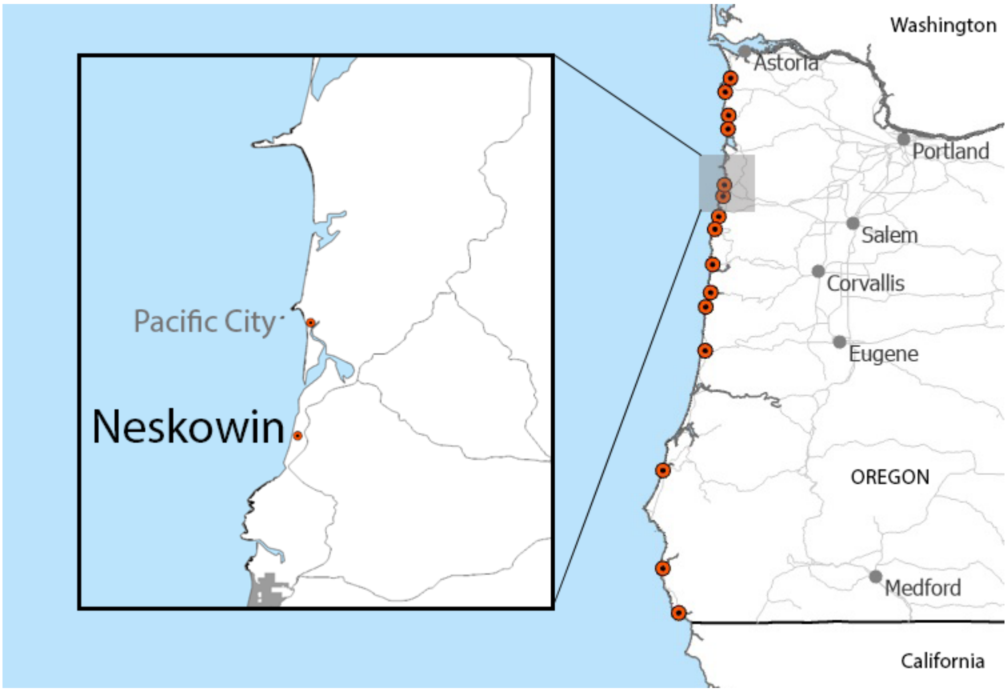
Q53 You stated that you are not sure how often you took **day trips** to Pacific City between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **day trip** to visit a **developed beach** in **<u>Pacific City</u>** for outdoor recreation **more or less than once a season (a 3 month period)**?

- ◯ More than once a season
- ◯ Less than once a season

Page Break

Q55 In each season during the previous 12 months, how many **day trips** did you take to a **developed beach** in **<u>Neskowin</u>**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any day trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q57 You indicated you are not sure how many **day trips** you took to Neskowin in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q55/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q55/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q55/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q55/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total day trips** did you

take to a **developed beach** in **<u>Neskowin</u>**? For frequent visitors, numbers are provided based on frequency in parentheses.

◯ 1-3 trips (less than once a season)

◯ 4-10 trips (about one to two times a season)

◯ 12-24 trips (about one to two times a month)

◯ 52-104 trips (about one to two times a week)

◯ 156+ (at least three times a week)

◯ Not sure

Page Break

Q59 You stated that you are not sure how often you took **day trips** to Neskowin between April 2021 and March 2022.

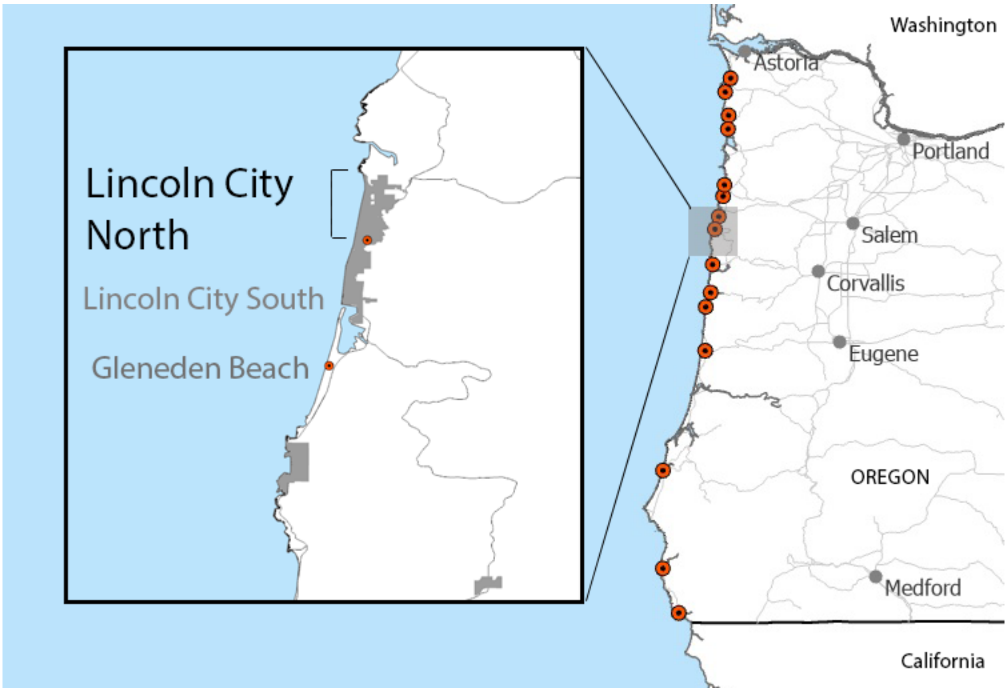
To the best of your recollection, during this 12-month period did you take a **day trip** to visit a **developed beach** in **<u>Neskowin</u>** for outdoor recreation **more or less than once a season (a 3 month period)**?

◯ More than once a season

◯ Less than once a season

Page Break

Q61 In each season during the previous 12 months, how many **day trips** did you take to a **developed beach** in **<u>Lincoln City north (D River to Roads End)</u>**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any day trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q63 You indicated you are not sure how many **day trips** you took to Lincoln City north (D River to Roads End) in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q61/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q61/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q61/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q61/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total day trips** did you

take to a **developed beach** in **Lincoln City north (D River to Roads End)**? For frequent visitors, numbers are provided based on frequency in parentheses.

○ 1-3 trips (less than once a season)

○ 4-10 trips (about one to two times a season)

○ 12-24 trips (about one to two times a month)

○ 52-104 trips (about one to two times a week)

○ 156+ (at least three times a week)

○ Not sure

Page Break

Q65 You stated that you are not sure how often you took **day trips** to Lincoln City north (D River to Roads End) between April 2021 and March 2022.

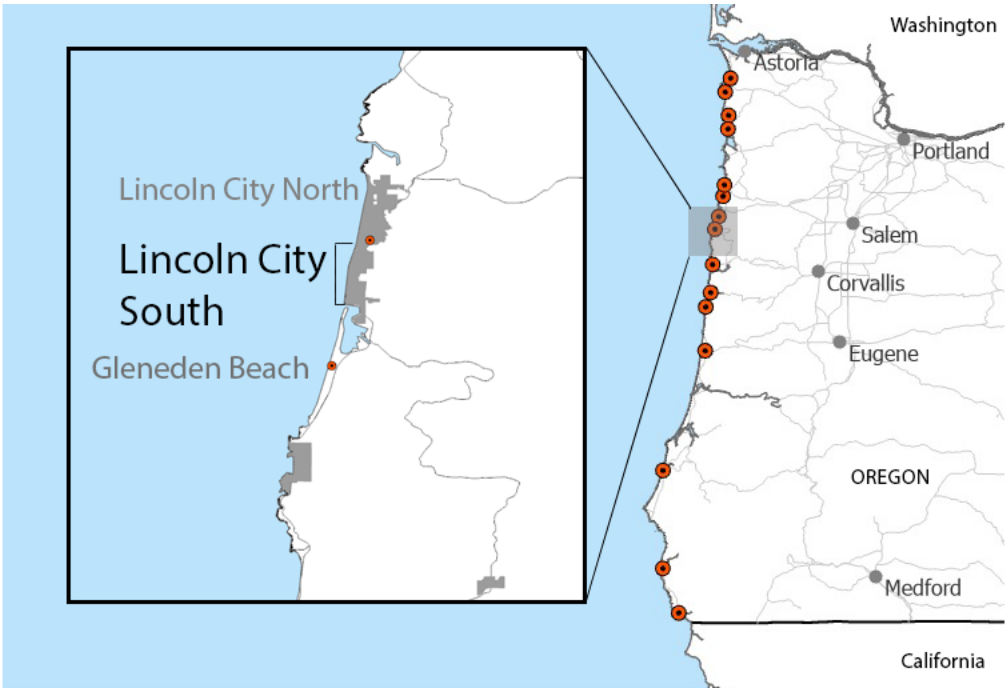
To the best of your recollection, during this 12-month period did you take a **day trip** to visit a **developed beach** in **Lincoln City north (D River to Roads End)** for outdoor recreation **more or less than once a season (a 3 month period)**?

○ More than once a season

○ Less than once a season

Page Break

Q67 In each season during the previous 12 months, how many **day trips** did you take to a **developed beach** in **Lincoln City south (Siletz Bay to Nelscott Beach)**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any day trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q69 You indicated you are not sure how many **day trips** you took to Lincoln City south (Siletz Bay to Nelscott Beach) in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q67/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q67/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q67/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q67/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total day trips** did you

take to a **developed beach** in **<u>Lincoln City south (Siletz Bay to Nelscott Beach)</u>**? For frequent visitors, numbers are provided based on frequency in parentheses.

- 1-3 trips (less than once a season)
- 4-10 trips (about one to two times a season)
- 12-24 trips (about one to two times a month)
- 52-104 trips (about one to two times a week)
- 156+ (at least three times a week)
- Not sure

Page Break

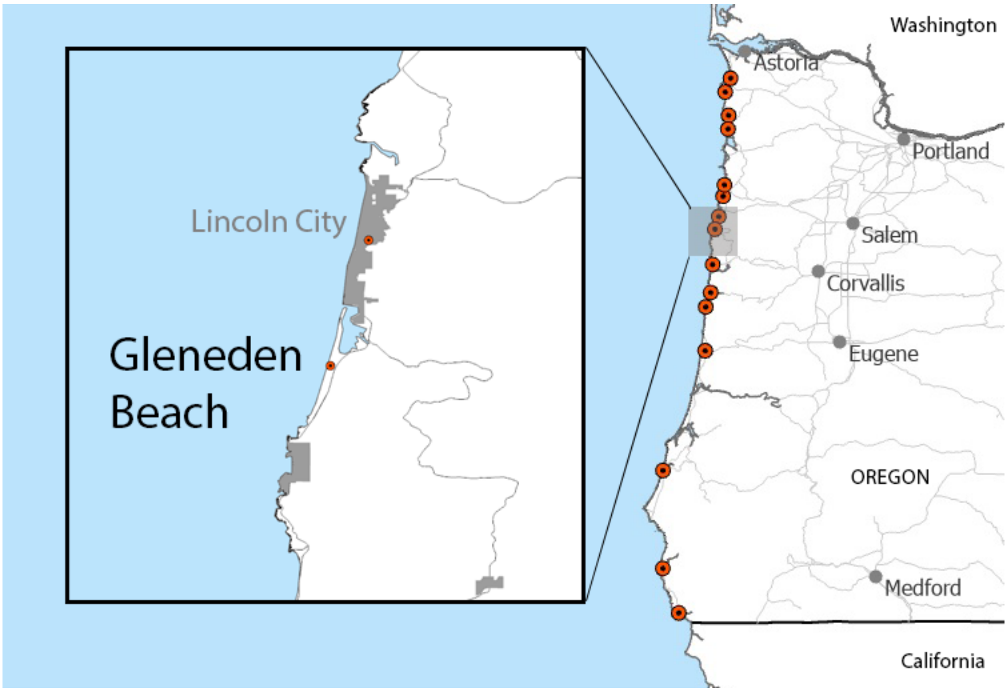
Q71 You stated that you are not sure how often you took **day trips** to Lincoln City south (Siletz Bay to Nelscott Beach) between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **day trip** to visit a **developed beach** in **<u>Lincoln City south (Siletz Bay to Nelscott Beach)</u>** for outdoor recreation **more or less than once a season (a 3 month period)**?

- More than once a season
- Less than once a season

Page Break

Q73 In each season during the previous 12 months, how many **day trips** did you take to a **developed beach** in the Gleneden Beach area (Lincoln Beach to Salishan)? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any day trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q75 You indicated you are not sure how many **day trips** you took to the Gleneden Beach area (Lincoln Beach to Salishan) in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q73/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q73/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q73/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q73/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total day trips** did you

take to a **developed beach** in the **Gleneden Beach area (Lincoln Beach to Salishan)**? For frequent visitors, numbers are provided based on frequency in parentheses.

- ○ 1-3 trips (less than once a season)
- ○ 4-10 trips (about one to two times a season)
- ○ 12-24 trips (about one to two times a month)
- ○ 52-104 trips (about one to two times a week)
- ○ 156+ (at least three times a week)
- ○ Not sure

Page Break

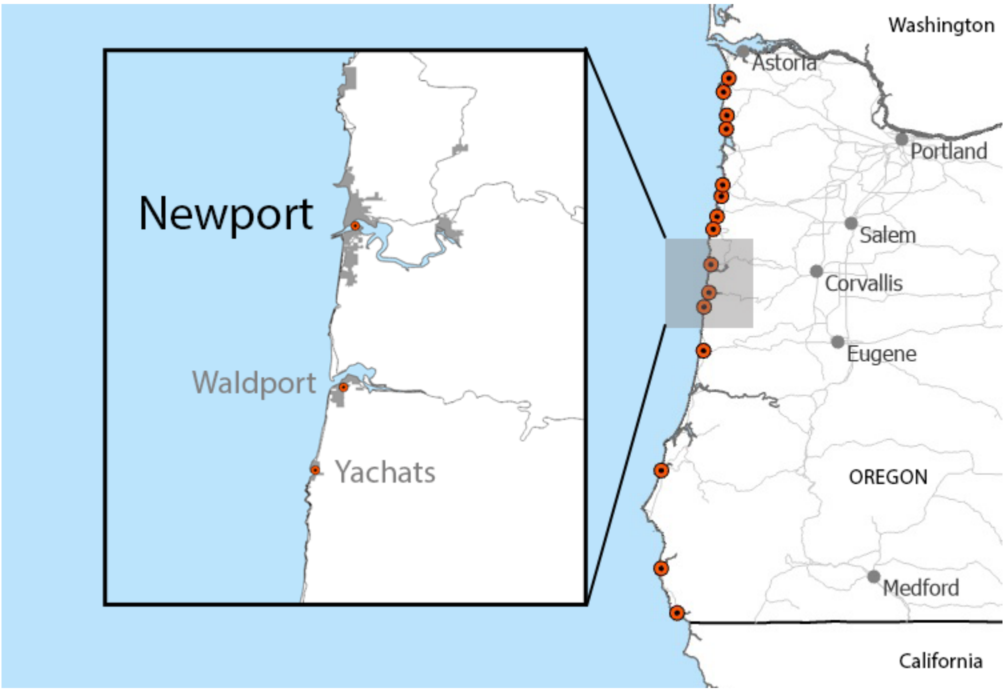
Q77 You stated that you are not sure how often you took **day trips** to the Gleneden Beach area (Lincoln Beach to Salishan) between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **day trip** to visit a **developed beach** in the **Gleneden Beach area (Lincoln Beach to Salishan)** for outdoor recreation **more or less than once a season (a 3 month period)**?

