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

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Abstract: Estimating risk perceptions related to natural disasters is critical to understanding behavioral responses of individuals and adaptive capacity of communities. Developed coastlines experience hazard risk from sources with different frequency and intensity, such as flooding, erosion, and sea-level rise. In the Pacific Northwest, there is an additional high severity but very low frequency risk: the Cascadia Subduction Zone earthquake and tsunami. This paper investigates the impact of tsunami risk information on coastal residents' risk perceptions, as capitalized into property prices, using difference-in-differences and triple differences hedonic frameworks. I study the coastal Oregon housing market response to three sets of risk signals: two exogenous events - the March 11, 2011 Tohoku earthquake and tsunami and the July 20, 2015 New Yorker article "The Really Big One"; a hazard planning change – the 2013 release of new official tsunami evacuation maps; and visual cues of tsunami risk – blue lines indicating the spatial extent of the hazard zone installed by Oregon's Tsunami Blue Line project. For the first analysis, results suggest that a property inside the primary tsunami inundation zone sells for 7-9% less than a property outside of the zone after the Tohoku event, with a return to baseline levels within 2.5 years. For the second analysis, I find evidence that the 2013 hazard planning change was capitalized into home values in only the most vulnerable new inundation zone. For the third analysis, results suggest houses near blue lines may be selling for 8.0% less compared to houses farther away. The potential risk discounts identified in these analyses suggest that tsunami risk signals can be salient to coastal residents.

Keywords: Natural hazard risk, tsunamis, hedonic models, difference-in-differences, triple differences

JEL codes: Q51, Q54

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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 percent 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 Cascadia earthquake and tsunami change people's risk perceptions and narrow the gap between subjective and objective risk? Here, I investigate whether a risk discount is 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 difference-in-differences (DID) and triple differences (DDD) frameworks. 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 test how these information shocks capitalize into home values in the three northernmost coastal counties in Oregon (Clatsop, Tillamook, and Lincoln). 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 7-9% less than a property outside of the zone after the Tohoku event. This result is robust to a number of 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.

I then use housing information from the entire Oregon coast to estimate the impact of the 2013 update of official tsunami inundation and evacuation maps based on a new series of modeled inundation maps for five CSZ scenarios (S, M, L, XL, XXL) (DOGAMI, n.d.-a). The largest of this series – the XXL scenario – 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 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 the most vulnerable inundation zone sell for 17-31% less after the map update. This risk discount does not have a statistically significant decay effect.

Lastly, the Tsunami Blue Line project has installed thermoplastic blue line signs on the 2013 XXL tsunami inundation line on roads 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 tsunami 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 suggest there is an 8.0% risk discount for properties that are within 1000' of a blue line. The DDD results for this model are not statistically significant, suggesting homebuyers 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 & Jaravel, 2017; de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2018; 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 then I 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 be able to estimate most of Callaway's and Sant'Anna's (2020) group-time average treatment effects. Treatment effect heterogeneity could be a problem for this analysis, however, this dataset is composed of small, rural communities so I do not have the power to precisely estimate treatment effects that account for treatment effect heterogeneity.

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 also 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 or the earthquake risk alone (Beron et al., 1997; Brookshire et al., 1985; Gu et al., 2018; Nakanishi, 2017; 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).

My results have important risk communication and policy implications for the Pacific Northwest. Research shows that Oregon is chronically under-prepared for a Cascadia earthquake and tsunami (OSSPAC, 2013). Policymakers and emergency managers will 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. House Bill 3309, passed and signed in June 2019 with nearly unanimous bipartisan support in both the Oregon House and Senate, 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, according to these findings, 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. And given Oregon's current and chronic under-preparedness for a Cascadia event, additional policies – or risk signals – are needed to mitigate hazard risk.

This paper proceeds as follows. Section 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 3 describes the study areas and their policy and news backgrounds. Section 4 describes the data collected and some key data limitations. Section 5 defines my empirical approach and discusses identification strategies for all three analyses. Section 6 presents results for all three analyses. Section 7 concludes by providing a summary of my current findings, potential next steps to identify these risk signals, and implications for resilience planning and policy.

2 Hazards and housing markets: previous research

The property attribute of interest in this paper is subjective tsunami risk and I use hedonic frameworks 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.[2] 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

[2] 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 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 study periods that span potentially large changes in house and neighborhood attributes – such as the eight-year duration of the first analysis (2009-2017). 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.

al., 2007). Since Rosen (1974), many studies have used this method to estimate capitalization of risk factors in housing prices.

Previous literature has used hazard events or regulatory hazard delineation to identify the impact of risk on housing prices. In one of the first studies of its kind, 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 have examined the impacts of specific earthquake events (Beron et al., 1997; Gu et al., 2018; Naoi et al., 2009). Other hedonic 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) and hazardous waste sites (McCluskey & Rausser, 2001).

Recently, difference-in-differences (DID) approaches have been used to show that disaster events can increase house 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). 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. 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 amenity and disamenity effects on property prices (Bakkensen et al., 2019; Muehlenbachs et al., 2015; Qiu & Gopalakrishnan, 2018). In the hazard risk literature, the DDD approach has typically exploited an additional treatment (control) group that is more (less) sensitive to the treatment, i.e., the DDD estimator compares the DID estimator for observations considered to be more sensitive to the treatment to the DID estimator for observations that are 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; Hallstrom & Smith, 2005; Kask & Maani, 1992; Kousky, 2010; McCluskey & Rausser, 2001; McCoy & Walsh, 2018; Parton & Dundas, 2020; Qiu & Gopalakrishnan, 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 behavior depends on the gap between objective risk and the consumer's initial subjective risk, e.g., above-average objective risk and a lower initial subjective probability will lead to increasing subjective probability and hedonic price as information increases (Kask & Maani, 1992).

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 (Atreya & Ferreira, 2015; 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). Leveraging multiple storm events in North Carolina, Bin and Landry (2013) find risk premiums between 6.0% and 20.2% following 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 have an effect on 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 – wherein individuals' subjective probability of an event occurring depends on how recent or memorable that event was (Atreya et al., 2013; Bin & Landry, 2013; Gallagher, 2014; Kousky, 2010; McCoy & Walsh, 2018). 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. 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.

3 Study area and background

Oregon is a geologic mirror image of northern Japan, where the March 11, 2011 magnitude 9.0 Tohoku earthquake 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.

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 7-15% 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 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).

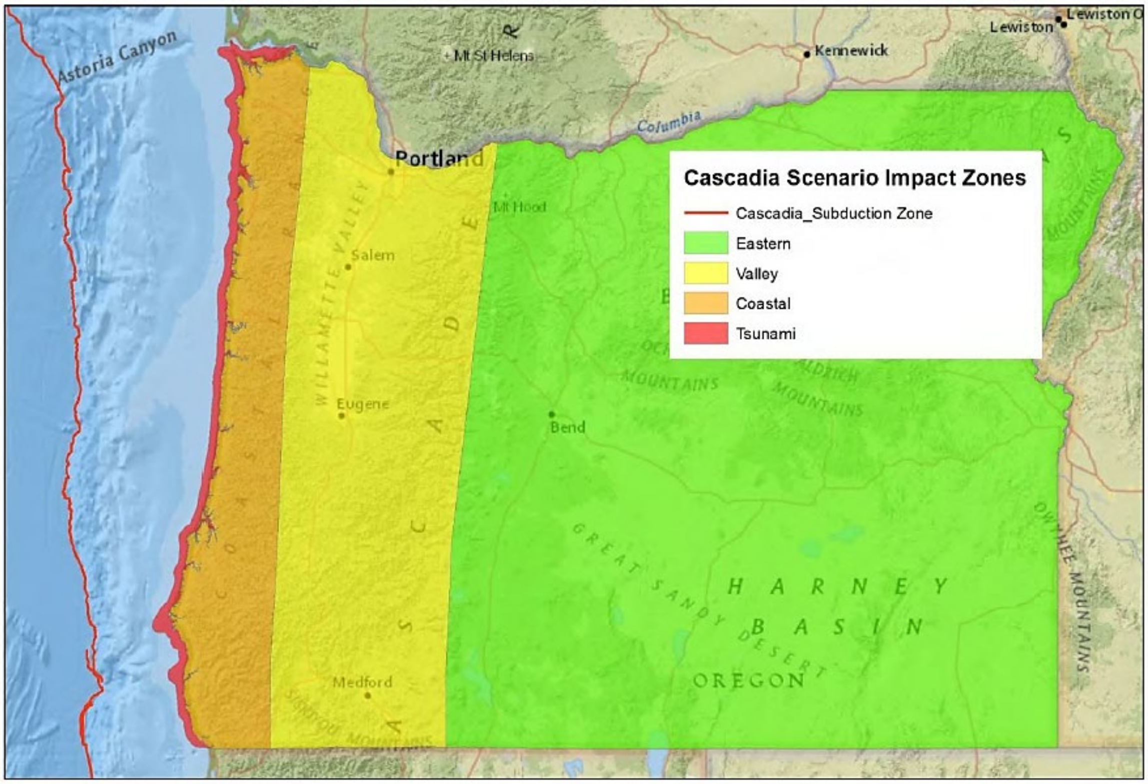

Figure 1. Impact zones for the magnitude 9.0 Cascadia earthquake scenario. Damage will be extreme in the Tsunami zone, heavy in the Coastal zone, moderate in the Valley zone, and light in the Eastern zone. From Figure 1.5 of the "Oregon Resilience Plan" (OSSPAC, 2013).

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 the 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). House Bill 3309 overturned the regulatory power of the SB 379 line in 2019. 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. 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

