Is the Risk of Sea Level Rise Capitalized in Residential Real Estate?

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Using a comprehensive database of coastal home sales merged with data on elevation relative to local tides, we compare prices for houses based on their inundation threshold under projections of sea level rise. The analysis separates the sensitivity of housing to rising seas from other confounding characteristics by exploiting cross-sectional differences in relative sea level rise due to vertical land motion. This provides variation in the expected time to inundation for properties of similar elevation and distance from the coast. In a variety of specifications and test settings, we find precisely estimated null results suggesting limited price effects. (*JEL* G10, R30, Q54)

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Over the past century, mean sea levels rose by 1.7 millimeters (mm) per year (National Oceanic and Atmospheric Administration 2017b). That rate has accelerated over the past several decades, and current forecasts suggest mean sea levels could rise by as much as two meters between 2000 and 2100 (DeConto and Pollard 2016). While extreme changes in sea level will have dramatic consequences across a wide range of sectors in the economy, to the extent these risks are priced, they should directly affect the value of coastal real estate. In this paper, we ask whether or not this is the case—do recent transaction prices for residential real estate capitalize the threat of sea level rise (SLR)? If so, what rate of SLR is consistent with housing price data?

To answer the above questions, we leverage a comprehensive data set of recent residential real estate transactions matched with property- level elevation

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and tidal data. This allows us to compare prices of otherwise similar homes but for which the time to inundation will differ depending on the pace of SLR. In contrast to the findings in recent work by Keenan, Hill, and Gumber (2018), Bernstein, Gustafson, and Lewis (forthcoming), and Baldauf, Garlappi, and Yannelis (2018), we find no evidence of significant valuation effects across a large number of specifications and test settings, suggesting some combination of optimistic market projections of SLR, low discount rates, or a belief in cheap and available mitigation. We reconcile the differences in findings and interpretations across the related papers at length in the sections below.

Of course, an immediate challenge to the task of linking house price levels to SLR projections is that other real estate amenities (and defects) are often associated with susceptibility to rising sea levels. In particular, housing elevation and location are characteristics that play a large role in any hedonic model of prices, independent of SLR. However, these same characteristics are also the key drivers of a property's useful life under rising sea levels. Tests that ignore these confounding effects risk confusing the price effects of SLR with the value of correlated characteristics. On the other hand, tests that control for these characteristics must rely on other sources of variation in SLR exposure that produce unique price predictions. Our paper provides such a framework for separating the impact of rising sea levels from the confounding effects of elevation and location. Specifically, our primary tests exploit cross-sectional variation in the expected speed of SLR relative to local land masses under the same global sea level rise scenarios to generate testable predictions for the local price of elevation.

Local variation in the speed of relative sea level rise (RSLR) comes from several sources, but most notably is a result of the combined effects of land subsidence related to local geological factors and land rebound related to past and current glacial melt. The former—land subsidence—can occur as a result of groundwater extraction, bedrock dissolution, tectonic motion, and/or soil degradation. For example, over the next century, the land mass in Galveston, Texas is expected to fall considerably more than the rest of the United States relative to local mean sea levels as a result of the withdrawal of water from clay aquifers. Over the past century, surface elevations in Galveston relative to local sea levels have fallen by 0.65 meters (National Oceanic and Atmospheric Administration, 2017a).

Land rebound, on the other hand, is largely a product of isostatic rebound, a slow and persistent uplift in land masses caused by the removal of heavy, now-melted glaciers that covered large parts of North America during the last ice age. These effects continue today, attenuating the relative effects of global SLR. Juneau, Alaska, provides an extreme example. A 2007 report for the City of Juneau (Kelly et al. 2007) asserted that "global sea level is projected to rise 0.3 ft to 3.0 ft. In the City and Borough of Juneau (CBJ), however, the land surface is rising as a result of the loss of glacial ice (isostatic rebound), and the rate of uplift is greater than the projected rate of global sea level rise. Over the next century, the relative sea level in the CBJ likely will decrease between 1.0 and 3.6 feet."

Although extreme, the comparison of Galveston to Juneau provides one example of cross-sectional variation in exposure to rising sea levels and the unique predictions it delivers. Two otherwise identical waterfront properties currently situated one foot above local mean high water levels will face dramatically different levels of exposure to globally rising water over the next century depending on their location. This variation due to land mass rise gives us a key testable prediction: if expectations of global SLR are reflected in real estate prices, the value of current elevation in areas where land is rising (or at least sinking more slowly than average) will be lower than in areas experiencing the fastest relative sea level rise due to local land subsidence. Hence, rather than identifying the effects of sea level rise from property-level variation in height, or by simply comparing the price levels in Galveston versus Juneau, we focus on the price effects of the interaction between regional relative sea level rise trends and property elevation. In short, if sea level rise is priced, elevation will be more valuable in areas slated to experience accelerated relative sea level rise.

Of course, the estimates are not limited to comparisons between just Alaska and Texas; there is substantial variation in expected RSLR impact due to a combination of factors both across and within states, discussed at length in Subsection 1.4. This cross-sectional heterogeneity allows for tests that use data from 23 coastal states. Moreover, by focusing on the specific prediction that elevation should be priced proportionately with the threat of relative sea level rise, we identify the effects of RSLR separately from (i) the average effect of elevation on price and (ii) cross-sectional local variation in overall price levels that might be correlated with land subsidence or rebound. Returning to the example, higher or lower average home values in Juneau versus Galveston due to regional economic activity (for example) will not affect our inference unless economic growth is specifically impounded in the price of housing elevation but not other housing amenities.

Using a variety of specifications, we consistently find that variation in exposure to RSLR has no effect on real estate prices that is statistically distinguishable from zero, at least until the end of our data in 2017. In simulations, we calibrate our regression parameters to match different models of expected global SLR under the assumption that a property's useful life ends when it is inundated at the daily high tide. The upper bounds of our estimates are consistent with the valuation impact that would arise from an unmitigated 0.27 mm per year average RSLR under 10% discount rates, an order of magnitude lower than the 2.8 mm per year experienced in recent history. Lower discount rates only reduce the required pace of unmitigated sea level rise needed to generate the observed coefficients, as price differences between houses flooding at different dates far in the future become larger as the present value of future rents grows.

As a matter of interpretation, while our findings could be consistent with public skepticism about SLR forecasts, they may also reflect beliefs in the availability of mitigation technologies to delay the time to inundation for even the riskiest properties. Densely populated areas may expect to benefit from sea walls; New York and Boston have already begun planning for large-scale barrier systems to protect against flooding from major storms and have developed mitigation plans to deal with nuisance flooding (Garfield 2018; City of Boston 2017).[1] Homes in less valuable areas can be protected by more modest sea walls. Forge Engineering advertises the cost of constructing a home-specific sea wall in Florida between $250 and $1000 a linear foot.[2] Coastal homes can also be elevated on stilts and increasingly are; raising a house by just one meter yields forty years of protection under the more extreme sea level rise forecasts (Jackson 2013). Hence, to the extent the null results reported herein are informed by market beliefs, they should be interpreted as consistent with a slow pace of expected SLR only after any available mitigation.

Our tests also depend on the assumption that unobservable housing characteristics that covary with the variation of interest (the interaction between regional RSLR trends and property elevation) do not also cause independent variation in house prices. This assumption would fail, for example, if the price of elevation due to better views was more pronounced in places experiencing faster RSLR but not because of it. While we can include controls for observable housing characteristics in our regressions and account for location via census tract fixed effects, in the absence of a correct and fully specified hedonic model, any missing characteristics that covary with our exposure variables will bias the coefficients of interest. We address these concerns with robustness tests on samples of vacant lots, for which unobservable price variation due to structures is limited, and with property fixed effects, using pre-1995 same property sales as a control. Finally, we present the results from an alternative identification strategy using coastal homes near the Great Lakes as a control group with which to estimate the value of elevation near the water. In each case, we consistently fail to find evidence suggesting that RSLR exposure affects home values.

Finally, we explore the data for heterogeneous price effects across a large set of potential mediating variables, mindful both that price effects might concentrate in pockets of the market, but also that we risk inviting false positives. Among 12 demographic variables that might arguably sort properties into areas where climate change is more or less salient, we find little in the way of convincing evidence that RSLR risk is clearly priced by some subgroups more than others. While we do find that elevation is priced differently in areas with a high fraction of rental properties, after accounting for differences in

[1] Even crude sea walls can be effective; one in Pondicherry, India, built in 1735 recently successfully protected area properties from the December 2004 tsunami that devastated many other Indian cities and towns (Tomlinson 2005).

[2] See Forge Engineering Inc., http://www.forgeeng.com/about forum qa1.php.

land subsidence and rebound that affect time to inundation, even in these areas, home prices no longer appear to incorporate RSLR risks.

Our paper contributes to a growing literature on the price impact from both recent and potential climatic catastrophes on real estate. Much of this work has focused on how local area prices react to major storms or their threat. Harrison, Smersh, and Schwartz (2001) use data from Alachua County, Florida, and find that homes located in flood zones do sell at a discount, but that discount is less than the capitalized cost of flood insurance. They conclude that the market does not price the potential impairment costs from future events above and beyond the cost of carrying insurance. Bin and Landry (2013) look at homes in Pitt County, North Carolina that were hit by hurricanes in both 1996 and 1999. Using a detailed hedonic pricing model, they estimate how prices reacted immediately after and in the years following the storms.[3] Their estimates indicate that homes in areas that flooded saw their value fall by about 7% in the immediate aftermath, but that discount dissipated within six years. These results echo those in Atreya, Ferreira, and Kriesel (2013) and Atreya and Ferreira (2015), who document that homes in flood plains that have not recently flooded do not sell at a discount relative to homes outside of flood plains. Overall, these papers fail to find evidence that natural disasters affect property values over the long run. In the short run, value effects are limited to those homes that sustained damage.[4]

More recently, several papers have directly asked how sea level rise has impacted home values. Keenan, Hill, and Gumber (2018) examine real estate in Miami-Dade County, Florida, and conclude that high-elevation homes appreciated more over the period 1971 to 2017 than low-elevation homes. However, the paper does not clearly identify the effects as relating to expectations of future sea level rise. Bernstein, Gustafson, and Lewis (forthcoming) demonstrate price effects in Zillow's ZTRAX housing data for homes identified by the National Oceanic and Atmospheric Administration (NOAA) as being at risk of future inundation for sea level rise. Section 1.4 compares identification strategies using the NOAA risk measure of sea level rise with ours and explores the benefits and costs of the different approaches. Section 4 further clarifies the key differences in experimental design and attempts to reconcile the findings. Meanwhile, contemporaneous work by Baldauf, Garlappi, and Yannelis (2018) examines heterogeneity in beliefs surrounding SLR and how it might manifest in prices. Finally, Stroebel et al. (2018) use textual analysis from property listings to construct a climate attention index and show that the threat of inundation far in the future is capitalized in transaction prices when the risk is made salient in the text of for-sale listings (e.g., via

3 Their attribute list includes items like distance to a major road, railroad, and the Tar River, among others at the micro area level.

4 Another paper indicating that environmental events have had a limited economic impact to date is that of Nováková and Tol (2017). Their paper looks at the relationship between local sea level rise and economic growth. They do not find any evidence to date that there has been a deleterious affect.

mentions of hurricane and flood zones). As discussed later, the results in these papers and ours largely differ due to our consideration of relative SLR as a both an important aspect of inundation risk and a means to separately identify inundation risk from the value of elevation, in contrast to other papers.