- ○ More than once a season
- ○ Less than once a season

Page Break

Q79 In each season during the previous 12 months, how many **day trips** did you take to a **developed beach** in **Newport (Nye and Agate Beaches)**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any day trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q81 You indicated you are not sure how many **day trips** you took to Newport (Nye and Agate Beaches) in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q79/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q79/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q79/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q79/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total day trips** did you

take to a **developed beach** in **Newport (Nye and Agate Beaches)**? For frequent visitors, numbers are provided based on frequency in parentheses.

○ 1-3 trips (less than once a season)

○ 4-10 trips (about one to two times a season)

○ 12-24 trips (about one to two times a month)

○ 52-104 trips (about one to two times a week)

○ 156+ (at least three times a week)

○ Not sure

Page Break

Q83 You stated that you are not sure how often you took **day trips** to Newport (Nye and Agate Beaches) between April 2021 and March 2022.

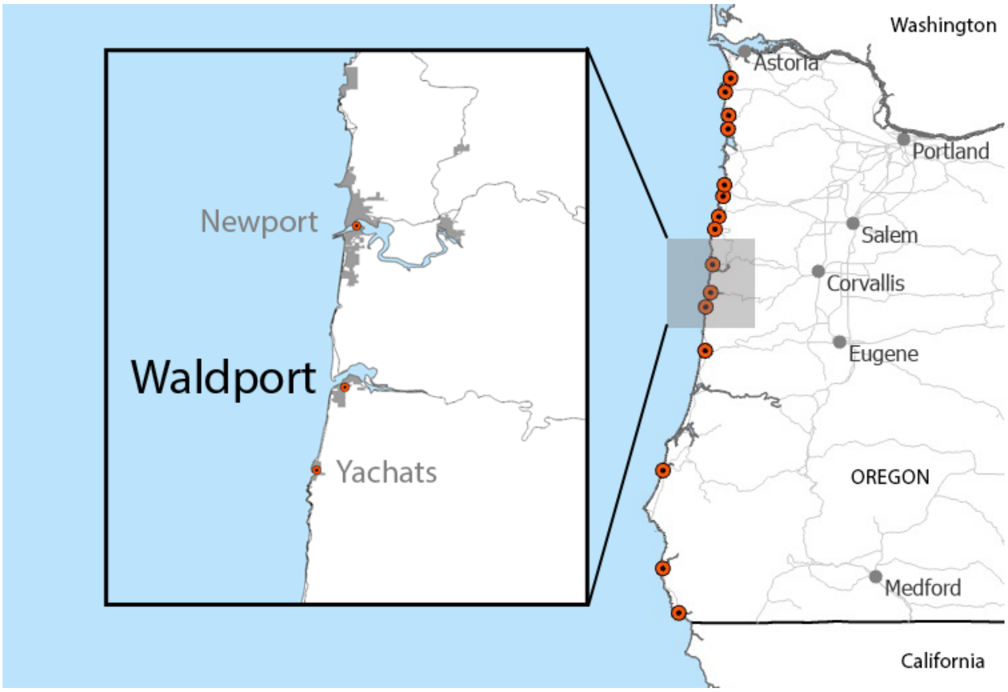
To the best of your recollection, during this 12-month period did you take a **day trip** to visit a **developed beach** in **Newport (Nye and Agate Beaches)** for outdoor recreation **more or less than once a season (a 3 month period)**?

○ More than once a season

○ Less than once a season

Page Break

Q85 In each season during the previous 12 months, how many **day trips** did you take to a **developed beach** in **Waldport (Bayshore)**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any day trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q87 You indicated you are not sure how many **day trips** you took to Waldport (Bayshore) in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q85/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q85/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q85/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q85/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total day trips** did you

take to a **developed beach** in **<u>Waldport (Bayshore)</u>**? For frequent visitors, numbers are provided based on frequency in parentheses.

- ◯ 1-3 trips (less than once a season)
- ◯ 4-10 trips (about one to two times a season)
- ◯ 12-24 trips (about one to two times a month)
- ◯ 52-104 trips (about one to two times a week)
- ◯ 156+ (at least three times a week)
- ◯ Not sure

Page Break

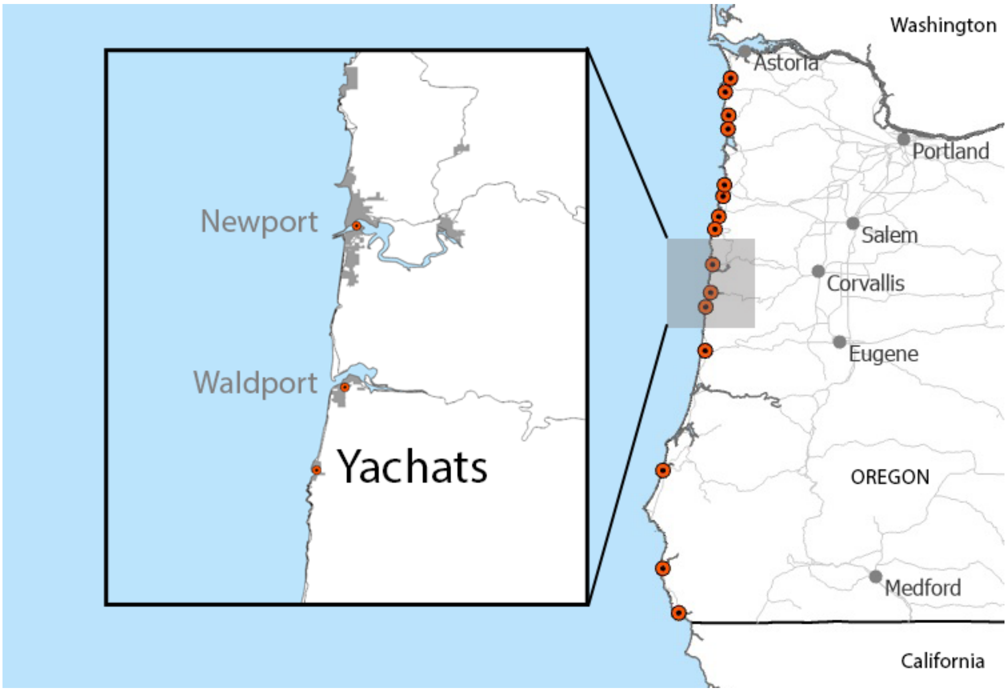
Q89 You stated that you are not sure how often you took **day trips** to Waldport (Bayshore) between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **day trip** to visit a **developed beach** in **<u>Waldport (Bayshore)</u>** for outdoor recreation **more or less than once a season (a 3 month period)**?

- ◯ More than once a season
- ◯ Less than once a season

Page Break

Q91 In each season during the previous 12 months, how many **day trips** did you take to a **developed beach** in **<u>Yachats</u>**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any day trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q93 You indicated you are not sure how many **day trips** you took to Yachats in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q91/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q91/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q91/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q91/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total day trips** did you

take to a **developed beach** in **Yachats**? For frequent visitors, numbers are provided based on frequency in parentheses.

- 1-3 trips (less than once a season)
- 4-10 trips (about one to two times a season)
- 12-24 trips (about one to two times a month)
- 52-104 trips (about one to two times a week)
- 156+ (at least three times a week)
- Not sure

Page Break

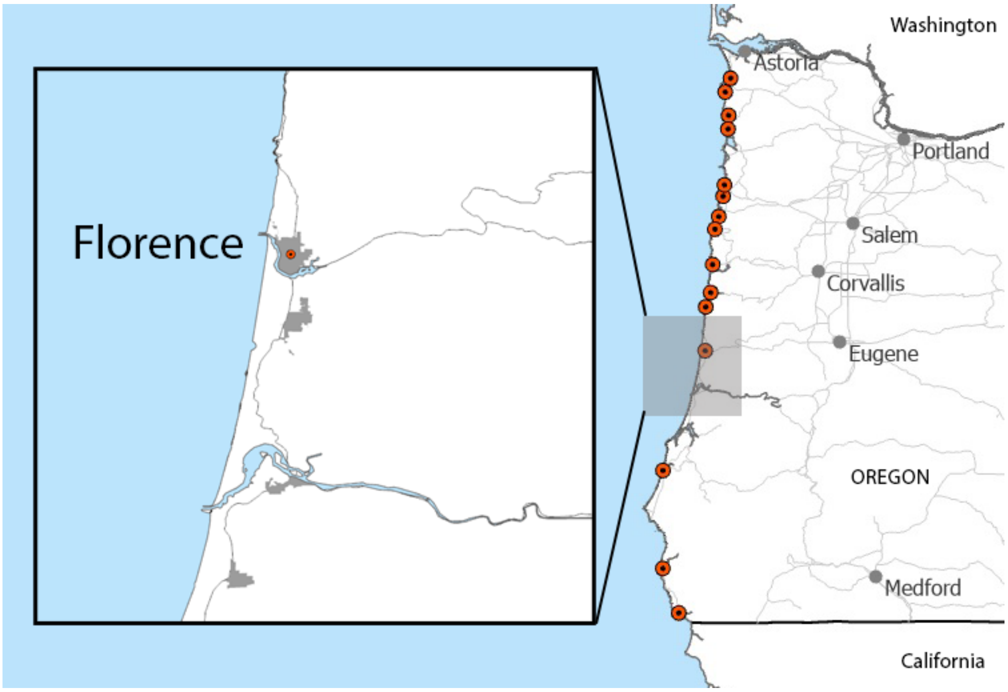
Q95 You stated that you are not sure how often you took **day trips** to Yachats between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **day trip** to visit a **developed beach** in **Yachats** for outdoor recreation **more or less than once a season (a 3 month period)**?

- More than once a season
- Less than once a season

Page Break

Q97 In each season during the previous 12 months, how many **day trips** did you take to a **developed beach** in **Florence (Heceta Beach)**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any day trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q99 You indicated you are not sure how many **day trips** you took to Florence (Heceta Beach) in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q97/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q97/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q97/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q97/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total day trips** did you

take to a **developed beach** in **<u>Florence (Heceta Beach)</u>**? For frequent visitors, numbers are provided based on frequency in parentheses.

◯ 1-3 trips (less than once a season)

◯ 4-10 trips (about one to two times a season)

◯ 12-24 trips (about one to two times a month)

◯ 52-104 trips (about one to two times a week)

◯ 156+ (at least three times a week)

◯ Not sure

Page Break

Q101 You stated that you are not sure how often you took **day trips** to Florence (Heceta Beach) between April 2021 and March 2022.

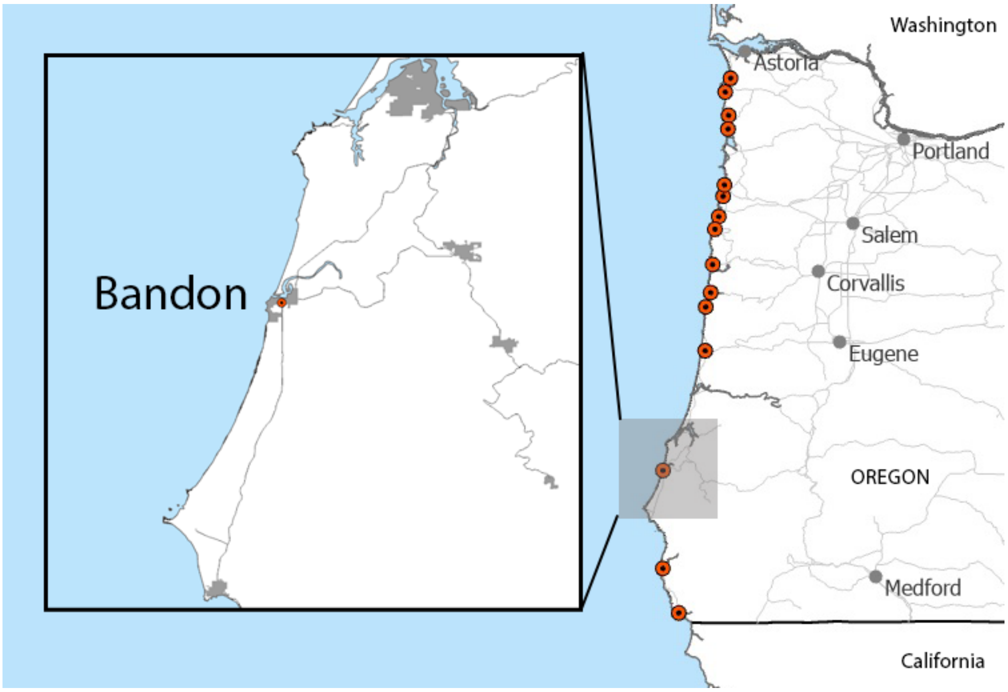
To the best of your recollection, during this 12-month period did you take a **day trip** to visit a **developed beach** in **<u>Florence (Heceta Beach)</u>** for outdoor recreation **more or less than once a season (a 3 month period)**?

◯ More than once a season

◯ Less than once a season

Page Break

Q103 In each season during the previous 12 months, how many **day trips** did you take to a **developed beach** in **<u>Bandon</u>**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any day trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q105 You indicated you are not sure how many **day trips** you took to Bandon in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q103/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q103/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q103/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q103/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total day trips** did you

take to a **developed beach** in **<u>Bandon</u>**? For frequent visitors, numbers are provided based on frequency in parentheses.

- 1-3 trips (less than once a season)
- 4-10 trips (about one to two times a season)
- 12-24 trips (about one to two times a month)
- 52-104 trips (about one to two times a week)
- 156+ (at least three times a week)
- Not sure

Page Break

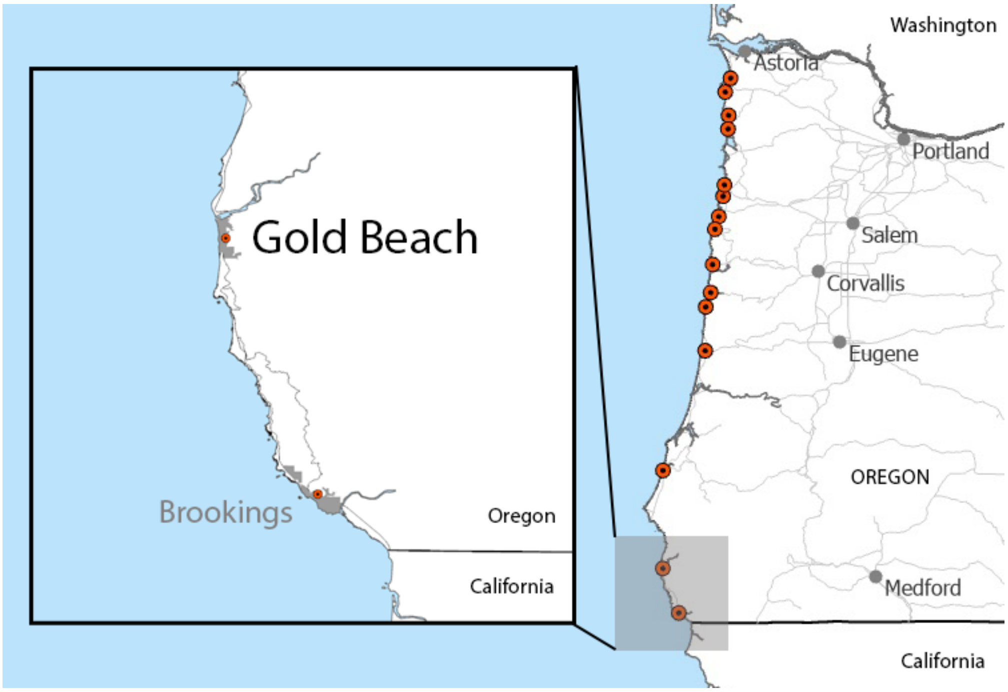
Q107 You stated that you are not sure how often you took **day trips** to Bandon between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **day trip** to visit a **developed beach** in **<u>Bandon</u>** for outdoor recreation **more or less than once a season (a 3 month period)**?

