UNIVERSITAT DE
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Essays on the Political Economy of Urbanization and Climate Change

Pierre Magontier

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2020

PhD in Economics | **Pierre Magontier**

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PhD in Economics

Essays on the Political Economy of Urbanization and Climate Change

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"I was placed halfway between poverty and the sun. Poverty kept me from thinking all was well under the sun and in history. The sun taught me that history was not everything. I wanted to change lives, yes, but not this world which I worshipped as divine."

– Albert Camus – *L'Envers et L'Endroit*, 1937

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This thesis is dedicated to her.

Barcelona, May 16th, 2020.

Contents

Chapter 1

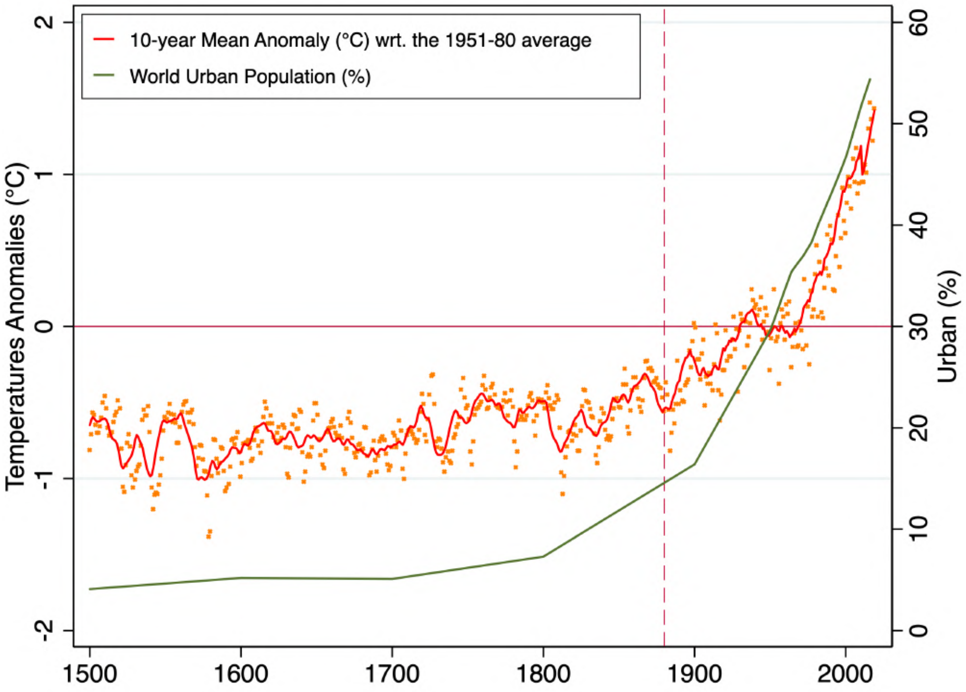
Introduction

Comprehending how spectacularly fast, communities agglomerated to shape our current cities is challenging. In the early 1900s, only 15% of the world population lived in urban areas. By the end of the 20th century, the combined effect of industrial revolutions, the rise of globalized interactions, and the development of transportation, led modern societies to experience the fastest urban growth ever witnessed in the entire human history. Within a four-generations time, the urban population had grown more than in the last thousands of years. In 2007, there were, for the first time in history, as many individuals living in rural spaces than in urban areas. There were more than 4.1 billion urbanites in 2018. According to the United-Nations projections, they should represent as much as two-thirds of the world population by 2050.

These extraordinary changes in settlement and production habits accompanied – if not, caused – no less drastic changes in climate patterns. According to the Intergovernmental Panel on Climate Change (Bernstein et al., 2008), temperatures rose more in the last fifty years than any other half-century period over the last half-millennium. The consequences of increased temperature anomalies are of gradual and sudden nature. On the one hand, the widespread melting of snow and ice surfaces across the globe, particularly in Greenland and Antarctica, leads to the general elevation of sea levels. On the other hand, higher variations in temperatures generate rapid air pressure differentials, which ultimately translate into more frequent and more intense storm phenomena, hurricanes, and floods.

Figure 1.1 summarizes these unprecedented patterns. While both the urban population and land temperatures remained relatively unchanged until the early 19th century, the 20th century displayed a significant boom

Figure 1.1: **500 Years of Urbanization and Climate Change**



Note: (i) Temperatures are systematically recorded since 1880 (dashed vertical bar). Before that date, temperature data needs to be reconstructed. (ii) Temperatures data was extracted from Moberg, Sonechkin, Holmgren, Datsenko, and Karlén (2005) and the Global Land-Ocean Temperature Index, for before and after 1880, respectively. Baseline period was normalized to the 1951-1980 average for both series. (iii) The historical urban population series were extracted from the website Our World in Data.

in both aspects. The ensuing question is: how to approach agglomeration economies in the 21st century – as the world experiences the consequences of climate change?

Despite well-known congestion costs, cities have always been attractive to both workers and firms because of the opportunities they provide (Glaeser and Gottlieb, 2009; Combes, Duranton, Gobillon, Puga, and Roux, 2012; Albouy, 2016; de la Roca and Puga, 2017; Duranton and Puga, 2020): wage stability, enhanced productivity, higher educational prospects, and so on. With climate change, cities are even more engaging because urban production inputs are not as vulnerable as their rural counterparts (Deschênes and Greenstone, 2007; Fisher, Hanemann, Roberts, and Schlenker, 2012). In particular, economic agents located in agricultural areas have a private incentive to diversify their sources of income and move to urban spaces when they are at the mercy of natural disasters (Todaro, 1969; Harris and Todaro, 1970; Barrett, Reardon, and Webb, 2001; Wouterse and

Taylor, 2008; Hornbeck, 2012).

While capital and human concentration lower personal vulnerability to natural disasters and foster recovery rates, such density can also reinforce global exposure to natural hazards. The societal, economic costs of natural disasters can reach a record high in densely populated areas. This is the case, for example, of coastal settlements. A 10% share of the world population currently lives in these spaces, which are less than 10 meters above sea level. Although these regions are favored for both their economic opportunities and environmental amenities, they are especially threatened by the consequences of climate change. Hallegatte, Green, Nicholls, and Corfee-Morlot (2013) estimate that in the absence of protection upgrades, flood-related losses among the world's largest coastal cities could reach as much as US$1 trillion per year by 2050.

Consequently, there is a trade-off for the 21st-century societies between the benefits from agglomeration and exposure to disasters. Both are essentially a function of urban development, and not internalizing climate risks when making development decisions might, eventually, lead to inefficiently oversized or unprepared cities.

This dissertation aims to document the role of local policymakers on these land-use decisions in regions subject to increased risks of natural disasters. As mentioned, such areas generally benefit from both amenities and economic advantages valued by societies (Glaeser, Kolko, and Saiz, 2001; Rappaport, 2008), while being exposed to environmental stress (sea-level rise, floods, storms, etc.). This setting fosters political failures as local governments might decide to overdevelop or to under-protect jurisdictions facing these potential hazards. These behaviors can occur because policymakers neither fear an electoral sanction nor a co-partisan punishment or because special interest groups capture them.

What political mechanisms could drive overdevelopment in environmentally stressed areas? The second chapter of this thesis, *'The Political Economy of Coastal Destruction*[1]*'*, focuses on Spain's massive coastal urbanization. Indeed, on average, 2.2 hectares of lands have been converted *every day* between 1987 and 2005 within Spain's first 500-meter fringe. Rather moderated rates of conversion would be expected from a social planner in the presence of such a public good. Nonetheless, in Spain, most of the responsibility for urban development falls onto politically fragmented

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[1] Co-authored with Albert Solé-Ollé and Elisabet Viladecans-Marsal

local governments who plan for future land use. That is, politically heterogeneous municipalities share development responsibilities within a given coastal segment. Urban development provides both costs (e.g., destruction of open space) and benefits (e.g., jobs) to both residents and non-residents. It follows that overdevelopment might occur if municipalities deciding in isolation fail to account for the non-residents' welfare. In the absence of a social planner, can political cooperation alleviate coastal overdevelopment?

To answer this question, we define a dummy for horizontal political alignment (i.e., one if the municipality is ruled by the same party or coalition that is ruling a majority of the municipalities in the same coastal segment). Political cooperation is likely to be fostered by political homophily (i.e., the similarity of the political traits of two jurisdictions). With this horizontal alignment dummy, we conduct a fuzzy close-elections regression discontinuity design adapted for the Spanish proportional vote system, using as a dependent variable the amount of land located close to the coastline that has been converted during a given year. We carry out our analysis on an extensive dataset of 423 Spanish coastal municipalities (including the Canary and Balearic Islands), during nine municipal terms between 1979 and 2014. Our dependent variable is built based on the combined Landsat IV imagery and census data from the Global Human Settlement Project and the CORINE Land Cover of the European Commission and the European Space Agency. Our covariates include partisan, economic, residential, climate, and topographic data, as well as accessibility measures.

First, we find that land conversion is significantly less important in jurisdictions whose local government is held by a similar party or the same coalition than its neighbors. Within the 1-kilometer fringe, a 'horizontally aligned' municipality would develop, on average, approximately 63% less land than a similar unaligned one. This result suggests that politically aligned local governments engage in inter-municipal cooperation over land development in the presence of coastal amenities, leading to lower rates of land conversion. Second, it appears that this effect decreases with distance to the coast. We motivate this result by arguing that cooperation is less and less likely to occur as coastal amenities vanish.

Even in the absence of overdevelopment, why do local governments not systematically prepare for natural disasters? In the third chapter of my thesis, *'Does Media Coverage Affect Government's Preparation for Natural Disasters?'*, I explore the incentives for local policymakers to prepare for these threats through the adoption of mitigation measures (e.g., storm shelters, floodwalls, infrastructures elevation, retrofitting, etc.). This study makes the central assumption that governments' mitigation initiatives both reduce and *signal* the hidden dangers of natural disasters in a given location. Because of this risk-signaling process, local governments who seek to protect property values in their jurisdiction are reluctant to take mitigation measures when (non-resident) potential investors are ignorant of the risks. When information about a location's exposure circulates, the trade-off between risk reduction and risk disclosure vanishes. Local governments are then encouraged to invest in mitigation to reassure these prospective investors.

I test this idea by creating an exogenous measure of newspaper coverage of storms. Indeed, the number of articles published about a natural disaster is endogenous to many political outcomes, including mitigation initiatives. To build my treatment, I first collect a unique dataset on newspapers' circulation at the ZIP code level in the United-States. This dataset, provided by the Alliance for Audited Media, includes the number of copies sold, both print and digital, by more than 2400 different newspapers between 2010 and 2018. Thanks to detailed NOAA data on storms' location and occurrence, I can precisely know the share of each newspaper's readership that experienced a storm in a given year. This will be my storm coverage measure. The rationale behind this approach is that, because of their editorial constraints, newspapers are more likely to report first about what hits its readership. I check this by scrapping articles published specifically about storms from more than 400 newspapers from the website *Newslibrary.com.* I show that my treatment is indeed a good predictor of the number of articles published about storms. The identifying assumption is that within a small geographical area (a county), the exact match between storms' spatial extents and a newspaper's readership is as good as random. To measure local governments' mitigation efforts, I collect data on FEMA's Hazard Mitigation Grant Program. This program has several advantages. In particular, it is one of the few only available to local governments' administrations, it is not as competitive as the other mitigation programs proposed by FEMA, and federal fundings largely finance it. To test whether local governments engage in more mitigation efforts when risk awareness is high, I compare, when local governments are eligible to this program, the number of mitigation projects in areas where my coverage measure is high to otherwise similar areas where my coverage measure is low.

I find that conditional on being hit by a storm, a one standard deviation increase in my treatment leads to a 54% increase in the mean number of mitigation projects. In the absence of any information shock, communities do not invest in mitigation technologies. My results are driven by mitigation infrastructures rather than non-structural actions, which underlines the signaling effect of mitigation projects. I interpret these results as indicative that local governments strategically underinvest in mitigation to avoid revealing the latent risk of storms in their jurisdiction to investors who would have remained otherwise uninformed. Additionally, I present some evidence suggesting that these results are primarily caused by non-resident investors rather than by homevoters trying to protect their property value. These facts bring up new insights as well as further questions about the potential capture of local disaster preparation policies by developers and property investors.

Finally, if local governments might overdevelop cities or do not protect them for future hazards, the remaining question with respect to climate change is: do individuals learn lessons from past disasters? I address this question in the fourth and last main chapter of this dissertation: *'The Dynamics of Land Development around Flood Zones[2]'*. In this paper, we wish to unravel the patterns of land conversion in the aftermath of an inundation in Spain. We show that, early on in history, individuals reckoned flood zones both as areas of risks and areas of potential economic opportunities and, later, environmental amenities. As a consequence, new development tends to cluster right outside the flood zone, where access to water is maximized while perceived risks are minimized. In Spain, one building out of ten locates in the first 100 meters outside these areas. The flood zone, while being an ad-hoc representation of the local dangers, serves as a focal point to identify risks of inundation. If individuals do learn from disasters history, then one should expect them to adapt by building less close to these designated hazardous spaces when a flood occurs, and displace land conversion farther away, or on higher ground.

To test the existence of such patterns, we exploit a unique dataset including all of the 12 million buildings of the Spanish territory, detailed flood zone and elevation maps, and historical flood records, spanning from 1900

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[2] Co-authored with Rodrigo Martínez Mazza

until 2010. Our construction data comes from the Spanish land register and includes building characteristics, such as their volume, floors, construction, and renovation dates. We complete this dataset with population records from the historical censuses. We rely on a flexible event-study framework to nonparametrically estimate the effect of historical floods on the relative change in land conversion at the municipal level and different distances from the nearest flood zone. The identifying assumption is that conditional on a municipality fixed effects – in particular, its geography, and yearly trends, whether or not a community is flooded in a particular year is random. The absence of pre-trends in the decade preceding the flood event confirms the unanticipated impact of the catastrophes.

We find that new surface built drops by a substantial -14.64% in the year following a flood with respect to the year preceding it, and peaks down to -26.7% in the sixth year after the event. This result is primarily influenced by municipalities having suffered at least another flood in the previous years. The post-1986 era, i.e., after the central government adopted a legal framework to regulate development around flood zones, also appears to drive most of this finding. The flood hazard's impact is, on average, persistent over more than 30 years in the flooded municipality. However, new buildings are neither developed farther away from the nearest floodplain nor on higher terrain. New structures' location is similar to what it was before the disaster. Several possible mechanisms could be consistent with our results. We speculate that a misinterpretation of local flooding probabilities caused by an availability bias, or an aversion to loss of amenities could explain this puzzle.

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Chapter 2

The Political Economy of Coastal Destruction

2.1 Introduction

Coastal spaces provide both economic opportunities and environmental amenities. An excessive amount of development might even affect the capacity of the coast to keep delivering economic benefits. At the same time, however, coastal development erodes the environmental value of the coastline. Overbuilding close to shore spoils coastal landscapes by shrinking forests, dunes, wetlands, and beaches. This affects the beauty of the landscape but also reduces bio-diversity and increases flood and wildfire risks. This situation is currently worsening due to climate change and rising sea levels (Greenpeace, 2018).

Both governments and international organizations are issuing recommendations on how to manage coastal development in the face of climate change (e.g., European Commission, 2009). At some point, all these reports mention the need to design a better governance system. Yet, there is not much research about the effect of different institutions on outcomes of such policies. In this paper, we focus on an important institutional feature, the degree of inter-governmental cooperation. Some papers have focused on the political economy of climate change (Campa, 2018; Pattachini, Paserman, and Gagliarducci, 2019), but just a few have studied the effect of decentralization (Burgess, Hansen, Olken, Potapov, and Sieber, 2012; Lipscomb and Mobarak, 2016). None has yet focused on coastal preservation.

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[*] This chapter was co-authored with Albert Solé-Ollé and Elisabet Viladecasn-Marsal.

In this paper, we study the role of cooperation among local governments in the preservation of coastal land from development in Spain. Keeping coastal land undeveloped may provide both benefits and costs to residents and to non-residents living outside the political jurisdiction. Local governments deciding in isolation about this issue will not take into consideration the welfare of non-residents and may end up either over-developing (if job-creation spillovers dominate those generated from open space preservation) or under-developing the coast (when preservation spillovers dominate). This suggests cooperation between coastal local governments on land use and other development policies might be welfare enhancing. In this paper, we investigate whether this is the case by estimating the effect of political alignment among mayors of neighboring municipalities on the amount of land developed close to shore. We expect mayors belonging to the same party (or coalition of parties) to have more incentives to cooperate than mayors of different parties (or coalitions). Politically aligned mayors have similar preferences, might have more opportunities to engage in policy conversations, are bound by internal party discipline or by coalition agreements, and may share the same electoral fate. All of this suggests that they might trust each other more than it is the case with unaligned mayors.

To study this question, we rely on high-resolution satellite images of the Spanish coast spanning over four decades. The data sources used to measure coastal development are the *'Global Human Settlement Layer'* Project, and the *'CORINE Land Cover'* Project. To identify the effects of political alignment, we rely on a close-elections Regression Discontinuity Design. This latter approach has been previously used by Durante and Guiterrez (2015) to study the effect of cooperation in crime prevention between Mexican's local governments. To account for the specificities of the Spanish Proportional Electoral system, we follow the method recently proposed by (Curto-Grau, Solé-Ollé, and Sorribas-Navarro, 2018).

We find that development close to shore is much lower in municipalities where a mayor winning the local election by a thin *Vote margin* belongs to the same party (or coalition of parties) that rules a majority of its neighbors than when this is not the case. The same qualitative result is obtained when using Panel Fixed effects, albeit the size of the effect is smaller. These results remain after performing many robustness checks. We also show that they cannot be explained by the ideology of the mayor or by political alignment with the regional government. The average result suggests that local governments deciding in isolation may not be accounting for the benefits that preservation provides to non-residents and that they are over-developing the coast. Additionally, we find that the extent of over-development is greater where undeveloped land is scarce and more environmentally valuable, and lower in places with high unemployment. These heterogeneous results are consistent with anecdotal evidence regarding the main drivers of coastal development in Spain (preserving the environment versus providing jobs).

The paper is related to various strands of the literature. First, several papers are looking at the effect of inter-jurisdictional cooperation on policy outcomes. In addition to the already-mentioned paper by Durante and Guiterrez (2015), which studies horizontal cooperation, Dell (2015) looks at the effects of vertical cooperation in the fight against organized crime in Mexico. Second, papers are looking at the effects of local government fragmentation and of decentralization reforms. For example, Hoxby (2000) looks at the effect of the number of schools in a given area on educational outcomes, using as an instrument the number of water streams. Galiani, Gertler, and Schargrodsky (2008) and Salinas and Solé-Ollé (2018) study the effect of education decentralization reforms in Argentina and Spain, respectively. On topics closer to the one we deal with here, Burgess, Hansen, Olken, Potapov, and Sieber (2012) and Lipscomb and Mobarak (2016) look at the impact of decentralization on deforestation in Indonesia and on river pollution in Brazil. Both papers find evidence of positive spillovers and suggest decentralization might have been detrimental.

The paper also contributes to the literature on local land-use regulations. For example, Fischel, Hale, and Hale (2008) studies the role of jurisdictional fragmentation on land use decisions, while Helsley and Strange (1995) and Brueckner (1998, 2003) show that cities deciding in isolation on 'urban growth controls' do not take into account the externalities they impose on each other. Suburban local governments might be constraining too much residential development in their jurisdiction, creating a housing affordability problem in the whole metro area and, ultimately, harming its growth prospects. The same logic can be applied to a system of cities in a country (Hsieh and Moretti, 2019). The idea is going to be very similar in our paper, except for the type of externality relevant in the case of non-residential development along the coast. While most of the literature

– which focuses on urban areas – looks at positive externalities and undersupply problems, we mostly focus on negative externalities and oversupply.

Finally, our work is also related to some recent papers trying to evaluate the impact of tourism on economic development and on environmental amenities. For example, Faber and Gaubert (2019) find that tourism along the Mexican coast had a positive effect on inland areas through its impact on manufacturing, thus suggesting there are positive geographical spillovers related to job creation. This paper does not consider the impact on coastal amenities. The paper by Hilber and Schöni (2016) evaluates the effect of a Swiss ban on secondary residences. The paper finds a detrimental effect of the ban on housing prices, which they interpret as evidence the local development's negative effects dominate over the positive effects of amenity preservation.

The paper is organized as follows. In the next section, we describe the process of coastal development in Spain during recent decades and provide institutional context to our study. In section three, we set up a simple theoretical model that we use to guide the interpretation of the empirical results. In section four, we describe the empirical methodology. Section five presents the results. The last section concludes.

2.2 Coastal Development in Spain

The Spanish coast experienced a development boom starting at the beginning of the 1960's after the Franco regime decided to open the country to tourism and foreign investment. These years are known as the *'desarrollismo'* period, a concept that means development was the only priority, and that its collateral effects in terms of destruction of open space and loss of cultural character were sidelined.

The destruction of the Spanish coast kept more or less the same pace after the arrival of democracy. Decades of tourist development have left its mark on the Spanish coast. In Figure A.1 in the Appendices, we show aerial photos from 1956 and 2012 of two examples of extreme development. The photos show a completely undeveloped stripe of white sand and of farmland in 1956, both completely developed as of 2012. Nowadays, the Spanish coastline is heavily developed: 36.5% of the shore is urbanized, and this number rises to 74.3% in Valencia or to 100% in the city of Marbella. When one looks at the 1-kilometer fringe, 15% of all land is already developed, and this number is as high as a 23% in Valencia and 26% in Catalunya (data from the 'CORINE' Land Use Cover). Today, development is still growing at a fast pace, as illustrated in the Appendices by Figure A.2 for the period 1979-2011. Between 1987 and 2005, an area equivalent to two soccer fields was developed per day.

The consequences of development on coastal amenities are varied (Greenpeace, 2018). Development alters coastal landscapes by shrinking forests, dunes, wetlands, and of the beaches themselves. This affects the beauty of the landscape but also reduces bio-diversity and increases flood and forest fire risks. Some of these risks are increasingly difficult to manage in the face of climate change, leading to hotter and drier summers, and rising sea levels. It also increases pollution and exhaustion of water resources and generates congestion reducing the quality of amenity consumption. Moreover, most of the effects are not reversible: once a coastal site has been destroyed, it is nearly impossible to bring it back. All these concerns have been gaining room in the Spanish debate on the convenience of preserving the remaining undeveloped coastal land[1]. However, economic benefits also appear prominently in the discussion. For example, in one recent conflict regarding the construction of a huge hotel in a protected area, the mayor of the town insisted on the jobs generated and on the high unemployment rate in the town[2].

In this paper, we are interested in advancing our knowledge about the institutional determinants of coastal development. The main players in this field in Spain are the local governments. The local landscape in Spain is highly fragmented: there are more than 8,000 municipalities, 423 of them located on the coast. Municipalities are responsible for land use regulations (subject to a regional regulatory framework) and provide traditional local public services funded by a mix of taxes and intergovernmental transfers (Solé-Ollé and Viladecans-Marsal, 2012). In 1979, the first democratic

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[1] This is evidenced by the rise in the number of conflicts between local environmental groups and local governments with development plans. See, for example: "A new platform is born to protect Costa Brava from new construction" in La Vanguardia 4/8/2018.

[2] See "The mayor of ... in favor of opening 'El Algarrobico' because 'it will bring jobs' ", in El Mundo, 11/10/2011; in the text, the mayor mentions the very high unemployment rate in the town. 'El Algarrobico' is a huge hotel already built in Cabo de Gata, a protected national park in the Coast of Almería, whose opening has been paralyzed by judicial intervention but that it is still pending a definitive decision. See also "The Partido Popular in Baleares justifies a hotel in a virgin beach because of job creation" in El País 4/3/2012.

Spanish local governments faced a scarcity of tax revenues and an economic crisis with soaring unemployment levels. Improving public services and providing jobs was, in many cases, the key to win local elections. In dealing with these hard situations, local governments use to make decisions in isolation without considering the effects on the rest of municipalities. For example, residents may enjoy visiting beaches in adjacent towns, and may also value the mere preservation of undeveloped coastland therein. Coastal development in one town may also both increase congestion in adjacent towns (e.g., more tourists staying there visit adjacent towns), and may harm the quality of the environment. In particular, construction may cause erosion of the beach of neighboring jurisdiction, and development may harm adjacent wetlands and aquifers, or even foster the spreads wildfires[3]. Finally, hotel over-supply may reduce prices and tax revenues in adjacent towns.

There are several ways of dealing with these problems. . First, the central and regional governments can intervene in protecting parts of the coast from development, set up a regulatory framework that helps minimize spillovers, or provide public goods. In Spain, the central government is responsible for protecting the coast, although it has been quite slow and ineffective. The first real attempt at preserving the coast did not arrive until 1988 with the 'Ley de Costas', enacted under a left-wing government. Even in this case, the law was imperfectly enforced[4] and years later a right-wing government turned down some of its precepts. The regional governments (*Comunidades Autonomas*) provide the legal framework for land use regulations, can ban some developments deemed unsustainable, and are responsible for enforcing the central legislation protecting the coast. As for the central government, it is not clear that regional governments have been able to adequately protect the coast.

Second, absent an active intervention by a higher layer of government, the localities might decide to fix these spillovers by cooperating voluntarily.

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[3] For example, the building of a port might create a barrier to the transport of sand along the coast, shrinking the beach of a nearby town. The canalization of a river bed might reduce the number of sediments that reach the sea and help erode surrounding beaches.

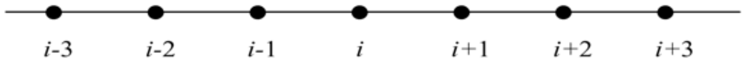
[4] See for example an article in El País in 2005 stating that 'the lack of demarcation of land close to shore prevents the punishment for the occupation of public land in 30% of the coastline' (see La Ley de Costas de 1988 sigue sin aplicarse en 3.000 kilómetros de playa, El País 14/11/2005). Another article in the same newspaper says that 'The 'Ley de Costas' has been unable to stop coastal deterioration' (see The destruction of Spanish beaches, El País, 10/08/2010)

They might choose to establish a voluntary association of municipalities (a *Mancomunidad*) to reach specific contractual deals (*Convenios*), or to coordinate their zoning and infrastructure policies. The main problem with this voluntary cooperation is that it is hard for the different members to commit to respecting the agreement. There is anecdotal evidence that voluntary agreements are more easily abandoned where the towns involved are ruled by different parties[5].

2.3 Theoretical Framework

2.3.1 Basic Setup

Consider a beach town i located in a linear coastal area and surrounded by neighboring beach towns i and $-i$ (i.e., ..., $i-3$, $i-2$, $i-1$,...,$i+1$, $i+2$, $i+3$...):



The indirect utility of a representative voter of this beach town can be expressed as $V(A_i, Y_i)$, where A represents coastal amenities and Y economic development benefits. The utility function has the usual properties: $V_A \geq 0$, $V_Y \geq 0$, $V_{AA} \leq 0$, and $V_{YY} \leq 0$.

These two arguments of the utility function represent the two main issues that appear in the debate regarding the convenience of keeping the coast undeveloped in Spain (see the previous section). The utility provided by coastal amenities might account for the benefits derived from direct access to an undeveloped coast (both in the municipality and in the coastal area) or by the option or existence value associated to the preservation of land close to shore. We can think of economic benefits as jobs, but also as higher salaries and business opportunities, or as higher local tax revenues. We will assume that keeping the coast undeveloped reduces the amount of

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[5] See, for example, two excerpts from local newspapers: 'Political clashes and partisanship blur the workings of voluntary associations (mancomunidades) in the district', in La Opinión de Malága, 19/ 09/2009, and 'A particularly difficult case is the voluntary association of ... where viability issues add up to the open conflict between municipalities with different political affiliation', in La Información, 23/02/2016.

these economic benefits. L_i is the amount of land available for development in town i to start with:

$$L_i = U_i + D_i \tag{2.1}$$

where U_i and D_i stand for the land the local government will decide to keep undeveloped, and the the land it will decide to develop, respectively. The amount of land available for development in a neighboring town $-i$ can be expressed similarly:

$$L_{-i} = U_{-i} + D_{-i} \tag{2.2}$$

We introduce environmental spillovers by assuming that coastal amenities in i depend on the amount of land kept undeveloped in both i and -i:

$$A_i = U_i + \theta U_{-i} \tag{2.3}$$

where $\theta \in [0, 1]$ is a parameter measuring the strength of this type of spillovers. Using (1) and (2) to find the expression for U_i and U_{-i} and substituting in (3), we get:

$$A_i = L_i + \theta L_{-i} - D_i - \theta D_{-i} \tag{2.4}$$

This expression says that the development decisions of governments i and $-i$ reduce amenities enjoyed by representative voter in i. In the previous section, we provided examples justifying the fact that $\theta > 0$: residents in i may use amenities in $-i$ and development activity in $-i$ may have adverse consequences on the coastal environment in i. Note, however, that we are assuming that $\theta < 1$, which means that the local effects are stronger than the ones coming from neighboring towns. The reason for that is that residents' use of coastal amenities rises with accessibility and that the harm on the coastal environment decays with distance.

The following expression introduces economic development spillovers:

$$Y_i = D_i + \delta D_{-i} \tag{2.5}$$

where $\delta \in [0, 1]$ is a parameter measuring the strength of this kind of spillover. This expression tells us that development in $-i$ does have an effect over job opportunities (or any other type of economic benefit) in i. It might be that residents in i might end up working in the construction or in the hotel industry in $-i$ or that jobs in the manufacturing sector in i increases due to development in $-i$. We assume, however, that job opportunities in i are more affected by development in i than by development in $-i$. Individuals are more likely to accept a job or have more information on job opportunities in their hometown than in more distant places.

2.3.2 Non-cooperative equilibrium

In order to focus on inefficiencies derived exclusively from failure to cooperate, we assume that local governments are benevolent and aim at maximizing the utility of the representative voter. We also assume that the indirect utility function is $V(A_i, Y_i) \equiv A_i^{\alpha} Y_i^{1-\alpha}$. Substituting (4) and (5) into this expression, we get:

$$V(A_i, Y_i) \equiv [L_i + \theta L_{-i} - D_i - \theta D_{-i}]^{\alpha}[D_i + \delta D_{-i}]^{1-\alpha} \tag{2.6}$$

where $\alpha \in [0, 1]$. The local government i maximizes 2.6 by choosing D_i assuming that the decision of local governments $-i$ are all set (i.e., behaving Nash). After obtaining the F.O.C. and rearranging the equations, we get the reaction functions for i and $-i$:

$$\begin{cases} d_i^* = \beta - \gamma d_{-i}^* \\ d_{-i}^* = \beta - \gamma d_i^* \end{cases} \tag{2.7}$$

where $d_i^* = D_i^*/L_i$, $d_{-i}^* = D_{-i}^*/L_{-i}$, $\beta = (1-\alpha)(1+\theta)$ and $\gamma = (1-\alpha)\theta + \alpha\delta$. Expression 2.7 suggests that development in i and $-i$ are strategic substitutes. When a local government in $-i$ increases the portion of developed land d_{-i}^* by one unit, the local government in i reacts by reducing the portion of developed land d_i^* by γ units.

We now focus on the symmetric equilibrium, where i and $-i$ are equal. The portion of land developed in a non-cooperative (symmetric) Nash equilibrium is:

$$d_N^* = \beta/(1+\gamma) \tag{2.8}$$

Note that the stronger the environmental spillovers (θ), and the smaller the economic benefit spillovers (δ), the higher will be the non-cooperative level of development.

2.3.3 The effect of cooperation

In the cooperative equilibrium, a benevolent government chooses the amount of development in the whole area at the same time. The solution can be found by maximizing:

$$W(A,Y) \equiv [(1+\theta)(L-D)]^{\alpha}[(1+\delta)D]^{1-\alpha} \tag{2.9}$$

with respect to D. After obtaining the F.O.C. and rearranging, the portion of land developed in the cooperative equilibrium is simply:

$$d_C^* = 1-\alpha \tag{2.10}$$

Note that the cooperative level of development depends on the weight of economic benefits relative to amenities in the utility function, but does not depend on spillovers' strength. We can now compare the non-cooperative and the cooperative solutions:

$$\Gamma = d_N^* - d_C^* = \lambda(\theta-\delta) \tag{2.11}$$

where $\lambda = \alpha(1-\alpha)/(1+\gamma)$. The following proposition summarizes the results.

Proposition 2.1 *When $\delta = 0$, $\Gamma = d_N^* - d_C^* > 0$, and non-cooperative decision-making generates over-development. When $\theta = 0$, $\Gamma = d_N^* - d_C^* < 0$, and non-cooperative decision-making generates under-development. When both $\theta > 0$ and $\delta > 0$, the sign of Γ is equal to that of $\theta - \delta$. The relative degree of over-development increases with θ and decreases with δ.*

This Proposition helps us to derive the main hypothesis to test in the paper, which says that local governments that able to cooperate with each other will choose a different amount of development than those deciding in isolation. Whether lack of cooperation results in over- or in under-development depends on whether isolated decision-making fails to account for the positive amenity spillovers linked to the preservation of undeveloped coastal land, or for negative spillovers linked to the adverse effects that preservation might have on the economy of neighboring towns. In fact, by estimating heterogeneous effects, one might be able to test some additional hypotheses related to the nature of spillovers. Note that the treatment of inter-jurisdictional spillovers is extremely simple in our model: undeveloped land in $-i$ and i are substitutes and the degree of substitutability is constant.

However, the substitutability of the effects of development in $-i$ and i might be stronger in some situations. For instance, one might speculate that θ is higher when the amount of undeveloped land to start with is larger, when land is more environmentally valuable, or when land is closer to shore. The idea is that the representative voter in i starts caring about development in the neighborhood when he perceives this undeveloped land close to shore irreversibly disappearing, especially when it has a high environmental value. Similarly, we can also speculate that δ will be larger when the level of unemployment is very high. The idea here is that it is an economic hardship that forces people to consider taking jobs outside of the hometown.

2.3.4 Discussion

The model presented above is simple and focuses on purpose on the two main arguments found in the Spanish debate regarding the convenience of further coastal development. Here we discuss the consequences of two possible extensions.

First, in the model, we do not address the role of owners of undeveloped land, of developers, or of the hotel industry (Hilber and Robert-Nicoud, 2013; Solé-Ollé and Viladecans-Marsal, 2012). These agents presumably care about the profits they obtain from the development of additional plots of land close to shore. However, notice that the more land they develop, the more they would erode the coastal environment, and the lower will be the profit (and tax revenues) obtained from subsequent operations. Since environmental erosion also affects neighboring towns, reducing profits (and tax revenues) therein would generate additional positive spillovers from preservation. So, this mechanism also suggests that lack of cooperation might generate overdevelopment.

Second, voters in our model do not care about the value of their home, as is the case in models of residential land supply (Brueckner, 1995). We make this assumption because we are thinking of a small town, populated by immobile homeowner-voters who must decide on the amount of non-residential development. This development may happen far from the town center, and it does not necessarily affect the supply of residential housing. These voters will favor development if they think it will improve employment prospects or if they want to make profits selling land. Owners of vacation homes may care about house values, but, like other non-resident visitors, they do not vote. Of course, if these assumptions do not hold, home-value maximizing voters might constrain residential supply and push up prices in the whole coastal area, potentially harming the neighbors' economy. Note that this would be a story of negative development spillovers, suggesting as well that lack of cooperation might generate underdevelopment.

2.4 Empirical Design

Our empirical design is based on the idea that belonging to the same political party facilitates cooperation. We measure political alignment among neighbors (hereby called 'horizontal political alignment') as a situation where the mayor belongs to the same party that rules a majority of municipalities in the neighborhood. This approach allows us to use a Regression Discontinuity Design (RDD). In the rest of the section, we provide arguments in favor of using 'horizontal political alignment' as a proxy for cooperation and describe the implementation of the RDD.

2.4.1 Horizontal political alignment

There is a literature showing that parties do help internalize spillovers in federations. The works by Riker (1964); Filippov, Ordeshook, and Shvetsova (2004) and Wibbels (2006) suggest that centralized political parties that compete in all jurisdictions can be a solution to the underlying collective action problem affecting federations. The contention is that centralized political parties help coordinate behavior across governments. The studies by Rodden (2003) and Enikolopov and Zhuravskaya (2007) provide evidence that party centralization enhances fiscal discipline and the provision of other national public goods.

According to Wibbels (2006), there are several reasons why centralized parties may help to internalize spillovers. First, local officials can have incentives to cooperate if they have co-partisans at the regional or central level whose electoral success influences their own electoral chances. There is evidence of such coattails in several countries (Campbell, 1986; Samuels, 2003). Second, regional or national party leaders can discipline co-partisans at other levels of government. For example, in closed-list systems, regional and national party leaders decide who runs for higher office. Third, local co-partisans interact more often and expect to have to rely on mutual support for building future alliances (Persico, Pueblita, and Silverman, 2011).

This logic may extend beyond formal, strict party limits for different reasons. First, parties entering a coalition government are bound by the agreements they reached. Parties might want to police these agreements, making sure junior party members do not harm the arrangement. For example, coalition agreements reached at higher levels of government may dictate the identity of the coalition partner chosen at the local level Falcó-Gimeno and Verge (2013). Second, politicians joining in a coalition will probably have similar preferences and backgrounds regarding some topics. Third, sharing government responsibilities means that there will be many opportunities for members of the different parties to meet and exchange policy views, constituting an informal network: the exchanges of information among network members will facilitate the convergence of opinions (Algan, Dalvit, Do, Le Chapelain, and Zenou, 2019).

There is some evidence supporting the claim that political alignment facilitates cooperation among local governments. First, some papers show that local governments merge more often with other governments controlled by co-partisans (see Sørensen (2006); Bruns, Freier, and Schumann (2015) for Denmark and Germany, respectively), and others show that differences in political affiliation make voluntary agreements among local governments more improbable and more unstable (Feiock, 2007). There is abundant anecdotal evidence that this is actually the case in Spain (see section two). Second, there exists a literature providing evidence that 'political homophily' fosters cooperation (Gerber, Henry, and Lubell, 2013). This literature focuses on similarity in politicians' individual traits (e.g., gender, education, hometown, alma mater) or of their electorate. Focusing on membership into the same coalition or similarity in ideology seems a sensible option too.

2.4.2 The RD Design

Motivation — We want to study how coastal development in a municipality responds to 'horizontal political alignment.' A first approach would be to estimate this relationship by OLS, controlling for a set of observed covariates. If we have access to panel data, we might also control for different types of fixed effects. However, this approach might still be problematic if omitted development shocks also affect the probability of a municipality becoming 'horizontally aligned.' Imagine, for instance, that in a booming coastal area, the voters in several municipalities turn towards a party that they believe will facilitate (or deter) development. This will surely increase the number of 'horizontally aligned' municipalities in the area and suggests the treatment is not random.

This is why we rely on a close-elections Regression Discontinuity Design (RDD) for identification. Intuitively, the RDD compares municipalities where the party ruling a majority of municipalities in the neighboring area won the local election by a thin *Vote margin* and municipalities where the same party lost by an equally small *Vote margin*. Because in these two cases winning and losing is a matter of a small number of votes, the treatment is essentially random. For this reason, this identification method is considered the closest one to an experiment and has been recently used by economists and political scientists to study the effect of party identity (Lee, 2008; Pettersson-Lidbom, 2008; Ferreira and Gyourko, 2009; Gerber and Hopkins, 2011; Folke, 2014).

RDD in PR systems — Nevertheless, the fact that local councils are elected in Spain using party-list proportional representation (PR) precludes the use of a traditional RDD. In PR systems, voters can vote for one of many party lists, and these votes are transformed into seats in the local council using a specific conversion method (i.e., the d'Hondt method in Spain). After that, representatives on the city council elect the mayor. The first challenge posed by such an institutional setting is that sometimes no single party holds a majority of seats in the council, which means that the mayor has to be supported by a coalition of parties. The second challenge concerns the difficulties in identifying the vote threshold at which an additional vote switches a seat from one party to another (and, thus, from the coalition supporting the mayor to the opposition). Here, we follow the solution proposed recently for Spain by Curto-Grau, Solé-Ollé, and

Sorribas-Navarro (2018), which follows other studies that already adapted the close-elections RDD to a PR system for other countries (Folke, 2014; Ade and Freier, 2013; Fiva and Halse, 2016; Fiva, Folke, and Sørensen, 2018).

The solution comes in two steps. First, although in around a third of Spanish local governments, the mayor's party does not hold a majority of seats in the council, ideology is a compelling driver of the formation of the coalition of parties that support the mayor. This allows us to define our treatment as a situation in which the ideological bloc of the 'dominant' party (or coalition of parties) in the neighborhood (i.e., the party controlling a majority of municipalities in the neighborhood) has a majority of seats in the local council. The idea is that when parties on the left of the ideological spectrum have a majority of seats in a local council, it is highly likely that the mayor will also belong to the left-wing party bloc. In this case, if the 'dominant' party (or coalition) belongs to a left-wing (right-wing) party, then we can say that the mayor and the 'dominant' party are aligned (unaligned). The same applies when right-wing parties hold a majority of seats. This is exactly the procedure used in Fiva, Folke, and Sørensen (2018) and Fiva and Halse (2016). However, the fact that a small proportion of local parties can support both right- and left-wing parties means that the ideological factor will not always work, which justifies the use of a 'fuzzy' RDD as in Fiva and Halse (2016).

Second, even if the treatment in terms of the discontinuity of seats is relatively straightforward to define, elections won or lost by a difference of one seat are probably not that close in terms of the number of votes. In small municipalities, in particular, a high percentage of votes is needed to win one more seat. Thus, using the number or the percentage of seats as our forcing variable might not be appropriate Fiva, Folke, and Sørensen (2018). Instead, we use a forcing variable computed as the percentage of votes that the ideological bloc of the 'dominant' party or coalition in the neighborhood must lose (win) in order to lose (win) the majority of seats in the council. We first have to identify the last seat that was won by the majority bloc in the town. Then, we have to compute how many votes the parties in that bloc would have to lose for that seat to be transferred to a party in the opposition bloc. This computation is far from straightforward because whether a seat is allocated to one party or to another depends on the vote shares of all the votes cast at the same time (Fiva and Halse, 2016; Fiva, Folke, and Sørensen, 2018). We follow the procedure proposed by Curto-Grau, Solé-Ollé, and Sorribas-Navarro (2018) to calculate the number of votes that have to be subtracted from the 'dominant' party's ideological bloc for that bloc to lose its majority in the council[6].

