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Nils aus dem Moore, Johannes Brehm, Philipp Breidenbach, Arijit Ghosh, and Henri Gruhl[1]

Flood Risk Perception after Indirect Flooding Experience: Null Results in the German Housing Market

Abstract

The frequency and severity of fluvial floods are expected to increase due to climate change. This paper investigates whether flood risk perception in the housing market changes across a country after the occurrence of a catastrophic fluvial flood. Using a comprehensive geocoded German house price data set and official flood risk maps, we exploit the July 2021 fluvial flood that was salient across Germany as an exogenous variation to causally measure the flood risk valuation update in a difference-in-differences setup. While we find that house prices decreased in the most inundated regions, no price changes occurred in flood risk regions that were not directly affected. This finding indicates that people did not update their risk perception after indirect exposure. With this paper, we contribute to the understanding of the impact of a salient flood on flood risk capitalization in places without direct exposure.

JEL-Code: Q54, Q51, D81, R31

Keywords: Flood risk; home prices; risk updating

October 2022

1 Nils aus dem Moore, RWI and Hertie School; Johannes Brehm, RWI and Hertie School; Philipp Breidenbach, RWI; Arijit Ghosh, RWI; Henri Gruhl, RWI. *– We thank Louise Meister, Marlene Merker, and Maraike Schäfer for excellent research assistance. The authors gratefully acknowledge the financial support of the German Environmental Agency (UBA) under research reference number 19 12 104 0.* *– All correspondence to: Henri Gruhl, RWI - Büro Berlin, Zinnowitzer Str. 1, 10115 Berlin, Germany, e-mail: henri.gruhl@rwi-essen.de*

1 Introduction

The latest report of the Intergovernmental Panel on Climate Change (IPCC) sheds much-needed light on flooding events with two crucial points. First, climate change has exacerbated extreme weather events such as floods, and second, both the frequency and the severity of coastal and fluvial floods have increased over time (IPCC Working Group I, 2021). The report further discusses how the potential damages to lives, infrastructure, and properties are expected to increase and be large. The report's findings, however, stand in contrast to the literature on real estate markets which finds that flood risks tend to be undervalued by homeowners. In other words, flood risks are not correctly capitalized in home prices (Bakkensen and Barrage, 2022; Hino and Burke, 2021; Muller and Hopkins, 2019).[1]

However, after floods have occurred, the associated risk perception changes. The literature finds that direct experience with a flooding event dampens house prices, property prices fall in the most affected areas due to damage and also increases in flood risk discounts (Beltrán *et al.*, 2019; Atreya *et al.*, 2013; Atreya and Ferreira, 2015; Bin and Polasky, 2004; Bin and Landry, 2013; Daniel *et al.*, 2009; Kousky, 2010). Effects on home prices in regions that are aware of the flood but not geographically close to the impact of the flooding have not been in the focus of research yet.[2] Nevertheless, this effect is important since only if markets react this channel could prevent dangerous disproportionate urban and economic development in these regions even before the damage occurs.

Against this backdrop, this paper investigates the impact of a salient flood event on flood risk valuation in areas that were not directly affected by the flood. To tackle this question, we focus on Germany and exploit the heavily damaging floods in July 2021 ("Jahrhunderflut"). The heavy precipitations between 12–19 July 2021 resulted in flash floods which led to 189 deaths[3] and severe damages.[4] Current estimates point to damages worth EUR 33 billion in residential, commercial, and industrial sectors as well as in the public sector and at infrastructures (Munich RE, 2022; Koks *et al.*, 2021). Given the impact, the flooding was broadly covered by the media, and the cause of the flood was attributed to climate change (Kahle *et al.*, 2022).[5] The salience of the flood to the public was further amplified

[1] See Beltrán *et al.* (2018) for a meta-analysis up to May 2014.

[2] In the broader social science literature, research on indirect exposure (e.g., through salient media coverage of an event and its impact) on risk perception is inconclusive, and reports mixed results (Binder *et al.*, 2014; Niu *et al.*, 2022). Many studies find an effect of media on risk perception (Binder *et al.*, 2014; Liu *et al.*, 2021). However, this positive relationship is mostly reported for health- or food-related issues (Bekalu and Eggermont, 2014; Garfin *et al.*, 2022; Han *et al.*, 2014; Ju and You, 2021; Mou and Lin, 2014; Ng *et al.*, 2018; Raupp, 2014; Vyncke *et al.*, 2017). The few studies that investigate the effect of indirect experience on environmental risk perception find a limited influence of indirect experience and did not look at flooding events (Brenkert-Smith *et al.*, 2013; Johnston *et al.*, 1999; Niu *et al.*, 2022).

[3] This death toll is the highest in Germany from water-related hazards since a storm surge in February 1962 along the North Sea Coast (Thieken *et al.*, 2022)

[4] See Dietze *et al.* (2022) for a scientific description the July 2021 flood.

[5] See Figure A.1 in the Appendix for Google trends data in Germany. Shortly after the event, search terms such as "Flooding", "High tide", "Rain" and "Climate Change" spiked.

by the flooding becoming a key topic of the 2021 federal election. Therefore, the flood event is suitable as an exogenous variation to measure indirect flood experience as it was unprecedented in the lives lost, and economic damage it caused, and this information was spread across the whole country and not just inundated regions.

We exploit the salient flooding event as an exogenous variation to determine whether people update their risk perception in the housing market after a fluvial flood. To that end, we use a comprehensive geocoded data set on German house prices and official flood risk maps. We apply a difference-in-difference design, which allows us to compare house prices in and outside flood risk zones with similar characteristics accounting for potentially different price levels before the flood. This setup allows us to investigate whether the flood event induced a decrease in home prices in flood risk zones[6] relative to a similarly characterized comparison group outside (but close by) flood risk zones.

While we provide indicative evidence that flood risks are capitalized in the German housing market, we find that the July 2021 flood event had no statistically significant impact on the price of houses in flood risk zones compared to comparable houses outside the flood risk zones. This finding is robust to various specifications and samples. On the other hand, we find a negative and statistically significant impact of the flood on house prices in regions that were directly affected – either by being inundated or recording casualties. This finding is in line with our expectations as those directly affected regions experienced physical damage and a likely update on flood risk perceptions as documented in the literature. Importantly, our finding suggests that people that do not directly experience the flood do not seem to update their flood risk perception. Our findings should alert policymakers seeking to protect lives and wealth from the outfalls of climate change. We discuss three possible explanations for those null results: first, the indirect experience of a flood event (e.g., media) only weakly increases risk perception. Second, risk perception may not translate into action, which would be reflected in house prices. And lastly, contrary to the US literature, house prices in Germany may already include rationally derived risk premiums.

