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Learning to prime climate risk: Salience and flood-risk capitalization in Spanish housing Markets after the 2024 flash flood of Valencia

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Learning to Price Climate Risk: Salience and Flood-Risk Capitalization in Spanish Housing Markets after the 2024 flash flood of Valencia*

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Abstract

Do housing markets price flood risk only when a disaster makes it salient? I combine 3.4 million notarial housing transactions in Spain (2007–2025) with official flood-hazard maps and use the October 2024 Valencia DANA, which killed 232 people, as a nationwide shock to the salience of long-available risk information. After the DANA, prices per square meter in 10-year flood zones fell by about 8 % relative to unexposed dwellings, even outside the affected municipalities; repeat-sales estimates imply a decrease of 4-5 %. Before the event, these zones sold at no discount. Within the affected municipalities, demand shifted toward nearby safe housing, increasing prices in those areas and decreasing housing prices in affected areas.

JEL classification: Q54, R31, Q51, R21.

Keywords: flood risk; climate change; housing prices; capitalization; risk salience; flash flood; Valencia; natural disasters.

1 Introduction

Flooding is the most widespread natural hazard on the planet, and its economic footprint is growing. Recent global assessments estimate that about 1.81 billion people—roughly 23 percent of the world population—are directly exposed to inundation depths above 0.15 meters in a 1-in-100-year flood event, with the vast majority living in low- and middle-income countries (Rentschler, Salhab, and Jafino 2022). Some US$9.8 trillion of economic activity is located inside these high-hazard zones (Rentschler, Salhab, and Jafino 2022), and the Intergovernmental Panel on Climate Change concludes that direct flood damages will rise disproportionately with warming, roughly doubling at 2°C and quadrupling at 3°C of global warming relative to 1.5°C (Intergovernmental Panel on Climate Change 2023). The financial materialization of this trend

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is already visible: the reinsurer Munich Re estimates global natural-catastrophe losses of US$320 billion in 2024, one of the costliest years on record, and attributes a growing share of losses to "non-peak" perils such as floods, severe thunderstorms , and wildfires, which are becoming more frequent and more extreme as the planet warms (Munich Re 2025).

These aggregate trends raise a first-order question for economics: how, and how well, do asset markets price physical climate risk? Housing is the natural laboratory for the question. Residential real estate is the single largest component of household wealth in most countries, it is immobile and long-lived, and its value should in principle embed the present discounted cost of expected future damages, insurance premiums, and the disamenity of living with risk. In practice, however, the capitalization of flood risk is often incomplete, unstable, and highly heterogeneous across places, buyers, institutional settings, and moments in time. A large literature, reviewed below, finds that flood risk is indeed negatively capitalized into house prices, but that the magnitude is frequently smaller than a fully informed, rational market would imply (Anshuka, Fu, and White 2025; Gourevitch et al. 2023; Hino and Burke 2021). This gap between objective risk and market prices is central for economic policy: if flood risk is under-priced, households over-invest in exposed locations, lenders and insurers accumulate hidden climate risk, and public authorities face larger future fiscal costs from disaster relief and reconstruction.

This paper studies how housing markets learn to price flood risk after a highly salient climate-related disaster. The empirical setting is Spain, where detailed transaction-level data can be linked to property characteristics and official flood-risk maps that classify buildings according to different probabilities (10-, 100- and 500-year return periods) of a large flood. The event that motivates the analysis is the destructive *DANA* that struck the province of Valencia on 29 October 2024. The DANA was not merely a localized physical shock; it was also a national information and salience shock that made flood risk highly visible to buyers, sellers, lenders, insurers, and public authorities. The central empirical question is whether this shock changed the implicit price of objective flood probability in the Spanish housing market.

The paper is designed to move beyond the standard question of whether flooded homes decline in value after a disaster. A price decline for physically damaged properties is economically important but not conceptually surprising. The more general question is whether the DANA caused markets to reprice the probability distribution of future flood risk, including among properties that were *not* directly damaged but share similar objective exposure. This distinction matters because the welfare-relevant market failure is not only the loss suffered by damaged homes. It is the possibility that, before a salient event occurs, households under-weight low-frequency risks and therefore fail to incorporate them into durable-asset prices.

The paper contributes to three literatures. The first is the literature on environmental risk and housing prices. Early hedonic studies generally find that floodplain location or proximity to flood hazards reduces property values, although estimated discounts vary widely across settings (MacDonald et al. 1990; Holway and Burby 1990; Bin and Polasky 2004a; Bin and Kruse 2006; Bin et al. 2008; Beltrán, Maddison, and Elliott 2018). More recent work emphasizes the limitations of cross-sectional estimates and the need for microdata, repeat-sales designs, boundary comparisons, and quasi-experimental variation in risk information (Bin and Landry 2013; Hino and Burke 2021; Skouralis, Lux, and Andrew 2024). The Spanish data allow a

national micro-level analysis in a setting where flood risk is measured probabilistically, across multiple return-period categories, rather than only through a binary floodplain indicator.

The second contribution is to the literature on information, salience, and belief updating in housing markets. Flood maps, climate projections, and technical risk classifications may be publicly available, but they need not be decision-relevant for households unless they are salient and credible. Hino and Burke (2021) show that information contained in US regulatory flood maps is not fully capitalized into property values, but that discounts are larger where disclosure rules are stricter and where buyers are more sophisticated. In Spain, Chakraborti, Gete, and Martínez-Toledano (2022) show that news about sea-level rise reduced housing prices in La Manga, with no corresponding effect on rents, suggesting that long-horizon climate news can affect asset values even when current housing services are unchanged. Lamas Rodríguez et al. (2023) show that the materialization of ecological deterioration in Mar Menor generated large real-estate losses once the risk became publicly visible. This paper brings these insights to the case of flood probabilities and asks whether a national disaster changes the pricing of objective risk even outside the directly affected area.

The third contribution is to the literature on climate risk, insurance, and public policy. In standard asset-pricing logic, the capitalization of flood risk should reflect the present value of expected damages, insurance costs, adaptation costs, and liquidity effects. Yet institutional arrangements can distort these signals. Subsidized insurance, mandatory coverage, public compensation, and *ex post* disaster relief may all weaken incentives to avoid risky locations or to invest in adaptation (Kousky 2010; Gruber and Solomon 2026). Spain is particularly interesting because the Consorcio system provides broad public catastrophe coverage, which may reduce the salience of private insurance costs while increasing the importance of non-insured and behavioral channels. The paper therefore asks whether flood-risk capitalization is primarily an expected-loss phenomenon, an information phenomenon, or an institutional phenomenon. Relative to existing Spanish evidence, which relies largely on regional case studies and news-driven salience shocks (Chakraborti, Gete, and Martínez-Toledano 2022; Menéndez et al. 2024; Lamas Rodríguez et al. 2023), the DANA offers a physically severe, spatially precise and climate-attributable event, together with the microdata infrastructure needed to estimate property-level effects and their heterogeneity across primary versus secondary homes, income groups, foreign buyers, and insurance and planning regimes.

The rest of the paper is organized as follows. Section 2 reviews the related literature on flood-risk capitalization, information and salience, disaster memory, and the emerging evidence for Spain. Section 3 describes the institutional setting and the nature of the DANA episode. Section 4 describes the transaction data, the flood-risk measures, and the construction of the DANA exposure and salience variables. Section 5 presents the empirical strategy. Section 6 reports the baseline capitalization results and the post-DANA event-study estimates. Section 7 studies heterogeneous effects and mechanisms. Section 8 analyzes the results of using a panel of repeat sales. Section 9 concludes.

2 Literature Review

This paper connects to four literatures, organized around the substantive questions they address: (i) the capitalization of flood risk into property prices; (ii) the role of information, disclosure , and salience in how markets learn about risk; (iii) disasters, historical memory, and belief updating, including the under-pricing of long-horizon climate risk and its aggregate consequences; and (iv) insurance, public compensation, and the Spanish evidence and institutional context.

2.1 Flood risk and the capitalization of environmental hazards

The hedonic theory of housing markets implies that property prices reflect the bundle of structural characteristics, neighborhood amenities, local public goods, and environmental risks associated with a dwelling. Flood risk should therefore be capitalized into prices to the extent that buyers understand the risk, expect to bear its costs, and are able to arbitrage across otherwise comparable locations. Early empirical work supports the existence of flood-risk discounts but documents substantial variation in magnitudes. MacDonald et al. (1990) and Holway and Burby (1990) provide early evidence that floodplain controls and hazard exposure are reflected in residential land and property values. Subsequent studies show that flood hazards reduce prices, but the discount can be offset by water-related amenities such as views, access to beaches, and recreation (Bin and Kruse 2006; Bin et al. 2008). Repeat-sales and event-study designs consistently find that severe floods induce sharp price reductions in affected areas relative to comparable non-flooded locations, with only partial recovery over time (Beltrán, Maddison, and Elliott 2018; Atreya and Ferreira 2015; Nguyen et al. 2022).

A simple comparison between homes inside and outside a floodplain may confound risk with proximity to water, coastal access, topography, tourism demand, and local income. This problem is particularly severe in Mediterranean housing markets, where the same locations that face flood risk may also command large amenity premia. The literature has therefore moved from simple cross-sectional hedonic models toward repeat-sales models, difference-in-differences designs, boundary comparisons, and continuous or property-level risk measures. Beltrán, Maddison, and Elliott (2018), using a repeat-sales approach, show that flooding has significant effects on property prices and that recovery dynamics vary over time. Bin and Landry (2013) show that implicit flood-risk premia change after major storms, consistent with learning and belief updating. This concern is reinforced by evidence that households sort systematically over flood risk itself: Bakkensen and Ma (2020) show that observed and unobserved risk perceptions shape residential sorting into flood-prone areas, implying that naive hedonic comparisons across risk categories can conflate price effects of risk with selection on risk tolerance and information.

