When Water Rises, Do Prices Fall?

A Difference-in-Differences Study on the 2021 Gävleborg Flood

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Abstract

This thesis explores whether the record 18 August 2021 cloudburst in Gävleborg affected the prices of homes situated on the flooded streets. I merge 10 888 residential transactions from the Booli database with address-level Depth × Rain scores to identify highly exposed dwellings and employ a Difference-in-Differences framework, complemented by a weekly event-study spanning two years around the storm. Controlling for fixed housing attributes and calendar-week trends, the preferred specification indicates a price discount of about 1 350 to 1 475 SEK/m^2, which corresponds to a decline of 6–7 % for highly exposed properties relative to very-low-risk comparators. The largest negative point estimates appear for ground-floor apartments and for sales concluded within the first post-event year, hinting at behavioural salience that may fade over time. Although Sweden's standard home-insurance cover is still broadly available, recent reporting suggests that both insurers and lenders are beginning to reassess climate-exposed collateral, which could amplify market responses in the future. Taken together, the evidence offers cautious support for the notion that pluvial-flood risk is capitalised in Swedish housing prices, but further research with additional events and longer horizons is needed before firm conclusions can be drawn.

Keywords: pluvial flooding, housing prices, difference-in-differences, event-study, climate risk, flood insurance, Sweden, Gävleborg cloudburst

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Abbreviations

DiD	Difference-in-Differences
MSB	the Swedish Civil Contingencies Agency
SMHI	the Swedish Meteorological and Hydrological Institute
SUTVA	Stable Unit Treatment Value Assumption

1. Introduction

Between 17 and 18 August 2021, 161 mm of rain fell on Gävle in barely six hours (County Administrative Board of Gävleborg County, 2022). The downpour swamped basements, paralysed rail traffic and generated 6 830 insurance claims that cost about 1.85 billion SEK, making it Sweden's largest weather loss since Storm Gudrun (Swedish Financial Supervisory Authority, 2023). As climate change lengthens atmospheric-river seasons over Scandinavia, such cloudbursts are projected to become both more frequent and more intense. This raises the question of how quickly real-estate markets adjust to the emerging risk.

This thesis investigates a simple, policy-relevant question: *did the August 2021 Gävleborg flood depress the sale price of exposed private owned dwellings and, if so, by how much and for how long?* A difference-in-differences design compares nearly 11 000 residential transactions inside and outside the inundated area between 2016 and 2025. The analysis combines Booli micro-data with high-resolution Depth × Rain scores supplied by Valueguard.

The existing literature in the field, such as Bin and Landry (2013), shows that making flood risk salient (through hazard maps, insurance reform, or actual flood events) can reduce local house prices by 4–13%. Votsis and Perrels (2016) examine the release of coastal flood maps in Finland and detected a localised 7–8% discount that gradually faded as households updated expectations. Evidence for Sweden remains scarce and methodologically mixed, so it is still unclear whether buyers here underprice pluvial flood risk when disclosure is not mandatory.

The present study hopes to shed light on this gap by treating the Gävleborg cloudburst as a natural experiment and estimating the causal effect of pluvial flood exposure on housing prices. By matching each transaction to flood score at address level, the approach reduces ecological bias and even permits a floor-by-floor analysis. As Swedish home insurance automatically covers pluvial flooding, any observed price response should mainly reflect behavioural salience and expected hassle costs rather than changes in insurable loss. The core estimate comes from a difference-in-differences analysis, which is then complemented by an event-study that tracks the effect weekly. Together, these approaches indicate that highly exposed dwellings sold for about 6–7% less than the control group. The effect appears within months, is strongest on ground-floor units and seems to weaken after the first year. Further work is needed to confirm the durability and mechanisms of this pattern.

The discussion begins by reconstructing the meteorology, emergency response and economic impact of the flood, then reviews the hedonic and quasi-experimental literature on flood-risk pricing and outlines a conceptual framework that blends Bayesian learning with salience. It proceeds to describe the data and matching procedures, set out the empirical strategy and present the main results with robustness checks. Finally, it considers caveats and policy implications for disclosure and urban drainage and ends with suggestions for future research.