Our work contributes to the literature relating flood risk to the real estate market in several ways. First, we focus on fluvial floods exploiting a country-wide data set of geocoded home offers and flood risk maps. The flooding events in 2021, which left behind severe damages to life and property, were flash floods caused due to excess rainfall concentrated within a brief period. Due to climate change, fluvial floods are expected to increase in magnitude and likelihood. Our paper thus contributes to the understanding of the impact of fluvial floods, which remains under-researched compared to coastal flooding. In contrast to sea level rise affecting coastal regions, flash floods are less predictable but also have a significant impact. Second, and connected to the first, we focus on the effect of the July

[6] Excluding those which ended up being part of the affected regions in the 2021 flooding events, see Figure A.3 for definition and data.

2021 flood on indirectly affected regions. In other words, we investigate whether the flood impacted home prices in flood-risk areas that were not affected by the flood. We thus contribute to the understanding of risk perception updating following a salient flood event which usually looks at updating in or close to inundated regions. These studies often lack the ability to disentangle physical damage from risk perception updating. Third, to the best of our knowledge, we are the first to analyze risk perception updating in the German real estate market. While a large body of literature focuses on the US housing markets, the German case differs on various points (e.g., larger rental market, less polarization in climate change belief). We comprehensively analyze the causal impact of the flood on risk perception updating and provide the first evidence of flood risk valuation in the German housing market.

The paper proceeds as follows. Section 2 presents the flood risk and housing data set and the German context. Section 3 introduces the empirical strategy we use to analyze the effect of the flood. In Section 4, we present the main results, test their robustness, and investigate heterogeneous treatment effects. Section 5 discusses our results and Section 6 concludes the paper.

2 Data

Our data is spatially explicit with two main strands, one for our outcome variable, home offer price for a given object at a given point in time, and one for our variable indicating the treatment group, that of being inside a flood risk zone.

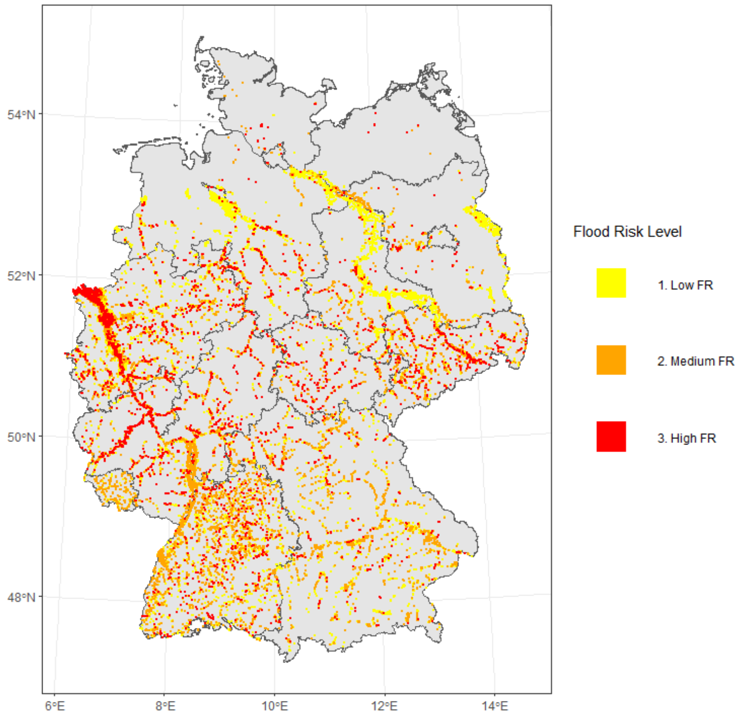
2.1 Flood risk data

The federal flood risk map, which acts as the basis for treatment assignment in our study, is obtained from the German Federal Institute of Hydrology (BfG, 2020). We use the latest version of the map, wherein the last updates were made towards the end of 2019. As a result, we are confident that no new information from the 2021 floods had an impact on the map. This static map is obtained as spatial polygons for fluvial and coastal flood risks. The flood risk is further categorized into low, medium, and high risk levels, which refer to flood occurrence probabilities of once within the next 200 years, once within the next 100 years, and once within the next 5 to 50 years, respectively. The higher-risk areas tend to be closer to water bodies. The median distance from a house to the nearest water body in low-risk areas is about 2681 meters, in medium-risk regions is about 1964 meters, and in high-risk areas is 1833 meters. For our analysis, we will only focus on fluvial flood risk since our treatment– the exogenous shock of the occurrence of the 2021 floods– was related to extreme precipitation, which causes flooding around rivers and not coasts.[7]

[7]Moreover, coastal flooding areas suffer from storm surges or sea-level rise but heavy precipitations should not affect coastal flooding.

As shown in Figure 1, while most of the homes in the flood risk areas appear to map out some of the major river networks in Germany, a large number of dots (houses) are affected by smaller water bodies. Table A.2 presents a map of German water bodies as a comparison. The North-East part of the map appears bare, without any homes subject to flood risk since we do not consider coastal flood risk in this study. Once again, our justification is that the 2021 floods were flash floods and provided a different set of information to households than coastal floods would.

Figure 1: Homes in Fluvial Flood Risk Zones, Germany



Notes: This figure depicts the homes which lie in fluvial flood risk areas, each dot represents one home

2.2 Housing data

To analyze the housing market, we use the RWI-GEO-RED real estate dataset (Schaffner, 2020) from the Research Data Center Ruhr at the RWI (FDZ Ruhr). The comprehensive data set on the German real estate market provides data from Germany's leading online real estate portal *ImmobilienScout24*. The data includes necessary information such as the offer price measured in Euros, the date on which the ad was first put up, and the date on which the ad was taken down. Advertisers also include other property-specific characteristics such as the living area, number of rooms, condition of the house, year of construction, and building characteristics. As the data comes from a cooperation with *ImmobilienScout24* we are able to use back-end information from the website such as the exact geo-coordinates. This enables us to combine both datasets, the flood risk areas, and the housing information.

We have four possible data sets for the analyses in the combination of rent and sale prices as well as apartments and houses. We focus on the sale of houses for two reasons. First, long-term and expensive purchasing decisions are likely more influenced by long-term risk perceptions, and second, owners of homes are more responsible for potential flood damage than renters. In our sample, one property appears only once – we ensure this by including only the last spell of an ad placement corresponding to a given property.