Recent systematic evidence confirms both the direction and the heterogeneity of these effects. A global review of 78 empirical studies finds that the largest discounts follow direct hazard *experience* (exceeding 20 percent in some settings), followed by zoning designations (roughly 5–15 percent) and insurance-premium signals (around 5 percent), while protective infrastructure raises prices and long-horizon sea-level-rise signals are frequently left un-capitalized (Anshuka, Fu, and White 2025). Recent work also emphasizes that flood risk should not be treated as a binary attribute. A floodplain dummy captures one regulatory threshold, but actual flood

risk varies with elevation, depth, hydrology, flood source, building type, construction quality, and future climate conditions. Using 4.8 million UK listings matched to a property-level flood score, Skouralis, Lux, and Andrew (2024) estimate an average discount of about 8 percent, rising above 30 percent for the highest-risk properties, with larger proportional discounts for lower-priced homes and for areas that recently experienced floods. Cross-country estimates for the UK and continental Europe likewise find that properties in high-risk zones sell at discounts of a few percent, rising to double digits in very high-risk or repeatedly flooded locations (Beltrán, Maddison, and Elliott 2018; Békés, Horváth, and Sápi 2016). Beyond level effects, flood-prone properties tend to appreciate more slowly and recover less fully after events, implying long-run divergence in wealth accumulation between exposed and non-exposed households (Nguyen et al. 2022; Beltrán, Maddison, and Elliott 2018). This is important for the Spanish case, where the data contain multiple risk-probability categories: the relevant empirical object is therefore not only the average discount for being in a floodplain, but the full price gradient with respect to flood probability.

2.2 Information, disclosure, and salience

One of the central findings of the recent literature is that the availability of flood-risk information does not guarantee full capitalization. Technical information may be public but difficult to interpret, disclosed late in the purchase process, or ignored by buyers focused on current affordability, amenities, or expected capital gains. Hino and Burke (2021) provide one of the clearest demonstrations. Using US flood-map updates and millions of home sales, they find that being mapped into a floodplain reduces values by less than the benchmark implied by full insurance-cost capitalization—about 2 percent on average, against a 5–11 percent benchmark. Their estimates are more negative in states with stricter disclosure requirements, in communities with greater flood exposure, and for business buyers, suggesting that information and sophistication affect the degree of pricing.

This evidence links flood-risk capitalization to broader models of limited attention and salience. Households may not continuously update their beliefs about low-probability events. Instead, beliefs may adjust after a salient event, a news shock, a map revision, or a regulatory disclosure (Pryce, Chen, and Galster 2011; Kousky 2010). In the housing market, salience is especially important because homes are durable assets. A buyer may discount future flood losses not only because of expected physical damage but also because of expected resale risk: the possibility that future buyers, lenders, or regulators will perceive the property as riskier than they do today. Realized events make risk salient and trigger belief updating: the capitalization of flood hazard strengthens after severe storms (Bin and Polasky 2004a; Bin and Landry 2013), and price discounts can fade as the memory of an event recedes (Atreya, Ferreira, and Kriesel 2013). The importance of experience over mere designation is sharpened by studies that find little or no discount for zone designation once past flooding is controlled for but sizeable discounts in locations that have actually flooded (Gharbia, Lyons, and Pilla 2018).

Policy-induced changes in risk classification provide some of the cleanest identification. Flood-map revisions and insurance reforms generate discrete price changes when properties are reclassified into or out of high-risk zones, and being newly mapped into a floodplain can reduce

values by more than the net present value of the associated insurance premium—implying that official designation carries a *signal* component over and above the mechanical cost channel (Bin and Landry 2013; Shr and Zipp 2019; Hino and Burke 2021). Beliefs also interact with who is doing the pricing: capitalization of sea-level rise and climate risk depends on buyers' priors and even their political orientation (Baldauf, Garlappi, and Yannelis 2020; Bernstein et al. 2022), and on whether local disclosure laws force information into the transaction (Hino and Burke 2021; Gourevitch et al. 2023).

A related literature shows that the salience and accuracy of flood-risk information depend on how it is produced and communicated by public agencies, not only on how households process it. Pralle (2019) documents that FEMA's flood-zone boundaries are shaped by political and administrative pressures as well as hydrological science, so that official maps may misrepresent risk in ways that are not random across communities. Gibson and Mullins (2020) show that households in New York floodplains update their beliefs about flood risk in response to map-based information, but that these updates are incomplete and heterogeneous across neighborhoods. Gourevitch et al. (2023) combine nationwide flood-map data with an independent risk model to show that unpriced flood risk is pervasive in US housing markets and that failing to update maps can generate systematic overvaluation—US flood-exposed homes are collectively overvalued by US$121–237 billion—with the gap concentrated in places lacking disclosure laws and falling disproportionately on low-income households and tax-reliant municipalities. Weill (2026) analyzes the effect of flood information on the distributional impact of residential flood-insurance take-up and concludes that outdated maps distorted risk and price signals, but that map correction improves welfare. Billings, Calder-Wang, and Liu (2026) use a difference-in-RD design around the ten cut-offs of First Street's Flood Factor to estimate the price effect of a large disclosure of property-level flood risk: the disclosure reduces sale prices for homes just above the cut-off by about 3.3 percent relative to properties just below, and increases time on market by roughly seven days.

Because floodplain maps also trigger regulatory and credit-market consequences, their effects on prices operate through channels beyond direct risk beliefs. Sastry (2026) shows that flood-map updates lead mortgage lenders to ration credit and reallocate lending toward wealthier borrowers in Florida, indicating that map-driven changes in financing conditions can affect who is able to buy or hold property in remapped areas. Ostriker and Russo (2024) shows that floodplain construction regulations triggered by map boundaries affect housing supply and choice, generating adaptation benefits that are capitalized independently of the risk information itself. These findings suggest that, in settings with strong map-contingent regulation or underwriting rules, part of any price response to updated risk information may reflect regulatory and credit constraints rather than a pure updating of beliefs about expected losses—a distinction directly relevant for the Spanish setting, where risk categories are used by the Consorcio de Compensación de Seguros but are not directly linked to a mandatory-purchase mortgage rule as in the US National Flood Insurance Program.

The DANA provides a different type of salience shock. Unlike a report or a gradual ecological crisis, it was a sudden disaster with large media coverage and immediate social consequences. It therefore allows a test of whether disaster salience changes the price of objective flood risk, even

in places that were not directly damaged—the key distinction between physical-damage effects and information effects.

2.3 Disasters, historical memory, and belief updating

A large literature studies how housing markets respond to realized disasters. Floods, hurricanes, and severe storms can reduce prices through physical damage, perceived future risk, reduced credit availability, and neighborhood decline. The persistence of these effects varies. Some studies find temporary discounts that decay as memory fades, while others find persistent effects in areas with repeated events or where disasters reveal previously underestimated risk (Kousky 2010; Bin and Landry 2013; Beltrán, Maddison, and Elliott 2018; Hennighausen and Suter 2020).

The distinction between one-shot salience and historical memory is important. A single flood can generate immediate attention, but repeated floods may create durable local knowledge. In frequently flooded areas, new events induce little additional repricing because baseline prices already embed the risk—evidence of stable, accurate risk awareness (Inoue and Komori 2017). Bellaver et al. (2025) provide complementary evidence for Italy using mortgage-financed transactions and flood-risk maps. They find that flood-risk discounts are concentrated in regions with frequent historical flooding, and generate persistent baseline discounts of up to 4 percent that are larger for older and higher-income buyers, while a single recent flood does not necessarily reduce prices for risky but undamaged properties. Their interpretation is that households rely on *historical memory* when formal disclosure and insurance signals are weak. This insight is highly relevant for Spain, where local flood histories differ sharply across basins and municipalities.

Belief updating about flood risk need not be uniform across households or persistent over time. Bakkensen and Barrage (2022) show that heterogeneity in flood-risk beliefs among coastal home buyers can generate slow-moving, boom-and-bust price dynamics as pessimistic and optimistic buyers sort differently into risk exposure, and that this heterogeneity helps explain why coastal flood risk appears to be incompletely and unevenly capitalized even decades after major storms. Using survey evidence, Conell-Price and Mulder (2024) find that homeowners update their beliefs asymmetrically in response to new climate-risk information, revising expectations upward after unfavorable news but only partially correcting overly pessimistic beliefs after favorable news. This asymmetry suggests that a salient disaster such as the DANA may generate discounts that are more persistent, or slower to reverse, than the arrival of comparable positive information would predict.

Despite robust evidence of discounts, a growing body of work argues that flood and climate risk remain *under*-capitalized in many markets. Hino and Burke (2021) estimate a floodplain discount of only about 2 percent using nationwide US transaction data and conclude that floodplain properties are collectively overvalued by tens of billions of dollars. Related work on sea-level rise finds that coastal values do not fully reflect projected inundation (Bernstein, Gustafson, and Lewis 2019; Murfin and Spiegel 2020; Ortega and Taşpınar 2018), echoing broader evidence that gradual, long-horizon risks are discounted too heavily (Mendelsohn, Nordhaus, and Shaw 1994). The persistence of under-capitalization is typically attributed to behavioral frictions (myopia, optimism, limited attention), institutional features (subsidized insurance, disaster relief), and incomplete or poorly understood risk information (Kousky 2010; Beltrán,

Maddison, and Elliott 2018). The consequences of flood risk extend beyond prices and beyond housing: natural disasters reduce homeownership by updating risk beliefs among in-migrants (Sheldon and Zhan 2019), and *expected* (as opposed to realized) flood risk depresses firm entry, employment and local output over the long run, with measurable aggregate effects (Jia, Ma, and Xie 2026). This last strand motivates attention to the broader economic footprint of the DANA beyond the residential market.