Equation specification — The Regression Discontinuity Design (RDD) involves the estimation of a discontinuity in coastal development at the close-elections threshold. We use the following two-equation model:

$$d_{it} = \alpha H_{it} + g(v^0_{it}) + \epsilon_{it} \tag{2.12}$$

$$H_{it} = \gamma M_{it} + l(\nu^0_{it}) + \epsilon_{it} \qquad \forall\ \nu^0_{it} \in [-h; h] \tag{2.13}$$

where d_{it} is the amount of land close to the coast developed by local government i during the term-of-office t, and $H_{it} = 1$ if there is *Horizontal alignment* and zero otherwise. The variable ν^0_{it} is the percentage of votes that the parties belonging to the ideological bloc of the 'dominant' party (or coalition of parties) in the neighborhood would have to lose (if this party holds the mayoralty) or win (if the party is in the opposition) to lose (win) a majority of seats in the council and so lose (win) the control of the government. We refer to this variable as the *Vote margin.* With $M_{it} = 1$ we denote a situation where the *Vote margin* is positive (i.e., $M_{it} = 1$ if $\nu^0_{it} > 0$, and 0 otherwise). The terms $g(\nu^0_{it})$ and $l(\nu^0_{it})$ are polynomials in ν^0_{it}, fitted separately at each side of the threshold using the observations in a neighborhood around it, which we label h, hereby referred to as the bandwidth. Equation 2.12 is used to estimate the effect of *Horizontal alignment* on coastal development. Equation 2.13 is the first stage and estimates the discontinuity in *Horizontal alignment* that we use for identification. We estimate 2.12 by 2SLS, using M_{it} as an instrument for H_{it}. The estimates obtained can be interpreted as a Local Average Treatment Effect or LATE (Lee and Lemieux, 2010).

RDD validity — The validity of the RD design rests on certain assumptions that have to be tested. First, we document that there is a genuine

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[6] Our calculations are based on assumptions that we consider reasonable in the Spanish case. The results are robust to changes in these assumptions. We explain in detail how this procedure works in the next section and refer to the Online Appendix in Curto-Grau, Solé-Ollé, and Sorribas-Navarro (2018) for more information.

discontinuity in the probability of treatment. We show graphically that this is the case. The jump in the probability of treatment is lower than one, and this justifies the use of a 'fuzzy' design. Second, we show that the forcing variable used is continuous around the threshold by inspecting the histogram and using the formal test proposed by McCrary (2008). The continuity test provides a means for discarding the manipulation of the forcing variable. Third, we also test for the continuity of predetermined covariates to show that all factors, besides *Horizontal alignment*, that could potentially influence the coastal development are continuous at the threshold.

Estimation and inference — Our RD estimation uses a local polynomial with h equal to the optimal bandwidth, h*, computed as per Calonico, Cattaneo, and Titiunik (2014), and which minimizes the mean squared error. We also report in the main table the results for h*/2 and in a graph the results for additional divisors and multiples of h*. The finding that the treatment is also precisely estimated for lower bandwidths would reassure our findings. As a complementary analysis, we also report the Panel Fixed effect results. In this last case, we present the results with and without controlling for covariates. In all the cases, we control for Term and Region fixed effects[7]. Their inclusion is not strictly needed in an RDD since covariates should be balanced at the threshold, but improve the precision of the estimates. The standard errors are clustered at the coastal area level.

2.5 Data

Sample — We use data for all Spanish coastal municipalities (N=423) during nine terms-of-office, delimited by ten local elections: 1979, 1983, 1987, 1991, 1995, 1999, 2003, 2007, 2011 and 2015. This gives us a total of 3,807 elections to work with, although we lose a few of them due to data availability issues.

Land Use — Our dependent variable is the amount of land developed during a term-of-office relative to the amount of land undeveloped at the

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[7] Coastal areas are 102 well known coastal denominations, defined based on topography and historical treats. The boundaries do not coincide with any administrative boundary (see Table A.1 in the Appendices).

start of the term. We retrieve the amount of artificial land (developed) and the total amount of land under the local jurisdiction from two sources. The primary source we rely on is the GHSL ('Global Human Settlement Layer,' a joint project of the European Commission Joint Research Center and the European Space Agency). They use satellite images from the Landsat IV-VII database from NASA. The information is available for four cross-sections (1975, 1990, 2000, and 2014) and at a very high resolution (38-meter cells). To obtain a series for all the election-years, we mix this data with the information coming from the 'CORINE Land Cover Project' of the European Environmental Agency, which is available for four additional cross-sections (1987, 2005, 2009 and 2015). This data has a lower resolution (100-meter cells) but provides more detail on land use types. We combine the two data sources to construct a series of developed and undeveloped land for each election year. We complement these two databases with information on housing construction from the Census of 1991, 2001, and 2011.

Horizontal alignment — We measure developed and undeveloped land at various distances from the coast. We use both overlapping bands (less than 100 meters, less than 200 m., less than 500 m., less than 1 Km, less than 5 Km and less than 10Km), and non-overlapping bands (less than 100m., 100 to 200m, 200 to 300m, 300 to 400m, 400 to 500m, 500 to 1km, 1 to 5km, and 5 to 10km). For the main analysis will rely on the 1 Km fringe. There is a reason for that. The pressure over the coast does not come only from development just on the shore. In some zones, the shore was fully developed early, and construction was displaced inland. Rugged terrain also means that development also happens inland. Because of this, Greenpeace focused in this fringe in its initial reports (Greenpeace, 2010), although they more recently focused on both closer (100 m.) and more distant (5 and 10 Km) fringes (Greenpeace, 2018). We will also present results for all distance fringes mentioned since one of the hypotheses we want to test is whether the incentives to cooperate really increase for development projects closer close to the shore. The information on the votes and seats of all the parties running at the local elections, and about the party of the mayor comes from The Spanish Home Office ('*Ministerio del Interior*'). Using this data, we define whether a municipality is Horizontally aligned or not. First, we define this variable for several orders of neighbors (first order and up to fifth order). We present our main results for the first and the second order of neighbors together (up to second-order). This means that a neighborhood surrounding a local government will have four neighbors (two at each side). In this case, we look at whether either a single party holds the mayoralty in the majority of municipalities in the neighborhood (i.e., two or more in the second-order case). If this is the case, then we have a 'dominant' party. If this is not the case, then we look at whether the regional coalition does rule in the majority of towns. If this is the case, we will have a 'dominant' coalition. Still, in this case, most of the links between municipalities refer to individual parties. However, there is a proportion of links where the party ruling the two municipalities is different, but both participate in the regional coalition. In addition to *Horizontal alignment*, we will also use a measure of Vertical alignment with the regional government computed as a dummy equal to one if the mayor and the regional president belong to the same party. We account for Vertical alignment because one might argue that the effect of *Horizontal alignment* is actually due to this treatment being confounded with Vertical alignment. This is plausible since the more municipalities become 'horizontally aligned' in a region, the more probable this party is also controlling the regional government. Additionally, one may think that Vertical alignment moderates the effect of *Horizontal alignment* because regional incumbents may have more means to discipline co-partisan mayors than the opposition. We focus on alignment with the regional government because (as we explained in section two), this level of government also has relevant responsibilities on coastal development.

Forcing variable — The forcing variable is the *Vote margin*, computed as the votes needed for the ideological bloc of the 'dominant' party or coalition (the one ruling in a majority of municipalities) expressed as the percentage of total votes cast at the local election. To define the ideological blocs, we classify all parties standing at local elections in two groups: left and right (see the Online Appendix in Curto-Grau, Solé-Ollé, and Sorribas-Navarro (2018)). The parties are classified as left or right based on statutes or -when this information is not available- on their name (e.g., typical leftist names are: socialist, communist, green, progressive, etc.). Local parties (i.e., independents, civic lists, neighbors' associations, etc.) are difficult to classify. Note that this poses no problem for the measurement of *Horizontal* *alignment* or to identify the 'dominant' party or coalition: the few municipalities with local party mayors are always unaligned, because by definition no local parties are ruling in more than one town. None of them can be 'coalition-aligned' because they do not participate in regional coalitions. They have a minor impact on the computation of the forcing variable. We either include them in the right-wing bloc or exclude them from the calculus. The results are not affected by this choice. To compute the forcing variable, we use the exact algebraic formulation developed in Curto-Grau, Solé-Ollé, and Sorribas-Navarro (2018), which is based on the working of the d'Hondt method used to translate votes into seats in Spanish local elections. We compute the forcing variable under different vote migration scenarios. In our preferred measure (used in the main results), we assume that the votes taken away from the party holding the marginal seat are transferred only to abstention and not to the parties in the other ideological bloc[8]. We also assume that negative vote shocks simultaneously affect all the parties within the 'dominant' party's ideological bloc, so we subtract votes not just from the party holding the marginal seat but from all the parties in the bloc in proportion to the initial votes received by each party. Intuitively, the method works as if we were subtracting a small number of votes from one of the blocs, distributing them between the parties of that bloc according to their initial vote share, while keeping the votes of the other bloc constant. We stop subtracting votes when we observe a shift in the seat majority from one bloc to the other (i.e., when the last seat that was giving the majority to one bloc moves to the other bloc). The number of votes needed to reach this stage, divided by the total number of votes, is our forcing variable.

Covariates — We have assembled several covariates (see Table A.2 in the Appendices). These variables are used in the validity checks and also as interaction variables. Some of the variables are time-invariant, and others vary over time. Among the time-invariant covariates, we have the coast length, the beach length, the index of terrain ruggedness and the area of the municipality (all measured using the GHSL database), the number of rainy days and the average temperature (data from Agencia Estatal de Metereologìa), a dummy for the ocean or sea (Atlantic/Cantabric v.

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[8] We believe this assumption to be plausible in Spain given the importance of vote transfers from/to abstention during all these years.

the Mediterranean) and a dummy for the Islands (Balearic and Canary Islands). The time-varying information comes from the Census of 1981, 1991, 2001 and 2011, and refers to population and employment by education level and sector. This data is interpolated for the years between census. We obtain the unemployment data from the ‘Anuario Económico, La Caixa’ (‘Anuario del Mercado Espanol, Banesto’ for the 1980s). This data is available biannually, so no interpolation is needed. The political variables are computed with the local elections data provided by the Spanish Home Office (*‘Ministerio del Interior’*).

2.6 Results

2.6.1 Exploring the discontinuity

Figure 1 plots the *Horizontal alignment* status (H) against the *Vote margin* (V). When the *Vote margin* is positive (negative), it means that the ‘dominant’ party or coalition (i.e., the one ruling in a majority of municipalities in the neighborhood) has (has not) a majority of seats in the local council. We see that there is a large jump in the probability of being horizontally aligned at the threshold. *Vote margin* measures the distance from the threshold in terms of the percentage of votes necessary to lose (gain) the seats that guarantee a majority in the council. The value of the discontinuity in the first stage (i.e., the discontinuity in the probability of *Horizontal alignment*) is around 60 percent. The results do not depend at all on the bandwidth and hold separately for all the elections studied and for all the regions.

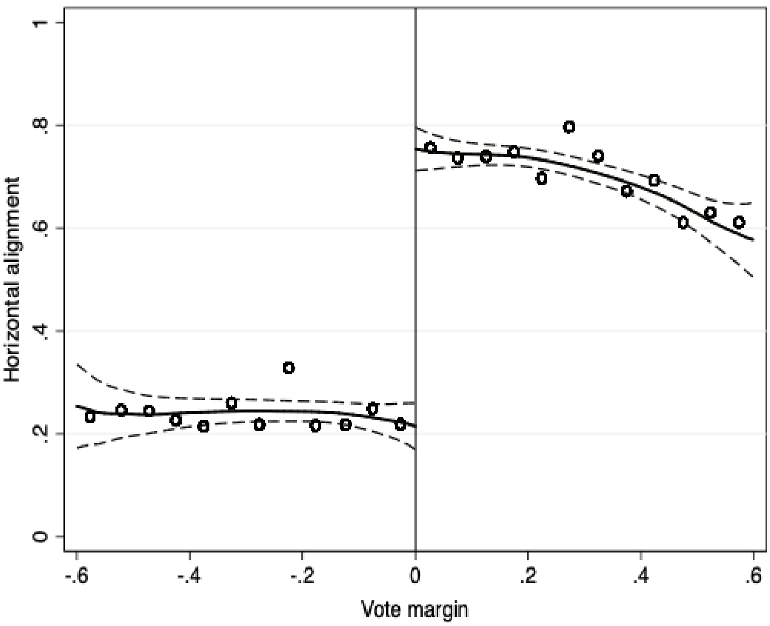
To test for manipulation, we examine the histogram and, more formally, we test for the continuity of this variable at the cut-off (see Figure 2). Both tests suggest that there is no evidence of manipulation.

Another validity check involves testing for the presence of a discontinuity in pre-determined covariates. In Table 2.1, we look at a large group of variables, and none of them seems to be discontinuous.

2.6.2 Horizontal alignment and coastal development

The discontinuity in coastal development around the cut-off is illustrated in Figure 2.3, which shows the plot between coastal development and the

Figure 2.1: First stage

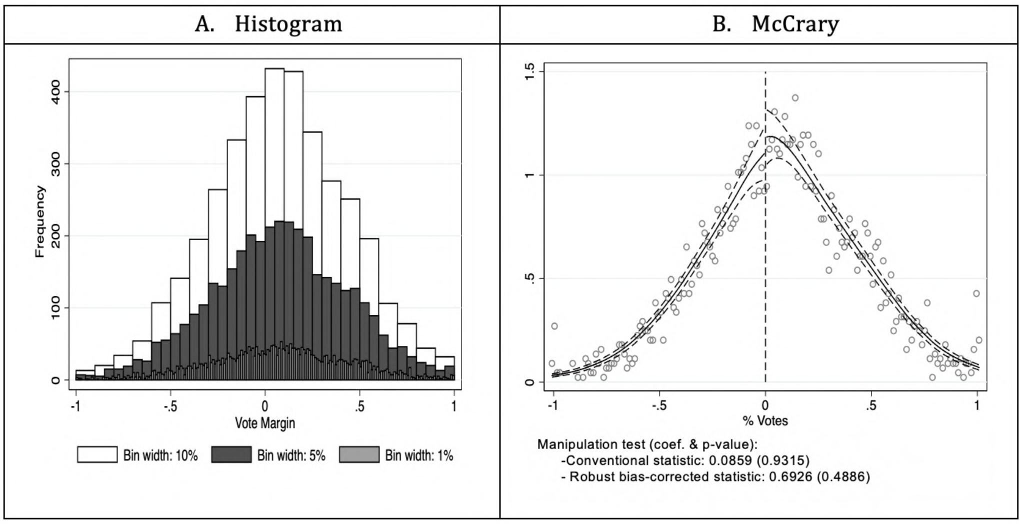


Note: (i) Elections: 1979, 1983, 1987, 1991, 1995, 1999, 2003, 2007 and 2011. (ii) *Horizontal alignment* (H) = dummy equal to one if the mayor belongs to the 'dominant' party (or coalition of parties), that is the one that controls a majority of municipalities in the neighboring area (in this case, neighboring area = up to second order neighbors, and majority = two or more municipalities). (iii) *Vote margin* (V) = change in votes (in % of total votes cast at the local elections) needed for the ideological bloc of the 'dominant' party (or coalition of parties) to move from having to not having a majority of seats in the local council. $M = 1$ indicates seats majority and $M = 0$ indicates seat minority, $V > 0$ indicates votes that the 'dominant' party bloc will have to lose while $V < 0$ indicates votes that the other bloc will have to gain. (iv) The dots are bin averages of 5 percent size. The solid line represents the predicted values of a local linear polynomial smoothing on each side of the threshold. The dashed lines are 95 percent confidence intervals.

forcing variable. The graph provides evidence of a clear and sizeable discontinuity around the threshold: municipalities marginally to the right of the cut-off (those that are more likely to be 'horizontally aligned') develop much less land than those marginally to the left (those more likely to be unaligned). The size of the reduced form effect (the 'intent-to-treat') is around 0.48. Municipalities where the ideological bloc of the party (or coalition) ruling in most nearby towns develop on average 28% less land than municipalities where this party does not hold a majority of seats in the local council.

Panel A of Table 2.2 presents the RD estimates, which correspond to the second stage of a 2SLS regression, where the dependent variable is the amount of coastal development. The optimal bandwidth h* is 25 percent, which is similar to other close-election studies (Meyersson, 2014). The 2SLS coefficient associated with the estimation of the local linear polyno-

Figure 2.2: Continuity of the forcing variable



A. Histogram

B. McCrary

Manipulation test (coef. & p-value):
-Conventional statistic: 0.0859 (0.9315)
- Robust bias-corrected statistic: 0.6926 (0.4886)

Note: (i) Dots for the McCrary graph: bin averages of the density of the forcing variables (*Vote margin*). Computed with McCrary's (2008) Stata program. (ii) Manipulation tests based on Calonico, Cattaneo, and Titiunik (2014).

mial with the optimal bandwidth (see column 1) is around -0.81 and is statistically significant at the 1% level. This quantity has to be compared with the level of development just at the left of the cut-off, which is 1.41. The 'horizontally aligned' municipality would develop, on average, approximately 57% less land than a similar unaligned one (-0.57=-0.81/1.41). The coefficients we get for h*/2 are a little smaller but also quite sizeable. The relative effect would be around 43% (-0.43=-0.61/1.42) and statistically significant at the 10% level. Figure A.4 in the Appendices shows that the local linear polynomial estimates are quite stable, provided we do not use a too large bandwidth. As we reduce bandwidth size, the estimates become noisier, but the effect is of a similar size.

The last two columns of Table 2.2 report the Panel Fixed Effects results, with and without covariates. The coefficient is much smaller than the RDD but is still statistically significant at the 1% level. The coefficient of -0.14 means that on average, a municipality whose mayor is aligned with the party (or coalition) ruling in the neighborhood will develop around a 10% less land than an unaligned one. As we already discussed, the discrepancy in the size of the RDD and the FE coefficients may be due to several reasons. First of all, *Horizontal alignment* might still be correlated with the error term even after controlling for the FE. For example, this could happen

Table 2.1: Covariates balance

	Coef.	P-value	Bandwidth	# Obs.
% Undeveloped land	-0.546	(0.712)	0.247	3.785
Coast length	0.028	(0.453)	0.281	3.785
Beach length	-0.086	(0.770)	0.199	3,785
Ruggedness	-1.774	(0.790)	0.162	3,785
# Rainy days	-0.009	(0.876)	0.315	3,633
Av. Temperature	-0.052	(0.421)	0.300	3,633
Mediterranean	-0.054	(0.210)	0.258	3.785
Islands	-0.042	(0.356)	0.262	3.785
log(Population)	0.024	(0.820)	0.263	3.785
log(Density)	-0.009	(0.943)	0.232	3.785
log(Area)	0.043	(0.679)	0.239	3.785
% Unemployed	-0.188	(0.652)	0.244	3.785
% No education	-0.006	(0.711)	0.243	2,358
% Primary education	0.002	(0.816)	0.249	2,358
% Secondary education	0.003	(0.921)	0.246	2,358
% Higher education	0.001	(0.415)	0.247	2,358
% Agriculture	0.003	(0.777)	0.234	3.785
% Industry	0.002	(0.840)	0.246	3.785
% Construction	-0.003	(0.266)	0.260	3.785
% Services	-0.001	(0.972)	0.315	3.785
Effective # of parties	0.017	(0.784)	0.263	3.785
% Electoral turnout	-0.002	(0.765)	0.245	3.785

Note: (i) Discontinuity of each covariate at the threshold, estimated with a local linear polynomial using the optimal bandwidth, calculated as per Calonico, Cattaneo, and Titiunik (2014) (indicated for each variable in column three).

if there are omitted unobservable shocks to the demand for development in a coastal area that influences party turnover in a similar direction in many towns (e.g., voters elect politicians that know will deal appropriately with the shock). If this is the case, then it is at least reassuring that our FE results point in the same direction as the RDD ones. Second, it could simply be that close elections are different from other elections. For example, in close elections, the possibility of losing office is much higher. The cost of catering to developers and not attending the demands of voters (for instance, to preserve the coast) may be higher too[9].

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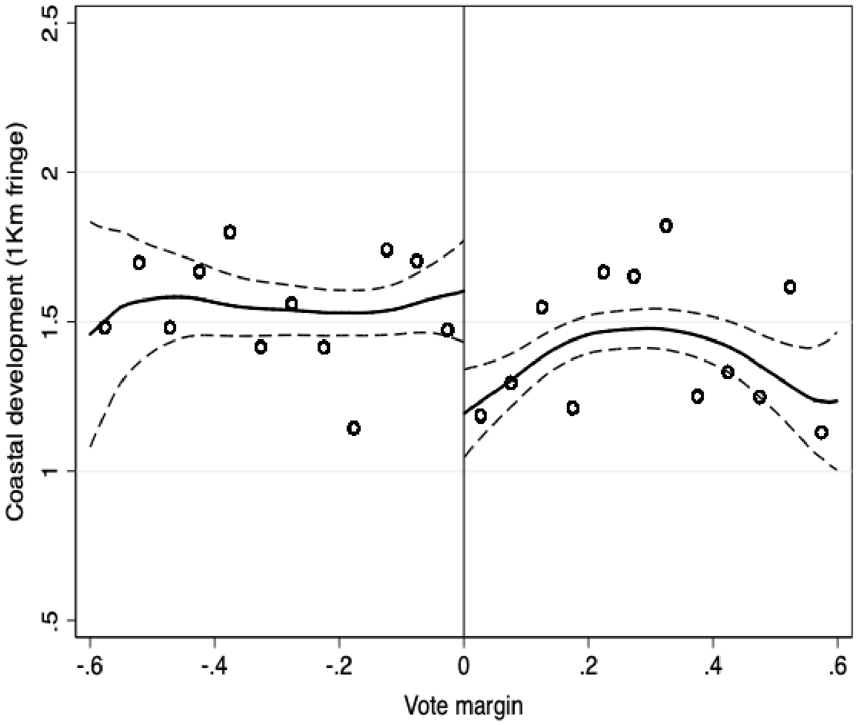
[9] See Solé-Ollé and Viladecans-Marsal (2012) for evidence of the effect of electoral competition (incumbent's party or coalition margin of victory) on local land-use policies.

Figure 2.3: Reduced Form



Note: (i) Coastal development = land developed during a term-of-office relative to undeveloped land at the start of the term. (ii) The size of the discontinuity corresponds to the Reduced form coeffi-cient which is equivalent to the ratio between the 2SLS and First-stage coefficients (expressions 2.12 and 2.13). (iii) See Figure 1.

2.6.3 Heterogenous Effects

Ideology and vertical alignment — We can be quite confident that the treatment (the ideological bloc of 'dominant' party or coalition having or not a majority of seats in the local council) is random. As we already showed in the previous section, all of the pre-treatment covariates analyzed are balanced at the threshold. However, a valid instrument should be ignorable but also should respect the exclusion restriction. That is, getting a majority of seats in the local council (M) should influence the outcome (d) only through its effects on *Horizontal alignment* (H). There are a couple reasons M could influence d through other channels. First, left-wing and right municipalities have a different propensity to be surrounded by aligned towns. This should not happen with the same number of right and left-wing municipalities and an even spatial distribution of both types. However, when working with real data, one cannot discard this possibility. Second, a similar thing could happen with vertical alignment (i.e.,

Table 2.2: Effect of *Horizontal alignment* (H) on Coastal development (d) – Period: 1979-2015

	RDD				FE	
	(1)	(2)	(3)	(4)	(5)	(6)
A. 2SLS or FE (Dep. variable: Coastal development, d)						
Horizontal alignment (H)	-0.813***	-0.867***	-0.602*	-0.608*	-0.147*	-0.143*
	(0.337)	(0.321)	(0.345)	(0.299)	(0.084)	(0.082)
B. First stage (Dep. variable: *Horizontal alignment*, H)						
Seat majority (M)	0.588***	0.589***	0.565***	0.564***	–	–
	(0.035)	(0.035)	(0.056)	(0.056)	–	–
F-Stat.	275.04	275.60	101.05	100.75	–	–
	[0.000]	[0.000]	[0.000]	[0.000]	–	–
Bandwidth (%)	h*=0.25		h*/2=0.125		100	
Year FE	Y	Y	Y	Y	Y	Y
Region FE	Y	Y	Y	Y	N	N
Controls	N	Y	N	Y	N	Y
Municipality FE	N	N	N	N	Y	Y
Observations	1.987	1.987	1.068	1.068	3.785	3.785

Note: (i) Elections: 1979, 1983, 1987, 1991, 1995, 1999, 2003, 2007 and 2011. (ii) RDD='Regression Discontinuity Design'. Columns 1-4 in Panel A show the second-stage estimated of the 2SLS where Seat Majority (equal to 1 if the ideological bloc of the'dominant party' in the neighborhood has a majority of seats in the local council) is used as an instrument for *Horizontal alignment* (equal to one if the mayor belongs to the'dominant party'). Panel B reports the first stage results. (iii) Columns 1-4 show the results when using a local linear polynomial with the optimal bandwidth (based on Calonico, Cattaneo, and Titiunik (2014)) and half the optimal bandwidth (columns 1-2 and 3-4, respectively). Columns 5-6 report the Panel Fixed Effects results. All the results are reported with and without control variables; controls variables include: log(Population), %Unemployed, %Undeveloped land, %Turnout, %Educated, and Left Mayor & Vertical Alignment dummies. (iv) Term and Region fixed effects included in columns 1-4; Time and Municipality fixed effects included in columns 5-6. (v) *, ** & ***: statistically significant at the 10, 5 & 1% levels; s.e. clustered at the Coastal area level (# clusters = 102).

the mayor belonging to party or coalition ruling at the regional level). In general, the party or coalition ruling at the regional level will also hold more mayoralties. Therefore, getting a seat majority might make a municipality aligned both horizontally and vertically at the same time[10]. We have checked both possibilities. We can totally discard the first one: the probability of having a Left-wing mayor is not affected by our instrument. This variable is clearly balanced at the threshold (see Panel A. in Figure A.5 in the Appendices). However, in the case of vertical alignment, there is a discontinuity at the threshold, albeit the jump in the probability of being vertically aligned is much smaller than the jump in the probability of be-

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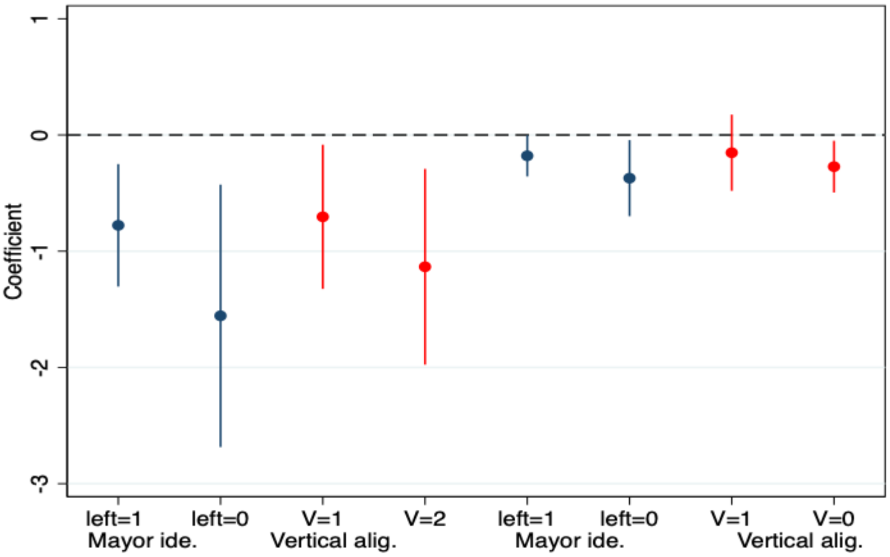
[10] This is a problem that plagues other close elections RD analysis (see, e.g., Albouy, 2014).

ing horizontally aligned. The discontinuity in Vertical alignment (V) at the threshold is 0.174, whereas it is about 0.6 for *Horizontal alignment* (H). We address this issue in different ways. First of all, we already included Vertical alignment in the set of controls used to estimate the results presented in Table 2.2. We also run some regressions using regression weights that correct the imbalance in the proportion of vertically aligned and unaligned mayors at each side of the threshold. This method has been recently proposed by Frölich and Huber (2019). The results are indistinguishable from the ones already presented and are available upon request. Second, we look at conditional effects, and we estimate the impact of *Horizontal alignment* separately for the municipalities that are and are not vertically aligned. In theory, it would be problematic if the effect of *Horizontal alignment* only appears in the sample of municipalities that are vertically aligned. This would suggest that our instrument is identifying the effect of V and not that of H, which is our variable of interest. These results are presented in Figure 2.4, which displays the H coefficient estimated separately for left and right-wing mayors and also for mayors than are vertically aligned ($V = 1$) or unaligned ($V = 0$) with the regional government. We present the results both for RDD and for the FE estimates. The first two lines show the effects of the ideology of the mayor. The RDD coefficient is clearly lower for left-wing mayors ($L = 1$) than for right-wing mayors ($L = 0$). The RDD coefficient is equal to -0.79 in the first case and to -1.45 in the second, which represent reductions of development due to horizontal cooperation of about 60% and 90%, respectively. Both effects are statistically different from zero, and the difference between the two is also statistically different from zero at the 5% level. Given that the treatment is really random in this case (recall Figure A.4), this is a heterogeneous effect. Left-wing parties in Spain are less pro-development than the right-wing ones (see, e.g., Solé-Ollé and Viladecans-Marsal (2012)). Horizontal cooperation might be less crucial to constraint development for left-wing than for right-wing governments because left-wing governments are already motivated to limit growth.

The following two lines show the effect of vertical alignment. The RDD shows that the impact of *Horizontal alignment* is smaller in the case of municipalities that are not aligned with the regional government. The RDD coefficient is equal to -0.51 in the first case and -1.10 in the second, which represents a reduction of development due to horizontal cooperation

Figure 2.4: Mayor ideology and Vertical alignment



Note: : (i) Elections: 1979, 1983, 1987, 1991, 1995, 1999, 2003, 2007 and 2011. (ii) RDD estimates using a local linear polynomial with an optimal bandwidth specific for each case. (iii) S.e. clustered at the Coastal area level, dashed lines indicate 95% c.i. (iv) Vertical alignment = mayor and regional president belong to the same party (V=1) or to a differ-rent party (V=0). Local ideology = mayor is left-wing (l=1) or right-wing (r=1). Regional ideology = regional presi-dent is left-wing (L= 1) or right-wing (R=1). Coastal Law = years after the approval of the‘Ley de Costas’ (Law=1) or before (Law=0).

of about 60% and 90%, respectively. The difference between both coefficients is sizeable but not statistically significant (p-value= 0.127). In any case, notice that the coefficient is substantial and very precisely estimated for unaligned municipalities, which suggests that our results on horizontal cooperation are not simply due to both treatments (H and V) being conflated. Moreover, in this case, some stories that could rationalize the type of heterogeneous effect we find: the impact of horizontal cooperation is lower for vertically aligned ($V = 1$) than unaligned ($V = 0$) municipalities because vertical and *Horizontal alignment* might be to some extent substitutes. The regional government has more incentives to internalize environmental spillovers, but lacks competences to influence local development, or is restricted to local governments controlled by the same party. Notice, however, that the effect of *Horizontal alignment* is still sizeable in this case. Finally, the last four lines of the graph show the Panel FE estimates. The effects go in the same direction: the impact of *Horizontal alignment* is larger for right than for right-wing mayors. It is also larger for those that are vertically unaligned than for those that are aligned with the regional government. Notice that, in this case, the differences are small and not statistically significant.

Undeveloped Land vs. Unemployment In the theory section, we suggested that the effect of horizontal cooperation on coastal development should be higher the lower the amount of share of undeveloped land to start with, and lower the higher the unemployment rate. Table 2.3 presents the estimation of a heterogeneous RDD (Eggers, Fowler, Hainmueller, Hall, and Snyder Jr, 2015). We interact the *Horizontal alignment* treatment (H) with the variable of interest (%Undeveloped land or %Unemployed) or with both variables at the same time, and also interacted with the local polynomial. Moreover, to make sure that the results are due to the effect of these variables, and not to other factors that can also moderate the effect of H on coastal development, we include a full set of interactions with other variables. We include interactions with beach length (which affects tourist demand), ruggedness (which affects the accessibility to shore), rainy days (which affect the possibility of enjoying the beach), an urban area dummy (since in urban areas the development might be driven by factors other than proximity to the beach), and %Educated and %Electoral turnout, which might be correlated with the demand for preserving the coast. Finally, in a final specification, we also include interactions between H and a set of Region and Term fixed effects. This means that the effect of the interaction term is identified from within-region variation. The results in Table 2.3 suggest that both %Undeveloped land and %Unemployment moderate the effect of *Horizontal alignment* on development. The coefficient in the first row indicates the impact of H measured at the average value of the interaction variable/s (all the interacting variables have been demeaned). Rows two and three display the interaction coefficients for the two variables we focus on. Both interaction coefficients are positive and statistically significant. The coefficients are quite stable across specifications and do not change when we include additional interactions.

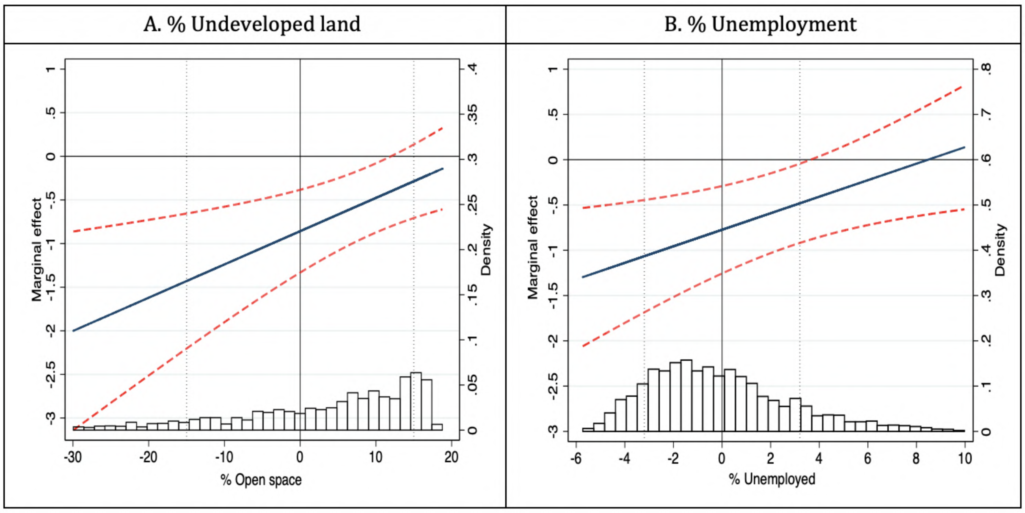
In order to be able to interpret the magnitude of the interacted coefficients, we look at the marginal effects, which are presented in Figure 2.5. In Panel A, we show the marginal effect for %Undeveloped land: the marginal effect of H ranges from -1.5 (-1 s.d. of the interacted variable) to zero (+1 s.d.). Remember that the average treatment effect was around -0.83. This result suggests that *Horizontal alignment* does not deter coastal develop-

Table 2.3: Heterogeneous effects: % Undeveloped land and %Unemployed

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Horizontal align. (H)	-0.851*** (0.288)	-0.773*** (0.293)	-0.923*** (0.292)	-1.019*** (0.374)	-0.886*** (0.277)	-0.961**** (0.371)	–
x % Undeveloped	0.038** (0.016)	–	0.043** (0.016)	0.038** (0.017)	0.039** (0.015)	0.045** (0.018)	0.052* (0.028)
x % Unemployed	–	0.091** (0.045)	0.066* (0.035)	0.075* (0.041)	0.069* (0.041)	0.061* (0.042)	0.082* (0.044)
x Beach length	–	–	–	0.838 (0.501)	0.748 (0.551)	0.731 (0.531)	–
x Ruggedness	–	–	–	-0.023 (0.354)	-0.033 (0.312)	-0.037 (0.364)	–
x Urban area	–	–	–	–	0.112 (0.145)	0.068 (0.131)	–
x # Rainy days	–	–	–	–	-0.076 (0.102)	-0.045 (0.044)	–
x % Higher education	–	–	–	–	–	-0.063* (0.031)	–
x % Electoral turnout	–	–	–	–	–	-0.058* (0.031)	–
Interacted Region & Term f.e.	N	N	N	N	N	N	Y

Note: (i) Elections: 1979, 1983, 1987, 1991, 1995, 1999, 2003, 2007 and 2011. (iii) RDD estimates using a local linear polynomial with the same optimal bandwidth than in Table 2. (ii) All columns control for Coastal area x Term f.e. The last column controls for interactions between *H* and Region & Term fixed effects. (iii) All the interacted variables have been de-meaned. (v) *, ** & ***: statistically significant at the 10, 5 & 1% levels; s.e. clustered at the Coastal area level (?clusters = 29).

Figure 2.5: Heterogeneous effects: % Undeveloped land and %Unemployed



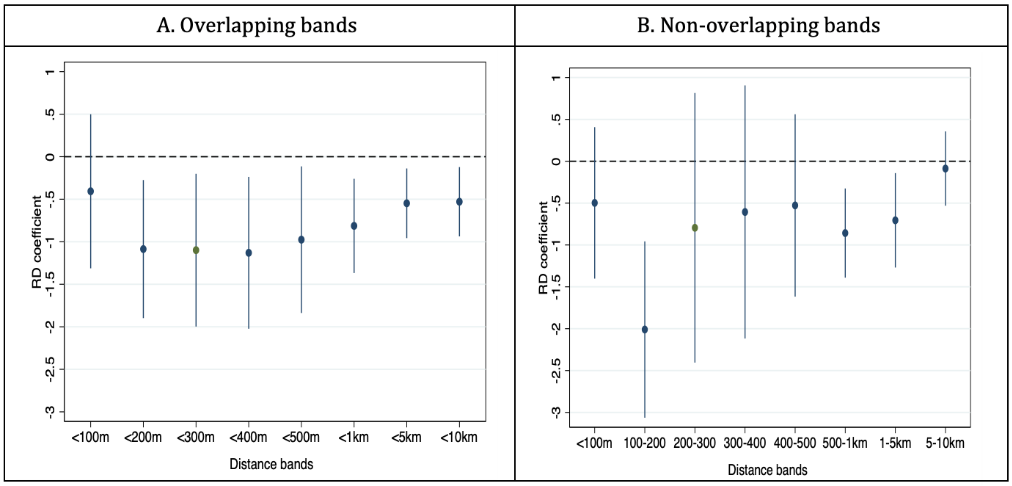
Note: : (i) Linear marginal effects (bold line) computed the results of column 3 in Table 3. Dashed lines are 95% c.i.

ment at all in places that already have plenty of undeveloped lands and that the effect becomes really strong as land close to the coast becomes scarcer. In Panel B, we show the marginal effect for %Unemployment. The graph looks similar to the previous one: in places with a high level of unemployment, *Horizontal alignment* does not limit development; conversely, in places with very low unemployment, aligned local governments end up developing much less land than the unaligned ones. The coefficient ranges from -1.5 (-1 s.d.) to -0.5 (+1 s.d). Note, however, that *Horizontal alignment* never creates a tendency to develop more, even in places with a very high level of development. In Panels A and B, we show the effects evaluated at the median of each tercile, to discard that the marginal effect is driven by some outliers and/or is non-linear (Hainmueller, Mummolo, and Xu, 2019). The effect grows from the lower to the middle tercile, and from the middle to the upper one, so does seem quite linear. We admit that these results have to be interpreted with care. The average treatment effect identified through the RD design can be interpreted as causal. However, it is less clear that the heterogeneous effects obtained either by splitting samples or through the interacted RDD can be interpreted as causal. In any case, notice that the different pieces of evidence point in the same direction: *Horizontal alignment* tends to limit development more in some stances than in others; horizontal cooperation kicks in when unemployment is low, coastal land scarce, and when development threatens land closer to the coast. The evidence regarding the effects of central and regional regulations is weaker, enhancing the possibility of spontaneous cooperation among local governments.

Distance to the coast — Our theory suggests that cooperation will have a stronger effect when the externality is also larger. This should happen the closer development is from shore. Constructions far inland will less damage the value of the coast and will less harm the utility of non-residents. In this section, we investigate whether the intensity of co-operation increases when we move close to shore. Figure 2.6 plots the effect of *Horizontal alignment* for different distance bands. In Panel A we show the results for several overlapping fringes: less than 100 meters, less than 200m, less than 300, less than 400m, less than 500m, less than 1km (which is the band used to present the main results), less than 5km and less than 10km. The results suggest that the effect of '*Horizontal alignment*' decays with distance to coast, except for the first fringe. Between the 200 meters fringe to the 5 Km fringe, the coefficient drops from -1.11 to -0.51, and the difference between these two coefficients is statistically significant. This seems to confirm the idea that 'horizontal cooperation' is more compelling the closer to the shoreline. The coefficient for the 100 meters fringe is smaller than for the 200 meters one (-0.80 v. -1.32). This seems to suggest that either central and regional regulations or environmental regulations make 'horizontal cooperation' a little less necessary close to the coast.