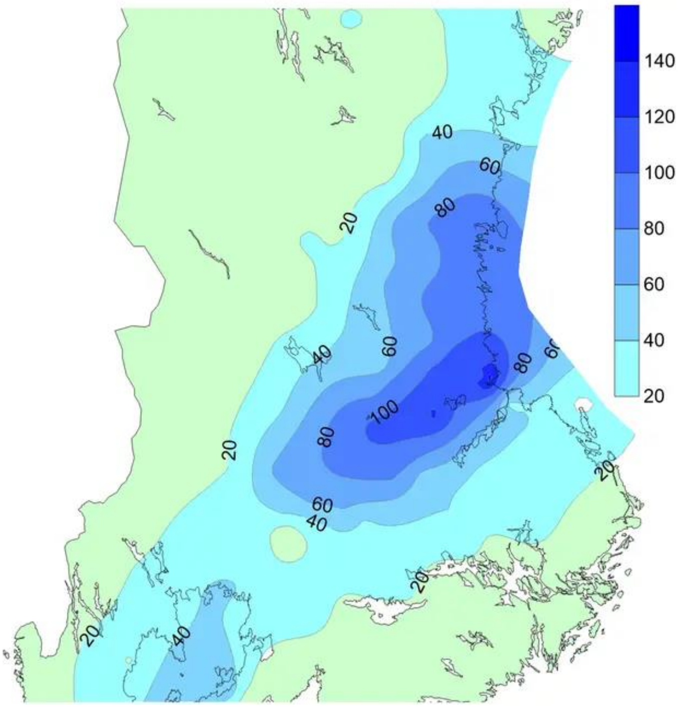
2. Background

2.1 Overview of the 2021 flood in Gävleborg

The event began on Sunday, 15 August 2021, when SMHI issued initial risk warnings for heavy rainfall in Gävleborg County, escalating to a class-2 warning on Tuesday, 17 August. During the night of 17–18 August, an exceptionally intense convective storm produced more than 160 mm of rain over parts of Gävleborg and Dalarna County, with the highest totals centred on the city of Gävle. The downpour was the heaviest ever recorded at the Gävle weather station and exceeded the area's mean two-month precipitation in a single six-hour window. The event resulted in extensive pluvial flooding (surface water flooding caused by heavy rain) especially in Gävle and surrounding municipalities (including Ockelbo, Sandviken, and Hofors), which were among the hardest hit. By the morning of August 18, large sections of Gävle were under water, with streets, basements, and some ground floors of buildings flooded (County Administrative Board of Gävleborg County, 2022).

The flooding was primarily driven by intense rainfall, with 161.6 mm recorded at Gävle weather station within 24 hours, and an extraordinary 101 mm within a two-hour span. This event represented a meteorological extreme, with hydrological analyses estimating its return period at approximately 1-in-1000 years overall, and potentially 1-in-4000 years for the most intense hourly burst (County Administrative Board of Gävleborg County, 2022). Although the Swedish Meteorological and Hydrological Institute (SMHI) had issued a class-2 warning for 'very large rainfall amounts' (> 70 mm), the observed totals far exceeded forecast guidance.

Figure 1: Observed precipitation during the 24-hour period from 17 August at 07:00 to 18 August at 07:00 (Swedish standard time). Source: SMHI (2021)



The severity of flooding was worsened by outdated municipal drainage infrastructure, much of it constructed between the 1950s and 1970s and inadequately sized for events of this magnitude. Existing sewer systems failed, causing sewage backups into basements and overwhelming storm pipes. Previous assessments had identified these vulnerabilities, but planned upgrades were only partially implemented due to funding constraints, highlighting a significant 'adaptation gap' in local infrastructure preparedness (Gästrike Water, 2024).

2.2 Emergency response and consequences

Gästrike Rescue Service received roughly 700 call-outs during the night, the majority relating to flooded basements, stranded vehicles and medical access (County Administrative Board of Gävleborg, 2022). Emergency crews prioritized life safety, and although no fatalities were reported, capacity constraints led to significant delays in attending to numerous private property losses. Key lifelines (Gävle hospital, municipal water supply) remained operational, but arterial roads and sections of the E4 highway closed temporarily. While no formal mass evacuation was declared, many families self-evacuated or were assisted out of flooded areas. It was reported that several households (especially those in ground-floor flats and senior housing) had to be relocated to temporary accommodations while their homes were dried and repaired (County Administrative Board of

Gävleborg, 2022). Some could not return to their residences for up to eight months due to the extent of repairs.

Insurance Sweden (2022a) reported that over 8,000 insurance claims and an estimated total of more than 1.6 billion SEK in damages were recorded following the floods in Gävleborg and Dalarna counties during the summer of 2021. This data was collected from the largest property insurance companies on the Swedish market. The event became Sweden's costliest weather-related disaster since Storm Gudrun in 2005 (Insurance Sweden, 2022b). According to the Swedish Financial Supervisory Authority's 2023 report examining the impacts of the flood event, insurance companies reported total gross insurance payouts (amounts already paid or to be paid to policyholders) amounting to approximately SEK 1.85 billion across 6,830 claims. On average, this equates to about 270,000 SEK per claim. The types of damage reported were mainly waterlogged basements, destroyed HVAC (heating, ventilation, and air conditioning) systems, electrical failures, compromised foundations, and secondary mould growth. Approximately 5% of all detached houses in Gävleborg sustained reportable damage (Swedish Financial Supervisory Authority, 2023). In addition to direct costs, households faced deductibles, costs for temporary accommodation, and non-material hardships, highlighting the financial vulnerability of homeowners to extreme rainfall events.

2.3 Climate change and urban flood risk

The Gävleborg cloudburst is consistent with recent climate-model projections that a warmer atmosphere will generate heavier, short-duration rainfall events across northern Europe (SMHI, n.d.). National climate-model projections confirm the upward trend. According to SMHI (2017), short-duration rainfall (15 minutes to 12 hours) is expected to increase across Sweden by around 10% in 2011–2040 under both medium- (RCP 4.5) and high-emissions (RCP 8.5) scenarios. From 2041 onward, the pathways diverge, with projected increases of approximately 15% (RCP 4.5) and 20% (RCP 8.5), rising further to 20% and 40% by the end of the century. Consequently, intense cloudbursts are expected to become both stronger and more frequent, even in areas not previously designated as high-risk flood zones, underscoring the need for updated urban drainage and planning standards.