The Spanish DANA setting allows a direct test of these mechanisms. If markets already price risk in historically flooded municipalities, the post-DANA repricing should be small in those locations. Conversely, if objective flood risk was previously ignored in places without recent flood experience, the DANA should produce a larger increase in the risk discount. This hypothesis connects behavioral learning to spatial heterogeneity in capitalization.

2.4 Insurance, public compensation, and moral hazard

Insurance affects housing prices because it changes who bears the cost of flood damage and how risk is communicated to buyers. In the United States, the National Flood Insurance Program links floodplain maps, mortgage requirements, and insurance premiums, creating a direct price signal—although subsidies and incomplete take-up weaken the link between objective risk and private costs. In the United Kingdom, flood insurance has historically been shaped by a mix of private provision and public-private arrangements, including Flood Re. Skouralis, Lux, and Andrew (2024) shows that flood risk is capitalized in England even in a setting with a reinsurance arrangement that supports insurance availability.

A growing body of work examines how insurance markets themselves respond to and shape flood-risk information. Wagner (2022) provides the first comprehensive welfare analysis of the US flood insurance market and shows that demand frictions —including under-estimation of risk and liquidity constraints —justify mandatory-purchase requirements on efficiency grounds. Bradt, Kousky, and Wing (2021) shows that voluntary flood-insurance purchases outside mandated zones are subject to adverse selection, with take-up concentrated among the highest-risk uninsured properties, which limits the risk-pooling benefits of voluntary markets. Kousky (2018) reviews the financing structure of the National Flood Insurance Program and highlights that chronic under-pricing and debt accumulation constrain its capacity to signal risk through premiums, while Kousky (2019) shows more broadly that insurance affects not only *ex ante* risk-taking but also the speed and completeness of post-disaster recovery. Take-up responds sluggishly and myopically to experience (Gallagher 2014), and underinsurance is pervasive and concentrated among lower-income households (Qin and Voorheis 2026).

Insurance also raises public-economics questions. Subsidized insurance can protect households and reduce uncompensated losses, but it may encourage development in risky areas or reduce incentives to adapt. *Ex post* disaster relief can create a Samaritan's dilemma: if households expect public compensation after a disaster, they may underinsure or underweight risk when choosing locations. **Kunreuther2019<empty citation>** emphasizes that expectations of disaster relief can reduce individual mitigation incentives. Gruber and Solomon (2026) develops a model of optimal flood insurance in a second-best world with fiscal spillovers, reclassification risk, and moral hazard, arguing that the welfare effects of subsidies depend on several offsetting

margins and that reforms toward actuarially fair pricing—such as the US NFIP's Risk Rating 2.0—generate fiscal spillovers onto *ex post* disaster aid that can justify substantial insurance subsidies on second-best grounds.

Spain differs from these settings because extraordinary flood losses are covered through a public catastrophe-insurance institution, the Consorcio de Compensación de Seguros, which has historically paid the bulk of flood-related compensation [1]. This may have ambiguous effects on capitalization. On the one hand, expected compensation can reduce the private cost of structural damage and therefore attenuate price discounts. On the other hand, even insured households face disruption, displacement, sentimental losses, delays, uncertainty, repair costs not fully covered, liquidity problems, and future resale risk. If the post-DANA discount exceeds the present value of expected uninsured physical losses, the evidence would suggest that market participants price broader risk and salience channels, not only actuarial expected damages.

3 Institutional Setting and the DANA Episode

Spain is among the European countries most exposed to hydrometeorological hazards, combining riverine, pluvial, and coastal flooding with decades of tourism-driven urbanization of floodplains and low-lying coastlines (Ribas, Olcina, and Saurí 2020). Physical evidence of intensifying risk on the Mediterranean coast is accumulating: studies of the eastern Iberian seaboard document rising flood damages under global warming and an increase in the frequency and intensity of short-duration precipitation extremes (Cortès et al. 2018; Fowler, Wasko, and Prein 2021). Public institutions have taken note. (Heymann 2024) treats climate change as a structural source of risk for Spanish real-estate assets, operating through both physical channels (heat, drought, flooding, sea-level rise) and transition channels, and warns that a non-negligible share of the housing stock sits in areas exposed to more frequent extreme events. Consistent with international findings, however, household-level evidence suggests that climate and flood risk are not yet central to most purchase decisions: although many Spaniards live in officially delineated flood-prone areas, only a small fraction of prospective buyers (12.1%) cite climate risk as a key factor, with price, neighborhood, and employment stability remaining dominant (Unión de Créditos Inmobiliarios 2026). This gap between objective exposure and subjective salience is precisely the condition under which risk can remain under-capitalized until a shock makes it concrete.

3.1 Spanish evidence and the remaining gap

Relative to the extensive Anglo-Saxon and northern European literature, rigorous empirical work on the capitalization of flood and climate risk in Spanish housing markets is limited but growing, and it is concentrated in coastal regions. A first line of research exploits salient news shocks. Chakraborti, Gete, and Martínez-Toledano (2022) study La Manga del Mar Menor, target of a widely publicized environmental report, and find via a difference-in-differences design that the news shock produced a significant and persistent decline in house prices while rents remained comparatively stable—interpreting the divergence as evidence that salient information

1. See (Espejo et al. 2021) for an analysis of flood loss data in Spain at the municipal level.

about long-run climate risk is capitalized into prices (which reflect expectations about future resale value) more than into short-horizon rents. The study also documents increases in demand and prices in nearby locations perceived as safer, indicating that climate-risk information can trigger spatial reallocation of housing demand along the coast.

A second line provides direct evidence on baseline flood-risk capitalization. Menéndez et al. (2024) combines *Idealista* listings with high-resolution hazard maps for the Basque Country and finds that risk is capitalized mainly in the highest-hazard (10-year return period) zones—discounts of roughly 4–10 percent—while lower-risk zones show no discount or even a premium, reflecting confounding coastal amenities. These results provide direct evidence that flood risk is negatively capitalized in at least one Spanish region and illustrate the feasibility of linking hazard maps and transaction data for quantitative assessment.

A third set of contributions studies the materialization of environmental and climate risk more broadly. Lamas Rodríguez et al. (2023) shows that once the ecological deterioration of the Mar Menor lagoon became widely recognized, housing returns in the affected area fell some 43 percent below a comparable control over six years, implying housing-wealth losses exceeding €4 billion, with the effect tightly linked to public perception measured through news and social-media sentiment.

Two institutional features distinguish the Spanish setting. First, the transposition of the EU Floods Directive has produced detailed hazard and risk maps and integrated them into regional and municipal planning, providing high-quality, property-linkable exposure measures. Second, the public catastrophe insurer Consorcio de Compensación de Seguros mutualizes extraordinary risks and has historically paid the bulk of flood-related compensation, which may dampen price discounts relative to private-insurance regimes.

Against this backdrop, three gaps stand out. Existing Spanish studies are largely confined to specific regional cases (La Manga, the Basque Country) or to broad climate proxies such as temperature; most exploit news shocks or ecological crises rather than the pricing of baseline hazards under normal conditions; and there is little evidence on the heterogeneity of the impact of flooding. This paper fills these gaps. It uses the DANA as a shock to attention, but the research design is Spain-wide and property-level. The contribution is not only to estimate a flood-risk discount. It is to estimate whether the discount changes after a salient event, whether the change is proportional to objective risk, and whether it differs from historical memory. By linking the climate risk capitalization literature with salience, memory, and institutions, the paper aims to provide evidence on when housing markets internalize physical climate risk and when they fail to do so.

3.2 Institutional framework

The policy environment is likewise in flux. Building on the 2015 regulation restricting construction in high-risk zones, the Spanish government has moved to sharply tighten land-use rules: a draft royal decree explicitly prohibits new residential uses and below-grade construction (basements, underground garages) in preferential-flow flood areas, requires that homes in lower-risk floodable land be built at least one meter above the design flood level, obliges owners to record flood exposure in the property registry at the point of sale, and mandates municipal flood-adaptation

plans; officials acknowledge that these measures "will affect the price" of affected parcels and dwellings (Planelles 2026). Spain also operates a distinctive institutional backstop—the public catastrophe insurer Consorcio de Compensación de Seguros—which mutualizes extraordinary risks and, in the flood context, may attenuate the price discount relative to countries where private agents bear more of the loss (Espejo et al. 2021). The coexistence of detailed hazard mapping under the EU Floods Directive, rich cadastral and registry microdata, dynamic coastal housing markets with large amenity premia, and strong foreign participation, as well as institutional risk-sharing, makes Spain an unusually informative setting for studying flood-risk capitalization.

On 29 October 2024, a DANA (*Depresión Aislada en Niveles Altos*, a cut-off low) produced record-breaking rainfall over the province of Valencia. Weather stations recorded more than a year's worth of precipitation in a matter of hours—491 mm in eight hours at Chiva and up to 771 mm at Turís—triggering catastrophic flash floods in the metropolitan area south of the city (World Meteorological Organization 2024). The disaster killed 232 people, making it the deadliest flood in modern Spanish and European history; Valencia alone accounted for roughly 70 percent of all flood-related deaths in Europe in 2024 (Planelles 2025). It affected around 303,000 people across some 75 to 78 municipalities, destroyed more than 100,000 vehicles, and damaged critical infrastructure; total losses are estimated at approximately €17 billion, of which the Consorcio de Compensación de Seguros received more than 116,000 claims (Pérez et al. 2025). Formal attribution studies conclude that anthropogenic warming intensified the event: human-induced climate change increased six-hour rainfall intensity by roughly 20 to 21 percent and expanded the area receiving more than 180 mm of rain by about 55 percent relative to a pre-industrial climate (Calvo-Sancho et al. 2026).