1. Data and Methodology

1.1 Real estate transaction characteristics and location

We use residential real estate data from CoreLogic that includes 148 million homes and 494 million transactions. The data covers over 99% of the total U.S. housing stock and represents the most comprehensive data set of U.S. residential deed and tax records according to the provider. The number of years of coverage varies by county, ranging up to 50, and the data set includes purchase price, tax amount, prior sale date, housing square footage, land acreage, and (with some county-by-county variation), number of bedrooms, number of bathrooms, year built, effective age, physical quality, and geocoded physical address (longitude and latitude), as well as other characteristics used by tax assessors to assign tax valuations.[5] Although we use the larger sample for placebo tests, most of our analysis focuses on coastal states in the continental United States and Alaska. Within these states, we cut the sample to properties within 30 kilometers (km) of the coast or closer. We also only include sales of single-family homes and duplexes coded as arm's-length transactions in the main analysis. The use of arm's-length transactions avoids potential biases associated with non-economic transactions that might be more prevalent for vacation properties or second homes.

Using FEMA-digitized floodplain maps collected by GiraffeGeo, property-level data has been merged with flood plain designation, which makes it possible to distinguish the effects of current and historical flood risk from expectations of future flood risk.[6]

Finally, we measure distance to the shore and designate beachfront properties using a combination of coastline maps.[7] To establish beachfront properties, we

[5] While most fields of interest are well populated, there is some variation by county. Fortunately, the data are sufficiently rich that excluding counties with missing controls (for example, some counties in Texas do not report the number of bedrooms) will have limited effect on test power.

[6] FEMA flood zone designations can be viewed as a noisy measure of the current or historical risk of flooding. They are also, however, nearly an exclusive determinant of flood insurance premiums under the National Flood Insurance Program (NFIP). Meanwhile, surveys of private market pricing appear to follow the same pattern– average quotes aggregated by one consumer finance website for properties in Florida, Texas, and New Jersey, respectively, were $13,732, $13,713, and $13,713 for homes in "V" zones; $2,870, $2,867, and $2,922 in "A" zones; and $402 in all states for properties in a "B" zone or better (https://www.valuepenguin.com/average-cost-flood-insurance). Hence, by controlling flood zone, we are removing price variation driven by current insurance costs. Given that available flood insurance is short term in nature, however, we are not controlling for, nor would we wish to control for, the hypothetical cost of long-term insurance against flooding and inundation.

[7] We start with NOAA's Global Self-consistent Hierarchical High-Resolution Geography Database (GSHHG). For properties within 0.3 km of the coast, as represented under the GSHHG, we replace distance with a more accurate, but computationally intensive set of polygons provided by NOAA that can be found at https://shoreline.noaa.gov/data/datasheets/composite.html.

create a dummy to indicate if distance from the shore is less than the square root of property square footage reported in CoreLogic.[8]

1.2 Sample Size and Test Power

The size and coverage of the CoreLogic data are important given the proposed tests. Accepting the possibility of a null result, it is important to take a stand *a priori* on sample size, and to that end, expected effect size. This in turn depends on household expectations for the pace of sea level rise. As a starting point, the U.S. Interagency Sea Level Rise Task Force provides six scenarios that can be used for assessment and risk-framing purposes (hereafter, the "Interagency Report") (Sweet et al. 2017). The low scenario of a 30 centimeter (cm) global mean sea level (GMSL) rise by 2100 is consistent with a continuation of the recent approximately 3 mm per year rate through 2100. The models estimate a probability of exceeding the 30 cm threshold of 94%–100%. Intermediate scenarios of 1.0 meter of global mean sea level rise by 2100 (relative to 2000) generate an average of 10 mm per year. Finally, the Interagency Report's extreme scenario of 250 cm by 2100 (25 mm per year) fits with several estimates of the maximum plausible level of sea level rise over the next century (DeConto and Pollard 2016, Pfeffer, Harper, and O'Neel 2008, Sriver et al. 2012, Bamber and Aspinall 2013). In contrast, the IPCC Fifth Assessment Report puts an upper bound on the likely rate of sea level rise at 15.7 mm per year (Church et al. 2013). Finally, more recent work incorporating different scenarios of socioeconomic development produces forecasts of 5.7 mm per year to 18.9 mm per year (Nauels et al. 2017). Based on the variance among these models, the National Climate Assessment recommends policymakers consider a range of global mean sea level rise scenarios including as little as 20 cm (8 inches) and as much as two meters (6.6 feet) by 2100.

Given plausible discount rates, even under the most pessimistic SLR expectations (the Interagency Report's extreme forecast of 25 mm per year, for example), price effects will be small. For example, with capitalization rates of 6%, two otherwise equivalent lots at 1.25 meters elevation and 1.875 meters elevation will be below high tides in 50 and 75 years, respectively. If we assume they cease producing rental flows as of those dates, they should still only differ in value by 4%. Under more moderate SLR rates of 12.5 mm per year where flooding occurs in 100 years versus 150 years, the value difference would be

[8] If properties are on square nxn lots, then this measure captures the fact that any property within n feet of the coast must touch the coast. Of course, this measure will be noisy for rectangular lots generally and will under-identify beachfront properties that have small amounts of waterfront access but for which the centroid of the property is far from the coast.

just 0.2% at a 6% discount rate.[9] Hence, serious tests of the effects of long-run expectations with respect to SLR will require a large sample to support precise estimation.

Simple power tests suggest that with our data, we should detect even small home price sensitivities to sea level rise. For example, conservatively assume that a hypothetical baseline hedonic regression model with 5,000 predictors (our models have $\leq$ 4,500, including fixed effects) achieves an R^2 of 50% and that, under the alternative hypothesis that sea level rise is priced, its effects are tiny—specifically, assume the sea level variables increase the model R^2 by just 0.001 percentage points. Even given such a tiny effect, a 5% test will reject with 90% power with just 525,000 observations. By way of comparison, our benchmark regressions will include between 813,000 and 4.2 million observations.

The size of the CoreLogic database also makes it possible to estimate housing prices with fine geographic fixed effects, given that identical homes even a few kilometers apart can support vastly different price levels. This makes it critical to estimate valuation models within compact geographic areas to minimize the impact unobserved variables may have on the results. This study uses census tracts as its basic geographic unit. These are the smallest areas used by the Census Bureau itself for which it also provides well-documented publicly available characteristic data. Table A1 in the Appendix reports several summary statistics regarding their size. As of the 2010 census, the coastal tracts in our sample have a median population of 4,123 and cover 3.19 square kilometers. The 95% range is also fairly compact. Between those percentiles, the population spans just 1,830 to 7,408 and the land area 0.31 to 83.49 square kilometers.[10] With census tract fixed effects, the empirical model identifies RSLR effects by taking the difference in house prices in the same census tract but at different elevations and comparing those differences with other towns with different RSLR levels. The use of fixed effects controls for base differences in housing prices due to, among other things, the quality of school systems, even within a city composed of multiple census tracts.

1.3 Elevation and tidal information

The housing data also includes latitude and longitudes for individual housing units, which makes it possible to link sales data with data from both the U.S. Geological Survey (USGS) and NOAA. Using a combination of USGS elevation data and NOAA data on local tidal levels and trends, we can estimate inundation risk for various levels of sea rise above current mean higher high

[9] This stylized example assumes rents are constant and not affected by the anticipation of future flooding, but once properties begin flooding, rental income drops to zero. Falling discount rates with long time horizons as suggested in Giglio, Maggiori, and Stroebel (2015) of course will lead to larger, more detectable effects.

[10] Not too surprisingly, the smaller areas tend to be in densely populated urban locations. The largest census tract in our data is located in Jefferson County, Washington, and is composed largely of Olympic National Park. The smallest tract can be found in Kings County, New York.

water levels, with or without consideration of trends in local relative sea level rise discussed below.

Our elevation measure is based on the tidally adjusted version described in Strauss et al. (2012). The first step identifies the elevation of individual properties based on the highest resolution publicly available elevation data with full coastal coverage under the USGS's National Elevation Dataset. The 1/3 arcsecond resolution map provides a resolution level of roughly 10 square meters, making it possible to cleanly capture differences in elevation across neighboring properties. In their raw form, these elevation data measure a location's elevation relative to a hypothetical zero elevation surface around the earth. The specific surface of interest reported by the USGS, the North American Vertical Datum of 1988 or NAVD88, is designed to take into account the effects of differential surface gravity at different coordinates. Colloquially, the NAVD88 reference datum is often described as elevation above the water level from a hypothetical grid of canals criss-crossing the continent.

Point elevations also need to be transformed to account for local differences between high tide and mean sea level elevation under NAVD88. This can be accomplished with NOAA's Vdatum program for transforming data across different reference datum. In our case, elevations produced under NAVD88 by the USGS are transformed to elevation relative to local mean higher high water lines (MHHW). MHHW is the average of the highest daily tidal elevation, averaged over available dates since 1983.[11]

As a matter of interpretation, when produced at the property level, elevation above local high tides identifies the level of average/global sea level rise that would have to occur such that the property would be marginally inundated once daily, at the highest tide of each day, assuming no vertical land motion and no mitigation occurs. Naturally, properties will be affected before they fall below MHHW. Tidal flooding in coastal zones is exacerbated by seasonal and lunar cycles, with spring and king tides causing predictable increases in tidal level that exceed local MHHW. Lower frequency events like the El Niño Southern Oscillation (ENSO) also exacerbate high tide flooding. Finally, extreme storms generating swells or storm surges will affect the inhabitability of properties elevated above zero elevation (Sweet et al., 2017). As an alternative to measuring elevation above local MHHW, local tidal stations also provide historical percentage exceedance levels. For example, the 10% exceedance level captures the elevation threshold below which a lot would be expected to flood at least once in every 10 years. Section 2.2 discusses the use of exceedance levels in elevation calculations and shows that the basic hypothesis tests do not change: properties will be affected differentially based on elevation, such that elevation should be valued more in areas where RSLR is forecast to happen on

[11] Given that the Vdatum transformation can only be made exactly at the coast, for inland properties, we estimate the adjustment for a fine vector of coastal points and then, for each census tract, use the tidal variation for the nearest coastal point to the median housing coordinates in that census tract.

an accelerated basis. If anything, the assumption that properties are affected at zero elevation over MHHW (rather than earlier) maximizes the potential speed of SLR that would be implied by the analysis.[12]

1.4 Cross-sectional variation in RSLR

The last data step involves integrating the tidal elevation of a property with information about local trends in relative sea level rise. These give us cross-sectional variation in future inundation risk across properties with the same current elevation relative to high tides, based on the fact that sea levels are projected to rise differentially in different areas when measured relative to local land masses.

Data on relative sea level rise is based on historical trends in regional mean sea levels, using at least 30 years from 142 tidal stations. NOAA describes the variation in sea level trends as predominantly reflecting "differences in rates and sources of vertical land motion" (National Oceanic and Atmospheric Administration 2013). For each census tract, the relative sea level rise trend is defined as the weighted average trend of the two nearest water stations, where weighting is done by inverse distance. Distance from stations is measured from the median housing longitude and latitude in each census tract.

To give a sense of this variation, Figure 1 plots the latitude and longitude of fixed tidal stations. Color and shape vary based on the trend in changing elevation recorded at the station relative to local land. Stations that recorded negative trends in elevation are represented as blue-gray circles (ranging from dark to light based on the magnitude of the trend). These are areas for which the land mass was actually rising, as is evident in Alaska and in parts of the northwest coast. Yellow and orange squares include land that is roughly holding a fixed elevation, but falling relative to rising seas. Red triangles indicate land masses that are jointly experiencing rising sea levels and land subsidence.