2.3 Treatment and control group

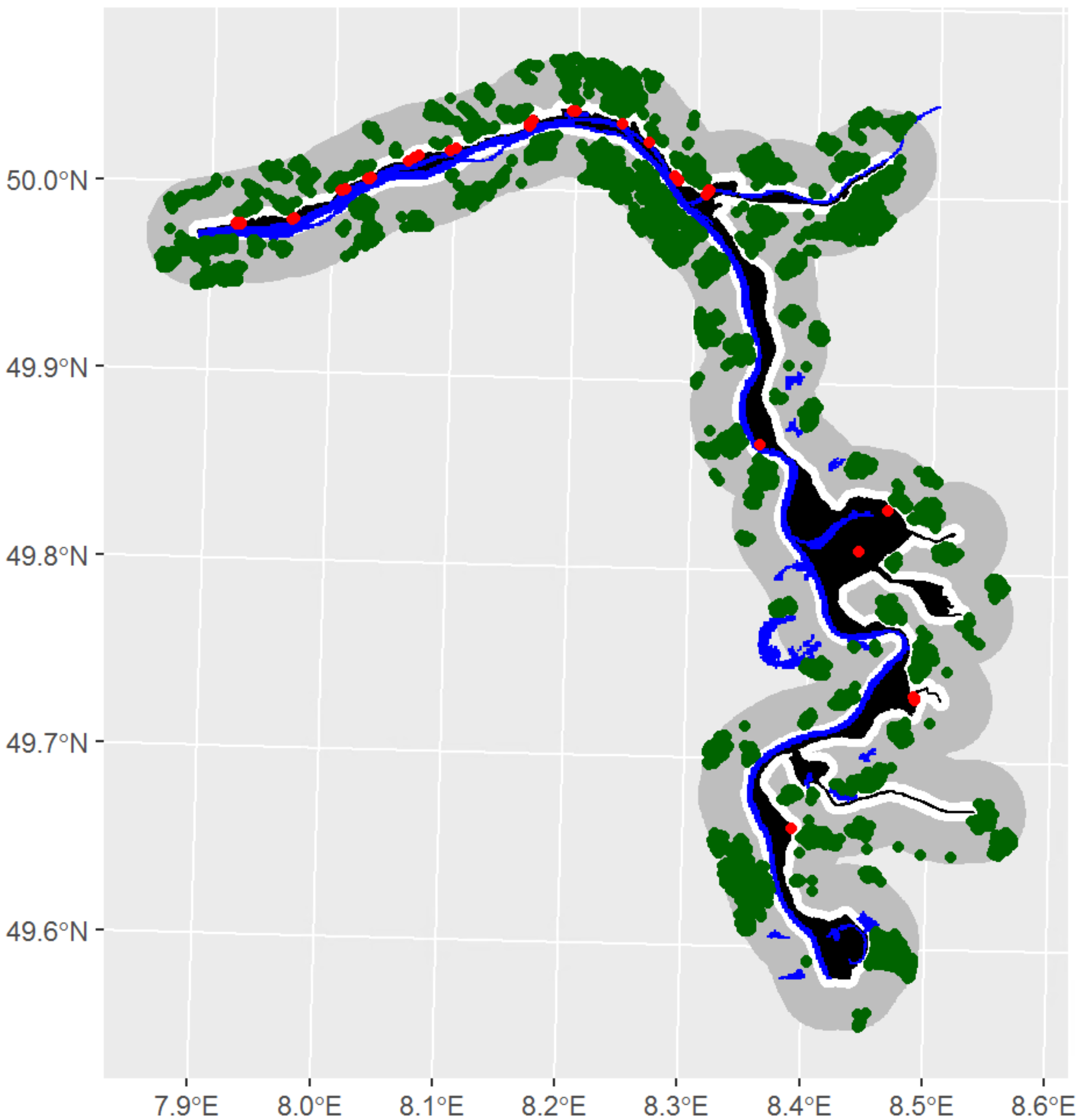
To combine the home prices data with the flood risk data, we merge the two datasets based on the geolocation of the properties and the flood risk zones. We then determine where each property is located with respect to a flood risk area. We focus on the treatment and control groups' adequate definition. For our main specification, we take houses that are located inside flood risk zones as the treatment group. To focus on the indirect impact of the flood event, we exclude areas that were directly impacted by the flood. To that end, we exclude counties that recorded casualties due to the flood. When estimating the effect in turn on only directly affected regions, we also apply a second definition that excludes regions that were inundated (see Figure A.3 for an illustration of these two definitions and the data sources used). The exclusion of directly affected areas ensures that potential effects from our estimations only stem from a change in the risk perception and are not driven by physical damages.

The control group needs to be closely aligned with the treatment group for identification. That is for the underlying theoretical assumption of the identification that the control group develops similar to the treatment group until the event takes place. Therefore, the control group needs to reflect how the treatment group would have developed in the absence of the event. The control group is best suited for this purpose if it is geographically very close to the treatment group. This geographical proximity assures that regional amenities, affecting the local housing prices, play a similar role in both groups. We exploit the proximity of houses to flood risk zones and choose houses with up to 3 kilometers distance to the nearest border of a flood risk zone as the control group in our main specification.

However, the definition of the control group in close proximity to the flood risk areas also poses some potential issues. First, inhabitants might not be aware of the exact delineation of the flood risk zones. Thus, they might feel affected by a change in risk perception, even when not located in the officially defined treatment group. In this case, the control group is affected in the same direction as the treatment group, leading to a potential underestimation of the effect. People buying houses outside but very close to flood risk zones might still have an increased risk perception. Therefore, we draw a 0.5-kilometer buffer around flood risk areas and exclude houses inside that buffer to address the spillover concerns.

Second, there might be spillovers from the treatment group to the control group. If people tend to relocate from the flood risk areas, they might move to the control group areas. Thus, the control group might be positively affected by the event, leading to an overestimation of the effect. We test the robustness of the results addressing those concerns in Section 4.2. Figure 2 shows an exemplary flood risk zone (treatment group) and the corresponding buffer and control group.

Figure 2: Treatment and Control Groups



Notes: This figure depicts a fluvial flood risk zone (black), the corresponding buffer/neutral zone of 0.5 km (white), and the corresponding control group region (gray) extending from 0.5 km to 3 km around the flood risk zone. In blue is the inland waterbody. Red dots are homes inside the black flood risk zone, the green dots are control homes in the 0.5 km to 3 km grey control ring.

Overall, our final data set covers a pooled cross-section of around 1.4 million properties that were put up for sale during January 2014 to May 2022. Table 1 presents summary statistics for the control group (Columns 1 and 2) and the treatment group (Columns 2 and 3) before and after treatment.[8]

Table 1: Descriptive Statistics: Treatment and Control Group

	Outside flood risk area		Inside flood risk area		$\Delta_{Outside-Inside}$	
	Before	After	Before	After	Before	After
Price per m^2						
mean	2051.873	2910.239	1868.409	2727,000	183.464	183.239
sd	(1157.898)	(1443.261)	(1055.989)	(1360.899)		
n	1,087,678	95,984	216,167	18,506		
Construction year						
mean	1974.044	1946.189	1962.558	1938.116	11.486	8.073
sd	(45.270)	(39.979)	(56.302)	(47.877)		
n	854,725	77,338	170,901	15,184		
Number of rooms						
mean	5.439	5.306	5.469	5.313	-0.03	-0.007
sd	(1.650)	(1.631)	(1.728)	(1.725)		
n	1,022,697	89,580	197,532	16,571		
Condition (categorical 1-4)						
mean	2.690	2.692	2.613	2.593	0.077	0.099
sd	(0.625)	(0.593)	(0.657)	(0.641)		
n	535,700	56,343	100,649	10,517		
Cellar (dummy)						
mean	0.343	0.308	0.339	0.326	0.004	-0.018
sd	(0.475)	(0.462)	(0.473)	(0.469)		
n	1,068,941	95,984	212,533	18,506		
Number of floors						
mean	2.128	2.101	2.186	2.159	-0.058	-0.058
sd	(0.708)	(0.699)	(0.742)	(0.763)		
n	555,792	54,474	108,190	10,636		

Houses outside of flood risk zones are, on average, more expensive than houses inside dedicated areas at risk of flooding, according to flood risk maps. The difference between the groups does not seem to change much after the treatment. The differences between the control variables are relatively small. The condition of the houses remains nearly unaffected before and after the treatment in both groups. There is no descriptive evidence that changes in the composition of the treatment group occurred due to the the flood event.