The DANA is well suited to serve as the identification device for this paper for three reasons. First, it was a large, sudden, and —at the household level—largely unanticipated shock, sharply increasing the salience of flood risk in and around the affected area while leaving comparable but unaffected Mediterranean locations as potential controls. Second, its spatial footprint is precisely delineated by hydrological and cadastral data, allowing treatment to be defined at the property level and mapped against pre-existing EU Floods Directive hazard layers, which distinguishes the pricing of *ex ante* baseline risk from the repricing induced by a realized *ex post* event. Third, its aftermath generated, following some journalistic accounts, market movements. In the "zona cero," the scarcity of habitable supply and the need to rehouse displaced families seem to have pushed prices up by about 18 % according to the regional real-estate association, even as roughly 15–20 percent of the local stock sat vacant (Picher 2026). In heavily hit municipalities such as Catarroja, rents rose by about 20 percent—seven points above Madrid—while unrehabilitated dwellings transacted at discounts of around 7 percent, and the regional government introduced targeted acquisition subsidies of up to 10 percent (Herrero Gutiérrez 2026). At the same time, market commentary points to a potential divestment of rental portfolios by large institutional owners in affected and risk-exposed areas. These contrasting movements—rising prices and rents for scarce habitable units alongside discounts for damaged or newly salient-risk properties—are exactly the kind of heterogeneity that a well-identified design can disentangle.

4 Data

We obtained the raw data for 3.9 million home transactions for the period from January 2007 to October 2025. The data come from the digital registry of the Technological Center of the Notaries of Spain. The information includes the exact date of the transactions, the type of building, the municipality where it is located, and the official sold price. It also includes an identifier for the unit, as well as the longitude and latitude of its location. The information does not include the size of the units.[2]

The units have also been matched with the floodplain maps of the Spanish Cartographic Institute. The flood-exposure measures used in this paper come from the *Sistema Nacional de Cartografía de Zonas Inundables* (SNCZI, National System of Floodplain Mapping), the official Spanish repository of flood hazard, flood risk, and public water-domain cartography maintained by the Ministry for the Ecological Transition and the river-basin authorities. The SNCZI is the Spanish institutional response to the European Floods Directive, which requires member states to assess flood risk, map flood hazard and flood risk, and adopt management plans in six-year cycles. The system was created by law in 2008 through a modification of the Regulation of the Public Water Domain (*Reglamento del Dominio Público Hidráulico*, RDPH; Royal Decree 849/1986), which implements the Spanish Water Act, and the Directive was transposed by Royal Decree 903/2010. The maps themselves are produced by combining hydrology, hydraulics, geomorphology, and the historical flood record. Design discharges for each probability scenario are estimated either statistically from gaging-station series or through rainfall–runoff models; these discharges are then routed over very high-resolution LiDAR digital terrain models from the national aerial orthophotography plan using one- and two-dimensional hydraulic models, which return the flooded extent, water depth, and flow velocity. Geomorphological and historical evidence (paleochannels, alluvial plains, past flood marks) is used to validate and correct the model output, and the resulting layers undergo public consultation and quality control before being published in the SNCZI viewer.

Following the Floods Directive and Royal Decree 903/2010, hazard is mapped for three probability scenarios, defined by the statistical return period T of the flood event (annual exceedance probability $1/T$):

1. **High probability** ($T = 10$ years; annual probability 10%). The most frequently inundated band of the floodplain, closest to the channel.
2. **Medium probability** ($T = 100$ years; annual probability 1%). The reference scenario for most regulatory purposes; the *zona de flujo preferente* (preferential flow zone, ZFP) is derived from the depth and velocity fields of this event.
3. **Low probability or extreme event** ($T = 500$ years; annual probability 0.2%). The legal definition of *zona inundable* in the broad sense under Article 14 of the RDPH, and the outer envelope of the mapped floodplain.

Figure 1 shows the official 2015 map of the locations with significant potential risk of flooding, as well as the rivers in Spain. The area affected by the DANA on the coast of Valencia contains large areas of high risk of flooding. Figure 2 includes the flooding areas of low risk (T=500).

2. When we use transactions with valid surface area information, the baseline estimation sample contains 3.4 million transactions

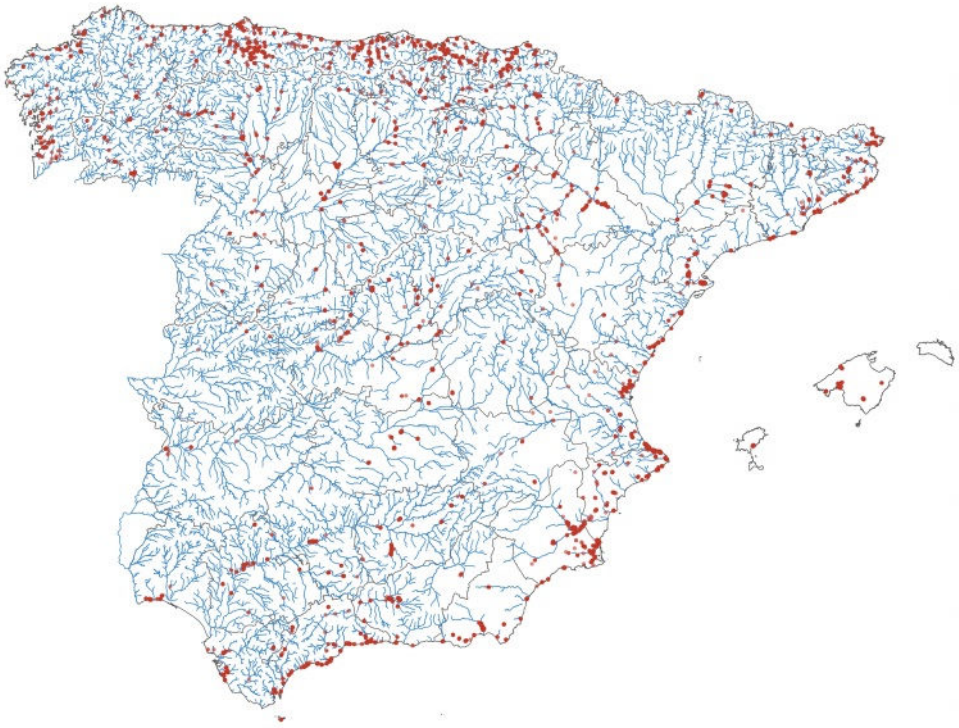
Figure 1: Official 2015 map of significant flooding risk areas



Figure 2: Map of flooding areas in T=500



Figure 3: Location of transactions of homes in T=10 areas



For each scenario, the system publishes a vector layer with the inundated extent and a raster layer with the expected water depth; risk maps combining hazard with exposed population, economic activity, cultural heritage, and environmentally sensitive sites are produced for the same three return periods. The zones are nested by construction, so that the $T = 10$ area is contained in the $T = 100$ area, which is contained in the $T = 500$ area.[3]

Figure 3 shows the approximate location of the transactions included in the dataset that correspond to areas of high risk of flooding (T=10), as well as the rivers of Spain. Each point may correspond to dozens of transactions located close together.

Figure 4 shows the approximate location of transactions included in the dataset that correspond to areas of low risk of flooding (T=500), where dots represent hundreds of observations in each location.[4] Notice that there are dots in the Canary Islands, but the island has been excluded to avoid reducing the size of the Peninsula. Areas associated with Ceuta and Melilla are outside the contour of Peninsular Spain.

3. In the empirical analysis, we exploit this ordering while avoiding the overlap between areas: a property's assignment to the 10-, 100-, or 500-year layer is a monotonic, officially certified measure of objective flood probability, which allows us to test whether the implicit price of *probability* — rather than the price of realized damage — changed after the DANA.

4. The largest count is 329 observations.

Figure 4: Location of transactions of homes in T=500 areas



Table 1: Basic results: flats and detached housing

	Mean	Standard deviation	Minimum	Maximum
Flood exposure: 10-year return period	0.014	0.12	0	1
Flood exposure: 100-year return period	0.028	0.16	0	1
Flood exposure: 500-year return period	0.070	0.26	0	1
Price	171443	208767.27	20000	15000000
Price for sqm	1720	1289.43	200	12000
Property age	1980	24.40	1750	2014
Living space (m2)	105	76.03	26	2984
Flat	0.832	0.37	0	1
Post-DANA indicator	0.085	0.28	0	1
Distance to the closest river	8.163	31.05	0	227
Distance to the closest coastline	77.051	112.55	0	361

Figure 5: Google searches' evolution

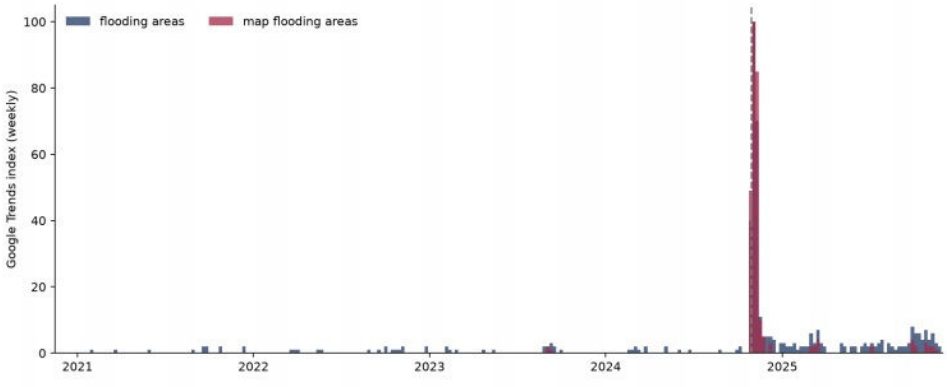


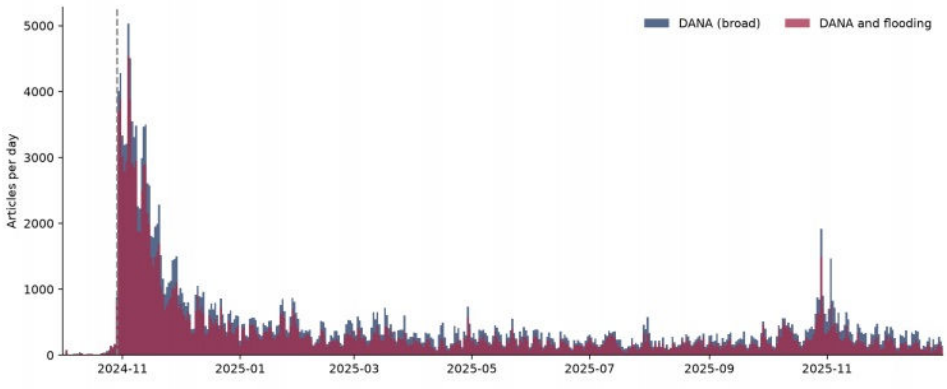
Table 1 presents the descriptive statistics. The first panel contains the flood exposures. Out of the total number of transactions, 1.4% are associated with areas that have a 10-year return period, 2.8% with a 100-year return period (excluding 10-year return areas), and 7% with a 500-year return period (excluding the 10 and 100-year areas). The average price is 171,000 euros, while the average size of the units included in the sample is 105 squared meters. The average year of construction is 1980. These averages include detached houses and flats. Almost 84% of the houses are apartments, which is the dominant type of construction in Spain. Apartments are smaller than detached houses, with a median of 82 squared meters, while detached units have a median of 145 squared meters. Finally, around 8.5% of the observations correspond to the post-DANA period.