While we take these trends as given, NOAA provides the following description of the variation: "The mid-Atlantic coastline along the United States is sinking slowly due to the glacial rebound effect of the uplift of the Hudson Bay region since the end of the last ice age. The Mississippi delta region of Louisiana is rapidly sinking due to the loading of the lithosphere and compaction of the sediments deposited by the Mississippi River. The Texas coastline is also sinking, likely due to similar causes, in addition to oil and gas extraction. The volcanically active Island of Hawaii is sinking relative to the other islands in the Hawaiian chain. Some areas of the northern California, Oregon, and Washington coastline are rising slowly due to the tectonic effects of subduction beneath the North American continent. Rapid uplift in southeastern Alaska is believed to be due to the melting of mountain glaciers" (National Oceanic and Atmospheric Administration 2017b). The diversity of causal

12 Using a lower base water level increases the time to inundation. That in turn increases the implied rate of SLR consistent with the resulting parameter estimates, since the water has to rise further.

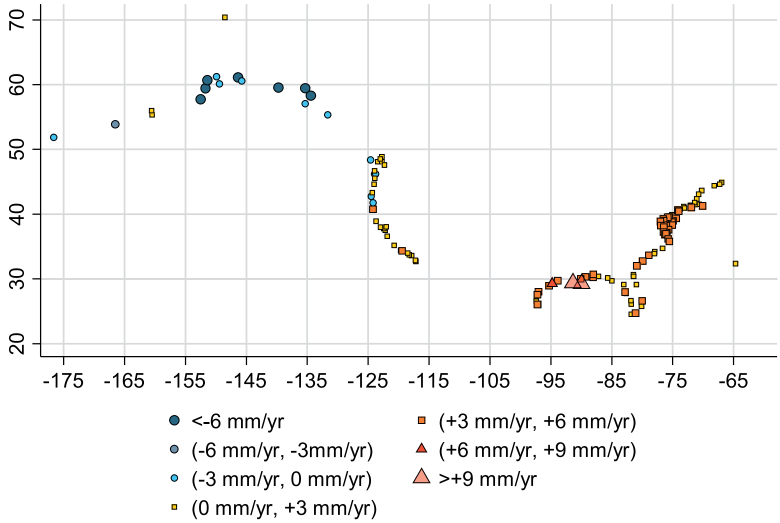

Figure 1
U.S. mean RSLR by latitude and longitude
The figure plots the latitude and longitude of tidal stations, along with historical trends in changing elevation measured relative to local land. Blue circles capture rising land mass. Yellow and orange squares include land that is roughly holding a fixed elevation, but falling relative to rising seas. Orange and red triangles represent land that is subsiding quickly, exacerbating global SLR effects. Data are from the National Oceanic and Atmospheric Administration (2017b).

explanations for RSLR is potentially helpful in limiting the likelihood of a baseline correlation with other local characteristics; less so than if it was driven by a single factor and was therefore more locally correlated. Figure 1 also suggests that there is substantive variation even within states—although coastal properties in smaller states are likely to face similar RSLR effects; within California, Texas, Washington, and Florida, there are neighborhoods in the same state facing dramatically different trends. To this point, the water station reporting the lowest trend in the most exposed state (Port Mansfield, Texas) has experienced lower RSLR than the water station reporting the highest trend in the least exposed state (Garibaldi, Oregon). Across states, meanwhile, RSLR does appear higher on average in the Gulf and along the eastern seaboard, and lowest closer to the Pacific Northwest. To the extent that this regional variation correlates with exposure to extreme weather events—hurricanes, for example—a casual correlation suggests that higher RSLR is associated with more extreme weather risk. If anything, this should help support the value of elevation in areas with both faster land subsidence and higher storm risk (Louisiana, for example). It therefore seems unlikely that a latent relationship between elevation prices and RSLR is masked by correlated storm exposure in the data—if anything, the opposite is likely to be true.

Our tests are rooted in the assumption that long-run expectations of relative sea level rise are influenced by historical trends in relative sea level rise.

This seems consistent with NOAA's own forecasts. Figure 2 plots NOAA's intermediate forecasts for SLR as of the year 2050 for areas that can be matched to local tidal stations (National Oceanic and Atmospheric Administration 2018b). The x-axis is the local historical trend. Historical trends explain greater than 80% of the variation in NOAA's own forecasts.

This paper's use of local tidally adjusted elevation and local RSLR trends to calculate SLR risk differs from the methodology of Bernstein, Gustafson, and Lewis (forthcoming) and Baldauf, Garlappi, and Yannelis (2018). Both designate homes characterized by NOAA as likely to be inundated if local seas rise by some value as more at risk. Bernstein, Gustafson, and Lewis (forthcoming) do so in one-foot increments, while Baldauf, Garlappi, and Yannelis (2018) use a dummy variable indicating if NOAA has designated a home at risk if seas rise six feet or more. Both, however, ignore land rebound and subsidence. In contrast, our regressions characterize SLR risk using a continuous measure based on a combination of the home's elevation relative to local MHHW as well as local rates of sea level rise net of land rebound. Critically, although NOAA forecasts clearly acknowledge the importance of historical RSLR as a determinant of future outcomes, their data documentation warns "the data in the maps do not consider natural processes such as erosion, subsidence, or future construction. Inundation is shown as it would appear during MHHW — the average of the higher high water height of each tidal day observed over the National Tidal Datum Epoch 19 years (excludes wind driven tides). The data, maps, and information provided should be used only as a screening-level tool" (National Oceanic and Atmospheric Administration 2018b).

The failure to account for trends in land movement treats properties at the same elevation over MHHW in Juneau and Galveston as equally risky. At the same time, we show in a later section that the difference in forecast trends due to land movement between Texas and Alaska is larger than the gap between low and intermediate global mean SLR trend projections for the twenty-first century. Given the significant impact of land movement on actual property exposure, incorporating this variation in our tests reflects a significant methodological departure from other work.

On the other hand, the NOAA maps used in Bernstein, Gustafson, and Lewis (forthcoming) and Baldauf, Garlappi, and Yannelis (2018) do take into consideration hydrological connectivity—man-made or natural land contours that impede the flow of water to potentially low-lying areas—while we do not. While this provides a unique source of variation in SLR risk that we eschew, note that these maps do not account for culverts or storm water systems that might allow areas designated as protected to flood (NOAA Office for Coastal Management 2017), inviting significant risk of misclassification under current topography. Meanwhile, the inability to forecast how hydrological connectivity will be affected by future erosion and construction, limiting the informativeness of this source of variation.

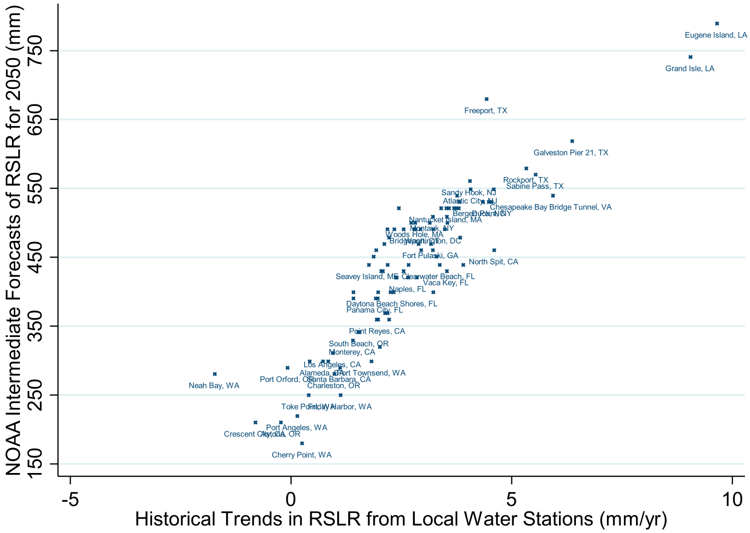

Figure 2
RSLR forecasts
The *y*-axis plots intermediate forecasts of RSLR for the year 2050 from NOAA's SLR viewer (https://coast.noaa.gov/digitalcoast/tools/slr) against cross-sectional variation in relative RSLR based on historical trends. For clarity of presentation, only some of the points are labeled with the location of the tidal station.

Therefore, the approach settled on here implicitly codes all low-lying areas as at risk, removing any variation from the data that depends on assumptions about hydrological connectivity within census tracts. To further mitigate concerns about our choice of measure, we run a variety of robustness tests. As will be seen, in all cases the findings are qualitatively unchanged. Overall, we believe that using RSLR as we have done in combination with various robustness tests using a variety of distance from shore cutoffs, subsets based on whether a home is protected by a levee and a comparison with shore homes near the Great Lakes should alleviate most concerns about the way this paper measures a home's inundation risk.

1.5 Summary statistics

Table 1 gives a snapshot of the variables of interest from CoreLogic, NOAA, and the USGS. With respect to the housing data, for the coastal states and for properties within 30 km of the shoreline, we observe 18,826,126 properties from either tax records or deeds. From tax records, we observe characteristics of the properties as well as tax valuations, while deed records give information on sales prices. In the summary statistics, when possible, home values are based on sales between 2015 and 2017. If no market transactions occurred in this period, a home's value is estimated using an adjusted tax assessment. This is calculated as the home's 2017 assessment multiplied by the average ratio of

Table 1
Summary statistics

Panel A: Key variables

	Mean	SD	P10	P50	P90
Home Value ($)	$ 379,514	$ 391,136	$ 81,263	$ 274,706	$ 750,996
Baths	2.87	1.61	1	2	6
Beds	4.33	1.88	2	4	7
Land (sq. feet)	38,291.75	2,430,310.00	3,000	8,130	40,624
Building (sq. feet)	1,895.32	2,747.38	1,024	1,681	2,992
Mobile Home	0.03	0.16	—	—	—
Effective Age	56.32	153.27	12	40	87
Recently Sold	0.12	0.33	—	—	1
Dist to shore (km)	9.88	8.50	0.83	7.26	23.67
Flood Zone (dummy)	0.12	0.33	—	—	1.00
Elevation NAVD88 (meters)	43.94	60.39	2.75	18.84	120.38
Elevation MHHW (meters)	42.98	60.04	2.34	17.86	118.73
Relative SLR trends (mm/yr)	2.78	1.10	1.39	2.67	4.16
Years to inundation (trend)	22,655.04	41,450.80	749.73	6,747.55	60,430.43
Years to inundation (intermediate)	3,523.24	5,099.50	177.89	1,403.65	9,667.24
Years to inundation (extreme)	1,578.10	2,239.86	83.11	643.82	4,347.47

Panel A reports means, standard deviations, and percentiles of housing and sea level rise (SLR) variables for homes within 30 km of the coast. For homes sold in the last two years of the sample, value is based on sales price. For those without recent sales, it is based on tax valuation adjusted for the county-level average ratio of assesed value to sales price.

Panel B: State risk exposures

State	Elevation (mm)	State	Relative sea level rise (mm/century)
LA	2,143	TX	563
GA	4,601	LA	562
SC	5,755	VA	413
NC	6,647	MS	384
FL	6,960	DE	377
…			
MD	57,539	ME	195
CT	60,232	WA	168
WA	86,158	CA	160
AK	89,431	OR	87
CA	89,492	AK	−410

In panel B, we rank the top and bottom five states in terms of average home elevation relative to mean higher high water (MHHW) and average relative sea level rise (RSLR) trend over at least the past 30 years. Averages are taken over residential properties within 30 km of a coastline. Elevation is reported in millimeters and relative sea level rise in millimeters per century for ease of comparison.