In Panel B, we repeat the exercise but with non-overlapping bands: less than 100 meters, 100 to 200m, 200 to 300 m, 300 to 400m, 400 to 500m, 500 to 1km, 1 to 5km, and 5 to 10km. The results from this analysis show that the effect of horizontal cooperation is very large in the second fringe. The coefficient is -2.01, which implies a reduction of 84% in development caused by horizontal cooperation (recall that his number was a 57% for the 1km band). Notice that this band has the characteristic of being the first one without full protection. When development is effectively banned from the first band (less than 100m), it might fly out of the municipality or move a bit inland to the following band. The other lines show that the results remain at a lower level for all the fringes until the last one (5 to 10km, where the coefficient is zero). The estimates are less precise, but the non-

Figure 2.6: Heterogeneous effects: Distance to the coast



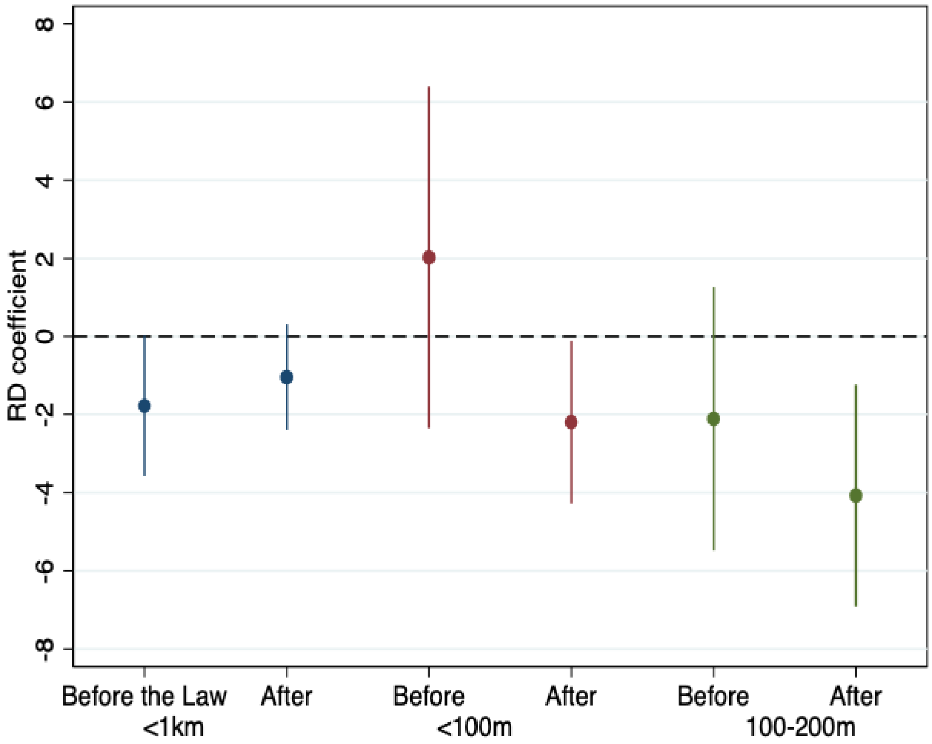
Note: : (i) Elections: 1979, 1983, 1987, 1991, 1995, 1999, 2003, 2007 and 2011. (ii) Distance to coast: development at distances lower that those indicated. (iii) RDD estimates using a local linear polynomial with an optimal bandwidth specific for each case. (iv) S.e. clustered at the Coastal area level, dashed lines indicate 95%.

overlapping estimates might be more affected by errors of construction than those obtained from the overlapping bands (which have the shortcoming of providing less detail).

Central Regulation — In this section, we dig a little bit deeper into the relationship between horizontal cooperation and the coastal protection frameworks of higher layers of government. We look at the possible effects on the intensity of horizontal cooperation of the enactment of the 'Ley de Costas'. We estimate the effect of *Horizontal alignment* separately for the terms before the law and after. We compare the two distance bands more plausible affected by the law (zero to 100m and 100 to 200m), and we also report results for the main band used in the analysis (less than 1km). Finally, since the intensity of development might follow a long-run trend, we control for an interaction of time and *Horizontal alignment* and also for interactions between the alignment and main control variables (%Unemployed and %Undeveloped). In any case, the results of this analysis should be considered with care because of their many limitations.

The results are shown in Figure 2.7. The first two lines show the estimated RDD coefficient for the 1km band before and after the law. The effect of *Horizontal alignment* seems a little smaller after the law, but the difference is small and not statistically significant. Hence, we may conclude

Figure 2.7: Heterogeneous effects: Central regulation



Note: : (i) Before/After the Law=terms before/after 1991-95. (ii) Distance to coast: development at distances lower that those indicated. (iii) RDD estimates using a local linear polynomial with an optimal bandwidth specific for each case, including all the control variables interacted with alignment. (iv) s.e. clustered at the Coastal area level, dashed lines indicate 95%.

that the law had no significant effect at this level. The other lines show the coefficients of *Horizontal alignment* before and after the legislation, for the first and second distance bands (less than 100m and 100 to 200m). What we observe is that the effect of horizontal cooperation increased in both bands. The effect is notably more substantial in the first one: the coefficient was positive before the law (but not statistically different from zero) and negative and significant after. An explanation for this effect is that the protection introduced by the law has not been entirely effective, making horizontal cooperation still necessary (or even more necessary). The impact of horizontal cooperation after the law also becomes larger in the second band (and not on the whole 1km band), suggesting that the pressure against development in the first fringe may have generated spillovers towards the second and made horizontal cooperation more needed at this level.

2.7 Conclusion

In this paper, we study the effect of intergovernmental cooperation on Spanish coastal development. Keeping coastal land undeveloped may provide benefits (e.g., preservation of open space) or costs (e.g., jobs) to both residents and non-residents in the political jurisdiction. Therefore, local governments deciding in isolation – and not accounting for the welfare of non-residents – may not choose the optimal amount of development.

We investigate how political alignment among neighboring coastal municipalities affects this decision. We argue that politically aligned politicians tend to cooperate more. Indeed, they have similar preferences, more opportunities to engage in policy conversations, and may be forced by party authorities to collaborate to enforce internal party rules or coalition agreements.

We find that municipalities with a mayor belonging to the same party (or coalition of parties) that rules in most neighboring municipalities develop much less land close to the shore than other municipalities. In particular, within the first-kilometer fringe, local governments sharing their neighbors' ideology convert 63% less land than otherwise similar but politically isolated governments.

The fact that cooperation among local governments, proxied by political alignment, leads to less development, suggests that the lack of inter-municipal cooperation might generate over-development in coastal areas. Shoreline constructions may not only have a dramatic impact on their local environment. They also tend to be more exposed to natural hazards, such as increasing sea-level rise and coastal storms. Inter-municipal cooperation, therefore, appears primordial in regard to climate change.

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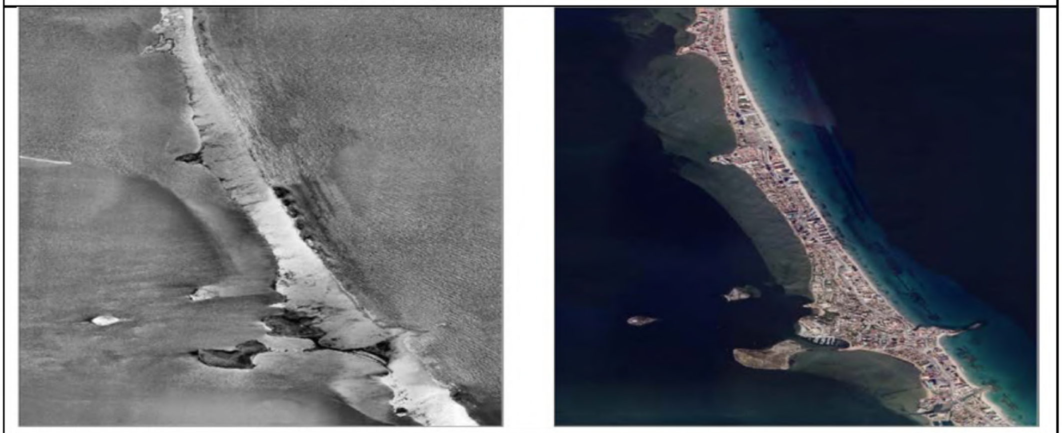
Appendices

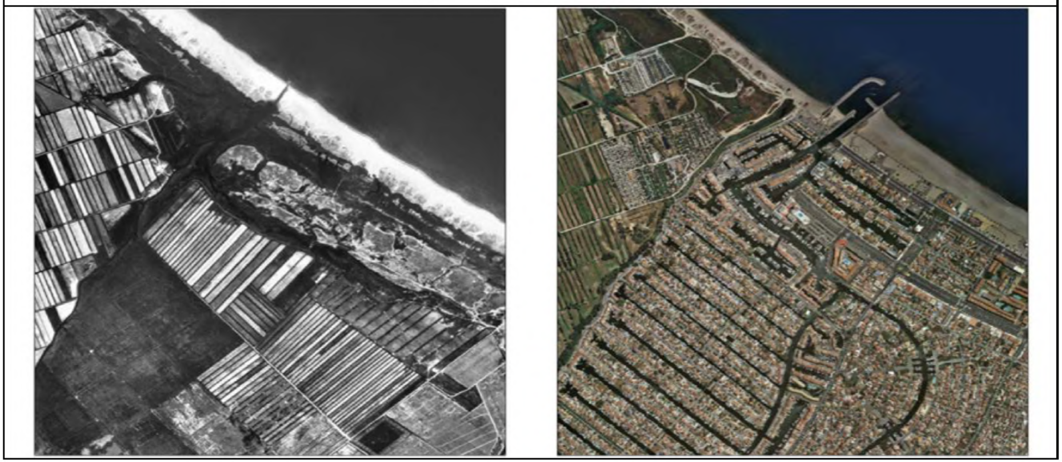
A Figures and Tables

Figure A.1: Intensity of Coastal development, 1956 v. 2012 (Examples)

(a) **La Manga del Mar Menor (Murcia)**

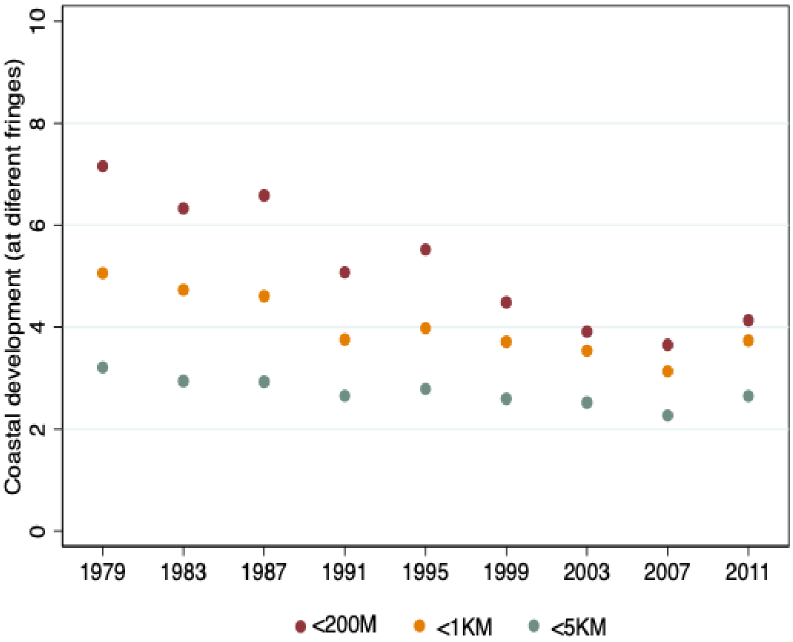


(b) **Empuriabrava (Girona)**



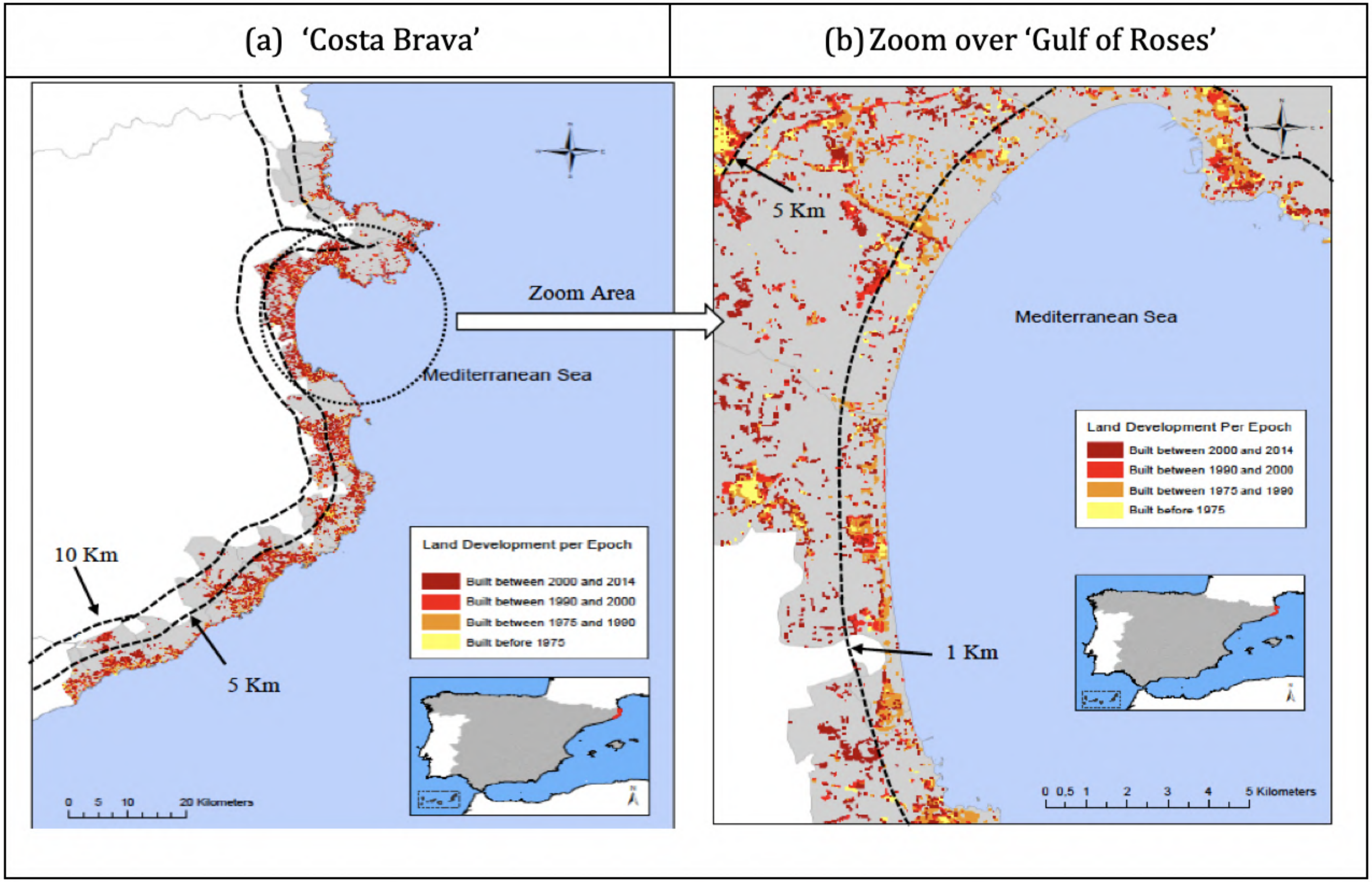
Sources:: : PNOA Americano Serie B (1956) and Google Earth (2012)

Figure A.2: Intensity of Coastal development. Periods and distance to coast



Note: : (i) $d_{i,t}$, measured as land developed in a given distance fringe during each term-of-office/land undeveloped in a given fringe at the start of each term; data shown for each of the nine terms starting the year indicated and for three fringes (between zero and 200 meters of the coast, between zero and 1 KM and between zero and 5 KM). (ii) Sources:'Global Human Settlement Layer','Corine Land Cover Project' and own elaboration.

Figure A.3: The‘Global Human Settlement Layer’ Database



(a) ‘Costa Brava’

(b) Zoom over ‘Gulf of Roses’

Note: : (i) Map of *?Costa Brava’* (Catalunya, Mediterranean sea) and zoom over‘Gulf of Roses’ (northern part of the coastal area). (ii) We show in different colors the amount of development in different periods (before 1975, from 1975 to 1990, from 1990 to 2000, and from 2000 to 2014). The dashed lines indicate the borders of different distance fringes from the coast (1 Km, 5Km and 10Km). (iii) Data from the‘Global Human Settlement Layer’ Database (https://ghsl.jrc.ec.europa.eu).

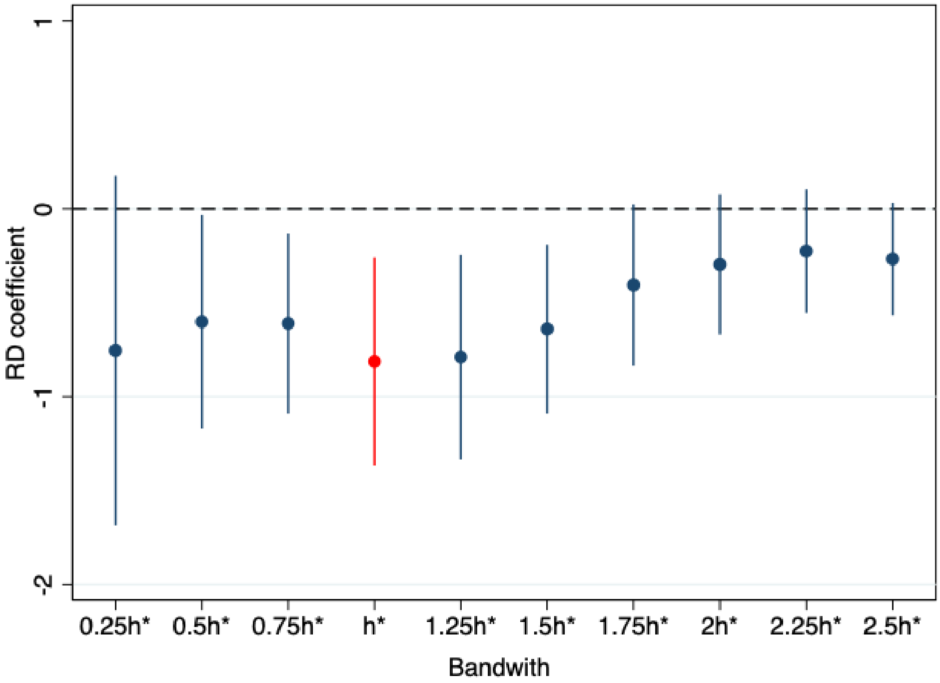
Table A.1: List of Coastal Areas in Spain

Region	Coastal area	Ocean/Sea
Galicia	Rias Baixas	Atlantic
	Costa da Morte	Atlantic
	Golfo rtabo	Atlantic
	Rias Altas	Atlantic / Cantabric
Asturias	Costa Verde	Cantabric
Cantabria	Costa Esmeralda	Cantabric
Pas Vasco	Costa Vasca	Cantabric
Catalunya	Costa Brava	Mediterranean
	Costa del Maresme	Mediterranean
	Costa del Garraf	Mediterranean
	Costa Daurada	Mediterranean
Valncia	Costa del Azahar	Mediterranean
	Costa de Valncia	Mediterranean
	Costa Blanca	Mediterranean
Balearic Islands	Mallorca	Mediterranean
	Menorca	Mediterranean
	Eivissa i Formentera	Mediterranean
Murcia	Costa Clida	Mediterranean
Andaluca	Costa de Almera	Mediterranean
	Costa Tropical	Mediterranean
	Costa del Sol	Mediterranean
	Costa de la Luz	Atlantic
Canary Islands	Tenerife	Atlantic
	La Gomera	Atlantic
	Gran Canaria	Atlantic
	La Palma	Atlantic
	El Hierro	Atlantic
	Lanzarote	Atlantic
	Fuerteventura	Atlantic

Table A.2: Descriptive statistics

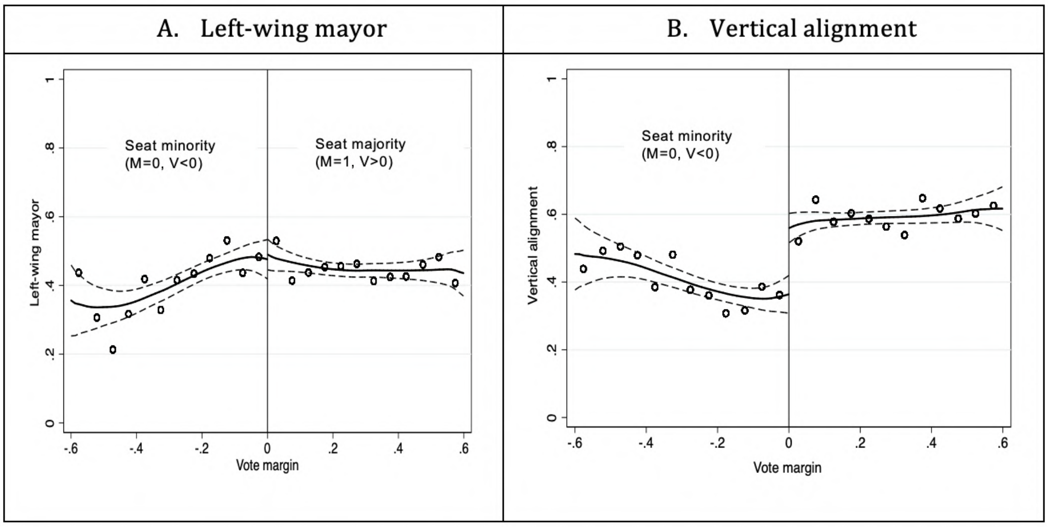
	Mean	s.d.	Min	Max	#Obs
Horizontal alignment (dummy)	0.497	0.500	0	1	3,785
Vertical alignment (dummy)	0.387	0.487	0	1	3,785
Left-wing mayor (dummy)	0.417	0.493	0	1	3,785
Left-wing region (dummy)	0.346	0.476	0	1	3,785
% Undeveloped land	83.08	17.62	4.21	99.77	3,785
Coast length (Km)	9.38	1.15	5.06	11.85	3,785
Beach length (km)	7.81	1.29	2.70	12.94	3,785
Ruggedness (% over 30% slope)	17.35	18.39	0	86.21	3,785
# Rainy days	8.69	3.91	2.63	16.94	3,633
Av. Temperature	16.84	2.22	11.43	21.77	3,633
Mediterranean (dummy)	0.72	0.45	0	1	3,785
Islands (dummy)	0.25	0.43	0	1	3,785
log(Population)	9.17	1.35	4.25	14.45	3,785
log(Density)	5.41	1.44	1.24	9.86	3,785
log(Area)	3.76	1.16	-0.29	7.24	3,785
% Unemployed	15.84	7.39	2.61	42.19	3,785
% No education	51.60	17.30	18.7	87.3	2,358
% Primary education	20.70	6.91	4.72	48.10	2,358
% Secondary education	9.82	3.11	2.80	17.91	2,358
% Higher education	4.93	1.65	2.13	9.44	2,358
% Agriculture	12.11	10.13	15.40	60.32	3,785
% Industry	17.16	9.08	3.64	45.49	3,785
% Construction	10.90	2.97	4.37	21.45	3,785
% Services	59.72	11.71	19.82	86.33	3,785
Effective # of parties	2.665	0.811	1	6.422	3,785
% Electoral turnout	67.30	9.87	28.72	96.92	3,785

Figure A.4: Effect of bandwidth choice



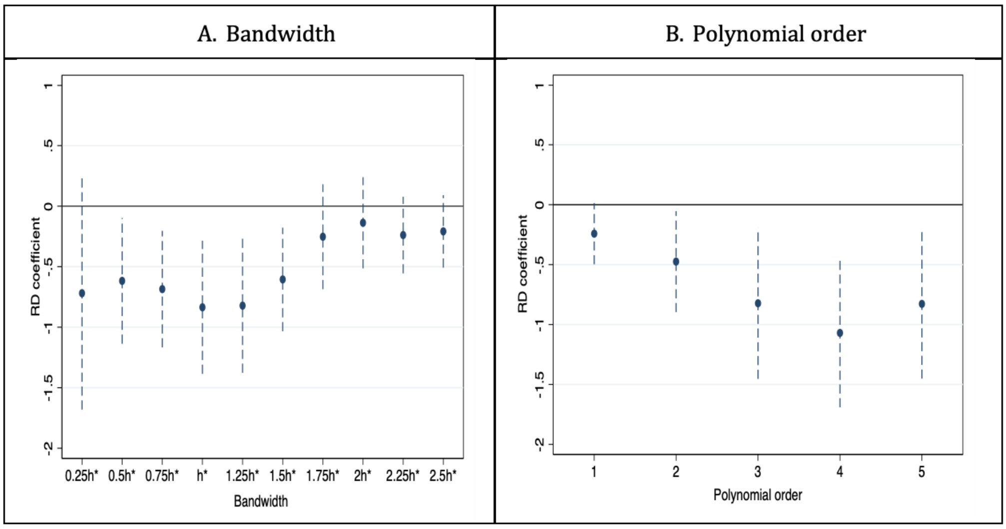
Note: :(i) Elections: 1979, 1983, 1987, 1991, 1995, 1999, 2003, 2007 and 2011. (ii) In Panel A, RDD estimates using a local linear polynomial with bandwidth that are divisors or multiples of the optimal bandwidth. In Panel B, RDD estimates of a global polynomial of different orders. (iii) s.e. clustered at the Coastal area level, dashed lines indicate 95%.

Figure A.5: Potential confounded treatments



Note: : (i) Elections: 1979, 1983, 1987, 1991, 1995, 1999, 2003, 2007 and 2011. (ii) The dots are bin averages of 5 percent size. The solid line represents the predicted values of a local linear polynomial smoothing on each side of the threshold. The dashed lines are 95 percent confidence intervals. (iii) Vertical alignment = dummy equal to one if the mayor belongs to the same party (or ?coalition of parties?) than the regional president. (iv) See Figure 1.

Figure A.6: Potential confounded treatments



Note: : (i) Elections: 1979, 1983, 1987, 1991, 1995, 1999, 2003, 2007 and 2011. (ii) In Panel A, RDD estimates using a local linear polynomial with bandwidth that are divisors or multiples of the optimal bandwidth. In Panel B, RDD estimates of a global polynomial of different orders (iii) S.e. clustered at the Coastal area level, dashed lines indicate 95%.

Chapter 3

Does Media Coverage Affect Governments' Preparation for Natural Disasters?

3.1 Introduction

It is a common mistake to confuse extreme weather events and natural disasters. Both are generally perceived as powerful, violent, unavoidable life hazards, or 'acts of God.' Yet, for disasters to occur, at least some buildings need to exist, and there need to be inadequate protective infrastructures. *'Nature did not construct twenty thousand houses of six to seven stories' on a seismic breach* – wrote Genevan philosopher Jean-Jacques Rousseau in 1755[1]. But quite often, individuals lack information about the risks they take when investing in a location. If individuals are not aware of their actual risk exposure, should we not expect their local governments to prepare for disasters on their behalf? Today, the preparation puzzle has never been so pressing. While the Federal Emergency Management Agency (FEMA) advertises that 1 dollar spent on mitigation saves taxpayers 6 dollars of potential losses[2], natural disasters cost the United-States' economy a record

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[1] This quote is extracted from a famous epistolary dispute between French philosopher Voltaire and Genevan philosopher Rousseau about the Great Lisbon earthquake of 1755 which caused the death of approximately 100,000 persons. Voltaire was shocked by what he perceived as an absurd, awful, unavoidable hazard. Rousseau argued that if the city had been less concentrated, and if the population had been evacuated in time, lives would have been spared. Strömberg (2007) also quotes this quarrel.

[2] National Institute of Building Sciences, "Natural Hazard Mitigation Saves: 2017 Interim Report"(2018), https://www.nibs.org/page/mitigationsaves. This report considered 23 years of federal grants awarded by FEMA, the Economic Devel-

306 billion dollars in 2017[3]. In the meantime, local administrations spent a more modest amount of 8.6 billion dollars worth of FEMA mitigation subsidies on that same year[4]. What explains local governments' apparent reluctance when it comes to mitigation?

The core assumption of this paper is that local governments face a trade-off between risk reduction and risk disclosure. Indeed, mitigation projects are designed to protect individuals and their wealth against future disasters – but their implementation also signals a place's hazard exposure. Where there is a floodwall, there is likely a flood risk, and where there is a storm shelter, there are surely strong winds. On the one hand, mitigation infrastructures might certainly reassure homevoters and well-informed homebuyers by reducing the perceived risks of suffering from future natural disasters. On the other hand, they can push a priori unaware investors to update their perception of local risks by signaling the inherent liability of a location, hence putting this location at a competitive disadvantage. The ensuing question is: why would a local government take mitigation measures if it reveals its liabilities?

In this paper, I study how local risks' awareness fosters local governments' decisions to prepare for storms. In particular, I look at how an increase in local newspapers' coverage of storms impacts the decision to provide storm mitigation. In a world of complete information, shrouding risk exposure by not taking mitigation measures is likely to hurt the local housing market as homebuyers are already aware of the risks (Milgrom, 1981; Jovanovic, 1982). However, in the presence of incomplete information, local governments seeking to protect property values in their jurisdiction have incentives not to disclose latent risks to otherwise uninformed investors (Gabaix and Laibson, 2006; Brown, Hossain, and Morgan, 2010). Increased press coverage of locations hit by a storm is likely to foster prospective homebuyers' risk awareness. In turn, governments ruling over these jurisdictions are encouraged to invest in mitigation infrastructures

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opment Administration, and the Department of Housing and Urban Development. Estimated savings (i.e., benefits) are derived from reductions in property losses; deaths, injuries, and post-traumatic stress disorder; direct and indirect business interruption; and other losses (https://www.pewtrusts.org/en/research-and-analysis/issue-briefs/2018/09/natural-disaster-mitigation-spending–not-comprehensively-tracked)

[3] https://www.cbsnews.com/news/us-record-306-billion-natural-disasters-last-year-hurricanes-wilidfires/

[4] Among which 8.3 billion were granted (https://www.fema.gov/openfema-dataset-hazard-mitigation-assistance-projects-v1). About 315 million dollars worth of subsidies were denied to applicants in 2017 (FOIA request – 2019-FEFO-00367).

to compensate for this negative effect on reputation. In the absence of any information shock, places hit by a storm remain virtually risk-free to prospective investors. In this case, I contend that governments who suffered a disaster have fewer incentives to invest in risk-signaling mitigation infrastructures, as uninformed homebuyers will adversely select into their jurisdiction (Akerlof, 1978)[5].

I focus on local newspapers' coverage because the local press remains a key source of information on the activities of U.S. communities. According to a 2011 survey conducted by the Pew Research Center[6], the American population classifies local newspapers as their top source of information when it comes to housing and real estate, local politics, and community events. Local television is preferred for watching sports or weather forecasts. The Internet is favored when individuals seek information about local jobs, restaurants, or schools. Yet, when it comes to learning about a community's daily life and its housing market, local press appears to be the favorite medium. Therefore, if storm risks matter for a given community's well-being, it is likely to be reported in the local newspapers. Additionally, according to a 2013 survey by the National Association of Realtors (2014), the typical investment property is only 20 miles from the buyer's primary residence (Gao, Sockin, and Xiong, 2018). The main reasons for investing in a new property are to derive a rental income (37%), because of low prices or because the buyer found a good deal (17%), and for potential price appreciation (15%)[7]. These finding suggests that the representative investor is living close to his investments and plans to extract a rent from these latter. The typical investor is then likely to read local newspapers before performing a property purchase. Finally, to measure local governments' mitigation initiatives, I will focus on the universe of local administrations' applications to the Hazard Mitigation Grant (HMGP) program, the largest subsidized program for mitigation activities available to local governments in the United States. Several reasons are indicating that this program cap-

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[5] Note that in the seminal Rosen-Roback model (Rosen, 1974; Roback, 1982), individuals are perfectly aware of a city's attributes. This assumption is relaxed in this paper. Generally, considering that individuals' spatial sorting differs with the information they receive about potential destination leads to broader questions on the valuation of local quality of life and local amenities.

[6] Pew Research Center – *'How people learn about their community'* – https://www.pewinternet.org/
2011/09/26/part-3-the-role-of-newspapers/

[7] https://www.prnewswire.com/news-releases/vacation-home-sales-soar-to-record-high-in-2014-investment-purchases-fall-300059334.html

tures local governments' will for mitigation. First, federal entities largely finance mitigation projects (at least 75% of the project cost)[8], so local administrations' budgets are not likely going to be dramatically affected by a project. Second, it is not a competitive program (contrary to the other mitigation programs proposed by FEMA) – which alleviates the risk of federal or state selection. Finally, local governments must send their applications within the year following a presidentially declared disaster, so the mitigation decision can be directly associated with a disaster. I further develop these arguments in Section 3.3.

An obvious challenge in identifying the impact of press coverage on these mitigation initiatives is that both local governments' preparation for storms and the amount of local news about storms are both driven by unobservable characteristics. For instance, constituents' beliefs regarding natural disasters and climate change are a direct cofounder of this effect. Local politicians' beliefs might also matter to the extent that media capture is always a latent risk. To establish causality, I compute the match between newspapers' markets and the spatial extent of storms at the ZIP code level. The rationale behind this measure, inspired by Snyder and Strömberg (2010), is that the more a newspaper's readership is hit by a storm, the more this newspaper is likely to report about this event[9]. I show that this measure is a good predictor of the number of articles published about storms by scraping data from the website *Newslibrary.com.* I argue that conditional on location, and county-year fixed effects, the socioeconomic determinants that shape local newspaper markets are unrelated to the topographical and climate factors that explain a storm's exact extent, so the match between local newspapers' markets and the spatial extent of storms is haphazard. I then average this match based on the market share of each newspaper in a given jurisdiction. The empirical strategy then consists in comparing, within counties whose local authorities are eligible to the Hazard Mitigation Grant Program, storm mitigation projects in ZIP code areas where media coverage of storms is high to ZIP code areas where it is low.

My findings are striking. My main results suggest that conditional on

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[8] Depending on the local legislation, the State administration typically participates in the payment of the remaining 25%.

[9] If the match is one, then all readers of a given newspaper suffered a storm. If the match is null, then none of them suffered a storm. All things being equal, a local newspaper located in Maryland is less likely to report about a storm occurring in Colorado than Colorado's newspapers whose readers have been directly experiencing the disaster.

being hit by a storm, a one standard deviation increase in my treatment leads to an increase of 54% in the mean number of mitigation projects. In the absence of any information shock, communities do not invest in mitigation technologies. I interpret these results as indicative that local governments strategically underinvest in mitigation to avoid signaling the latent risk of storms to investors who would have remained otherwise uninformed. Indeed, I find that the information shock especially matters for mitigation infrastructure projects, rather than non-structural mitigation projects like land acquisitions, which are less likely to signal the dormant risks. Additionally, right after being hit by a storm, a one standard deviation increase in storm coverage leads to a decrease both in housing sales and in the emission of new building permits, by almost 2% and 1%, respectively[10]. These substantial figures suggest that land investors divert their investment towards what appears as safer areas when information about risks circulates. Finally, I present some evidence suggesting that these results are driven by non-resident investors. In particular, the heterogeneous analysis shows that they are primarily induced by locations with high pre-treatment levels of renter-occupied housing, vacant housing units, housing units owned with a mortgage, and areas having experienced higher inflows of populations before a storm – which is consistent with the real-estate investment patterns described by the anecdotal evidence.

This paper relates to a growing body of literature on natural disasters. So far, this literature has mostly focused on individuals' perception of these tail events (Leiserowitz, 2006; Taleb, 2007; Myers, Maibach, Roser-Renouf, Akerlof, and Leiserowitz, 2013) and on the individuals' reaction to this latter (Boustan, Kahn, and Rhode, 2012; Bunten and Kahn, 2017). In particular, Gallagher (2014) shows that individuals update their beliefs of the likelihood of flood occurrence based on the discounted history of floods, and are more likely to get flood insurance when these beliefs are strong. In the political economy literature, most studies focus on the links between natural disasters and the provision of disaster relief (Besley and Burgess, 2002; Strömberg, 2004; Eisensee and Strömberg, 2007; Healy and Malhotra, 2009; Gasper and Reeves, 2011; Bechtel and Hainmueller, 2011) or between

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[10] As a means of comparison, the National Association of Realtors indicated that sales plunged by 13% in March 2007 compared to March 2006, in the midst of the mid-2000's housing bubble. As for the period of analysis, between 2010 and 2018, the Census Bureau reported that the average 12-month change in seasonally-adjusted housing sales was approximately -1.15%.

natural disasters and the adoption of green bills (Pattachini, Paserman, and Gagliarducci, 2019; Kahn, 2007). Interestingly, Healy and Malhotra (2009) argue that, contrary to disaster relief measures, voters do not seem to value risk preparedness a priori. Kahn (2005) notes that the quality of institutions is a strong determinant of proactive mitigation measures, as they foster political accountability. This paper also relates to the political economy of mass media (Besley and Burgess, 2002; Strömberg, 2004; Eisensee and Strömberg, 2007; Snyder and Strömberg, 2010; Enikolopov, Petrova, and Zhuravskaya, 2011; Prat and Strömberg, 2013; Durante, Pinotti, and Tesei, 2019) and shows how the distribution of risk information may influence local policies. Finally, some recent works explore the links between risk perception of natural disasters and housing prices (Barrage and Furst, 2019; Coulomb and Zylberberg, 2019; Singh, 2019; Bakkensen and Barrage, 2017). To my knowledge, this paper is the first to document why local governments might not prepare their jurisdictions for natural disasters while being effectively threatened, and to consider that mitigation infrastructures may signal the inherent risk of disaster.

3.2 Theoretical Framework

3.2.1 Conceptual Background

The seminal Rosen-Robach model (Rosen, 1974; Roback, 1982) assumes that individuals know ex-ante city attributes. Yet, while individuals are land consumers, all lands do not only display unshrouded features. We all recognize a forest, but inferring the risks of wildfires requires *costly* information. Most individuals do not often think about the hidden costs of a location when they decide where to settle. That is to say, they rarely think about the shrouded attributes of their new community.

This matter is particularly salient when it comes to anticipating a rare event's probability, like a natural disaster. Chapman University Survey on American fears (2014)[11] showed that while storms are respondents' number one natural disaster phobia, an overwhelming majority of them do not prepare – even in the riskiest places like Tornado Alley.

This form of consumers' myopia gives local governments incentives to

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[11] Chapman Survey of American Fears, Wave 1 (2014) – http://www.thearda.com/Archive/Files/Descriptions/CSAF2014.asp

hide risks from prospective investors. Indeed, revealing the dormant dangers to unsuspecting renters or homebuyers would put their jurisdiction at a competitive disadvantage. This is particularly salient when governments budgets critically depend on property sales and taxes. Appendix section B provides anecdotal evidence of such behavior in the United-States[12]. Equivalently, not exposing these dangers to well-informed consumers will also put their jurisdiction at a competitive disadvantage, since sophisticated investors will expect the worst from a community that puts efforts in hiding its known liabilities. For that *"all actions* [...] *are unjust if their maxim is not consistent with their publicity"* (Kant, 1795), rational investors shall be suspicious of places where they believe there is something to hide.

In this section, I build on the setting proposed by Gabaix and Laibson (2006) to show that local governments might strategically underinvest in mitigation actions to avoid revealing the hidden risks to otherwise uninformed land investors. In my model, mitigation actions both reduce and signal latent risks to uninformed individuals. Non-resident investors are initially unaware of the dangers but might become sophisticated as they receive exogenous shocks of information. Residents are always aware of the local risks in their community, but they might be ignorant of the state of risks in the neighboring communities.

To develop the intuition for my results, consider a region made of two independent, but similar cities. In particular, they both display a high chance of being hit by a natural disaster. The cost for the local governments of providing public mitigation is null. Assume that the first city – call it *city M*, has implemented a policy of systematic infrastructure elevation and wind retrofitting. On the contrary, the second city – call it *city N*, has not.

The population of this region receives different levels of information regarding the risks of natural disasters. Non-residents vary in their location decision based on their level of sophistication. A myopic risk-averse non-

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[12] According to the Urban Institute, property taxes in the United-States are generally an essential source of revenues for local governments. In 2015, they amounted 472.74 billion dollars, and they were the first source of local jurisdictions' own-collected revenues. Between 2000 and 2015, the share of local property taxes in local government revenues rose from 26.8% to 29.8%. This increase is partially explained by the decrease of intergovernmental transfers to local governments over the same period. In 2015, property taxes represented, on average, 46.6% of local governments' own-collected revenues and 72% of local governments' taxes on average. At a minimum, they amounted 41.5% of total local tax revenues in Alabama and more than 99% of total local tax revenues at a maximum in New Hampshire.

resident will speculate that the cost of living in N is cheaper, as there are no *apparent* risks there[13]. She will choose to settle in this virtually risk-free location. A sophisticated risk-averse consumer, on the other hand, will question the lack of preparedness of *city N* and grow wary that it is, actually, the riskiest location. She will then choose to locate in *city M*, as engaging in private mitigation in *city N* would come at a personal cost.

As mentioned earlier, residents living in both cities are ex-ante aware of the dangers in their city of residence but might be ignorant of the risks in the neighboring town. In the case of *city M*, residents already benefit from public mitigation. Migrating to *city N* and possibly engaging in private mitigation would, therefore, be inefficient. Residents of *city N*, on the other hand, do not benefit from such public resilience policies. If sophisticated, they will migrate to *city M* if it is cheaper than enduring the disaster. If myopic, they will prefer to stay as there is no *apparent* reason to make a costly move.

Local governments, who wish to maximize land value in their jurisdictions, solve this game backward. In a world of incomplete information, investing in mitigation infrastructures might reassure incumbents, but will scare uninformed outsiders. Consequently, if a sufficient share of non-residents becomes aware of the dangers, local officials will take mitigation measures. However, if a sufficient share of non-residents is ignorant of the latent risks of natural disasters, local officials will remain inactive, and hence hide these dormant dangers from prospective investors.

3.2.2 The model

Consider an economy with a population density of mass 1. Consider two similar cities, A and B, with the same high probability of being hit by a natural disaster. Each one is populated with a share $\alpha/2$ of residents. A share $1-\alpha$ of the population does not live in any of the cities and wishes to locate in either A or B (see Figure 3.1). The cost of living in either one of the cities is $p \in \mathbb{R}^+$, and the individual cost of a natural disaster is $\hat{p}$. Individuals are either myopes (m) if they are ignorant of the disaster risk, or sophisticated (s) if they are aware. The level of public mitigation efforts is defined by $m \in]0;\hat{p}[$. Public mitigation is costless to both individuals

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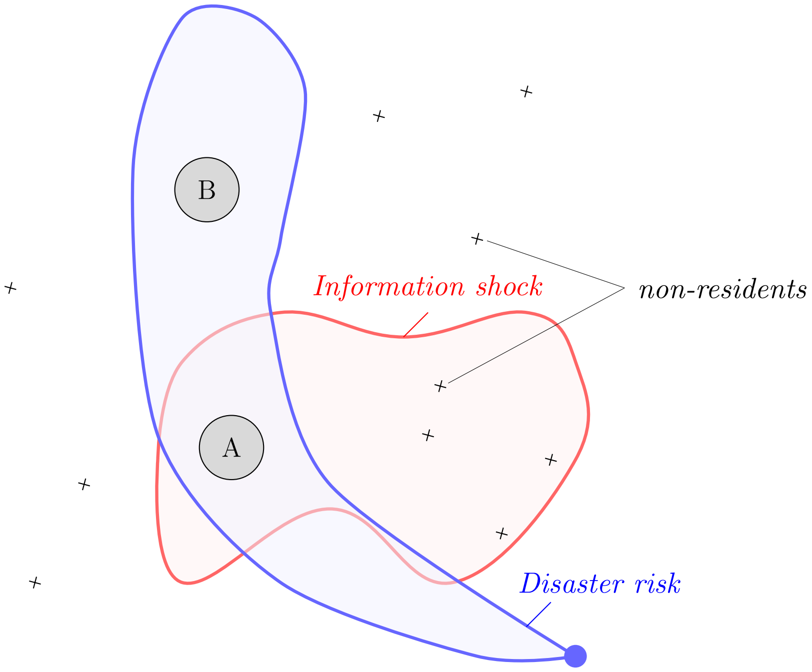
[13] Note that a myopic *risk-neutral* agent would be indifferent between living in N where the risk is virtually null, and living in M where the dangers are possibly offset by the investment in mitigation infrastructures.