5 Empirical Strategy

The empirical strategy exploits the timing of the October 2024 DANA and the cross-sectional variation in *ex ante* flood probability. There is a large literature on the impact of information provision on the risk of flooding. That work estimates the impact of risk information on housing values. In this paper, we analyze the effect of the salience of this information due to a large episode of flooding that produced a significant economic impact and resulted in the loss of many lives. Well before the DANA episode, there were publicly available comprehensive flood maps. We analyze the effect of increasing the salience of flooding risks due to the DANA on the value of housing in floodplain areas other than those directly affected by the DANA. Figure 5 shows the weekly evolution of searches on Google for the terms "floodplain areas" and "maps of floodplain areas". There is a surge of searches for these terms around the beginning of the DANA, and it continues for several weeks. This is the public opinion shock that makes the existence of areas with a high risk of flooding very salient, despite already having precise maps available.

The same effect can be observed using the references in newspapers. Using FACTIVA, I have computed the number of daily references to DANA in broad terms or to DANA and flooding since October 2024. Figure 6 shows that there was a large surge in references to DANA and flooding starting in November 2024. There are a total of 221,995 citations of DANA in broad terms and 166,522 citations for DANA and flooding. The references in newspapers decrease after the DANA period and surge again on the anniversary of the event, as can be seen in Figure 6.

Figure 6: Evolution of references in newspapers



The baseline specification estimates how the price gradient with respect to flood-risk categories changes after the DANA, controlling for municipality and month-time fixed effects.

$$\ln(P_{jt}/m_j^2) = \sum_{g=10,100,500} (\beta_g \, \text{RoF}_{gj} + \delta_g \, (\text{POST}_t \times \text{RoF}_{gj})) + \mathbf{X}'_{jt}\gamma + \mu_t + \mu_m + \varepsilon_{jt}$$

where P/m^2 are house transaction prices per square meter, $POST$ is a dummy variable that takes the value of 1 after the DANA flooding, RoF represents the flood zone status of the area, and $\mathbf{X}$ adjusts for several property-specific characteristics: the age of the property at the time of sale, living space (size), and type of property (apartment or detached house). The specification also includes month-year fixed effects, μ_t, and municipality fixed effects, μ_m.

We study whether the impact of the destruction from the flooding associated with DANA extended outside the affected area and, in particular, to areas that were already classified as zones with a high probability of flooding. The effect of DANA is direct in the area affected by the actual flooding, while the indirect effect results from a change in beliefs associated with the salience of the event. For this reason, in the empirical estimation, we always consider the whole sample as well as the sample that excludes properties in areas directly flooded or physically damaged by DANA. In fact, it could be the case that housing prices increase in some parts of municipalities affected by DANA due to the reduction in supply caused by destruction from the flooding. This effect is suggested by some journalistic accounts.[5]

6 Basic results

This section presents the basic results of the empirical analysis. In Table 2 we consider all the residential real estate (flats and detached houses). Columns 1 to 4 include the basic specification without controls for the characteristics of the properties.[6] Column 1 shows that properties with the highest probability of flooding, after the DANA, show a reduction in price of 8%, which is statistically significant. The properties in the area of medium risk probability do not show a statistically significant reduction in price after the DANA, while the low risk probability

5. See El Pais, August 4, 2026. In the extensions of the model, we analyze whether the impact on unaffected areas is similar to the impact in the DANA affected area.

6. All the regressions include month-year and municipality fixed effects.

properties have a 5% reduction, but only marginally significant. The results are robust to different alternatives for calculating the clustered standard errors (clustered standard errors at the municipal level and at the combined municipal and month-year level) and the exclusion of observations that do not correspond to the area that suffered from the DANA (Columns 3 and 4).[7] Therefore, the reduction in price associated with the effect of DANA occurs even in areas not affected by DANA. Columns 5 to 8 present the same regression but include as explanatory variables the living space, the age of the building at the time of selling, and the dummy that identifies flats. The results are robust to this new specification with property specific characteristics. As expected, living space is statistically significant, as well as the age of the building at the time of selling the property. Large and older properties have a lower price than smaller and newer houses. The dummy for flats is not statistically significant, possibly due to its correlation with size.

7. The DANA-affected municipalities are defined by official Decree 164/2024 and BOE RDL 6/2024 (Law 6/2024).

Table 2: Basic results: flats and detached housing

	(1) lpm20	(2) lpm20	(3) lpm20	(4) lpm20	(5) lpm20	(6) lpm20	(7) lpm20	(8) lpm20
RoF_10 x Post	-0.08***	-0.08***	-0.08**	-0.08**	-0.08**	-0.08***	-0.07**	-0.07**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
RoF_100 x Post	-0.03	-0.03	-0.03	-0.03	-0.03	-0.03	-0.03	-0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
RoF_500 x Post	-0.05*	-0.05*	-0.05*	-0.05*	-0.05*	-0.05*	-0.05*	-0.05*
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
RoF (T=10)	0.09***	0.09***	0.09***	0.09***	0.08**	0.08**	0.08**	0.08**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
RoF (T=100)	0.09**	0.09**	0.09**	0.09**	0.07*	0.07*	0.07*	0.07*
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
RoF (T=500)	0.07	0.07	0.06	0.06	0.07*	0.07*	0.07	0.07
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)
Flat					-0.00	-0.00	-0.01	-0.01
					(0.02)	(0.02)	(0.02)	(0.02)
Age at time of sale					-0.00***	-0.00***	-0.00***	-0.00***
					(0.00)	(0.00)	(0.00)	(0.00)
Living space (m2)					-0.00***	-0.00***	-0.00***	-0.00***
					(0.00)	(0.00)	(0.00)	(0.00)
Cluster s.e.	Muni	Muni Year-month	Muni	Muni Year-Month	Muni	Muni Year-month	Muni	Muni Year-Month
Area	All	All	No DANA	No Dana	All	All	No DANA	No DANA
Observations	3,416,704	3,416,704	3,225,606	3,225,606	3,416,121	3,416,121	3,225,054	3,225,054
R-squared	0.526	0.526	0.531	0.531	0.555	0.555	0.560	0.560

Standard errors in parentheses

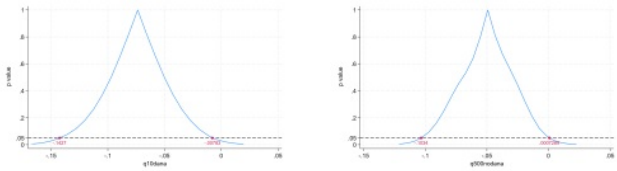
All the regressions include municipality and year-month fixed effect

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

Although the sample contains several thousand municipalities, identifying variation for the coefficients of interest is concentrated in a much smaller set of clusters: those with transactions in 10 year return-period zones after October 2024. When the effective number of clusters is small, or cluster sizes are very heterogeneous, conventional cluster-robust t-tests over-reject (MacKinnon and Webb 2017; MacKinnon, Nielsen, and Webb 2023). For the high risk zones after DANA the t like ratio is -2.32 with a p-value of 0.027 and a 95% interval of [-0.14, -0.01]. For the low risk zones, the t is -2.01 with a p-value of 0.054. Figures 7a and 7b present the 95% confidence set of the estimates. The bootstrap p-values are less than 0.05 only below the 0.05 line.[8]. This result confirms the general conclusion of Table 2 on the robustness of the parameter of the high risk areas in comparison with the parameter of the low risk areas.

Figure 7: Wild bootstrap distributions

(a) $\text{RoF}_{10} * POST$ (b) $\text{RoF}_{500} * POST$



Notes: Estimates correspond to the specification in column 6

Since detached properties and flats are very different, and most of the sample is composed of flats, in Table 3 we show the same regressions as in the previous table but only for the sample of flats. The results are robust to using the sample of flats. From this estimation, we find that for floodplains with a high risk of flooding, there is a negative effect of prices after the DANA. In fact, there is a 10% reduction in price in areas of high risk after the DANA, which is slightly higher than in the full sample. Considering only flats, the low risk floodplains show a marginally significant negative effect. The results are robust to different clustering criteria and the consideration of all areas, or the elimination of the directly affected DANA area.

8. The p-values and confidence intervals are obtained from the restricted wild cluster bootstrap (WCR) of Cameron, Gelbach, and Miller (2008), implemented with the algorithm of Roodman et al. (2019), using 9,999 replications and Webb weights. Confidence intervals are obtained by test inversion. Randomization inference, reassigning the Post date at random within the pre-period and keeping the geography of the bands fixed, leads to similar conclusions.