(*Continued*)

sales price to assessed value in the same county from 2015 to 2017.[13] Doing this county by county allows for local variation in tax assessments relative to market values. The average value of properties in the data is roughly $380,000. The median value is $275,000.

This highlights another difference between this paper's methods and those employed by Bernstein, Gustafson, and Lewis (forthcoming). They restrict their sample to homes within 0.25 miles that sold for between $50,000 and

13 Again, this is only for the purpose of establishing summary statistics. Regressions based on home value will all be executed based on transaction value.

Table 1
(Continued)

Panel C: Value impact

Hypothetical value impact (scenario)	Cap. rate	Mean	SD	P10	P50	P90
Current trend	2%	−0.16%	2.61%	0.00%	0.00%	0.00%
Intermediate forecast	2%	−1.63%	6.79%	−2.85%	0.00%	0.00%
Extreme forecasts	2%	−5.07%	12.51%	−18.97%	0.00%	0.00%
Current trend	4%	−0.07%	1.83%	0.00%	0.00%	0.00%
Intermediate forecast	4%	−0.49%	4.03%	−0.08%	0.00%	0.00%
Extreme forecasts	4%	−1.82%	7.18%	−3.60%	0.00%	0.00%
Current trend	6%	−0.04%	1.49%	0.00%	0.00%	0.00%
Intermediate forecast	6%	−0.25%	3.15%	0.00%	0.00%	0.00%
Extreme forecasts	6%	−0.91%	5.19%	−0.68%	0.00%	0.00%
Current trend	8%	−0.03%	1.28%	0.00%	0.00%	0.00%
Intermediate forecast	8%	−0.17%	2.70%	0.00%	0.00%	0.00%
Extreme forecasts	8%	−0.55%	4.22%	−0.13%	0.00%	0.00%

Panel C asseses the value impact of RSLR. We report the hypothetical value impact of RSLR under 12 combinations of SLR scenarios and capitalization rates. See text for details. We also provide the sum of current property values within 30 km of a coastline that, based on current elevation over mean higher high water (MHHW), will be inundated by 2100 if RSLR follows the local historical RSLR trend, the historical RSLR trend plus 10 mm per year (intermediate forecast), or the local historical RSLR trend plus 25 mm per year (extreme forecast).

Panel C: Value impact (cont).

	Value inundated by 2100 ($M)		
State	Low (+0 mm)	Intermediate forecast (+10 mm/yr)	Extreme forecast (+25mm/yr)
AK	—	—	—
AL	84.94	803.69	3,398.82
CA	2,578.25	17,951.36	54,237.20
CT	381.35	3,045.82	12,162.61
DE	377.46	2,270.83	6,207.47
FL	11,160.60	84,307.28	333,109.82
GA	661.99	3,497.20	11,411.64
LA	814.92	3,122.04	6,341.05
MA	1,078.65	9,174.80	32,485.59
MD	383.44	4,106.44	13,963.40
ME	126.22	738.31	1,534.92
MS	34.10	181.25	485.15
NC	509.92	7,146.70	21,668.72
NH	25.34	388.54	1,430.55
NJ	1,661.26	27,450.14	70,313.54
NY	1,661.00	18,651.84	94,417.28
OR	15.40	151.22	674.45
RI	61.25	695.86	2,921.13
SC	1,768.02	15,504.20	41,408.34
TX	276.59	9,216.81	16,980.95
VA	609.06	4,177.15	21,484.73
WA	176.93	1,582.06	3,821.44
Total	24,447	214,164	750,459

(*Continued*)

$10,000,000. While our data are initially restricted to homes within 30 km of the coast, later tests subdivide it further. We also do not include a price filter. This yields a larger data set, but also includes properties on average further from the coast.

Table 1
(Continued)

Panel D: Census tract characteristics and RSLR risk

Correlation with avg. elevation	Main sample	≤1.0 km to shore
Median Age	−0.0945	−0.1606
% HS Grad	0.0172	0.0661
% Bachelors	0.1647	0.1418
Median Earnings	0.2193	0.1701
Mean Earnings	0.2329	0.1209
Pop. Density	0.0485	−0.0036
Housing Density	−0.01	−0.0772
Household Size (mean)	0.1704	0.0795
Happening	0.2522	0.2845
Worried	0.2817	0.2457
Dem Leaning	0.2219	0.2779
% Rented	−0.0189	0.048
Correlation with local RSLR	**Main sample**	**≤1.0 km to shore**
Median Age	−0.0074	−0.0436
% HS Grad	0.0421	−0.1358
% Bachelors	−0.0967	−0.1152
Median Earnings	−0.0156	−0.0509
Mean Earnings	−0.0637	−0.0477
Pop. Density	−0.148	0.1121
Housing Density	−0.1335	0.1357
Household Size (mean)	−0.0862	0.0546
Happening	−0.2563	−0.1265
Worried	−0.2952	−0.0743
Dem Leaning	−0.3283	−0.1817
% Rented	−0.1284	0.0021

We report correlations of elevation and relative SLR with census tract–level variables for both the main sample and for a sample of properties within a kilometer of the shoreline.

In terms of characteristics, the median property has four bedrooms, two baths, 1,681 universal building square feet, and 8,130 in property square footage (< 0.2 acres). The median effective age is 40 years. Effective age is a variable that CoreLogic obtains from the local assessor's office and represents the building's age based on its current condition. For example, a home built in 1960 without any updates may earn an effective age of 1960, while the same house with a remodeled kitchen and bathrooms may be given an effective age of 1985. Naturally, the assessor's office puts a higher value on homes with more recent effective ages and adjusts the local tax bill accordingly. When the effective age is missing, we use the actual property age.

Regarding exposure to the risk of inundation due to SLR, we report summary statistics on elevation, trends in relative SLR, and tidal variation. The average property's elevation in our sample is 44 meters relative to NAVD88. Elevation under NAVD88, however, ignores local tidal variation. When we remeasure elevation relative to local high tides, defined based on the mean higher high water market discussed earlier, properties lose one meter of elevation on average, but with dramatic cross-sectional variation. For example, the difference in MHHW and NAVD88 reference datum in Key West, Florida, is just 0.02 meters, whereas in Seattle, Washington, the difference is 2.75 meters.

Failing to account for these differences has a large effect on geographic risk exposure.

Combining elevation, historical trends in relative SLR, and forecasts for the future, allows us to estimate a measure of time to inundation. We define low, intermediate, and high scenarios for SLR as the historical trend for any given area, the trend plus 10 mm per year, and the trend plus 25 mm per year, respectively. These roughly correspond to the Interagency Report's low, intermediate, and extreme forecasts for 0.3, 1 and 2.5 meters global mean SLR by 2100 relative to 2000. Note that even under the extreme scenario, the median property within 30 km of the coast remains unaffected for 643 years. However, 10% of properties will be completely underwater in just 83 years. Thus, the identification of any price effects is going to have to come from the extremes of the housing distribution.

Panel B summarizes elevation and relative SLR dimensions of risk by state, listing the top and bottom five states in terms of both variables. Louisiana is at risk in terms of both low-lying properties and historical trends in RSLR, while Washington and Alaska are both well protected. Otherwise, the rankings of risk exposure are largely independent.[14] Note that accounting for regional trends in land movement has an appreciable impact on risk. For example, historic trends in Texas and Alaska generate differences in RSLR of 9.7 mm per year. This is larger than the gap between low and intermediate global mean SLR trend projections for the twenty-first century provided by NOAA. The gap between Oregon and Louisiana of 4.8 mm per year more than doubles the difference in trend between NOAA's low and intermediate-low forecasts. In short, cross-sectional differences in RSLR expectations need to be accounted for when establishing the risk distribution. As a frame of reference, the average backwards-looking RSLR for properties in our data is 2.8 mm per year.

How large are potential value effects of RSLR? In panel C, we calculate a hypothetical value impact for each property as Pe^{-iTTI} where P is the house's value, i is an assumed discount rate, and TTI is the forecast inundation point under the scenarios laid out above. Even with tiny 2% discount rates and extreme forecasts, the median house within 30 km of the shore is unaffected. Yet, the most exposed 10th percentile of properties would lose 19% of their value as a result of RSLR when priced using the simple model above. Panel C lays out the distribution of price effects for the low, intermediate, and extreme scenarios, each at discount rates of 2% 4%, 6%, and 8%.

Panel C also summarizes the potential impact of RSLR on home values by state and lists the amount at risk under each scenario by the year 2100.[15] In

[14] Of course, many low-elevation areas in Louisiana are protected by federal levees. Table A3 explores the robustness of the main analysis to the exclusion of leveed areas.

[15] These numbers reflect summations of current home values that will be inundated at a specific date, not the present value of potential damages.

the leftmost column, where RSLR follows the same trend it followed over the prior century, roughly $24 billion in homes will be inundated by the end of the current century. It is important to recognize that this forecast does not depend on projections of future global warming, but instead requires only that preexisting trends in RSLR continue.

Moving to intermediate and extreme forecasts, the values become more economically relevant, with $214 billion and $750 billion in property value at risk. These numbers are based on the projected inundation dates of affected properties and do not capture flooding that would occur in extreme storm events, as in McKenna (2018), Milman (2018), Sisson (2018) and the Union of Concerned Scientists (2018). Furthermore, we also consider only the value of residential properties. To put our numbers in perspective, work by the Union of Concern Scientists calculates $1.07 trillion of residential and commercial property will be inundated by 2100, assuming sea levels rise at the highest projected rate from the 2014 National Climate Assessment.[16]

Finally, panel D demonstrates the central challenge with interpreting naïve estimates of the value impact of sea level rise as it relates to elevation. We report the correlations between several demographic characteristics by census tract and (i) elevation net of the MHHW and (ii) local RSLR. Census tract–level variables were selected from public data sources based on their likely impact on home values and perhaps the degree to which the population might be concerned about sea level rise. Median age, percent high school graduate or higher, percent bachelor degree or higher, mean earnings, median earnings, population density per square kilometer, housing density per square kilometer, percentage of the housing stock that is rented, and average household size are all taken from the 2010 census (earnings and density measures are logged). *Happening* and *worried* variables are from the Yale Climate Opinion survey. The former is the estimated percentage of the population that believes global warming is happening and the latter is the fraction worried about it. *Democratic leaning* is calculated based on a partisan value index reported by the Cook Political Report. The variable has been scaled so that positive numbers are associated with census tracts that lean toward Democratic candidates. It is only available at the congressional district level. A census tract is therefore assigned the value for the congressional district it is in. If a census tract is in more than one congressional district, then the average value for two districts is assigned to the census tract.

Immediately, elevation is strongly positively correlated with measures of education, earnings, beliefs about global warming, and political affiliation. This is true using the full sample (which includes houses in coastal states within 30 km of the coast), as well as those properties within a kilometer of the shore. Similar patterns are repeated when we look at the historical trends in sea level

16 Combining estimates from CREDA Affiliates (2017) and Gudell (2016) would suggest that roughly 62% of the value of all real estate is residential, helping to reconcile the differences.

rise. Places experiencing higher local relative sea level rise have lower levels of education, have lower earnings, are less likely to lean towards Democratic party affiliations, and are less likely to believe in or be worried about global warming. In particular, this sorting of population characteristics with risk would be consistent with the predictions from models where agents endogenously move in or out of at-risk areas based on heterogenous preferences or beliefs (see McNamara and Keeler 2013, Bunten and Kahn 2017, and Bunten and Kahn 2014, for example), limiting observable price effects even if the median real estate participant forecasts that SLR will continue at the current pace or faster.