- More than once a season
- Less than once a season

Page Break

Q109 In each season during the previous 12 months, how many **day trips** did you take to a **developed beach** in **<u>Gold Beach</u>**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any day trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q111 You indicated you are not sure how many **day trips** you took to Gold Beach in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q109/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q109/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q109/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q109/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total day trips** did you

take to a **developed beach** in **<u>Gold Beach</u>**? For frequent visitors, numbers are provided based on frequency in parentheses.

○ 1-3 trips (less than once a season)

○ 4-10 trips (about one to two times a season)

○ 12-24 trips (about one to two times a month)

○ 52-104 trips (about one to two times a week)

○ 156+ (at least three times a week)

○ Not sure

Page Break

Q113 You stated that you are not sure how often you took **day trips** to Gold Beach between April 2021 and March 2022.

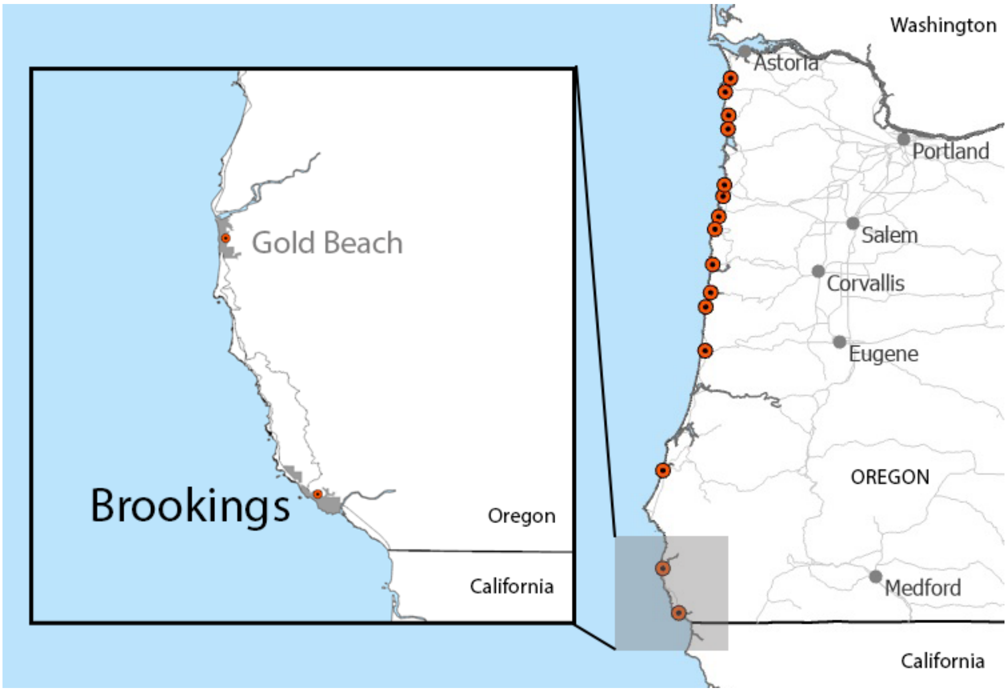
To the best of your recollection, during this 12-month period did you take a **day trip** to visit a **developed beach** in **<u>Gold Beach</u>** for outdoor recreation **more or less than once a season (a 3 month period)**?

○ More than once a season

○ Less than once a season

Page Break

Q115 In each season during the previous 12 months, how many **day trips** did you take to a **developed beach** in **<u>Brookings</u>**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any day trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q117 You indicated you are not sure how many **day trips** you took to Brookings in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q115/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q115/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q115/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q115/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total day trips** did you

take to a **developed beach** in **Brookings**? For frequent visitors, numbers are provided based on frequency in parentheses.

- 1-3 trips (less than once a season)
- 4-10 trips (about one to two times a season)
- 12-24 trips (about one to two times a month)
- 52-104 trips (about one to two times a week)
- 156+ (at least three times a week)
- Not sure

Page Break

Q119 You stated that you are not sure how often you took **day trips** to Brookings between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **day trip** to visit a **developed beach** in **Brookings** for outdoor recreation **more or less than once a season (a 3 month period)**?

- More than once a season
- Less than once a season

Page Break

Q121 In the **previous 12 months (April 2021 to March 2022)**, about how many **day trips** did you take to **developed** Oregon Coast beaches for outdoor recreation? Please answer to the best of your recollection.

- ○ 0 trips
- ○ 1 trip
- ○ 2-4 trips
- ○ 5-7 trips
- ○ 8-10 trips
- ○ 11+ trips
- ○ Not sure

Page Break

Q123 On June 30, 2021 Oregon lifted COVID-19 restrictions requiring indoor capacity limits and physical distancing.

Did you take **more, fewer, or the same amount of day trips** to visit **developed** Oregon Coast beaches after the lifting of these COVID-19 restrictions?

- ○ More trips after the lifting of these restrictions
- ○ The same amount of trips after the lifting of these restrictions
- ○ Fewer trips after the lifting of these restrictions
- ○ Not sure

End of Block: Revealed preference questions - day trips

Start of Block: Revealed preference questions - overnight trips

Q124 Next, we would now like to ask you about the number of **short overnight trips** (for example, renting a vacation home for 3 nights or less) you've taken to **developed** Oregon Coast beaches. Recall that a developed beach is in a coastal town and has buildings and other structures behind the beach.

We define a **short overnight trip** as leaving your home and traveling to a developed Oregon Coast beach for a multiple day visit that lasts **3 nights or less**, for the purpose of outdoor recreation and leisure.

If your **primary residence is near the Oregon Coast** and you visited the nearby developed beach for outdoor recreation, we would consider that a short overnight trip only if you spent the night(s) camping or staying at lodging facilities other than your home.

Above is a map with **<u>developed</u>** Oregon beaches labeled. In the last 12 months (**April 2021 to March 2022**) did you take **at least one (1) short overnight trip to a <u>developed</u> Oregon Coast beach for outdoor recreation** as the primary purpose?

◯ Yes

◯ No

Page Break

Q126 On a typical **short overnight trip** to a **developed** Oregon Coast beach, how many people do you go to the beach with?

- By myself
- 1 person besides myself
- 2-4 people besides myself
- 5 or more people besides myself
- Not sure

Page Break

Q128 On your **short overnight trips** to **developed** Oregon Coast beaches, do you usually spend the night(s) camping or staying at lodging facilities? Select all that apply.

- Lodging facilities - hotel, motel, inn, cabin, Airbnb, etc.
- Camping in an RV or other vehicle
- Camping in a tent or other temporary structure
- Other ____
- ⊗Not sure

Q129 How many nights do you spend on a typical **short overnight trip (3 nights or less)** to a **developed** Oregon Coast beach?

○ 1 night

○ 2 nights

○ 3 nights

○ Not sure

Page Break

Q131
Much of Oregon experienced a record-breaking heat wave during the last week of June 2021. In most places temperatures peaked well above 100 degrees Fahrenheit between June 26th and June 28th.

Did you take at least one (1) **short overnight trip** to visit **developed** Oregon Coast beaches for recreation because of the extreme heat during the last week of June 2021?

○ Yes

○ No

○ Not sure

Page Break

Q133 You indicated you took **short overnight trips (3 nights or less)** to visit **developed** Oregon beaches in the previous 12 months. Please review the map below and then answer the question below the map.

Please select all **<u>developed</u>** Oregon Coast beaches you took **at least one (1) short overnight trip** to for outdoor recreation between **April 2021 and March 2022**.

- ☐ Gearhart/Seaside
- ☐ Cannon Beach
- ☐ Manzanita
- ☐ Rockaway Beach
- ☐ Pacific City
- ☐ Neskowin
- ☐ Lincoln City north (D River to Roads End)
- ☐ Lincoln City south (Siletz Bay to Nelscott Beach)
- ☐ Gleneden Beach area (Lincoln Beach to Salishan)
- ☐ Newport (Nye and Agate Beaches)

▢Waldport (Bayshore)

▢Yachats

▢Florence (Heceta Beach)

▢Bandon

▢Gold Beach

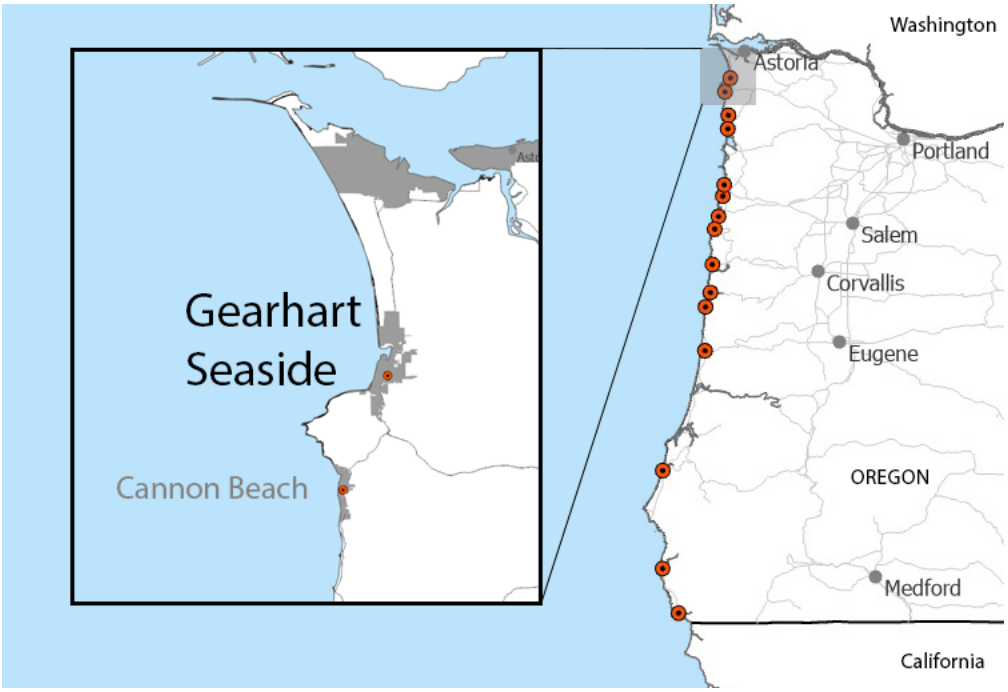
▢Brookings

▢⊗None of the above

▢⊗Not sure

Page Break

Q135 In each season during the previous 12 months, how many **short overnight trips** did you take to a **developed beach** in **<u>Gearhart/Seaside</u>**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any short overnight trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q137 You indicated you are not sure how many **short overnight trips** you took to Gearhart/Seaside in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q135/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q135/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q135/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q135/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total short overnight**

trips did you take to a **developed beach** in **Gearhart/Seaside**? For frequent visitors, numbers are provided based on frequency in parentheses.

- 1-3 trips (less than once a season)
- 4-10 trips (about one to two times a season)
- 12-24 trips (about one to two times a month)
- 52+ trips (at least once a week)
- Not sure

Page Break

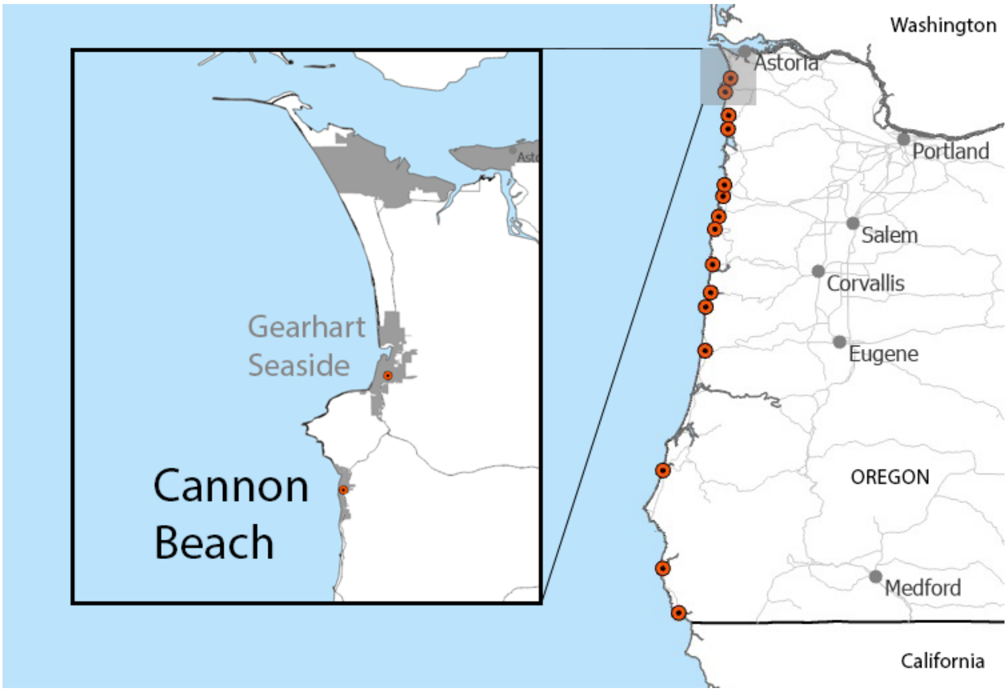
Q139 You stated that you are not sure how often you took **short overnight trips** to Gearhart/Seaside between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **short overnight trip** to visit a **developed beach** in Gearhart/Seaside for outdoor recreation **more or less than once a season (a 3 month period)**?

- More than once a season
- Less than once a season

Page Break

Q141 In each season during the previous 12 months, how many **short overnight trips** did you take to a **developed beach** in **Cannon Beach**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any short overnight trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q143 You indicated you are not sure how many **short overnight trips** you took to Cannon Beach in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q141/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q141/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q141/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q141/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total short overnight**

trips did you take to a **developed beach** in **<u>Cannon Beach</u>**? For frequent visitors, numbers are provided based on frequency in parentheses.