National agencies including the Swedish Civil Contingencies Agency (Myndigheten för samhällskydd och beredskap, MSB), the National Board of Housing, Building and Planning (Boverket), and the Swedish Water & Wastewater Association (Svenskt Vatten), recommend that municipalities incorporate cloudburst mapping, larger stormwater pipes, and blue-green infrastructure into both their master plans and detailed local plans (Swedish Civil Contingencies Agency, 2023; National Board of Housing, Building and Planning, 2023; Swedish

Water & Wastewater Association, 2018). Yet implementation proves slow because storm-water responsibility is diffusely split among water utilities, planning departments, road authorities and private developers, with no single actor accountable for funding or delivery (Swedish Water & Wastewater Association, 2018). A recent evidence-synthesis by the Swedish Environmental Protection Agency (Naturvårdsverket) reaches the same conclusion, noting that "storm-water management responsibilities are spread across a variety of agencies and individuals", fuelling conflicts and hampering both the roll-out and the upkeep of nature-based drainage solutions (Swedish Environmental Protection Agency, 2025).

Legal complexity compounds the problem. After the 2021 flood, several insurers pursued regress claims against Gävle Water for alleged sewer negligence; the utility contests liability, citing the extraordinary rainfall (Gästrike Water, 2024; Gefle Dagblad, 2023). From a consumer standpoint, the risks are clear. The Swedish Financial Supervisory Authority reports that Swedish non-life insurers currently face a low risk of insolvency, yet it warns that a warmer climate will likely bring more frequent and severe natural catastrophes. Re-insurers could respond by raising premiums or limiting coverage, and insurers would pass these higher costs on to policyholders. This process could widen Sweden's "insurance gap," making flood protection unaffordable or unavailable for some households and firms. If the credit quality of global re-insurers weakens, direct insurers would need to hold more capital, which would again raise premiums. Very extreme events could also exceed the limits of current re-insurance programmes, an exposure that the Solvency II 200-year capital standard may underestimate (Swedish Financial Supervisory Authority, 2023).

3. Literature review

3.1 Hedonic housing-price theory and quasi-experiments

Empirical research on the pricing of flood risk begins with Rosen (1974) and its first environmental application by Nelson (1978), followed quickly by flood-specific studies such as Damianos and Shabman (1976). In a hedonic price model, the sale price of a dwelling is decomposed into the implicit values of its attributes (Rosen, 1974; Dubin, 1988; Sheppard, 1999). Environmental economists have used this approach to estimate the value of amenities such as clean air (Nelson, 1978; Chay & Greenstone, 2005) and the cost of disamenities such as traffic noise (Nelson, 2008) or proximity to hazardous waste (Kiel & Williams, 2007). It has also been applied to the valuation of school quality (Black, 1999; Fiva & Kirkebøen, 2011).

Cross-sectional studies in the United States find that homes within the Federal Emergency Management Agency's (FEMA's) Special Flood Hazard Areas sell for about 4-9% less than comparable properties outside those zones. For example, Pope (2008) estimates a discount of about 4% after North Carolina introduced mandatory flood-zone disclosure. A meta-analysis of nineteen U.S. studies places the results on a common scale and reports an average −0.6% price change for every 1-percentage-point increase in annual flood-loss probability (Daniel et al., 2009). These estimates can however, be biased if unobserved factors such as construction quality or neighbourhood characteristics are correlated with both flood risk and price (Gibbons & Machin, 2008).

To reduce bias, researchers increasingly combine hedonic models with quasi-experimental designs. Pope (2008) exploits North Carolina's 1996 disclosure law in a boundary difference-in-differences framework, while Bin and Landry (2013) use the 1999 Hurricane Floyd as a natural experiment. By comparing prices before and after the event across flooded and unflooded areas, these studies show that the price discount, small or insignificant beforehand, widened sharply afterwards.

Other event-driven studies reach similar conclusions. Troy and Romm (2004) find that California's 1998 Natural Hazard Disclosure Law immediately reduced the prices of floodplain homes by about 4%. Harrison et al. (2001) analyse nearly 30,000 sales in Florida and document a baseline discount of 4-5% that expanded after the 1994 National Flood Insurance Reform Act raised expected insurance costs. In Sydney, Eves (2002) finds that flood-prone houses sell for about 5% less than comparable dry-land dwellings in typical years; immediately after major Georges River floods the gap widens to 10–20% and then narrows over the following three to five years.

Taken together, these quasi-experiments show that whenever legislation, policy changes, or recent disasters heighten flood risk salience, buyers rapidly adjust housing prices to reflect that exposure. This study tests whether this salience-driven pricing also occurs in Sweden.