Figure 3.1: Conceptual representation of the effect of media coverage on Natural Disaster mitigation



Note: Non-residents must decide whether to move to A or B. Meanwhile, residents must choose whether to leave their hometown, and local governments in each city must decide whether to prepare or not for the local dangers. Residents are always aware of their hometown liabilities but might be ignorant of the risks in the other city. At the beginning of the game, a share $\lambda \in [0, 1]$ of the population becomes informed of the local risks.

and governments.

At any moment, sophisticated individuals take into account the level of risks in both location, while myopics infer the risks if they observe that the government has taken mitigation measures. Formally:

$$\mathbb{E}(\hat{p}|m = 0) = \begin{cases} 0 & \text{if the individual is myopic} \quad ; \\ \hat{p} & \text{if the individual is sophisticated} \end{cases}$$

$$\mathbb{E}(\hat{p}|m > 0) = \hat{p} - m \qquad \text{whether the individual is myopic or sophisticated}$$

In other words, for sophisticated individuals, $\mathbb{E}(\hat{p}|m > 0) < \mathbb{E}(\hat{p}|m = 0)$; whereas for myopics, $\mathbb{E}(\hat{p}|m = 0) < \mathbb{E}(\hat{p}|m > 0)$. That is to say, for sophisticated individuals, the expected cost of a disaster is higher in the absence of mitigation, whereas for myopics it is lower. Note that residents are always informed about their community level of risks, but might be ignorant of the other city's status.

The game unfolds as follows: in the initial period, a share of the population learns from an exogenous shock of information about the dormant risks in both communities. In the meantime, local governments must decide whether or not to take mitigation measures. In period 1, residents decide whether to stay in their hometown or to move, and residents choose where to settle. To do so, each category of individual compares his net gains from staying to his net gains from moving – that is to say, the difference between each city's rent net of the expected cost of a disaster. At the end of the game, each government collects individuals' rent. Therefore, to maximize their revenues, local authorities need to solve this game backward.

To do so, each government makes a mitigation decision conditional on the other's action, the share of the resident population, and the share of sophisticated individuals. If individuals are well-informed, not preparing for the dangers can scare away prospective residents. On the contrary, if individuals are unaware of the risks, preparing for a disaster that virtually does not exist can potentially put a community at a competitive disadvantage too. Conditional on the shares of residents and informed individuals, there are two possible equilibria: a *Shrouded* one, under which it is never in the best interest of a government to take mitigation measures, and an *Unshrouded* one, under which it is always in their best interest to take mitigation measures.

Formally, each period unravels as follow:

Period 0:

- Non-residents are by default unaware of the risks present in each location.
- Residents are by nature sophisticated regarding their community as they observe the local risks. However, they are by default unaware of the risks in the other location.
- At the end of the period, both residents and non-residents receive different levels of information about the latent dangers in A and B.

A share $\lambda \in [0;1]$ of the population becomes sophisticated, while the remaining $1-\lambda$ remains myopic.

- Both governments A and B observe the information shock and decide whether or not to take mitigation action, m_A and m_B, respectively.

Period 1:

- Non-residents (nr) choose a location between A and B. Conditional on their sophistication status $w \in \{m, s\}$, the anticipated net surplus from choosing city $i \in \{A; B\}$ is:

$$x_{i,w,nr} = [-p_i - \mathbb{E}(\hat{p}|m_i)] - [-p_{-i} - \mathbb{E}(\hat{p}|m_{-i})]$$

- Residents (r) choose whether to stay, or to move to the other city at a cost c. Conditional on their sophistication status $w \in \{m, s\}$, their anticipated net surplus from staying in city i is:

$$x_{i,w,r} = [-p_i - \mathbb{E}(\hat{p}|m_i)] - [-p_{-i} - c - \mathbb{E}(\hat{p}|m_{-i})]$$

- The demand for a location i is defined as:

$$D_{i,w,\{r;nr\}} = \begin{cases} 1 & \text{if} \quad x_{i,w,\{r;nr\}} > 0 \\ 1/2 & \text{if} \quad x_{i,w,\{r;nr\}} = 0 \\ 0 & \text{if} \quad x_{i,w,\{r;nr\}} < 0 \end{cases}$$

Period 2: At the end of the game, government i receives:

$$\Pi_i = p.\{\frac{\alpha}{2}[\lambda(1 + D_{i,s,r} - D_{-i,s,r}) + (1-\lambda)(1 + D_{i,m,r} - D_{-i,m,r})] \\ +(1-\alpha)[\lambda D_{i,s,nr} + (1-\lambda) D_{i,m,nr}]\}$$

We can now characterize the sequential equilibrium of the game. The proof of the following proposition is demonstrated in Appendix A.

Proposition 3.1 *When the share of sophisticated individuals,* λ, *is larger than* $\frac{1}{2-\alpha}$, *there exists a Non-Shrouded equilibrium in which governments systematically take mitigation measures. On the contrary, when the share of myopes,* $1-\lambda$, *is larger than* $\frac{1}{2-2\alpha}$ *there exists a Shrouded equilibrium in which governments systematically avoid mitigation.*

Figure 3.2 illustrates the existence of these equilibria. Indeed, solving the above game involves considering several cases. Namely, (i) when the migration cost c is larger than both mitigation gains m_i, and the net disaster cost $\hat{p}-m_i$; (ii) when the migration cost is larger than mitigation gains, but not the net disaster cost; (iii) when the migration cost is smaller than both mitigation gains, and the net disaster cost; (iv) when the migration cost is smaller than mitigation gains, but not the net disaster cost.

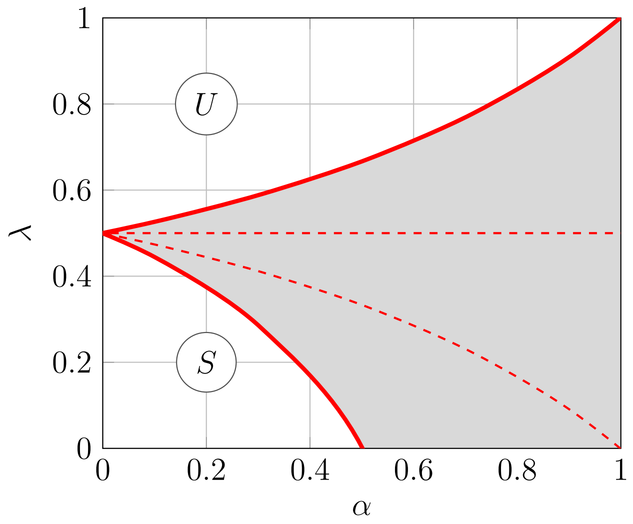
Red lines in Figure 3.2 depict, for each of these cases, the shares of sophisticated and residents individuals, λ and α, for governments to be indifferent between preparing and not preparing for natural disasters. The area above each line depicts situations in which the government prefers to provide mitigation. Respectively, the area under each line depicts situations in which the government prefers to shroud risks by not taking mitigation measures.

The area under the lower plain red line in Figure 3.2, S, represents a Shrouded Risks Equilibrium in which local governments decide to *systematically* not take mitigation measures to avoid disclosing their latent jurisdiction's dangers to myopic land consumers. Uninformed agents adversely sort in riskier location and the anticipated rent p paid by these uninformed individuals does not capture the actual costs, $p+\hat{p}$, of living in a place that shrouds its liabilities.

The area above the higher plain red line in Figure 3.2, U, represents an Unshrouded Risks Equilibrium in which local governments decide to *systematically* take mitigation measures as not doing so would put them at a competitive disadvantage. Note that, the larger the share of residents, α, the larger must be the share of informed individual, λ, for a government to systematically invest in mitigation.

Corollary 3.2 *In particular, when the population is only made of non-residents (i.e.,* $\alpha = 0$*), governments implement (avoid) mitigation measures if more (less) than half of the population is sophisticated. Respec-*

Figure 3.2: Depiction of the *Shrouded* and *Unshrouded* Equilibria



Note: Red lines correspond to different subcases detailed in Appendix A. From top to bottom: (i) when the migration cost is larger than both mitigation gains, and the net disaster cost; (ii) when the migration cost is larger than mitigation gains, but not the net disaster cost; (iii) when the migration cost is smaller than both mitigation gains, and the net disaster cost; (iv) when the migration cost is smaller than mitigation gains, but not the net disaster cost. The upper blank area designates the *Unshrouded* (U) equilibrium, while the lower blank area depicts the *Shrouded* (S) one.

tively, when the population is only made of residents, it is never in the interest of the government to implement mitigation measures.

In other words, high levels of resident population, α, require high levels of informed individuals, λ, for governments to take mitigation actions. In a real-world situation, these residents could be local homeowners, who are generally less mobile, possibly bonded by a mortgage, and responsible for the modification of their own infrastructures[14]. Following a natural disaster, visitors (i.e. renters, tourists, secondary homeowners) are more likely to move first. To replace this leaving population, it is then in the best interest of local officials to continue shrouding their jurisdictions' liabilities if prospective future residents are unaware of the risks.

3.2.3 Discussion

This set-up naturally makes several simplifying assumptions. Hereafter, I discuss three of them. First, having individuals make only a location decision, I disregard the possibility for them to vote in ballots rather than with

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[14] Note that, between 2010 and 2018, homeowners were present in approximately 74% of all occupied housing units (Census Bureau).

their feet. Second, the model does not address the possibility for sophisticated individuals to invest in private mitigation. Finally, by assuming that local governments might not invest in preparation measures to hide the latent dangers to prospective investors, I presume that local officials are a priori aware of the possibility of mitigation measures.

First, could homevoters petition their representatives in adopting protective measures? It is possible that the affected populations did not know their government could develop a mitigation plan and learn it through media reports. In this case, they could indeed petition their local officials for action. More generally, when a disaster receives increased media attention, the political response is likely to become a salient issue (Besley and Burgess, 2002). Therefore, increased media coverage in places hit by the disaster is likely to foster mitigation action.

That being said, this political accountability approach should be considered with care. Indeed, it is not clear that resident voters respond to disaster preparedness – as opposed to disaster relief (Healy and Malhotra, 2009). Indeed, benefits from disaster preparedness are less observable and generally less immediate. For instance, between 2010 and 2018, mitigation projects undertaken under the Hazard Mitigation Grant program took on average one year and a half before being initially approved, and almost four extra years before being closed. Preparation to future disasters is also more complicated to evaluate, as voters usually lack a proper counterfactual situation to assess the policy. On the contrary, disaster relief measures lead, in general, to more instantaneous, clear-cut, measurable gains that easily allow myopic voters to reward or punish their local officials.

Second, could sophisticated residents invest in private mitigation to protect themselves? Yes, but it is not clear how the private mitigation decision relates to the public one. In particular, if private mitigation acts as a substitute to public mitigation, the model's predictions remain unchanged – a risk-averse individual will prefer, conditional on potential migration costs, to locate where he benefits from the additional, costless, protection of public mitigation. However, this would also be assuming that individuals value any additional unit of mitigation similarly. For instance, would one be willing to invest in a personal tailor-made tornado safehouse, if his jurisdiction has already provided him one? Studying how risk aversion and the desire for preparation are affected by the knowledge, or experience, of a natural disaster, requires further hypothesis which are, although very interesting, beyond the scope of this paper[15].

Finally, this model assumes that local governments are ex-ante aware of the possibility to provide costless mitigation, and voluntarily choose not to prepare as it would signal the risks to unsuspecting residents. Here, a possible pitfall is that local officials may not be sophisticated, and learn about the mitigation possibilities in the news. In this case, local governments would avoid mitigation measures simply because they are unaware of them. Fortunately, some federal mitigation programs in the United-States require the granting authorities to notify eligible applicants. How to measure 'enlighten' mitigation efforts is, ultimately, an empirical question.

3.3 Natural Disasters and Mitigation Policies in the US

3.3.1 How to measure mitigation efforts?

Measuring local governments' mitigation efforts is challenging. In particular, it is difficult to identify a common mitigation policy applying to all local governments in the United States. Local governments' finances neither disclose the details of local initiatives, nor the location of the projects. Most of all, it is complicated to disentangle the true will for mitigation of budget-constrained administrations from other unobserved policies.

In that respect, the Hazard Mitigation Grant (HMG) program from FEMA provides a unique setting for capturing real mitigation efforts. First of all, it is only available to local administrative entities. The State is required by law to let these local administrations know they are entitled to apply to this program. Second, it is not as competitive as the other mitigation grants proposed by FEMA[16]. The HMG program's eligibility rules are more lenient, and the rejection rate is very low[17]. This reduces the

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[15] Note that while the literature largely considers that risk aversion is constant over time (Stigler and Becker, 1977), there is a debate on whether repeated negative experiences pushes individuals towards to more risk-averse (Jakiela and Ozier, 2019; Malmendier and Nagel, 2011; Brown, Montalva, Thomas, and Velásquez, 2019), or more risk-prone behaviors (Eckel, El-Gamal, and Wilson, 2009; Voors, Nillesen, Verwimp, Bulte, Lensink, and Van Soest, 2012; Callen, Isaqzadeh, Long, and Sprenger, 2014).

[16] Such as the Pre-Disaster Mitigation Grant, the Flood Mitigation Assistance program.

[17] Between 2010 and 2018, about 2% of HMGP applications were downturned by FEMA.

risk of federal selection of the project. Third, mitigation projects are largely financed by FEMA. The rest is shared between non-federal entities. Local governments' participation in the project cost is typically smaller than 25% of the project's cost. Finally, local governments are required to apply within 12 months following a major disaster, allowing me to associate the mitigation decision with the corresponding level of information circulation. In the following paragraphs, I review the historical set-up and the details of the Hazard Mitigation Grant program's procedure.

3.3.2 Historical and Institutional Set-up

Since 1978, the Federal Management Emergency Agency (FEMA) centralizes most of the emergency competencies previously shared between several federal departments. The agency is under the direct authority of the President of the United States. The 1988 Stafford Act gives the President the authority to issue Emergency or Major Disaster declarations, which allow federal intervention. With the end of the Cold War, funds allocated to disaster response started to benefit the preparation for non-nuclear hazards. The 1990's denuclearization treaties and the Great Flood of 1993 (320,000 squared miles flooded) fostered further political action in that sense. The Volkmer Amendment of December 1993 increased FEMA funds for hazard mitigation or relocation assistance and increased from 50 to 75% the federal subsidy to mitigation projects. By the mid-'90s, the agency's primary objective was officially to build resilient communities, away from hazard-prone areas.

In October 1997, the Clinton administration launched the program 'Project Impact', supported by FEMA. The goal was to build resilient communities through public-private partnerships. Three years later, the Disaster Mitigation Act amended the Stafford Act to include a program of technical and financial assistance designed to foster pre-mitigation disasters. However, it has been claimed that both policies have had a relatively small impact owing to the difficulty for federal governments to compel local governments to engage in mitigation efforts (Sylves (2019)). That being said, FEMA remains by far the most important agency for funding hazard mitigation grants and loans. Since 2003, and in the context of the war on terror, the Department of Homeland Security is in charge of the Federal Emergency Management Agency.

3.3.3 FEMA's Hazard Mitigation Grant Program

A Presidential Declaration of Disaster (PDD), as defined by the Stafford Act of 1988, identifies counties eligible for federal assistance. Between 2010 and 2018, 2.841 counties have received a PDD. The Hazard Mitigation Grant (HMG) is available for local governments or state agencies after a PDD is issued for their county. Contrary to other mitigation grants program, the HMG program is not *per se* a federal program. It is rather a state and local program for which FEMA determines the total amount of available funds and ensures basic eligibility rules are respected. The total amount available under the HMG program is determined as a percentage of the total FEMA funds allocated to a State for a declared disaster[18].

Within 30 days after a disaster is declared, State emergency management agencies must send FEMA a letter of intent indicating whether or not the State will request HMG funds. Local governments and state agencies interested in applying to HMG funds must write an application project for the properties they think need to receive mitigation against potential future risks. Local governments are eligible to the Hazard Mitigation Grant Program as long as the county to which it belongs has received a Presidential Declaration of Disaster, unconditional on having been directly hit by the disaster or not. Individuals and businesses are not eligible for HMG funds, but they may request their local representatives to apply on their behalf.

State emergency management agencies then review applications for general eligibility, project cost-effectiveness, feasibility, and environmental compliance. They are also in charge of prioritizing projects in case the total amounts requested would be higher than the total amount available under HMG fundings. Later we show that media coverage at the ZIP code level does not matter for the State emergency management agencies ranking. All applications must be submitted to FEMA by State emergency management agencies within the 12 months following a Presidential Declaration of Disaster. FEMA then officially selects projects following State's agency priorities subject to the total amount allocated in HMG funds. Federally-obligated share amounts 75 percent of the total project amount.

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[18] The sliding scale formula for the determination of HMG funds (also called 'lock-in' amount) is the following: 15% of the first 2 billion dollars, 10% of the next 8 billion, and 7.5% of the next 25.33 billion. If the State for which the disaster is declared has an enhanced plan of mitigation, total funds can go up to 20% of the first 35.33 billion dollars provided for the disaster under the other FEMA programs.

The remaining 25 percent is split between non-federal entities.

When awarded with an HMG funding, sub-applicants (i.e., local governments or state agencies) are notified by their State emergency management agency and FEMA. Mitigation work may only start after receiving this notification. Project monitoring is undertaken by both the State emergency management agency and FEMA. In particular, the grant recipient must send quarterly progress reports to FEMA regional offices. Upon the project's closure, State emergency management agency staff visits the project's site to ensure conformance with the previously agreed application's scope of work.

3.4 Empirical Strategy

The empirical strategy consists in comparing (within counties having received a Presidential Disaster Declaration) areas where information circulation related to storms is high to areas where it is low, conditional on being hit by a storm or not. I am primarily interested in the number of mitigation projects implemented under the HMG Program. To proxy for information circulation and avoid endogeneity issues, I construct a measure of congruence between media markets and storms' spatial extent inspired by Snyder and Strömberg (2010). This measure is essentially a weighted average fit of the newspapers' markets to storms' spatial extents. My main identifying assumption is that the match between media markets and storm extents is haphazard. In other words, I expect the socio-economic determinants that shape media markets to be unrelated to the topographical and climate factors that would explain the exact spatial extent of a given natural disaster at the ZIP code level. I explore more thoroughly this assumption in what follows.

3.4.1 Databases

Newspapers Circulation — I collect data on newspapers' sales at ZIP code level[19]. This data was kindly provided by the Alliance for Audited

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[19] ZIP code areas are geographically defined by ZCTAs. ZCTA stands for ZIP Code Tabulation Area. It is a geographical representation of ZIP code areas computed by the Census Bureau. There are 33,144 ZCTAs covering the contiguous United-States, Alaska, Hawaï and Porto Rico.

Media (AAM), formerly known as the Audit Bureau of Circulations (ABC) for the years 2010-2018. AAM independently verifies and collects print and digital circulation of most newspapers in the U.S. and Canada. I have circulation data for 2,403 newspapers from 2010 until 2018. Newspaper's circulation appears to have dropped by 31% during this period. Such a huge decrease is in line with the figures already published in the media and by some think tanks. For each newspaper-ZIP code, I compute the average monthly circulation in a given year. I then compute the market share of each newspaper in each ZIP code, which I will use for my fit measure. Most ZIP codes (62.9%) have a normalized Herfindahl index above .25 – seemingly indicating a highly concentrated market[20].

Storms extent — Storm data is extracted from the Storm Events Database, which is maintained by the National Oceanic and Atmospheric Administration (NOAA). This database contains records on significant weather events *'having sufficient intensity to cause loss of life, injuries, significant property damage, and/or disruption to commerce'*. It is the same data NOAA uses for its monthly Storm Data Publication. I collect this storm data for the years 2010-2018. I am able to locate each episode both in time and space and thus associate each event to a given ZIP code-year. The database includes multiple subtypes of extreme climate events, which I sort in 3 categories: floods (42% of all events), wind-related events (tornados, thunderstorm, tropical storms, etc. : 34.4% of all events), and hail (18.6% of all events). The remaining 5% are mostly unclassifiable events (lightenings, dust flows, avalanches, etc.). For simplicity reasons, I will be referring to the wind-related events as 'storms.' Note that an episode might not be exclusive of a given subtype. Sometimes, the same episode is associated with a tornado, a thunderstorm, or flash floods depending on its evolution in time and space. The main reason for focusing on storms rather than floods is because the spatial extent of a storm is typically exogenous, as opposed to the spatial extent of floods which is correlated with a multitude of local patterns (i.e., the geomorphology of the terrain, the degree of impervious soil, previous mitigation actions, etc.). The Storm

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[20] This latter statistic shall be considered with caution, especially because newspapers may not be competing in the type of news they report. Moreover, even though ZIP codes are pretty small spatial units (the average land surface is 86 square miles – the equivalent of Beaumont city, Texas – and the median land area is 35 square miles – the equivalent of Manhattan, New-York), newspapers might not necessarily be competing spatially either.

Event Database also informs us about the potential damages of an event: the number of direct and indirect deaths or injured individuals, the property, and crop damages. Most storm events in my sample (97.5%) do not imply deaths nor injuries. Nevertheless, more than half (56.4%) of the ZIP codes hit by at least a storm in a given year display some property damages. The median cumulated property damage estimate for these areas is $10,000. More generally, 90% of the ZIP codes hit by at least a storm in a given year and who suffered property damages display cumulated property damage under $175,000. The other climate-related variables (rainfall, wind speed, snowfall, ...) are extracted from the Integrated Surface Database's daily summary files and the U.S. Historical Climatology Network, both hosted by NOAA.

Mitigation Projects — I gather information on HMGP mitigation accepted projects, which are publicly provided by FEMA. Thanks to a Freedom of Information Act request made to the Department of Homeland Security (2019-FEFO-00367), I complete this dataset with the denied projects. Very few projects (about 2% of the whole sample) were downturned. This is explained by the fact that the HMGP is not a competitive program (contrary to other mitigation grants provided by FEMA), and because eligibility rules are quite lenient. Among other things, I am able to observe which type of mitigation action was undertaken (wind retrofitting, structural elevation, property acquisition, etc.), which PDD and which extreme climate event is associated to the project, which type of properties are targeted (public or private, residential, owned or rented, etc.), the project's amount, the program's fiscal year, and the location's ZIP code. Most mitigation projects (68.7%) are associated with a storm event. Among all mitigation actions, the development of saferooms (almost 40% of mitigation projects) appears to be preferred. When the associated extreme climate event is a storm, this statistic goes up to 54.4%. In general, investments in structural mitigation infrastructures represent about 60% of all mitigation actions. Among non-structural mitigation investments, acquisition of lands and damaged properties appears to be the preferred option. All the storm mitigation projects and the vast majority (82.2%) of mitigation actions, in general, are subsidized through the HMG program described in the previous sections. Table C.1 of the Appendices summarizes the different types of mitigation projects undertaken during the period. Percentages in parentheses are shares to the total. Figure C.2 in the Appendices displays the location of mitigation projects.

Additional Databases — Last but not least, I compile ZIP code-level social and economic data from the Census of Population and the Zillow website. I have information on the age distribution, the racial composition, educational attainment, employment status, employment sectors, the number of housing sales, etc. The building permits data comes from the Building Permits Survey, which is also extracted from the Census website. The spatial unit is the individual permit-issuing jurisdiction. Most of them are municipalities; the remainder is counties, townships, or unincorporated towns. I have information on the number and the value of building permits emitted for both new residential units and for residential units' repairs. I will focus on the permits for new residential units. In the case of building permits emission, I aggregate my media coverage data at the permit-issuing jurisdiction level in order to approximate the average media coverage of natural disasters in these regions. Figure C.3 in the Appendices displays the location of building permits for new residences.

3.4.2 Media coverage measure

My media coverage measure is a weighted average of the fit between natural disasters spatial extent and newspaper markets:

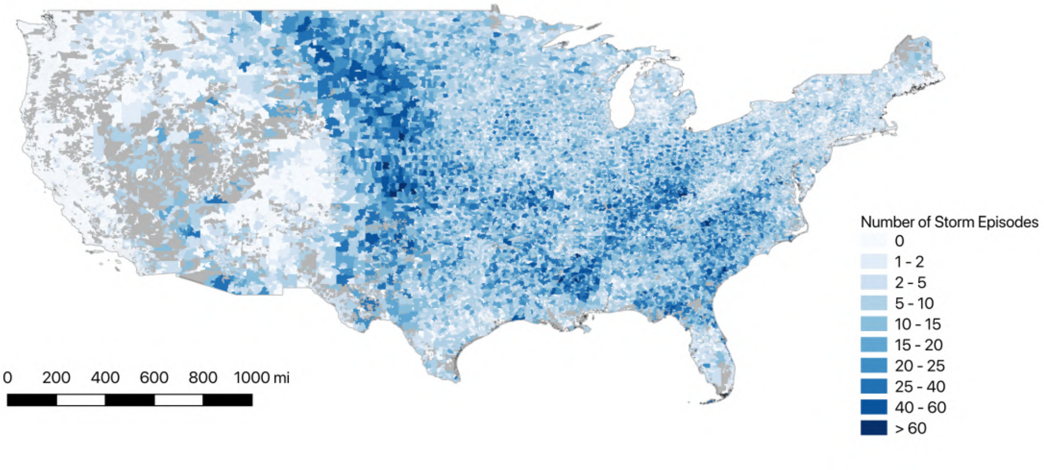
$$Coverage_{it} = \sum_{j} Fit_{jt} \times MarketShare_{ijt} \tag{3.1}$$

The *Fit* measure is computed as the ratio between the number of copies of newspaper's j distributed in a zone hit by a natural disaster in year y, to the total number of copies distributed by this newspaper in this same year. *Market Share* is simply the number of copies of newspaper j that circulate in a given ZIP code in a given year to the total circulation of newspapers in this same ZIP code and in this same year. While Fit_{jt} is intended to capture the propensity of a newspaper to report about storm events in a given year, $MarketShare_{ijt}$ is intended to capture how much of this propensity reaches a given ZIP code. This implies that if there is only one newspaper circulating in ZIP code i, $MarketShare_{ijt}$ is equal to 1, and then $Coverage_{it}$ is equal to Fit_{jt}. Note that *Coverage* is not embedding newspapers' penetration in ZIP code (i.e., the share of households receiving a given newspaper), which could be correlated with social characteristics like wage or education.

As said earlier, the main identifying assumption is that the socio-economic determinants that shape media markets are unrelated to the topographical and climate factors that would explain the exact spatial extent of any given natural disaster, such that the resulting match between these two aspects is haphazard. This is a pretty weak assumption to make to the extent that communities have little control over local meteorological variations. I provide a graphical explanation of this statement in what follows.

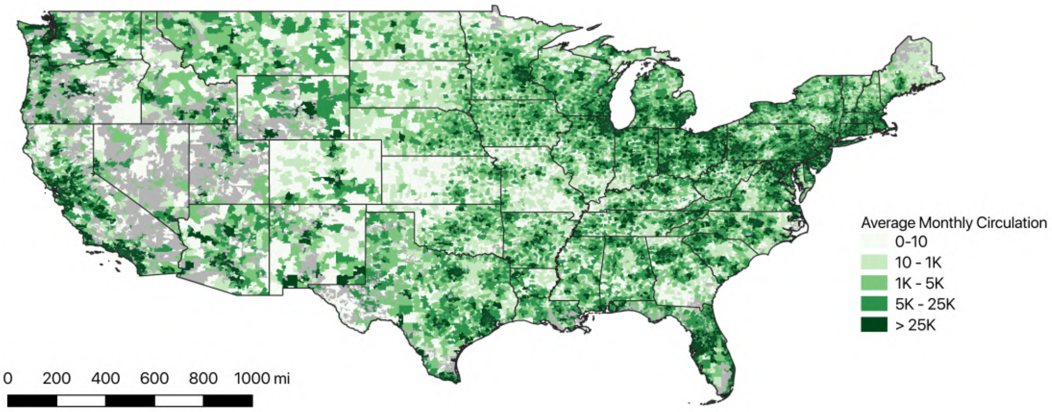
To understand how *Coverage* varies at the national level, first, consider Figure 3.3, which represents the total number of storm events identified at the ZIP code level between 2010 and 2018. What the media refers to as the *"Tornado Alley"* and the *"Dixie Alley"* are clearly visible here. Although these areas have no clear-cut borders, they both extend in the greater mid-west plains along with the Rocky and the Appalachian Mountains, respectively. Tornado outbreaks are more frequent and more violent in theses regions where the hot, humid air drifting up north from the equator meets the cold and dry mountain air. These two *alleys* display similar characteristics both in the frequency and intensity of the outbreaks. However, they differ in terms of related casualties. Indeed, Dixie Alley storms are often hidden by other meteorological phenomenons such as heavy rains. They also tend to happen more at night and on hilly terrain. These combined factors end up causing more damages and injuries. Moreover, Figure 3.3 informs us of the propensity of coastal storms, which clearly appears to be higher along the eastern shoreline. These storms are generally referred to as hurricanes, typhoons, or cyclones. Hurricanes are specific storms that form above the Atlantic or the Pacific ocean. The warm air above the ocean's surface rises, causing lower air pressure below. The air from the surrounding higher pressure areas flows in, gets warmer, rises above too, and so on. Because of Earth's axis of rotation, hurricanes in the northern hemisphere spin counter-clockwise, thereby moving west-north-west. Additionally, the Gulf Stream in the Atlantic is a constant source of warmth that triggers their formation and maintenance. This is why they often encounter the U.S. eastern shores when they form in the Atlantic ocean while drifting away from California coastline when they form in the Pacific. The need for warm air above the ocean surface is also why increasing temperatures

Figure 3.3: Storms Distribution (2010 - 2018)



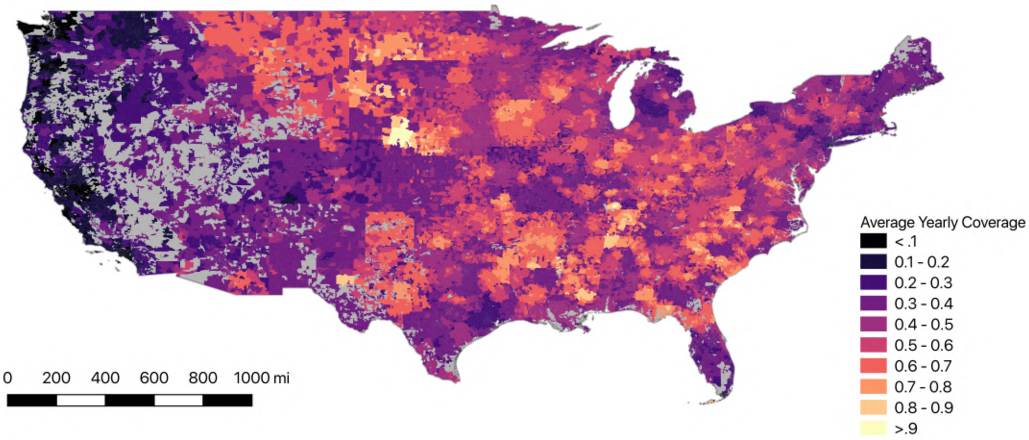
Notes: Spatial distribution of storm episodes by ZIP code areas between 2010 and 2018. In this figure, storms were summed by ZIP code over the period 2010-2018. Tornado Alley is depicted by the dark blue band east of the Rocky Mountains. Dixie Alley Data is the thiner blue band west of the Appalachians. Information on the location of storms was extracted from the NOAA Storm Events Database. Gray zones correspond to unpopulated areas.

Figure 3.4: Average Monthly Newspaper Circulation (2010 - 2018)



Notes: Spatial distribution of newspaper copies by ZIP code areas between 2010 and 2018. In this figure, monthly circulation was averaged per ZIP code over the period 2010-2018. This information was graciously provided by the Alliance for Audited Media (AAM). Gray zones correspond to unpopulated areas.

Figure 3.5: Media Coverage of Storms (2010 - 2018)



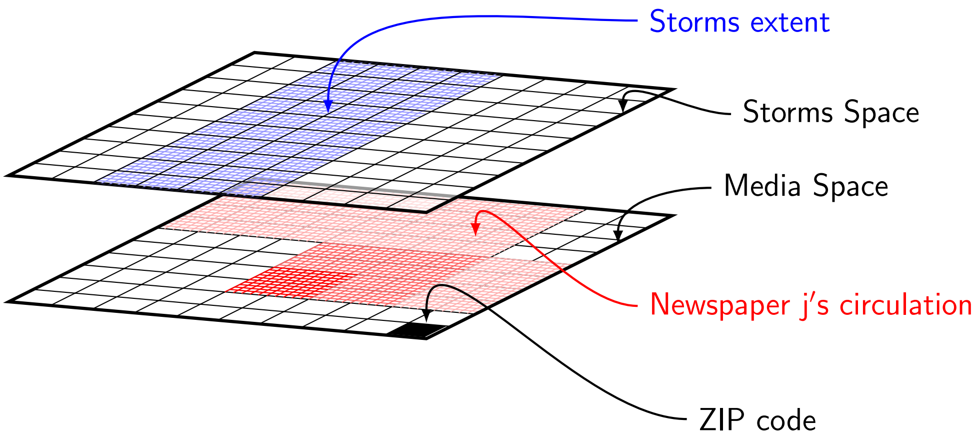
Notes: Spatial distribution of *Coverage* by ZIP code areas between 2010 and 2018. *Coverage* was computed following equation 3.1. It corresponds to the average *Fit* between storm extents and newspaper circulation. In this figure, *Coverage* was averaged per ZIP code over the period 2010-2018. Information on was graciously provided by the Alliance for Audited Media (AAM). Gray zones correspond to unpopulated areas.

caused by climate change are likely to foster the creation and intensity of those hurricanes.

Newspapers are published in cities mostly located in the north-east of the United-States (see Figure C.1 of the Appendices). Newspapers' copies then circulate all over the country, primarily to urban areas, as represented by Figure 3.4. When we interact with this information the storm distribution data, we can display the average media *Coverage* of storms as depicted by Figure 3.5. If newspaper markets were uniform, we would observe an image similar to the storm distribution. However, the main storm zones described earlier are clearly less apparent here. Regions having suffered storm events do not necessarily display high media coverage (e.g., in western Kansas), while zones apparently safer may display high coverage ratios (e.g., Pennsylvania). A closer inspection also allows us to see how *Coverage* varies greatly within states, at a very local level. Indeed, apart from some states west of the Rocky Mountains, almost every region displays *Coverage* of all degrees.

To describe how *Coverage* works at such a local level, first consider Figure 3.6. It shows newspaper j's market and zones that were hit by one or many storm events. The lighter are the red regions, the lower is j's

Figure 3.6: Example of newspaper *Fit* decomposition



Notes: Spatial decomposition of the *Fit* between storms extent and newspaper circulation. Blue areas correspond to the extent of storms over ZIP code areas in a given year. Red areas correspond to newspaper j's circulation in the same region, in the same year. Darker red ZIP codes depict areas where newspaper j's circulation is stronger. The *Fit* between newspaper j and storms extent is the ratio between the number of copies circulating in ZIP codes hit by storms and the total number of newspaper j's copies in circulation in a given year.

circulation. Clearly, this newspaper's fit would have been lower if the storm extent had been lagged by one ZIP code unit on the left, and higher if it had been lagged by one ZIP code unit on the right. If we assume that the medium red areas display a circulation twice higher than the light red areas, and the dark red areas display a circulation three times higher than the light red areas, then the $Fit_{j,t}$ is about 47.7%. The large dispersion in both the extent and the frequency of storms and the dispersion of newspaper markets ensure the high variation of *Coverage* at the local level.

The main reason for using this *Coverage* measure is that the propensity of a given newspaper to report about a given natural disaster increases with the number of readers who were impacted by this disaster. Several studies have already described such a relationship. When the fit between media markets and congressional jurisdictions is high, readers are more likely to be exposed to news related to their local politicians (Snyder and Strömberg (2010)). Similarly, when the fit is high between media markets and judicial jurisdictions, readers are more likely to be exposed to news related to court sentencing (Lim, Snyder, and Strömberg (2015)). The rationale driving this assumption is that newspapers report noteworthy news, that is to say, news their readers are interested in. In other words, major newspapers, like the *New-York Times*, are less likely to report about small tornado

Table 3.1: Effect of *Newspaper Coverage of Storms*

	Articles about storms		
	(1)	(2)	(3)
Newspapers' *Fit*	0.699***	0.694***	0.710***
	(0.232)	(0.232)	(0.234)
Weather Controls	N	Y	Y
Socio-Economic Controls	N	N	Y
Observations	3,753	3,753	3,753
R^2	0.42	0.42	0.42

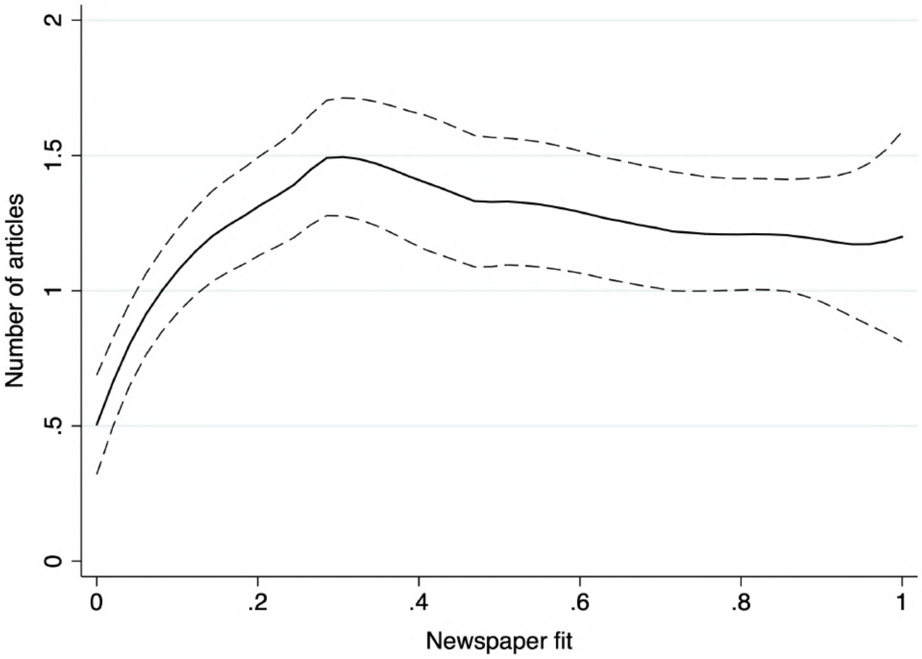
Note: The dependent variable is the number of articles mentioning *'storm'* in its headline (μ=1.47 ; s=.06). The unit of observation is a newspaper's market by year. All regressions include year and newspaper fixed effects and standard errors are robust to heteroskedasticity. *: p < 0.1 ; **: p < 0.05 ; ***: p < 0.01. Standard errors are reported in parentheses.

events that only hit 1% of their readers, but they are more likely to report about major hurricane events. Similarly, a small local newspaper, like *The Milford Times* (Milford, MI), is more likely to report about an extreme climate event hitting their readership than to write about an equivalent event located in Hot Springs, Arkansas.

To test for this relationship, I scrapped news articles mentioning at least the word *'storm'* in its headline and *'weather'* in their main body from the website *Newslibrary.com.* I was able to associate journals' names to their circulation data for 417 newspapers. Table 3.1 presents the results of regressing the number of articles about storms against Fit_{jt}. Column (2) and (3) include several controls that are likely to influence the coverage of storms. These regressors are related to the local weather (extreme precipitations, winds, and temperatures), economic and social conditions (log population, the share of population above 60 years old, educational attainment, unemployment). Table 3.1 informs us that a strong positive relationship exists between the number of articles published about storms and my variable of interest, the newspapers' fit to storm extents. Indeed, when the fit between storms and newspaper markets is perfect, newspapers tend to publish more articles about storms (about .7 more). In other words, the number of articles reporting about storms increases by almost 47% of its mean when the fit between newspaper markets and storms increases from 0 to 1. This relation appears to be relatively robust to the introduction of the aforesaid controls and remains significantly different from zero at the 1%-level in all specifications.

Figure 3.7 presents a local polynomial depiction of this relationship.

Figure 3.7: Relation between newspapers' *Fit* and the Number of articles mentioning storms



Note: Local polynomial fit of the relation between newspaper j's circulation and storms' spatial extents in t, Fit_jt, and the number of articles published by the mentioning *'storm'* in its headline (μ=1.47 ; s=.06). The unit of observation is a newspaper's market by year. Dashed lines correspond to the 95% confidence interval bounds.

3.4.3 Empirical design

My main variable of interest is *Coverage*. In order to facilitate its interpretability, I center *Coverage* at its ZIP code mean. An increase in my treatment captures a positive information shock about storms with respect to the average amount of news a location receives about storms. I build on a reduced-form setting to analyze the impact of such information shock on local governments' mitigation initiatives when their county receives a Presidential Declaration of Disaster. My main empirical model studies the impact of *Coverage* on the number of mitigation projects undertaken under the HMGP conditional on being hit by a storm:

$$\begin{aligned} Mitigation_{it} = \beta_1.Coverage_{it} + \beta_2.Storm_{it} + \beta_3.[Coverage_{it} \times Storm_{it}] \\ + \delta.X_{it} + \alpha_i + \gamma_{ct} + \epsilon_{it} \end{aligned}$$

where *Mitigation* is the number of mitigation infrastructures projects undertaken in a given ZIP code i in year t. *Storm* is a dummy variable equal to one if the ZIP code has been hit by at least one storm in year t. The regression model includes ZIP code area fixed effects, α_i, and county-year fixed effects, γ_{ct}. I match county and ZIP code areas with the amount of residential addresses located in a county. About 80% of all ZIP codes areas belong entirely to a single county, and more than 93% of all ZIP codes have at least 75% of their residential addresses identified in a single county, and less than 1% have less than half of their residential addresses in a single county. Finally, X is a vector of control variables, which I think could possibly impact both on media $Coverage$ and the demand for $Mitigation$. These controls include information on (1) demographics (population, age, immigration, racial distribution, newspapers' readership); (2) income and education (household revenues, distribution by educational attainment); (3) labor composition (labor force, employment, sectors of employment); (4) weather (average temperatures, wind speed, and precipitations); and finally (5) the housing composition (number of housing units, sales, median value, the number of vacant units, occupied by their owner, with mortgage status, and the unit's age). The main descriptive statistics of these variables for the sample of counties having received a Presidential Declaration of Disaster are available in Tables C.2 to C.4 of the Appendices.