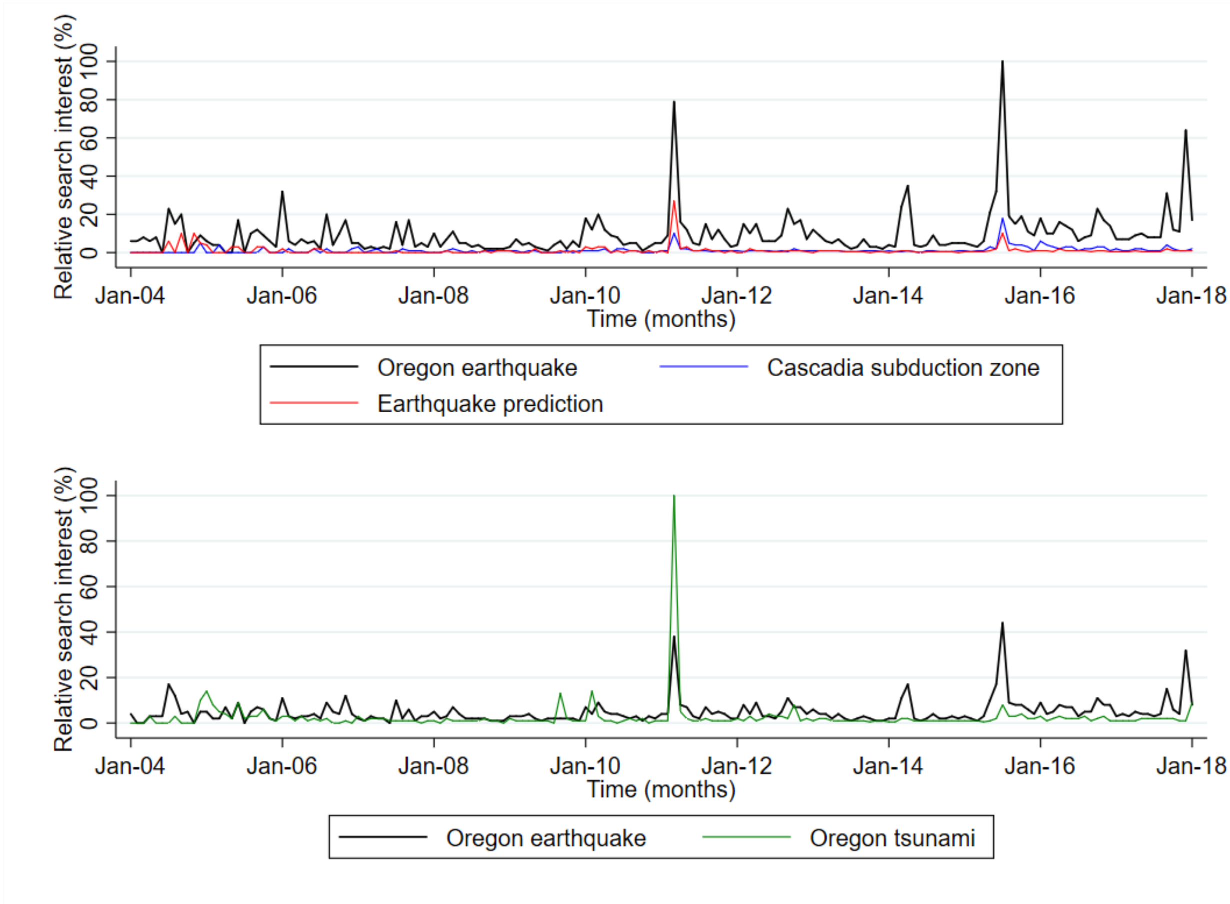

Figure 2. Google searches between 1/1/04 and 1/1/18 in Oregon as measured by search interest relative to the highest point on the chart for the given region and time range. (a) For terms "Oregon earthquake", "Cascadia subduction zone", and "Earthquake prediction". (b) For terms "Oregon earthquake" and "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.

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, 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, e.g., 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(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(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 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 3(a), though some only have the "Leaving Tsunami Zone" sign without an accompanying blue line, as seen in Figure 3(b). Thus, the

Figure 3. Tsunami blue line signage in (a) Newport, OR (courtesy of Mike Eastman) and (b) Seaside, OR (courtesy of Anne McBride).

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., if a road is intersected by another street they decided to place a marker directing evacuees toward safety. 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).[3] Some local governments (e.g., Tillamook County) also held community meetings to elicit feedback and input about tsunami wayfinding efforts.[4] 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).[5] 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 land in Oregon's tsunami inundation zone is classified as undeveloped, 48% of Clatsop County's tsunami zone, 34% of Lincoln County's tsunami zone, and 21% of Tillamook County's tsunami zone are 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

[3] 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).

[4] 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).

[5] So as 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).

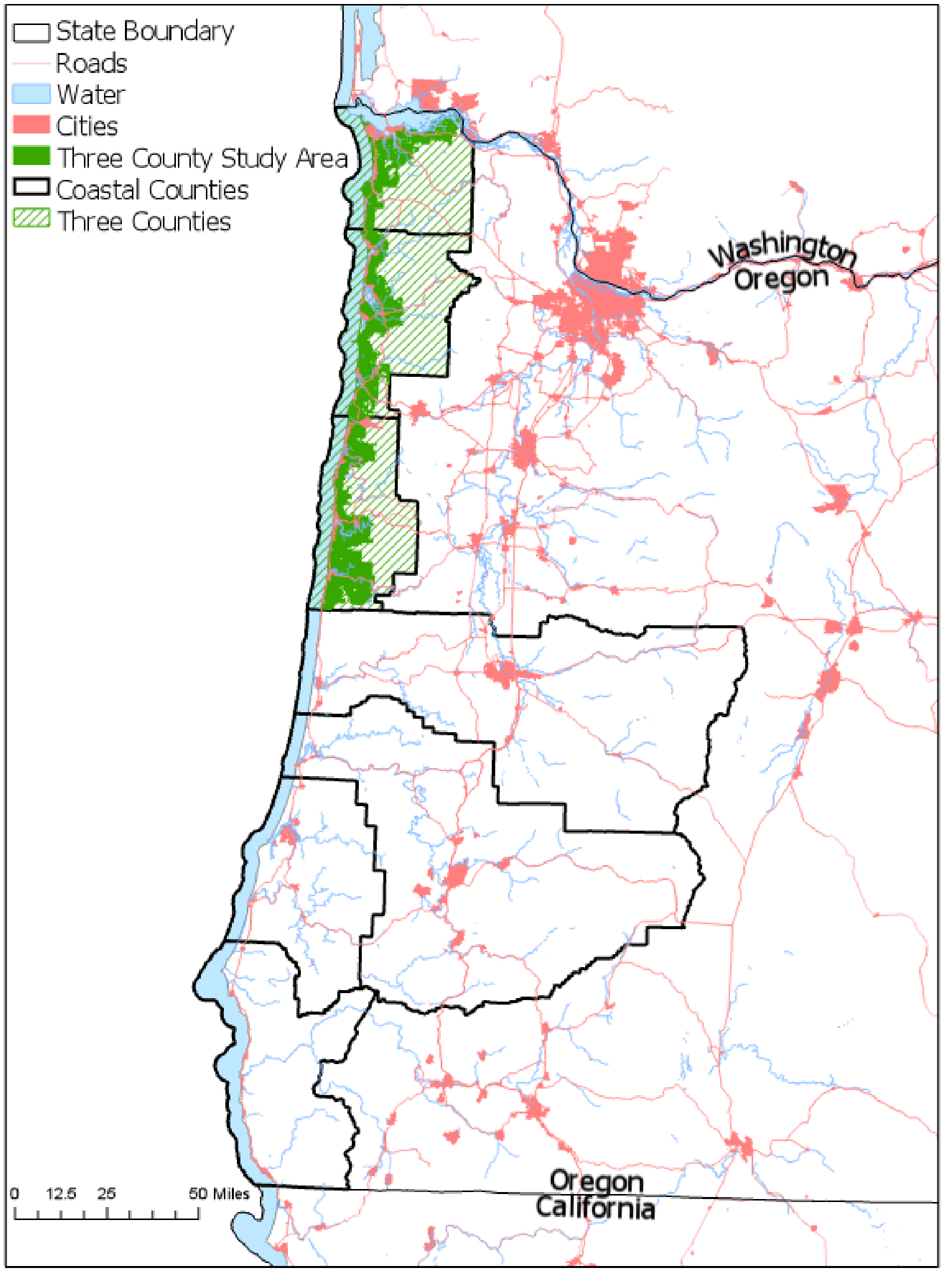

Figure 4. GIS data for three county study area (green hatching) and the seven coastal counties (black border). Coastal counties from north to south (unlabeled): Clatsop, Tillamook, Lincoln, Lane, Douglas, Coos, and Curry. The study area for the first analysis (solid green) is defined to be within 1 mile of the tsunami inundation zone given by the 1995 SB 379 line.

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 HWY 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. Figure 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 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.

4 Data

Multiple data sources including property sales data, tsunami inundation maps, Census block group data, and other GIS data are used for these analyses. Property sales data was aggregated from tax assessor records in Zillow's ZTRAX database and spans residential, agricultural, and commercial sales from 1995 through 2018 (Zillow, 2020). These data were cleaned to remove all non-residential transactions and transactions missing key structural variables (age, etc.). In each year, transactions with prices in the bottom one percent were removed because they may reflect non-arms-length transfers, e.g., intra-family transfers. Transactions in the top one percent in each year were also removed. 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. The Zillow ZTRAX data does not have reliable second home indicators, however, so identifying second home ownership is not possible at this time.[6] Following this cleaning, the remaining transactions contain only arms-length, single-family residential sales that reflect the valuations of potential homeowners. Some of the key structural covariates from the Zillow data include the effective 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 are collected from several state and federal sources. The majority 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). Coastal towns are small and likely have only one central business district. 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) primarily came from the USGS Protected Areas Database of the United States (PAD-US). Federal public lands were trimmed to 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 were trimmed to include only state forests, state parks, and wildlife management areas. Elevation data was collected in 10m-by-10m pixels from the Oregon Department of Geology and Mineral Industries (DOGAMI). GIS software was used to calculate the elevation of each property and the distance from each property to the nearest location amenity.[7] For oceanfront properties,

[6] 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 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.

[7] 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

additional data on shoreline armoring and armoring eligibility is included. Shoreline armoring is a private option to protect oceanfront properties from erosion and storm surges by installing hardened shoreline protection structures.[8] 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 primarily due to a lack of parcel-level earthquake and flood insurance data.[9] 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. 2010 Census information was collected at the Census block group level to be used for these neighborhood-level spatial fixed effects. Block groups generally contain between 600 and 3,000 people. 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. Mortgage lenders must also inform homebuyers if the property is located in an 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 (Table 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

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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.

[8] Oregon's Statewide Planning Goal 18 designates which parcels are eligible to install shoreline armoring (Department of Land Conservation & Development, n.d., 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.

[9] 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.

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.[10] 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.[11]

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 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. 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.[12] The change in tsunami inundation and evacuation maps between the two events of interest presents a model specification problem that is addressed in section 5.1. See Appendix A.2 for figure comparisons of the 2013 TIM and 1995 SB 379 tsunami inundation scenarios for the city of Tillamook.

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[10] 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.

[11] Table A1 in Appendix A.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.

[12] See Table A1 in Appendix A.3.

Table 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					
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

Table 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
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		

The last column of Table 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

Table 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

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 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 2011 to 2015 so that the 2013 evacuation map change is bracketed by two years of property sales data before and after the event.[13]

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 and 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.[14] Table A4 in Appendix A.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

[13] Table A2 in Appendix A.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 A.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.

[14] 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).

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 times. Some locations were given as being in the vicinity of street intersections or nearby landmarks so I approximate the location of the blue line based on the location of the 2013 XXL tsunami inundation line and this firsthand information. Timing information was provided as the month and year of installation. However, sometimes 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.