2. Identification and Results

To identify the price effects of sea level rise expectations, the analysis exploits the intersection of two sources of variation in exposure to SLR risk. First, elevation serves as a hedge against rising water levels. Second, different geographies are forecast to experience relative sea level rise at different rates based on local land motion. Combined, these two facts generate a unique testable prediction regarding prices: as a hedge against sea level rise, elevation should be more valuable in places experiencing higher rates of relative sea level rise. That is, in the regression below (where X represents a vector of hedonic controls and geographic, and time fixed effects),

$$ln(price) = \beta_1 ln(elevation) + \beta_2 ln(elevation) \times RSLR + \beta_3 RSLR + \gamma X + e \tag{1}$$

β_2 should be positive if predicted sea level rise effects influence prices.[17] Focusing on the interaction effect does not rely on the price level of elevation to inform the price effects of sea level rise—elevation on its own likely enters into the consumer's utility function—nor does it rely on coarse geographic correlations, for which confounding interpretations are likely to be many. Instead, alternative regression interpretations must generate the prediction that the value of elevation will covary with the regional pace of RSLR, but for reasons other than RSLR.

Meanwhile, while the sign and significance will answer the question of whether or not sea level rise risk is priced in the cross-section, the level of β_2 will also inform the aggregate pace of sea level rise that is priced. For example, simulating price effects in our main sample under a Gordon growth model, a fixed discount rate of 8%, and baseline sea level rise levels (plus or minus local historical trends in relative sea level rise) of 1, 3, and 5 mm per year generates simulated β_2 coefficients of roughly 0.02, 0.025, and 0.03, respectively. Hence, depending on the range of coefficients observed and an assumed discount rate, we can and will recover a range of estimates of market-implied baseline sea level rise scenarios after the effects of any potential mitigation.

[17] Another way to interpret the prediction of $\beta_2 > 0$ is that the accelerated pace of sea level rise due to land subsidence will be primarily felt among low-elevation properties affected first.

2.1 Baseline results

Table 2 reports the first and most direct tests of the hypothesis suggested above by regressing log home prices on elevation, local relative sea level rise, and their interaction, along with a full set of controls, including census tract–level fixed effects (standard errors are also clustered at the census tract–level). Other controls include the log building square footage, standardized by CoreLogic to accommodate different reporting styles across counties and states, dummy variables for the number of bedrooms and bathrooms, land square footage, an indicator for mobile homes, fixed effects for appraiser codes for property view type when available, floodplain designation fixed effects, log distance to the nearest shoreline, an indicator for beachfront properties, defined as homes for which distance from the shore is less than the square root of the property square footage and fixed effects for effective age in the regressions. Because some counties do not report either the number of bedrooms and/or the number of bathrooms but otherwise provide good coverage of sales, when bedrooms and bathrooms are missing we include these with separate fixed effects.[18] The sample includes sales between 2012 and 2017 of properties within 30 km of the coast.

Column 1 begins with a descriptive regression that includes elevation uninteracted with relative sea level rise. The hedonic model offers few surprises, with property and building square footage serving as key price determinants. Beachfront properties and those otherwise close to the shore are priced at a premium, as well as newer homes and those with more bathrooms (full model coefficients are available from the authors, but for the sake of brevity, only key variables appear here). Elevation's effect is close to zero and insignificant, although without a credible identification strategy, it would be impossible to differentiate price effects of RSLR associated with elevation from better views, air quality, and other amenities that might enter into housing preferences.

Column 2 provides our baseline regression: a test of the price of elevation in the cross-section as it interacts with local relative SLR. Again, if SLR is priced, elevation should be valued more in areas expected to experience faster relative sea level rise. Instead, the interaction coefficient's estimate is near zero with a standard error of just 0.003. In Column 4, we look at the quintile of sales nearest to the shore—properties within 1.68 km of the water—and find a similar null result in Column 4. This time, standard errors are nearly double those of the full sample, but even so, the upper end of the confidence interval on the interaction coefficient is just 0.0103. We return to an economic interpretation of this magnitude in a few paragraphs.

Column 5 shows that the results remain unchanged when focusing on properties outside of FEMA's special flood hazard areas. Excluding these

[18] We also replicate the main specifications for the 588,000 coastal property sales from 2012–2017 within 30 km of the shore with nonmissing, nonzero bedroom and bath variables. Because the results are qualitatively unchanged from the full sample, the results are relegated to the Appendix.

Table 2
Main results

	Full sample		Shoreline sample		Excl. flood zone	2015-2017
ln(Price)	(1)	(2)	(3)	(4)	(5)	(6)
ln(Elevation over MHHW)	−0.003	−0.004	0.002	0.002	0.013	−0.004
	(0.003)	(0.008)	(0.005)	(0.013)	(0.010)	(0.009)
ln(Elevation over MHHW) x local RSLR trend		0.000		0.000	−0.004	0.001
		(0.003)		(0.005)	(0.003)	(0.003)
Relative local SLR trend	—	—	—	—	—	—
ln(Sq. feet)	0.566***	0.566***	0.571***	0.571***	0.548***	0.529***
	(0.006)	(0.006)	(0.009)	(0.009)	(0.006)	(0.008)
ln(Land sq. ft)	0.116***	0.116***	0.123***	0.123***	0.121***	0.117***
	(0.003)	(0.003)	(0.005)	(0.005)	(0.003)	(0.003)
ln(Distance to coast)	−0.117***	−0.117***	−0.132***	−0.132***	−0.105***	−0.113***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)
Beachfront	0.160***	0.160***	0.102***	0.102***	0.299***	0.171***
	(0.017)	(0.017)	(0.016)	(0.016)	(0.020)	(0.017)
Other Controls: Bed, Bath, Age, Flood Zone, Mobile	YES	YES	YES	YES	YES	YES
Year, Census Tract Fixed Effects	YES	YES	YES	YES	YES	YES
Observations	4,292,176	4,292,176	813,794	813,794	3,858,372	2,096,230
R^2	0.564	0.564	0.605	0.605	0.583	0.608

Using a sample of sales from 2012 to 2017 of homes within 30 km of the coast, we regress log price on log elevation over mean higher high water lines, local RSLR trends, and the interaction of the two, plus controls including log distance to the coast, log property and land square footage, indicators for beachfront, mobile homes, and propeties in flood zones, and fixed effects for the number of bedrooms, bathrooms, the effective age decile, the year, and the census tract. Columns 3 and 4 limit the sample to the quintile of homes nearest to the coast. Column 5 excludes flood zones. Column 6 limits the sample to 2015–2018. Standard errors are clustered at the census tract–level, are robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1%, 5%, and 10% levels, respectively.

properties ensures that expectations of future flood risk are not due to existing flood hazards. Column 6 limits the sample to 2015–2017 to address the possibility that including sales from 2012 dilutes price effects that have only occurred more recently. Again, the interaction term has a near zero estimate.[19]

2.2 Bounding economic magnitudes

To put the economic magnitudes of the reported coefficients and their upper bounds in perspective, we can ask what combination of discount rates and RSLR scenarios will deliver regression coefficients close to the estimated parameters. We begin with a control group of shoreline properties that were transacted from 1985 to 1995. Sales from this period are unlikely to produce RSLR price effects, or do so only to a negligible degree given the limited availability of information and awareness of RSLR risk prior to 2000. This yields a clean baseline to which RSLR effects can be added without worrying that price discounts on affected properties have been double counted. Maximum distance to shore is set to match the shoreline sample from Table 2.

Next, we adjust actual transaction prices to account for sea level rise under various scenarios by calculating a time to inundation (TTI) for each property. This is defined here as the property's elevation over local mean higher high water divided by local trends in relative sea level rise from the nearest NOAA water station plus some baseline aggregate trend affecting all areas:

$$TTI = \frac{Elevation}{BaselineTrend + LocalRelativeTrend}. \quad (2)$$

Under the assumption that homes cease to deliver rental flows as of the date at which they are forecast to be inundated at mean higher high water, we can then adjust the unaffected prices to what they would be in a world where RSLR is priced using the simple present value of an annuity. Specifically, the adjusted price is $P - Pe^{-iTTI}$ where i is the discount rate. If we assume rental growth keeps pace with inflation, then we should interpret i as a real discount rate. Alternatively, for any other nominal growth rate in rents g, we can easily reinterpret the assumed discount rate i as a growth adjusted discount rate, $r - g$, where r is a nominal rate.[20]

For any combination of i and the baseline trend in SLR, we can simulate new transaction prices and run regressions from Table 2 to recover the projected

[19] In the Appendix, we report several ancillary tests. Appendix Table A2 breaks down the time series of the price effect year by year from 2012 to 2017. Only 2014 and 2015 produce a positive interaction, but both are small in magnitude and not close to significantly different from zero, nor from the 1985–1995 period. Meanwhile, Table A3 replicates Column 4 of Table 2 but excluding observations with missing bedroom and bathroom information (Column 1) and any counties in which a federal levee is maintained (Column 2) (Army Corp of Engineers, nld.usace.army.mil). Of concern is that elevation may not be priced in protected areas. Instead, in subsamples of 558,000 and 561,000 observations, we find the main coefficients are largely unchanged from earlier tables.

[20] For census tracts that would experience negative RSLR under an assumed baseline trend, total RSLR is set to zero so that TTI is arbitrarily large and property values are unaffected.

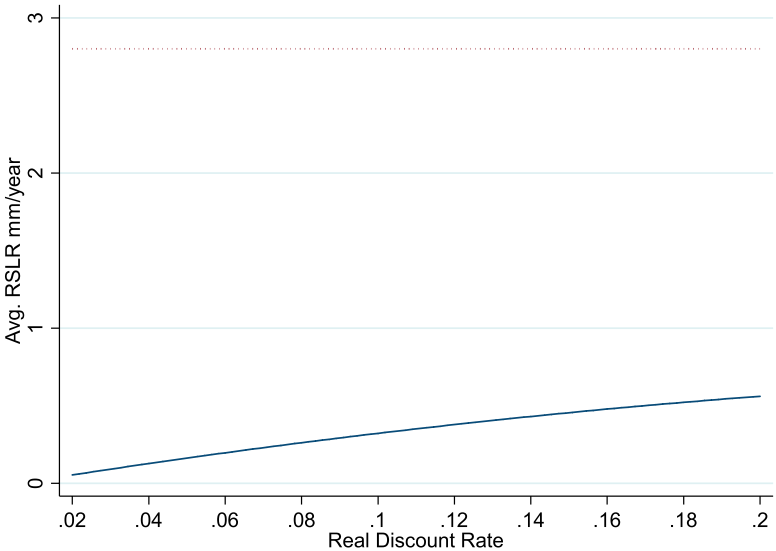

Figure 3
Bounding economic magnitudes
The axes plot the combination of real discount rate on housing cash flows and required forecasts of SLR per year that can generate an interaction coefficient of 0.0103 in the regression from Table 2, Column 4. The plot is generated by taking transactions within 1.68 km of the shore from 1985 to 1995 (to match the shoreline sample in Table 2), before SLR was plausibly priced. Prices are then adjusted for time to inundation, where the adjusted price is $P - Pe^{-iTTI}$, i is the discount rate, and TTI is the forecast inundation point for the property. For each discount rate, local RSLR affecting a property is defined as a baseline trend plus the local RLSR trend from NOAA. The y-axis is the average baseline plus local RSLR that delivers a coefficient of 0.0103 on the interaction between $ln(elevation) \times RSLR$. Meanwhile, for comparison, the dotted line plots the average RSLR experienced historically for the homes in the sample.

regression coefficients that should arise under that scenario. In the spirit of simulated method of moments, we can then match the simulated regression slopes to a range of actual coefficients.