3.5 Results

3.5.1 Main Results: Impact on mitigation initiatives

Table 3.2 reports the impact of $Coverage$ on the number of mitigation projects implemented under the HMGP. Columns (1) - (2) report the unconditional effect of $Coverage$, columns (3) - (4) the unconditional effect of $Storm$, and columns (5) - (6) report the interaction of both terms. The results indicate that both $Coverage$ and $Storm$ have a significant positive unconditional impact on the number of mitigation actions occurring at a given location. Yet, this effect appears to be mostly driven by the interaction term. It is quite substantial: conditional on being hit by a storm, a one standard deviation increase in $Coverage$ increases the number of mitigation projects by 54% of its average.

The magnitude of these latter results indicates that local governments' decision to take mitigation initiatives is very responsive to how the information about the risks in the jurisdiction circulates. Neither an increase

Table 3.2: Impact on mitigation initiatives

	All Mitigation Projects					
	(1)	(2)	(3)	(4)	(5)	(6)
Coverage	0.113***	0.114**			-0.030	-0.054
	(0.042)	(0.049)			(0.045)	(0.060)
Storm			0.021**	0.024**	-0.003	-0.003
			(0.009)	(0.010)	(0.010)	(0.011)
Coverage × *Storm*					0.443***	0.486***
					(0.131)	(0.147)
ZIP Code FE	Y	Y	Y	Y	Y	Y
County-year FE	Y	Y	Y	Y	Y	Y
All Controls	N	Y	N	Y	N	Y
Baseline Population	N	Y	N	Y	N	Y
Observations	82,360	82,360	82,360	82,360	82,360	82,360
R^2	0.57	0.57	0.57	0.57	0.57	0.57

Note: *Coverage* is centered at its mean (μ=0 ; s=0.101). The outcome variable includes the total number of properties having received mitigation against future storms under the HMG program between 2010 and 2018 (μ=0.083 ; s=0.975). *: p < 0.1 ; **: p < 0.05 ; ***: p < 0.01. Standard errors are clustered at the county level and reported in parentheses.

above mean *Coverage* conditional on not experiencing a storm nor experiencing a storm at mean *Coverage*, has any significant effect on the number of mitigation projects. These results are consistent with the theory: jurisdictions affected by the storm remain virtually risk-free to investors in the absence of any information shock. In this case, local governments have fewer incentives to take mitigation actions. Note that the combination of these findings also implies that local governments are not incentivized to take mitigation action because local voters are updating their beliefs about storm risks. Indeed, in this case, experiencing a storm in the absence of news reports would affect the mitigation decision.

3.5.2 Design Validity

Selection on covariates — A major identification concern would arise if pre-determined variables were to be correlated with either *Storm* or *Coverage*. Tables C.5 - C.10 present balance tests for more than 60 pre-treatment characteristics related to the local demography, labor, composition income and education levels, as well as housing markets. Tables C.5 - C.7 investigate these correlations for *Storm* and *Coverage*, respectively;

while tables C.8 - C.10 look at these same correlations when following the main specification. All tables report means and standard deviations of the pre-treatment covariates in the last column.

Overall, these tests are quite reassuring. The *Storm* dummy is balanced across almost all characteristics, and the few unbalances are both economically and statistically insignificant above the 10%-level when controlling for county-year fixed effect. As in Snyder and Strömberg (2010), and Lim, Snyder, and Strömberg (2015), I find that my *Coverage* measure is mechanically correlated with changes in log population, the share of elderlies, and the share of the population employed in the agricultural sector[21]. These correlations are expected as a newspaper's market share tends to be lower in densely populated areas, where competition for readership is strong. Additionally, regions with a lower density of population also tend to be on average older and working more in the agricultural sector. Snyder and Strömberg (2010) argue that using the temporal variation in jurisdiction redistricting[22] to include location fixed-effects mitigates the risk of omitted variable bias (OVB) related to population and location characteristics. However, in this case, using the temporal variation in storms' spatial extent does not fully alleviate this possibility. That said, controlling for baseline population removes most of the correlations with pre-treatments, as with the share of elderlies. Generally, my *Coverage* treatment is balanced across almost all characteristics, and only a couple of unbalances remain out of 61 covariates. Testing for the joint orthogonality of these variables against my treatment (and omitting a category when suited to avoid collinearity issues) returns an insignificant F-statistic ($F_{56,1774} = 1.25$, $p > 0.1$).

While these results are encouraging, I cannot fully exclude the risk of omitted variable bias. Following Altonji, Elder, and Taber (2005), I further infer the degree of selection on unobservable based on the selection on observables[23]. To this end, I study whether the variation of *Mitigation* predicted by observable characteristics (excluding location and region-year

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[21] Snyder and Strömberg (2010) and Lim, Snyder, and Strömberg (2015) control for the share of the rural area instead of the share of the population working in the agricultural sector.

[22] The treatment variable used in Snyder and Strömberg (2010) is a weighted average across newspapers of the share of readers belonging to a specific congressional district. It is computed at the county level.

[23] This procedure is also followed by Lim, Snyder, and Strömberg (2015), who do not have any exogenous temporal variation in redistricting since they study judicial districts and that media markets are quite immobile through time

Table 3.3: Selection on unobservables

	$\widehat{Mitigation}$					
	(1)	(2)	(3)	(4)	(5)	(6)
Coverage	0.005	0.002			0.005	0.002
	(0.004)	(0.002)			(0.004)	(0.003)
Storm			-0.001	-0.001	-0.001	-0.001*
			(0.000)	(0.000)	(0.000)	(0.000)
Coverage ×*Storm*					0.002	0.002
					(0.003)	(0.003)
ZIP Code FE	Y	Y	Y	Y	Y	Y
County-year FE	Y	N	Y	N	Y	N
State-year FE	N	Y	N	Y	N	Y
Observations	82,360	82,360	82,360	82,360	82,360	82,360
R^2	0.92	0.91	0.92	0.91	0.92	0.91

Note: *Coverage* is centered at its mean (μ=0 ; s=0.101). The outcome variable includes the total number of properties having received mitigation against future storms under the HMG program between 2010 and 2018, as predicted by observable charactistics (μ=.0745 ; s=0.091). *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$. Standard errors are clustered at the county level and reported in parentheses.

fixed effects), $\widehat{Mitigation}$, is correlated with the variation of my main treatments, *Coverage* and *Storm*. I use the large set of covariates presented in Tables C.5 to C.10, excluding a variable by category of regressors to avoid collinearity issues. Results are reported in Table 3.3. The high R^2 statistic indicates that almost all of the variation of $\widehat{Mitigation}$ is indeed captured by location fixed effects. It is reassuring to see that there is little if no selection on these observables characteristics, which considerably alleviates the fear of cofounding factors.

Finally, the inclusion of the different categories of control variables in my main specification does not change the estimates significantly – which further mitigates the concern for potential cofounding factors (see Table C.11 in the Appendices).

Placebo tests — Table 3.4 present the results for a battery of different placebo tests. The first two panels look at the unconditional effect of *Coverage* and *Storm*, respectively, and the third type of regression focuses on the interaction of both coefficients. In Columns (1)-(2) are the results when the outcome is lagged by one period. Columns (3)-(4) present the

Table 3.4: Placebo tests

	All Mitigation Projects					
	Lagged Outcome		Shuffled Treatment		Fake Treatment	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Unconditional Effects of Coverage						
Coverage	0.046	0.024	-0.053	-0.050	0.019	0.010
	(0.037)	(0.048)	(0.052)	(0.065)	(0.033)	(0.036)
Panel B: Unconditional Effects of Storm						
Storm	-0.008	-0.012	-0.007	-0.007	-0.004	-0.007
	(0.010)	(0.012)	(0.008)	(0.009)	(0.010)	(0.011)
Panel C: Main Specification						
Coverage	0.074	0.058	-0.046	-0.046	0.001	-0.004
	(0.046)	(0.058)	(0.054)	(0.070)	(0.040)	(0.044)
Storm	-0.005	-0.008	-0.007	-0.007	-0.004	-0.007
	(0.010)	(0.011)	(0.009)	(0.009)	(0.010)	(0.011)
Coverage × *Storm*	-0.078	-0.092	-0.020	-0.012	0.072	0.053
	(0.068)	(0.073)	(0.073)	(0.076)	(0.055)	(0.059)
ZIP Code FE	Y	Y	Y	Y	Y	Y
County-year FE	Y	Y	Y	Y	Y	Y
All Controls	N	Y	N	Y	N	Y
Observations	62,329	62,329	82,360	82,360	82,360	82,360
R^2	0.47	0.49	0.57	0.57	0.57	0.57

Note: The main coefficients are as in Table 3.2. Standard errors are clustered at the county level and reported in parentheses. *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$. Standard errors in parentheses.

results with the predicators of interest being randomly attributed to another ZIP code area within the same county. Finally, in columns (5) - (6), I generate a fake random *Storm* dummy with the same mean and standard deviation of the original variable. With these new placebo storms, I recompute the corresponding placebo *Coverage*, thus generating a pair of fake treatments. The results are reassuring overall. None of these coefficients are significantly different from zero, and some even take a different sign compared to the main regression table. The results suggest that *Coverage* is neither subject to any anticipatory effects nor is it correlated with the level of mitigation in a random ZIP code area, even when this latter lies in the same county.

State Selection — One potential source of concern arises when considering the selection of mitigation projects. Indeed, States' administrations are an intermediary in the attribution of the HMG grants. If the total hazard mitigation funding allocated after a declared disaster is lower than the total funds requested, then the selection of the mitigation projects will be based on the State's emergency agency priorities. This may be a problem if States' priorities are somehow determined by *Coverage*. As a matter of fact, the literature has already demonstrated that differences in media coverage of national events matter for political priorities, thereby generating a bias in the targeting of public transfers. However, this literature looks at media coverage of specific events in regions administratively close to the government deciding of the transfer. For instance, Snyder and Strömberg (2010) looks at transfers overseen by Congress representatives to specific counties in the congressional jurisdiction. Strömberg (2004) looks at how state administration distributed economic federal relief grants given radio distribution in counties as well. I essentially study the same process but for much smaller geographies (ZIP codes). For this reason, it seems unlikely that States' emergency agencies account for the level of *Coverage* at the ZIP code level. Indeed, there are, on average, 650 ZIP code areas per state, and I already control for county-year variation. It seems unlikely that state emergency administration specifically targets a ZIP code area based on *Coverage*.

That being said, in order to fully convince that the main results are indeed driven by local governments and not by any upper-government administration, I split my sample between grant recipients: I reproduce my empirical specification only considering local administration subgrantees[24] and only state administration subgrantees, respectively. Projects with a state administration subgrantee represent about 25% of all projects. The reason for doing this is that if state administrations indeed prioritize storm mitigation projects according to the local media coverage of storms, then we should expect a positive and significant impact of *Coverage* on the number of projects whom subgrantee is a state administration. Table 3.5 presents such results for mitigation projects. As expected, the variable of interest does not seem to have any influence on the State's decision to apply for mitigation projects – as compared to local government's applications. Although I cannot exclude that State administrations select projects on other aspects, it is at least reassuring to know these projects are not selected based on local media coverage. Additionally, this reinforces the fact

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[24] In an HMG procedure, the subgrantee is the administration in charge of applying for the grant. In case of approval, it is also the administration in charge of managing the funds and supervising the project at the community level.

Table 3.5: State Selection

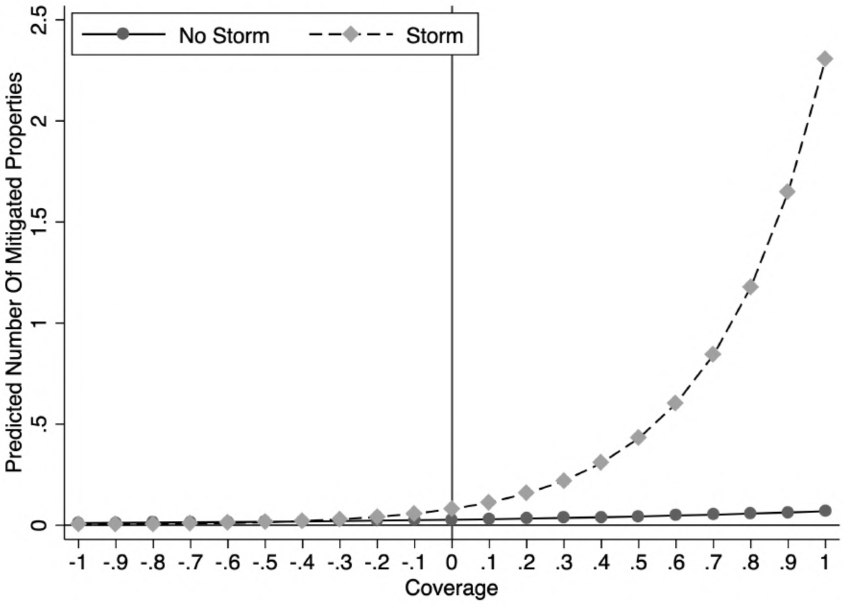
	All Mitigation Projects					
	(1)	(2)	(3)	(4)	(5)	(6)
Coverage	-0.008	-0.018			0.001	0.002
	(0.041)	(0.056)			(0.053)	(0.067)
Storm			-0.019	-0.022	-0.018	-0.020
			(0.015)	(0.018)	(0.013)	(0.016)
$Coverage \times Storm$					-0.007	-0.031
					(0.068)	(0.074)
ZIP Code FE	Y	Y	Y	Y	Y	Y
County-year FE	Y	Y	Y	Y	Y	Y
All Controls	N	Y	N	Y	N	Y
Baseline Population	N	Y	N	Y	N	Y
Observations	82,360	82,360	82,360	82,360	82,360	82,360
R^2	0.38	0.38	0.38	0.38	0.38	0.38

Note: Coverage is centered at its mean (μ=0 ; s=0.101). The outcome variable includes the total number of properties having received mitigation against future storms under the HMG program between 2010 and 2018 when the subgrantee was a State administration (μ=0.007 ; s=0.733). Standard errors are clustered at the county level and reported in parentheses. *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$. Standard errors in parentheses.

that the theory is actually suited for local government dynamics rather than for any upper-level administration.

Exploring Non-Linearities — Although fixed effects provide a flexible approach to estimating the impact of *Coverage* on new mitigation initiatives, the results might be complicated to grasp because of the outcome distribution. Because the number of mitigation projects is count data that exhibits both overdispersion and excess zeros, I also estimate a zero-inflated negative binomial model. Zero-inflated models assume that excess zeros are generally generated by an independent process that can be modeled separately. Here, I use population density as the main predictor of excess zeros. Figure 3.8 displays the impact of *Coverage* conditional on being hit by a storm on the number of mitigation projects, as predicted by this empirical approach. Like in the main table, a positive shock above mean *Coverage* has a strongly significant positive effect on the number of mitigation projects. Local governments do not seem to implement any mitigation measure under mean *Coverage*. Note that most of the effect occurs

Figure 3.8: Effect of *Coverage* conditional on *Storm*



Notes: Marginal impact of Coverage conditional on Storm when estimated with a zero-inflated negative binomial model.

at high levels of *Coverage*, which is consistent with the theory. Indeed, high levels of the resident population require high levels of information shock to motivate mitigation action. In the United States, about 83% of all housing units are occupied, and 73.5% of them are occupied by their owner.

3.5.3 Impact on the local housing markets

The theory also suggests that local governments react to storm coverage because prospective investors respond to increased media attention by shifting their demand towards what appears as safer places. In this case, we should expect a negative impact on both housing sales and housing values driven by places having received strengthen media attention. Consequently, a revenue-maximizing local government whose jurisdiction has been hit by a storm will emit less building permits when information about storm risks circulates. The impact of *Coverage* on the number of housing sales, the number of building permits emitted by a permit-issuing jurisdiction, and on the subsequent property tax revenues collected by the local government

Table 3.6: Housing sales

	Log Number of Property Sales (Zillow)					
	(1)	(2)	(3)	(4)	(5)	(6)
Coverage	-0.001	0.005			0.061	0.059
	(0.036)	(0.040)			(0.038)	(0.043)
Storm			-0.007	-0.006	0.003	0.002
			(0.006)	(0.006)	(0.006)	(0.007)
Coverage×*Storm*					-0.188***	-0.157***
					(0.044)	(0.046)
ZIP Code FE	Y	Y	Y	Y	Y	Y
County-year FE	Y	Y	Y	Y	Y	Y
All Controls	N	Y	N	Y	N	Y
Baseline Population	N	Y	N	Y	N	Y
Observations	82,360	82,360	82,360	82,360	82,360	82,360
R^2	0.96	0.96	0.96	0.96	0.96	0.96

Note: *Coverage* is centered at its mean (μ=0 ; s=0.101). The outcome variable includes the log number of properties sold in a ZIP code area between 2010 and 2018 when the related county received a Presidential Disaster Declaration (μ=3.176 ; s=2.049). Standard errors are clustered at the county level and reported in parentheses. *: p < 0.1 ; **: p < 0.05 ; ***: p < 0.01. Standard errors in parentheses.

Table 3.7: Housing Supply

	Log Number of Building Permits (new residence)					
	(1)	(2)	(3)	(4)	(5)	(6)
Coverage	-0.000	0.004			0.020	0.024
	(0.023)	(0.024)			(0.024)	(0.025)
Storm			-0.006	-0.006	-0.003	-0.003
			(0.005)	(0.005)	(0.005)	(0.005)
Coverage × *Storm*					-0.083***	-0.079***
					(0.029)	(0.030)
ZIP Code FE	Y	Y	Y	Y	Y	Y
County-year FE	Y	Y	Y	Y	Y	Y
All Controls	N	Y	N	Y	N	Y
Baseline Population	N	Y	N	Y	N	Y
Observations	180,623	180,623	180,623	180,623	180,623	180,623
R^2	0.91	0.91	0.91	0.91	0.91	0.91

Note: *Coverage* is centered at its mean (μ=0 ; s=0.145). The unit of observation is a permit-issuing jurisdiction per year. The outcome variable includes the log number of new residential building permits issued by a permitting jurisdiction, between 2010 and 2018 (μ=1.6 ; s=1.667). A spatial distribution of these building permits is presented in Figure C.3 of the Appendices. Standard errors are clustered at the county level and reported in parentheses. *: p < 0.1 ; **: p < 0.05 ; ***: p < 0.01. Standard errors in parentheses.

in the following year, are presented in Table 3.6, 3.7, and 3.8, respectively. Like in the previous tables, columns (1) - (2) report the unconditional effect of *Coverage*, columns (3) - (4) the unconditional effect of *Storm*, and columns (5) - (6) report the interaction of both terms. Once the location and county-year fixed effects are discounted, a *Storm* impacts negatively the housing markets only if when is a positive information shock. Neither being hit by a storm at mean *Coverage* nor an increased *Coverage* in the absence of a storm has any impact on the housing markets. However, conditional on being hit by a storm, a one standard deviation increase in *Coverage* decreases the number of housing sales and property tax revenues by 1.5-1.8%, and the number of newly emitted building permits by .8%. These results suggest that local governments accommodate the demand shift generated by the information shock through a decreased housing supply when prospective investors become aware of the risks.

3.5.4 Heterogenous analysis

So far, I have argued that because they wish to protect property values, local governments underinvest in mitigation measures to avoid disclosing

Table 3.8: Housing taxes

	Lead Log Property Tax Revenues					
	(1)	(2)	(3)	(4)	(5)	(6)
Coverage	-0.034	-0.001			0.015	0.050
	(0.064)	(0.063)			(0.071)	(0.070)
Storm			0.000	0.000	0.006	0.007
			(0.005)	(0.005)	(0.006)	(0.006)
Coverage×*Storm*					-0.148***	-0.156***
					(0.046)	(0.046)
ZIP Code FE	Y	Y	Y	Y	Y	Y
County-year FE	Y	Y	Y	Y	Y	Y
All Controls	N	N	N	N	N	N
Baseline Population	N	Y	N	Y	N	Y
Observations	71,505	71,505	71,505	71,505	71,505	71,505
R^2	0.96	0.96	0.96	0.96	0.96	0.96

Note: Coverage is centered at its mean (μ=0 ; s=0.101). The outcome variable includes the aggregated property tax revenues in the aftermath of a Presidential Declaration of Disaster (μ=14.16 ; s= 2.30). Standard errors are clustered at the county level and reported in parentheses. *: p < 0.1 ; **: p < 0.05 ; ***: p < 0.01. Standard errors in parentheses.

their jurisdiction's latent risks to otherwise uninformed individuals. First of all, if this signaling assumption is true, we should observe that the effect of media coverage on mitigation actions is stronger when these mitigations are actually observable. This is the case of mitigation infrastructures, which take time to build, and which are built to last over the years. Because it is more costly for potential homebuyers to investigate ownership – and infer location's risk from this ownership, *Coverage* should not matter much for non-structural actions like land acquisitions. To study the differences between non-structural and structural mitigation, I split the sample between properties that received infrastructure projects and properties that were subject to acquisition projects. The results are presented in Table 3.9.

As expected, structural mitigation projects drive the main results. Conditional on being hit by a storm, a one standard deviation increase in *Coverage* increases the number of properties receiving mitigation infrastructures by almost 77% of its average. On the contrary, the interaction term does not seem to have any significant effect on land acquisitions. These findings support the idea that local policymakers are reluctant to undertake mitigation projects when the risks are ignored, especially if the

Table 3.9: Heterogenous analysis by Mitigation type

	All Projects		Infrastructures		Land acquisitions	
	(1)	(2)	(3)	(4)	(5)	(6)
Coverage	-0.030	-0.054	0.026	0.012	-0.028	-0.033
	(0.045)	(0.060)	(0.032)	(0.039)	(0.026)	(0.039)
Storm	-0.003	-0.003	-0.014**	-0.013*	0.008	0.008
	(0.010)	(0.011)	(0.007)	(0.007)	(0.006)	(0.007)
Coverage × *Storm*	0.443***	0.486***	0.353***	0.346***	0.097	0.134
	(0.131)	(0.147)	(0.099)	(0.111)	(0.083)	(0.095)
ZIP Code FE	Y	Y	Y	Y	Y	Y
County-year FE	Y	Y	Y	Y	Y	Y
All Controls	N	Y	N	Y	N	Y
Baseline Population	N	Y	N	Y	N	Y
Observations	82,360	82,360	82,360	82,360	82,360	82,360
R^2	0.57	0.57	0.69	0.71	0.42	0.43

Note: *Coverage* is centered at its mean (μ=0 ; s=0.101). *Infrastructures* includes the total number of properties having received structural mitigation against future storms under the HMG program between 2010 and 2018 (μ=0.0457 ; s= 0.705). *Land acquisitions* includes the total number of properties having been subject to a complete or a partial land acquisition under the same program (μ=.0265 ; s=0.621). Standard errors are clustered at the county level and reported in parentheses. *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$. Standard errors in parentheses.

so-called project is more likely to signal the presence of risks.

The theory presented in Section 3.2 suggests that the impact of new information about local risks on governments' mitigation initiatives is first and foremost driven by mobile non-residents. The larger the share of non-residents, the smaller the information shock needs to be to motivate the government to prepare for a disaster. In this setting, residents are less responsive to the information shock because they are tied to their hometowns. Although there is no proper way to empirically measure individuals' mobility or individuals' bounds to their town of residence in the wake of a natural disaster, I can interact with my coefficients different pre-treatment variables positively correlated to these aspects. Table 3.10 then presents the interaction of the *Coverage* and *Storm* regressors with pre-treatment measures of (1) the log number of vacant housing units, (2) the log number of renter-occupied housing units, (3) the log number of housing units owned with a mortgage, (4) a dummy for an above-median inflow of population in the ZIP code, (5) the log of the median household income, and (6) a dummy for positive growth in real-estate tax revenues in the ZIP code.

First, the main results from 3.2 seem to be driven by ZIP codes areas with high levels of vacant housing units, housing units occupied by renters, housing units owned with a mortgage, and by ZIP code areas having experienced immigration above their median levels previous to the treatment. In particular, conditional on being hit by a storm and for every one-standard-deviation increase in *Coverage*, a 1% increase in the pre-treatment number of vacant units in a ZIP code lead to a 0.47% increase in the subsequent average number of mitigation projects. Under these conditions, a 1% increase in the pre-treatment number of renter-occupied units, and housing units owned with a mortgage, lead to a 0.35% and a 0.5% increase respectively, in the subsequent average number of mitigation projects. Additionally, *Coverage* seems to affect preparation efforts in places that recently experienced above-median immigration levels. In particular, conditional on being hit by a storm, a one-standard-deviation increase in *Coverage* leads to a 120% increase in the average number of mitigation projects in these ZIP codes compared to ZIP codes that experienced below-median immigration before the treatment. Finally, note that unconditional on being hit by a storm, the impact of media coverage on mitigation efforts is larger when households residing in the ZIP code were richer previous to the treatment (see column (5)).

Table 3.10: Heterogenous analysis by Pre-Treatment X

	All Mitigation Projects					
	(1)	(2)	(3)	(4)	(5)	(6)
Coverage	-0.229*	-0.357**	-0.413***	0.047	-2.561***	0.018
	(0.132)	(0.168)	(0.156)	(0.056)	(0.837)	(0.060)
Coverage × *X*	0.050*	0.082**	0.083***	–0.133	0.230***	-0.079
	(0.029)	(0.037)	(0.031)	(0.082)	(0.075)	(0.081)
Storm	-0.004	-0.014	-0.018	-0.053***	0.264	-0.036**
	(0.037)	(0.031)	(0.037)	(0.018)	(0.300)	(0.016)
Storm × *X*	-0.001	0.001	0.000	0.074***	-0.025	0.042**
	(0.007)	(0.006)	(0.007)	(0.024)	(0.027)	(0.021)
Storm × *Coverage*	-1.664***	-1.282**	-2.274***	-0.015	0.071	0.114
	(0.636)	(0.603)	(0..881)	(2.228)	(0.187)	(0.131)
Storm × *Coverage* × *X*	0.390***	0.289**	0.413***	0.959***	0.045	0.673***
	(0.130)	(0.115)	(0.148)	(0.312)	(0.203)	(0.254)
X	-0.015**	0.005	-0.031***	-0.022***	0.011	-0.002
	(0.007)	(0.006)	(0.012)	(0.008)	(0.011)	(0.006)
ZIP Code FE	Y	Y	Y	Y	Y	Y
County-year FE	Y	Y	Y	Y	Y	Y
All Controls	N	N	N	N	N	N
Baseline Population	N	N	N	N	N	N
Observations	62,329	62,329	62,329	62,329	62,329	62,329
R^2	0.62	0.62	0.62	0.62	0.62	0.62

Note: The main regressors descriptive statistics are as in Table 3.2. X is a pre-treatment variable corresponding to: (1) the log number of vacant housing units, (2) the log number of renter-occupied housing units, (3) the log number of housing units owned with a mortgage, (4) a dummy for an above-median inflow of population in the ZIP code, (5) the log of the median household income, and (6) a dummy for a positive growth in real-estate tax revenues in the ZIP code. The impact of an increased media attention on the mitigation efforts. Standard errors are clustered at the county level and reported in parentheses. *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$. Standard errors in parentheses.

These results are in line with the theory, which states that larger shares of mobile individuals, and generally lower migration costs, foster mitigation initiatives. Non-resident investors, who seek to derive a rental income[25], could either react to the information shock by diverting their investment towards places that appear safer to renters – or if they have already acquired their property, pressuring the local administration in taking mitigation action to maintain their previous rents. The local government would then be incentivized to invest in mitigation projects to preserve revenues from

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[25] As mentioned earlier, these investors typically live near their housing investments (20 miles in 2013), and a majority (37%) seeks to derive rental income[26].

housing taxes and sales.

As a matter of fact, ZIP code areas that experienced growing real-estate tax revenues before the treatment were more likely to receive mitigation infrastructures (see column (6) in Table 3.10). In particular, conditional on being hit by a storm, a one standard-deviation increase in $Coverage$ lead to an 82% increase in the average number of mitigation projects in these ZIP codes compared to ZIP codes that did not experience growing real-estate tax revenues before the treatment.

3.6 Conclusion

In this paper, I study the impact of media coverage on the implementation of local resilience policies – namely, mitigation investments – designed to reduce the risks of future natural disasters. I challenge the view that investments in mitigation actions are solely driven by the objective risk. I make the central assumption that these mitigation actions might signal the true risk to potential investors. Because of this risk-signaling process, local governments who seek to protect housing values in their jurisdiction are reluctant to invest in mitigation infrastructures when investors are not aware of the risks.

The main results suggest that conditional on being hit by a storm, more mitigation projects are implemented when information about storms does circulate. I interpret this result as meaning that local policymakers are not prone to disclose risks through mitigation when prior risk information is inexistent. A one standard deviation increase in the treatment variable leads to an increase of 54% of the average number of mitigation projects in a ZIP code area that suffered a storm. In the absence of increased media coverage, jurisdictions hit by the disaster do not implement mitigation projects. Additionally, the number of housing sales, the revenues from property taxes, and the number of building permits issued in the affected jurisdictions decrease significantly when newspaper coverage is higher than its mean. This suggests that developers and prospective investors might be reacting to the demand shock by shifting their demand towards virtually risk-free locations. Local governments whose jurisdiction suffers from this redistribution are incentivized to invest in mitigation technologies to signal that the risks are under control – when investors are informed about this risk. These findings seem to be primarily driven by structural projects – which supports the risk-signaling channel; and by places with high pre-treatment levels of vacant housing units, housing units occupied by renters, or housing units owned with a mortgage, seemingly indicating that non-resident property investors are the ones primarily reacting to the information shock. This latter result brings up new insights as well as new questions about the potential capture of local disaster preparation policies by developers and real-estate buyers.

Overall, this paper provides novel evidence on local governments' motivations to prepare for natural disasters. In particular, local information distribution appears to be paramount to explain policymakers' incentives to make their jurisdiction resilient to storms. Less informed places end up being more vulnerable, thereby fostering spatial inequalities in the capacity to resist the consequences of climate change. Considering both the decline of local news and the increase in the frequency of natural disasters, these results could help designing more comprehensive mitigation policies both at the local and federal governments levels.

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Appendices

A Proof of Proposition 3.1

The timeline is described in Section 3.2. We are interested in unraveling the local government's best mitigation strategy given an exogenous information shock on the local disaster dangers, *all else being equal.* In particular, we assume the distribution of rent prices *before* the information shock is similar in both municipalities, i.e., $p = p_i = p_{-i}$. Several papers in the literature study how the information of a local natural disaster is dynamically captured in prices (Barrage and Furst, 2019; Coulomb and Zylberberg, 2019; Singh, 2019; Bakkensen and Barrage, 2017). We further assume that the information shock occurs immediately after the natural disaster, so we study the agents' decision in the immediate aftermath of the catastrophe. Finally, we acknowledge that in this setting, only the local authorities can invest in public mitigation, but one can easily extend the framework to allow sophisticated residents to invest in private mitigation without changing the main results. The analysis then consists in comparing each government's payoffs in the following cases: when (1) $m_i = m_{-i} = 0$, (2) $m_i > 0$ and $m_{-i} = 0$, respectively when (3) $m_i = 0$ and $m_{-i} > 0$, and finally, when $m_i = m_{-i} > 0$. This is done by inferring the individuals' sorting in city $i \in \{A; B\}$ from their *anticipated* surplus. This anticipated surplus will depend on whether the individual is a resident (r), a non-resident (nr), sophisticated (s) or myopic (m).

- **Case 1:** $m_i = m_{-i} = 0$

  The net anticipated surplus for each group of individuals is:

$$\begin{cases} x_{i,s,nr} = [-p_i - \hat{p}] - [-p_{-i} - \hat{p}] = 0 \\ x_{i,m,nr} = [-p_i] - [-p_{-i}] = 0 \\ x_{i,s,r} = [-p_i - \hat{p}] - [-p_{-i} - \hat{p} - c] = c \\ x_{i,m,r} = [-p_i - \hat{p}] - [-p_{-i} - c] = c - \hat{p} \\ x_{-i,s,r} = [-p_{-i} - \hat{p}] - [-p_i - \hat{p} - c] = c \\ x_{-i,m,r} = [-p_{-i} - \hat{p}] - [-p_i - c] = c - \hat{p} \end{cases}$$

  Non-residents will split evenly between each municipality as there is no mitigation measures, making each city similar for both informed and myopic individuals. Residents are always aware of the risks in their own town. Myopic residents will then move to the other city if c¡$\hat{p}$, that is to say, if the cost of migration is lower than bearing the disasters' costs if staying. In this case, municipalities will swap myopic residents. Informed residents will remain in their town of origin as they understand that there is no apparent reason of moving at a cost c. The distribution of the population in this economy remains unchanged.

In this case, governments' revenues are similar: $\Pi_i = \Pi_{-i} = p/2$.

- **Case 2:** $m_i > 0$ **and** $m_{-i} = 0$
The net anticipated surplus for each group of individuals is:

$$\begin{cases} x_{i,s,nr} = [-p_i - \hat{p} + m_i] - [-p_{-i} - \hat{p}] = m_i \\ x_{i,m,nr} = [-p_i - \hat{p} + m_i] - [-p_{-i}] = m_i - \hat{p} \\ x_{i,s,r} = [-p_i - \hat{p} + m_i] - [-p_{-i} - \hat{p} - c] = m_i + c \\ x_{i,m,r} = [-p_i - \hat{p} + m_i] - [-p_{-i} - c] = m_i + c - \hat{p} \\ x_{-i,s,r} = [-p_{-i} - \hat{p}] - [-p_i - \hat{p} + m_i - c] = c - m_i \\ x_{-i,m,r} = [-p_{-i} - \hat{p}] - [-p_i - \hat{p} + m_i - c] = c - m_i \end{cases}$$

Because of the mitigation measures taken by i, the risk of disaster is always observed in city i. Sophisticated non-residents will choose to locate in i since the known dangers are not mitigated in city $-i$. However, uninformed non-residents will prefer to move to the virtually safer city $-i$, where there is no apparent risks rather than moving to a hazardous area, even if the risks are alleviated by m_i. Sophisticated residents of city i will stay in their town of origin: they are aware they benefit from mitigation measures that do not exist for their neighbors. Myopic residents of city i, however, will move if the net cost of staying in i, $\hat{p} - m_i$, is larger than c, the cost of moving to $-i$. Residents of $-i$, who are aware of the dangers in both cities will only move if the gains from mitigatiion, m_i, compensate the costs of migration, c.

This leads to four possible subcases[27]:

a) $c > m_i$ *and* $c > \hat{p} - m_i$; which implies $\Pi_i = p.[\lambda + \alpha(\frac{1}{2} - \lambda)]$;

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[27] Note that if the share of residents in the economy, α, is null, $\Pi_i = p.\lambda$ and $\Pi_{-i} = p.(1 - \lambda)$ in every subcases.

b) $c < m_i$ *and* $c > \hat{p} - m_i$; which implies $\Pi_i = p.[\lambda + \alpha(1-\lambda)]$;

c) $c > m_i$ *and* $c < \hat{p} - m_i$; which implies $\Pi_i = p.\lambda(1-\alpha/2)]$;

d) $c < m_i$ *and* $c < \hat{p} - m_i$; which implies $\Pi_i = p.[\lambda + \frac{\alpha}{2}(1-\lambda)]$.

Therefore, conditional on $m_{-i} = 0$, it is always in the best interest city i to take mitigation measures if:

a) $\lambda > \frac{1}{2}$; when $c > m_i$ *and* $c > \hat{p} - m_i$;

b) $\lambda > \frac{1-2\alpha}{2-2\alpha} \in [-\infty; \frac{1}{2}]\ \forall\ \alpha \in [0;1]$ when $c < m_i$ *and* $c > \hat{p} - m_i$;

c) $\lambda > \frac{1}{2-\alpha} \in [\frac{1}{2}; 1]\ \forall\ \alpha \in [0;1]$ when $c > m_i$ *and* $c < \hat{p} - m_i$;

d) $\lambda > \frac{1-\alpha}{2-\alpha} \in [-\infty; \frac{1}{2}]\ \forall\ \alpha \in [0;1]$ for $\alpha \in [0;1]$ when $c < m_i$ *and* $c < \hat{p} - m_i$.

It follows that, conditional on $m_{-i} = 0$, it is *always* in the best interest of city i to take mitigation measures if at least a share $\frac{1}{2-\alpha}$ of the population is sophisticated. Consider now the cases where the neighboring city takes mitigation measures:

- **Case 3:** $m_i = 0$ **and** $m_{-i} > 0$

  The net anticipated surplus for each group of individuals is:

$$\begin{cases} x_{i,s,nr} = [-p_i - \hat{p}] - [-p_{-i} - \hat{p} + m_{-i}] = -m_{-i} \\ x_{i,m,nr} = [-p_i] - [-p_{-i} + \hat{p} + m_{-i}] = \hat{p} - m_{-i} \\ x_{i,s,r} = [-p_i - \hat{p}] - [-p_{-i} - \hat{p} + m_{-i} - c] = c - m_{-i} \\ x_{i,m,r} = [-p_i - \hat{p}] - [-p_{-i} + \hat{p} + m_{-i} - c] = c - m_{-i} \\ x_{-i,s,r} = [-p_{-i} - \hat{p} + m_{-i}] - [-p_i - \hat{p} - c] = c + m_{-i} \\ x_{-i,m,r} = [-p_{-i} - \hat{p} + m_{-i}] - [-p_i - c] = c + m_{-i} - \hat{p} \end{cases}$$

This case is symmetric to Case 2. The four possible subcases are:

a) $c > m_{-i}$ *and* $c > \hat{p} - m_{-i}$; which implies $\Pi_i = p.[(1-\lambda)(1-\alpha) + \alpha/2]$;

b) c¡m_{-i} *and* $c > \hat{p} - m_{-i}$; which implies $\Pi_i = p.[(1-\lambda)(1-\alpha)]$;

c) $c > m_{-i}$ *and* c¡$\hat{p} - m_{-i}$; which implies $\Pi_i = p.[1 - \lambda.(1-\alpha/2)]$;

d) c¡m_{-i} *and* c¡$\hat{p} - m_{-i}$; which implies $\Pi_i = p.[(1-\lambda)(1-\alpha/2)]$.

- **Case 4:** $m_i > 0$ **and** $m_{-i} > 0$

  The net anticipated surplus for each group of individuals is:

$$\begin{cases} x_{i,s,nr} = [-p_i - \hat{p} + m_i] - [-p_{-i} - \hat{p} + m_{-i}] = m_i - m_{-i} \\ x_{i,m,nr} = [-p_i - \hat{p} + m_i] - [-p_{-i} - \hat{p} + m_{-i}] = m_i - m_{-i} \\ x_{i,s,r} = [-p_i - \hat{p} + m_i] - [-p_{-i} - \hat{p} + m_{-i} - c] = m_i - m_{-i} + c \\ x_{i,m,r} = [-p_i - \hat{p} + m_i] - [-p_{-i} - \hat{p} + m_{-i} - c] = m_i - m_{-i} + c \\ x_{-i,s,r} = [-p_{-i} - \hat{p} + m_{-i}] - [-p_i - \hat{p} + m_i - c] = m_{-i} - m_i + c \\ x_{-i,m,r} = [-p_{-i} - \hat{p} + m_{-i}] - [-p_i - \hat{p} + m_i - c] = m_{-i} - m_i + c \end{cases}$$

In this case, both municipalities decide to take mitigation measures. Therefore, the dangers are revealed to everyone, unconditional on individuals' sophistication level. In this case, the decision to move either to i or $-i$ depends entirely on the net difference in mitigation levels. Non-residents will move to city i if it implements more mitigation than $-i$, and residents will move to the neighboring town if the mitigation gains offset the migration costs. In equilibrium, both cities will then supply the same level of mitigation, i.e. $m_i = m_{-i}$, and governments revenues will be $\Pi_i = \Pi_{-i} = p/2$.

Consequently, conditional on $m_{-i} > 0$, it is always in the best interest city i to take mitigation measures if:

a) $\lambda > \frac{1}{2}$; when $c > m_{-i}$ *and* $c > \hat{p} - m_{-i}$;

b) $\lambda > \frac{1-2\alpha}{2-2\alpha} \in [-\infty; \frac{1}{2}]\ \forall\ \alpha \in [0;1]$ when $c < m_{-i}$ *and* $c > \hat{p} - m_{-i}$;

c) $\lambda > \frac{1}{2-\alpha} \in [\frac{1}{2}; 1]\ \forall\ \alpha \in [0;1]$ when $c > m_{-i}$ *and* $c < \hat{p} - m_{-i}$;

d) $\lambda > \frac{1-\alpha}{2-\alpha} \in [-\infty; \frac{1}{2}]\ \forall\ \alpha \in [0;1]$ for $\alpha \in [0;1]$ when $c < m_{-i}$ *and* $c < \hat{p} - m_{-i}$.

That is to say, conditional on $m_{-i} > 0$, it is *always* in the best interest of city i to take mitigation measures if $\lambda \geq \frac{1}{2-\alpha}$.

Finally, if the migration costs offset the mitigation gains, like in subcases (a) and (c), unprotected residents will remain in their town of origin. In this case, the larger the share of non-residents, the smaller should be the share of informed individuals for the government to choose mitigation. However, for subcases (b)-(d) – i.e., when mitigation gains offset migration costs, the larger is the share of non-residents, the larger should be the share of informed individuals for the government to choose mitigation. As the share of non-residents converges to 1, the share of informed individuals for the government to adopt protective measures should be higher than $\frac{1}{2}$. Generally, since $\frac{1-2\alpha}{2-2\alpha} \leq \frac{1-\alpha}{2-\alpha} \leq \frac{1}{2} \leq \frac{1}{2-\alpha}$, a Non-Shrouded equilibrium exists as both governments will always have a strategic interest in choosing to mitigate if $\lambda > \frac{1}{2-\alpha}$. Respectively, a Shrouded equilibrium exists for $\lambda < \frac{1-2\alpha}{2-2\alpha}$ – which is equivalent to $1 - \lambda > \frac{1}{2-2\alpha}$, as both governments will always have a strategic interest in choosing not to mitigate .