5 Methodology

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

In the first analysis, I use two exogenous information shocks to distinguish between the effect of coastal amenities and the increased subjective risk of tsunami inundation. I use a difference-in-differences (DID) 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 DOGAMI tsunami inundation maps changed in 2013 from the SB 379 line to the new TIM Plate 1 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:

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

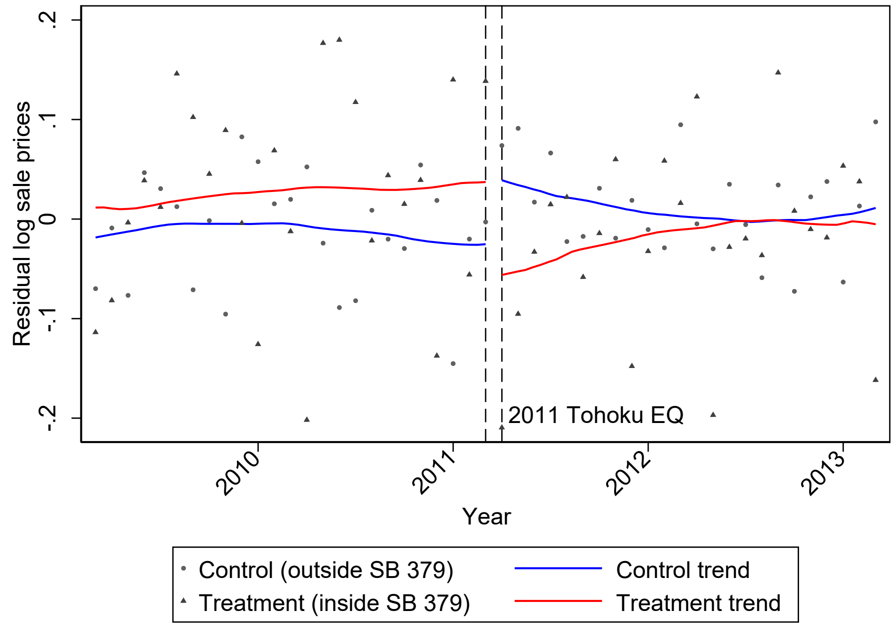
where $price_{ict}$ is the sale price (in constant 2019 dollars) of house i with structural and location characteristics $\boldsymbol{X}$ in Census block group c at time t. The log transformation of $price_{ict}$ was chosen as the dependent variable in all models because taking the log of $price$ narrows 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).[15] 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.[16]

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 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:

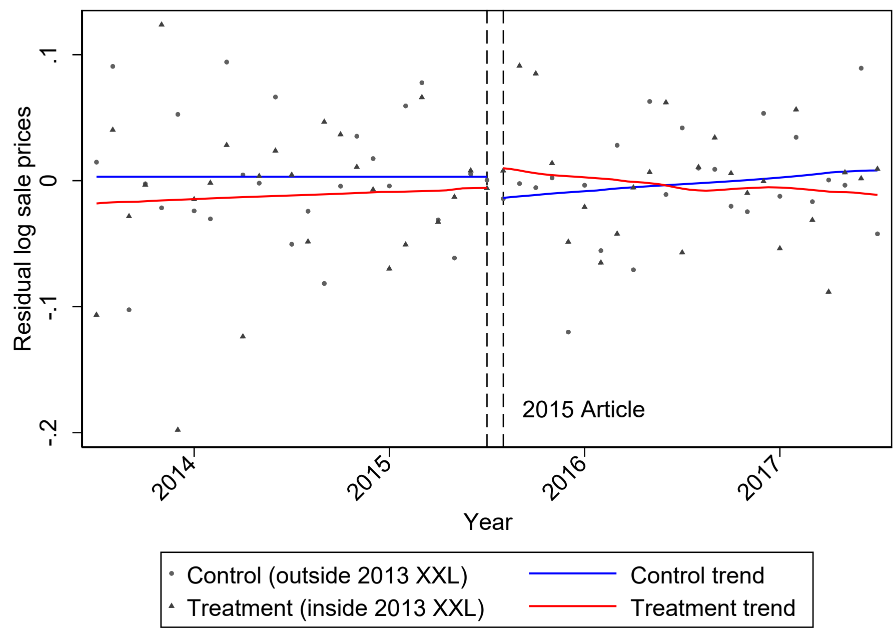
$$\begin{aligned} ln(price_{ict}) = \boldsymbol{X'_{it}}\beta_1 + \beta_2 xxl2013_i + \beta_3 article_t + \delta_1 xxl2013_i * article_t \\ + quarter_t + blckgrp_c * year_t + \varepsilon_{ict} \end{aligned} \tag{2}$$

[15] 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.

[16] 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.



(a)



(b)

Figure 5. Housing price trends inside and outside of the treatment inundation line – SB 379 or 2013 XXL – for the three counties. Plot of residual (log) sale prices net of structural attributes, location covariates, and fixed effects aggregated by month with local polynomial trend lines. (a) For Model I's time range. (b) For Model II's time range.

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 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 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.[17] Figure 5(b) plots the

[17] 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).

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, a counterintuitive result.

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:

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

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).

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 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.[18] 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.[19]

As another robustness check, I also 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

[18] NNM allows for exact matching the event variable.

[19] The other three matching methods can also generate weights to be used in the DID regressions.

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.

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. Thus, as an alternative to the DID specification, I specify event study designs for the models with only one event of interest. 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.

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 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:

$$\begin{aligned} ln(price_{ict}) = \boldsymbol{X'_{it}}\beta_1 + \beta_2 xxl2013_i + \beta_3 newmaps_t + \ \delta_1 tsu2013_i * newmaps_t \\ + quarter_t + blckgrp_c * year_t + \varepsilon_{ict} \ , \end{aligned} \quad (5)$$

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).[20] The time range for this

[20] 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).

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. 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.[21] 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 A.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 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).

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

[21] 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 A.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) 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.

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 6 shows a taxlot map with example treatment and control groups around a blue line (small red squares) in Manzanita, OR. The treatment group is given by those property sales (small gray 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 blue 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 6 shows that the control group (green circular buffer) encompasses a blue line in its lower left. 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.[22] 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 A.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.

[22] 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.

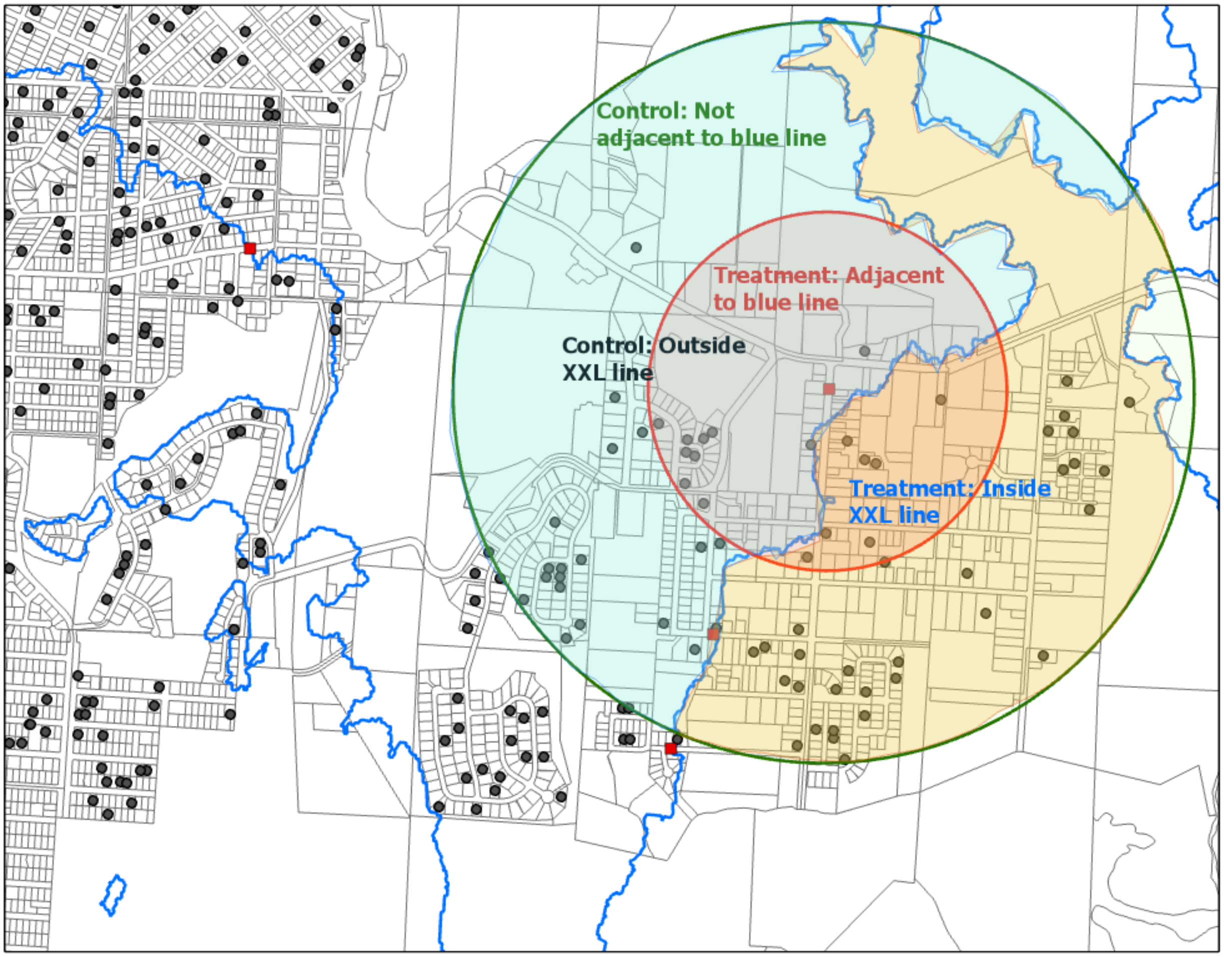


Figure 6. Taxlot map with example treatment and control groups around a blue line (small red squares) in Manzanita, OR. The treatment group (red circular buffer labeled "Treatment: Adjacent to blue line") and control group (green circular buffer outside of the red circular buffer labeled "Control: Not adjacent to blue line") represent whether property sales (small gray circles) are adjacent to the blue line or not, respectively. The 2013 XXL inundation and evacuation line (thick blue line) separates houses that are more sensitive to the treatment (yellow area labeled "Treatment: Inside XXL line") from those that are less sensitive to the treatment (green area labeled "Control: Outside XXL line").

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 3000' and the control buffer radius between 1000' and 8000'.[23] 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:

[23] 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.

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

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). Several recent studies have found that under treatment effect heterogeneity the TWFE estimator recovers a weighted average of some underlying treatment effect parameters (Borusyak & Jaravel, 2017; de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2018; Sun & Abraham, 2020).[24] 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, 2018; Sun &

[24] 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.

Table 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

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

Abraham, 2020). 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 group g at time t where a "group" is defined by the time when units are first treated. The group-time average treatment effects can be averaged into an aggregate measure: the "average effect 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. I estimate both of these new estimators (Callaway & Sant'Anna, 2020; de Chaisemartin & D'Haultfœuille, 2020).