- ◯ 1-3 trips (less than once a season)
- ◯ 4-10 trips (about one to two times a season)
- ◯ 12-24 trips (about one to two times a month)
- ◯ 52+ trips (at least once a week)
- ◯ Not sure

Page Break

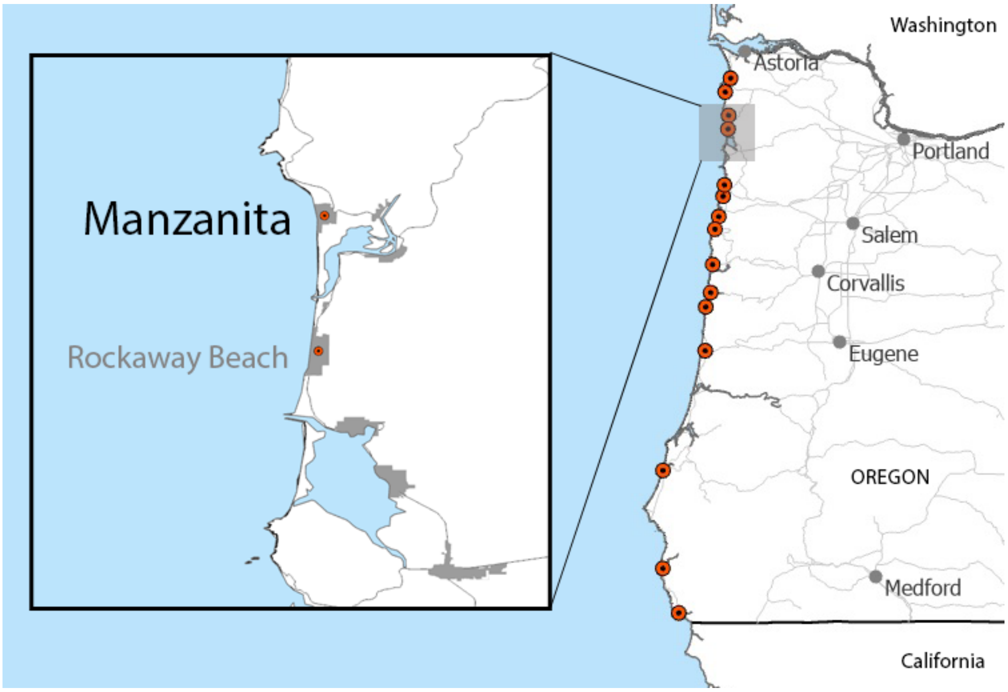
Q145 You stated that you are not sure how often you took **short overnight trips** to Cannon Beach between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **short overnight trip** to visit a **developed beach** in Cannon Beach for outdoor recreation **more or less than once a season (a 3 month period)**?

- ◯ More than once a season
- ◯ Less than once a season

Page Break

Q147 In each season during the previous 12 months, how many **short overnight trips** did you take to a **developed beach** in **<u>Manzanita</u>**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any short overnight trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q149 You indicated you are not sure how many **short overnight trips** you took to Manzanita in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q147/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q147/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q147/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q147/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total short overnight**

trips did you take to a **developed beach** in **Manzanita**? For frequent visitors, numbers are provided based on frequency in parentheses.

- ◯ 1-3 trips (less than once a season)
- ◯ 4-10 trips (about one to two times a season)
- ◯ 12-24 trips (about one to two times a month)
- ◯ 52+ trips (at least once a week)
- ◯ Not sure

Page Break

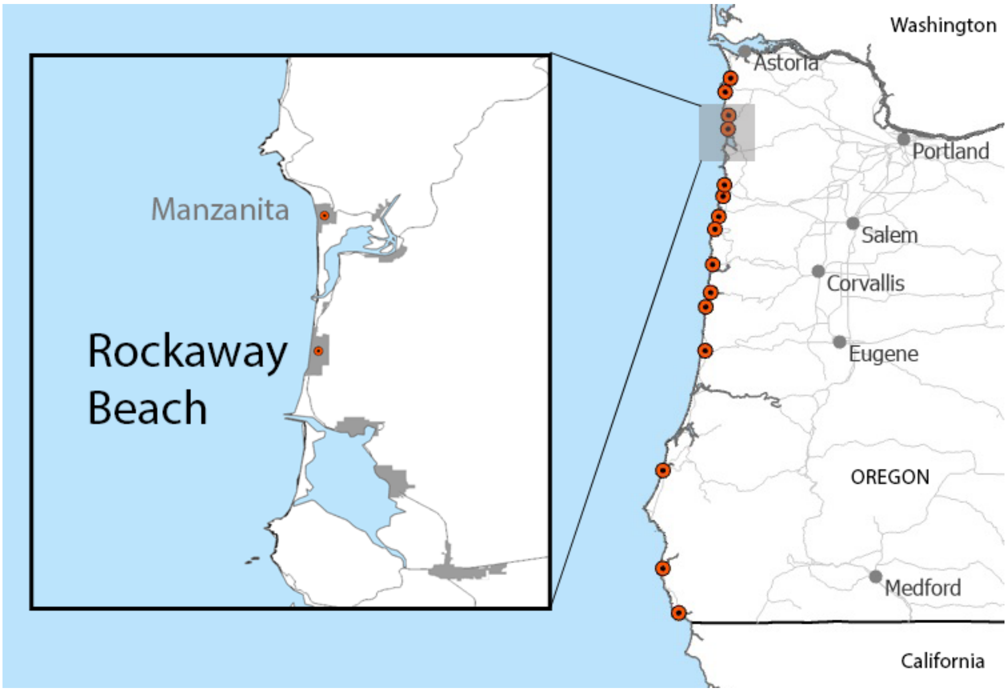
Q151 You stated that you are not sure how often you took **short overnight trips** to Manzanita between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **short overnight trip** to visit a **developed beach** in Manzanita for outdoor recreation **more or less than once a season (a 3 month period)**?

- ◯ More than once a season
- ◯ Less than once a season

Page Break

Q153 In each season during the previous 12 months, how many **short overnight trips** did you take to a **developed beach** in **Rockaway Beach**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any short overnight trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q155 You indicated you are not sure how many **short overnight trips** you took to Rockaway Beach in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q153/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q153/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q153/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q153/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total short overnight**

trips did you take to a **developed beach** in **Rockaway Beach**? For frequent visitors, numbers are provided based on frequency in parentheses.

- 1-3 trips (less than once a season)
- 4-10 trips (about one to two times a season)
- 12-24 trips (about one to two times a month)
- 52+ trips (at least once a week)
- Not sure

Page Break

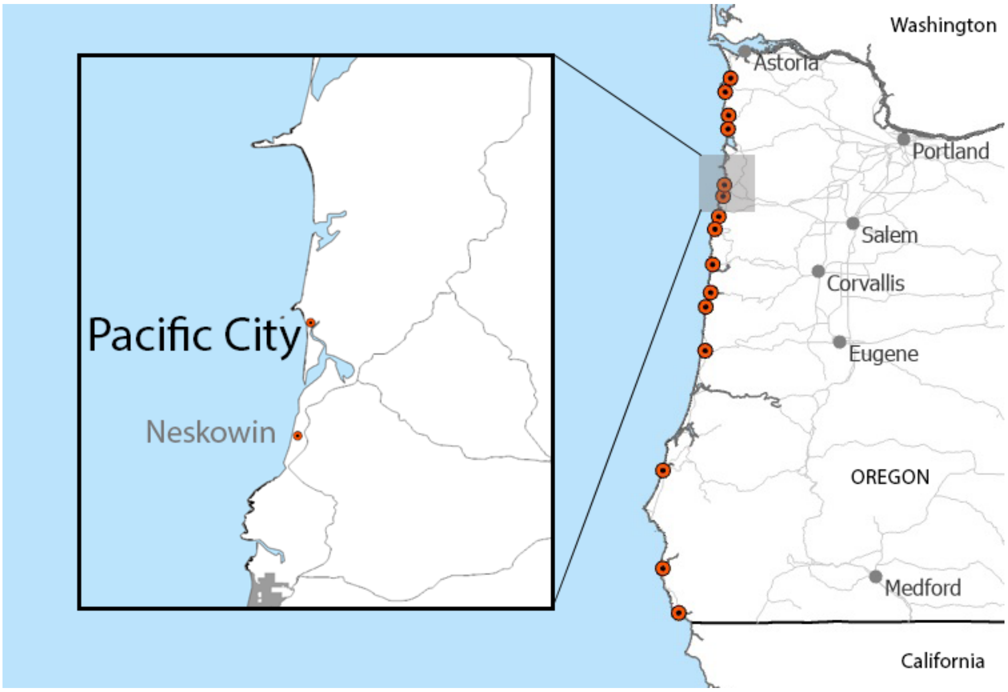
Q157 You stated that you are not sure how often you took **short overnight trips** to Rockaway Beach between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **short overnight trip** to visit a **developed beach** in **Rockaway Beach** for outdoor recreation **more or less than once a season (a 3 month period)**?

- More than once a season
- Less than once a season

Page Break

Q159 In each season during the previous 12 months, how many **short overnight trips** did you take to a **developed beach** in **Pacific City**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any short overnight trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q161 You indicated you are not sure how many **short overnight trips** you took to Pacific City in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q159/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q159/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q159/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q159/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total short overnight**

trips did you take to a **developed beach** in **Pacific City**? For frequent visitors, numbers are provided based on frequency in parentheses.

- ○ 1-3 trips (less than once a season)
- ○ 4-10 trips (about one to two times a season)
- ○ 12-24 trips (about one to two times a month)
- ○ 52+ trips (at least once a week)
- ○ Not sure

Page Break

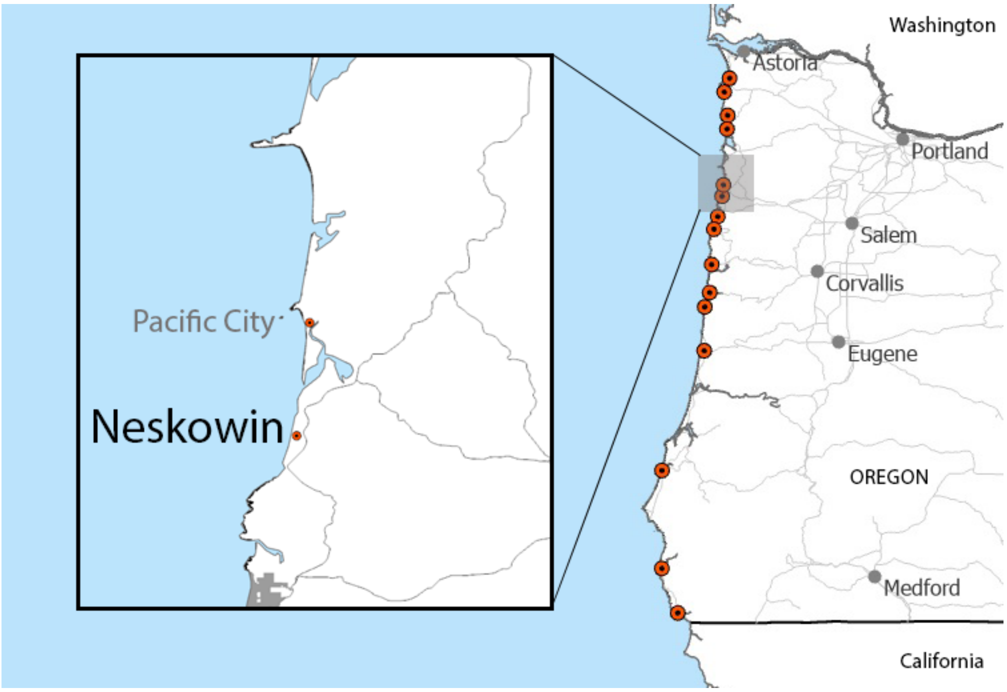
Q163 You stated that you are not sure how often you took **short overnight trips** to Pacific City between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **short overnight trip** to visit a **developed beach** in **Pacific City** for outdoor recreation **more or less than once a season (a 3 month period)**?

- ○ More than once a season
- ○ Less than once a season

Page Break

Q165 In each season during the previous 12 months, how many **short overnight trips** did you take to a **developed beach** in **Neskowin**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any short overnight trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q167 You indicated you are not sure how many **short overnight trips** you took to Neskowin in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q165/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q165/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q165/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q165/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total short overnight**

trips did you take to a **developed beach** in **<u>Neskowin</u>**? For frequent visitors, numbers are provided based on frequency in parentheses.

- ◯ 1-3 trips (less than once a season)
- ◯ 4-10 trips (about one to two times a season)
- ◯ 12-24 trips (about one to two times a month)
- ◯ 52+ trips (at least once a week)
- ◯ Not sure

Page Break

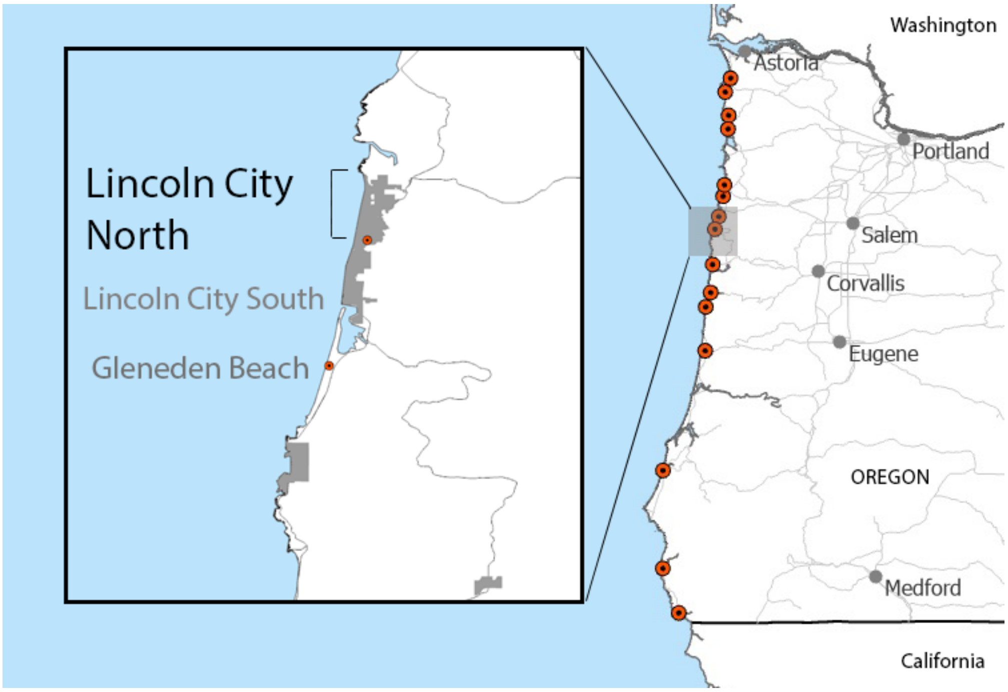
Q169 You stated that you are not sure how often you took **short overnight trips** to Neskowin between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **short overnight trip** to visit a **developed beach** in **<u>Neskowin</u>** for outdoor recreation **more or less than once a season (a 3 month period)**?

- ◯ More than once a season
- ◯ Less than once a season

Page Break

Q171 In each season during the previous 12 months, how many **short overnight trips** did you take to a **developed beach** in **<u>Lincoln City north (D River to Roads End)</u>**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any short overnight trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q173 You indicated you are not sure how many **short overnight trips** you took to Lincoln City north (D River to Roads End) in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q171/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q171/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q171/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q171/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total short overnight**

trips did you take to a **developed beach** in **<u>Lincoln City north (D River to Roads End)</u>**? For frequent visitors, numbers are provided based on frequency in parentheses.