B The Real Estate Industry and Risks disclosure

This section presents some recent anecdotal evidence that realtors are reluctant to any form of risk disclosure that could put them at a competitive disadvantage on the real estate market when buyers are unaware of the asset's exposure. There is some evidence that the real-estate industry lobbies brokers and State governments, while cities' revenues crucially depend on property taxes and transactions.

On February 15th, 2019, an anonymous developer expressed his view in the Guardian[28]:

"I am surprised that people are still buying, building and investing in coastal Florida.". He estimated that "A decade ago, only one in 10 buyers asked about the property elevation, or expressed concerns about rising seas. Today, nearly six of 10 ask and many decide not to buy in these same critical areas." "I'm worried we're one bad storm away from a rush for the exits" he added.

To avoid a collapse for the industry, some realtors seem to be withholding valuable information on the risks of natural disasters. Albert Slap, owner of Coastal Risk Consulting, a company that help insurance companies and prospective buyers sizing up flood risks said that Florida's housing market kept afloat by *"systemic fraudulent nondisclosure"* from real-estate agents[29].

In a New-York Times' inquiry published on Nov. 24th 2016[30], Ian Urbina reported:

"Most real estate agents say they try to tackle the issue head-on, providing clients with maps indicating federally declared high-risk flood zones, and using climate-change preparedness as a selling point, emphasizing if the house has a backup generator or shingles that can withstand hurricane-strength winds. But real estate agents risk putting themselves at a competitive disadvantage by overstating threats. Good information is hard to come by. No one knows whether, when or by how much properties will depreciate, seas will encroach or flood insurance policies will change. Valerie Amor, a real estate agent in Fort Lauderdale, said that, unlike most in her industry, she does a feasibility study before she assists in either buying or

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[28] https://www.theguardian.com/environment/2019/feb/15/florida-climate-change-coastal-real-estate-rising-seas

[29] https://www.insurancejournal.com/news/southeast/2018/01/02/475789.htm

[30] https://www.nytimes.com/2016/11/24/science/global-warming-coastal-real-estate.html

selling property.”

It appears not to be an isolated agent case. The industry seems to lobby brokers and governments to avoid disclosing natural disaster risks. Urbina continues: *“After strong objections from real estate companies, which threatened to stop providing data, Attom Data Solutions - “a multi-sourced national property data warehouse”, took down its web page that integrated real estate listings with plot-by-plot information about the risks of floods, hurricanes, wildfires and other natural hazards.”*

Local governments are extremely dependent on the industry’s good economic health. Indeed, local governments’ revenues critically rely on property taxes and transactions. Interviewed about the role of cities in mitigation investments, James Murley, Miami-Dade’s chief resilience officer, said it was *“important to avoid spooking the [housing] market since real estate investment produces much of the revenue that pays for these upgrades.”*

Jim Cason, former mayor of Coral Gables confirmed this view in the Insurance Journal of January 2nd, 2018 note1:

“Cason, who left office in May, attended a regular gathering of South Florida elected officials in Fort Lauderdale in December to talk about the effects of climate change. Unlike previous years, he said, the event this time was “totally sold-out.” He said mayors and city managers shared their anxiety about what rising seas mean for their cities’ property values. Those worries range from the mundane – finding more money to update infrastructure damaged by storms – to the existential: How long will banks keep issuing 30-year mortgages?”

States’ budgets do not rely on property taxes and transactions. However, disclosure laws are set at the State government level. States’ legislation varies regarding what real-estate agents and sellers must disclose about their property. Some, like California, Washington, or Pennsylvania, have strict legally-binding disclosure statements that must be signed upon sales. Others, like West Virginia or Alabama, do not have a standard disclosure document but instead, employ the “Caveat Emptor” or “Buyer Beware” rule [31]. This rule states that it is the buyer’s responsibility to figure out if there are any issues with the home.

The real estate industry is also trying to influence states into passing laws preventing the disclosure of any sensitive information regarding potential natural disasters. Indeed, some states, like New-Jersey or Mas-

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[31] https://www.homelight.com/blog/mandated-disclosures-real-estate/

sachusetts, are moving towards stricter regulations. But it is not reflective of a general trend. For instance, in North Virginia, lawmakers confirmed the responsibility for discovering the risk exposure falls on the buyers note2:

Within a year, state lawmakers passed a real estate disclosure law that the industry hailed as a major step forward. "We are immensely satisfied," Deborah Baisden, then president of the Virginia Association of Realtors, said of the law. While the law encourages home buyers to exert due diligence in investigating the risk of living in a flood hazard area, it also explicitly states that the seller of a home is not obligated to disclose whether the home is in a zone that FEMA regards as high risk.

In some – yet more extreme – cases the industry sponsors policy-makers passing laws preventing from developing using building codes acknowledging climate risks[32]. This was the case in North-Carolina, where a 2012 law bans the state from basing coastal policies on the latest scientific predictions of how much the sea level will rise[33]. ABC's reporter Alon Harish wrote about McElraf, who drafted the law[34]:

The largest industry contributors to McElraft's campaigns have been real estate agents and developers, according to the National Institute on Money in State Politics. Her top contributor since she was elected to the General Assembly in 2007 has been the North Carolina Association of Realtors, followed by the North Carolina Home Builders' Association. McElraft, who is a former real estate agent and lives on Barrier Island off the coast, denied that campaign contributions ever influence her decisions as a lawmaker, and said her votes have not always favored increased development.

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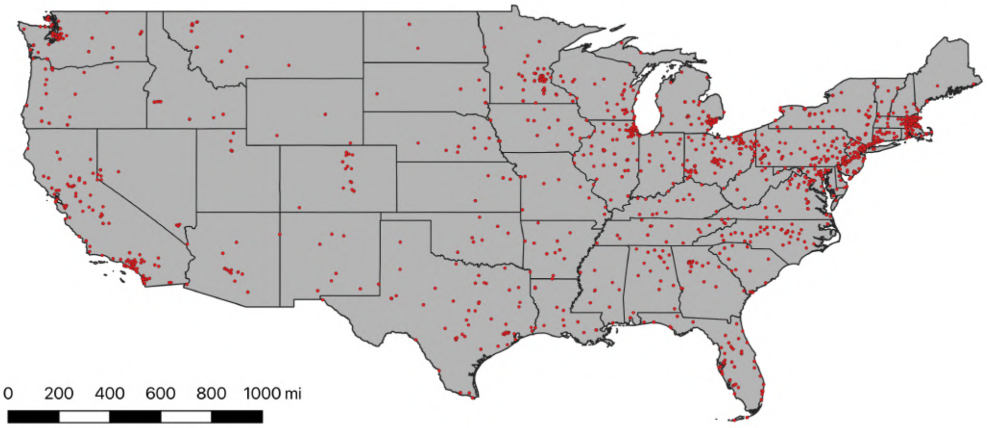
[32] https://repository.wellesley.edu/cgi/viewcontent.cgi?article=1100context=thesiscollection

[33] https://www.theguardian.com/us-news/2018/sep/12/north-carolina-didnt-like-science-on-sea-levels-so-passed-a-law-against-it

[34] https://abcnews.go.com/US/north-carolina-bans-latest-science-rising-sea-level/story?id=16913782

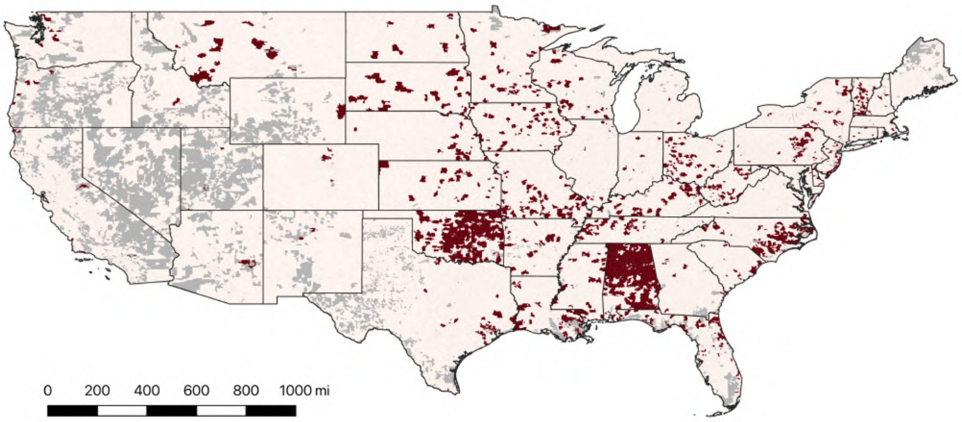
C Figures and Tables

Figure C.1: Publishing Cities (2010 - 2018)



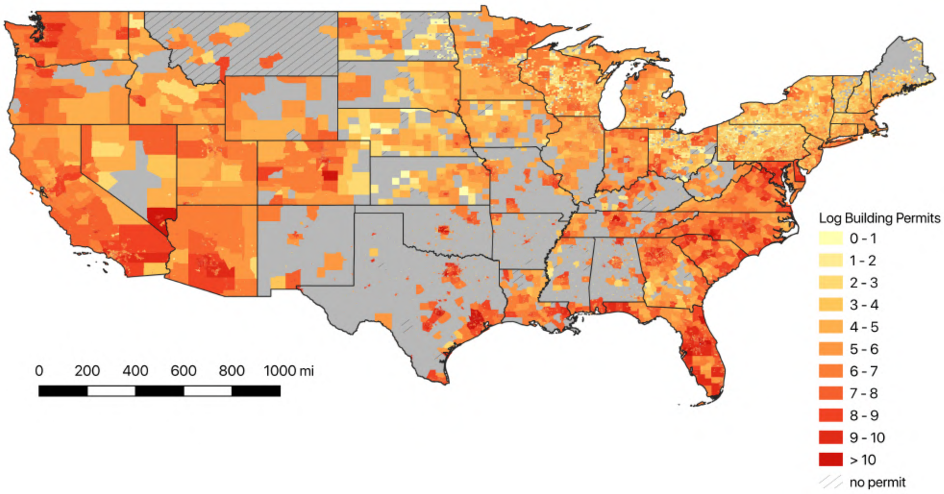
Notes: Spatial distribution publishing cities, defined as the city where a newspaper publisher is located, between 2010 and 2018. This information was graciously provided by the Alliance for Audited Media (AAM).

Figure C.2: Mitigation projects under the HMG Program (2010 - 2018)



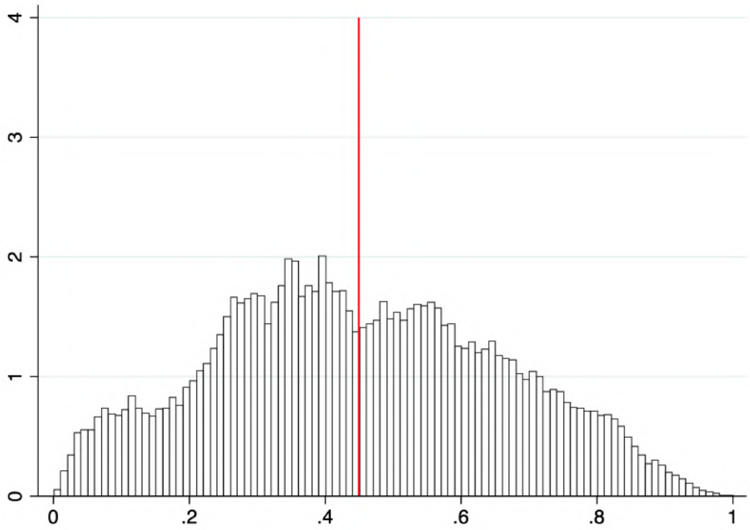
Notes: Spatial distribution of mitigation projects' ZIP code location under the HMG Program, between 2010 and 2018. This information was extracted from FEMA's online databases. Gray zones correspond to unpopulated areas.

Figure C.3: New Residential Building Permits (2010 - 2018)



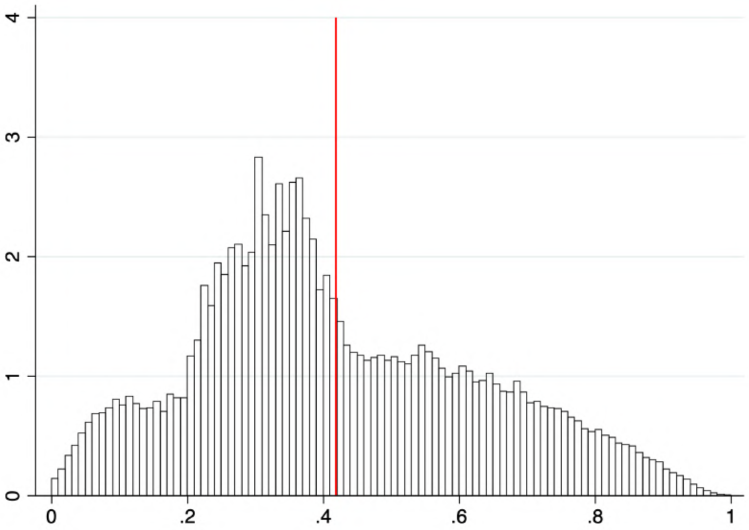
Notes: Spatial distribution of new residential building permits' location, by permit-issuing jurisdiction, between 2010 and 2018. This information was extracted from the Census Building Permits Survey. The sample contains 20,864 permit-issuing jurisdictions. Plain gray zones correspond to areas with unavailable information.

Figure C.4: Distribution of *Coverage* at the Permit-issuing Place level



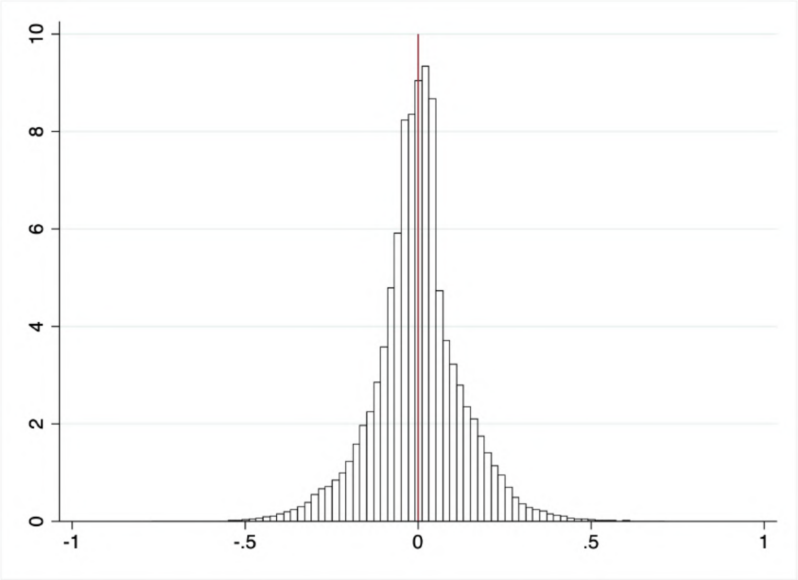
Notes: Information about storms and newspaper circulation has been aggregated at the permit-issuing jurisdiction level. *Coverage* has been computed following equation 3.1. The vertical red line corresponds to mean *Coverage*.

Figure C.5: Distribution of *Coverage* at the ZIP code area level



Notes: Coverage has been computed following equation 3.1. The vertical red line corresponds to mean *Coverage*.

Figure C.6: Distribution of *Coverage* at the ZIP code area level, and centered at its ZIP code mean



Notes: Coverage has been computed following equation 3.1. The vertical red line corresponds to mean *Coverage*, centered at its ZIP code mean.

Table C.1: Mitigations projects under HMG fundings (2010-2018)

Mitigation Type	Total Number of Projects	Share concerning private properties	Total number of properties mitigated	Project amount (in millions of $)	Federal Share Obligated (in millions of $)	Average Project amount (in millions of $)
Saferooms	4394 (39.91 %)	85.08%	8220 (34.17%)	1793.64 (4.79%)	1198.87 (4.25%)	.41
Acquisition	4280 (38.88 %)	96%	8754 (36.39%)	28107.24 (74.93%)	20832.3 (73.87%)	6.57
Elevation	1470 (13.35%)	98.03%	4523 (18.80%)	5847.77 (15.59%)	4991.08 (17.7%)	3.98
Wind Retrofit	195 (1.77%)	15.38%	308 (1.28%)	164.92 (.44%)	123.89 (.44%)	.85
Seismic Retrofit	145 (1.32%)	37.93%	594 (2.47%)	181.23 (.48%)	107.91 (.38%)	1.25
Flood Controls	136 (1.24%)	0%	596 (2.48%)	361.74 (.96%)	193.51 (.69%)	2.66
Mitigation Reconstruction	104 (.94%)	0%	209 (.87%)	231.05 (.62%)	187.09 (.66%)	2.22
Wildfire Retrofit	99 (.9%)	96.97%	353 (1.47%)	145.65 (.39%)	84.27 (.3%)	1.47
Engineering and Design Studies	68 (.62%)	0%	261 (1.09%)	584.09 (1.56%)	431.94 (1.53%)	8.59
Floodproofing	24 (.22%)	37.5%	29 (.12%)	14.22 (.03%)	10.07 (.04%)	.59
Others	22 (.2%)	0%	29 (.12%)	6.56 (.02%)	4.33 (.02%)	.3
Relocation	20 (.18%)	85%	40 (.17%)	8.83 (.02%)	6.89 (.02%)	.44
Vegetation Managment	18 (.16%)	0%	47 (.2%)	6.87 (.02%)	5.06 (.02%)	.38
Public Infrastructures Protection	16 (.15%)	0%	42 (.17%)	39.6 (.11%)	12.95 (.05%)	2.5
Generators	8 (.07%)	0%	25 (.1%)	2.93 (.01%)	2.18 (.01%)	.37
Warning Systems	6 (.05%)	0%	19 (.08%)	6.03 (.02%)	4.83 (.02%)	1
Land Stabilization	4 (.04%)	0%	6 (.02%)	7.18 (.02%)	5.3 (.02%)	1.8
TOTAL	11009	86.26%	24055	37509.53	28202.47	2.56 (mean)

Table C.2: Descriptive statistics

Covariate	Mean	Sd	Min	Max
Demographics				
Log population	7.950	1.919	0	11.79
share above 65 year-old	0.188	0.109	0	1
Share of males	0.487	0.073	0	1
Share of immigrants	0.058	0.071	0	1
Share of non-native speakers	0.109	0.158	0	1
Share of foreign-born	0.064	0.096	0	1
Share of whites	0.836	0.205	0	1
Share of blacks	0.081	0.162	0	1
Share of natives	0.015	0.075	0	1
Share of asians	0.022	0.054	0	1
share of hawaiian	0.001	0.010	0	0.729
Share fo others	0.024	0.057	0	1
Log newspaper copies	13.89	1.185	6.204	17.01
Income and Education				
Log Income	11.04	0.468	0	13.47
Share less than highschool	0.089	0.071	0	1
Share of highschool dropout	0.057	0.068	0	1
Share of highschool diploma	0.341	0.133	0	1
Share of college dropout	0.206	0.087	0	1
Share of associate diploma	0.079	0.054	0	1
Share of Bachelor diploma	0.148	0.101	0	1
Share of Graduate diploma	0.087	0.091	0	1

Table C.3: Descriptive statistics

Covariate	Mean	Sd	Min	Max
Weather				
Log temperatures	2.612	0.336	0.416	3.319
Log wind speed	1.898	0.298	0	3.539
Log rainfalls	4.401	0.522	1.206	5.718
Labor Composition				
Share labor force	0.470	0.125	0	1
Share in the agriculture	0.061	0.105	0	1
Share in the construction	0.077	0.067	0	1
Share in the manufacture	0.112	0.092	0	1
Share in wholesales	0.026	0.035	0	1
Share in retail	0.112	0.073	0	1
Share in transportation	0.056	0.057	0	1
Share in information	0.017	0.027	0	1
Share in finance	0.052	0.052	0	1
Share in professorship	0.082	0.071	0	1
Share in education	0.226	0.103	0	1
Share in arts	0.081	0.074	0	1
Share in public administration	0.053	0.062	0	1
Share in others	0.047	0.048	0	1
Share unemployed	0.086	0.089	0	1

Table C.4: Descriptive statistics

Covariate	Mean	Sd	Min	Max
Housing Composition				
Log housing units	7.142	1.889	0	10.77
Log housing sales	3.176	2.049	0	8.016
Log median value	11.87	0.724	0	15.19
Share under \$50.000	0.157	0.171	0	1
Share \$50.000 - \$100.000	0.203	0.176	0	1
Share \$100.000 - \$150.000	0.144	0.118	0	1
Share \$150.000 - \$200.000	0.125	0.105	0	1
Share \$200.000 - \$300.000	0.150	0.133	0	1
Share \$300.000 - \$500.000	0.128	0.157	0	1
Share \$300.000 - \$1.000.000	0.071	0.135	0	1
Share above \$1.000.000	0.022	0.074	0	1
Share built in the 2010's	0.011	0.028	0	1
Share built in the 2000's	0.121	0.115	0	1
Share built in the 1990's	0.134	0.099	0	1
Share built in the 1980's	0.132	0.095	0	1
Share built in the 1970's	0.157	0.096	0	1
Share built in the 1960's	0.100	0.078	0	1
Share built in the 1940-1950's	0.153	0.120	0	1
Share built before 1940's	0.193	0.184	0	1
Share of owner-occupied units	0.735	0.178	0	1
Share of vacant units	0.166	0.152	0	1
Share with mortgage	0.562	0.180	0	1

Table C.5: Balance tests for *Storm* and *Coverage*, separately.

Pre-Treatment Outcome	*Storm*		*Coverage*			μ/sd
Demographics						
Log Population	-0.002	-0.001	-0.096***	–	–	7.950
	(0.002)	(0.002)	(0.025)	–	–	(1.919)
Share above 65 year-old	0.000	0.000	0.014**	0.007	0.006*	0.188
	(0.001)	(0.001)	(0.006)	(0.006)	(0.003)	(0.109)
Share of males	0.000	-0.000	0.006	0.006	0.002	0.487
	(0.000)	(0.000)	(0.005)	(0.005)	(0.003)	(0.073)
Share of immigrants	-0.000	-0.000	0.005	0.006	0.003	0.058
	(0.000)	(0.000)	(0.004)	(0.004)	(0.002)	(0.071)
Share of non native speakers	0.023	0.018	0.174	-0.371	-0.216	0.109
	(0.041)	(0.036)	(0.391)	(0.392)	(0.220)	(0.158)
Share of foreign-born	-0.000	-0.000	0.000	0.001	-0.000	0.064
	(0.000)	(0.000)	(0.003)	(0.003)	(0.002)	(0.096)
Share of whites	-0.000	0.000	-0.003	-0.003	-0.002	0.836
	(0.001)	(0.000)	(0.004)	(0.004)	(0.002)	(0.205)
Share of blacks	-0.000	-0.000	-0.001	-0.001	0.000	0.081
	(0.000)	(0.000)	(0.003)	(0.003)	(0.002)	(0.162)
Share of natives	-0.000	0.000	0.001	0.001	0.001	0.015
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.075)
Share of asians	-0.000	-0.000	-0.000	-0.000	0.000	0.022
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.054)
Share of hawaians	-0.000	-0.000	0.000	0.001	0.000	0.001
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.010)
Share of others	0.000	0.000	0.002	0.002	0.001	0.024
	(0.000)	(0.000)	(0.002)	(0.002)	(0.001)	(0.057)
Log newspaper copies	0.002	-0.009	0.090	0.093	0.006	13.89
	(0.005)	(0.007)	(0.071)	(0.071)	(0.062)	(1.185)
Income and Education						
Log Income	0.002	0.001	-0.007	0.006	-0.003	11.04
	(0.003)	(0.003)	(0.028)	(0.028)	(0.017)	(0.468)
Share less than highschool	0.001	0.001	0.007	0.008	0.003	0.089
	(0.001)	(0.000)	(0.006)	(0.006)	(0.003)	(0.071)
Share highschool dropout	0.000	0.000	-0.002	-0.002	-0.000	0.057
	(0.000)	(0.000)	(0.005)	(0.005)	(0.002)	(0.068)
Share highschool diploma	-0.000	-0.000	0.002	0.003	0.000	0.341
	(0.001)	(0.001)	(0.008)	(0.008)	(0.004)	(0.133)
Share college dropout	0.001	0.000	0.004	0.003	-0.001	0.206
	(0.001)	(0.001)	(0.007)	(0.007)	(0.003)	(0.087)
Share associate diploma	-0.000	-0.000	-0.002	-0.002	0.001	0.079
	(0.000)	(0.000)	(0.004)	(0.004)	(0.002)	(0.054)
Share bachelor diploma	-0.000	-0.000	-0.007	-0.007	-0.002	0.148
	(0.001)	(0.000)	(0.005)	(0.005)	(0.003)	(0.101)
Share graduate studies	-0.000	-0.000	-0.003	-0.003	-0.002	0.087
	(0.000)	(0.000)	(0.004)	(0.004)	(0.002)	(0.091)
ZIP Code FE	Y	Y	Y	Y	Y	–
County-Year FE	Y	N	Y	Y	N	–
State-Year FE	N	Y	N	N	Y	–
Baseline Population	N	N	N	Y	Y	–

Table C.6: Balance tests for *Storm* and *Coverage*, separately.

Pre-Treatment Outcome	*Storm*		*Coverage*			μ/sd
Weather						
Log temperatures	-0.000	0.000	-0.001	-0.001	-0.003	2.612
	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)	(0.336)
Log wind speed	0.002	0.003*	0.005	0.005	0.041***	1.898
	(0.001)	(0.002)	(0.008)	(0.009)	(0.013)	(0.298)
Log rainfalls	0.002	-0.001	0.003	0.003	0.016	4.401
	(0.001)	(0.002)	(0.007)	(0.007)	(0.015)	(0.522)
Labor Compostion						
Share labor force	-0.026	-0.025	-0.554	0.258	-0.123	0.470
	(0.060)	(0.057)	(0.696)	(0.690)	(0.368)	(0.067)
Share in the agriculture	-0.000	-0.001	0.013**	0.013**	0.007***	0.067
	(0.001)	(0.001)	(0.005)	(0.005)	(0.003)	(0.105)
Share in the construction	0.000	0.001	0.005	0.006	0.001	0.077
	(0.001)	(0.000)	(0.005)	(0.005)	(0.003)	(0.067)
Share in the manufacture	0.001*	0.001**	-0.007	-0.007	-0.003	0.112
	(0.001)	(0.001)	(0.005)	(0.005)	(0.003)	(0.092)
Share in wholesales	-0.000	0.000	-0.001	-0.001	0.001	0.026
	(0.000)	(0.000)	(0.003)	(0.003)	(0.001)	(0.035)
Share in retail	-0.001*	-0.001*	0.002	0.003	-0.003	0.112
	(0.001)	(0.001)	(0.005)	(0.005)	(0.003)	(0.073)
Share in transportation	-0.001	-0.001	0.002	0.002	0.003	0.056
	(0.000)	(0.000)	(0.004)	(0.004)	(0.002)	(0.057)
Share in information	-0.000	-0.000	-0.001	-0.002	0.001	0.017
	(0.000)	(0.000)	(0.002)	(0.002)	(0.001)	(0.027)
Share in finance	0.000	0.000	-0.001	-0.001	0.001	0.052
	(0.000)	(0.000)	(0.003)	(0.003)	(0.002)	(0.052)
Share in professorship	-0.000	-0.000	-0.006	-0.006	-0.000	0.082
	(0.000)	(0.000)	(0.004)	(0.004)	(0.002)	(0.071)
Share in education	0.001	0.000	-0.005	-0.005	-0.006	0.226
	(0.001)	(0.001)	(0.008)	(0.008)	(0.004)	(0.103)
Share in arts	-0.000	-0.000	0.005	0.006	0.001	0.081
	(0.001)	(0.000)	(0.004)	(0.004)	(0.002)	(0.074)
Share in public administration	0.000	0.000	-0.001	-0.001	-0.001	0.053
	(0.000)	(0.000)	(0.004)	(0.004)	(0.002)	(0.062)
Share in others	-0.001	-0.000	-0.005	-0.006	-0.001	0.047
	(0.000)	(0.000)	(0.005)	(0.005)	(0.002)	(0.048)
Share unemployed	0.000	0.000	-0.004	-0.004	-0.001	0.086
	(0.001)	(0.001)	(0.006)	(0.006)	(0.003)	(0.089)
ZIP Code FE	Y	Y	Y	Y	Y	–
County-Year FE	Y	N	Y	Y	N	–
State-Year FE	N	Y	N	N	Y	–
Baseline Population	N	N	N	Y	Y	–

Table C.7: Balance tests for *Storm* and *Coverage*, separately.

Pre-Treatment Outcome	*Storm*		*Coverage*			μ/sd
Housing Markets						
Log housing Units	0.001	0.001	-0.042*	0.005	0.001	7.142
	(0.002)	(0.002)	(0.025)	(0.022)	(0.010)	(1.889)
Log housing sales	-0.000	-0.006	-0.037	-0.035	-0.057	3.176
	(0.007)	(0.008)	(0.038)	(0.038)	(0.043)	(2.049)
Log median value	-0.001	-0.002	-0.017	-0.015	-0.010	11.87
	(0.002)	(0.002)	(0.022)	(0.021)	(0.012)	(0.724)
Share under \$50.000	0.000	0.001	0.008	0.008	0.005	0.157
	(0.001)	(0.001)	(0.007)	(0.007)	(0.004)	(0.171)
Share \$50.000 - \$100.000	0.001	0.000	0.009	0.008	0.006	0.203
	(0.001)	(0.001)	(0.008)	(0.008)	(0.004)	(0.176)
Share \$100.000 - \$150.000	-0.000	-0.001	-0.005	-0.005	-0.005	0.144
	(0.001)	(0.001)	(0.006)	(0.006)	(0.004)	(0.118)
Share \$150.000 - \$200.000	-0.000	-0.000	-0.002	-0.003	-0.001	0.125
	(0.001)	(0.001)	(0.006)	(0.005)	(0.003)	(0.105)
Share \$200.000 - \$300.000	-0.001	0.000	-0.012**	-0.012**	-0.004	0.150
	(0.001)	(0.001)	(0.005)	(0.005)	(0.003)	(0.133)
Share \$300.000 - \$500.000	0.000	0.000	0.003	0.004	-0.000	0.128
	(0.001)	(0.001)	(0.005)	(0.005)	(0.003)	(0.157)
Share \$500.000 - \$1.000.000	-0.000	-0.000	-0.004	-0.004	-0.000	0.071
	(0.000)	(0.000)	(0.003)	(0.003)	(0.002)	(0.135)
Share above \$1.000.000	-0.000	-0.000	0.003	0.003	0.001	0.022
	(0.000)	(0.000)	(0.002)	(0.002)	(0.001)	(0.074)
Share built in the 2010's	0.000	0.000*	-0.001	-0.001	-0.000	0.011
	(0.000)	(0.000)	(0.002)	(0.002)	(0.001)	(0.028)
Share built in the 2000's	-0.000	-0.000	0.000	0.001	0.000	0.121
	(0.001)	(0.001)	(0.005)	(0.005)	(0.003)	(0.115)
Share built in the 1990's	0.000	0.000	-0.003	-0.003	-0.001	0.134
	(0.001)	(0.001)	(0.005)	(0.005)	(0.003)	(0.099)
Share built in the 1980's	-0.000	-0.000	0.007	0.007	0.003	0.132
	(0.001)	(0.001)	(0.005)	(0.005)	(0.003)	(0.095)
Share built in the 1970's	0.000	-0.000	-0.002	-0.001	-0.003	0.157
	(0.001)	(0.001)	(0.006)	(0.006)	(0.003)	(0.096)
Share built in the 1960's	-0.001*	-0.001*	-0.005	-0.006	0.000	0.100
	(0.001)	(0.000)	(0.004)	(0.004)	(0.003)	(0.078)
Share built in the 1940-1950's	0.001	0.001	0.003	0.003	-0.001	0.153
	(0.001)	(0.001)	(0.005)	(0.005)	(0.003)	(0.120)
Share built before 1940	0.000	0.000	0.001	0.001	0.002	0.193
	(0.001)	(0.001)	(0.006)	(0.006)	(0.003)	(0.184)
Share of owner-occupied units	-0.001	-0.001	-0.002	-0.004	0.003	0.735
	(0.001)	(0.001)	(0.007)	(0.007)	(0.004)	(0.178)
Share of vacant units	0.009	-0.002	1.091*	0.228	0.420	0.166
	(0.065)	(0.062)	(0.618)	(0.573)	(0.335)	(0.152)
Share with mortgage	-0.001	-0.001	0.008	0.011	0.012***	0.562
	(0.001)	(0.001)	(0.008)	(0.008)	(0.004)	(0.180)
ZIP Code FE	Y	Y	Y	Y	Y	–
County-Year FE	Y	N	Y	Y	N	–
State-Year FE	N	Y	N	N	Y	–
Baseline Population	N	N	N	Y	Y	–

Table C.8: Balance tests for main specification

Pre-Treatment Outcome	Model 1			Model 2			μ/sd
	β_1	β_2	β_3	β_1	β_2	β_3	
Demographics							
Log Population	-0.105***	-0.001	0.029	–	–	–	7.950
	(0.028)	(0.002)	(0.020)	–	–	–	(1.919)
Share above 65 year-old	0.016**	-0.000	-0.004	0.008	-0.000	-0.002	0.188
	(0.007)	(0.001)	(0.005)	(0.006)	(0.001)	(0.005)	(0.109)
Share of males	0.008	0.001	-0.007	0.008	0.001	-0.007	0.487
	(0.006)	(0.000)	(0.004)	(0.005)	(0.000)	(0.004)	(0.073)
Share of immigrants	0.006	-0.000	-0.003	0.007	-0.000	-0.003	0.058
	(0.005)	(0.000)	(0.004)	(0.005)	(0.000)	(0.004)	(0.071)
Share of non native speakers	0.281	0.040	-0.367	-0.308	0.036	-0.230	0.109
	(0.454)	(0.042)	(0.394)	(0.447)	(0.043)	(0.380)	(0.158)
Share of foreign born	0.000	0.000	-0.000	0.001	0.000	-0.000	0.064
	(0.003)	(0.000)	(0.003)	(0.003)	(0.000)	(0.003)	(0.096)
Share of whites	-0.002	-0.000	-0.003	-0.002	-0.000	-0.003	0.836
	(0.005)	(0.001)	(0.004)	(0.005)	(0.001)	(0.004)	(0.205)
Share of blacks	-0.001	0.000	0.001	-0.001	0.000	0.001	0.081
	(0.004)	(0.000)	(0.003)	(0.004)	(0.000)	(0.003)	(0.162)
Share of natives	0.002	-0.000	-0.001	0.001	-0.000	-0.001	0.015
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.075)
Share of asians	-0.000	-0.000	0.000	-0.001	-0.000	0.001	0.022
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.054)
Share of hawaiians	0.001	-0.000	-0.000	0.001	-0.000	-0.000	0.001
	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.010)
Share of others	0.001	0.000	0.001	0.001	0.000	0.001	0.024
	(0.002)	(0.000)	(0.002)	(0.002)	(0.000)	(0.002)	(0.057)
Log newspaper copies	0.111	0.004	-0.075	0.115	0.004	-0.077	13.89
	(0.076)	(0.005)	(0.053)	(0.076)	(0.005)	(0.053)	(1.185)
Income and Education							
Log income	-0.030	-0.001	0.070*	-0.016	-0.000	0.066	11.04
	(0.034)	(0.003)	(0.041)	(0.034)	(0.003)	(0.040)	(0.468)
Share less than highschool	0.006	0.000	0.005	0.006	0.000	0.004	0.089
	(0.007)	(0.001)	(0.005)	(0.007)	(0.001)	(0.005)	(0.071)
Share highschool dropout	-0.003	-0.000	0.002	-0.003	-0.000	0.002	0.057
	(0.005)	(0.000)	(0.003)	(0.005)	(0.000)	(0.003)	(0.068)
Share highschool diploma	0.006	0.000	-0.014*	0.007	0.000	-0.014*	0.341
	(0.009)	(0.001)	(0.008)	(0.009)	(0.001)	(0.008)	(0.133)
Share college dropout	0.004	0.001	-0.002	0.004	0.001	-0.001	0.206
	(0.007)	(0.001)	(0.005)	(0.007)	(0.001)	(0.005)	(0.087)
Share associate diploma	0.000	-0.000	-0.006	0.000	-0.000	-0.006	0.079
	(0.004)	(0.000)	(0.004)	(0.004)	(0.000)	(0.004)	(0.054)
Share bachelor diploma	-0.010*	-0.001	0.012***	-0.011*	-0.001	0.013***	0.148
	(0.006)	(0.001)	(0.005)	(0.006)	(0.001)	(0.005)	(0.101)
Share graduate diploma	-0.003	-0.000	0.002	-0.004	-0.000	0.002	0.087
	(0.005)	(0.000)	(0.004)	(0.005)	(0.000)	(0.004)	(0.091)
ZIP Code FE	Y	Y	Y	Y	Y	Y	–
County-Year FE	Y	Y	Y	Y	Y	Y	–
State-Year FE	N	N	N	N	N	N	–
Baseline Population	N	N	N	Y	Y	Y	–

Table C.9: Balance tests for main specification

Pre-Treatment Outcome	Model 1			Model 2			μ/sd
	β_1	β_2	β_3	β_1	β_2	β_3	
Weather							
Log temperatures	-0.001	-0.000	0.002**	-0.001	-0.000	0.002**	2.612
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.336)
Log wind speed	0.001	0.001	0.009	0.002	0.001	0.009	1.898
	(0.009)	(0.001)	(0.009)	(0.009)	(0.001)	(0.009)	(0.298)
Log rainfalls	0.005	0.002	-0.006	0.004	0.002	-0.006	4.401
	(0.008)	(0.001)	(0.008)	(0.008)	(0.001)	(0.008)	(0.522)
Labor Compostion							
Share labor force	-0.850	-0.061	0.962	0.028	-0.055	0.757	0.470
	(0.798)	(0.063)	(0.665)	(0.779)	(0.064)	(0.642)	(0.125)
Share in the agriculture	0.014**	-0.000	-0.003	0.015***	-0.000	-0.003	0.067
	(0.006)	(0.001)	(0.005)	(0.006)	(0.001)	(0.005)	(0.105)
Share in the construction	0.008	0.001	-0.010**	0.009	0.001	-0.010**	0.077
	(0.006)	(0.001)	(0.005)	(0.006)	(0.001)	(0.005)	(0.067)
Share in the manufacture	-0.010*	0.001*	0.009*	-0.011*	0.001*	0.009*	0.112
	(0.006)	(0.001)	(0.005)	(0.006)	(0.001)	(0.005)	(0.092)
Share in wholesales	-0.001	-0.000	-0.000	-0.001	-0.000	-0.000	0.026
	(0.003)	(0.000)	(0.002)	(0.003)	(0.000)	(0.002)	(0.035)
Share in retail	0.002	-0.001*	0.001	0.003	-0.001*	0.001	0.112
	(0.006)	(0.001)	(0.005)	(0.006)	(0.001)	(0.005)	(0.073)
Share in transportation	0.003	-0.000	-0.001	0.003	-0.000	-0.002	0.056
	(0.005)	(0.000)	(0.004)	(0.005)	(0.000)	(0.004)	(0.057)
Share in information	-0.001	-0.000	0.000	-0.002	-0.000	0.000	0.017
	(0.002)	(0.000)	(0.002)	(0.002)	(0.000)	(0.002)	(0.027)
Share in finance	-0.001	0.000	-0.001	-0.001	0.000	-0.001	0.052
	(0.004)	(0.000)	(0.003)	(0.004)	(0.000)	(0.003)	(0.052)
Share in professorship	-0.005	0.000	-0.004	-0.005	0.000	-0.004	0.082
	(0.004)	(0.000)	(0.004)	(0.004)	(0.000)	(0.004)	(0.071)
Share in education	-0.009	-0.000	0.013**	-0.009	-0.000	0.013**	0.226
	(0.009)	(0.001)	(0.006)	(0.009)	(0.001)	(0.006)	(0.103)
Share in arts	0.007	0.000	-0.006	0.008	0.000	-0.006	0.081
	(0.005)	(0.001)	(0.004)	(0.005)	(0.001)	(0.004)	(0.074)
Share in public administration	-0.002	0.000	0.003	-0.002	0.000	0.003	0.053
	(0.004)	(0.000)	(0.003)	(0.004)	(0.000)	(0.003)	(0.062)
Share in others	-0.005	-0.000	0.000	-0.006	-0.000	0.001	0.047
	(0.005)	(0.000)	(0.003)	(0.005)	(0.000)	(0.003)	(0.048)
Share unemployed	-0.002	0.001	-0.007	-0.002	0.001	-0.007	0.086
	(0.006)	(0.001)	(0.005)	(0.006)	(0.001)	(0.004)	(0.089)
ZIP Code FE	Y	Y	Y	Y	Y	Y	–
County-Year FE	Y	Y	Y	Y	Y	Y	–
State-Year FE	N	N	N	N	N	N	–
Baseline Population	N	N	N	Y	Y	Y	–

Table C.10: Balance tests for main specification

Pre-Treatment Outcome	Model 1			Model 2			μ/sd
	β_1	β_2	β_3	β_1	β_2	β_3	
Housing Markets							
Log housing units	-0.047	0.001	0.014	0.005	0.001	-0.000	7.142
	(0.028)	(0.002)	(0.021)	(0.024)	(0.002)	(0.018)	(1.889)
Log housing sales	-0.028	0.001	-0.029	-0.026	0.001	-0.030	3.176
	(0.040)	(0.008)	(0.046)	(0.040)	(0.008)	(0.046)	(2.049)
Log median value	-0.020	-0.001	0.009	-0.017	-0.001	0.009	11.87
	(0.024)	(0.002)	(0.019)	(0.024)	(0.002)	(0.019)	(0.724)
Share under \$50.000	0.008	0.000	-0.001	0.009	0.000	-0.002	0.157
	(0.008)	(0.001)	(0.007)	(0.008)	(0.001)	(0.007)	(0.171)
Share \$50.000 - \$100.000	0.007	0.001	0.005	0.007	0.001	0.005	0.203
	(0.008)	(0.001)	(0.007)	(0.008)	(0.001)	(0.007)	(0.176)
Share \$100.000 - \$150.000	-0.004	-0.000	-0.003	-0.004	-0.000	-0.003	0.144
	(0.007)	(0.001)	(0.006)	(0.007)	(0.001)	(0.006)	(0.118)
Share \$150.000 - \$200.000	-0.004	-0.001	0.005	-0.004	-0.001	0.005	0.125
	(0.006)	(0.001)	(0.006)	(0.006)	(0.001)	(0.005)	(0.105)
Share \$200.000 - \$300.000	-0.011*	-0.000	-0.003	-0.011*	-0.000	-0.003	0.150
	(0.006)	(0.001)	(0.005)	(0.006)	(0.001)	(0.005)	(0.133)
Share \$300.000 - \$500.000	0.005	0.000	-0.004	0.005	0.000	-0.004	0.128
	(0.005)	(0.001)	(0.004)	(0.005)	(0.001)	(0.004)	(0.157)
Share \$500.000 - \$1.000.000	-0.005	-0.000	0.004	-0.005	-0.000	0.004	0.071
	(0.003)	(0.000)	(0.003)	(0.003)	(0.000)	(0.003)	(0.135)
Share above \$1.000.000	0.003	-0.000	-0.002	0.004*	-0.000	-0.002	0.022
	(0.002)	(0.000)	(0.002)	(0.002)	(0.000)	(0.002)	(0.074)
Share built in the 2010's	-0.001	0.000	-0.002	-0.000	0.000	-0.002	0.011
	(0.002)	(0.000)	(0.002)	(0.002)	(0.000)	(0.002)	(0.028)
Share built in the 2000's	-0.001	-0.001	0.005	-0.001	-0.001	0.005	0.121
	(0.005)	(0.001)	(0.004)	(0.005)	(0.001)	(0.004)	(0.115)
Share built in the 1990's	-0.004	0.000	0.004	-0.004	0.000	0.004	0.134
	(0.005)	(0.001)	(0.005)	(0.005)	(0.001)	(0.005)	(0.099)
Share built in the 1980's	0.007	-0.000	-0.000	0.007	-0.000	-0.000	0.132
	(0.006)	(0.001)	(0.005)	(0.006)	(0.001)	(0.005)	(0.095)
Share built in the 1970's	-0.003	-0.000	0.003	-0.002	-0.000	0.003	0.157
	(0.006)	(0.001)	(0.005)	(0.006)	(0.001)	(0.005)	(0.096)
Share built in the 1960's	-0.005	-0.001**	-0.001	-0.006	-0.001**	-0.000	0.100
	(0.004)	(0.001)	(0.004)	(0.004)	(0.001)	(0.004)	(0.078)
Share built in the 1940-1950's	0.002	0.001	-0.000	0.003	0.001	-0.000	0.153
	(0.006)	(0.001)	(0.005)	(0.006)	(0.001)	(0.005)	(0.120)
Share built before 1940	0.003	0.001	-0.008	0.004	0.001	-0.008	0.193
	(0.006)	(0.001)	(0.005)	(0.006)	(0.001)	(0.005)	(0.184)
Share of owner-occupied units	-0.004	-0.002*	0.008	-0.006	-0.002**	0.008	0.735
	(0.008)	(0.001)	(0.007)	(0.008)	(0.001)	(0.007)	(0.178)
Share of vacant units	1.399**	0.040	-0.972*	0.466	0.034	-0.755	0.166
	(0.697)	(0.070)	(0.575)	(0.648)	(0.066)	(0.550)	(0.152)
Share with mortgage	0.010	-0.001	-0.006	0.013	-0.001	-0.007	0.562
	(0.008)	(0.001)	(0.007)	(0.008)	(0.001)	(0.007)	(0.180)
ZIP Code FE	Y	Y	Y	Y	Y	Y	–
County-Year FE	Y	Y	Y	Y	Y	Y	–
State-Year FE	N	N	N	N	N	N	–
Baseline Population	N	N	N	Y	Y	Y	–

Table C.11: Estimates sensitivity

	All Mitigation Projects							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coverage	-0.030	-0.032	-0.033	-0.047	-0.045	-0.050	-0.029	-0.055
	(0.045)	(0.045)	(0.049)	(0.055)	(0.048)	(0.054)	(0.046)	(0.060)
Storm	-0.003	-0.003	-0.004	-0.005	0.001	-0.003	-0.002	-0.002
	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)	(0.010)	(0.010)	(0.011)
Coverage × *Storm*	0.443***	0.444***	0.451***	0.454***	0.443***	0.473***	0.446***	0.486***
	(0.131)	(0.131)	(0.134)	(0.139)	(0.135)	(0.137)	(0.132)	(0.147)
ZIP Code FE	Y	Y	Y	Y	Y	Y	Y	Y
County-year FE	Y	Y	Y	Y	Y	Y	Y	Y
Labor Composition	N	N	Y	N	N	N	N	Y
Housing Composition	N	N	N	Y	N	N	N	Y
Media Markets	N	N	N	N	Y	N	N	Y
Demographics	N	N	N	N	N	Y	N	Y
Weather	N	Y	N	N	N	N	Y	Y
Baseline Population	N	Y	N	N	N	N	N	N
Observations	82,360	82,360	82,360	82,360	82,360	82,360	82,360	82,360
R^2	0.57	0.57	0.57	0.57	0.57	0.57	0.57	0.57

Note: Coverage is centered at its mean (μ=0 ; s=0.101). The outcome variable includes the total number of properties having received mitigation against future storms under the HMG program between 2010 and 2018 (μ=0.083 ; s=0.975). Standard errors are clustered at the county level and reported in parentheses.