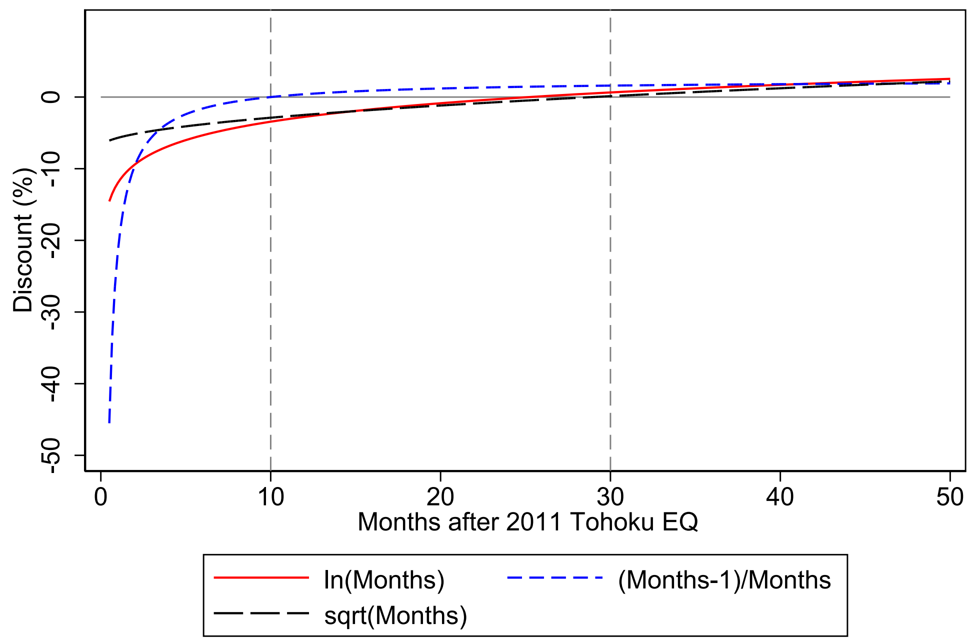


Figure 7. Decay effects of tsunami risk over time after the Tohoku earthquake and tsunami. Plot of coefficients from equation (4) as in Bin and Landry (2013).

6 Results

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

Table 3 reports selected estimation results of the key coefficients for Models I through III in the first analysis.[25] 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 8.9% following the Tohoku event. The coefficient estimate from Model III implies a slightly smaller risk discount of 6.8%. Taken together, these results imply that a property inside the tsunami inundation zone sells for 7% to 9% 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 8.6% 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 8.6% between the Tohoku

[25] Table A9 of Appendix A.7 reports the full estimation results with all coefficients.

Table 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

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 6.2 to 6.7% (at the 10% significance level). This 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.[26]

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).[27] Figure 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

[26] 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.

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

Table 5. Event study results for the first analysis, full data

	Model I Coefficient	SE	Model II 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

overall result for this analysis suggests that a property inside the SB 379 tsunami inundation zone sells for 7-9% 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 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 8.9% 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.

Table 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 14.0% 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

Table 6. Difference-in-differences selected results, matched data

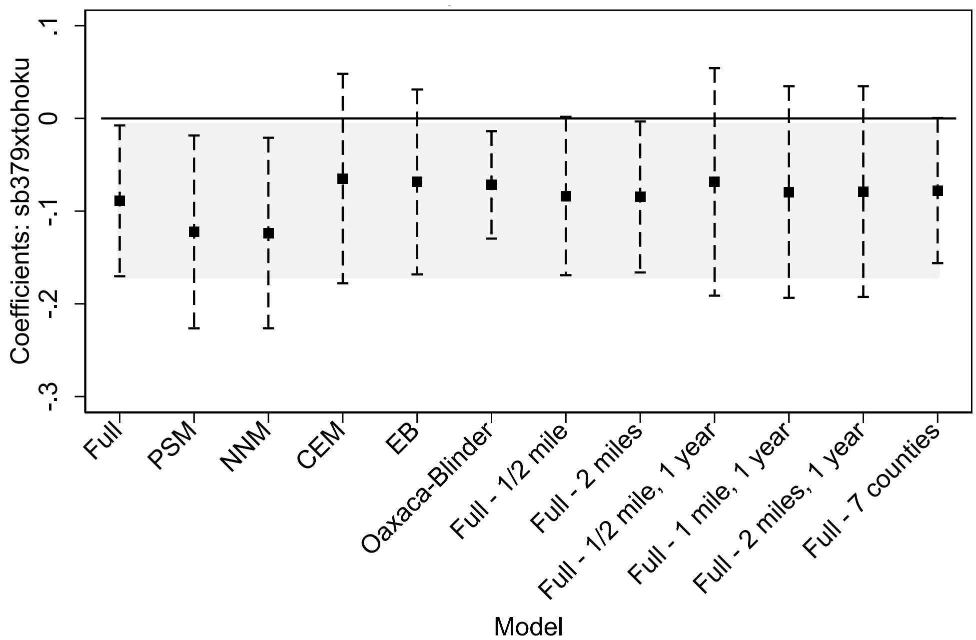
Matching method and Diff-in-Diff estimators	Model I Coefficient/SE	Model II Coefficient/SE	Model III Coefficient/SE
Nearest neighbor propensity score (PSM)			
SB 379 zone (sb379) x sold after 2011 Tohoku EQ (tohoku)	-.1224** (.0530)		-.1056** (.0426)
2013 XXL zone (xxl2013) x sold after 2015 article (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

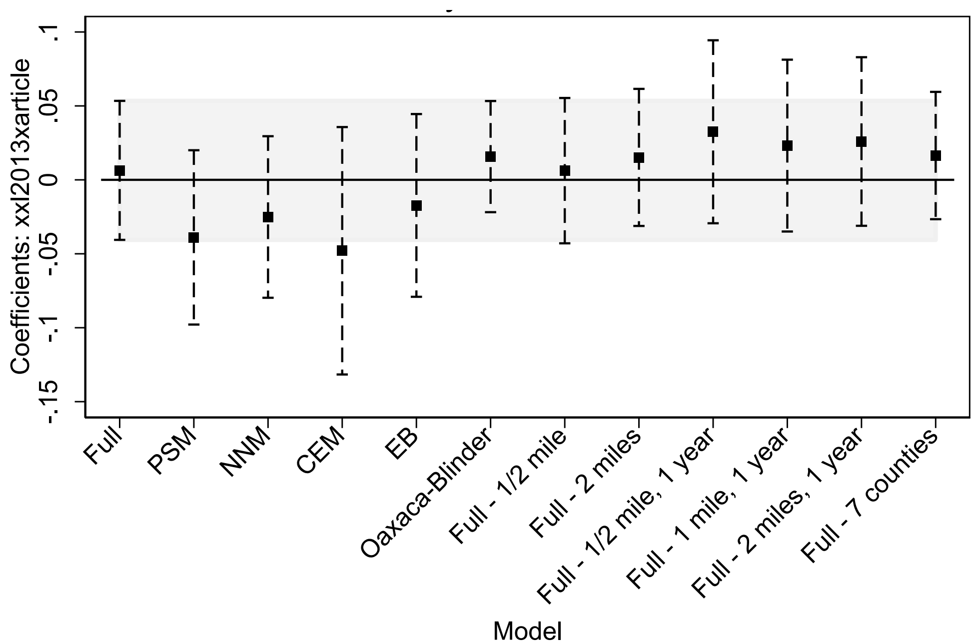
of interest occurs ($lag0$) is positive and two quarters later the second lag variable is negative. Thus, the event study results are inconclusive about the direction of the risk discount, which is complementary to the full data OLS results that suggest a null result for Model II.

Appendix A.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 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)



(a)



(b)

Figure 8. Average treatment effect on the treated estimates with 95% confidence intervals for the first analysis' models. 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.

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 10-12% following the Tohoku event. After NNM, the DID estimator for the Tohoku event is suggestive of a 12% 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 a 9% 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 A.7. In all four tests the DID estimates for Model I are smaller in magnitude compared to the main full data estimate of 9% 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 7% 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 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.[28] For each model, 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. 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 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

[28] See Figures A4(a) and A4(b) in Appendix A.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 8(a) and 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.

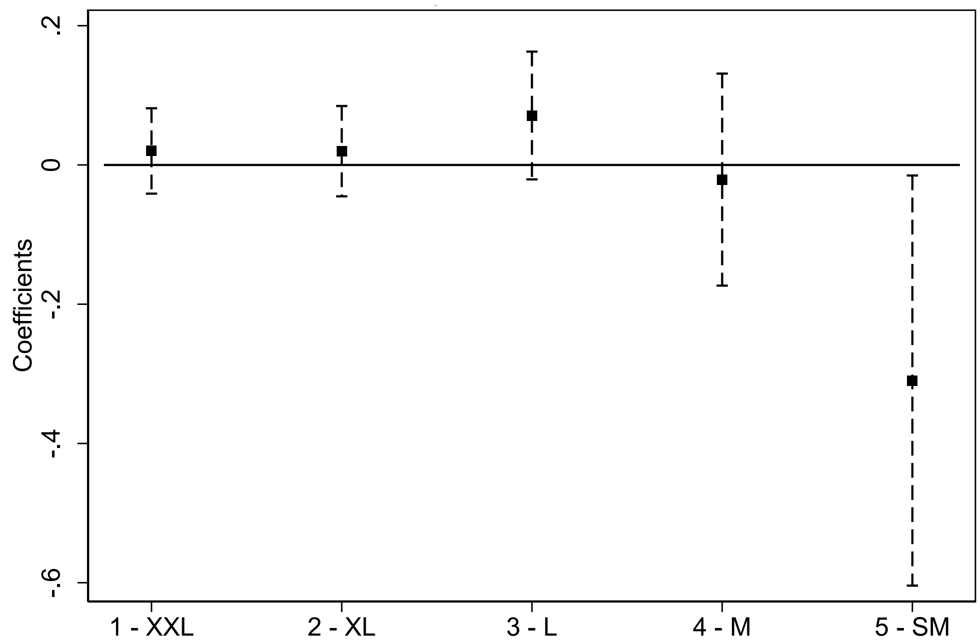


Figure 9. Average treatment effect on the treated estimates with 95% confidence intervals for Models 1 through 5 of the second analysis.

sign and approximately same magnitude as the coefficient from the full data results. Figure 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.

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 31.3% following the 2013 map change. These results are summarized in Figure 9, which plots the full data DID estimators with 95% confidence intervals for Models 1 through 5.[29] 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.

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.[30] This model implies

[29] Table A11 of Appendix A.7 reports the full estimation results with all coefficients.

[30] Table A12 of Appendix A.7 reports the combined model results.

that a property inside the 2013 SM inundation zone has a risk discount of 23.9% 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).[31] However, the Oaxaca-Blinder estimator is marginally significant for Model 5 and suggestive of a 17.2% risk discount for properties inside the 2013 SM tsunami inundation zone following the 2013 map change (p-value = 0.1047). 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.[32] The results of the four sets of falsification tests are presented in Table A14 of Appendix A.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 31.3% and are not statistically significant.[33] 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 17-31% less after the 2013 map change.

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 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. 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 (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

[31] Table A13 of Appendix A.7 reports the Oaxaca-Blinder results.

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

[33] 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.