[8] In preparation of the dataset, we disregard extreme outlier observations. More specifically, we exclude the top and bottom 1 percent of the selling price and living space distribution each year and then the top and bottom 1 percent of the price per square meter distribution.

3 Empirical Strategy

We use a difference-in-differences design to estimate the changes in the net prices of houses after the July 2021 flood. The difference-in-differences methodology to estimate the average treatment effect on the treated (ATT) is a two-way fixed effects (TWFE) model of the following form:

$$log\, Y_{i,g,t} = \alpha\, flood\_risk_i \times post_t + \beta\, flood\_risk_i + \gamma\, X_{i,g,t} + \lambda_g + \phi_t + \varepsilon_{i,g,t} \tag{1}$$

where $Y_{i,g,t}$, the price Y of house i located in grid g offered in year t, is regressed on the time-invariant indicator $flood\_risk_i$, a vector of house characteristics ($X_{i,g,t}$), the fixed effects on 1×1km grid level (λ_g) and year-month fixed effects (ϕ_t). The grid fixed effects control for all unobserved grid-specific effects that are the same across time, while the year-month fixed effects control for all unobserved trends across time that are the same across regions. The coefficient β belonging to $flood\_risk_i$ indicates the general effect of being located within a flood risk zone. Note that this coefficient cannot give a marginal impact on the valuation of being located in a flood risk zone. Houses located in flood risk zones typically benefit from natural amenities like the proximity to the respective river. Furthermore, the connectivity might be affected by the proximity to the river. Thus, the coefficient β subsumes all amenities and disamenities correlated with the specific location in a flood risk zone, but it cannot directly be interpreted as a causal estimate.

The key coefficient of our analysis is the coefficient α corresponding to the interaction $flood\_risk_i \times post_t$. As $post_t$ indicates the period after the flood in July 2021, the interaction reflects the changed risk perception in flood risk areas. In contrast to the time-invariant location within a flood risk area, we can derive causal estimates on the changed risk perceptions. There is no reason that the existing amenities (or disamenities) correlating with the higher flood risk have undergone any change right in the time of the flood event (in the areas which have not experienced flooding on their own).[9]

Across specifications, we also show alternatives that control for different trends across counties by either including county by year-month fixed effects or county by year-month linear trends. Additionally, the house characteristics control for the construction year of the house (categorical), the number of floors, the number of rooms, the presence of a cellar, and the conditions of the object (categorical).

The identifying assumption in the canonical difference-in-difference framework is the parallel trends assumption. It assumes that untreated and treated potential outcomes for the treatment and control group follow parallel trends conditional on the specification. The

[9] This underlines the necessity to exclude directly flooded areas in the July 2021 flooding since these areas experienced further changes due to the local destruction.

effect is identified even if shocks affect the potential outcome, as long as it is not correlated with the flood risk zones. Although it is impossible to test this assumption conclusively, it is common practice to perform an *event study* to show that there is no statistically significant effect on property prices between the groups in a period before the flood occurred. Methodologically, the concept can be written as

$$\begin{aligned} log\, Y_{i,g,t} = \sum_{s=\underline{t}}^{t-1} \alpha_s(flood\_risk_i \times Post_{s,j}) + \sum_{s=t}^{\bar{t}} \alpha_s(flood\_risk_i \times Post_{s,j}) \\ + \beta\, flood\_risk_i + \gamma\, X_{i,g,t} + \lambda_g + \phi_t + \varepsilon_{i,g,t}, \quad (2) \end{aligned}$$

where $\underline{t}$ is the first pre-treatment period for which we want to rule out anticipation effects, while $\bar{t}$ represents the last post-treatment period for which we expect adjustment effects. We take α_{-1} as the reference category (setting it to zero). Therefore, the period-specific effects in the first sums term (anticipatory effects) and the second sums term (reactive effects) are interpreted relative to the period $t-1$.

The examination of event study estimates (Figure 3) provides evidence that there are no significantly different pre-trends, making it more likely that the parallel trends assumption holds.

4 Results

The results section is structured as follows. After presenting the results in the difference-in-differences and the event study setup, we take a closer look at possible heterogeneities of the effect. Finally, we run an extensive set of further analyses to test the validity of our results.

Table 2 presents our main results. We run our model in four specifications by translating the presented identification strategy into a testable model. Column 1 does not control for any property characteristics of the offered houses. Although this is not our preferred model, it may hinge on a potential effect of compositional changes of the offered houses before and after the event. While the property characteristics are added in column 2, column three additionally controls for county-specific year-month fixed effects. This gives the model additional flexibility to control for time-specific effects at the county level. Such high flexibility for counties' temporal development is associated with the drawback of a large range of additional fixed effects added to the model. In column 4, this temporal flexibility on the county level is parameterized in county-specific linear time trends, reducing the temporal flexibility but still allowing for county developments without taking as much variation out of the model.

Nevertheless, the flood had no statistically significant effect across all specifications. Therefore, we find no evidence that the July 2021 flood event led to changes in house prices in flood-risk areas that are located in regions that were not directly affected by the flood. The risk perceptions seem not to be updated by the flooding event.

Table 2: Main Results

	Dependent variable: log (price per m^2)			
	(1)	(2)	(3)	(4)
Treatment effect	-0.0107	-0.0031	0.0030	0.0034
	(0.0099)	(0.0085)	(0.0078)	(0.0079)
Property characteristics	no	yes	yes	yes
Grid FE	yes	yes	yes	yes
Year-Month FE	yes	yes	no	yes
County×year-month FE	no	no	yes	no
County×year-month linear trends	no	no	no	yes
Number of observations	1,407,548	1,407,548	1,406,662	1,407,548

Notes: Standard errors clustered at zip code level. Standard errors in parentheses. In specification 3 we control for continuous construction year and a dummy indicating missing construction year information instead of the categorical control since some categories were too sparse with the restrictive set of fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

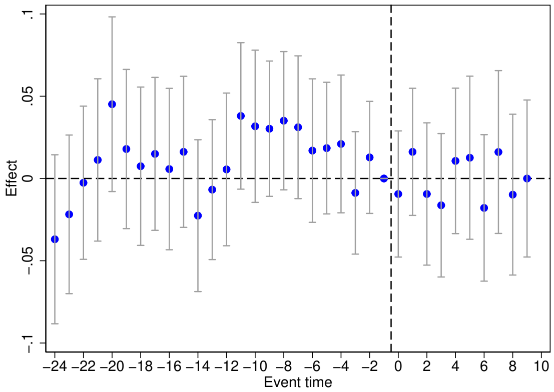
Figure 3 presents the event study estimates. The event studies have clear advantages regarding the potential recognition of violated parallel trends in the pre-treatment period and the detection of temporal developments of the treatment effect over time. As treatment effects are estimated individually for every month, the number of observations forming each treatment effect decreases substantially compared to the difference-in-differences approach. Consequently, individual monthly treatment effects might form outliers, not in line with the expected effects.