Chapter 4

The Dynamics of Land Development around Flood Zones

4.1 Introduction

Do people learn their lesson when a flood occurs? This provocative question is not so straightforward. Of course, floods are rare stochastic events, usually violent, often causing material losses, injuries, and sometimes even death. In Spain, they have killed more than heat waves and wildfires combined between 1995 and 2015[1]. Every year, they cost an average 800 million euros to the Spanish economy, and are related to almost half of insurance compensation requests since 1971[2].

However, floods tend to occur near water bodies, which are historically valued both for their amenities and intrinsic economic potential. In Spain, the vast majority of development occurs right outside flood zones, which are defined as spaces with a probability equal to or higher than one chance out of 500 of being inundated per year. The trade-off between apparent risks and water accessibility has lead individuals to cluster as close as possible from these zones. One building out of ten is located within the first

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[*] This chapter was co-authored with Rodrigo Martínez Mazza.

[1] Spanish Ministry of the Interior – Civil Protection and Emergencies `http://www.interior.gob.es/web/archivos-y-documentacion/proteccion-civil-y-emergencias`

[2] Spanish Ministry of the Ecology – Water Department `https://www.miteco.gob.es/es/agua/temas/gestion-de-los-riesgos-de-inundacion/`

100-meter fringe outside a flood zone, thus exposing large shares of development to potential disasters. A simple model of location decision could predict that all else equal, after an inundation, forward-looking individuals develop less or farther away from these risky zones as their expected risks increase. Yet, we do not know how people form long-term investment decisions such as development, after a disaster. In particular, it is unclear how risk preferences are affected by a rare, infrequent, natural hazard. For example, it could very well be that with the prospect of unsure future losses, some individuals become more risk-prone. Therefore, as a flood occurs, do agents adapt by developing less or father away from flood zones?

In this paper, we study the dynamics of land development in municipalities having experienced a flood. First, we want to know if new development is affected by a flood event and whether this new development takes place farther away from flood zones or on higher ground. Second, we are interested in knowing the duration, if any, of these effects over time. Finally, we examine whether these outcomes are influenced by factors like historical flood frequency or distance to flood zones.

Our primary dataset includes the universe of buildings in Spain as provided by the Land Register Administration — that is, approximately 12 million georeferenced units[3]. We combine this information with the complete dataset of digitalized floodplains maps for Spain to identify buildings' location with respect to flood zones. Additionally, we extract detailed terrain elevation data from satellite images. Finally, we gather nearly 1.800 historical flood records identified at the municipal level and spanning between 1900 and 2010. We complete our analysis with socio-economic covariates gathered at the municipality level.

We use changes in surface, distance to flood zones, and elevation of new development with respect to the year before a flood event to capture the new development response – that is to say, we use an event-study framework to investigate the effect of historical floods on land conversion decisions. We make this analysis both at the municipal level and at different fringes outside the floodplain. We are primarily interested in the spaces right outside flood zones as flood zones are historically identified as areas at risk. Therefore, production facilities, risk-prone, or uninformed agents could select inside these areas, leading to less interpretable results. Our empirical strategy relies on the assumption that conditional on municipality and year

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[3] Excluding the Basque Country and Navarre, who have an independent land register.

fixed-effects, the timing and the extent of a flood is as good as random. This unanticipated shock allows us to conduct a difference-in-difference analysis around the appearance of a flood event for each endangered municipality.

Our main results indicate that experiencing a flood leads to a decrease of -14.6% in new development in the year following the flood, which peaks down to -26.7% in the sixth year after the flood. However, new buildings are neither developed farther away from the nearest flood zone nor developed on higher terrain: new development location is similar to what it was before the disaster. The flood hazard's impact is statistically strong and homogeneous in the first 250 meters outside the flood zone. It is also persistent over more than 30 years in the flooded municipality. This outcome is influenced by municipalities having suffered at least another flood in the previous years. Municipalities not having experienced any flood for more than two decades experience a similar decrease in development immediately after the flood, but which is reabsorbed after only three years. Finally, the post-1986 era, i.e., after the central government adopted a legal framework to regulate development around flood zones, is driving most of these findings. Before that, a flood had very little if no impact on new development.

Several mechanisms could explain this effect. First, it could be that the permanent decrease in new development among municipalities having suffered more frequent episodes in the past – and despite the absence of new floods, suggests that individuals learn from the history of disasters. If this is the case, the impact of the post-1986 era could be indicative of the central role of legislation in raising individuals' awareness. Alternatively, it could also be that repeated floods lead to permanent degradation of the most vulnerable production factors, in particular in rural areas (Deschênes and Greenstone, 2007; Fisher, Hanemann, Roberts, and Schlenker, 2012), hence pushing the municipal economy into a local poverty trap, whose consequences affect new development several decades after the last flood. While we cannot totally discard this last possibility, it is interesting to note that not only are our results almost entirely driven by the most recent decades, but they also appear to be caused by densely populated areas, whose economy is, in general, less vulnerable to natural disasters.

However, if individuals do learn from previous catastrophes, our results question the formation of beliefs when a disaster occurs. Indeed, individuals do not seem to insure themselves by deciding to build farther away, or on higher ground, with respect to the source of risks. New development decreases permanently irrespective of the distance to flood zones. We speculate that, if individuals do learn from history, these facts could be explained by a misinterpretation of the local probabilities of being flooded. This cognitive failure could be either caused by an availability bias (Tversky and Kahneman, 1974)[4], or an aversion to the expected loss of amenities from developing farther away from water sources (Kőszegi and Rabin, 2006, 2007).

Overall, this paper contributes to several strands of the new climate-economy literature[5], and particularly on how economic agents form disaster beliefs in the wake of climate change. First of all, we wish to understand how urban development varies after a flood event. Climate-driven variations in income are well-documented (Barrios, Bertinelli, and Strobl, 2010; Dell, Jones, and Olken, 2009, 2012; Hsiang, 2010; Hsiang and Narita, 2012; Lobell, Schlenker, and Costa-Roberts, 2011; Nordhaus, 2006, 2010), and a large share of the literature has argued those earnings windfalls fostered out-migration as an adaptation strategy (Munshi, 2003; Feng, Krueger, and Oppenheimer, 2010; Feng, Oppenheimer, and Schlenker, 2012; Boustan, Kahn, and Rhode, 2012; Hornbeck, 2012; Hornbeck and Naidu, 2014). Research has so far identified these population movements at the inter and intra-national levels. Albeit this paper does not identify climate-driven migration per se, it certainly contributes to the research on settlement choices in the aftermath of a disaster: increased perception of flood risk impacts expected quality of life, hence the final location and housing decisions.

This study is also closely related to the dynamics of risks' perception. For instance, using the bombing in Japanese cities during WWII, Davis and Weinstein (2002) shows that urban areas quickly converge back to their population levels after a negative shock. Although their study setting differs from ours, it is interesting to learn how agents respond to a rare, infrequent, and unexpected shock. Their results raise the question of individuals' risk preferences and how they are affected by a disaster. For instance, Barrage and Furst (2019) shows that coastal development is negatively as-

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[4] The availability bias is the human tendency to think that examples of things that come readily to mind are more representative than is actually the case. For instance, shark attacks were not a significant source of fear until the 1975 movie 'Jaws.' It is a case of miscorrelations based on experience. Here, it corresponds to the belief that an entire municipality has the same probabilities of being flooded because part of it was inundated once.

[5] For an extensive literature review, see Dell, Jones, and Olken (2014).

sociated with sea-level rise risk in US counties where individuals believe in climate change. While the economic theory assumed that individuals' risk preferences are stable across time (Stigler and Becker, 1977), recent studies state that negative shocks — in particular, from natural disasters; induce changes in these preferences. For instance, Gallagher (2014) shows that individuals update their beliefs of the likelihood of flood occurrence based on the discounted history of floods, and are more likely to get flood insurance when these beliefs are strong. Some even argue that individuals become more risk-tolerant (Eckel, El-Gamal, and Wilson, 2009; Voors, Nillesen, Verwimp, Bulte, Lensink, and Van Soest, 2012; Callen, Isaqzadeh, Long, and Sprenger, 2014; Hanaoka, Shigeoka, and Watanabe, 2018), whereas others find that people become more risk-averse in the aftermath of a negative event (Jakiela and Ozier, 2019; Malmendier and Nagel, 2011; Brown, Montalva, Thomas, and Velásquez, 2019; Cameron and Shah, 2015; Fisman, Jakiela, and Kariv, 2015a; Fisman, Jakiela, Kariv, and Markovits, 2015b). We contribute to these empirical questions by discussing how new development occurs after a flood and the channels at play in these particular changes.

4.2 Flood Zones in Spain

Spatial Concentration — Flood zones, or floodplains, are defined based on the historical and geomorphological probabilities of being flooded in a given period. This does not mean that the zone was necessarily flooded, or that it will ever be, but it reflects the potential risk. For instance, a 100-year floodplain corresponds to an area with an average of 1%-chance of being flooded in any given year. This is equivalent to one chance out of four to be flooded over a 25-year mortgage period. By extension, a 500-year floodplain corresponds to a .2%-chance of suffering the disaster, whereas a 10-year floodplain indicates a chance of 1 out of 10 of being flooded in a given year. These definitions are ad-hoc, and in Spain, the legislator considers as a flood zone (*"zona inundable"*) any area located within a 500-year floodplain. Although today's legislation varies slightly depending on the space definition, in practical terms, most of these areas' frontiers are very close. In more than 90% of the cases, 500-year floodplain borders are only 20 meters away from their 100-year counterparts. The spatial concentration of flood probabilities points to the existence of recognizable terrain patterns.

A Common Wisdom — Despite this modern definition, it is highly improbable that individuals were ignorant of flood zones before the existence of a legislative apparatus. For more than ten centuries, people located out of floodplain as they learned to recognize these spaces early on in history. The earliest traces of adaptation to flood events date back to the Middle Ages.

While individuals may have considered these disasters as a divine outcome, local authorities already began to modify the terrain accordingly, building levees and floodwalls. Ancient fragments of dams named *'turciae'*, made of wood, rocks, and dirt, are mentioned for the first time in a 816 codex – *'De aggeribus juxta Ligerim fadendis*[6]*'*. Other examples of adaptation to flooding risks span across history. In 1150, the French royal authorities created a corporation of engineers specifically meant to fight flood disasters. In 1160, Henry the 2nd of England commanded that local engineers stayed in villages to take care of the levees. In the 17th century, philosophers and mathematicians started to advocate for a higher knowledge of these catastrophes[7].

Although not as detailed as today, the risk was already inferred based on the regular observation of flood events in some areas – in particular agricultural regions. Engineers and statisticians mastered the cyclical prediction of floods, and geographers drew the first official flood maps by the mid-19st century. Figure 4.1 is an early example of such cartographic exercises made in the aftermath of the Santa Teresa Flood in Spain.

In Spain, the so-called 'Law of Waters' – that administered rivers and lakes in the country, was implemented shortly before, in 1866. During the first half of the 20th century, a series of regional and central government

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[6] Which translates to 'On the (*De*) production/construction (*faciendis*) of levees (*aggeribus*) next to (*juxta*) the Loire river (*Ligerim*).'. The king Louis the 1st, son of Carolus Magnus, ordered the report. Knowing it was written in the early 9th century, the local populations likely knew about flood risks way before.

[7] In 1637, French philosopher and mathematician René Descartes wrote: "In place of the speculative philosophy taught in the schools we might find a practical philosophy through which knowing the power and the actions of fire, water, air, the stars, the heavens and all the other bodies in our environment as clearly as we know the various crafts of our artisans, we could (like artisans) put these bodies to use in all the appropriate ways, and thus make ourselves the masters and (as it were) owners of nature. This is desirable [...] for the preservation of health, which is certainly the chief good and the basis for all the other goods in this life."

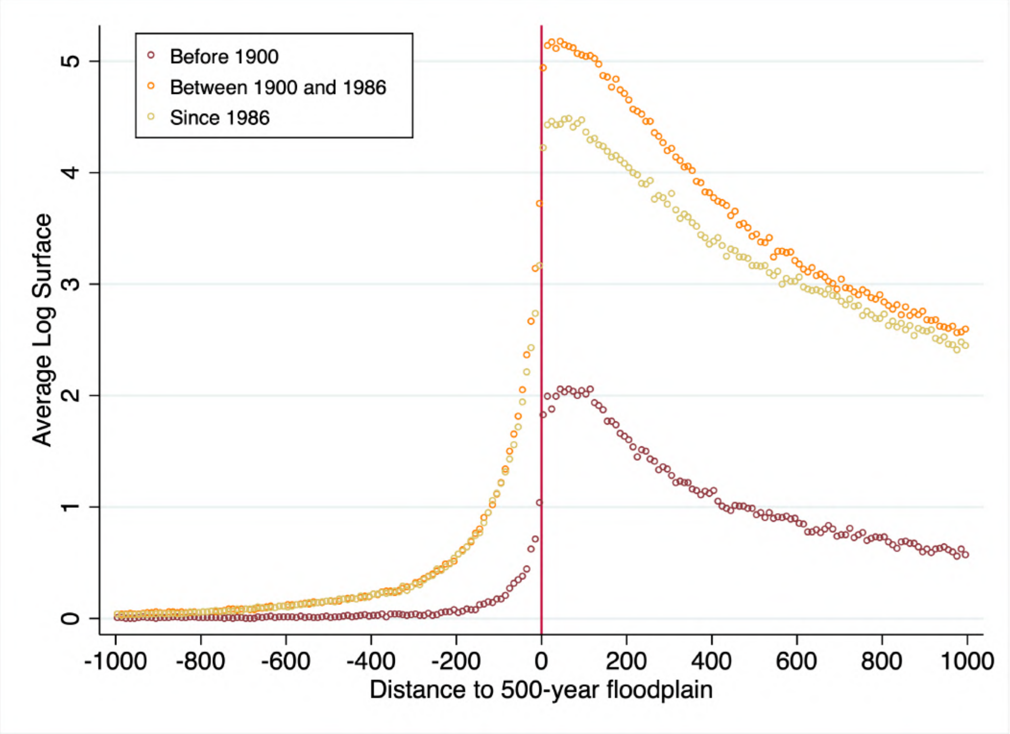
Figure 4.1: The 1879 Santa Teresa flood along the Segura river

Notes: This map depicts the cartography of the zones affected by the Santa Teresa flood, on October 18th,1879. Extracted from the *'Crónica General de las Inundaciones en Alicante, Murcia y Almería de 14 y 15 de Octubre de 1879*, from Benedicto Mollá (1883, Spanish National Archives).

policies organized the use of water resources. However, it was only in 1986 that the central government started to regulate development inside flood spaces while making recommendations for outside fringes. The 1986 regulation specified that the central government could implement limitations to the urban growth inside flood areas and that the regional governments could establish additional norms to these decrees. Any developer needed to receive the authorization to build inside a floodplain from the regional water authorities before construction begins. This law has been amended multiple times in the early 21st century to fit the local risks. It was finally entirely modified and enhanced in 2016. Any new development in flood areas must now comply with several specific requirements and benefit from the special authorization of the local government and the regional water authorities.

Risks vs. Amenities — If the local dangers of flood risks have been known for more than ten centuries, it is reasonable to think that the decision to develop inside or outside of the floodplain reveals the heterogeneity of risk preferences towards inundations – at least before any legislation took effect. Figure 4.2 displays the yearly average of the log of surface newly

Figure 4.2: Development around Floodplains' borders



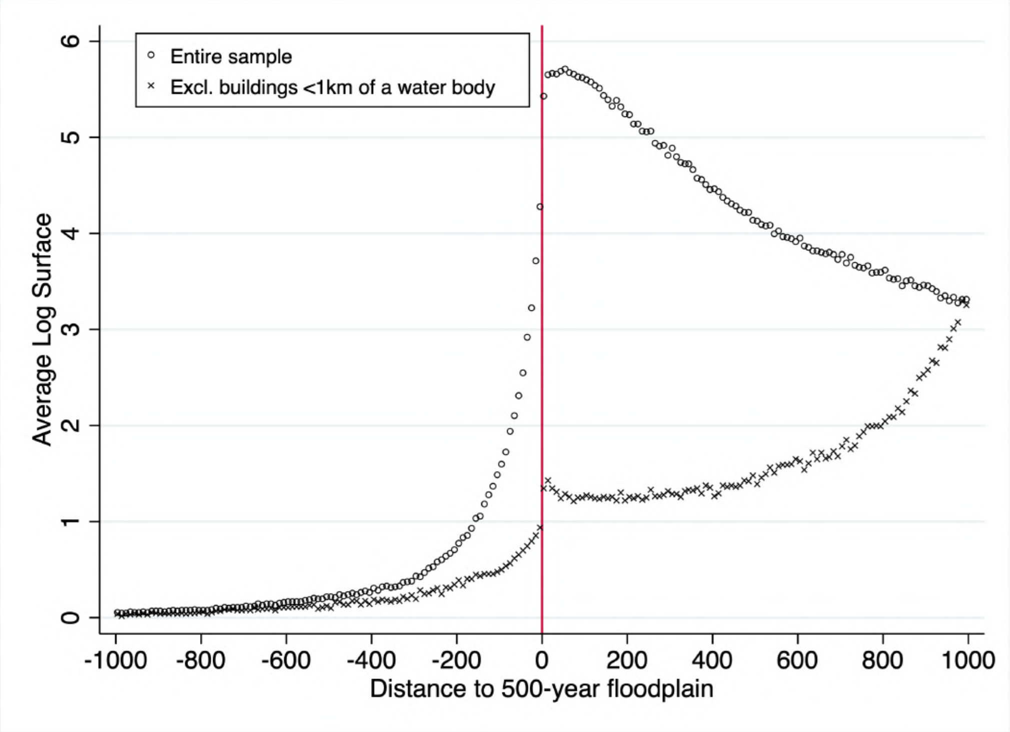
Notes: The outcome is the yearly average log new development (measured in squared meters, per year) across municipalities in Spain. On the x-axis is the distance in meters to a floodplain border. Negative values on the x-axis correspond to the inside of a flood zone. The flood zone is defined as a 500-year floodplain, like specified by the Spanish law. Each dot represents the outcome within a 10-meter buffer.

developed around a 500-year floodplain's border for different moments in history.

It is interesting to see similar patterns in the location of new development. Indeed, it appears that new development concentrates right outside the flood zone border across all the periods studied. In particular, this pattern does not seem to be driven or initiated by the introduction of flood zone regulation. Similarly, some development occurs inside the floodplain in all periods. Available land could explain the lack of development far inside the flood zone. However, the flood zone frontier clearly characterizes a break in the density of new development at all time.

Then, what explains such sorting at the edge of the flood zone? Water spaces seem to absorb most of the observed discontinuity. Once we exclude buildings within 1km from a water body – rivers, lakes, or sea; that is, once we compare the density of new development around flood zones' borders in the absence of nearby water bodies, most of the discontinuity vanishes.

Figure 4.3: Development & Water amenities (1900-2010)



Notes: The outcome is the yearly average log new development (measured in squared meters) across municipalities in Spain. On the x-axis is the distance in meters to a floodplain border. Negative values on the x-axis correspond to the inside of a flood zone. The flood zone is defined as a 500-year floodplain, like specified by the Spanish law. Each dot represents the mean outcome within a 10-meter bin.

Figure 4.3 describe this phenomenon.

In the absence of water nearby, development outside the floodplain increases with distance from the flood zone border. With water, development outside the floodplain decreases with distance from the flood zone border. A potential explanation could be that risk-averse individuals, considering both water benefits and the risks represented by flood zones, bunch right outside the floodplain where both the amenity and economic gains net of the perceived risks are maximized. On the contrary, when water bodies are far away, the trade-off between the perceived dangers and expected gains vanish, and there is no apparent reason for building close to a hazardous area. In the rest of this paper, we consider flood zones to capture individuals' flood risk perceptions.

If individuals, motivated by access to water, bunch right outside flood zones because they recognize flood zones as hazardous areas, how does new development occur when a flood occurs? Do individuals build less? Farther away? On higher terrain? How long does it take before building again close to flood zones?

4.3 Data

The Spanish Land Register — We construct our main dataset with the Spanish land register, which contains information on any building ever developed and currently standing in Spain, to the exception of the Basque Country and Navarre[8]. The dataset contains more than 11.7 million geo-referenced units, their total floor surface, the number of dwellings, and the building's current use. We are able to measure the base surface of every unit. The dataset additionally provides information on the dates of development, of the last renovation, and of registration in the land register. One limitation of the dataset is that we do not observe destroyed buildings and that some dates appear to be roughly rounded towards the nearest decade, especially for ancient buildings. Figure 4.4 displays this information, with a zoom on the city of Málaga, in the region of Andalucia, which we use to provide a visual description of our data.

Floodplains and Elevation data — We use the information provided by the Spanish National Institute of Geography to get the digitized floodplain maps that enter our second dataset[9]. This information is available for 10, 50, 100, 500-year flood maps, and water bodies. In what follows, we refer to floodplains, or flood zones, the 500-year flood maps, following the official geographic definition used by the national authorities. Finally, we add the digital elevation information derived from the LIDAR 25-meter grid. Figure 4.5 provides a visualization of our final dataset for the city of Málaga mentioned above.

These detailed plans allow us to precisely measure the base surface of each building and its distance to the nearest flood zone, water space, and the corresponding terrain elevation. For instance, we can see that the neighborhood of *Campanillas*, at the north-west of Málaga, has a large share of development built along or inside the flood area, despite the adjacent higher terrain that rises above the valley (see Figure 4.6).

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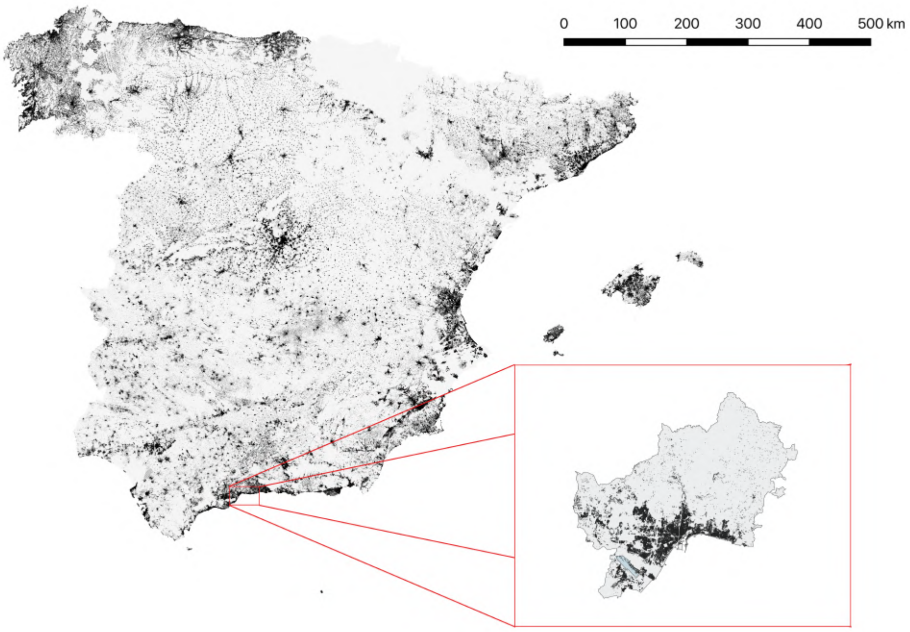
[8] These regions have their own land register, which we cannot access at the moment.

[9] The data for the region of Catalunya must be downloaded from the Catalan Minister of Waters website.

Figure 4.4: Distribution of Buildings in Spain



Notes: In the main picture, each dot corresponds to a building's centroid. The black sprawl describes the density of development in the country. The Basque Country and the Navarre regions are excluded, as they have a different land register. The Canary Islands are excluded from the map for practical overall display. The city represented in the secondary picture is Málaga (Andalucia). A graphical representation of the city is displayed in Figure 4.5.

Historical Floods — Campanillas (Figure 4.6) has been flooded six times in the last decade[10]. We collect data on historical floods from the *National Catalogue of Historical Floods.* The goal of this dataset is to compile information on historical floods to identify hazardous locations and prevent future catastrophes. Thanks to this information, we identify more than 5000 municipalities affected by a total of nearly 1800 flood events between 1900 and 2010. Figure 4.7 presents the spatial distribution of these disasters.

The main advantage of this unique dataset is that we can identify local floods spanning over more a century. However, the first limitation of this data is that we cannot determine the exact extent of a specific flood within a municipality. That is to say, contrary to modern digitalization tools, we

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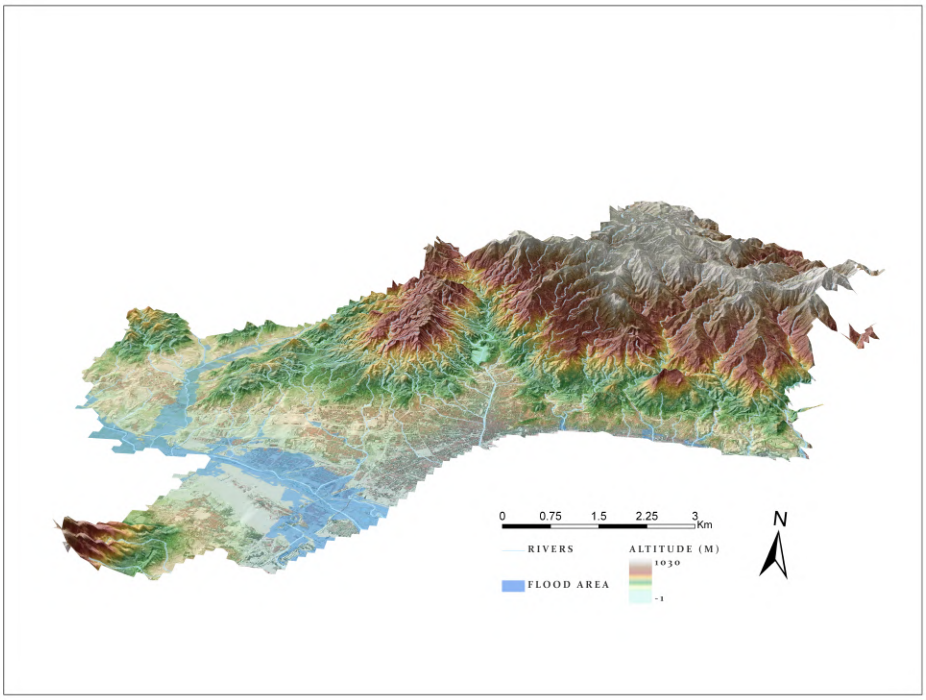
[10] More recently, Campanillas suffered from the storm, Gloria. More than 400 liters of water caused the flooding of the river. `https://www.malagahoy.es/malaga/inundaciones-malaga-gloria-campanillas-litros-lluvia_0_1432957293.html`

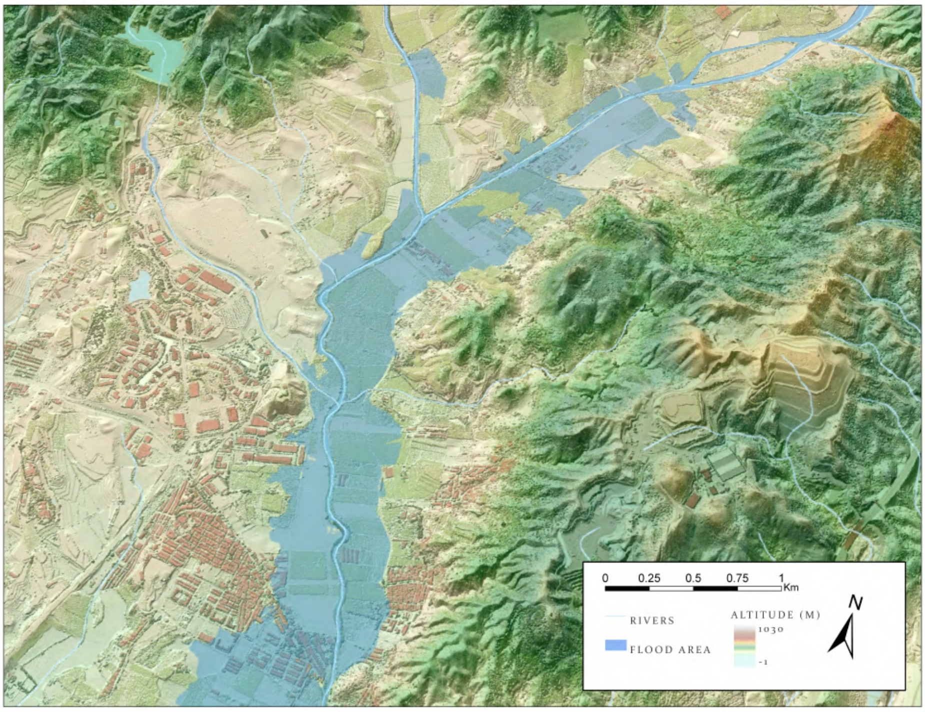
Figure 4.5: Digital Model of the city of Málaga (Andalucia, Spain)



Notes: This picture is a digitalized representation of the city of Málaga (Andalucia) with exaggerated heights, that generated by combining the information from (a) the Spanish Land Register for the buildings, (b) the Spanish National Institute of Geography for the floodplains, and (c) LIDAR 25-meter grid for elevation. This is a visual representation of one of the cities of our raw final dataset – that includes all the Spanish municipalities (except from the Basque Country and the Navarre region).

cannot identify which parcel or parcels within a municipality have been flooded. Hence not having a within-municipality intensive measure of a flood extent forces us to use an extensive measure of floods. This extensive measure consists in identifying whether a municipality suffered a flood or not in a given year. The second limitation comes from the very nature of historical records: it could be that not all floods were registered in this historical log. Indeed, the accompanying methodological document states that the only way to compile the different entries was to 'run an inquiry in the documentation of the official archives of the General Directory of Hydraulic Works, bishoprics, deputations, universities, newspaper archives, etc.'. Therefore, we must acknowledge that despite the data collection effort, this information is more of a retrospective historical survey rather than a systematic data gathering. It might then be subject to measurement

Figure 4.6: Digital Model of the neighborhood of Campanillas (Andalucia, Spain)



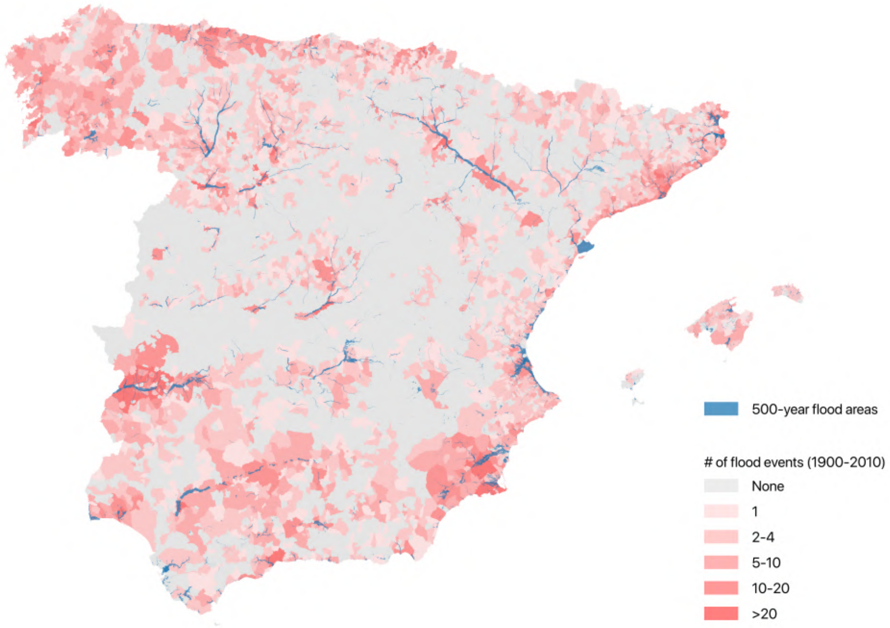
Notes: This is a zoom on the neighborhood of Campanillas, at the north-west of Málaga (see Figure 4.5). Details on the location and the elevation of the different buildings with respect to the floodplain are visible here.

error, especially for the most ancient decades. Nonetheless, the fact that we will be studying municipalities that have been hit at least once over the period mitigates such fear. That is to say, municipalities whose floods were systematically disregarded in the historical logs – for instance, because they took place in remote areas – will not be included in our final dataset.

Overall, we can see from Figure 4.7 that most floods occurred along coastal areas, near mountain chains, and along the most important drainage basins. Spain's central plateau is historically not as populated as the rest of the country (except for the region of Madrid). This could be both a cause and a consequence of the absence of known flood events. The weather in that zone is arid, with few rivers compared to the rest of the country.

Final Dataset — Our final dataset is a balanced panel of Spanish municipalities that have been flooded at least once between 1900 and 2010

Figure 4.7: Distribution of the Floods in Spain



Notes: Spatial distribution of flood events in Spain (1900-2010) according to the *National Catalogue of Historical Floods*, and location of the 500-year floodplains. Note that many flood spaces are not visible at this national scale and would require a closer look to be noticeable. For instance, the Malága floodplain (Figures 4.5 and 4.6) is barely visible here.

(except for the Basque Country and Navarre). This represents 4,411 municipalities over 110 years, and more than 8.68 million buildings developed. On average, municipalities in our sample were flooded 3.7 times. The median number of flood events per municipality in our sample is 2. In total, we observe more than 15,000 municipality-flood events. For each municipality-year, we know the base surface developed inside and outside a floodplain, the total floor surface, the average distance to the nearest flood zone border, and the average terrain's elevation where the buildings are constructed. Distances and elevations have been computed from the centroid of each building. We also calculate alternative distance and elevation measures weighted by the surface developed for a more accurate measure of new development's exposure.

4.4 Empirical Strategy

We are interested in capturing the development response in the aftermath of a flood. In particular, we look at the new development (measured as *new* buildings' base surface in squared meters), the elevation of new development (measure as the terrain's height in meters), and the distance of new development from floodplains (measured as a geodesic distance in meters). We look at these variables at the municipal level, and later on, we study fringes right outside flood zones. The empirical strategy will follow closely that of Gallagher (2014). Our main dataset is a balanced panel of Spanish municipalities having been hit by a flood between 1900 and 2010, as described in Section 4.3, and our principal specification is:

$$y_{mt} = \sum_{\tau=-T;\tau\neq-1}^{\tau=T} \beta_\tau Flood_{m\tau} + \alpha_m + \gamma_t + \epsilon_{mt} \tag{4.1}$$

where y_{mt} is the (log of the) outcome of interest in municipality m at calendar year $t \in T$. Our variable of interest, $Flood_{m\tau}$, is an event time indicator that takes value 1 if a municipality m was hit by at least one flood in $t - \tau$. Then, α_m and γ_t denote municipality and year fixed effects, respectively. ϵ_{mt} is the error term.

It is important to notice that by using pre-event and post-event dummies, we do not impose any particular functional form on the effects of floods in the various outcome variables. The dummies $Flood_{m\tau}$ capture the average of the outcome variable across all municipalities that were affected by a flood event τ periods before or after treatment, controlling for nationwide shocks and municipalities invariant characteristics.

The results are relative to the year previous the flood event, which is the omitted category in the regression. As consecutive flood events can hit municipalities within a year, $\tau = 1$ can represent both the year after a flood event or the year after a series of flood events.

The municipalities fixed effects capture time-invariant characteristics within a municipality, such as a municipality's geographical patterns. By accounting for year fixed effects, we control for shocks to the Spanish economy, as well as regulation changes issued in a given year that affect all municipalities. Our identifying assumption is that conditional on municipality fixed characteristics, particularly its geography and time trends, the timing of inundation is as good as random. Finally, we allow for unobserved correlations between municipalities within a county (*'comarca'*) by clustering standard errors at that level.

Our main window of interest looks at new development responses ten years before and after a flood event. Coefficients are binned in the tails following Schmidheiny and Siegloch (2020). Formally, these endpoints coefficients are defined as $Flood_{m,T} = \sum_{s=t+10}^{T} Flood_{ms}$ after the flood, and $Flood_{m,-T} = \sum_{s=-T}^{t-10} Flood_{ms}$ before the flood. Note that as we are restricting our window to a finite number of lags and leads around our treatment, we explicitly assume that the development response to floods is similar above this window.

Additionally, to study the long term effects of flood events on development, we adapt 4.1 by expanding the observation window up to 30 years after the event.

4.5 Results

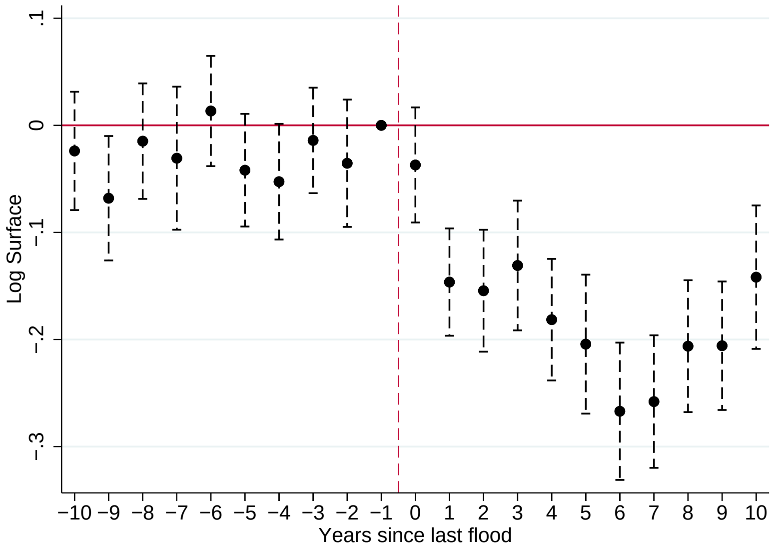
4.5.1 Main results

Short-run effects — Figure 4.8 depicts the results of using Equation 4.1 to assess the impact of a flood event on new development - measured as the log of the total base surface built in a given year, showing the coefficients in β_τ in our 1900-2010 municipality panel. The x-axis depicts the distance in years to the flood event, with the years in indexed with negative numbers being the ones preceding the flood event. All results are normalized to the year previous to the flood so that coefficients can be interpreted as the percentage change in the surface built relative to the year before the flood event. Vertical dashed bars represent a 95% confidence interval around the estimated coefficients.