- ○ 1-3 trips (less than once a season)
- ○ 4-10 trips (about one to two times a season)
- ○ 12-24 trips (about one to two times a month)
- ○ 52+ trips (at least once a week)
- ○ Not sure

Page Break

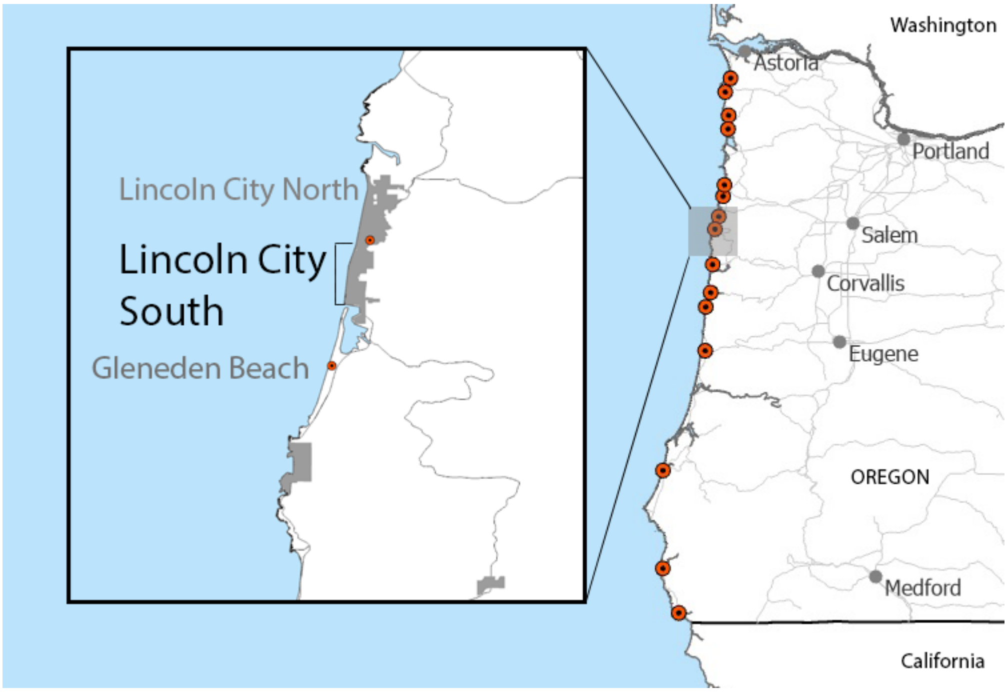
Q175 You stated that you are not sure how often you took **short overnight trips** to Lincoln City north (D River to Roads End) between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **short overnight trip** to visit a **developed beach** in **<u>Lincoln City north (D River to Roads End)</u>** for outdoor recreation **more or less than once a season (a 3 month period)**?

- ○ More than once a season
- ○ Less than once a season

Page Break

Q177 In each season during the previous 12 months, how many **short overnight trips** did you take to a **developed beach** in **<u>Lincoln City south (Siletz Bay to Nelscott Beach)</u>**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any short overnight trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q179 You indicated you are not sure how many **short overnight trips** you took to Lincoln City south (Siletz Bay to Nelscott Beach) in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q177/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q177/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q177/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q177/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total short overnight**

trips did you take to a **developed beach** in **<u>Lincoln City south (Siletz Bay to Nelscott Beach)</u>**? For frequent visitors, numbers are provided based on frequency in parentheses.

○ 1-3 trips (less than once a season)

○ 4-10 trips (about one to two times a season)

○ 12-24 trips (about one to two times a month)

○ 52+ trips (at least once a week)

○ Not sure

Page Break

Q181 You stated that you are not sure how often you took **short overnight trips** to Lincoln City south (Siletz Bay to Nelscott Beach) between April 2021 and March 2022.

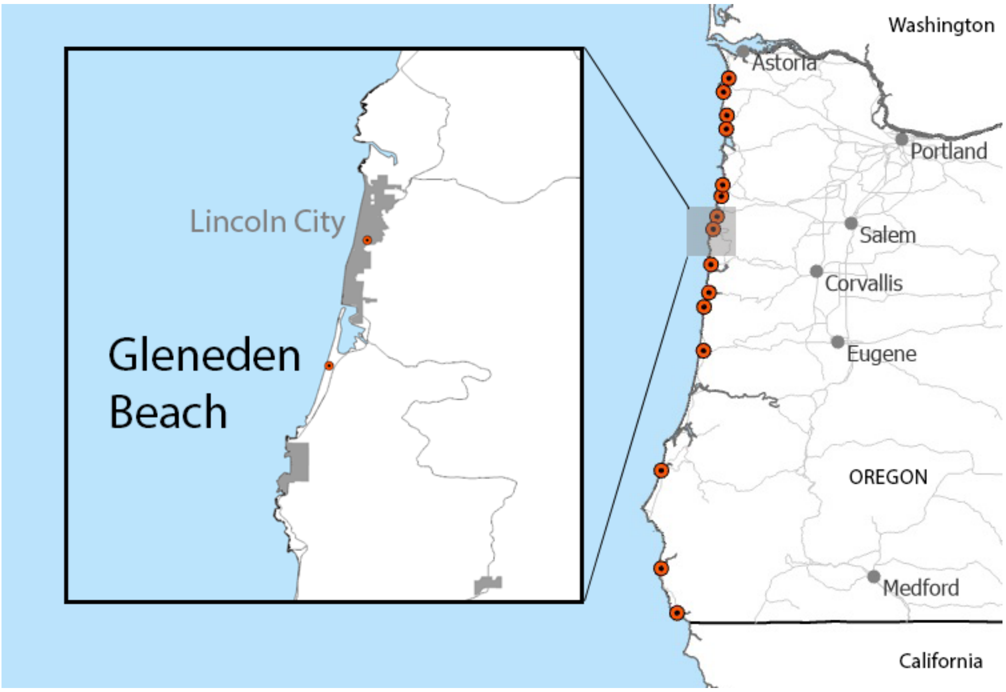
To the best of your recollection, during this 12-month period did you take a **short overnight trip** to visit a **developed beach** in **<u>Lincoln City south (Siletz Bay to Nelscott Beach)</u>** for outdoor recreation **more or less than once a season (a 3 month period)**?

○ More than once a season

○ Less than once a season

Page Break

Q183 In each season during the previous 12 months, how many **short overnight trips** did you take to a **developed beach** in the Gleneden Beach area (Lincoln Beach to Salishan)? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any short overnight trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q185 You indicated you are not sure how many **short overnight trips** you took to the Gleneden Beach area (Lincoln Beach to Salishan) in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q183/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q183/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q183/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q183/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total short overnight**

trips did you take to a **developed beach** in the **<u>Gleneden Beach area (Lincoln Beach to Salishan)</u>**? For frequent visitors, numbers are provided based on frequency in parentheses.

○ 1-3 trips (less than once a season)

○ 4-10 trips (about one to two times a season)

○ 12-24 trips (about one to two times a month)

○ 52+ trips (at least once a week)

○ Not sure

Page Break

Q187 You stated that you are not sure how often you took **short overnight trips** to the Gleneden Beach area (Lincoln Beach to Salishan) between April 2021 and March 2022.

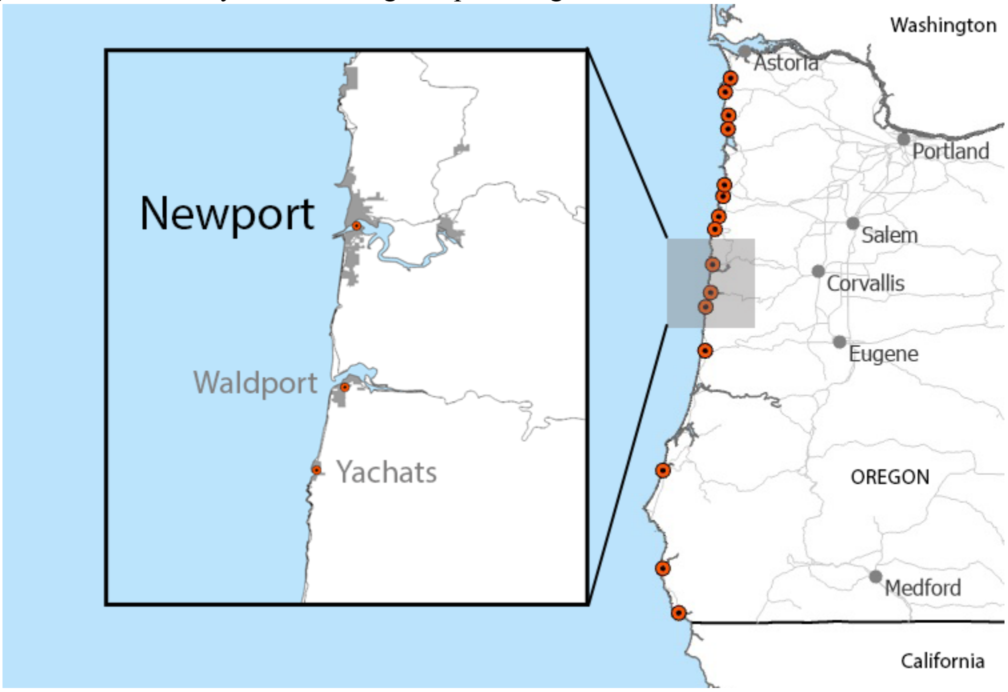
To the best of your recollection, during this 12-month period did you take a **short overnight trip** to visit a **developed beach** in the **<u>Gleneden Beach area (Lincoln Beach to Salishan)</u>** for outdoor recreation **more or less than once a season (a 3 month period)**?

○ More than once a season

○ Less than once a season

Page Break

Q189 In each season during the previous 12 months, how many **short overnight trips** did you take to a **developed beach** in **<u>Newport (Nye and Agate Beaches)</u>**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any short overnight trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q191 You indicated you are not sure how many **short overnight trips** you took to Newport (Nye and Agate Beaches) in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q189/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q189/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q189/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q189/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total short overnight**

trips did you take to a **developed beach** in **Newport (Nye and Agate Beaches)**? For frequent visitors, numbers are provided based on frequency in parentheses.

- ○ 1-3 trips (less than once a season)
- ○ 4-10 trips (about one to two times a season)
- ○ 12-24 trips (about one to two times a month)
- ○ 52+ trips (at least once a week)
- ○ Not sure

Page Break

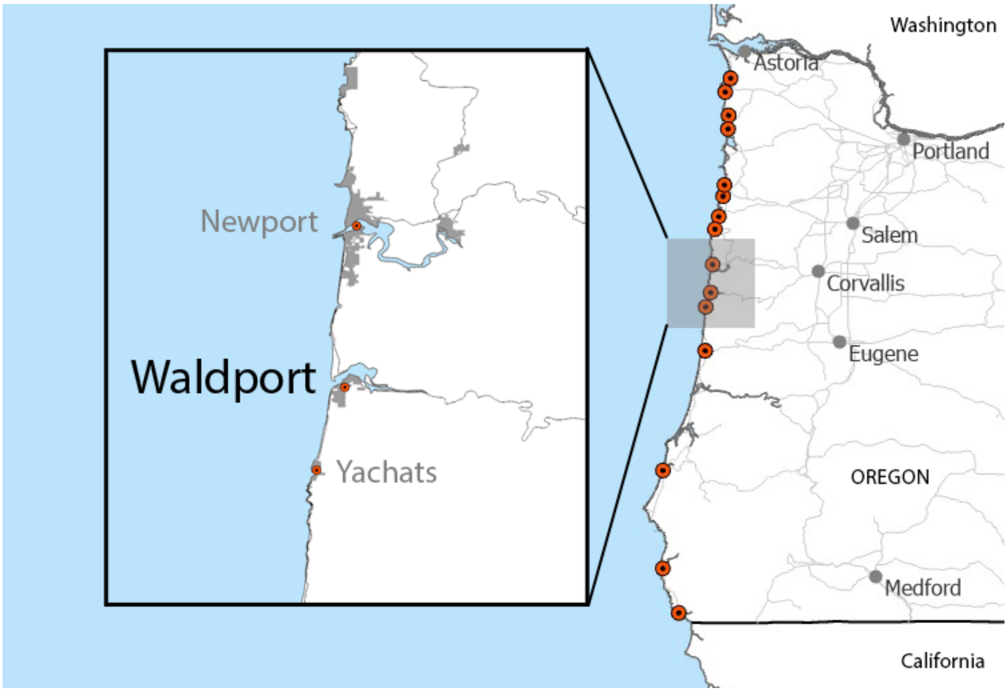
Q193 You stated that you are not sure how often you took **short overnight trips** to Newport (Nye and Agate Beaches) between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **short overnight trip** to visit a **developed beach** in **Newport (Nye and Agate Beaches)** for outdoor recreation **more or less than once a season (a 3 month period)**?

- ○ More than once a season
- ○ Less than once a season

Page Break

Q195 In each season during the previous 12 months, how many **short overnight trips** did you take to a **developed beach** in **Waldport (Bayshore)**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any short overnight trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q197 You indicated you are not sure how many **short overnight trips** you took to Waldport (Bayshore) in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q195/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q195/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q195/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q195/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total short overnight**

trips did you take to a **developed beach** in **Waldport (Bayshore)**? For frequent visitors, numbers are provided based on frequency in parentheses.

- ◯ 1-3 trips (less than once a season)
- ◯ 4-10 trips (about one to two times a season)
- ◯ 12-24 trips (about one to two times a month)
- ◯ 52+ trips (at least once a week)
- ◯ Not sure

Page Break

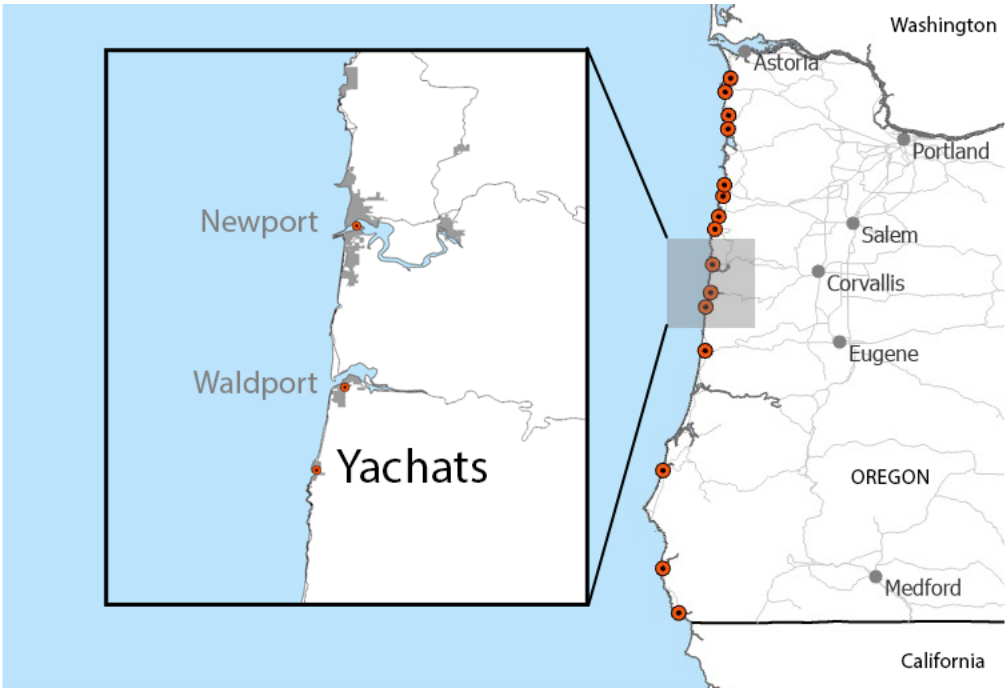
Q199 You stated that you are not sure how often you took **short overnight trips** to Waldport (Bayshore) between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **short overnight trip** to visit a **developed beach** in **Waldport (Bayshore)** for outdoor recreation **more or less than once a season (a 3 month period)**?