Regarding the four different specifications in panel a) to panel d) refer to the same specifications exploited in Table 2. The estimates for the pre-treatment periods are binned after 24 months. Regarding the treatment effects, the results illustrate the same picture as the difference-in-differences model. The reactive period-specific effects after the treatment also show no pattern. The flooding event did not lead to any change in the risk perception.

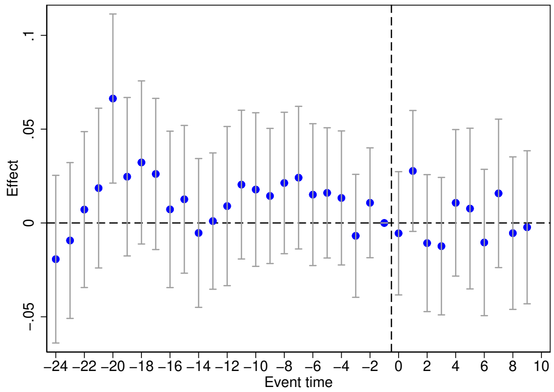
Besides, the event-study setup allows for a deeper inspection of potential misspecifications. A concern against any type of difference-in-differences analysis is the different development between treatment and control groups, even in the absence of treatment. Though this is not directly testable, the pre-trends do not show significant differences between the groups.

Figure 3: Event Study Results

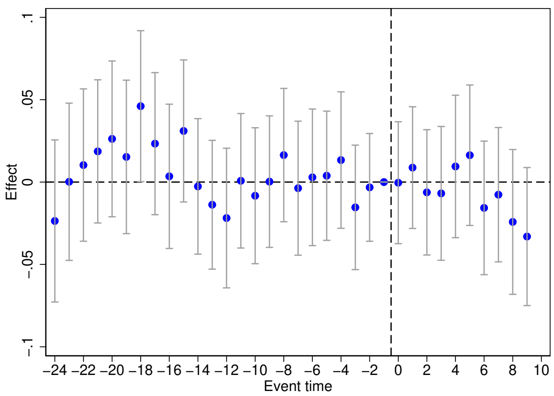
(a) Specification 1



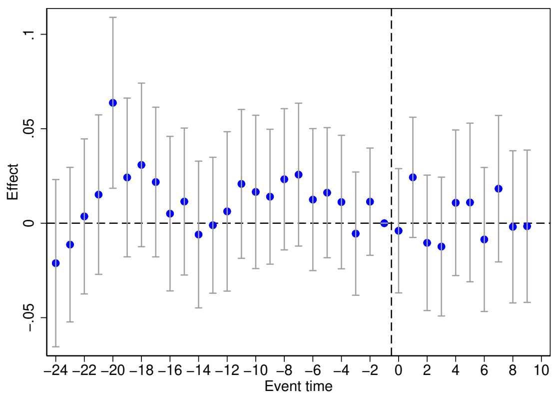
(b) Specification 2



(c) Specification 3



(d) Specification 4



4.1 Heterogeneities

Risk level

Next, we investigate whether the effect differs depending on the level of flood risk. One concern might be that people have too little information about flood risk and flood risk maps. Therefore, we exploit the information from the German Federal Institute of Hydrology which divides flood risk into "high", "medium" and "low" since low-risk areas might be less aware of flood risk. Table 3 presents the results when we restrict our treatment group to the respective risk level. We do not find heterogeneous treatment effects across risk levels and estimate null effects for all levels. Even the high-risk regions do not seem to update their risk premia after the event. Therefore, too little information about being in a flood risk zone does not seem to be the main driver of the null result.

To further investigate the concern that people are not aware of the risk level in flood risk zones we look at whether the risk level is capitalized when controlling for house characteristics and our sets of fixed effects. Table A.1 in the Appendix presents the results of this analysis. We consistently find statistically significant negative effects of a house being located in a flood risk area on prices per square meter ranging from 5 percent to 14 percent. We caution against a causal interpretation of the point estimates as we cannot control for all characteristics which affect property prices and are correlated with flood risk zones, however, it provides suggestive evidence that people are indeed aware of flood risk to a certain extent and it reflects in housing prices. Furthermore, we show that all levels of risk are statistically significantly associated with lower prices indicating that all types of flood risk are known by the market (Table A.2. This also corroborates our choice that we use all levels of risk together as treatment group indicator and not just for example high risk areas.

Table 3: Heterogeneity: Risk level

	Dependent variable: log (price per m²)		
	Low risk	Medium risk	High risk
Treatment effect	0.0013	-0.0001	0.0240
	(0.0102)	(0.0115)	(0.0215)
Property characteristics	yes	yes	yes
Grid FE	yes	yes	yes
County×year-month FE	yes	yes	yes
Number of observations	1,283,439	1,265,485	1,204,418

Notes: Standard errors clustered at zipcode level. Standard errors in parentheses. We control for continuous construction year and a dummy indicating missing construction year information instead of the categorical control since some categories were too sparse with the restrictive set of fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Climate change belief

Following Baldauf *et al.* (2020) and Bakkensen and Barrage (2022) we explore whether there are heterogeneous effects based on beliefs about climate change risks. Baldauf *et al.* (2020) find that in the US, houses projected to be underwater in areas where people believe in climate change sell at a discount compared to denier areas. As we do not have any direct measure on the local intensity of climate change belief, we have to proxy this information. We use the *Sinus-Milieus* designed by the *Sinus Institute* which are provided by *microm Geomarketing GmbH* on the 1km×1km level and easily links to the RWI-GEO-GRID data. The Sinus-Milieu describes the social background of grids in two dimensions: the social status (lower, middle upper class) and the basic values (traditional, modernization and re-orientation). Within these two dimensions, ten groups are formed ranging from a "traditional milieu" to a "neo-ecological milieu". Based on the prevalence of these ten groups within a grid, we classify every grid into a higher likeliness to be belong to the group of climate change believer or denier. To explore whether this could translate into a possible heterogeneity for Germany and for updating risk, we perform a median split of the share of people who believe in climate change. Table 4 depicts the results for the climate change believe heterogeneity. We do not find a significant effect on the outcome variable in both subsamples. This indicates that climate change beliefs do not seem to affect house price updates in Germany. A likely explanation is that in Germany, compared to the US, there is less heterogeneity in the belief in long-run climate change risks.