Figure 3 reports the findings of this exercise for the upper bound of 0.0103 for the interaction coefficient in Column 4 of Table 2 based on a 95% confidence interval. The x-axis reports the chosen discount rate i. The y-axis displays the average baseline trend plus the local relative trend used in the denominator of Equation 2 and applied to individual properties that delivers a coefficient of 0.0103 for any given discount rate. As the graph shows, reasonable discount rates imply SLR scenarios that are modest to nonexistent, with an upper bound of 0.5 mm per year assuming discount rates of 20%. This compares to historical SLR trends of 2.8 mm per year for the same set of properties. These estimates suggest not only that accelerated predictions for SLR are not accounted for in market prices, but even observed historical trends in relative SLR do not appear to be capitalized, or are capitalized using improbably high discount rates.

Implicitly, the exercise assumes that rental flows from housing stop accruing as of the date a home is inundated at the higher high water mark—that is, the forecast point at which the home is flooded twice daily. This is admittedly an ad hoc assumption and perhaps an extreme one. At the same time, it places a lower bound on value effects—if abandonment occurs sooner, then the pace of RSLR consistent with the observed coefficient magnitudes can only be slower.[21]

Appendix Table A4 recasts the test from Table 2, this time assuming property desertion occurs upon reaching the 10% tidal exceedance levels from the same local water stations used to measure RSLR. A 10% tidal exceedance level represents the local threshold that storm tides will exceed once every 10 years on average. Exceedance levels are established at the census tract–level based on the nearest station reporting. Using the transformed elevation, we reestimate the regressions from Table 2 and again find no evidence of priced RSLR effects. More importantly though, we can again simulate the expected coefficient magnitudes as in Figure 3 and match these to the combination of discount rate and pace of RSLR. This time, because inundation occurs earlier, tiny levels of RSLR will be needed to match the observed coefficient; as an example, using a 10% rate at which to discount the lost cash flows due to flooding, an average RSLR of just 0.05 millimeters/year should generate a coefficient of 0.006 (the upper bound on the 95% confidence interval from Table A4) on the interaction coefficient on RSLR and elevation. In sum, accelerating the speed of abandonment should only make price effects of RSLR larger.

Of course, the assumption of a constant growth in cash flows in perpetuity ignores the finite lifespan of physical structures and the need for ongoing reinvestment, even in the absence of RSLR effects. While the rental flows embedded in the assumed valuation model should be interpreted as net of property maintenance costs, for simplicity, we abstract away from what those costs are and how they might depend on the effects of sea level rise, although this will obviously affect how damages appear in our simulations as well as in the data.

Finally, as noted in the introduction, our model ignores the potential of mitigation, which could protect cash flows even after the projected time to inundation. As a result, small price effects might be interpreted not as a belief in limited sea level rise, but instead as a belief in cheap mitigation. Hence, it may be reasonable to interpret the y-axis in Figure 3 as the pace of expected effective SLR after mitigation. Alternatively, McNamara and Keeler (2013), Bunten and Kahn (2014) and Bakkensen and Barrage (2017) all

[21] To see this, consider two homes *A* and *B* identical in all ways other than RSLR flood risk. Suppose that they sell for $43,295 and $200,000, respectively, in an economy with a 5% discount rate and that *B* will never flood. However, *A* is just 15 mm above the current high high water mark and will become uninhabitable at some point if RSLR continues apace. Based on *B*'s value, the two homes yield annual consumption dividends of $10,000. Given the discount rate, this implies that the market believes *A* will be abandoned in 5 years. Assuming, as this paper does, that homes are deserted when they become flush with the higher high water mark, a market belief the future rate of RSLR will be 3 mm per year (15/3 = 5) is consistent with the available data. However, suppose instead homes are abandoned when they are still 6 mm above the high high water mark. This leaves *A* just 9 mm above the abandonment level (15-6 = 9). However, because it actually sells for $43,295, it remains the case that the market must believe it has 5 years of useful life left. To generate a useful life of 5 years with just a 9 mm cushion, the market must believe in a rate of RSLR of just 2.2 mm per year.

conclude that given heterogenous beliefs and preferences, price effects related to inundation risk will be subtle. In these models, equilibrium prices and preferences cause like-minded populations to co-locate. This natural market sorting causes the marginal buyer of an at-risk property to also have a low risk assessment. The summary statistics presented in Table 1, Panel D appear consistent with sorting of different population types across protected and at-risk neighborhoods. It is also possible that inefficiencies in the real estate market produce prices that are not fully reflective of the existing fundamental information. Depending on the severity of inefficiencies, this information may be incorporated only with a considerable lag if at all. In short, the absence of overwhelming price effects related to SLR may reflect a skeptical marketplace or high discount rates, but it may also reflect a belief in affordable or publicly funded mitigation, heterogeneous beliefs, slow-moving prices, or all of the above.

2.3 Test power

In the presence of a null result, an obvious consideration is power. In a world where sea level rise is priced in the cross-section of houses, would we still fail to reject the null hypothesis that it is unpriced based on the size of the effect, sample size, and available variation in the data? Using simulations similar to those described above for intermediate forecasts of sea level rise and moderate discount rates, we show the tests in Table 2 should routinely reject the null.

Table 3 uses the early sample of transactions from 1985 to 1995 and adjusts prices as before based on discount rates of 8% and TTI implied by a baseline trend of 10 millimeters/year. As before, time to inundation is defined as elevation divided by a baseline trend plus local RSLR. The chosen baseline trend of 10 mm per year falls within NOAA's intermediate distribution of global mean sea level rise forecasts for the century (Sweet et al. 2017). Meanwhile, while real discount rates of 8% are high relative to the long-run discount rates on real estate recovered in Giglio, Maggiori, and Stroebel (2015), lower discount rates will only increase the size of the effect and, hence, test power. To replicate the smaller shoreline samples from Columns 3 and 4, we restrict the sample to the quintile of properties nearest to the coast. This again results in properties with a distance to shore of less than 1.68 km.

Column 1 replicates the regression from Table 2 on the simulated prices and recovers a highly significant magnitude of 0.019. Under the alternative hypothesis that RSLR is priced in the way specified here, the tests should easily reject the null using any standard test size. The ability to detect effects obviously will be more limited as the assumed pace of sea level rise slows. In Column 2, however, when we rerun the simulation under low forecasts of SLR (the historical pace of RSLR with no additional baseline trend), we report a coefficient of 0.009, significant at the 5% level. Finally, Column 3 pushes the simulation to its limit by retaining the lower forecasts for SLR and imposing a rounding error on home prices, which tend to be set at coarse increments

Table 3
Simulations

ln(Price + estimated value impact) (8% cap rate)	Intermediate trend (1)	Low trend (2)	Low trend/ coarse prices (3)
ln(Elevation over MHHW)	−0.004	−0.020*	−0.019*
	(0.011)	(0.011)	(0.011)
ln(Elevation over MHHW) x local RSLR trend	0.019***	0.009**	0.008*
	(0.004)	(0.004)	(0.004)
Relative local SLR trend	—	—	—
ln(Sq. feet)	0.461***	0.462***	0.463***
	(0.008)	(0.008)	(0.008)
ln(Land sq. ft)	0.090***	0.091***	0.090***
	(0.006)	(0.006)	(0.006)
ln(Distance to coast)	−0.092***	−0.086***	−0.086***
	(0.004)	(0.004)	(0.004)
Beachfront	0.108***	0.140***	0.141***
	(0.016)	(0.016)	(0.016)
Other Controls: Bed, Bath, Age, Flood Zone, Mobile	YES	YES	YES
Year, Census Tract Fixed Effects	YES	YES	YES
Observations	1,057,301	1,057,301	1,055,291
R^2	0.559	0.560	0.616

We replicate Table 2, Column 2, using adjusted prices to account for the hypothetical value effects of SLR on a sample of transactions from 1985 to 1995. Adjusted prices are $P-Pe^{-iTTI}$, where i is the discount rate set at 8% and TTI is the forecast inundation point for the property under a pace of 10 mm per year SLR plus the local historical RSLR trend. The chosen baseline trend of 10 mm per year falls within intermediate distribution of global mean sea level rise forecasts for the century. Column 2 reestimates prices under the low forecasts for SLR based on just the local historical RSLR trend. Column 3 rounds simulated prices from Column 2 to the nearest $5,000. Standard errors are clustered at the census tract–level, are robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1%, 5%, and 10% levels, respectively.

in practice. Specifically, after simulating the effects of RSLR, we then round prices to the nearest $5,000 before estimating our regressions.[22] Even here, we detect a significant coefficient estimate, albeit marginally so. In sum, while failure to detect RSLR price effects could still be a result of misspecification or the wrong population sample, it is unlikely to be due to ex ante low test power.

2.4 Robustness

Although the identification strategy is designed to generate unique predictions that are difficult to map into plausible alternative interpretations, it is not impossible to do so. If RSLR risk captured by the interaction of RSLR and elevation is correlated with priced omitted variables, our coefficient estimates will confound the true price effect of RSLR with the price effects of omitted factors. By examining a subset of sales where the variation in property characteristics that could covary with RSLR risk is limited, we can minimize these types of concerns. In that spirit, in Tables 4, 5, and 6, we explore the robustness of the prior result that potential sea level rise does not

22 Thanks to the referee for this suggestion.

currently affect real estate prices first, among a sample of unimproved vacant properties. Second, we employ property fixed effects to control for unobserved heterogeneity. Third and finally, we present an alternative experiment that employs properties proximate to the Great Lakes as a coastal control group for which SLR exposure is limited and invariant to elevation.

Table 4 focuses on a sample of properties without structures that were sold between 2012 and 2017. In principle, these vacant lots represent a relatively clean test of sea level rise effects because they isolate the value of land from that of improvements, many of which might be unlikely to survive the time frame needed for intermediate forecasts of SLR to affect the majority of our sample. This also eliminates concerns about misspecification due to unobserved characteristics associated with structures. For example, in a world where homeowners anticipate RSLR, exposed structures might be subject to underinvestment, affecting their prices indirectly.

Column 1 presents the baseline regression without an interaction between local RSLR and elevation. Because the properties are vacant, the relevant controls are distance to coast, beachfront, and land square footage in addition to census tract and year fixed effects. Here we see elevation is positively and significantly priced. When interacted with the speed of local sea level rise, however, the interaction is not significantly different from zero. And although the magnitude of the interaction appears larger than in the full sample, the much smaller sample prevents the difference from being statistically significant at the traditional levels.

Table 5 presents yet another alternative specification, this time controlling for fixed housing characteristics via property fixed effects. While vacant lots help control for unobservable structure characteristics, fixed effects control for unobservable land characteristics (as well as fixed structure characteristics). Meanwhile, because property fixed effects control for the interaction between RSLR and elevation (both are fixed at the property level), we can even absorb inadvertent regional correlations between the price of elevation and RSLR that are unrelated to sea level rise and hence would be expected to appear even in the pre-1995 control period.