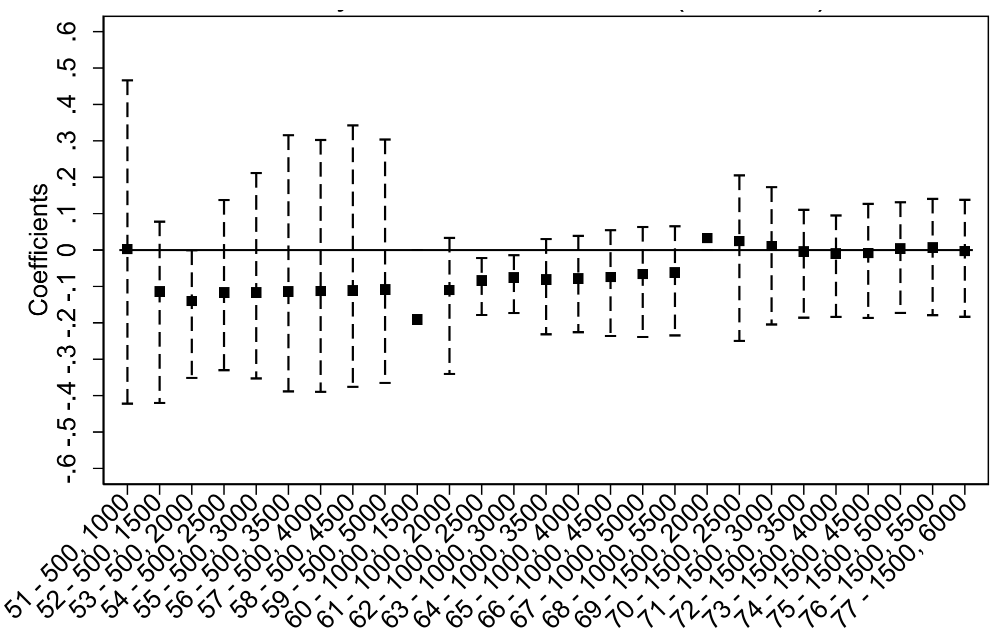
wild bootstrapping method has been shown to perform well when the number of clusters is small and when cluster sizes vary (MacKinnon & Webb, 2018).

Models 1 through 50 (Figures A5(a) and A5(b) in Appendix A.7) use Euclidian distances and Models 51 through 100 (Figures 10(a) and 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 10(a) and 10(b)).

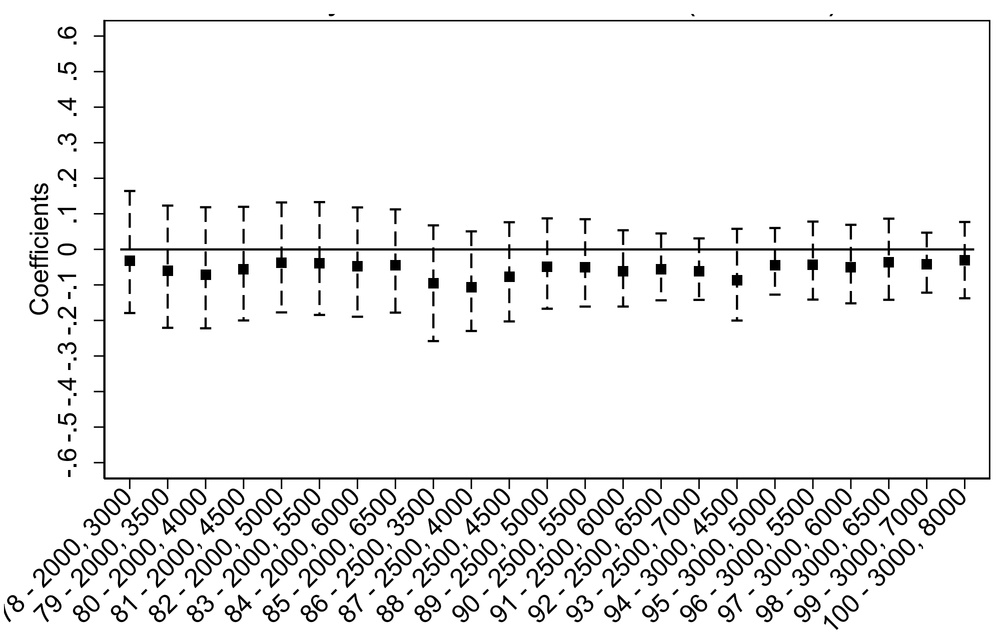
Figure 10(a) shows the estimates for the 500', 1000', and 1500' treatment buffers defined using road network distances. The first nine model estimates in this figure are for the 500' treatment buffer with the control buffer expanding from 1000' to 5000'. The next nine estimates are for the 1000' treatment 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 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 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.[34] 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

[34] I could apply multiple hypothesis testing procedures to a larger subset of models but, as expected, the adjusted p-values are very high.



(a)



(b)

Figure 10. Average treatment effect on the treated estimates with 95% confidence intervals for Models 51 through 100 of the third analysis. 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.

and 63 is 0.089.[35] 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.

[35] The full set of q-values is not reported here but is available upon request.

Table 7. Difference-in-differences and triple differences results for the third analysis, Model 62

	DID		DDD	
	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
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

Following these tests, I choose one model to continue the analysis with: Model 62.[36] It has a 1000' treatment buffer and a 2500' control buffer. Table 7 reports selected DID and DDD estimation results of the key coefficients for Model 62.[37] 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 level, uncorrected). 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).

[36] Once this model is selected, subsequent p-values are generated using the subcluster wild bootstrapping procedure and are not corrected for multiple testing procedures.

[37] Table A15 of Appendix A.7 reports the full estimation results with all coefficients.

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.

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 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) 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 for only two groups and, while negative, these group average treatment effects are not statistically significant. 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 TWFE but the magnitudes and significance are likely impacted by the small sample in this rural location.

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 7-9% 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.

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 31.3% 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 17.2%. 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.

DID results from the third analysis on local visual risk cues using a 1000' treatment buffer and a 2500' control buffer suggest an 8.0% risk discount for properties that are within 1000' of a blue line. This result could be invalidated by the presence of treatment effect heterogeneity, a potential concern for this analysis. 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. 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. The third analysis has several next steps that are in progress. First, I need to include recently acquired housing transactions for the years 2019 and 2020. This may help with some of the data limitations in this analysis and may even make it possible to calculate the de Chaisemartin and D'Haultfœuille (2020) and Callaway and Sant'Anna (2020) estimators. Second, 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, if additional data makes it possible to calculate this estimator then another possible step for this analysis is to use this estimator to test for parallel pre-trends.

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 A.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.

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 exogenous tsunami risk signals 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 signals 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). And 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.

8 References

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A. Online Appendix

A.1 Expected utility model modified from Hallstrom and Smith (2005)

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

$$\begin{aligned} E(V) = {} & p(r, I) U_T\big(r, h, m - R\big(r, h, i_0, p(r, I)\big) - L(r, h, i_0)\big) \\ & + \big(1 - p(r, I)\big) U_{NT}\left(r, h, m - R\big(r, h, i_0, p(r, I)\big)\right), \end{aligned} \quad (A.1)$$

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

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

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.[38] Under my hypothesis that the tsunami risk signals – or 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 $(R(r, h, i_0, p(r, I)))$.

A.2 Tsunami inundation zone scenario comparison

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.

[38] 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.

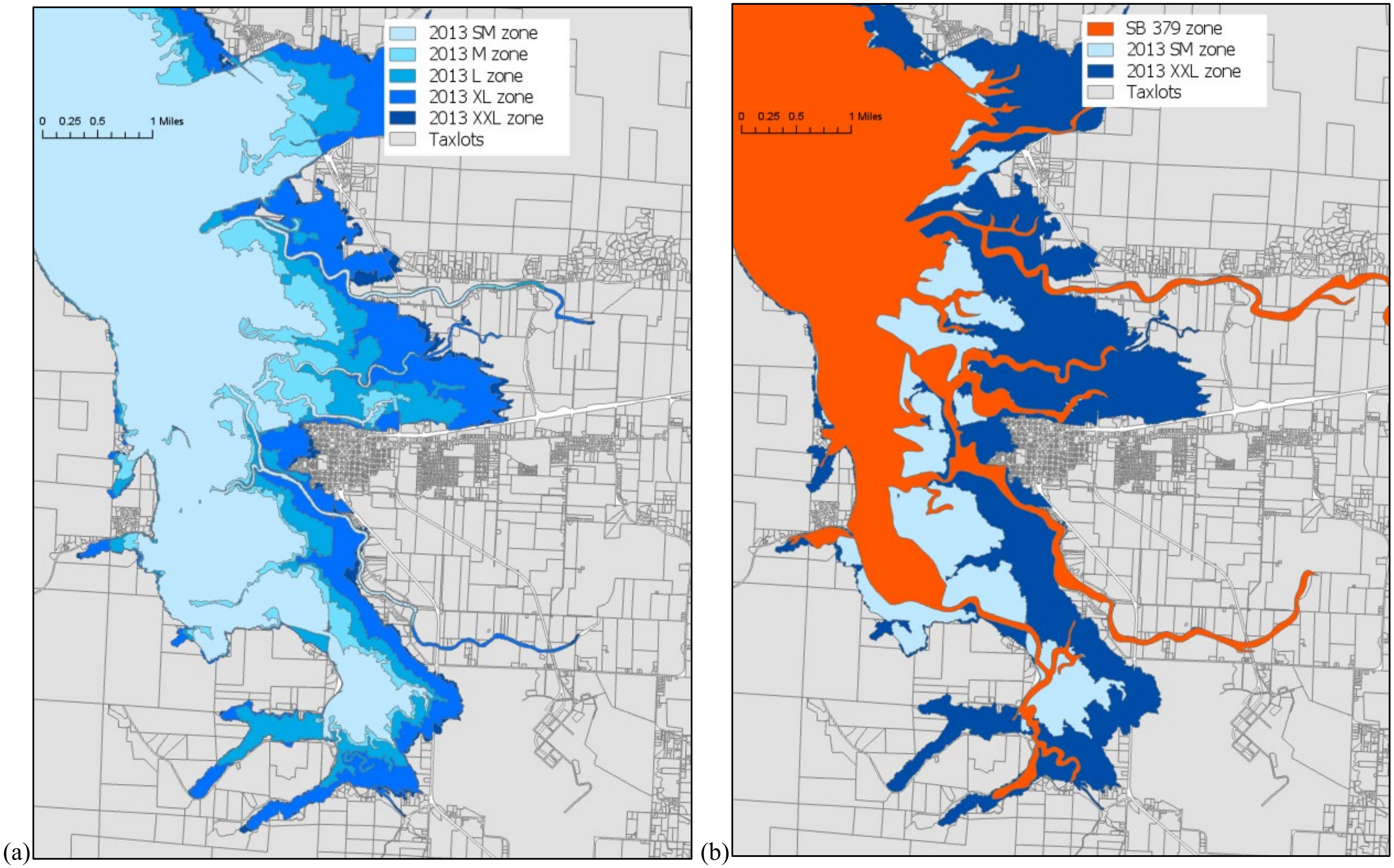


Figure A1. City of Tillamook, Tillamook County. (a) Tsunami inundation zones given by the five 2013 tsunami scenarios: SM, M, L, XL, XXL. (b) Comparison of tsunami inundation zones between the 1995 SB 379 line (orange) and the SM and XXL 2013 scenarios (blue).