A growing body of work demonstrates that forward-looking flood and sea-level-rise (SLR) risk is already reflected in U.S. property and credit markets. Giglio et al. (2021) use ninety-nine-year ground leases and show that the leasehold discount, a proxy for higher long-run discount rates, appears only for properties in low-elevation, flood-prone areas. Bernstein et al. (2019) estimate a discount of roughly 7% for coastal homes expected to be underwater by 2100, whereas Murfin and Spiegel (2020) detect no systematic SLR effect, pointing to market inertia or confidence in future mitigation. Gourevitch et al. (2023) use national flood models and calculate that incomplete flood-risk pricing inflates U.S. housing values by 121 to 237 billion dollars. Consistent with belief heterogeneity, Bakkensen and Barrage (2022) show that coastal prices exceed flood-adjusted fundamentals by about 13% on average in Rhode Island, with even larger gaps where awareness is low. Ouazad and Kahn (2022) find that, in the United States, lenders securitize a disproportionate share of flood-exposed mortgages after major hurricanes, thereby shifting climate risk onto the U.S. government-sponsored enterprises Fannie Mae and Freddie Mac.

3.2 Information-shock studies

Another line of research relies on information shocks rather than physical inundation. In Finland, Votsis and Perrels (2016) treat the staggered release of high-resolution flood-risk maps as a difference-in-differences experiment and find short-run discounts of about 10-13% in Helsinki and Pori and 6-8 % in Rovaniemi for homes newly shown inside the hazard zones. In Germany, Aus dem Moore et al. (2022) exploit the nationwide attention generated by the July 2021 fluvial floods: prices fall only in districts that were inundated, while equally mapped but ultimately spared areas show no change. Gillespie et al. (2025) use Ireland's 2011 Preliminary Flood Risk Assessment maps and estimate a 4% discount for dwellings newly classified at a probability of at least one per cent per year, again highlighting the salience channel.

European 'information-shock' studies suggest that, in the absence of flood maps or recent flood experience, markets tend to under-price flood risk. In Sweden the delineation of Areas with Potentially Significant Flood Risk (APSFRs) has expanded in stages: 18 areas were designated in 2010 – 2015, 25 in 2016 – 2021 (none of which included Gävle) and only in the current third cycle (2022 – 2027) has Gävle been classified as a cloudburst-related APSFR (MSB 2018; MSB 2025). Consequently, the 18 August 2021 flood struck before any formal risk labelling, offering a unique opportunity to test whether housing prices adjust while memories of the event are still fresh.

3.3 Swedish and European context

To the best of my knowledge, empirical evidence on Swedish flood-risk pricing remains scant and is confined to two master's theses rather than peer-reviewed studies. Berggreen-Clausen (2016), analysing transactions around the 2000–01 Lake Vänern flood, finds no lasting price penalty for flood-plain homes, a result the author attributes to Sweden's universal, bundled flood insurance. Fredriksson (2021) exploits newly released SMHI flood-hazard maps in a distance-based difference-in-differences design and shows that owner-occupied coastal properties gain roughly 0.19 % in price for every additional metre of distance from the projected inundation line (0.045 % for stream floods), with the effect disappearing within a year. These muted or short-lived Swedish responses stand in sharp contrast to stronger markdowns elsewhere in Europe. For example, Békés et al. (2016) report about –2 % per 10 % increase in expected flood depth along Hungary's Danube and Tisza rivers, while Skouralis et al. (2024) document an 8 % average discount (up to 32 % in the highest-risk band) for English homes scored as flood-exposed.

3.4 Contribution of this thesis

In contrast to earlier Swedish work that finds muted or short-lived price effects, this master's thesis offers the first transaction-level, event-driven analysis of flood impacts on Swedish property values. It uses a difference-in-differences setup on residential sales data before and after the August 2021 Gävleborg flood. Flooded areas are compared to similar, unflooded neighbourhoods. This study fills two main gaps. First, it addresses the absence of Swedish quasi-experimental research on flood-risk pricing. Second, it provides causal estimates of how an actual flood event changes property values in a market with universal, bundled flood insurance.

4. Conceptual framework

In Rosen’s (1974) hedonic model, a dwelling sell for the sum of its valued attributes. Expected flood losses constitute one such attribute, so the market should discount a risky property by the present value of future damages and higher insurance costs. When a rare event reveals that flood probabilities or damages are higher than previously believed, rational buyers update their priors. Through Bayesian learning the demand curve for exposed properties shifts left, lowering equilibrium prices. Event-study evidence supports this adjustment: Bin and Landry (2013) document a price discount after Hurricane Floyd, and Votsis and Perrels (2016) find similar effects following the release of Finnish flood-risk maps. These results show that new risk information is capitalised quickly in well-functioning markets.

Markets may nevertheless under-price low-probability hazards until a vivid disaster makes the risk salient. After a flood, graphic images dominate memory; the availability heuristic and recency bias cause people to over-weigh the chance of another event (Tversky & Kahneman, 1973). Loss aversion amplifies the response because potential losses loom larger than equivalent gains, while ambiguity aversion pushes prices down further if future rainfall extremes are viewed as uncertain rather than merely risky. Gallagher (2014) records a surge and later decay in U.S. flood-insurance take-up that mirrors this cycle of heightened and then fading salience. Bakkensen and Barrage (2022) show that such heterogeneous beliefs help explain the gap between fundamentals and observed coastal prices. These insights predict an overshoot: prices may fall more than actuarial re-pricing alone would justify and then gradually rebound as memories fade.