Table 3: Basic results: flats

	(1) lpm20	(2) lpm20	(3) lpm20	(4) lpm20	(5) lpm20	(6) lpm20	(7) lpm20	(8) lpm20
RoF_10 x Post	-0.10***	-0.10***	-0.09**	-0.09***	-0.09***	-0.09***	-0.08**	-0.08**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)
RoF_100 x Post	-0.04	-0.04	-0.03	-0.03	-0.03	-0.03	-0.03	-0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
RoF_500 x Post	-0.06**	-0.06**	-0.05*	-0.05*	-0.06**	-0.06**	-0.05*	-0.05*
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
RoF (T=10)	0.10***	0.10***	0.10***	0.10***	0.10***	0.10***	0.10***	0.10***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
RoF (T=100)	0.09*	0.09*	0.09*	0.09*	0.08*	0.08*	0.08*	0.08*
	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
RoF (T=500)	0.06	0.06	0.06	0.06	0.08*	0.08*	0.08	0.08
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)
Age at time of sale					-0.00***	-0.00***	-0.00***	-0.00***
					(0.00)	(0.00)	(0.00)	(0.00)
Living space (m2)					-0.00***	-0.00***	-0.00***	-0.00***
					(0.00)	(0.00)	(0.00)	(0.00)
Cluster s.e.	Muni	Muni Year-month	Muni	Muni Year-Month	Muni	Muni Year-month	Muni	Muni Year-Month
Area	All	All	No DANA	No DANA	All	All	No DANA	No DANA
Observations	2,845,572	2,845,572	2,672,968	2,672,968	2,845,119	2,845,119	2,672,544	2,672,544
R-squared	0.531	0.531	0.537	0.537	0.553	0.553	0.559	0.559

Standard errors in parentheses

All the regressions include municipality and year-month fixed effects

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

For robustness, we also estimate our main specifications with the log of the transaction price as the dependent variable rather than the log of the price per square meter. The price per square meter is only defined for transactions in which the size variable is observed. Since missingness could be non-random, it is interesting to check if the estimates are robust to using the price instead of the price per square meter. This sample loss is not specific to our data.[9] Columns 1 to 4 of Table 4 show that the results are not affected by using the price instead of the price per square meter.[10] High risk areas show a 7-8% reduction in price after DANA, independent of the extension of the geographical sample (all or no DANA locations) and the clustering criteria. Columns 5 to 8 include living space as an additional explanatory variable. The main effect is identical, but the fit of the regression improves significantly, showing the relevance of the size of flats in the hedonic regression of house prices.

9. In their study of the effect of Hurricane Sandy on the New York housing market, Ortega and Taşpınar (2018) observes the price per square foot for only 385,139 of their 578,500 observations, so about one third of the sample is lost when the outcome is normalized by size. For the same reason, their specifications that control for square footage are restricted to one- and two-family houses, which leaves 165,642 of 571,940 observations (29 percent). This is why they take the log of the sale price as the dependent variable throughout. Menéndez et al. (2024) report a similar constraint for the Basque Country: observations with missing attributes cannot be used, and adding regressors would "drastically reduce the available observations."

10. The sample considers only flats. Results for the whole sample are similar.

Table 4: Basic results: ln(prices) for flats

	(1) lpv	(2) lpv	(3) lpv	(4) lpv	(5) lpv	(6) lpv	(7) lpv	(8) lpv
RoF_10 x Post	-0.07**	-0.07**	-0.07**	-0.07**	-0.08**	-0.08**	-0.07**	-0.07**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
RoF_100 x Post	-0.04	-0.04	-0.03	-0.03	-0.04	-0.04	-0.04	-0.04
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
RoF_500 x Post	-0.05*	-0.05*	-0.05	-0.05*	-0.06*	-0.06**	-0.06**	-0.06**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
RoF (T=10)	0.05	0.05	0.05	0.05	0.07*	0.07*	0.07*	0.07*
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
RoF (T=100)	0.04	0.04	0.05	0.05	0.04	0.04	0.04	0.04
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)
RoF (T=500)	0.04	0.04	0.04	0.04	0.06	0.06	0.06	0.06
	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
Age at time of sale					-0.01***	-0.01***	-0.01***	-0.01***
					(0.00)	(0.00)	(0.00)	(0.00)
Living space (m2)					0.00***	0.00***	0.00***	0.00***
					(0.00)	(0.00)	(0.00)	(0.00)
Cluster s.e.	Muni	Muni Year-month	Muni	Muni Year-Month	Muni	Muni Year-month	Muni	Muni Year-Month
Area	All	All	No DANA	No DANA	All	All	No DANA	No DANA
Observations	3,300,796	3,300,796	3,116,873	3,116,873	2,863,739	2,863,739	2,690,618	2,690,618
R-squared	0.341	0.341	0.345	0.345	0.432	0.432	0.435	0.435

Standard errors in parentheses

All the regressions include municipality and year-month fixed effect

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

The coefficients on the mutually exclusive flood-risk bands are frequently statistically significant in the previous tables. This result may be due to the omission of relevant variables in the hedonic specification. I have constructed two variables to measure the distance of each home sold to the closest river and the closest coastline. I use distance bins instead of a linear term, so the band dummies do not pick up leftover nonlinear effects of distance.[11]. When distances to the nearest river and to the coastline are included, the band dummies are no longer statistically significant, as shown in Table 5. The interactions of the high risk bands with the post-DANA indicator remain significant and stable, while the low risk band is also statistically significant, although smaller than the high risk. The two sets of coefficients are identified from different variations. The level effects are cross-sectional: within county-year cells, they compare dwellings inside and outside each band. Because SNCZI flood zones are defined by fluvial and coastal inundation, band membership is strongly correlated with proximity to water, which carries amenity value (views, beach access, waterfront location) and possibly disamenities. Without distance controls, the band dummies therefore combine a risk discount with a proximity effect, the omitted-variable problem documented in hedonic flood studies (Bin and Polasky 2004b; Bin, Kruse, and Landry 2008). Conditional on distance, the remaining variation in band membership is limited, and the estimated level effects are small and imprecise. This is consistent with weak capitalization of mapped risk before the DANA, although the lack of statistical significance should not be read as evidence of a zero effect.

The post-DANA interactions are instead identified from changes over time in the price gap between risk bands and the reference group. Time-invariant distances whose price effect does not change over time are already contained in the pre-DANA gap and drop out of its change, as in any difference-in-differences comparison. Adding distance controls can move the interaction coefficients only if the distance-to-water composition of transactions within a band changes after the DANA, or if the event changes the valuation of proximity itself. The stability of the interactions therefore indicates that the DANA response is not driven by these channels.

11. All the distance variables are binned at 0 to 100 meters, 100 to 500 m, 500 to 1 km, 1 km to 2 km, 2 to 3 km, 3 to 4 km, 4 to 5 km, 5 to 10 km and more than 10 km

Table 5: Controlling for distance to rivers and coast

	(1) lpm20	(2) lpm20	(3) lpm20	(4) lpm20	(5) lpm20	(6) lpm20
RoF_10 x Post	-0.07**	-0.07**	-0.09**	-0.09**	-0.08**	-0.08**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)
RoF_100 x Post	-0.03	-0.03	-0.04	-0.04	-0.03	-0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
RoF_500 x Post	-0.05**	-0.05**	-0.06**	-0.06**	-0.05**	-0.05**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
RoF (T=10)	0.05	0.05	0.07	0.07	0.06	0.06
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
RoF (T=100)	0.05	0.05	0.06	0.06	0.06	0.06
	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
RoF (T=500)	0.07	0.07	0.07	0.07	0.07	0.07
	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)
Flat	-0.02	-0.02				
	(0.02)	(0.02)				
Age at time of sale	-0.01***	-0.01***	-0.00***	-0.00***	-0.00***	-0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Living space (m2)	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Cluster s.e.	Muni	Muni Year-month	Muni	Muni Year-Month	Muni	Muni Year-month
Area	All	All	No DANA	No DANA	No DANA	No DANA
Type of home	All	All	Flat	Flat	Flat	Flat
Observations	3,407,304	3,407,304	2,837,867	2,837,867	2,665,304	2,665,304
R-squared	0.580	0.580	0.579	0.579	0.586	0.586

Standard errors in parentheses

All the regressions include municipality and year-month fixed effect and binned distances to the closest river and coastline.

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

To assess whether the estimated post-DANA repricing of flood exposure reflects a specific response to the October 2024 disaster rather than spurious changes in the relative price of flood-prone housing, we conduct placebo difference-in-differences exercises using three earlier dates. We re-estimate the baseline specification after replacing the October 2024 treatment date with placebo post indicators beginning in 2011, 2015, and 2018. These dates are informative because they correspond to episodes that could, in principle, have increased awareness of flood risk without generating a shock comparable in scale, visibility, and national salience to the Valencia DANA: 2011 marks the public disclosure and diffusion of official floodplain maps; 2015 coincided with media attention to floodplain maps following important flooding episodes; and 2018 was characterized by severe floods that may have raised public awareness of exposure. The placebo tests are consequently demanding: a significant negative coefficient for the interaction between the high-risk flood-zone indicator and any placebo post period would suggest that the baseline estimate captures a recurrent or map-related change in the valuation of risky locations, rather than a discrete effect of the 2024 DANA. In implementing these exercises, we restrict each placebo estimation to a symmetric window around the relevant date and terminate the sample immediately before the next candidate salience episode, thereby avoiding contamination of a placebo post period by later floods, subsequent map-related information, or the 2024 DANA itself. We find statistically small and insignificant placebo coefficients: the parameter estimate of the interaction between the placebo date for the post period and the dummies of the risk level (high, medium, and low) is never statistically significant. This finding supports the interpretation that the observed repricing is specific to the exceptional salience generated by the Valencia DANA.