- ◯ More than once a season
- ◯ Less than once a season

Page Break

Q201 In each season during the previous 12 months, how many **short overnight trips** did you take to a **developed beach** in **Yachats**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any short overnight trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q203 You indicated you are not sure how many **short overnight trips** you took to Yachats in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q201/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q201/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q201/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q201/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total short overnight**

trips did you take to a **developed beach** in **Yachats**? For frequent visitors, numbers are provided based on frequency in parentheses.

- 1-3 trips (less than once a season)
- 4-10 trips (about one to two times a season)
- 12-24 trips (about one to two times a month)
- 52+ trips (at least once a week)
- Not sure

Page Break

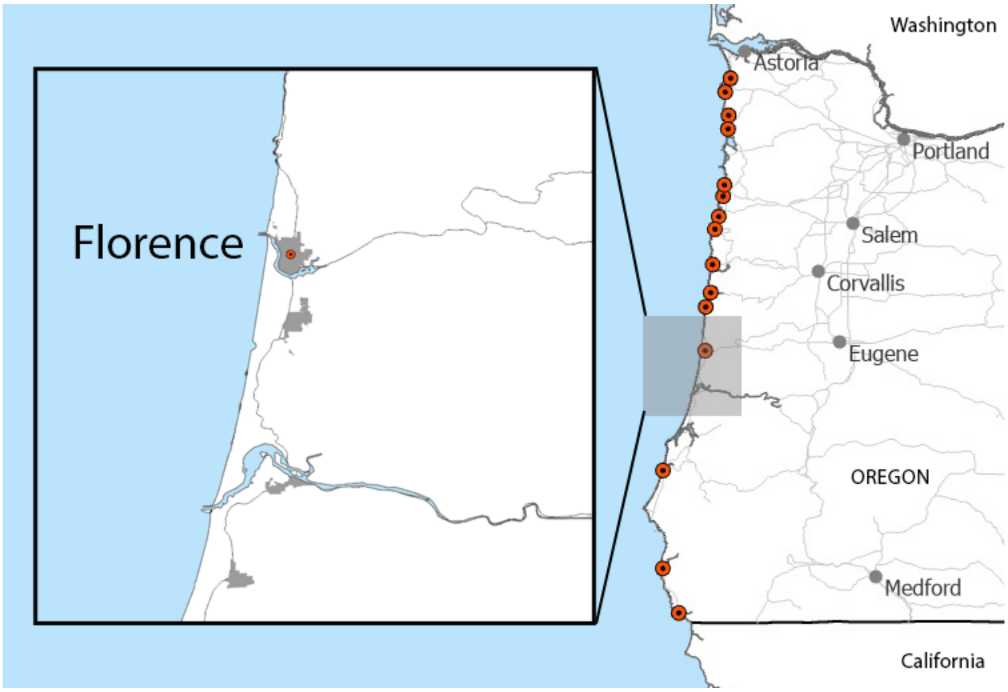
Q205 You stated that you are not sure how often you took **short overnight trips** to Yachats between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **short overnight trip** to visit a **developed beach** in **Yachats** for outdoor recreation **more or less than once a season (a 3 month period)**?

- More than once a season
- Less than once a season

Page Break

Q207 In each season during the previous 12 months, how many **short overnight trips** did you take to a **developed beach** in **Florence (Heceta Beach)**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any short overnight trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q209 You indicated you are not sure how many **short overnight trips** you took to Florence (Heceta Beach) in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q207/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q207/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q207/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q207/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total short overnight**

trips did you take to a **developed beach** in **Florence (Heceta Beach)**? For frequent visitors, numbers are provided based on frequency in parentheses.

- ◯ 1-3 trips (less than once a season)
- ◯ 4-10 trips (about one to two times a season)
- ◯ 12-24 trips (about one to two times a month)
- ◯ 52+ trips (at least once a week)
- ◯ Not sure

Page Break

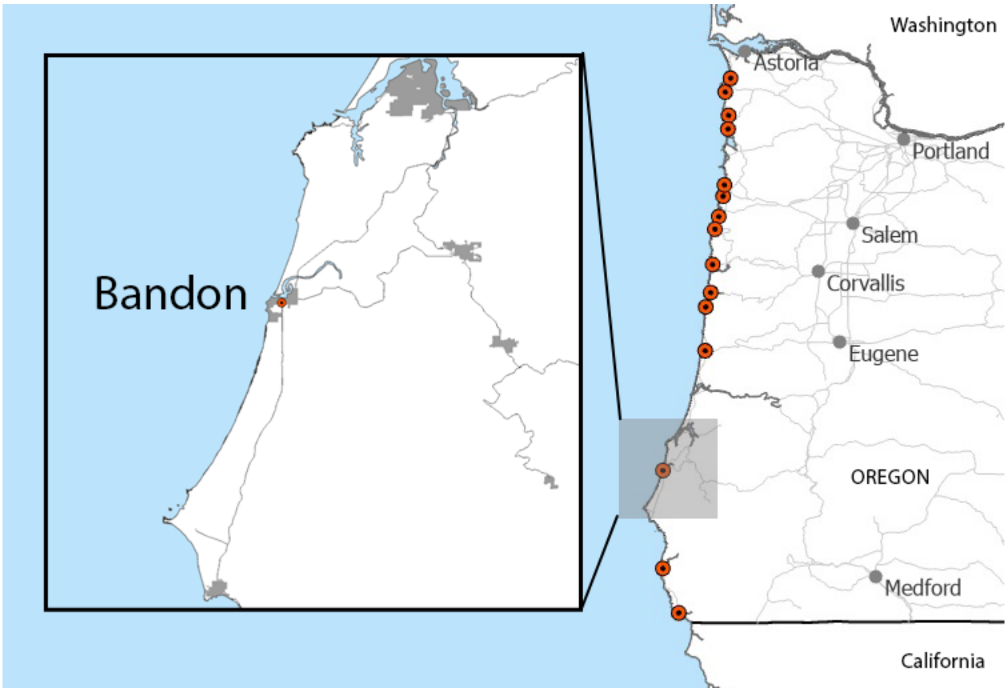
Q211 You stated that you are not sure how often you took **short overnight trips** to Florence (Heceta Beach) between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **short overnight trip** to visit a **developed beach** in **Florence (Heceta Beach)** for outdoor recreation **more or less than once a season (a 3 month period)**?

- ◯ More than once a season
- ◯ Less than once a season

Page Break

Q213 In each season during the previous 12 months, how many **short overnight trips** did you take to a **developed beach** in **Bandon**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any short overnight trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q215 You indicated you are not sure how many **short overnight trips** you took to Bandon in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q213/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q213/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q213/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q213/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total short overnight**

trips did you take to a **developed beach** in **<u>Bandon</u>**? For frequent visitors, numbers are provided based on frequency in parentheses.

- ◯ 1-3 trips (less than once a season)
- ◯ 4-10 trips (about one to two times a season)
- ◯ 12-24 trips (about one to two times a month)
- ◯ 52+ trips (at least once a week)
- ◯ Not sure

Page Break

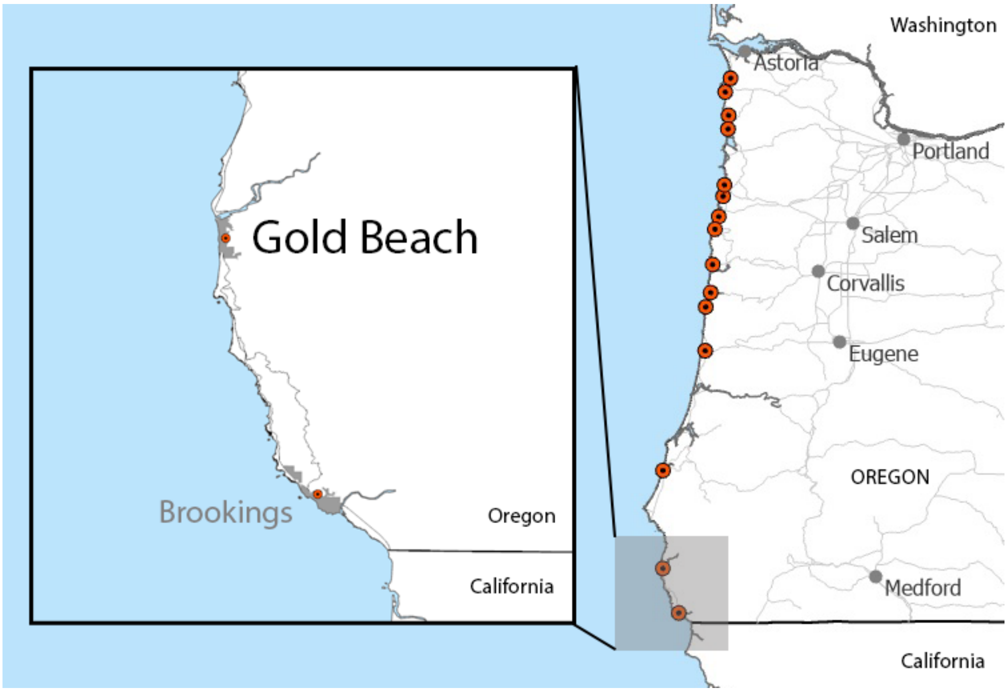
Q217 You stated that you are not sure how often you took **short overnight trips** to Bandon between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **short overnight trip** to visit a **developed beach** in **<u>Bandon</u>** for outdoor recreation **more or less than once a season (a 3 month period)**?

- ◯ More than once a season
- ◯ Less than once a season

Page Break

Q219 In each season during the previous 12 months, how many **short overnight trips** did you take to a **developed beach** in **<u>Gold Beach</u>**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any short overnight trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q221 You indicated you are not sure how many **short overnight trips** you took to Gold Beach in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q219/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q219/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q219/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q219/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total short overnight**

trips did you take to a **developed beach** in **Gold Beach**? For frequent visitors, numbers are provided based on frequency in parentheses.

- ◯ 1-3 trips (less than once a season)
- ◯ 4-10 trips (about one to two times a season)
- ◯ 12-24 trips (about one to two times a month)
- ◯ 52+ trips (at least once a week)
- ◯ Not sure

Page Break

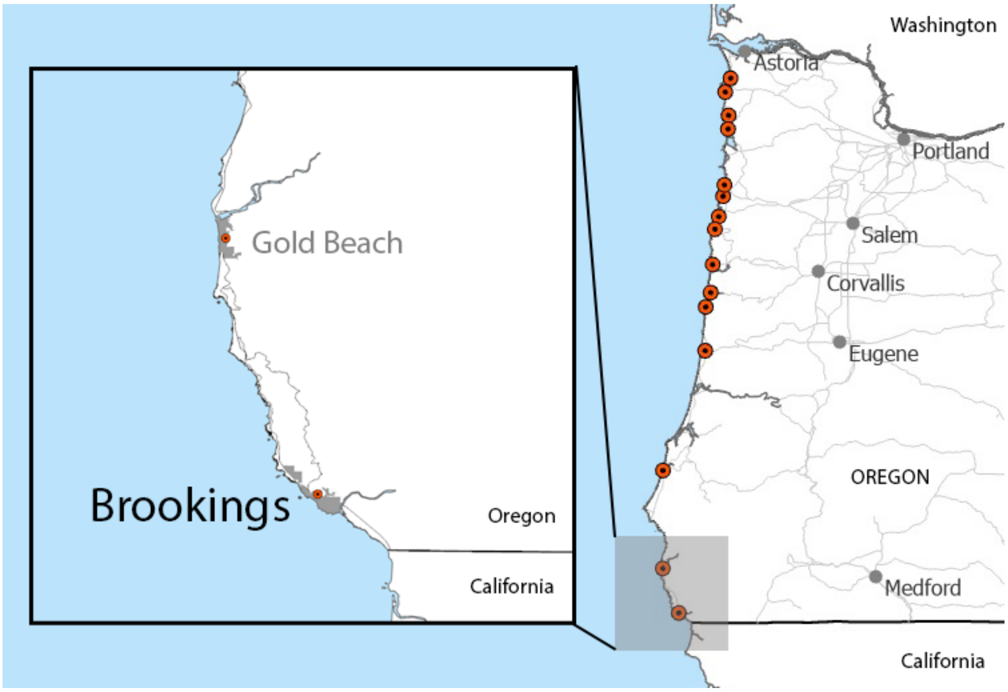
Q223 You stated that you are not sure how often you took **short overnight trips** to Gold Beach between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **short overnight trip** to visit a **developed beach** in **Gold Beach** for outdoor recreation **more or less than once a season (a 3 month period)**?

- ◯ More than once a season
- ◯ Less than once a season

Page Break

Q225 In each season during the previous 12 months, how many **short overnight trips** did you take to a **developed beach** in **Brookings**? Please answer to the best of your recollection. For frequent visitors, numbers are provided based on frequency in parentheses. Please select "0 trips" if you did not take any short overnight trips during that season.



Spring: April through June 2021	▼ 0 trips ... Not sure
Summer: July through September 2021	▼ 0 trips ... Not sure
Fall: October through December 2021	▼ 0 trips ... Not sure
Winter: January through March 2022	▼ 0 trips ... Not sure

Page Break

Q227 You indicated you are not sure how many **short overnight trips** you took to Brookings in at least one season. Below are the responses you gave:

Spring: Apr - Jun 2021: ${Q225/ChoiceGroup/SelectedAnswers/10}
Summer: Jul - Sep 2021: ${Q225/ChoiceGroup/SelectedAnswers/11}
Fall: Oct - Dec 2021: ${Q225/ChoiceGroup/SelectedAnswers/12}
Winter: Jan - Mar 2022: ${Q225/ChoiceGroup/SelectedAnswers/13}

To the best of your recollection, in the **previous 12 months**, about how many **total short overnight**

trips did you take to a **developed beach** in **<u>Brookings</u>**? For frequent visitors, numbers are provided based on frequency in parentheses.

- ◯ 1-3 trips (less than once a season)
- ◯ 4-10 trips (about one to two times a season)
- ◯ 12-24 trips (about one to two times a month)
- ◯ 52+ trips (at least once a week)
- ◯ Not sure

Page Break

Q229 You stated that you are not sure how often you took **short overnight trips** to Brookings between April 2021 and March 2022.