For each property sold in the last three years of our sample (2015 to 2017) and within the shoreline sample from Columns 3 and 4 of Table 2—properties that are a maximum of 1.68 km from the shoreline—we look for sales of the same property before 1995 and have a construction age prior to 1995. (The age filter drops homes that were torn down and rebuilt between 1995 and 2015.) Examining the same set of properties over time, before and after widespread awareness of SLR risk (relative or otherwise), adds an additional dimension to our earlier tests: instead of comparing the value of elevation based across RSLR exposure, the inclusion of property fixed effects now allows us to see how the value of elevation in high RSLR areas has changed over time for the same set of properties as RSLR awareness has increased. Property fixed effects serve to

Table 4
Robustness to misspecification (vacant lots)

ln(Price)	(1)	(2)
ln(Elevation over MHHW)	0.090***	0.045
	(0.024)	(0.041)
ln(Elevation over MHHW) x local RSLR trend		0.017
		(0.011)
ln(Land sq. ft)	0.167***	0.167***
	(0.014)	(0.014)
ln(Distance to coast)	−0.178***	−0.178***
	(0.029)	(0.029)
Beachfront	0.381***	0.380***
	(0.086)	(0.086)
Year, Census Tract Fixed Effects	YES	YES
Observations	401,987	401,987
R^2	0.440	0.440

We replicate Columns 1 and 2 from Table 2 using a sample of vacant lots. Standard errors are clustered at the census tract–level, are robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1%, 5%, and 10% levels, respectively.

Table 5
Robustness to misspecification (property fixed effects)

ln(Price)	(1)	(2)	(3)
Elevation x Post (Year>2014)	0.046***		0.060***
	(0.007)		(0.027)
Local RSLR trend x Post (Year>2014)		−0.247***	−0.201***
		(0.015)	(0.030)
Local RSLR trend x Elevation x Post (Year>2014)			−0.027**
			(0.011)
Property Fixed Effects	YES	YES	YES
Year Fixed Effects	YES	YES	YES
Observations	646,672	660,258	646,672
R^2	0.778	0.782	0.781

We construct a panel of properties that sold at least once between 2015 and 2017 and at least once before 1995. We then estimate interactions between elevation, the local RSLR trend, and a post variable turned on for sales after 2014 in a model with property fixed effects. Standard errors are clustered at the census tract–level, are robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1%, 5%, and 10% levels, respectively.

remove property-invariant characteristics that might otherwise be mismeasured or unobserved, thereby mitigating concerns about misspecification.

After dropping singleton observations and requiring elevation and RSLR values, we end up with a sample of 694,402 observations on which to run property fixed effects regressions of log price on $POST \times ln(elevation)$, $POST \times RSLR$, and $POST \times ln(elevation) \times RSLR$, where $POST$ is defined transactions occurring in 2015–2017. Because property characteristics are fixed in the CoreLogic sample (as are elevation and distance to the coast, at least approximately over this time period), these are absorbed by house fixed effects.

Columns 1 and 2 of Table 5 present the interactions between $POST \times ln(elevation)$ and $POST \times RSLR$. Here, the results are more nuanced than those from the prior tests. For example, the positive and significant interaction between post and elevation in Column 1 shows that prices on higher elevation properties appreciated more from pre-1995 to post-2015 than those at lower elevations. While that might be interpreted as evidence of RSLR price effects, it is also consistent with housing preferences simply changing over time in a way that favors higher elevation properties or areas. Similarly, Column 2 demonstrates that prices have tended to rise less in areas experiencing higher RSLR. Again, while this geographic trend matches with what we might expect if RSLR was priced, it is also consistent with broader trends in housing markets that have benefited the West Coast relative to the southeast and Gulf Coast. However, the better identified triple difference specification in Column 3 calls into question any interpretation of the earlier columns as evidence of priced RSLR. The interaction between $POST \times ln(elevation) \times RSLR$ establishes the change in value of high-elevation properties relative to low-elevation properties in areas subject to the greatest level of sea level rise risk. The negative and significant coefficient suggests that properties most at risk of RSLR have, if anything, appreciated more relative to unexposed properties.

Finally, Table 6 presents an alternative experiment using properties in Illinois, Indiana, Michigan, Minnesota, Ohio, and Wisconsin that are within 30 km of Lake Superior, Lake Michigan, Lake Huron, or Lake Erie (because New York and Pennsylvania are included in the coastal sample, we have chosen to exclude properties in these states as controls). These properties are useful in that they allow us to estimate the value of elevation in a sample unrelated to the price impact of sea level rise (this does assume that land rebound and subsidence around the lakes does not pose a hazard, or if it does, a uniform one). By comparing the value of elevation in and around the Great Lakes to the value of elevation for coastal properties on the ocean, we can use the differential price effects of elevation to detect the effects of RSLR. Compared with the specifications in Table 2, rather than using the speed of relative sea level rise as a source of treatment, the treatment dummy is simply turned on for coastal properties in states bordering oceans. Because tidal variation in the Great Lakes is minuscule, elevation is measured relative to NAVD88 in the control group, but relative to MHHW in the treatment.

The level effect on elevation in Column 1 shows a positive price of elevation for properties near the water, but not directly at risk from the threat of rising sea levels; this serves as our baseline valuation for elevation in a world without sea level rise.[23] Yet when examining the interaction, rather than observing an even higher value for elevation in the at-risk properties, Table 6 suggests the opposite. Elevation appears less valuable exactly when it might serve as a hedge to RSLR. In Column 2, we limit the sample to the quintile of sales closest to either shore (lake or coast). Again, the insignificant but negative coefficient

[23] Most climate models actually predict long-term declines in lake levels due to accelerated evaporation, although the range of potential projections is large (National Oceanic and Atmospheric Administration 2018a)

Table 6
Great Lakes

ln(Price)	Full sample (1)	Shoreline sample (2)
ln(Elevation over MHHW)	0.379***	0.489
	(0.097)	(0.351)
ln(Elevation over MHHW) x Coastal	−0.383***	−0.486
	(0.097)	(0.351)
ln(Sq. feet)	0.549***	0.563***
	(0.005)	(0.008)
ln(Land sq. ft)	0.118***	0.130***
	(0.002)	(0.005)
ln(Distance to coast)	−0.117***	−0.131***
	(0.004)	(0.005)
Beachfront	0.205***	0.148***
	(0.021)	(0.021)
Other Controls: Bed, Bath, Age, Flood Zone, Mobile	YES	YES
Year, Census Tract Fixed Effects	YES	YES
Observations	5,313,069	1,016,736
R^2	0.645	0.623

We supplement the coastal sample from Table 2 with a set of properties in Illinois, Indiana, Michigan, Minnesota, Ohio, and Wisconsin that are within 30 km of Lake Superior, Lake Michigan, Lake Huron, or Lake Erie to serve as a control group—properties near a coast but not subject to direct SLR risk. Because tidal variation in the Great Lakes is minuscule, elevation is measured relative to NAVD88 in the control group, but relative to MHHW in the treatment. In Column 2, we limit the sample to the quintile of sales closest to either shore (lake or coast). Standard errors are clustered at the census tract–level, are robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1%, 5%, and 10% levels, respectively.

on the interaction between elevation and treatment appears inconsistent with the price patterns expected from a hedonic model incorporating RSLR risk. Of course, like the baseline model used in earlier tables, it is not impossible to construct examples of how proximity to the Great Lakes or regional variation in RSLR could be spuriously correlated with local prices of elevation or other characteristics—in either case this would affect our interpretation of the results. However, the consistent absence of an obvious price effect from exposure to sea level rise across different specifications and characterizations of risk exposure should provide some reassurance of the robustness of the null result.

3. Heterogeneity and Cross-Sectional Effects

Although not broadly detectable, price effects may be concentrated in parts of the real estate market or country—see, for example, Bernstein, Gustafson, and Lewis (forthcoming) and Baldauf, Garlappi, and Yannelis (2018). Of course, as we search across a potentially large set of local characteristics that might plausibly generate positive results, we should be wary of Type I errors that will inevitably occur.

In developing the list of local attributes that might mediate the pricing effects of sea level rise, we begin with a list of census level variables capturing basic population characteristics, all at the level of census tract—median age, the percentage of the population that graduated from high school, the percentage of the population with a bachelor's degree or higher, median and mean earnings, population and housing density (people or houses per square kilometer), mean household size, and the percentage of rented properties. We also consider both beliefs and political affiliation. For beliefs about climate change, we use the Yale Climate Opinion survey estimates of the percentage of a county's population that believes global warming is happening and is worried about it (Howe et al. 2015).[24] For political affiliation, we use a variable called *Democratic leaning* constructed using the partisan value index published by the Cook Political Report.[25] To create a census tract–level variable, we average across all districts within a census tract.

To see if some subset of prices in the real estate market reflects concerns over sea level rise, we estimate two-stage regressions at the county level. First, we estimate the model

$$ln(price)=\beta_i ln(elevation)+\gamma_i X+e \tag{3}$$

at the tract–level with controls for the natural logarithm of building and land square footage, number of bedrooms, bathrooms, effective age, distance from the beach, and dummy variables for flood zone and year of sale. The unit of observation is a housing transaction. We limit the sample to census tracts that include properties within 10 km of the shore.

In the second-stage regression, we project the price elasticity of elevation (β_i from (3)) on RSLR, local characteristics, and the interaction of the two. That is, we run the regression

$$\widehat{\beta_i}=\lambda_1 RSLR+\lambda_2 RSLR\times CENSUS+\lambda_3 CENSUS+v_i \tag{4}$$

where $CENSUS$ is one of the local census tract–level variables described in the previous paragraph. The λ_1 parameter determines if the value of elevation varies with the pace of relative sea level rise generally, as we might expect if RSLR is priced. The estimated parameter λ_2 establishes whether this is more or less true in tracts with characteristics captured by the various CENSUS variables (column names in the table indicate which particular census variable is included in a particular regression). Finally, λ_3 indicates if some census tract characteristics are associated with a higher value of elevation than others.

Table 7, Column 1, begins with just the level effect of RSLR to estimate λ_1. We again find no effect from regressing the local value of elevation

[24] This is the same survey used by follow Bernstein, Gustafson, and Lewis (forthcoming) and Baldauf, Garlappi, and Yannelis (2018) in their work.

[25] Cook reports a partisan index as +X R or +X D for leans Republican or Democratic by X%, respectively. This is recoded as a positive score for congressional districts leaning more Democratic, so that a +X R is recorded as -X.

measured by $\widehat{\beta}_{1,i}$ on RSLR. The magnitudes are similar to those observed in Table 2.

In Columns 2—13, we focus additionally on λ_2 and λ_3—the level effect of census tract variables and the interaction of RSLR with the same local characteristics. As a matter of interpretation, the interaction between the census variable—"worried," for the sake of example—and RSLR gives an indication of whether or not the value of elevation increases with relative sea level rise more in places that express worry about global warming (we find the interaction is economically and statistically very close to zero).

Immediately, notice that in a few cases, the level coefficients on the census variables (λ_3's) are economically small but statistically significant. Alone, these coefficients might indicate that at the mean level of RSLR, certain subgroups do price RSLR risk more than others. However, the λ_2 coefficients indicates something else is likely at play. Across the 12 chosen census tract characteristics, the interaction between RSLR and the census tract characteristics is insignificant. That is, when taking into account the variation in time to inundation due to relative SLR, price effects disappear. The null result for percent rented is particularly noteworthy. Although the λ_2 is again small and insignificant, the level effect of percent rented on the value of elevation (λ_3) is positive and highly significant, reproducing the findings of Bernstein, Gustafson, and Lewis (forthcoming). Column 14 shows this directly by including only the level effect of percent rented on the right-hand side. That is, consistent with prior work, we find that elevation is more valuable in areas with low rates of owner occupancy. This correlation might indicate that sophisticated real estate investors price RSLR exposure. But, it is difficult to reject a simpler interpretation of this fact—that preferences for housing characteristics like elevation covary with other local characteristics like percent rented. Meanwhile, given that land subsidence and rebound are significant determinants of exposure to SLR risk (and are less obviously related to preferences than elevation), if areas with high rates of rental occupancy are more likely to price RSLR effects, we would expect both a positive coefficient on the interaction and/or a positive and significant effect of RSLR for extreme values of percent rented. Yet neither appears evident. Even setting percent rented at the maximum of its support (100%), the effects of local RSLR (λ_1+100$\times\lambda_2$) remain insignificant.