Because Sweden’s bundled, universal flood cover prevents large, property-specific premium shocks, buyers are not immediately confronted with higher insurance bills. Prices can still fall, however, for three reasons. Firstly, residual costs – deductibles, uninsured contents, and the "hassle cost" of temporary displacement remain the buyer’s responsibility. Secondly, policy risk – future reforms could raise premiums or introduce risk-based pricing, especially after a record loss year. Lastly, adaption mandates – municipalities may require costly measures such as drainage upgrades, elevation, or waterproofing, which buyers discount today.

Evidence from other universal-insurance settings, e.g. Aus dem Moore et al (2022), shows that even when direct financial exposure is muted, housing markets respond to the inconvenience, uncertainty, and regulatory spill-overs that floods generate.

Taken together, the framework predicts that the 18 August 2021 cloudburst triggered both rational re-pricing (higher expected damages) and behavioural overshooting (salience-driven risk perception), which should lead to lower prices in the affected areas. To test this, I use a difference-in-differences approach to assess whether the average transaction price per square metre of properties classified as high-exposure decreased relative to those in very-low-exposure areas during the 12-month post-event window.

5. Data

5.1 Sources

This study relies on two proprietary, micro-level datasets, acquired under data-use agreements with their respective providers.

5.1.1 Booli residential transactions

Residential transaction data were retrieved via the Booli[1] API for Gävleborg County, covering the period 1 January 2016 to 31 March 2025. The final sample comprises 14 126 transactions, including condominiums, single-family houses, holiday homes, and vacant lots. For each observation, I record: final sale price (SEK), sale date, living area (m²), plot size (m²), construction year, floor level (if apartment), and geocoordinates. A complete list of corresponding municipality codes is provided in Appendix Table A.1.

5.1.2 Valueguard flood-exposure scores

Valueguard AB supplied flood-risk scores for 47 014 dwellings listed in the Swedish mapping, cadastral and land registration authority (Lantmäteriet) dwelling register (FNR property identifiers). For each dwelling i, Valueguard compute:

$$\text{Score}_i = Depth_i \times Rain_i \tag{1}$$

$Depth_i$ is the maximum bluespot inundation depth (in metres), based on the County Administrative Board of Gävleborg's and SMHI's high-resolution flood model. Rain_i is the total radar-recorded precipitation (in millimetres) during the 17-18 August 2021 cloudburst event.

Higher scores imply greater flood exposure under extreme rainfall. Scores range from 0 to 100; see the distribution in Appendix Table A.2.

5.2 Merging and sample construction

I use Python to merge Booli residential transactions with Valueguard flood-exposure scores, resulting in an analytical dataset of 10,888 matched sales. The initial Booli sample comprised 14 126 transactions; unmatched records were excluded due to missing addresses or lack of a nearby flood-score record.

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[1] Booli is, according to themselves, a housing platform with Sweden's largest combined offering of homes for sale, final sale prices, and statistical property valuations.

Matching is performed on street address and postal code. To accommodate minor suffix differences (e.g., Östigårdsvägen 1A vs. 1B), I geocode all Valueguard addresses using the Google Maps Geocoding API and accept the nearest dwelling within a 75 m radius[2]. For each match, I record (a) whether it is an exact address match or a nearby (geocoded) match, and (b) the geospatial distance in metres. When multiple dwellings fall within the buffer, I select the one with the smallest distance. I observe that Booli's provided coordinates sometimes deviate from those returned by the API, which motivates the 75 m threshold.

The dependent variable is price per square metre (SEK/m^2), computed as the transaction price divided by living area. For tenant-owned apartments (bostadsrätter), I also calculate the monthly fee/m² as an additional control variable.

5.3 Data limitations

When it comes to Booli, a minority of brokers occasionally remove listings before the final contract price appears; in such cases Booli records the last observed bid, which may differ from the true sale price.

The Depth $\times R$ain index is derived from Level 1–2 "Blue-Spot" GIS screening, which highlights local terrain depressions and their cloudburst sensitivity (European Environment Agency, 2016). Because the screen assumes uniform rainfall and omits hydrodynamic routing between depressions, it cannot capture flow-connected pathways or back-water effects and therefore tends to underestimate inundation in flat, highly connected catchments (Balstrøm and Crawford, 2018). Sweden's national cloudburst-mapping guidance consequently recommends using the Blue-Spot screen only for hotspot prioritisation; detailed risk or damage assessments should be supplemented with 2-D hydrodynamic modelling or empirical depth–damage functions (Swedish Civil Contingencies Agency, 2023).