To the best of your recollection, during this 12-month period did you take a **short overnight trip** to visit a **developed beach** in **<u>Brookings</u>** for outdoor recreation **more or less than once a season (a 3 month period)**?

- ◯ More than once a season
- ◯ Less than once a season

Page Break

Q231 In the **previous 12 months (April 2021 to March 2022)**, about how many **short overnight trips** did you take to **developed** Oregon Coast beaches for outdoor recreation? Please answer to the best of your recollection.

- ○ 0 trips
- ○ 1 trip
- ○ 2-4 trips
- ○ 5-7 trips
- ○ 8-10 trips
- ○ 11+ trips
- ○ Not sure

Page Break

Q233 On June 30, 2021 Oregon lifted COVID-19 restrictions requiring indoor capacity limits and physical distancing.

Did you take **more, fewer, or the same amount of short overnight trips** to visit **developed** Oregon Coast beaches after the lifting of these COVID-19 restrictions?

- ○ More trips after the lifting of these restrictions
- ○ The same amount of trips after the lifting of these restrictions
- ○ Fewer trips after the lifting of these restrictions
- ○ Not sure

End of Block: Revealed preference questions - overnight trips

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Start of Block: Questions to get at the substitutability of developed beaches

Q235 Which of the following recreational activities did you engage in at the **developed** beaches you visited from **April 2021 to March 2022**? Select all that apply.

▢ Walking / Hiking

▢ Picnicking / Sunbathing / Kiting

▢ Exploring tide pools / Collecting rocks or shells

▢ Surfing / Swimming

▢ Recreational fishing and/or shellfishing (clamming, crabbing, etc.)

▢ Photography

▢ Wildlife viewing

▢ Frisbee, football, or other ball sports

▢ Horseback riding

▢ Camping

▢ Driving an off-highway vehicle or ATV

▢ Boating, canoeing, kayaking, or sailing

▢ Activities in town or a nearby town, e.g., going to restaurants, shops or art galleries

▢ Other activities on the developed beach, e.g., geocaching

▢ ⊗Not sure

Page Break

Q237 On a typical trip to the **developed** beaches you visit, do you also visit nearby undeveloped Oregon Coast beaches like state parks or federal lands?

- ◯ Yes
- ◯ No
- ◯ Not sure

Page Break

Q239
When you consider visiting a beach, what type of beach do you prefer to visit?

- ◯ Developed
- ◯ Undeveloped
- ◯ Both
- ◯ Other ____
- ◯ Not sure

End of Block: Questions to get at the substitutability of developed beaches

Start of Block: Background information about safety

Q241
The second and final part of the survey focuses on future management of safe access to **Oregon's developed beaches**. Recreation opportunities on **developed** Oregon Coast beaches may be impacted by **erosion** driven by winter storms, currents, winds, rain, runoff, and elevated water levels caused by rising sea levels.
Beach erosion is a process where waves, storms, and local sea level rise remove beach sand and wear away the dunes and bluffs of the Oregon Coast, often resulting in a **narrower** beach. In the United States, beach erosion results in approximately $500 million dollars per year in property damages and loss of land.

Developed beaches (shown below on the left) tend to be **more vulnerable to the effects of erosion** than undeveloped (or natural) beaches. This is because coastal development is fixed in place and erosion narrows the beach in front of that development. These beaches may require active management to

<u>preserve safe recreation access</u> in the future due to increasing erosion and sea level rise.

Undeveloped beaches (shown below on the right) are natural systems that can move and change with erosion due to a lack of development behind the beach and continue to preserve safe access for recreation.

Left: DEVELOPED BEACH: Nye Beach in Newport. Right: UNDEVELOPED BEACH: South Beach State Park in Newport.

For example, erosion may cause the shoreline of an undeveloped beach to move inland but the beach can preserve width (and access) because it isn't confined by development behind the beach. On a developed beach, however, the beach gets narrower as it erodes because structures behind the beach fix the shoreline in place. As the beach loses width, beach access will decrease and oceanfront properties will become more vulnerable to erosion.

Before today, were you aware that developed beaches tend to be more vulnerable to erosion compared to undeveloped beaches?

- ○ Yes
- ○ No
- ○ Not sure

Page Break

Q243
Safety is a key concern for people making recreation trips to the Oregon Coast. Three common safety hazards on the Oregon Coast are:

- **Sneaker waves**, which are waves that surge high up on the beach with deadly force, often appearing without warning.
- **Rip currents**, which are strong, narrow currents that can carry even the strongest swimmers away from shore.
- **King Tides**, which are extreme high tides that can reduce the amount of beach that is safely accessible and can also increase erosion to beaches and dunes.

For much of the West Coast of the U.S., sneaker waves result in more fatal accidents than all other weather hazards combined.

Waves of a winter storm near Depoe Bay

Before today, were you aware that sneaker waves are considered one of the deadliest natural hazards in Oregon?

○ Yes

○ No

○ Not sure

End of Block: Background information about safety

Start of Block: Define our accessibility/safety metric and impacts to it

Q245
These safety hazards impact the **number of daylight hours per day that people can safely access the beach and engage in recreation activities**, which we are defining here as **safe hours**. Safe hours can vary each day depending on the amount of daylight, the tides, the season (summer compared to winter), and the weather.

As an example, the images below show the **same beach** on a sunny winter day at high tide in Lincoln City. The photo on the left has a relatively wide beach with safe access for recreation. In the photo on the right, the ocean is covering the beach and it would be unsafe to walk or recreate on the beach. The photo on the right shows what this beach could look like in the future with the same tide, season, and weather conditions but with fewer safe hours.

LEFT: An example of an accessible safe hour in Lincoln City. RIGHT: An example of an unsafe hour on the same Lincoln City beach.

There is growing evidence that **erosion** and **rising sea levels** along the Oregon Coast have the potential to **decrease beach safe hours** now and in the future. Fewer (i.e., a loss of) safe hours on **developed** Oregon beaches will lead to a reduction in safely accessible beach areas and will increase the risks of safety hazards such as sneaker waves.

Before today, were you aware that the number of safe hours may decrease as erosion on developed beaches increases?

- ◯ Yes
- ◯ No
- ◯ Not sure

End of Block: Define our accessibility/safety metric and impacts to it

Start of Block: Background information about policy setting

Q247 In addition to impacts to safe access for recreation, erosion also poses an increased threat to houses, businesses, roads, and other infrastructure behind **developed beaches**.

Currently, Oregon land use policy allows one option for homeowners to protect infrastructure behind developed beaches from erosion, known as **shoreline armoring**. This is a type of engineered infrastructure that involves the construction of seawalls, riprap revetments (rock piles), and other hard structures by private individuals on their own property.

Riprap in Neskowin, OR

Oregon's **Statewide Planning Goal 18** **was originally implemented in 1977 to restrict armoring of private property** to conserve and protect Oregon's beaches and dunes in their natural state for all beach users. Goal 18 restricts armoring eligibility to land parcels where development existed prior to January 1st, 1977. All properties developed since that date are **not eligible** to install shoreline armoring, thus this option is not available to every homeowner. Given concerns about erosion and rising sea levels, there is currently debate across the state about relaxing, maintaining, or more strictly enforcing armoring rules.

About half (4,500) of Oregon's 9,000 oceanfront parcels are eligible for armoring and half are not eligible. A recent study by Oregon State University found about 1,000 eligible parcels have installed shoreline armoring to date. **If Goal 18 is maintained**, projections suggest another 300 eligible parcels will install shoreline armoring in the next 30 years. **If Goal 18 is relaxed**, that number would rise to 550 parcels, including many that are not currently eligible.

Before today, were you familiar with shoreline armoring on developed Oregon beaches?

○ Yes

○ No

○ Not sure

Page Break

Q249
Reasons Oregon residents **may support shoreline armoring for private property** include:

It may be effective at preventing land loss and damage to homes due to erosion. In addition to the erosion control benefits to property owners, it may also benefit beach visitors if they visit the places (for example, restaurants, hotels, shops) protected by the structures.

Reasons Oregon residents **may not support shoreline armoring for private property** include:

It may lead to narrower and steeper beaches compared to those that are not armored, which can interfere with public access and make beaches less desirable and less safe for recreation. It may have negative effects such as loss of beach sand and beach habitat for native plants, birds, and wildlife.

Do you believe it is likely that Goal 18's armoring policy will be maintained in its current form for the foreseeable future?

○ Very likely

○ Somewhat likely

○ Neither likely nor unlikely

○ Somewhat unlikely

○ Very unlikely

Page Break

Q251 A policy option used in other parts of the U.S. to control erosion is **sediment management**. This occurs when sand or other sediment is taken from another location and spread on a beach to increase beach width. **Beach nourishment** is the most used method along sandy coastlines in the U.S., especially along the East and Gulf Coasts.

To date, there have been **no** federal or state efforts to control erosion using sediment management on beaches in Oregon.

Beach nourishment project on Long Beach Island in New Jersey

Before today, were you familiar with beach nourishment as a sediment management option?

○ Yes

○ No

○ Not sure

End of Block: Background information about policy setting

Start of Block: Description of policy - Intro

Q253 Given the potential impacts to Oregon's **eroding developed beaches**, the state is considering **future policies to <u>prevent a loss of safe hours</u> for recreation while also <u>protecting property</u> from erosion**.

All beaches in Oregon (both developed and undeveloped) are **public** and managed by **Oregon State Parks** (under the Oregon Parks and Recreation Department). So, this agency may be tasked with implementing any new policy.

Oregon State Parks is currently **not funded** by tax dollars and relies on funding from the Oregon Lottery along with camping and parking fees, and RV registration fees. However, current revenue sources cannot cover the additional responsibilities created by a new policy.

Before today, did you know that Oregon State Parks manages Oregon's beaches?

- ○ Yes
- ○ No
- ○ Not sure

End of Block: Description of policy - Intro

Start of Block: Description of policy - Relax

Q255 Below are two options that the State of Oregon is considering.

<u>Option 1:</u>
The **first option** would **<u>increase</u> your household's annual state income taxes** by a small amount to implement **a new coastal management plan**. This plan would do two (2) things:

1. **Create an <u>Oregon Public Beach Fund</u>** to manage sediment on eroding **developed** beaches. This fund would be overseen by Oregon State Parks and used to address erosion and **preserve access and safe hours for recreation**.
2. **<u>Relax armoring restrictions</u>** under Statewide Planning Goal 18 to address erosion issues on oceanfront parcels. Relaxing Goal 18 would mean that all oceanfront homeowners would become eligible to install shoreline armoring when their property becomes vulnerable to erosion. This represents a significant change to Oregon's current land use policy and will **increase the amount of shoreline armoring** on developed beaches. Additional armoring structures would protect more private property but will also take up space for recreation and further reduce the width of these

beaches. Armoring would not be funded by the state income tax increase and will continue to be the financial responsibility of the coastal homeowners who decide to armor.

Q256

<u>**Option 2:**</u>

The **second option** is to **do nothing** and, instead, let people and nature deal with the effects of erosion and sea level rise. Under this option, there would be **no Oregon Public Beach Fund** and the **current armoring restrictions under Goal 18 remain unchanged**. There would also be <u>**no increase**</u> **to your household's annual state income taxes**.

Left: Recreation on an armored beach WITH erosion. Right: Recreation on an armored beach WITHOUT erosion.

How much do you agree or disagree with the following statement?
All properties that are vulnerable to erosion should be able to install shoreline armoring.

- ◯ Strongly agree
- ◯ Somewhat agree
- ◯ Neither agree nor disagree
- ◯ Somewhat disagree
- ◯ Strongly disagree
- ◯ Not sure

End of Block: Description of policy - Relax

Start of Block: Description of policy - Maintain

Q258 Below are two options that the State of Oregon is considering.

Option 1:
The **first option** would **increase your household's annual state income taxes** by a small amount to implement **a new coastal management plan**. This plan would do two (2) things:

1. **Create an Oregon Public Beach Fund** to manage sediment on eroding **developed** beaches. This fund would be overseen by Oregon State Parks and used to address erosion and **preserve access and safe hours for recreation**.
2. **Maintain current armoring restrictions** under Statewide Planning Goal 18. This will maintain Oregon's current land use policy and will not change the amount of properties that are eligible for armoring on developed beaches. Although debates across the state have suggested both relaxing or more strictly enforcing armoring rules, **armoring restrictions under Goal 18 will remain unchanged in this option**. Armoring would not be funded by the state income tax increase and will continue to be the financial responsibility of the coastal homeowners who decide to armor.

Q259
Option 2:
The **second option** is to **do nothing** and, instead, let people and nature deal with the effects of erosion and sea level rise. Under this option, there would be **no Oregon Public Beach Fund** and the **current**

armoring restrictions under Goal 18 remain unchanged. There would also be **<u>no increase</u> to your household's annual state income taxes**.

Left: Recreation on an armored beach WITH erosion. Right: Recreation on an armored beach WITHOUT erosion.

How much do you agree or disagree with the following statement?
All properties that are vulnerable to erosion should be able to install shoreline armoring.

- ○ Strongly agree
- ○ Somewhat agree
- ○ Neither agree nor disagree
- ○ Somewhat disagree
- ○ Strongly disagree
- ○ Not sure

End of Block: Description of policy - Maintain

Start of Block: Description of policy part 2

Q261 **Your opinion matters.** You will be asked to **vote on a new proposed coastal management plan**. Your vote **may help inform the State of Oregon** about what plan to put on a state ballot measure in an upcoming election. **The plan would be implemented if chosen by a majority of Oregon voters.**

Research suggests that people sometimes respond to questions like these one way, but then act differently. For example, people may vote "yes" on an ambitious project that involves higher costs than what they are actually willing to pay. We are interested in your opinion to best inform state policy - **there is no right or wrong answer**. Your answer will be **kept confidential**.

When making this decision, please remember your household budget and other items you may want to spend money on. Remember that **if you spend money on this coastal management plan, you will have less money for other things.**

End of Block: Description of policy part 2

Start of Block: Contingent valuation section - Relax

Q263
Consider that the **Oregon Public Beach Fund** and **the Goal 18 policy change to relax shoreline armoring restrictions** described previously are part of a state ballot measure. This measure would increase your household's annual state income taxes **per year for the next 30 years** to allow Oregon State Parks to implement the coastal management plan so as to meet both goals of preserving safe access for recreation and protecting private property from erosion.

This ballot initiative would: Increase funding for sediment management to **prevent a ${e://Field/safe-hours-reduction} % loss of safe hours** for recreation at developed beaches at the highest risk of erosion. **Relax** Oregon's Goal 18 shoreline armoring policy so that all oceanfront property owners become eligible to armor the shoreline in front of their homes.

The ballot initiative would **(1) preserve access to these beaches and their safe hours for the next 30 years** and **(2) relax the shoreline armoring policy**.

If this ballot measure passes, it would cost every household in Oregon an additional **$${e://Field/bid} in state income taxes every year for the next 30 years**.

If this measure is on the ballot in the next election, would you vote for (yes) or against (no) the ballot measure?

○ I would vote "yes"

○ I would vote "no"

Page Break

Q265 How certain are you of your vote?