First, there seems to be no significant difference in the new development in the municipality in the years preceding the flood event. In other words, the effect of a future flood is insignificant at the 5% level (except for the 9th year before the event), and systemically economically quite small. This absence of pre-trends largely alleviates potential anticipation effects.

In the year of the flood, we do not observe any significant change in new development. It might be either because building permits were already emitted, or because the flood occurred late during the year. That said, in the year following a flood, the amount of developed land in a municipality

Figure 4.8: Effect of a flood event on new development.



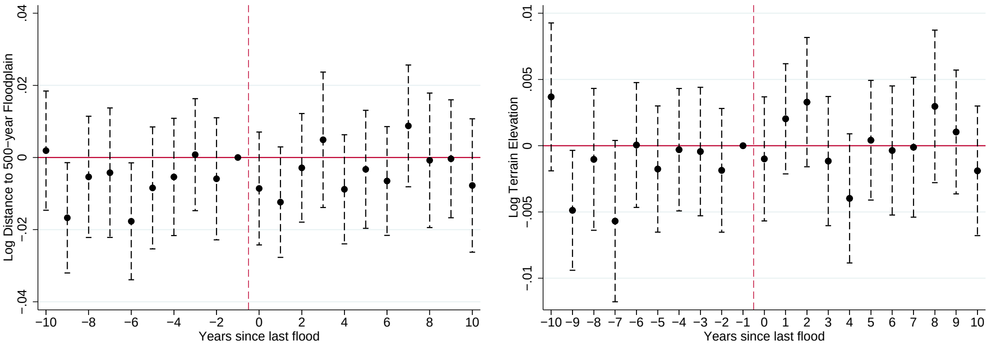
Notes: The vertical axis measures the effect of a flood event on log of the new surface built. Results are based on Equation 4.1. The coefficient for the year before a flood is normalized to zero. The bars show the 95 percent confidence interval. Standard errors are clustered by county. There are 4,411 municipalities observed between 1900 and 2010 in the event study.

drops by a strong, immediate, and significant -14.6% with respect to the year preceding the event. The effect tends to deepen in the following years reaching a negative coefficient of -26.7% in the sixth year after the flood.

However, while results suggest a decrease in new development, there is no evidence for a reallocation of this development away from flood zones. Figure 4.9 depicts the dynamic effect of a flood event on the average distance of new surface built in a given year to the nearest flood zone: the flood event has no impact on this outcome, neither before nor after the catastrophe, and the coefficients of interest are also economically quite small.

Results are quite similar when looking at the average elevation of new buildings right after a flood (see Figure 4.9). Not only are the average treatment effects not significant statistically, but they are also economically very small. Indeed, all confidence intervals are varying between -1.1% and +1% at maximum. Hence, while suffering a flood leads to a massive decrease in the amount of newly developed buildings, we do not observe that this development takes place neither farther away from floodplains nor on higher ground. Figure A.1 in the Appendices shows a similar result when looking at the distance to water.

Figure 4.9: Effect of a flood event on distance to nearest flood zone and on elevation.



(a) Effect on distance to flood zone **(b)** Effect on elevation

Notes: These figures plot the impact of a flood on the log distance (in meters) and the log terrain elevation of new development. Results are based on Equation 4.1. See Figure 4.8 for more details.

Long-run effects — Looking a the impact of floods around floodplains up to 30 years after the event[11], we find that individuals appear to reduce development permanently (Figure 4.10). Indeed, the catastrophe's impact is still present several decades after it occurred. In particular, 15 years after the flood, most estimates remain significantly negative, and varying between -15% and -20% with respect to the year before the inundation.

Interestingly, the permanent reduction in development with respect to the year preceding the flood contrasts with Gallagher (2014), who finds that households are more likely to contract a flood insurance policy only in the immediate years after a large flood. This latter effect vanishes quickly within the decade. One reason that could explain this difference is that flood insurances are more likely to apply to already built units. Here, we focus on new development, which is a long term decision involving high costs.

4.5.2 Heterogeneity Analysis

Floods Saliency — Do past floods impact development in the aftermath of an inundation?

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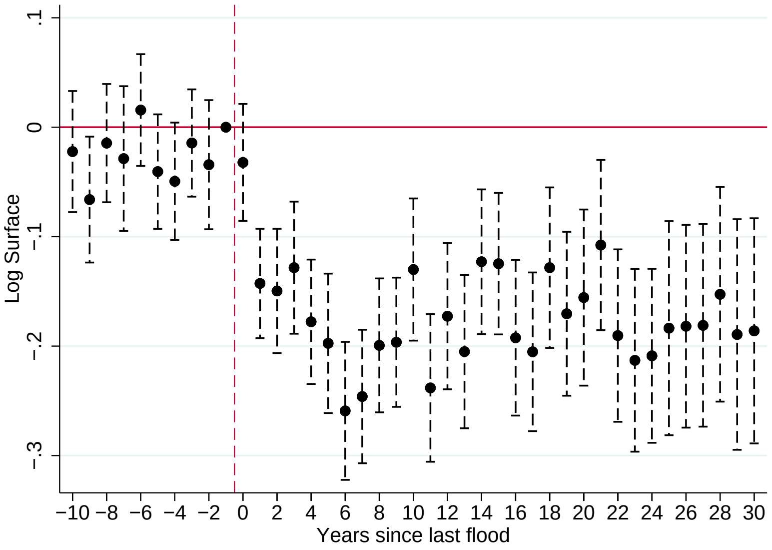
[11] In comparison, in 2018, the average mortgage length was 24 years for the residential estate (https://www.ine.es/daco/daco42/daco426/h1218.pdf).

Figure 4.10: Long run effect of a Flood event on surface built.



Notes: Long run effect of a Flood event on log of the new surface built. Results are based on Equation 4.1. See Figure 4.8 for more details.

To answer this question, we estimate the following specification:

$$y_{mt} = \sum_{\tau=-T;\tau\neq-1}^{\tau=T} \beta_\tau Flood^{l}_{m\tau} + \sum_{\tau=-T;\tau\neq-1}^{\tau=T} \beta_\tau Flood^{m}_{m\tau} + \alpha_m + \gamma_t + \epsilon_{mt} \quad (4.2)$$

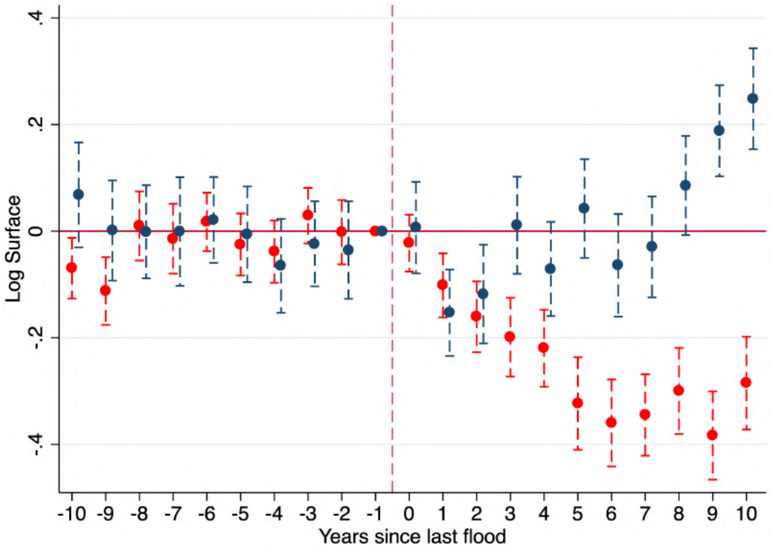
where $Flood^{l}_{m\tau}$ is a dummy variable equal to one if, in $t-\tau$, another flood occurred in the previous x years; and $Flood^{m}_{m\tau}$ takes the value 1 if, in $t-\tau$, a flood occurred for the first time in *more* than x years. The rest of the equation is similar to 4.1.

Figure 4.11 presents the results when x equals 20 years. Red estimates depict the impact of a flood when there has been at least another inundation occurring less than 20 years ago. The blue series describe the impact of a flood event when there has not been another flood in the last two decades.

Pre-flood coefficients are not significant, and the absence of pre-trend suggests that before the flood, development increased as fast for municipalities hit in the previous 20 years, as for spared municipalities. In contrast, the sizeable decreasing trend after the flood for municipalities having suffered another inundation in the past two decades suggests that municipalities having suffered more floods drive the permanent average decrease in development. Municipalities not having sustained any catastrophe in the previous 20 years experience a slight decline in development, which is resorbed after three years, and quickly compensated by an increase in new development eight years after the flood. This suggests that history has little impact on beliefs if a repeated rare event happened a long time ago.

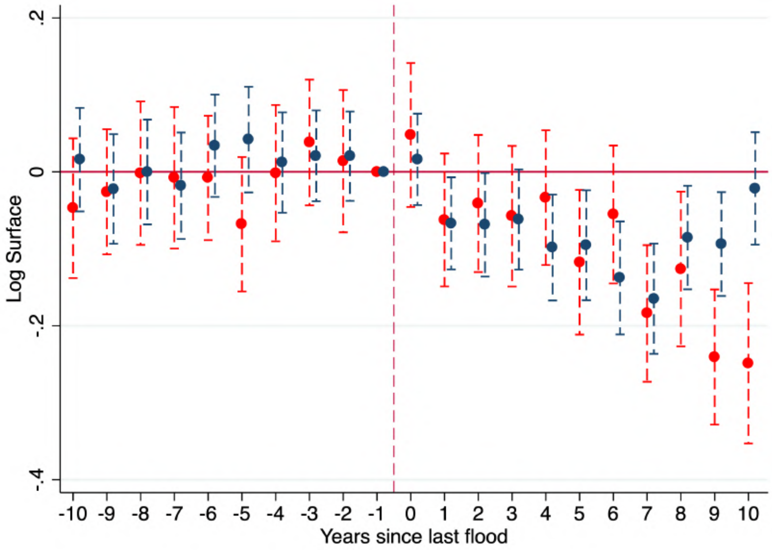
In comparison, when we reproduce the estimation 4.2 with a 3-year saliency period (rather than a 20-year saliency period), we see that both trends follow a quasi-similar path (Figure 4.12). First, in the absence of floods in the past three years, development takes a decade to resorb to its pre-inundation levels (compared to 3 years in the absence of floods in the past 20 years). We observe that coefficients are statistically similar until the 8th year after the flood (compared to the 3rd year in the absence of floods in the past 20 years). These results suggest that saliency, or repetition, of a rare event, matters for development only if it is sufficiently spaced in time. In particular, repeated history matters if the repetition is sufficiently distant in time to be unexpected but close enough not to be ignored. Figure A.3, in the Appendices, reproduces these results with different periods of return (from one to 30-year periods).

Figure 4.11: Effect of a Flood event on the average surface built according to flood history, 20-year period.



Notes: Effect of a Flood event on the average surface built according to saliency history. Red bars represent estimates when at least one flood occurred in the previous 20 years. Blue bars represent estimates when no flood occurred in the previous 20 years. Results are based on Equation 4.2.

Figure 4.12: Effect of a Flood event on the average surface built according to flood history, 3-year period.



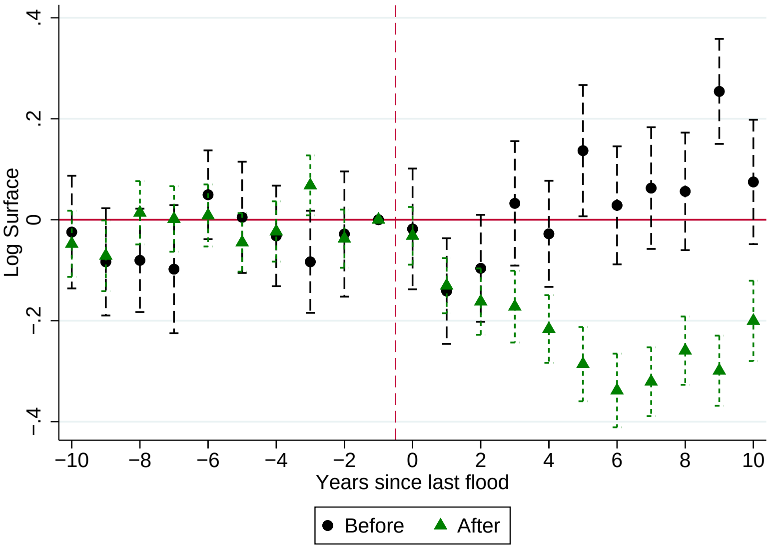
Notes: Effect of a Flood event on the average surface built according to saliency history. Red bars represent estimates when at least one flood occurred in the previous 3 years. Blue bars represent estimates when no flood occurred in the previous 3 years. Results are based on Equation 4.2.

Law of Waters — As mentioned earlier, an essential event in the prevention against flood episodes in Spain was the Law of Waters, implemented in 1986. This law was the first to establish nation-wide criteria for development inside floodplains and made recommendations for the architectures of buildings at risk around flood zones. Could this event have affected how development responds to floods? Figure 4.13 describes the impact of a flood on municipalities before and after 1986.

This suggests that the negative and permanent effect on development shown in Figure 4.8, is being driven by more recent flood events. Flood previous to the establishment of the Law of Waters had a short-lived impact on development, being only statistically different from zero on the year after the flood, and then returning to zero. Looking at more recent flood events, the effect on the surface developed is both more substantial and permanent. As in the main results, there is an initial negative impact on development on the year immediately after the flood event, which becomes larger in the following years, reaching its largest value in the sixth year after the flood.

Spatial Distribution of the effects — Although floods do not impact on the *average* distance of new development from the flood zone, could the

Figure 4.13: Effect of a Flood event on the average surface built before and after the Law of Waters' (1986).



Notes: Effect of a Flood event on the average surface built with respect to 1986. Here, we account for region-year fixed-effects to capture regional amendments to the Law of Water. Results are based on Equation 4.2.

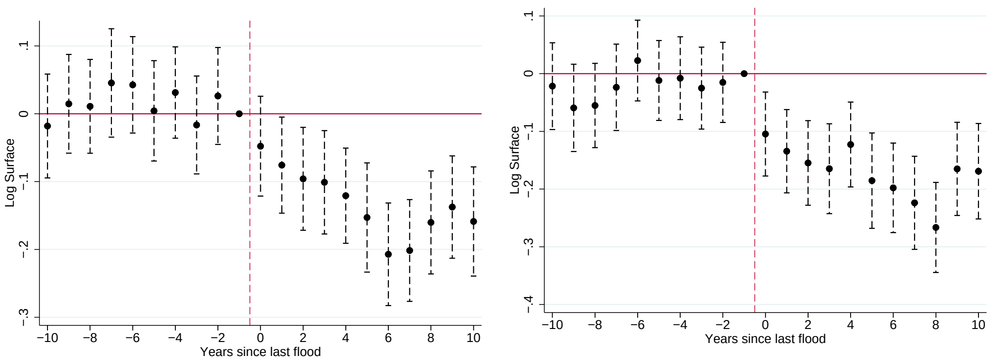
decrease in development still be stronger close to the source of hazards? To answer this question, we perform our analysis on samples restricted a couple of fringes right outside the floodplain limits. In particular, we look at development that occurred (a) less than 100 meters, and (b) between 100 and 250 meters from a flood zone border.

Experiencing a flood has a significant negative impact on new development, whatever the fringe of interest. If anything, the relative decrease in development between 100 and 250 meters from the nearest floodplain is larger than the one experienced less than 100 meters away from these spaces. In other words, the impact of a flood seems slightly stronger, farther away from the source of danger. This could be partly explained by the amount of available land at the different fringes. As we know, development concentrates at the border of the flood zone.

During the year following a flood, municipalities experience a -7.5% decrease within the 100-meter fringe from the nearest floodplain (see Figure 4.14). The shock peaks down on the seventh year after a flood, with -20.7% less new development relative to the year preceding the flood. The immediate downfall in the year following a flood is similar when looking at the 100-250m fringe. Indeed, at this fringe, municipalities experience a -13.4% change in new development right after the event. However, the shock is slightly sharper as development peaks down at an average -23.1% in the eighth year after the inundation. That said, it is important to remark that the overall amount of new surface built is typically lower in absolute terms farther from the flood zone. Overall, the patterns do not change dramatically across fringes – we observe a sharp decrease in new development irrespective of the distance to the nearest floodplain. Results are similar when focusing on the distance to water (see Figure A.2). Not only are individuals not transferring new buildings on safer ground, but they are also lowering development prospects there.

Finally, we are interested in knowing how developments respond to a flood event *inside* the floodplain. However, we know from section 4.2 that it is unlikely that individuals are completely unaware of the presence of flood zones. Individuals building inside flood zones are either uninformed or willing to take some extra risks. If the agents building inside flood spaces are risk-prone, we might observe very little change when a flood occurs as it is expected. We find that the impact is about twice smaller inside floodplains than the municipal average (see Figure A.5). There is no immediate bust in new development. Only three to four years after the inundation can we observe a 3.77% decrease in the new surface built, and this outcome peaks down to -8.8% on the eighth year after the flood, and in the absence of new hazards. After only ten years, new development is

Figure 4.14: Effect of a Flood event on the average surface built, according to different distances from the nearest 500-year floodplain.



(a) Less than 100m of a floodplain

(b) Between 100 and 250m of a floodplain

Notes: Effect of a Flood event on average surface built according to different fringes from the nearest floodzone. Results are based on Equation 4.1.

back to its pre-inundation levels inside the floodplain. A flood then causes a substantial diminution of new development everywhere in a municipality, and particularly so outside the floodplain – where risks are supposedly lower.

4.6 Discussion

Our main results indicate that a flood causes, on average, a large and permanent decrease in new development, irrespective of the distance to the flood zones. This effect is consistent with several potential mechanisms. In particular, the long-run impact of floods on development could be caused by a substantial negative shock to the local economy. Repeated inundations could affect the factors of production, and push the municipal finances in a local poverty trap. This would induce outflows of populations, and eventually, a potential lack of revenues for the local government that would explain the observed decreases in development.

While it is difficult to discard the former hypothesis entirely, it is interesting to note that the more recent decades fully drive our main results, especially since the central government implemented the 'Law of Waters.' The shock of the inundation on new development was quickly absorbed, before 1986. This could suggest that something different than an impact on the local factors of production drives the permanent impact on new development. Indeed, it would otherwise imply that municipalities hit by an inundation were more affected after implementing a reform designed to regulate development inside and around flood zones. Additionally, Figure A.4 in the Appendices indicates that rural municipalities do not trigger our main effect. Albeit rural municipalities differ in many aspects from their urban counterparts, they are known to be more vulnerable to natural disasters too (Deschênes and Greenstone, 2007; Fisher, Hanemann, Roberts, and Schlenker, 2012).

A Bayesian learning model could also explain the substantial changes in quantities of land developed in the aftermath of a flood (Gallagher, 2014). In particular, the lack of new hazards could generate a rising virtual sensation of safety, fostering development close to flood zones. In contrast, repeated flood episodes would fuel a feeling of danger and prevent new development from occurring.

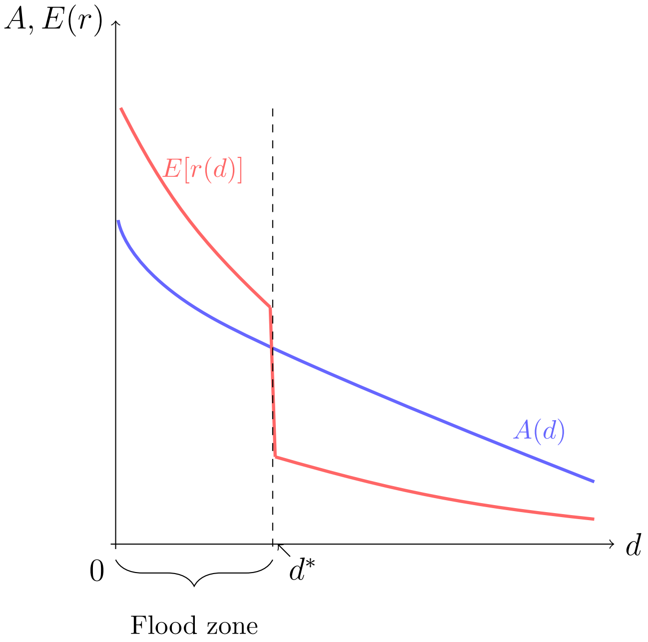
Nonetheless, while this model could explain variations in the quantity of new development near flood zones – which is an extensive measure of insurance against future inundations, it is less clear that it can simultaneously describe the location of new development with respect to flood zones – which is an intensive measure of insurance against future inundations. Following a flood, urban development decreases even in the areas far from the closest flood zones. If individuals update their risk beliefs based on flood history, we could expect them to insure themselves by locating away from the flood zone. Why do individuals prefer not to build rather than simply building farther away from the risks?

Indeed, one could think that changes in the expected risks would also translate into changes in the location of new development. However, this proposition only holds if the impact of a disaster on risk expectations varies with the distance from the source of hazards. On the contrary, if changes in risk beliefs are orthogonal to the proximity of the danger, then experiencing a flood leads to an absolute decrease in new development in the entire municipalities, but not a relocation of new development.

To see that, assume that water amenities (or economic gains), $A(.)$, and expected flood risks, $E[r(.)]$, are both a decreasing function of distance to water bodies d. The expected risk function is discontinuous at the flood zone border. In this simplified framework, a risk-averse individual will decide to convert land at a distance d if and only if $A(d) > E[r(d)]$. Because of the discontinuity in expected risks generated by the floodplain, she will maximize his expected amenity gains by building as close as possible from the floodplain's external border. The distance value that maximizes the expected net amenity gains is noted d^* in Figure 4.15.

In a Bayesian learning framework, this individual will update his beliefs about floods' occurrence when experiencing such a disaster. We write the updated risk beliefs $E_f[r(d)]$. Note that $E_f[r(d)] - E[r(d)] > 0$ for any distance d. That is to say, for any distance, d, the expected risks are higher after a flood. The ensuing question is whether the updated risks vary with distance d. First, consider the case when risk beliefs are updated independently from the distance to the source of the hazard (Figure 4.16a). In this case, $E_f[r(d)] - E[r(d)] = c \in R^+$ for any distance d, and d^* remains the same than in Figure 4.15. Agents do not have any incentive to build farther away from the source of the risks because the change in expected risk is the same at any point in space. Note, however, that since $E_f[r(d)] > E[r(d)]$, the expected amenity gains at d^* are now smaller than

Figure 4.15: Risk beliefs *before* a flood



Notes: The horizontal axis represents the distance of development to water. This figure depicts the trade-off between benefits from water accessibility, $A(d)$, and expected risks of floods, $E[r(d)]$. These latter drop at the flood zone border, fostering the concentration of development at distance d^*.

in Figure 4.15.

Now consider the case where risks beliefs are updated with respect to the distance: $E_f[r(d)] - E[r(d)] = g(d)$ (Figure 4.16b). In this case, d^*, the distance that maximizes expected amenity gains is now larger than in the previous examples. Because the change in expected risk is now greater for spaces close to the floodplain border, it is in the agents' best interest to build farther away from the source of hazards. Note that the expected amenity gains, in this case, are also smaller than in Figure 4.15.

Consequently, if prospective developers update their beliefs about flood risks irrespective of the distance to water, then we should observe lower development rates and no transfer of new development. This possibility has particularly strong policy consequences, as it implies that individuals apply a similar increased probability of being flooded to a large area. An *availability bias*[12] could cause a misinterpretation of flooding probabilities: 'because the disaster happened in a municipality, it must be that the

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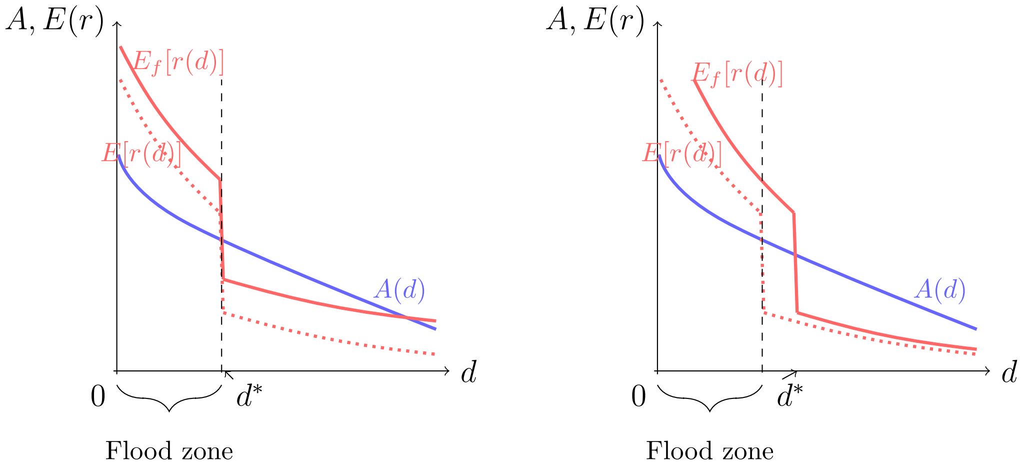
[12] Tversky and Kahneman (1974) describe this bias as "Situations in which people assess the frequency of a class or the probability of an event by the ease with which instances or occurrences can be brought to mind."

Figure 4.16: Risk beliefs *after* a flood



(a) Orthogonal beliefs **(b)** Dependent beliefs

chances that it happens again anywhere in this same municipality are similar.' This form of myopia has been extensively described by Tversky and Kahneman (1974). In particular, "Availability provides a natural account for the illusory-correlation effect. The judgment of how frequently two events co-occur could be based on the strength of the associative bond between them. When the association is strong, one is likely to conclude that the events have been frequently paired.". In this particular case, spatial proximity leads to the wrong interpretation of risk probabilities.

Alternatively, it could be that developers are averse to the expected amenity losses (Kőszegi and Rabin, 2006, 2007), and the decision to build farther from the floodplain is actually more costly than not developing at all. In this case, a building at the floodplain frontier is considered as the typical outcome of reference, and contemplating the idea of converting land on higher ground causes a downfall in expected utility that prevents any new development decision.

4.7 Conclusion

Using a rich dataset on historical flood records and the universe of buildings in Spain, we document the patterns of land development in the aftermath of an inundation. First, we show that development tends to historically cluster right outside identified flood zones, except in the absence of a water body nearby. We infer that individuals might want to maximize their access to water – either for economic, or amenity reasons, while remaining outside the hazardous area.

Using a flexible event-study framework, we find that after a flood, on average, new development decreases significantly and permanently relative to the year preceding the flood event. This result is driven by municipalities having suffered at least another flood episode in the previous years, and the period following the implementation of the 'Law of Waters' – a reform regulating land development around flood zones. Interestingly, we find that new development does not take place farther away from the flood zones or on higher ground after an inundation.

Several explanations could be consistent with this puzzle. In particular, we speculate that following a flood, individuals might update their belief regarding the overall probabilities of return, but misunderstand its spatial distribution. An availability bias could explain such behavior. Further research is needed to pinpoint the exact mechanisms behind these findings. In particular, we wish to disentangle the economic reasons from the expectations causing the downfall in development. In the near future, we plan to study how a flood occurring in the same river, amongst neighboring jurisdictions, or in the same media market, affect local development at the municipal level and along flood zones.

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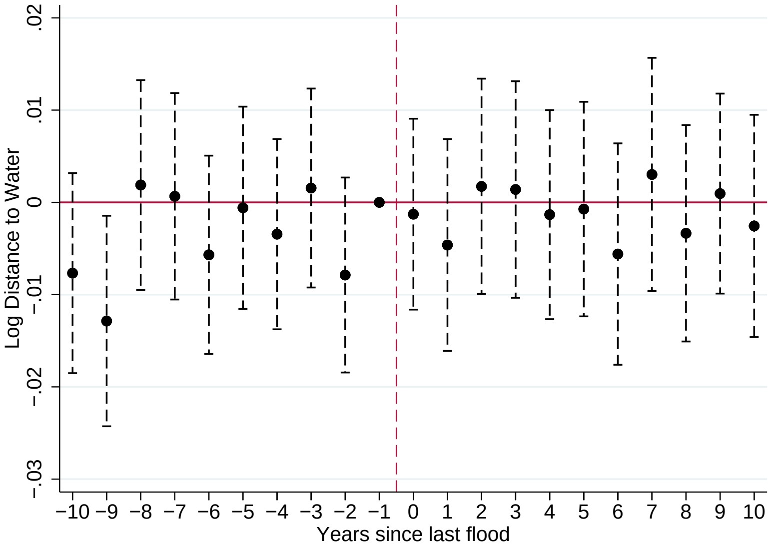
Appendices

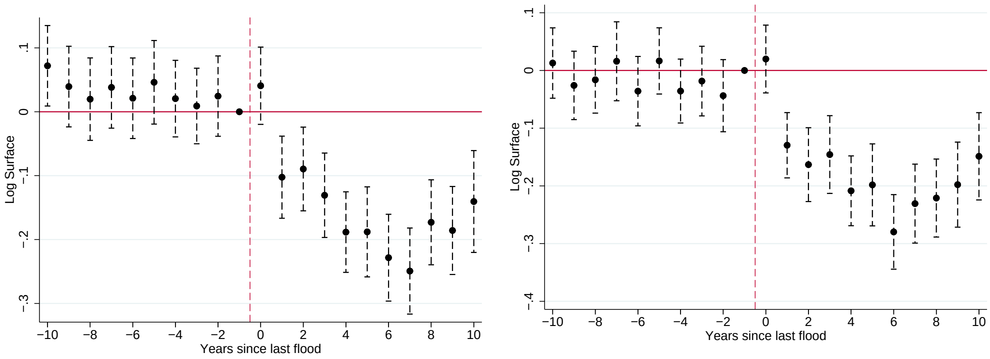
A Figures

Figure A.1: Effect of a flood event on new development' distance to the nearest water body



Notes: Effect of a Flood event on the log distance to the nearest water body. Results are based on Equation 4.1.

Figure A.2: Effect of a flood event on new development, according to different distances from the nearest water body.

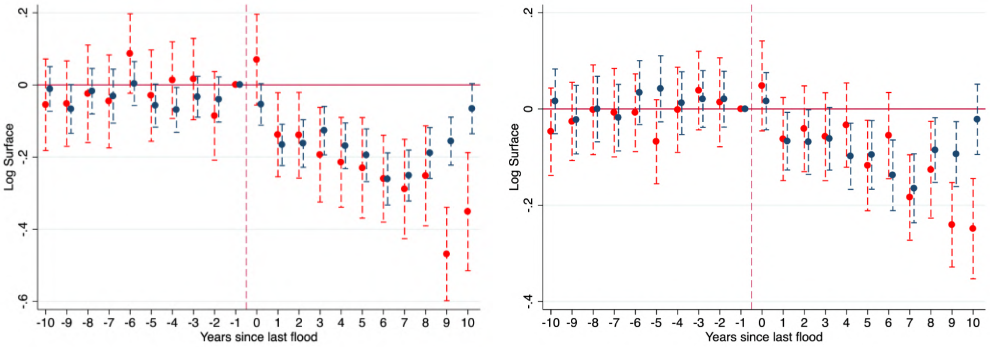


(a) Less than 100m of a water body

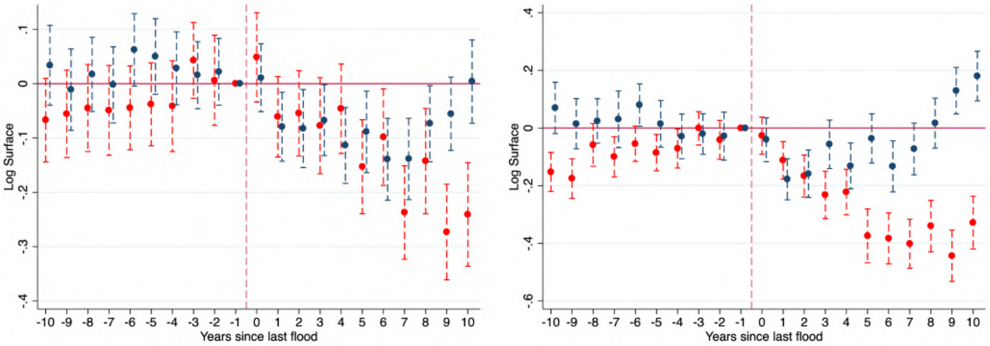
(b) Between 100 and 250m of a water body

Notes: Effect of a flood event on log surface built according to different fringes from the nearest a water body. Results are based on Equation 4.1.

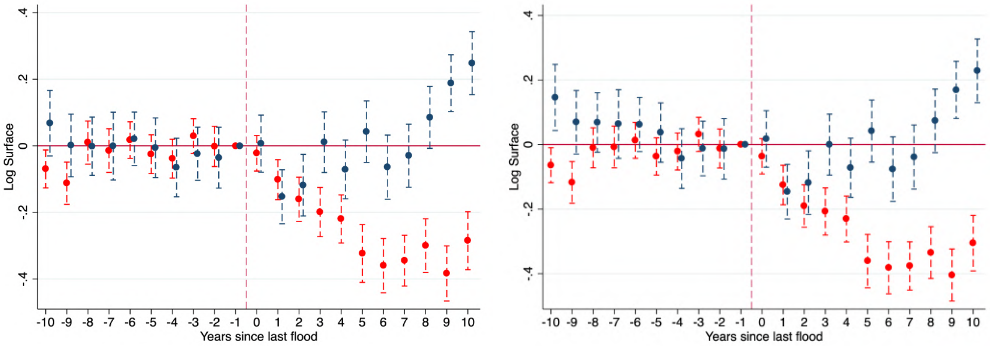
Figure A.3: Heterogenous analysis according to past flood events.



(a) 1-year period **(b)** 3-year period



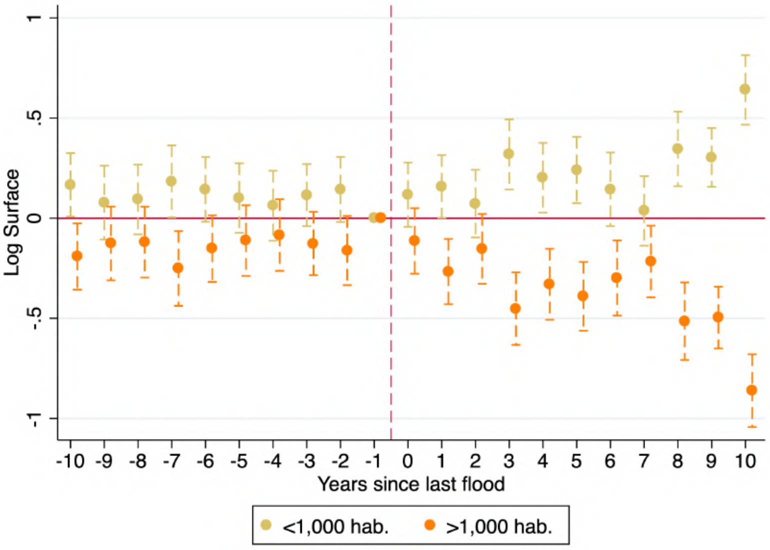
(c) 5-year period **(d)** 10-year period



(e) 20-year period **(f)** 30-year period

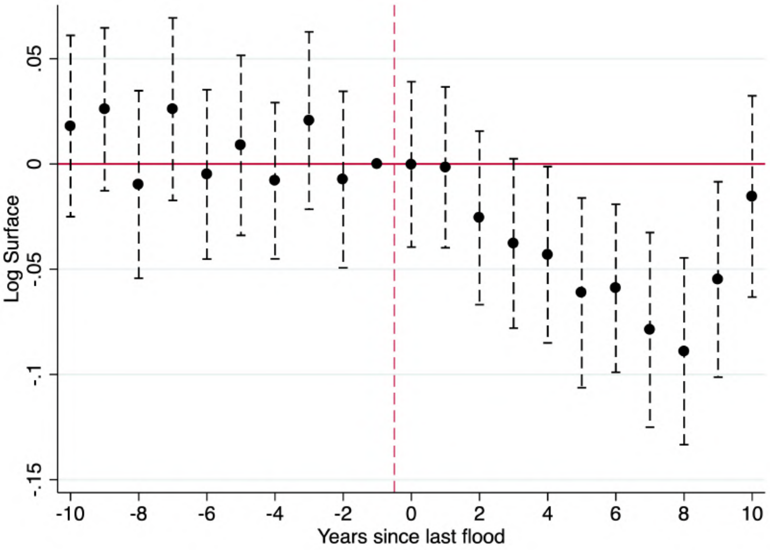
Notes: Effect of a flood event on the log of average surface built according to saliency history. Red bars represent estimates when at least one flood occurred in the previous x-year period. Blue bars represent estimates when no flood occurred in the previous x-year period. Results are based on Equation 4.2.

Figure A.4: Heterogenous analysis by population size



Notes: Effect of a flood event on the log of average surface built according to population size. Yellow bars represent estimates when a municipal population never got larger than 1,000 individuals (approximately 30% of all municipalities in the sample). Orange bars represent estimates when a municipal population got larger than 1,000 individuals at least once. Results are based on Equation 4.2.

Figure A.5: Effect of a flood event on new development inside a 500-year Floodplain



Notes: Effect of a Flood event on log of the average surface built inside a 500-year floodplain. Results are based on Equation 4.1.

Chapter 5

Conclusions

The world has been urbanizing at an incredible pace during the last century. Meanwhile, the global rise in temperatures has led to the increased probabilities of gradual and sudden natural disasters, putting large shares of developed lands at risk. While the benefits from agglomeration economies are well documented, less is known on how local stakeholders make land-use decisions in the context of climate change. Understanding how economic agents in charge of land conversion cope with climate threats while trying to preserve urban opportunities is a paramount challenge for the next decades. This dissertation aimed to shed some light on a few of the mechanisms at play, looking at spaces threatened by diverse environmental catastrophes.

In this regard, the second chapter of this thesis, *'The Political Economy of Coastal Destruction*, studies the impact of political cooperation on coastal development choices, made in Spain between 1979 and 2015. We argue that political cooperation between municipal neighbors is fostered by local political alignment. We rely on a fuzzy regression discontinuity design in close elections to assess the impact of political homophily on coastal development. We show that coastal municipalities who decide on coastal development in isolation may overdevelop as they fail to internalize the positive amenity spillovers caused by land preservation. Within the first-kilometer fringe, local governments sharing their neighbors' ideology develop 63% less than otherwise similar but politically isolated governments. This effect vanishes as we consider farther distances from the coastline, suggesting that amenity spillovers are an essential driver of this result.

While overdevelopment induces higher exposure to hazards when locating in disaster-prone areas, appropriate preparation can mitigate the chances of suffering from a natural catastrophe. However, mitigation measures do not only reduce but also signal the inherent risks of a location. I focus on the trade-off between risk reduction and risk disclosure in the third chapter of my thesis, *'Does media coverage affect government preparation for natural disasters?'*. I demonstrate that in the absence of information circulating about local dangers, local governments, who seek to protect property values in their jurisdiction, have an incentive not to prepare to avoid signaling the latent risks to otherwise uninformed investors. To test this hypothesis, I construct an exogenous measure of newspaper coverage of storms, which is a good predictor of the number of newspaper articles published about these events. I show that conditional on being hit by a storm, a one-standard-deviation increase in my Coverage measure leads to a 54% increase in the number of mitigation projects implemented in a ZIP code. This result is primarily driven by neighborhoods with high pre-treatment levels of vacant houses, renters, and housing-units owned with a mortgage, suggesting that non-resident investors are the firsts to respond to the information shock.

Considering that real estate interests could capture governments' preparation incentives, I questioned whether individuals learn from past disasters when making a development decision. In the last paper of this thesis, *'The Dynamics of Land Development around Flood Zones'*, we study the land conversion response to an inundation. Exploiting a rich dataset on historical flood records in Spain, we show that new development drops at the municipal level by -14.64% in the year following an inundation, and peaks down at -26% in the sixth year. The decrease in land conversion is, on average, permanent. This outcome is primarily driven by municipalities with higher historical flood frequencies, and by floods occurring after the central government regulated constructions around flood zones, in 1986. New development neither occurs farther away from flood zones nor on the higher ground. These results could be consistent with several underlying mechanisms. In particular, if individuals do account for disaster history when making a development decision, it is puzzling to observe they prefer not to build rather than building away from the acknowledged source of dangers. We speculate that a misinterpretation of the risks caused by an availability bias, or an aversion to amenity losses, could explain this response.

We can draw three lessons from these essays. First, environmental amenities may drive overdevelopment if local governments in charge of land-use decisions fail to cooperate with their counterparts. Overdevelopment in regions displaying both environmental amenities and environmental stress can increase exposure to natural disasters. Second, even in exposed areas, local governments may not prepare their jurisdiction for natural hazards if prospective residents are unaware of the dangers. The decline of local news, combined with the increased probabilities of natural disasters, could potentially cause dramatic inequalities in communities' capacity of resilience. Finally, individuals seem to convert less land when floods repeatedly hit a municipality in the past, but irrespective of the distance to dangers. This latter result could be yet another indication of individuals' difficulties in interpreting correctly local risk probabilities.

The climate adaptation challenge requires both economists and decision-makers to think twice about land conversion. Do all regulators have incentives to prevent individuals from building in hazardous areas? Do agglomeration benefits compensate for disaster exposure? How do we think of social justice when adapting to climate change? If individuals can learn from increasingly frequent disasters, can they adapt to increasingly violent ones? Of course, more research is still needed to understand how development decisions interact with past or prospective natural disasters.

Nevertheless, the few results presented in this dissertation suggest that it is urgent to internalize the consequences of climate change in urban policy design by fostering risks transparency and inter-jurisdictional cooperation in areas subject to both environmental opportunities and stress. Privileging benefices from agglomeration over short-term exposure could potentially lead to dramatic welfare consequences. Rather than merely limiting urbanization at potentially high costs too, transparent and collaborative urban decision-making must be favored for the future of cities, as our modern societies learn to cope with climate change.

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