Table 4: Heterogeneity: Climate change believers

	Dependent variable: log (price per m^2)	
	Low belief	High believe
Treatment effect	-0.0076	0.0104
	(0.0114)	(0.0101)
Property characteristics	yes	yes
Grid FE	yes	yes
County×year-month FE	yes	yes
Number of observations	654,850	744,027

Notes: Standard errors clustered at zipcode level. Standard errors in parentheses. We control for continuous construction year and a dummy indicating missing construction year information instead of the categorical control since some categories were too sparse with the restrictive set of fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

4.2 Validity checks

Spillovers

Another crucial assumption to ensure our method estimates the true effect is the stable unit treatment value (SUTVA) assumption (Imbens and Rubin, 2015). We assume there is no spillover between the treatment and control group due to the treatment. That is, we expect house prices in the control group not to change due to the flood. Since people might not see the borders of the flood risk zones sharply, we addressed the spillover concern in our main specification by including a 500-meter buffer around the flood risk zone. To investigate whether the choice of the size of the buffer may have impacted the results and to rule out spatial spillovers even more convincingly, we increase the buffer size to 1km. Table 5 presents the results with the increased buffer size. The estimated coefficients stay statistically insignificant.

Placebo test - Directly affected regions

A conventional placebo test investigates whether a statistically significant result was found by chance or due to suboptimal data or method. It estimates the same model in a setup where there should be no effect by definition. If the test yields a result, the finding is not robust. In our case, this logic is turned around. We want to investigate a null result and prove that it is not found by chance or due to suboptimal data or method. Therefore, we estimate our model in a case where we expect an effect by definition. If we do not find an effect, our data or method might not be suited to detect the impact.

Therefore, we restrict our sample to the regions directly affected by the flood the most.

Table 5: Extended buffer: 1 kilometer

	Dependent variable: log (price per m^2)		
	(1)	(2)	(3)
Treatment effect	-0.0144	-0.0054	-0.0033
	(0.0104)	(0.0091)	(0.0087)
Property characteristics	no	yes	yes
Grid FE	yes	yes	yes
Year-Month FE	yes	yes	no
County×year-month FE	no	no	yes
Number of observations	1,008,885	1,008,885	1,007,394

Notes: Standard errors clustered at zipcode level. Standard errors in parentheses. In specification 3 we control for continuous construction year and a dummy indicating missing construction year information instead of the categorical control since some categories were too sparse with the restrictive set of fixed effects. Specification 4: variance matrix is nonsymmetric or highly singular * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

By definition, these regions are not in the focus of our original research question on indirect effects as they were affected directly. Damages to houses and surrounding factors such as infrastructure should lead to a negative effect on house prices (Beltrán *et al.*, 2019; Atreya *et al.*, 2013; Atreya and Ferreira, 2015; Bin and Polasky, 2004; Bin and Landry, 2013; Daniel *et al.*, 2009; Kousky, 2010). Furthermore, due to the direct flood experience, the literature suggests that risk perception in these regions will respond strongly, leading to negative effects. Therefore, we restrict our sample to the counties most affected by the flood. We use two alternative ways to categorize "most affected" regions. First, we identify counties with at least one casualty caused by the flood. Second, we identify areas that were inundated due to the flood.[10] Figure A.3 displays the geographic coverage of the two alternatives.

Table 6 presents the results for both alternatives. We find that the flood negatively affected house prices. The estimated coefficients are statistically significantly different from zero at the 1 and 5 percent level, respectively. The point estimates suggest an effect of around 6.6 percent when using casualties to identify the most affected regions and around 3.9 percent in inundated areas. We do not include county-by-time fixed effects or county trends since the most affected regions are relatively homogeneous and located in specific parts of Germany.

Placebo test in time

Next, in a similar spirit to test our data and model, we perform a placebo test in time to prove that our setup does not produce results by chance when looking at a hypothetical treatment that should not have an effect by definition since no event occurred. To that end, we use a hypothetical flood three years before the July 2021 flood. In other words, the hypothetical treatment timing in this placebo example is July 2018. We remove observa-

[10] Please see Notes of Figure A.3 for identification of affected regions and data sources used.

Table 6: Placebo Results: Most affected regions

	Dependent variable: log (price per m^2)	
	Most affected counties (deaths)	Most affected counties (inundated)
Treatment effect	-0.0659***	-0.0394**
	(0.0224)	(0.0189)
Property characteristics	yes	yes
Grid FE	yes	yes
Year-Month FE	yes	yes
Number of observations	145,594	295,393

Notes: Standard errors clustered at zipcode level. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

tions from July 2021 onward to produce a clean and independent placebo result. Table 7 depicts the results. As expected, the treatment effect remains insignificant, indicating that the hypothetical flooding event did not affect house prices in flood-risk areas.

Table 7: Placebo results: Variation in treatment timing

	Dependent variable: log (price per m^2)		
	(1)	(2)	(3)
Treatment effect	-0.0105	0.0025	0.0094
	(0.0067)	(0.0058)	(0.0057)
Property characteristics	no	yes	yes
Grid FE	yes	yes	yes
Year-Month FE	yes	yes	no
County×year-month FE	no	no	yes
Number of observations	1,293,117	1,293,117	1,292,298

Notes: Hypothetical treatment: July 2018. We exclude actually treated observations (all observations from July 2021 onward). Standard errors clustered at the zip code level. Standard errors in parentheses. In specification 3 we control for continuous construction year and a dummy indicating missing construction year information instead of the categorical control since some categories were too sparse with the restrictive set of fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Different samples

The null result might also be the due to the chosen comparison group. Therefore, we show three alternatives: a) a sample with a control group looking at the distance to the nearest treated house instead of the border of a flood risk zone, b) one which does not restrict the control group in any way except for dropping, as in any specification, coastal flood risk and directly affected regions and c) a sample where the control group consists of houses that are in three-kilometer distance to water bodies to proxy for the amenity value of treated houses. Table 8 displays the results for those alternative sample definitions. Across all samples, we find qualitatively similar results to our main sample. The flood has no effect on house prices that is statistically significantly different from zero.

Table 8: Different Samples

	Dependent variable: log (price per m^2)		
	Nearest Neighbour	Full Sample	Near Waterbodies (3km)
Treatment effect	0.0008	-0.0004	-0.0000
	(0.0081)	(0.0078)	(0.0092)
Property characteristics	yes	yes	yes
Grid FE	yes	yes	yes
County×year-month FE	yes	yes	yes
Number of observations	1,449,576	3,587,183	1,010,516

Notes: Standard errors clustered at zipcode level. Standard errors in parentheses. We control for continuous construction year and a dummy indicating missing construction year information instead of the categorical control since some categories were too sparse with the restrictive set of fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Different market responses

Lastly, we investigate whether the flood had an alternative impact on the housing market, which might mask an effect on house prices. The offering prices, which we exploit in our model may not directly show lower willingness to pay of the buyers as the only reflect the asking price of the sellers. Thus we switch to an alternative measure using the time on market of houses. If houses in flood risk areas loose attractiveness which does not translate into prices, consequently the time on market should increase. Therefore, we estimate the effect of the flood on the natural logarithm of the days that an offer is online. We take the main sample and specification but additionally control for the natural logarithms of size in square meters and offering price of a house. We find that the flood had no statistically significant impact on the number of days that house offers stay online (Table 9). This indicates that the market did not respond in any other way which might mask an effect on risk premiums.