7 Heterogeneity and mechanisms

In this section, we consider potential sources of cross-sectional and time heterogeneity. We analyze the incidence of past flooding (frequency, severity, and timing), the differences between the DANA affected area and the rest of the sample, and the evolution of the temporal impact. Since the previous results indicate that the only robust effect is associated with high risk floodplains, in this section we only consider these areas as at risk.

7.1 The effect of previous flooding

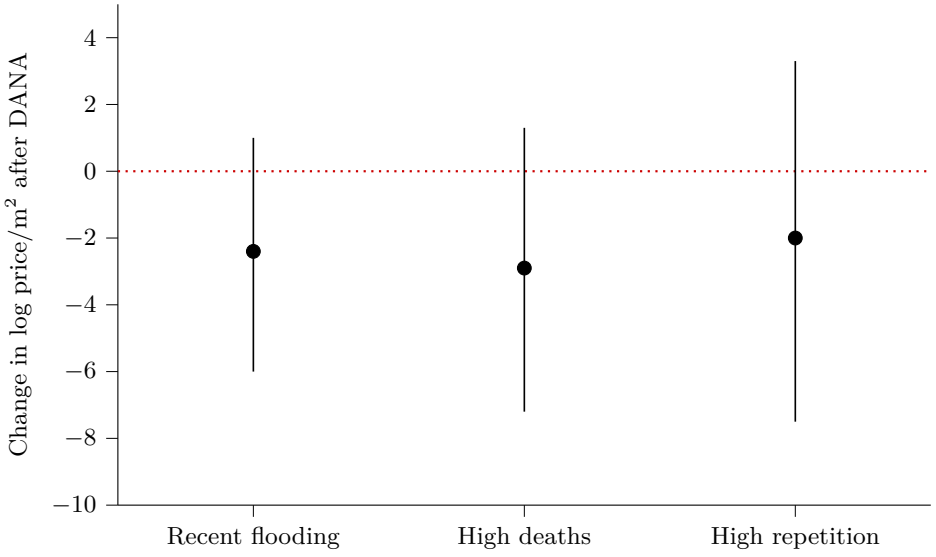
Recent research has analyzed the potential effect of the memory of previous flood events on the impact of a particular flood. Bellaver et al. (2025) concludes that specific floods do not decrease home prices in areas at risk. It is repeated exposure to floods in floodplains that leads to significant declines in home prices. Bellaver et al. (2025) estimates that repeated exposures lead to a 4% decline in prices, with a heterogeneous response depending on the age and income of the buyer. But is there a differential effect of having suffered a recent previous flood or repeated floods on the impact of the salience of DANA on home prices in floodplains?

We use the *Catálogo Nacional de Inundaciones Históricas* (CNIH, National Catalog of Historical Floods), the official inventory of significant riverine flood episodes recorded in Spain, to obtain measures of severity, frequency, and timing of the previous floods.

We use the CNIH to construct the municipality-level measure of *historical flood memory* employed in the heterogeneity analysis: the count and severity of past significant flood episodes affecting each municipality prior to the sample period. This lets us test whether the post-DANA repricing of objective flood probability is concentrated in places without a recent experiential record - where the disaster plausibly carried the most new information - or whether it also appears where risk was arguably already salient.

We have constructed three measures in the form of dummies that take a value of 1 above the median of the sample. First, the recent flooding dummy is equal to one for flooding that occurred after 2018. Second, the high deaths flood dummy takes a value of 1 for floods that led to more than 12 deaths. Finally, the high repetition dummy is equal to 1 if that area has suffered more than one flood during the last 20 years.

Figure 8: Heterogeneous effects of the DANA on housing prices



Notes: Points are estimated coefficients; vertical lines are 95% confidence intervals.

We have estimated two specifications. The first one is a triple difference-in-differences estimation to check if different risk floodplains after the DANA suffered a larger reduction in prices in the case of a recent flood, a high death toll flood, or a high repetition flood area. These interactions are always negative but statistically not significant. The second specification includes an interaction of the POST dummy (post-October 2024) and these dummies. Figure 8 shows the estimated coefficients for this interaction. In general, the effect is negative but imprecisely estimated.[12]

7.2 Dana affected area versus not affected

Another interesting dimension of the heterogeneous effect of the DANA is the impact on the area affected directly versus the area not affected by the flooding. To study this issue, we run a

12. Future versions of the paper will analyze more detailed measures of the severity and frequency of previous flooding, including the time since the last flooding and interactions between intensity and recency.

triple difference-in-differences specification

$$\begin{aligned}\ln(P_{jt}/m^2_{jt}) = {} & \beta_1 \text{RoF}_{10j} \cdot \text{Post}_t + \beta_2 \text{RoF}_{10j} \cdot \text{Post}_t \cdot \text{DANA}_m \\ & + \beta_3 \text{RoF}_{10j} \cdot \text{DANA}_m + \beta_4 \text{DANA}_m \cdot \text{Post}_t \\ & + \beta_5 \text{RoF}_{10j} + \mathbf{X}'_{jt}\gamma + \mu_m + \mu_t + \varepsilon_{jt},\end{aligned}$$

where μ_m and μ_t absorb the municipality and time effects. The omitted group is low-risk properties outside the DANA-affected area.The coefficient β_2 measures how much more the flood-risk discount widened in the affected area than elsewhere. Within the affected area, high-risk properties fell relative to low-risk properties by -46.2% in the full sample and -25.2% for apartments (Column 2). The large change in the parameter β_2 from -0.55 to -0.21 in the sample of only flats suggests that the significant effect in column (1) comes from detached houses and ground-level structures. The coefficient β_4 indicates that low-risk properties in the affected area gained 13-15% relative to low-risk properties elsewhere, which is consistent with demand shifting toward nearby safe housing in municipalities directly affected by the DANA. Because properties in the affected area were also exposed to physical damage, β_2 combines a salience effect with a damage effect and should be read as an upper bound on the pure information channel.

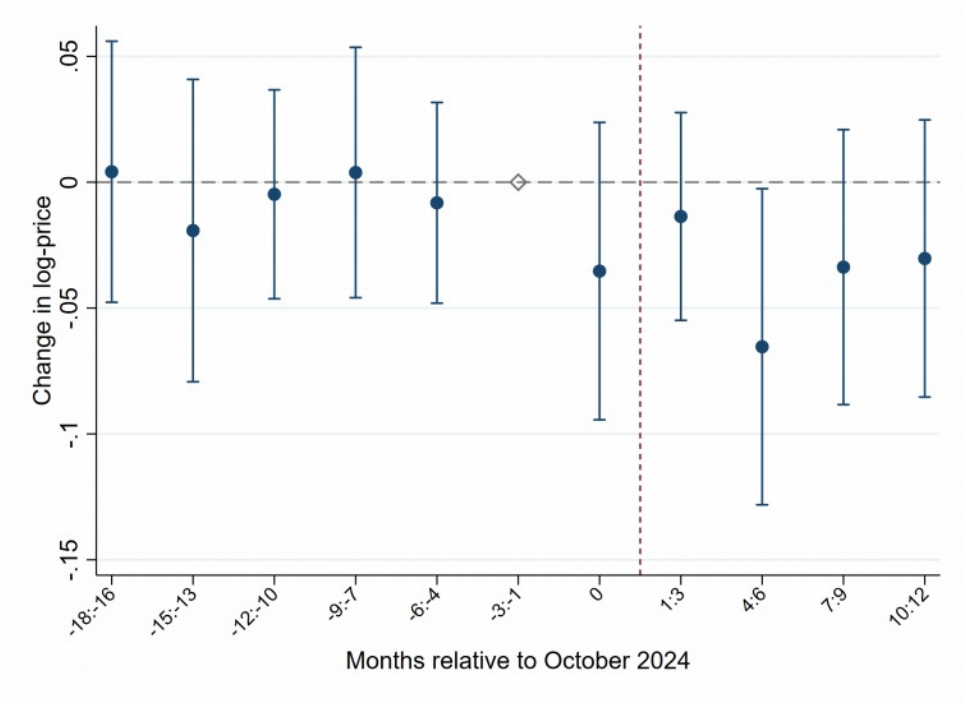
Table 6: Heterogeneous effects: municipalities affected versus high-risk areas

	(1) lpm20	(2) lpm20	(3) lpm20
RoF_10 x Post	-0.07**	-0.08**	-0.08**
	(0.03)	(0.03)	(0.03)
RoF_10 x Post x DANA area	-0.55**	-0.21	-0.215**
	(0.20)	(0.13)	(0.10)
RoF_10 x DANA area	0.032	0.022	0.022
	(0.10)	(0.12)	(0.12)
DANA area x Post	0.12**	0.14**	0.14***
	(0.06)	(0.06)	(0.05)
RoF_10	0.06**	0.08***	0.08***
	(0.03)	(0.03)	(0.03)
Age at time of sale	-0.00***	-0.00***	-0.00***
	(0.00)	(0.00)	(0.00)
Living space (m2)	-0.00***	-0.00***	-0.00***
	(0.00)	(0.00)	(0.00)
Constant	7.48***	7.51***	7.51***
	(0.06)	(0.08)	(0.08)
Cluster s.e.	Muni	Muni	Muni Year-Month
Type	All	Flats	Flats
Observations	3,416,121	2,845,125	2,845,125
R-squared	0.55	0.55	0.55

Standard errors in parentheses

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

Figure 9: Timing of impact



7.3 Dynamic impact

The main section of results imposes a step change after the DANA episode. However, in principle, the impact of the salience of DANA on house prices in high risk floodplains could evolve over time. To analyze this possibility, we substitute the POST_t dummy with a full set of period indicators relative to October 2024:

$$\ln(P_{jt}/m_{jt}^2) = \sum_{k=-K,\, k\neq -1}^{K} \delta_k \, \mathbf{1}[t - t_0 = k] \times \text{RoF}_{10j} + \mathbf{X}'_{jt}\gamma + \mu_j + \mu_m + \mu_t + \varepsilon_{jt}, \qquad (1)$$

normalizing $\delta_{-1} = 0$. Given the low number of observations available when we consider the high risk flooding areas after the DANA at a monthly frequency, the estimation is performed using quarterly data. Figure 9 shows that only the second quarter after the DANA exhibits a significant reduction in prices. Afterward, the changes are also negative but imprecisely estimated.[13]. For this reason, and using the data currently available, it is not possible to conclude whether the discount in prices associated with the DANA is a persistent repricing or a transitory attention effect that reaches its minimum after 4 months and decays as media salience fades. It is interesting to note that the period pre-DANA shows no significant differences between treated and controlled areas, while after the shock, the estimates become negative, although quite imprecisely estimated given the available sample size.