5.4 Descriptive statistics

Table 1 and 2 summarises the pre-flood characteristics of the properties in the control and treatment groups. The analysis covers 6,043 control observations and 3,738 treated observations. Mean sale price per square metre is higher in the treated area (SEK 21,506) than in the control area (SEK 18,845), although the dispersion is considerable in both groups (SD ≈ 8,300–8,900). Treated dwellings are, on average, around 16 m² smaller (66.8 m² vs 79.4 m²) and contain roughly half a room fewer (2.48 vs 2.92). The average floor level is virtually identical across groups (≈

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[2] The 75 m buffer is based on empirical geocoding-accuracy studies, which report typical positional errors of 30–60 m and a 75th percentile around 50–75 m (Zandbergen, 2009).

2.3), and the difference in monthly rent per square metre is modest (SEK 56.3 vs 59.1). The raw figures therefore indicate that treated transactions are skewed toward smaller apartments, while other observable attributes are broadly balanced. In the regression analysis that follows, I control for these covariates and include property-type fixed effects to isolate the causal impact of the flood. Section 4.2 demonstrates that the pre-flood price trajectories of the two groups are parallel, alleviating concerns that cross-sectional differences alone drive post-flood estimates.

Table 1: Descriptive statistics for the control group

Variable	N	Mean	SD	Min	Median	Max
sqmPrice	6 043	18 844.92	8 939.60	833	17 946	94 000
livingArea	6 043	79.36	39.85	18	72	630
rooms	6 024	2.92	1.43	1	3	13
floor	3 906	2.22	1.41	-1	2	9
rentSqm	5 011	59.10	12.77	22	58	124

Table 2: Descriptive statistics for the treatment group

Variable	N	Mean	SD	Min	Median	Max
sqmPrice	3 738	21 505.73	8 309.70	930	21 814	75 000
livingArea	3 738	66.80	27.83	17	64	571
rooms	3 742	2.48	1.11	1	2	15
floor	3 228	2.35	1.38	-1	2	15
rentSqm	3 533	56.27	11.30	26	55	127

6. Empirical strategy

6.1 Treatment indicator

The August 18, 2021, cloudburst constitutes an abrupt, plausibly exogenous shock that raised the salience of pluvial-flood risk overnight. The treatment group is defined based on Valueguards flood score (Section 5.1.2). Specifically,

$$T_i = \begin{cases} 1 & if\ Score_i \geq 20, \\ 0 & if\ Score_i = 0 \end{cases} \tag{2}$$

Dwellings with intermediate scores are excluded from the main analysis to maximise separation between high- and very-low-risk observations and to avoid functional-form assumptions.

6.2 Event-time indexing

For dynamic estimation every transaction is re-indexed by the number of complete weeks between its sale date and the flood:

$$\tau_{it} = round\left(\frac{soldDate_{it} - t_0}{7}\right), \quad t_0 = 18\text{ August }2021 \tag{3}$$

To focus the analysis and ensure interpretability, I restrict event time to a symmetric 103-week window around the flood date. Within this window, I define three mutually exclusive sets of indicator variables that enter the event-study regression (Table 3).

Table 3: Definition and role of event-time indicator variables

Indicator	**Definition**	**Role**
$1\{\tau^*_{it} = k\}, = -51 \dots 51$	$weekly\ dummies$	Allows treatment effects to vary flexibly week-by-week
$first\_dummy_{it}$	$1\{\tau_{it} < -51\}$	Collects leads earlier than one year before the flood
$last\_dummy_{it}$	$1\{\tau_{it} < +51\}$	Collects lags later than one year after the flood

Week –1 is omitted and serves as the reference period. A balance check confirms ≥3 transactions in every bin, satisfying the minimum-cell requirement for clustered inference (Bertrand, Duflo & Mullainathan 2004).

6.3 Baseline DiD & event-study

To estimate the main effect of interest, the following equation is estimated in R:

$$\text{sqmPrice}_{id} = \alpha + \beta(\text{treatment}_i \times \text{post}_d) + X_i'\gamma + \mu_{w(d)} + \varepsilon_{id} \qquad (4)$$

In equation (4) i indexes property and d indexes day of the transaction, while $w(d)$ maps each day d to it its calendar week. The dependent variable, $sqmPrice_{id}$ records the price per m^2 of property i on day d. The treatment indicator, $treatment_i$ is the time-invariant indicator (1 $if\ Score_i \geq 20$, 0 otherwise), and the post-period dummy, $post_t = 1\{t \geq 18\ August\ 2021\}$. X_i is the vector of property characteristics (e.g. living area, rooms). $\mu_{w(d)}$ are weekly calendar-time fixed effects (one dummy for each calendar week). The error term, ε_{id} is clustered at the postal-code level.

This specification follows canonical DiD implementations (e.g. Bertrand et al., 2004) and quasi-experimental studies of flood events (Bin & Landry, 2013; Troy & Romm, 2004).

6.4 Event-Study and assumptions

A difference-in-differences design hinges on two core identification assumptions: Parallel Trends and the Stable Unit Treatment Value Assumption (SUTVA). Both are essential to the credibility of my causal estimates, yet each presents different challenges for validation in this study.