5 Discussion

Our first possible explanation for our null result is that indirect experience is insufficient to trigger higher risk perception. As many studies have shown, direct experience is one of the most influential predictors for risk perception (e.g. Bubeck *et al.*, 2012; Bustillos Ardaya *et al.*, 2017; Frondel *et al.*, 2017; Ge *et al.*, 2021; Grothmann and Reusswig, 2006; Kellens *et al.*, 2011; Lindell and Hwang, 2008; Lujala *et al.*, 2015; Miceli *et al.*, 2008; O'Neill *et al.*, 2016; Qasim *et al.*, 2015; Siegrist and Gutscher, 2006; Wachinger *et al.*, 2013; Zaalberg *et al.*, 2009). Since this influential predictor is missing for the people in our study, the risk

Table 9: Online duration of house offer

	Dependent variable: log (days)		
	(1)	(2)	(3)
Treatment effect	-0.0003	-0.0038	-0.0051
	(0.0227)	(0.0210)	(0.0225)
Property characteristics	no	yes	yes
Grid FE	yes	yes	yes
Year-Month FE	yes	yes	no
County×year-month FE	no	no	yes
County×year-month linear trends	no	no	no
Number of observations	1,407,548	1,407,548	1,406,662

Notes: Standard errors clustered at zipcode level. Standard errors in parentheses. We control for continuous construction year and a dummy indicating missing construction year information instead of the categorical control since some categories were too sparse with the restrictive set of fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

perception of people that only have indirectly experienced the flood might not increase and willingness-to-pay does not change.

In addition, we believe it is essential to differentiate between personal and social risk perception. Personal risk perception describes the individual's belief that risk affects them personally, while social risk perception characterizes the risk associated with a group or society (Liu *et al.*, 2021). According to the "impersonal impact hypothesis" (Tyler and Cook, 1984), media can only influence social risk perception but not alter the perceived personal risk. Since we believe that personal risk perception is key to changing individual behavior, the media's impact on social risk perception might not be sufficient to alter an individual's willingness-to-pay. Several studies support the impersonal impact hypothesis (Brenkert-Smith *et al.*, 2013; Liu *et al.*, 2021; Wahlberg and Sjoberg, 2000; Young *et al.*, 2013) while others find evidence for the impact of media on social risk perception (Frewer *et al.*, 2002; Ng *et al.*, 2018). However, most studies investigating media's impact on risk perception do not distinguish between social and personal risk perception. Therefore, further research is needed to assess this explanation of our null result.

A second explanation of our findings may be that risk perception does not translate into action. The literature on the relationship between risk perception and taking action (e.g., taking mitigation measures, buying insurance, adjusting willingness-to-pay) is ambiguous. Some studies suggest that no or only a weak relationship exists (Bubeck *et al.*, 2012; Roder *et al.*, 2019), while others report that people act upon their risk perception (Atreya *et al.*, 2013; Deng *et al.*, 2015; Ge *et al.*, 2021; Martin *et al.*, 2009; Ruin *et al.*, 2007; Zhang *et al.*, 2010). Direct experience is one of many factors that can prompt people to act upon their risk perception (Bin and Polasky, 2004; Bubeck *et al.*, 2012; Deng *et al.*, 2015; Ge *et al.*, 2021; Wachinger *et al.*, 2013). Since we focus at regions that were not directly affected by the flood, the direct experience nudge might be missing when translating risk perception into lower housing prices. Furthermore, Wachinger *et al.* (2013) explain that individuals may not realize agency since they assume that the authorities will protect them, or that individuals may not have the abilities or resources to act upon their perceived risk.

The third possible explanation describes that a perfectly rational evaluation of risk is already capitalized in house prices in Germany. Therefore, no adjustment in housing prices after the flood could be observed since the prices reflect the rational valuation of flood risk. However, we do not believe that this explanation holds in our case since literature from the US context consistently finds that people do not rationally value flood risk (Bakkensen and Barrage, 2022; Beltrán *et al.*, 2018; Hino and Burke, 2021; Muller and Hopkins, 2019). Instead, an undervaluation of flood risk explains the – rationally speaking – overvalued housing prices in flood risk zones. However, further research in the German context is needed to fully rule out this possible explanation.

6 Conclusion

The likelihood and severity of flash floods are expected to increase due to climate change. It is well documented in the literature that floods result in lower house prices in inundated regions due to damages and updated flood risk perceptions in the population. However, what is less clear is whether a flood event will result in risk perception updating in regions that did not directly experience the flood but were only indirectly exposed (e.g., through media).

We exploit the salient flooding event in July 2021 as an exogenous variation to determine risk perception updating in the German real estate market. To identify the causal effect, we apply a spatial difference-in-differences approach which allows us to compare houses in flood-risk areas with comparable houses outside these zones before and after the flood. Across various specifications and samples, we find no evidence that the event changed house prices in flood-risk areas in not directly affected regions. Moreover, we find that house prices in directly affected regions decreased, likely due to the direct damages and risk perception updating after direct exposure to the flood.

Our findings suggest that people do not update their risk perceptions if they were only indirectly exposed to a flood event. Government action may thus be warranted to, for example, provide protective measures, discourage development in flood-risk areas or ensure adequate protection of individuals. In the short term, given that flash floods are expected to increase, state authorities should have flood emergency protocols and early warning systems in place to protect lives. In the longer term, land-use and zoning policies may discourage development in flood-risk areas. This may include increasing the natural space along riverbanks and flood plains. Moreover, municipalities may take adaptive measures such as increasing permeable surfaces or building-scale rainwater tanks. This policy option may require large-scale investments. Furthermore, the government may support and/or require property owners in flood-prone areas to take preventive measures. Last but not least, the option of protection is to obligate people living in flood risk zones to purchase flood insurance. While this policy option shifts the costs of protection towards housing owners, it can only ensure financial damage protection. It might incentivize appropriate development in these regions but not prevent damage in the first place.