- Extremely certain
- Very certain
- Somewhat certain
- Slightly certain
- Not at all certain

End of Block: Contingent valuation section - Relax

Start of Block: Contingent valuation section - Maintain

Q267 Consider that the **Oregon Public Beach Fund** and **the continuation of the state's current Goal 18 shoreline armoring policy** described previously are part of a state ballot measure. This measure would increase your household's annual state income taxes **per year for the next 30 years** to allow Oregon State Parks to implement the coastal management plan so as to meet both goals of preserving safe access for recreation and protecting private property from erosion.

This ballot initiative would: Increase funding for sediment management to **prevent a ${e://Field/safe-hours-reduction} % loss of safe hours** for recreation at developed beaches at the highest risk of erosion. **Maintain** Oregon's Goal 18 shoreline armoring policy that limits private shoreline armoring to those currently eligible.

The ballot initiative would **(1) preserve access to these beaches and their safe hours for the next 30 years** and **(2) maintain the current shoreline armoring policy**.

If this ballot measure passes, it would cost every household in Oregon an additional **$${e://Field/bid} in state income taxes every year for the next 30 years**.

If this measure is on the ballot in the next election, would you vote for (yes) or against (no) the ballot measure?

- I would vote "yes"
- I would vote "no"

Page Break

Q269 How certain are you of your vote?

- Extremely certain
- Very certain
- Somewhat certain
- Slightly certain
- Not at all certain

End of Block: Contingent valuation section - Maintain

Start of Block: Follow-up question

Q271 If the state ballot measure would increase your household's annual state income taxes by **$${e://Field/follow-up-bid} per year for the next 30 years** to implement the previously described coastal management plan, would you vote for (yes) or against (no) the ballot measure?

- I would vote "yes"
- I would vote "no"

Page Break

Q273 How certain are you of your vote?

- Extremely certain
- Very certain
- Somewhat certain
- Slightly certain
- Not at all certain

End of Block: Follow-up question

Start of Block: Question ranking policy outcomes - Relax

Q275 We would like to understand how the outcomes of the ballot initiative influenced your vote. Please select how important each factor was in influencing your vote.

	Very important	Moderately important	Neutral	Slightly important	Not important at all	Not sure
Increasing funding for sediment management to prevent a ${e://Field/safe-hours-reduction} % loss of safe hours for recreation at developed beaches at the highest risk of erosion.	○	○	○	○	○	○
Relaxing Oregon's Goal 18 shoreline armoring policy so that all oceanfront property owners become eligible to armor the shoreline in front of their homes.	○	○	○	○	○	○
The increase to your household's annual state income taxes per year for the next 30 years.	○	○	○	○	○	○

End of Block: Question ranking policy outcomes - Relax

Start of Block: Question ranking policy outcomes - Maintain

Q277 We would like to understand how the outcomes of the ballot initiative influenced your vote. Please select how important each factor was in influencing your vote.

	Very important	Moderately important	Neutral	Slightly important	Not important at all	Not sure
Increasing funding for sediment management to prevent a ${e://Field/safe-hours-reduction} % loss of safe hours for recreation at developed beaches at the highest risk of erosion.	○	○	○	○	○	○
Maintaining Oregon's Goal 18 shoreline armoring policy and limiting private shoreline armoring to those currently eligible.	○	○	○	○	○	○
The increase to your household's annual state income taxes per year for the next 30 years.	○	○	○	○	○	○

End of Block: Question ranking policy outcomes - Maintain

Start of Block: More questions to get at substitutability with undeveloped beaches

Q279 If this coastal management plan were to be implemented, would you still visit the developed beaches you visited between April 2021 and March 2022?

◯ Yes

◯ No

◯ Not sure

Q280 You indicated that you would **not** continue visiting these developed beaches if the coastal management plan was implemented. Would you continue to visit Oregon beaches but choose a nearby undeveloped beach instead?

◯ Yes

◯ No

◯ Not sure

End of Block: More questions to get at substitutability with undeveloped beaches

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Start of Block: De-briefing "no" question - "no" to follow-up CV question

Q281 **You voted "no" on the *second* ballot measure. We would like to understand what factors may have influenced that choice.**

End of Block: De-briefing "no" question - "no" to follow-up CV question

---

Start of Block: De-briefing "no" question - "no" to initial CV question

Q282 **You initially voted "no" on the *first* ballot measure. We would like to understand what factors may have influenced that choice.**

End of Block: De-briefing "no" question - "no" to initial CV question

---

Start of Block: De-briefing "no" question - "no" to all CV questions

Q283 **You voted "no" on the ballot measure. We would like to understand what factors may have influenced that choice.**

End of Block: De-briefing "no" question - "no" to all CV questions

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Start of Block: De-briefing "no" question part 2 – Relax

Q285 How much do you agree or disagree with the following statements:

	Strongly agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Strongly disagree	Not sure
I can't afford to pay for the proposed coastal management plan.	○	○	○	○	○	○
Protection of Oregon Coast beaches is not important to me.	○	○	○	○	○	○
I do not think a coastal management plan is necessary.	○	○	○	○	○	○
I think the proposed management plan is too risky.	○	○	○	○	○	○
I think the proposed management plan is too costly.	○	○	○	○	○	○
I object to paying more in state income taxes.	○	○	○	○	○	○
I do not think the Goal 18 policy should be relaxed.	○	○	○	○	○	○
I do not believe that a policy to preserve safe hours would be paired with relaxing Goal 18 in a state ballot measure.	○	○	○	○	○	○

I shouldn't have to pay for a program that benefits private oceanfront homeowners by allowing them to install armoring on the public beach.	○	○	○	○	○	○
Other (Please select "Not sure" if you have no comment)	○	○	○	○	○	○

End of Block: De-briefing "no" question part 2 - Relax

Start of Block: De-briefing "no" question part 2 - Maintain

Q287 How much do you agree or disagree with the following statements:

	Strongly agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Strongly disagree	Not sure
I can't afford to pay for the proposed coastal management plan.	○	○	○	○	○	○
Protection of Oregon Coast beaches is not important to me.	○	○	○	○	○	○
I do not think a coastal management plan is necessary.	○	○	○	○	○	○
I think the proposed management plan is too risky.	○	○	○	○	○	○
I think the proposed management	○	○	○	○	○	○

plan is too costly.						
I object to paying more in state income taxes.	○	○	○	○	○	○
I do not think the Goal 18 policy should stay the same.	○	○	○	○	○	○
I do not believe that a policy to preserve safe hours would be paired with keeping Goal 18 the same in a state ballot measure.	○	○	○	○	○	○
I shouldn't have to pay for a program that benefits private oceanfront homeowners.	○	○	○	○	○	○
Other (Please select "Not sure" if you have no comment)	○	○	○	○	○	○

End of Block: De-briefing "no" question part 2 - Maintain

Start of Block: Remaining de-briefing questions

Q289 How much do you agree or disagree with the following statement?

The results of this survey will influence Oregon state agencies and policymakers as they make their decisions about future coastal management plans for developed beaches.

- Strongly agree
- Somewhat agree
- Neither agree nor disagree
- Somewhat disagree
- Strongly disagree
- Not sure

Page Break

Q291 We would like to understand what factors may or may not have influenced your vote on the ballot measure. Below is a list of statements people have made in similar surveys about why they responded as they did.

How much do you agree or disagree with each of the following statements:

	Strongly agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Strongly disagree	Not sure
I had enough information to make an informed vote.	○	○	○	○	○	○
I do not trust the Oregon State government to protect Oregon beaches.	○	○	○	○	○	○
I do not believe it is the state government's responsibility	○	○	○	○	○	○

to fund a coastal management plan.						
If applied, I do not think the proposed coastal management plan will succeed at preserving safe hours for recreation on Oregon's developed beaches.	○	○	○	○	○	○
Please select Strongly Disagree here. Thank you for reading carefully.	○	○	○	○	○	○
I considered whether I can afford to pay for the proposed management plan.	○	○	○	○	○	○

Page Break

Q293 How much do you agree or disagree with each of the following statements:

	Strongly agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Strongly disagree	Not sure
I enjoy contributing to a good cause no matter what it is.	○	○	○	○	○	○
It is my right to have well-managed Oregon Coast beaches and I	○	○	○	○	○	○

should not have to pay extra for it.						
Beach erosion is a serious problem.	○	○	○	○	○	○
Sea levels are rising on most of the Oregon Coast.	○	○	○	○	○	○
The global climate is changing.	○	○	○	○	○	○
I am more sensitive to costs today than I was a couple of years ago before the economic changes brought on by the COVID-19 pandemic.	○	○	○	○	○	○

End of Block: Remaining de-briefing questions

Start of Block: Risk preferences and SLR questions

Q295 Do you think the risk of erosion to developed beaches 30 years from now will be:

○ Much greater

○ Somewhat greater

○ Equal

○ Somewhat smaller

○ Much smaller

○ Not sure

Page Break

Q297 Why do you think erosion risk will change this way? Select all that apply.

- ▢ Coastal land use change
- ▢ Sea level rise
- ▢ Climate change
- ▢ Stronger El Niño events
- ▢ Stronger winter storms
- ▢ Other ____
- ▢ ⊗Not sure

Page Break

Q299 Do you believe that Oregon’s climate is changing?

- ○ Yes
- ○ No
- ○ Not sure

Page Break

Q301 Do you believe that climate change will increase the erosion risk to developed beaches?

- Yes
- Maybe
- No
- Not sure

Page Break

Q303 How do you see yourself: Are you generally a person who is fully prepared to take risks or are you unwilling to take risks? Please check a box on the scale, where the value 1 means: “always unwilling to take risks” and the value 5 means: “always prepared to take risks”.

- 1 (Always unwilling to take risks)
- 2 (Somewhat unwilling to take risks)
- 3 (Neutral)
- 4 (Somewhat prepared to take risks)
- 5 (Always prepared to take risks)
- Not sure

End of Block: Risk preferences and SLR questions

Start of Block: Demographic questions

Q305 **IMPORTANT: <u>Your individual responses to all questions in this survey will be kept confidential.</u> Any material linking you to your survey responses will not be released and will be destroyed at the end of the study.**

Q306 To help make our study as accurate as possible, we want to account for whether you have lived in the same 5-digit Postal/ZIP code during the time period we asked questions about, or whether you moved.

Did you move sometime between April 2021 and March 2022?

◯ No

◯ Yes, to the same ZIP code

◯ Yes, to a new ZIP code

◯ Prefer not to say

Page Break

Q308 Please select the month and year you moved to your current residence.

▼ April 2021 ... March 2022

Q309 To help make our study as accurate as possible, please provide us with the 5-digit Postal/ZIP code of your **previous** residence:

____

Page Break

Q311
Is your primary residence a coastal property (within 1 mile of the coast)?

◯ Yes

◯ No

◯ Prefer not to say

Q312 Do you own a second home on the Oregon Coast (within 1 mile of the coast)?

- Yes
- No
- Prefer not to say

Page Break

Q314 To help make our study as accurate as possible, please provide us with the 5-digit Postal/ZIP code of your second home:

____

Q315 Is your coastal property subject to erosion risk?

- Yes
- No
- Not sure

Page Break

Q317 Which of the following best describes your race or origin?

- ○ American Indian or Alaska Native
- ○ Asian
- ○ Black or African American
- ○ Native Hawaiian or other Pacific Islander
- ○ White
- ○ From multiple races
- ○ Prefer not to say

Q318 Are you of Hispanic, Latino, or Spanish origin?

- ○ Yes
- ○ No
- ○ Prefer not to say

Page Break

Q320 What was your household's total annual income (before taxes) in 2021?

- Less than $20,000
- $20,000 to $24,999
- $25, 000 to $29,999
- $30, 000 to $49,999
- $50,000 to $74,999
- $75,000 to $99,999
- $100,000 to $124,999
- $125,000 to $149,999
- $150,000 to $174,999
- $175,000 to $199,999
- $200,000 or more
- Prefer not to say

Q321 What is the highest level of education you have completed?

- ○ Some High School or less
- ○ High School Diploma / GED
- ○ Some College
- ○ Associate's Degree / Trade School
- ○ Bachelor's Degree
- ○ Some Graduate School
- ○ Master's Degree
- ○ Doctorate Degree
- ○ Other
- ○ Prefer not to say

Page Break

Q323 Which of the following best describes your current marital/partnered status?

- ○ Single
- ○ Cohabiting/Living with a partner
- ○ Married
- ○ Widowed
- ○ Divorced/Separated
- ○ Prefer not to say

Q324 How many people, including yourself, currently live in your household?

- 1
- 2
- 3
- 4
- 5
- 6
- 7 or more
- Prefer not to say

Q325 How many children under 18 years old live in your household?

- None
- 1
- 2
- 3
- 4
- 5 or more
- Prefer not to say

Page Break

Q327 Do you typically visit developed Oregon Coast beaches with children?

- Yes
- Sometimes
- No
- Prefer not to say

Page Break

Q329 How old are the children that visit developed Oregon Coast beaches with you? Select all that apply.

- 0-3 years old
- 4-7 years old
- 8-11 years old
- 12 or older
- ⊗Prefer not to say

Page Break

Q331 Which of the following best describes your current employment situation?

- Self-employed or small business owner
- Employed, working full time
- Employed, working part time
- Not employed, looking for work
- Not employed, not looking for work
- Retired
- Disabled, not able to work
- Full-time student
- Full-time caregiver or parent
- Other
- Prefer not to say

Q332 In terms of politics, how would you describe yourself?

- ○ Extremely liberal
- ○ Moderately liberal
- ○ Slightly liberal
- ○ Neither liberal nor conservative
- ○ Slightly conservative
- ○ Moderately conservative
- ○ Extremely conservative
- ○ Prefer not to say

End of Block: Demographic questions

Start of Block: Social media questions

Q334 Have you ever engaged with a coastal advocacy group like the Surfrider Foundation or the Oregon Shores Conservation Coalition? Engagement can include activities like donating to, being a member of, or participating in an event put on by that organization.

- ○ Yes
- ○ No
- ○ Prefer not to say

Page Break

Q336 In the last 12 months (between April 2021 and March 2022), have you engaged in any of the following activities with a coastal advocacy group? Please select all that apply.

▢ Followed them on social media

▢ Received their newsletters

▢ Donated to them

▢ Attended an event put on by them

▢ Attended their meeting

▢ Became or remained a dues-paying member

▢ Volunteered at or helped organize an event put on by them

▢ Other ____

▢ ⊗Prefer not to say

Page Break

Q338 Do you use social media?

○ Yes

○ No

○ Prefer not to say

Page Break

Q340 Do you post to social media about your trips to the Oregon Coast? Select the option that **best** fits how frequently you post.

○ Yes, for all my trips

○ Yes, for more than half my trips

○ Yes, for less than half my trips

○ Yes, but only occasionally

○ Never

○ Prefer not to say

Page Break

Q342 Please indicate which social media platforms you use to post about your trips:

▢ Twitter

▢ Instagram

▢ Facebook

▢ Flickr

▢ Other ____

▢ ⊗ Prefer not to say

End of Block: Social media questions

Start of Block: Conclusion

Q344 Did we overlook anything that is important to you? Would you like to make a comment? Please use the space below.

____

____

____

____

____

End of Block: Conclusion