In this context, for the DiD design to identify the flood's causal impact, sale prices inside and outside the inundated area must have evolved in parallel prior to 18 August 2021. Because the cloudburst struck on one clearly defined day, I test a single break in the trend rather than staggered treatment dates. I check this by plotting the weekly coefficients in an event-study for the period before the flood; if the line is flat, the parallel-trends assumption looks credible (Freyaldenhoven, Hansen & Shapiro 2019). SUTVA has two parts. (i) No interference: a dwelling's price should depend only on whether it was flooded, not on its neighbours' status (Cox 1958). (ii) No hidden versions of treatment: "flood exposure" must mean the same thing for every treated property (Rubin, 1980). A fuller discussion of these and related threats to identification is provided in Section 8.1.

I implement the following flexible event-study model:

$$
\begin{aligned}
\text{sqmPrice}_{it} = \alpha + \sum_{k=-51,\, k\neq -1}^{51} \beta_k \big(\text{treatment}_i \times 1(\text{event\_time\_bin}_{it} = k)\big) \\
+ \beta_F(\text{treatment}_i \times \text{first\_dummy}_{it}) \\
+ \beta_L(\text{treatment}_i \times \text{last\_dummy}_{it}) + X'_{it} + \varepsilon_{it}
\end{aligned} \tag{5}
$$

Equation (5) includes $1(\text{event\_time\_bin}_{it} = k)$, a set of dummy variables for each event-time bin $k \in \{-51, \ldots, 51\}$, with the values clamped at ±51. The week immediately prior to the event, k = –1, is omitted and serves as the reference category. The variable $\text{first\_dummy}_{it} = 1\{\text{event\_time\_week}_{it} < -51\}$ captures observations that occurred more than 51 weeks before the event, while $\text{last\_dummy}_{it} = 1\{\text{event\_time\_week}_{it} > 51\}$ captures those that occurred more than 51 weeks after. The coefficients β_k represent the event-study estimates for the central bins, whereas β_F and β_L capture the effects of the two extreme bins. The covariate vector $X_{it} = (livingArea_{it}, rooms_{it})'$ includes property-level controls. For apartment sales only, I additionally include controls for floor level and rent/m^2. The ϵ_{it} is clustered at the postal-code level to account for spatial correlation in residuals.

The event-study framework mirrors recent applied work on information-shock studies in flood-risk pricing (Votsis & Perrels, 2016; Aus dem Moore et al., 2022). I cluster the standard errors at the postal-code level to capture spatially correlated shocks, such as neighbourhood amenities, local demand conditions, and systematic geocoding discrepancies, so that statistical inference remains valid even when observations within the same area are not independent.

7. Results

7.1 DiD estimates

This section presents the headline causal estimates of how the 18 August 2021 cloudburst affected residential property prices. Table 4 reports results for the full sample of dwellings (houses, apartments, holiday homes and vacant lots); Table 5 narrows the focus to apartment transactions only. In each table, the left-hand panel defines "treated" dwellings as those whose centroid lies within 75 m of a parcel that recorded floodwater on 18 August, while the right-hand panel tightens the definition to exact address matches. All regressions include event-time-bin fixed effects (103 weekly bins) and cluster standard errors at the postal-code level.

Table 4: Regression results all residential properties: 75 radius vs. exact match

Variable	**75m radius**	**Exact match**
Treatment × Post	–1354.1930**	–1352.9000***
	(410.1800)	(395.7610)
Treatment	2918.6880**	2903.1150**
	(891.7420)	(904.9890)
livingArea	–64.1050***	–75.5590***
	(13.7690)	(16.5360)
rooms	1202.9960**	1423.1540***
	(386.8320)	(372.4590)
Observations	9724	9062
RMSE	8 392.7	8 333.9
Adj. R^2	0.0747	0.0785
Within R^2	0.0395	0.0429

Notes: The dependent variable is the property's sale price in SEK/m^2. Significance levels: * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. Standard errors (cluster-robust at the postal-code level) are in parentheses. All regressions include event-time-bin fixed effects (103 bins).

Table 5: Regression results apartments only: 75 radius vs. exact match

Variable	**75m radius**	**Exact match**
Treatment × Post	–1475.4030**	–1476.3700***
	(472.6503)	(433.6419)
Treatment	2625.4690**	2681.4200**
	(929.2200)	(925.5375)
livingArea	–150.1180***	–157.8400***
	(27.6470)	(29.4715)
rooms	1479.3250**	1636.6520**
	(543.5992)	(547.9581)
floor	696.3390***	720.2170***
	(165.0488)	(162.6647)
rentSqm	–130.9300**	–130.1300**
	(44.5882)	(44.9864)
Observations	6 989	6 690
RMSE	7 941.8	7 875.7
Adj. R^2	0.1386	0.1439
Within R^2	0.1051	0.1094

Notes: The dependent variable is the property's sale price in SEK/m^2. Significance levels: * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. Standard errors (cluster-robust at the postal-code level) are in parentheses. All regressions include event-time-bin fixed effects (103 bins).

7.1.1 All residential properties

The main coefficient of interest, i.e. the coefficient in front of the interaction Treatment × Post is –1 354 SEK/m² (s.e. ≈ 410) in the 75 m specification and –1 353 SEK/m² (s.e. ≈ 396) under exact matching (Table 4). Both estimates are highly significant ($p < 0.01$ for the 75 m radius and $p < 0.001$ for the exact-match specification), implying a price drop of roughly 6,3% relative to the pre-flood mean of 21 505 SEK/m² (for treatment group, see Table 2).