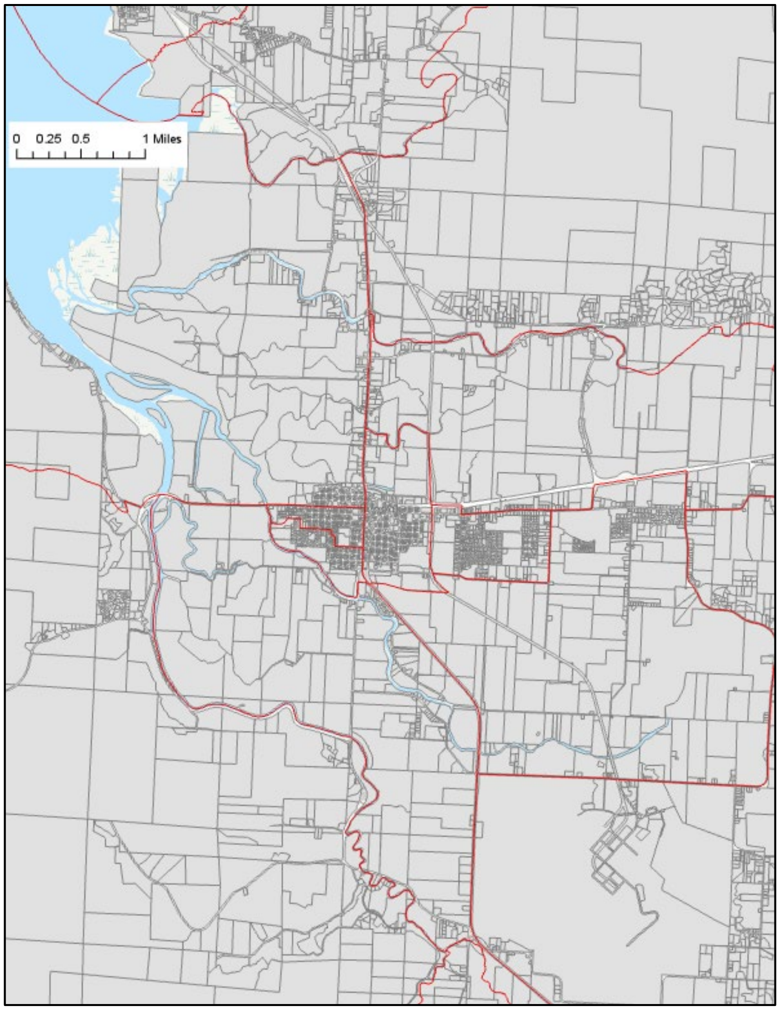


Figure A2. Approximate scale of Census block groups in the city of Tillamook (red).

A.3 Summary statistics

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

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

Variables	Mean	Std Dev	Min	Max
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

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

Variables	Mean	Std Dev	Min	Max
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		Standardized diff. in means
	Mean	Std Dev	Mean	Std Dev	
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)	-

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
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
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

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
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

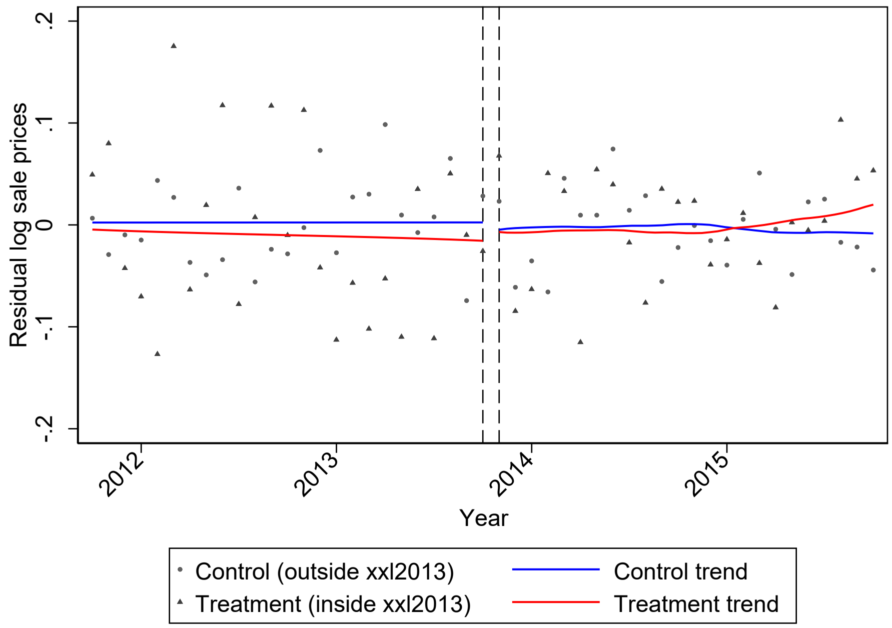
Table A4. Variable Definitions and Descriptive Statistics, by treatment, Third Analysis Sample, Model 62, 2014-2019

	Outside blue line neighborhood (>1000')		Inside blue line neighborhood (≤1000')		
	Mean	Std Dev	Mean	Std Dev	Standardized diff. in means
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 (ft)	8,247.52	(7,645.72)	9,927.78	(7,845.34)	-0.22
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	2,164.05	(2,831.09)	2,212.84	(2,349.62)	-0.02

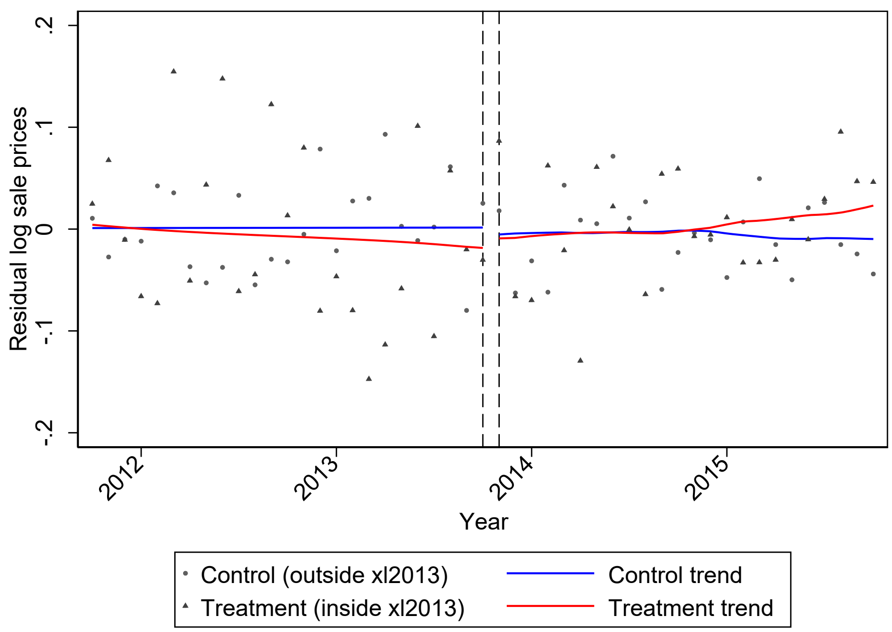
Table A4. Variable Definitions and Descriptive Statistics, by treatment, Third Analysis Sample, Model 62, 2014-2019

	Outside blue line neighborhood (>1000')		Inside blue line neighborhood (≤1000')		
	Mean	Std Dev	Mean	Std Dev	Standardized diff. in means
highway or interstate (ft)					
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		

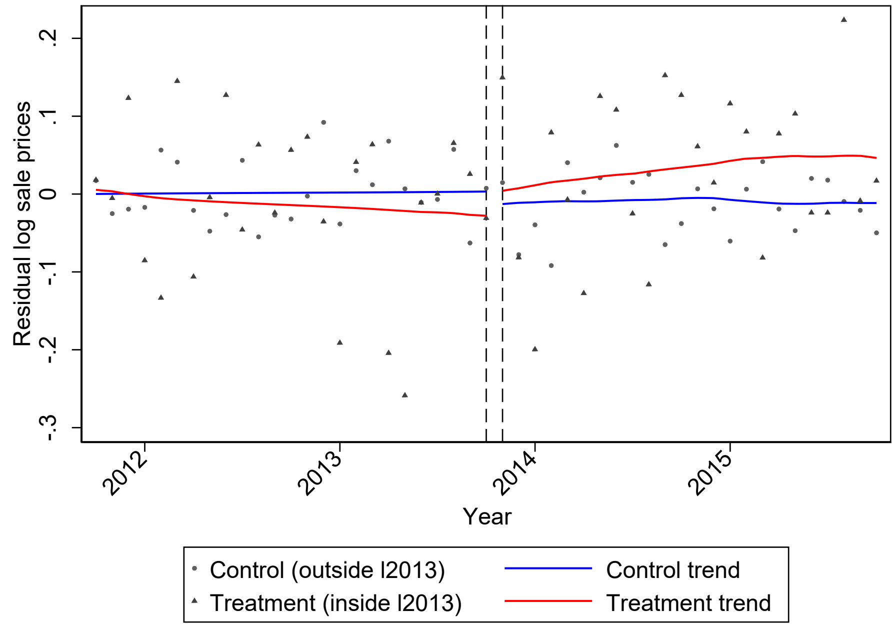
A.4 Price trends plots for the second analysis



(a)

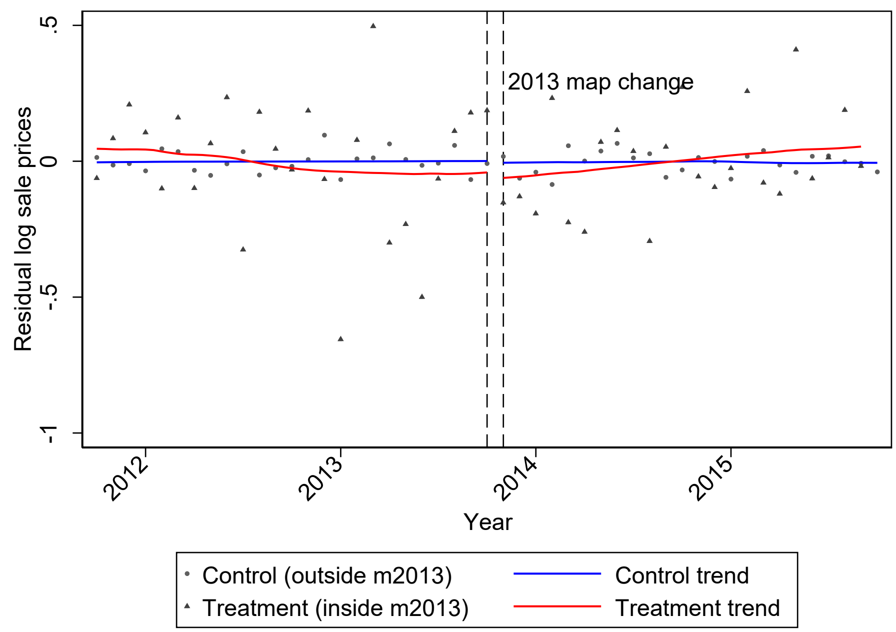


(b)