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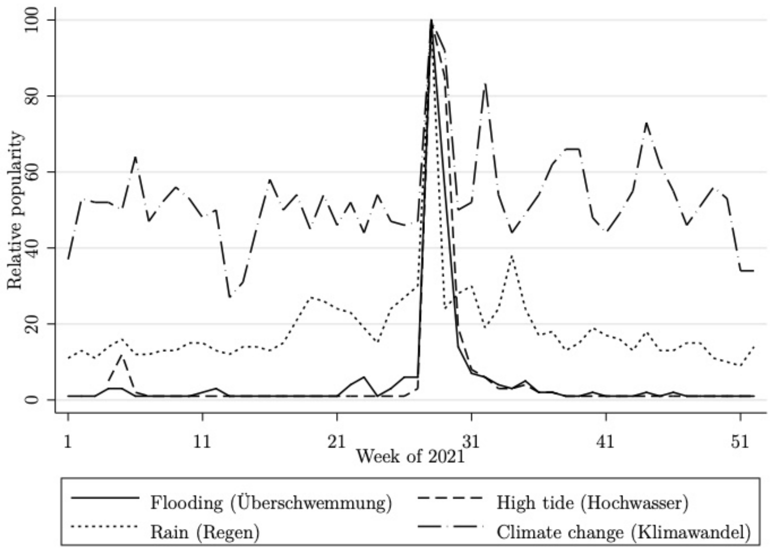
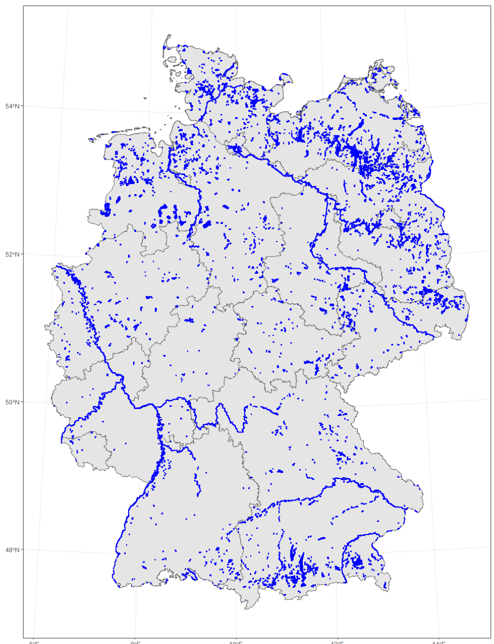
Appendix A: Descriptives

Figure A.1: Google trends for Germany in 2021



Notes: This figure depicts different Google trends search terms over the year 2021. All of the search terms spike in the week of the flooding.

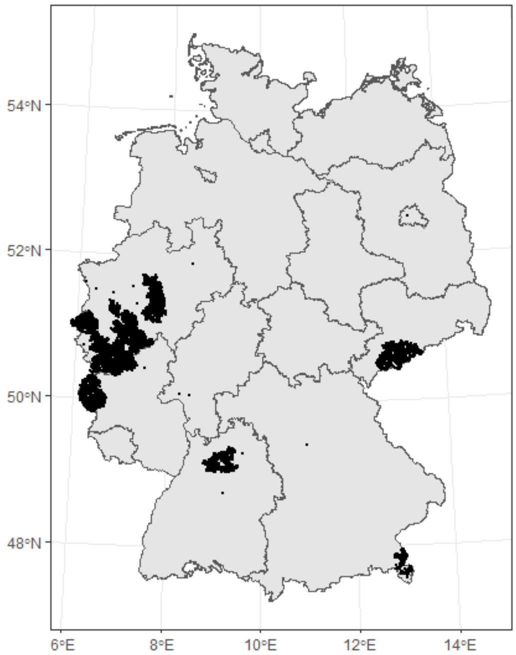
Figure A.2: Water bodies in Germany



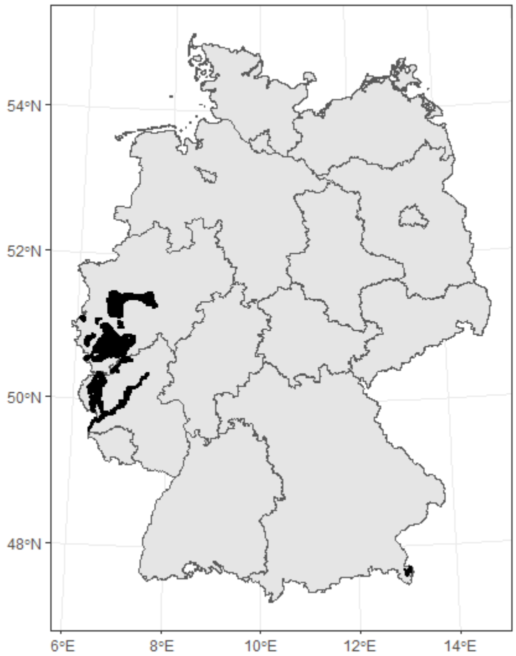
Notes: This figure depicts the water bodies in Germany. The data is provided by Copernicus CLC 2012.

Figure A.3: Definition most affected regions

(a) Definition 1: Deaths



(b) Definition 2: Inundated



Notes: Panel A displays districts in which there were casualties due to the flood. Districts with casualties are identified based on newspaper articles mentioning deaths related to the flood and collected on Wikipedia. Panel B displays the areas which were inundated as a result of the July 2021 flood. The data is provided by COPERNICUS EMSR517: Flood in Western Germany.

Appendix B: Valuation

Table A.1: Valuation

	Dependent variable: log (price per m^2)					
	(1)	(2)	(3)	(4)	(5)	(6)
Flood risk	-0.1416***	-0.0941***	-0.0721***	-0.0939***	-0.0498***	-0.0597***
	(0.0176)	(0.0145)	(0.0139)	(0.0144)	(0.0057)	(0.0181)
Property characteristics	no	yes	yes	yes	yes	yes
Grid FE	yes	yes	yes	yes	no	yes
Zip code FE	no	no	no	no	yes	no
Year-Month FE	yes	yes	no	yes	yes	no
County×year-month FE	no	no	yes	no	no	no
Zip code×year-month FE	no	no	no	no	no	yes
County×year-month linear trends	no	no	no	yes	no	no
Number of observations	1,407,548	1,407,548	1,406,662	1,407,548	1,416,366	1,300,194

Notes: Standard errors clustered at the zip code level. Standard errors in parentheses. In Columns 3 and 6, we control for continuous construction year and a dummy indicating missing construction year information instead of the categorical control since some categories were too sparse with the restrictive set of fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.2: Valuation by flood risk level

	Dependent variable: log (price per m^2)			
	(1)	(2)	(3)	(4)
Low flood risk	-0.1475***	-0.0984***	-0.0761***	-0.0981***
	(0.0173)	(0.0144)	(0.0138)	(0.0143)
Medium flood risk	-0.1401***	-0.0921***	-0.0705***	-0.0919***
	(0.0187)	(0.0153)	(0.0147)	(0.0151)
High flood risk	-0.1171***	-0.0787***	-0.0570***	-0.0792***
	(0.0243)	(0.0194)	(0.0183)	(0.0194)
Property characteristics	no	yes	yes	yes
Grid FE	yes	yes	yes	yes
Year-Month FE	yes	yes	no	yes
County×year-month FE	no	no	yes	no
County×year-month linear trends	no	no	no	yes
Number of observations	1,407,548	1,407,548	1,406,662	1,407,548

Notes: Standard errors clustered at the zip code level. Standard errors in parentheses. In Column 3, we control for continuous construction year and a dummy indicating missing construction year information instead of the categorical control since some categories were too sparse with the restrictive set of fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$