Table 7 should also allay some concerns that the null results on RSLR and housing prices are the result of some omitted demographic variable. For example, wealthy populations are more likely to value elevation and be concerned about global warming. If so, then one would have found significant results for the columns related to wealth or a belief in climate change. But in neither case is there any indication that wealthier communities place a higher value on elevation due to RSLR.

Table 7
Cross-sectional heterogeneity

		Census Variables												
β(ELEVATION)	(1)	Median Age (2)	% HS Grad (3)	% Bachelor (4)	Median Earnings (5)	Mean Earnings (6)	Pop. Density (7)	Housing Density (8)	Household Size (Mean) (9)	Happening (10)	Worried (11)	Dem. Leaning (12)	% Rentals (13)	% Rentals (14)
local RSLR	0.006	0.452	−0.131	−0.011	−0.075	−0.085	−0.010	−0.010	0.005	0.237	−0.000	−0.003	−0.036	
	(0.013)	(0.316)	(0.164)	(0.036)	(0.529)	(0.534)	(0.049)	(0.048)	(0.098)	(0.210)	(0.131)	(0.013)	(0.034)	
Census × local RSLR		−0.121	0.002	0.000	0.007	0.008	0.003	0.003	0.000	−0.003	−0.000	−0.001	0.001	
		(0.084)	(0.002)	(0.001)	(0.050)	(0.047)	(0.008)	(0.009)	(0.037)	(0.003)	(0.002)	(0.001)	(0.001)	
Census		−0.022	0.000	−0.000	−0.034	−0.030	0.009	0.012	−0.037	−0.007**	−0.003	−0.002**	0.002**	0.002**
		(0.090)	(0.002)	(0.001)	(0.051)	(0.043)	(0.009)	(0.010)	(0.042)	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)
Observations	5,676	5,676	5,676	5,676	5,668	5,676	5,643	5,652	5,676	5,676	5,676	5,668	5,676	5,676
R^2	0.000	0.001	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.001	0.000	0.001	0.002	0.001

We estimate two-stage regressions, beginning with a census tract–level regression of $ln(price) = \beta_i ln(elevation) + \gamma_i X + e$ where X includes natural logarithm of building and land square footage, number of bedrooms, bathrooms, effective age, distance from the beach, and dummy variables for flood zone and year of sale and i indexes the census tract. In the second stage, we regress β_i on RSLR, local census variables, and the interaction of the two. We report the estimates from the second stage. Standard errors are robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1%, 5%, and 10% levels, respectively.

4. Discussion and Conclusion

Given the prior work presenting evidence of price discounts for properties at risk of current or future inundation, how should we interpret the absence of detectable price effects in this study? On one hand, Bernstein, Gustafson, and Lewis (forthcoming), Stroebel et al. (2018) and Keenan, Hill, and Gumber (2018) use different data, in terms of house prices, characteristics, and risk. Keenan, Hill, and Gumber (2018) only use housing from Miami-Dade county as a case study, whereas our identification depends on variation across cities, so it is less surprising our findings would differ substantially. Bernstein, Gustafson, and Lewis (forthcoming), Baldauf, Garlappi, and Yannelis (2018), and (Stroebel et al., 2018) use comprehensive coastal housing data provided directly by Zillow, in contrast to our data purchased from CoreLogic. But in both cases, the primary source of the data is the same—deed and tax records. Thus, it seems unlikely that differences in housing data explain the contrasting findings.

A more likely explanation lies in the way those papers and this one measure future inundation risk. In contrast to the papers above, we emphasize the important role of vertical land movement as a major contributor to SLR risk. Meanwhile, while raw elevation is a clear and first-order determinant of SLR risk, it also introduces confounding variation in housing amenities that cross-sectional variation in RSLR allows us to avoid. Thus, one possible explanation for our different findings relative to Baldauf, Garlappi, and Yannelis (2018) and Bernstein, Gustafson, and Lewis (forthcoming) is related to the confounding effects of hedonic preference for elevation. Moreover, our risk measures avoid variation in exposure implied by projections derived from models of local hydrological connectivity. To ensure that the results are not due to classification errors that these models may correct, we conduct a number of robustness tests. In all cases, no evidence is found for the hypothesis that RSLR is priced.

Correlations reported in our summary statistics may also help shed light on differences in findings. Table 1, Panel D, suggests that people who live in places experiencing higher local relative sea level rise and at low elevation have lower levels of education, have lower earnings, and importantly, are less likely to believe in or be worried about global warming. These correlations are also particularly intriguing in light of the results in Baldauf, Garlappi, and Yannelis (2018). Based on a model of homophily, which might suggest exactly the type of geographic sorting we observe, Baldauf, Garlappi, and Yannelis (2018) estimate a difference-in-difference regression measuring the price effects of properties exposed to SLR in geographic areas with differing levels of belief and concern about climate risk, as measured by the *happening* and *worried* variables from the Yale Climate Opinion survey. They find that SLR risks appear to be priced in areas professing concern over climate risk ("believers") but not in areas populated by "deniers." While this might appear inconsistent with the null result regarding RSLR pricing we report, the negative correlation between our measure of risk exposures (RSLR and elevation) and concern over climate risk nicely reconciles these two facts: if exposed populations do not believe in SLR and populations that do believe are actually relatively unexposed, a model that measures the priced effects of actual risk exposure like ours (as opposed to perceived risk exposure) should find no effects on average.

Finally, the absence of pricing effects in our study could be due to a limited understanding of RSLR risk by the market; for example, if the market views property elevation as a sufficient statistic for SLR exposure and misses the substantial cross-sectional component of risk, we would fail to detect price effects. In other words, we cannot reject a market that prices RSLR risk using a misspecified model (or at least one that is inconsistent with consensus forecasts). In the same way, we cannot reject homebuyers with sophisticated expectations of RSLR risk but who believe mitigation efforts will be largely successful.

All of the papers mentioned above suffer, our own included, from the complaint that they depend on within-geography variation to varying degrees. By comparing at-risk properties with protected properties within the same area, we control for local fixed effects in prices and risk, but we also ignore RSLR pricing that occurs between cities and not within them. Thus, if agents discount all properties in a city due to the risk faced by some properties there—perhaps because of spillovers as neighbors abandon properties or the loss of local amenities due to RSLR—we will miss these potentially important effects. While further work remains, our findings at a minimum suggest substantial ambiguity as to whether or not sea level rise is already affecting the market for residential real estate, at least as of 2017.

Appendix

Table A1
Census tract characteristics

Variable	Min.	5%	25%	50%	75%	95%	Max.
Population	158	1,830	3,067	4,123	5,385	7,408	33,041
Housing units	324	836	1,360	1,852	2,426	3,464	14,754
Total area	0.100	0.31	1.37	3.19	9.74	83.49	4,182
Water area	0	0.00	0.00	0.05	0.91	15.59	674.38
Land area	0.100	0.31	1.29	2.80	7.69	64.26	3,995
Pop. density	0.540	62.90	550.08	1,430	2,969	10,224	35,506
Housing density	0.420	32.34	262.47	653.79	1,276	3,987	13,161

The cells report percentile break points for several housing and population characteristics by census tract. Area and density are per square kilometer within a census tract. For example, the median census tract contains 653.79 housing units per square kilometer.

Table A2
Time series of effects

ln(Price)	1985-1995 (6)	2012 (1)	2013 (2)	2014 (3)	2015 (4)	2016-2017 (5)
ln(Elevation over MHHW)	0.003	0.007	−0.004	−0.010	−0.010	0.000
	(0.008)	(0.010)	(0.011)	(0.010)	(0.010)	(0.009)
ln(Elevation over MHHW) x local RSLR trend	0.002	−0.005	−0.001	0.001	0.004	−0.000
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
Local RSLR trend	—	—	—	—	—	—
ln(Sq. feet)	0.420***	0.595***	0.614***	0.588***	0.570***	0.507***
	(0.004)	(0.009)	(0.008)	(0.008)	(0.008)	(0.010)
ln(Land sq. ft)	0.085***	0.125***	0.110***	0.114***	0.116***	0.118***
	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
ln(Distance to coast)	−0.081***	−0.128***	−0.119***	−0.119***	−0.118***	−0.110***
	(0.004)	(0.006)	(0.005)	(0.005)	(0.005)	(0.004)
Beachfront	0.184***	0.162***	0.133***	0.150***	0.164***	0.171***
	(0.017)	(0.029)	(0.026)	(0.027)	(0.022)	(0.017)
Other Controls: Bed, Bath, Age, Flood Zone, Mobile	YES	YES	YES	YES	YES	YES
Year, Census Tract Fixed Effects	YES	YES	YES	YES	YES	YES
Observations	5,865,709	680,544	751,305	762,721	815,876	1,279,923
R^2	0.553	0.543	0.570	0.551	0.562	0.667

Table 2, Column 2, is reestimated for various time periods. Standard errors are clustered at the census tract–level, are robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1%, 5%, and 10% levels, respectively.

Table A3
Excluding missing bedrooms/bathrooms, leveed areas

ln(Price)	Non-missing bed/bath (1)	Excl. leveed areas (2)
ln(Elevation over MHHW)	0.012	−0.032
	(0.013)	(0.023)
ln(Elevation over MHHW) x local RSLR trend	−0.004	0.010
	(0.005)	(0.007)
Relative local SLR trend		–
ln(Sq. feet)	0.572***	0.573***
	(0.010)	(0.011)
ln(Land sq. ft)	0.124***	0.113***
	(0.006)	(0.006)
ln(Distance to coast)	−0.140***	−0.131***
	(0.005)	(0.005)
Beachfront	0.084***	0.112***
	(0.017)	(0.018)
Other Controls: Bed, Bath, Age, Flood Zone, Mobile	YES	YES
Year, Census Tract Fixed Effects	YES	YES
Observations	558,161	561,309
R^2	0.626	0.556

Table 2, Column 4 is reestimated excluding (i) observations with missing bedroom or bathroom information in Column 1 and (ii) counties that contain federal levees in Column 2. Standard errors are clustered at the census tract–level, are robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1%, 5%, and 10% levels, respectively.

Table A4
Tidal exceedance thresholds

ln(Price)	Full Sample (1)	Shoreline Sample (2)
ln(Elevation over 10% exceedence level)	0.002	0.015
	(0.008)	(0.015)
ln(Elevation over 10% exceedence level) x local RSLR trend	0.001	−0.002
	(0.003)	(0.004)
Relative local SLR trend	—	—
ln(Sq. feet)	0.564***	0.570***
	(0.007)	(0.009)
ln(Land sq. ft)	0.115***	0.125***
	(0.003)	(0.005)
ln(Distance to coast)	−0.119***	−0.137***
	(0.005)	(0.005)
Beachfront	0.185***	0.115***
	(0.021)	(0.020)
Other Controls: Bed, Bath, Age, Flood Zone, Mobile	YES	YES
Year, Census Tract Fixed Effects	YES	YES
Observations	4,042,226	700,304
R^2	0.566	0.616

Table 2, Columns 2 and 4 are reestimated using elevation relative to local 10% tidal exceedance levels. 10% tidal exceedance levels represent the local thresholds that storm tides—those produced by astronomical tide, the storm surge, and wave formation—will exceed once every 10 years on average. Exceedance levels are established at the census tract–level based on the nearest station reporting. Standard errors are clustered at the census tract–level, are robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1%, 5%, and 10% levels, respectively.

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