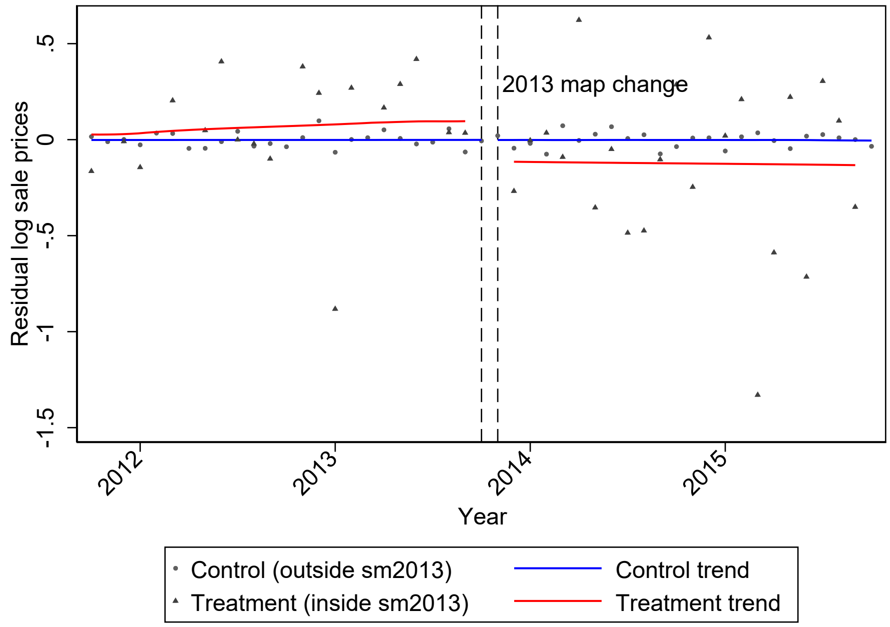
Residual log sale prices
-.3
-.2
-.1
0
.1
.2
2012
2013
2014
2015
Year
Control (outside l2013)
Treatment (inside l2013)
Control trend
Treatment trend

(c)



Residual log sale prices
-1
-.5
0
.5
2013 map change
2012
2013
2014
2015
Year
Control (outside m2013)
Treatment (inside m2013)
Control trend
Treatment trend

(d)



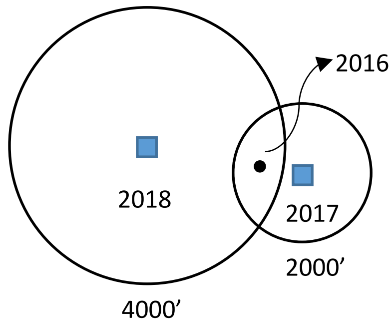
(e)

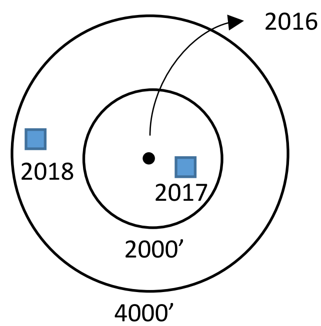
Figure A3. Housing price trends inside and outside of the treatment inundation line – XXL, Xl, L, M, or SM – for the seven coastal counties and the second analysis. Plot of residual (log) sale prices net of structural attributes, location covariates, and fixed effects aggregated by month with local polynomial trend lines. The time range is 2 years before and after the 2013 map change. Figures (a)-(e) present plots for Models 1-5.

A.5 Tsunami blue line overlap cases

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 below. 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 on the left 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 on the right 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 on the left to visualize the following overlap cases.

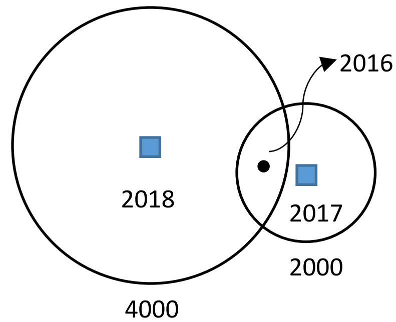




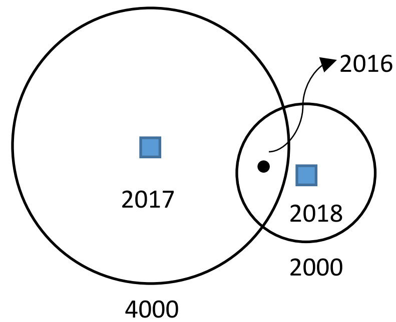
The central idea of treatment and event assignment is that “earliest supersedes nearest.” If a transaction lies within a given buffer distance of two different blue lines and one of the blue lines is installed before the transaction and the other is installed after the transaction, I use the first-installed 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 cases that can occur when a treatment buffer and control buffer overlap for a transaction. I look at 11 cases below but cases 1 and 2 are identical as are cases 3 and 4.

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.

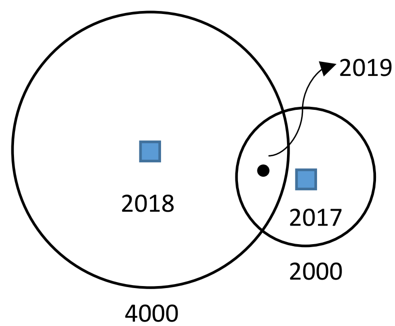
Case 1: Treated pre-installation and control pre-installation: the transaction is in the treated buffer before the blue line's installation and in the control buffer before the blue line's installation. Timing "doesn't matter" here because the transaction occurs before both the "treated" and "control" 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.



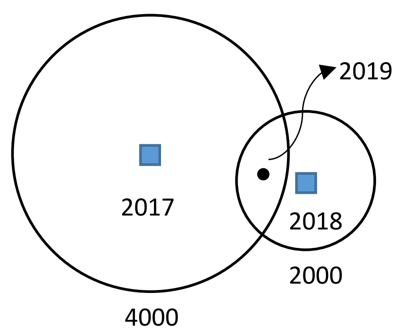
Case 2: Treated pre-installation and control pre-installation. The transaction should be used as treated pre-installation as in case 1.



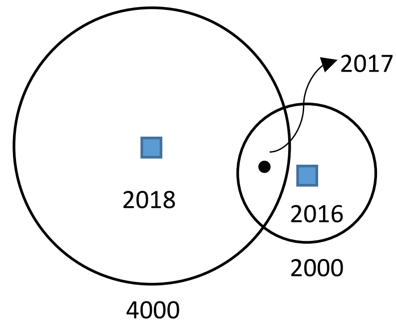
Case 3: Treated post-installation and control post-installation. Timing "doesn't matter" here because the transaction occurs after both the "treated" and "control" blue lines are installed. Since timing doesn't matter, distance determines whether it's treated or control. In this case, since it's in both,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



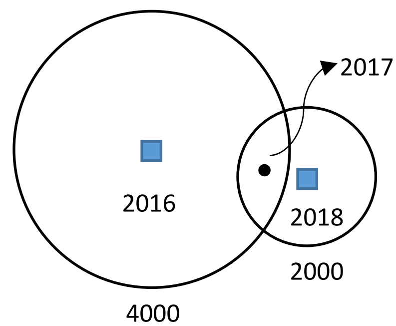
Case 4: Treated post-installation and control post-installation. The transaction should be used as treated pre-installation as in case 3.



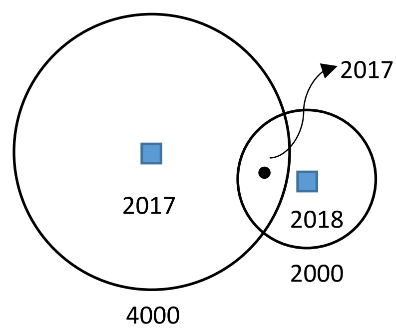
Case 5: Treated post-installation and control pre-installation. Now timing matters because the transaction occurs between the installation of the blue line whose control group it's in and the blue line whose treatment group it's in. "Earliest supersedes nearest" means that it's the blue line that's installed 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.



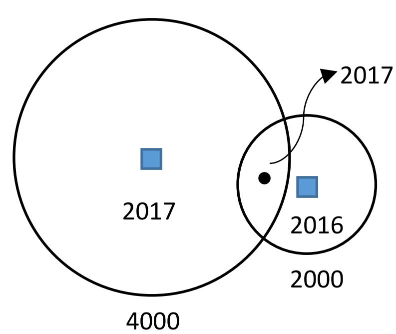
Case 6: Treated pre-installation and control post-installation. Timing matters because the transaction occurs between the installation of the blue line whose control group it's in and the blue line whose treatment group it's in. "Earliest supersedes nearest" means that it's the blue line that's installed 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.



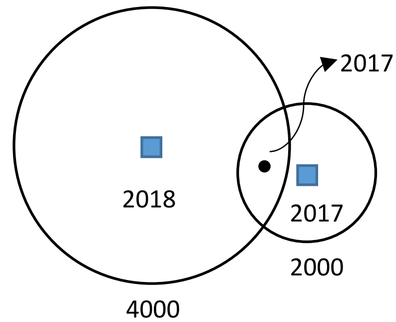
Case 7: Treated pre-installation and control is at installation (the transaction date and installation date of the blue line defining the control buffer is the same). When the transaction date is at the same time as the blue line installation date this is considered to be "pre-installation" because the 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.



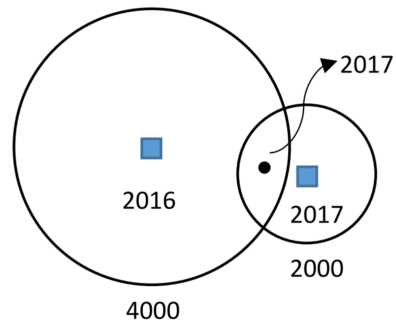
Case 8: Treated post-installation and control is at installation (the transaction date and installation date of the blue line defining the control buffer is the same). For the same reasons as in case 7, this is technically a "control pre-installation" situation. Thus, this is like case 5 and the transaction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



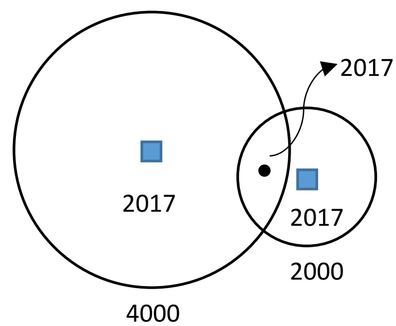
Case 9: Control pre-installation and treated is at installation (the transaction date and installation date of the blue line defining the treatment buffer is the same). For the same reasons as in case 7, this is technically a "treatment pre-installation" situation. Thus, this is like case 1 and the transaction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



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.



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.



A.6 Matching results for the first analysis

Tables A5, A6, A7, and A8 report the covariate balance results for the PSM, NNM, CEM and EB matching/weighting methods, respectively. The standardized difference in means for the variables used in each procedure is measured for all the primary models before matching/weighting (raw) and after matching/weighting (matched/weighted). 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 still 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) did not appreciably improve covariate balance.[39] The absolute standardized difference in means for elevation and distance to the ocean did not decrease to below the rule of thumb. However, the CEM method does not drop 90% of control observations, unlike the PSM and NNM methods. 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.[40] Unlike the other three methods, however, the EB method is purely a weighting method and, as such, does not drop observations. However, an inspection of the weights generated by EB shows that many observations are assigned very small weights, suggesting that this method also effectively "drops" many control observations. In summary, 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.