13. The temporal impact in medium and low risk flooding areas is not statistically significant at any time period when included in the event study

8 Repeat sales evidence

Most of the previous literature uses cross-sectional analysis to decompose the sales price into classical hedonic characteristics and floodplain categorization. This method relies on the assumption that there are controls for all the relevant property characteristics that are correlated with the floodplain status. In the previous section, we adopted a hedonic difference-in-differences approach that considers floodplain status and the DANA episode to measure the impact on property values. In this case, I need to account for the difference in the composition of houses sold at different times. Since the number of controls we have access to is limited, I consider a sample of panel repeat sales in this section.[14] In this approach, the property fixed effect accounts for time-invariant confounds, including property characteristics. We also include a fixed effect for municipality and month-year.[15] The sample of panel repeat sales is smaller than the original sample

Let j index dwellings and add a dwelling fixed effect μ_j:

$$\ln P_{jt}/m_j^2 = \sum_{g=10,100,500} \delta_g \, \mathrm{POST}_t \times \mathrm{RoF}_{gj} + \mu_j + \mu_m + \mu_t + \varepsilon_{jt}. \tag{2}$$

Equation (2) is the hedonic analog of the repeat-sales logic and is how the flood-discount literature typically upgrades cross-sectional hedonics (Bin and Landry 2013; Ortega and Taşpınar 2018). Table 7 presents the results of the sample of panel repeat sales. Column 1 corresponds to all the observations, while column 2 includes only flats, and column 3 excludes those in the DANA affected area from the flats. The estimate of the high risk floodplain is smaller than in the basic estimation but still around 5%, while the low risk floodplain coefficient is similar to the previous estimates, around 4%, and is statistically significant. The smaller repeat-sales estimate is informative about the role of compositional differences across properties sold before and after the DANA. By comparing the same dwelling across transactions, the repeat-sales design absorbs all time-invariant dwelling and micro-location characteristics, including unobserved quality and floodplain-related amenities. The persistence of a statistically significant 4–5% post-DANA discount in this demanding specification strengthens the conclusion that DANA changed the relative valuation of exposed housing, while the attenuation relative to cross-sectional hedonic estimates indicates that part of the larger cross-sectional effect reflects changing composition or unobserved spatial sorting.

14. **hino2021** argues that the use of cross-sectional methods relies on the very unlikely assumption that the researchers have controlled for every property characteristic that is correlated with floodplain status and prices. For this reason, **hino2021** argues that it is better to rely on the results of diff-in-diffs estimation and panel repeat sales.

15. In some specifications, we also include a fixed effect for the age at the time of selling the property. Notice that this age changes for each property in subsequent selling periods.

Table 7: Panel data: repeat sales sample

	(1) lpm20	(2) lpm20	(3) lpm20
RoF_10 x Post	-0.04***	-0.05***	-0.04***
	(0.01)	(0.01)	(0.01)
RoF_100 x Post	-0.02**	-0.02*	-0.01
	(0.01)	(0.01)	(0.01)
RoF_500 x Post	-0.05***	-0.05***	-0.04***
	(0.01)	(0.01)	(0.01)
Observations	162,579	127,709	120,715
R-squared	0.907	0.909	0.911

Standard errors in parentheses

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

9 Conclusion

This paper asks whether a highly salient climate disaster changes the price that housing markets attach to objective flood probability. The raw data of notarial transactions includes 3.9 million operations (flats and detached houses) for the period from January 2007 to October 2025. These data are matched to cadastral characteristics and the official SNCZI hazard maps that classify every dwelling by the 10-, 100-, and 500-year return periods of large floods. The Valencia DANA of 29 October 2024 serves as the identifying shock. The hazard maps had been public for more than a decade, so the event added little new information about where floods occur. What it changed was attention: Google searches for flood-zone maps and press coverage of the DANA and flooding rose sharply after the event and again on its first anniversary. The research design, therefore, separates the effect of salience on the pricing of risk from the direct effect of physical damage.

The results can be summarized in six points. First, before the DANA, the market did not discount mapped flood risk. Conditional on municipality and month fixed effects and on property characteristics, dwellings in 10-year and 100-year zones sold at a premium of 7-10 percent per square meter relative to unexposed dwellings. This premium is consistent with the amenity value of locations close to rivers, coastlines, and flat, central urban land, and it confirms the concern in the hedonic literature that cross-sectional comparisons conflate risk with amenities. Official risk information, although freely available, was not reflected in prices.

Second, the DANA triggered a repricing concentrated in the highest-hazard band. After October 2024, prices per square meter in 10-year return-period zones fell by about 8 percent relative to unexposed properties and by 9-10 % in the sample of flats. The estimate is stable across clustering choices and property controls, and it is essentially unchanged when the municipalities officially designated as affected by the DANA are excluded from the estimation set. The exclusion of officially affected municipalities substantially reduces concerns that the estimate is driven by direct physical damage. The remaining effect is consistent with a national repricing of objectively measured flood exposure following the DANA, although it may reflect a combination of salience, changed beliefs, anticipated policy responses, and other market-wide adjustments. The shock removed most of the pre-existing premium for high-risk locations.

Third, the repricing is not proportional to flood probability. Dwellings in 100-year zones show no statistically significant change. Dwellings in 500-year zones show a decline of around 5 percent that is less robust across samples and specifications than the effect in the 10-year band. The market response is thus concentrated where risk is highest and most easily understood, a pattern consistent with coarse, threshold-based updating rather than a smooth repricing of the full probability gradient.

Fourth, the result is not driven by the normalization of prices or by changes in the composition of sales. Using the log of the total transaction price, which does not require surface information, gives a post-DANA decline of 7-8 percent in 10-year zones. Estimates from a repeat-sales panel with dwelling fixed effects, which absorb all time-invariant property and location characteristics, imply a discount of 4-5 percent in both the 10-year and the 500-year zones and a smaller one in the 100-year zone. The smaller magnitude in the repeat-sales sample suggests that part of the hedonic estimate reflects compositional change, but the direction and significance of the effect

survive the most demanding specification.

Fifth, the effect is much larger in the area directly hit by the flood, and the affected market reallocated demand within its own boundaries. In the DANA municipalities, high-risk dwellings fell by an additional 0.55 log points relative to high-risk dwellings elsewhere, and by 0.21 log points in the sample of flats; the difference indicates that detached houses and ground-level units bore most of the loss. At the same time, low-risk dwellings in the affected municipalities appreciated by 12-14 percent relative to low-risk dwellings in the rest of the country, consistent with displaced households and new buyers moving toward nearby safe housing when the habitable stock contracted. Because damage and salience coincide in this area, these estimates provide an upper bound on the information channel.

Sixth, the dynamics and the heterogeneity of flood history are imprecise. A quarterly event study shows no differential pre-trends between high-risk and unexposed dwellings. After the shock, the coefficients become negative and reach their largest value two quarters after the event, the only quarter in which the estimate is individually significant; later coefficients remain negative but are imprecisely estimated. The data, therefore, cannot yet distinguish a persistent repricing from a transitory attention effect that fades with media coverage. Similarly, interactions with municipal flood memory from the National Catalog of Historical Floods—recent floods, deadly floods, and repeated floods—are mostly negative but not statistically significant. The DANA seems to have repriced high-risk zones broadly, without a clear dependence on local experience.

Taken together, the evidence shows that Spanish housing markets priced mapped flood risk only after a catastrophic event made it salient, and that the repricing extended well beyond the damaged area. This pattern supports models of limited attention in which low-probability risks are ignored until a salient event draws attention to them, and it complements evidence that disclosure and experience, rather than the mere availability of maps, determine capitalization (Hino and Burke 2021; Bellaver et al. 2025). It also suggests that before October 2024, the Spanish housing stock in high-hazard zones was overvalued relative to its risk, as documented for the United States by Gourevitch et al. (2023).

The findings have three policy implications. First, publishing hazard maps is not enough to ensure that risk is priced; information must reach buyers at the point of decision. The recent proposal to record flood exposure in the deed of sale at the moment of transaction addresses this gap directly, and the results suggest that it could make the pricing of risk less dependent on the occurrence of disasters. Second, the broad public coverage of extraordinary risks by the Consorcio de Compensación de Seguros lowers the expected private cost of damage and may explain why baseline discounts were absent. The fact that prices responded after the DANA, even though coverage did not change, indicates that buyers value uninsured costs such as disruption, loss of life, displacement, and resale risk. Third, tighter land-use rules in flood zones are likely to be capitalized into the value of affected parcels, which has distributional consequences for current owners that should be considered in the design of the reform.

Several questions remain for future work. The post-DANA window is still short, and additional quarters of data will be needed to establish whether the discount persists or decays as attention fades. Richer measures of flood memory, including the time since the last flood

and the interaction between intensity and recency, would help test whether experience shapes the response. It is also possible to exploit the property-level latitude and longitude coordinates to characterize the spatial propagation of flood-risk repricing. Finally, information on buyer characteristics, second homes, foreign purchasers, mortgage financing, and rents would allow for a sharper separation of the belief, insurance, and credit channels through which a salient disaster reshapes the price of climate risk.

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