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

[39] Table A7 reports unweighted standardized differences. Future iterations of this paper will report weighted standardized differences since CEM is a weighting method and therefore drops few observations.

[40] Table A8 reports unweighted standardized differences. Future iterations of this paper will report weighted standardized differences since EB is a weighting method and therefore drops few observations.

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

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	Matched	Raw	Matched	Raw	Matched
Sold after 2011 Tohoku EQ (tohoku=1)	-0.0463	-0.0738			-0.0136	0.0105
Sold after 2015 article (article=1)			0.0079	-0.0567	0.0032	-0.0211
Elevation (ft)	1.5211	1.2699	1.7209	1.1031	1.5148	1.2832
Log distance to ocean shoreline	0.6606	0.4441	0.7361	0.5445	0.6743	0.3887
Sale year of the property	-0.0368	-0.0640	0.0037	-0.0421	-0.0017	0.0081
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

A.7 Regression results and figures

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)

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
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**
	(.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***

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

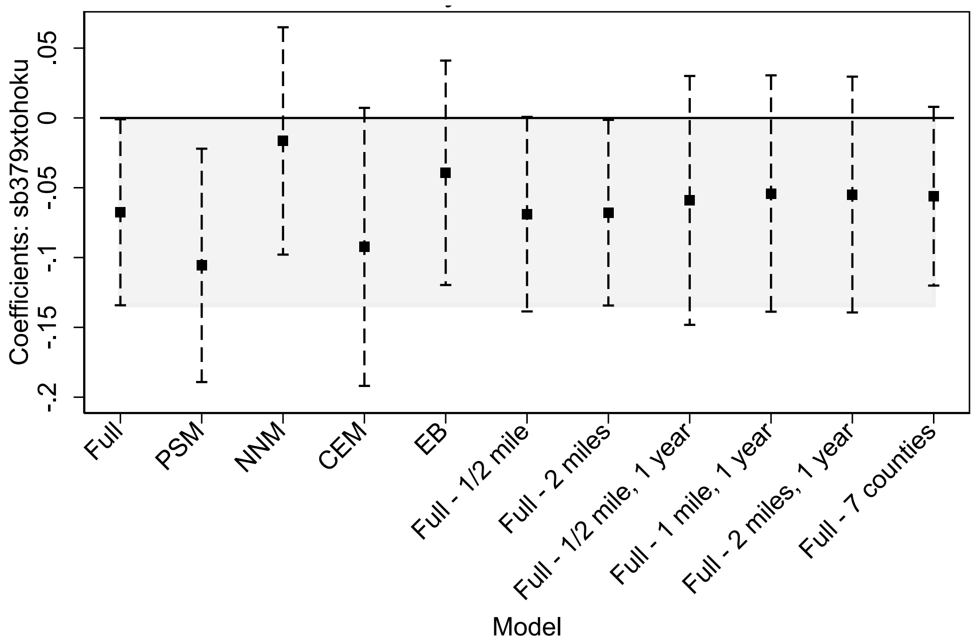
Variables	Model I Coefficient/SE	Model II Coefficient/SE	Model III Coefficient/SE
	(.0167)	(.0117)	(.0100)
Log distance to nearest airport	.0434*	.0213	.03066**
	(.0223)	(.0160)	(.0128)
Log distance to nearest k-12 school	.0264*	.0305***	.0244***
	(.0149)	(.0104)	(.0084)
Log distance to nearest wastewater 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

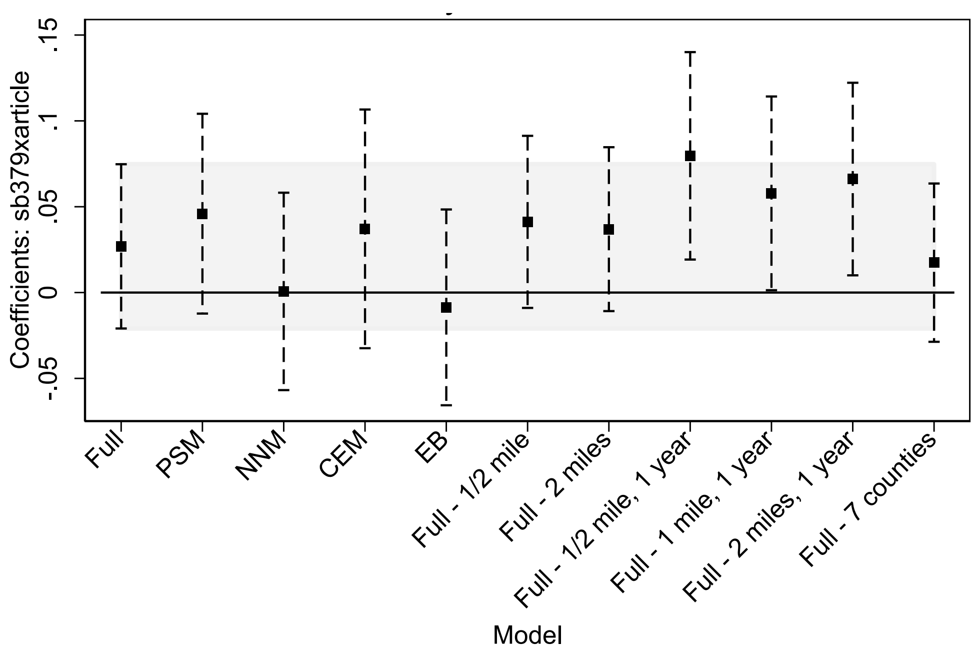
Table A10. DID falsification test results for the first analysis, full data

	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 A4. Average treatment effect on the treated estimates with 95% confidence intervals for the first analysis' models. 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)				.06539	
				(.07219)	
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***
	(.0077)	(.0078)	(.0084)	(.0090)	(.0086)

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
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)
Slope (angular degrees of slope)	-.0150*	-.0130	-.0235***	-.0165*	-.0040

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
	(.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
	(.0141)	(.0144)	(.0156)	(.0168)	(.0175)
Log distance to nearest hospital	-.0175	-.0214	-.0106	-.0220	-.0397

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
	(.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)

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

	Coefficient	SE
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)
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		-.0728*			

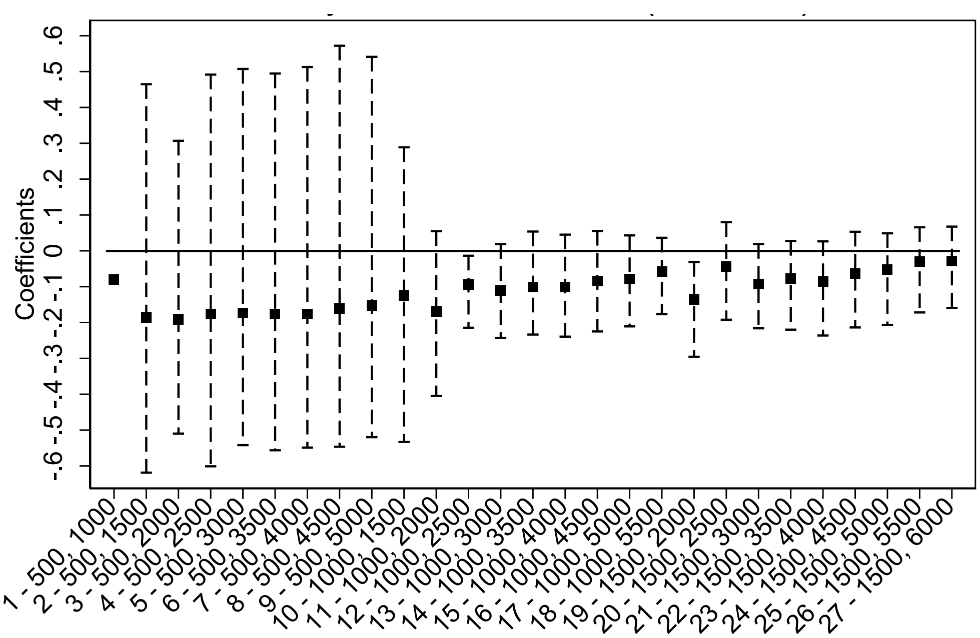
Table A14. DID falsification test results for the second analysis, full data

	Model 1 Coefficient/SE	Model 2 Coefficient/SE	Model 3 Coefficient/SE	Model 4 Coefficient/SE	Model 5 Coefficient/SE
sold after 10/2/12 (falsenewmaps)					
		(.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 status (randomevent)	.0248				
	(.0251)				
2013 XL zone (xl2013) x placebo event status (randomevent)		.0300			
		(.0266)			
2013 L zone (l2013) x			-.0025		

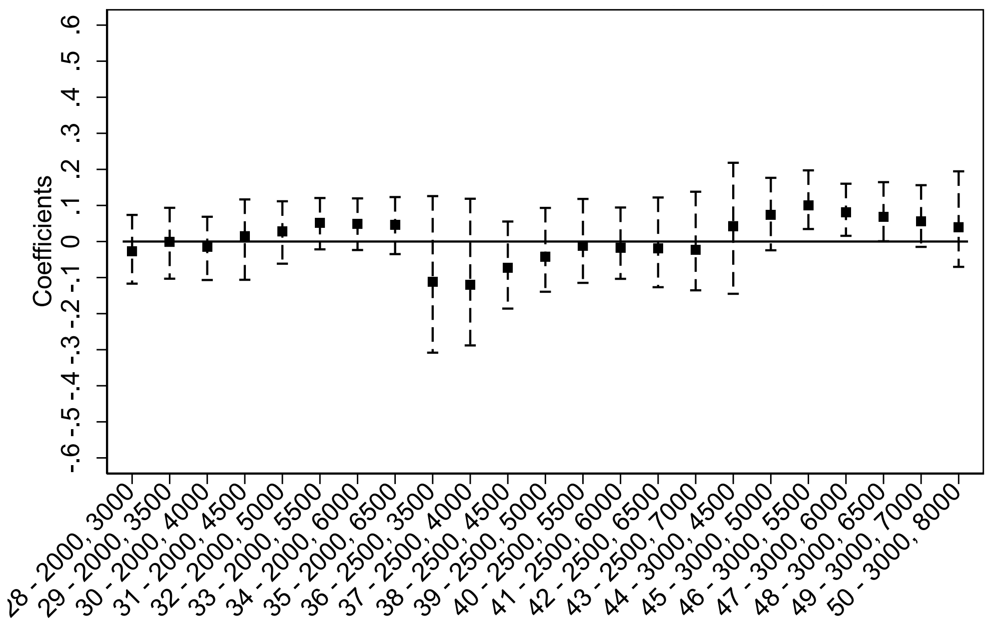
Table A14. DID falsification test results for the second analysis, full data

	Model 1 Coefficient/SE	Model 2 Coefficient/SE	Model 3 Coefficient/SE	Model 4 Coefficient/SE	Model 5 Coefficient/SE
placebo event status (randomevent)					
			(.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 A5. Average treatment effect on the treated estimates with 95% confidence intervals for Models 1 through 50 of the third analysis. 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

Variables	DID Coefficient	DID p-value	DDD Coefficient	DDD 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

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

	DID		DDD	
Variables	Coefficient	p-value	Coefficient	p-value
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
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