The positive and significant treatment coefficient (≈ 2 900 SEK/m²) indicates that, prior to the flood, homes in the ultimately inundated neighbourhoods commanded higher price-per-square-metre levels than the control group. The DiD design absorbs this level difference, so the post-flood decline should be interpreted as a change relative to each group's own pre-trend rather than a convergence toward the control-group price level.

Consistent with hedonic expectations, a larger living area is associated with a lower price per square metre (–64 SEK per additional square metre), while an extra room raises the unit price by roughly 1 930 SEK/m². These coefficients are stable across both exposure definitions, giving further confidence that model specification is not driving the flood coefficient.

7.1.2 Apartments only

Restricting the sample to condominiums sharpens the analysis by holding property type constant and allowing the inclusion of apartment-specific controls (monthly fee and floor level).

For apartments, the flood penalty is slightly larger in absolute terms:
–1 475 SEK/m² (s.e. ≈ 473) under the 75 m radius and –1 476 SEK m² (s.e. ≈ 434) for exact matches (see Table 5). The exact-match coefficient is significant at the 0.1 % level, whereas the 75 m estimate is significant at the 1 % level, underscoring the added precision from using the stricter exposure definition. Given a pre-flood average apartment price of 21 485 SEK/m², this translates into a roughly 6.9 % decline, very close to the estimate for the full dwelling sample.

As expected, higher-floor units are more expensive (≈ 639 SEK/m² per floor). The monthly fee enters with the anticipated negative sign (–0.130 SEK/m² per SEK of fee), reflecting buyers' capitalisation of recurring costs into purchase prices. These coefficients remain stable across exposure definitions, reaffirming model robustness.

Taken together, the DiD estimates point to a statistically and economically meaningful loss of about 1 350–1 475 SEK/m² (≈ 6–7 %) for properties hit by the 2021 cloudburst. The close agreement between the 75 m and exact-match specifications alleviates concerns that the result hinges on an arbitrary buffer choice. Likewise, the similarity between all-property and apartment-only samples indicates that the penalty is not driven solely by detached houses; even apartments (often perceived as less flood-prone) experience a commensurate markdown.

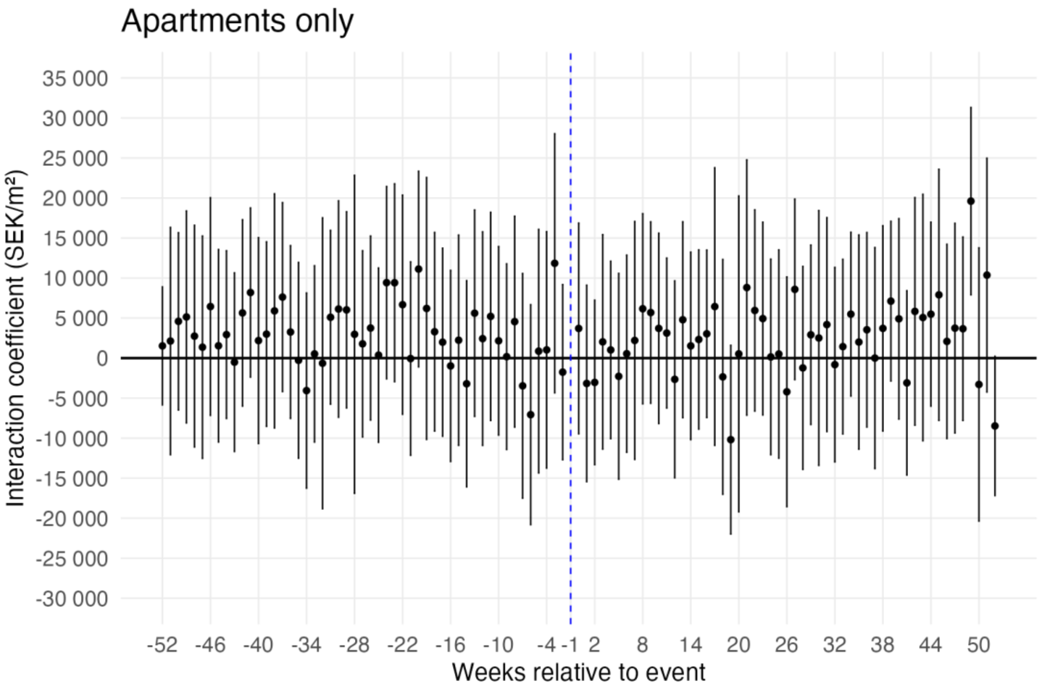
7.2 Event-Study estimates

Figure 2: Event-Study of flood impact on all residential properties

Figure 2 displays the weekly event-time coefficients from Equation (4) together with their 95 % cluster-robust confidence intervals; the coefficient for week –1 is normalised to zero. All coefficients from week –51 to week –2 hover tightly around zero and none is statistically significant, lending visual support to the parallel-trends assumption.

The first two post-event months (weeks 0–8) show small, imprecisely estimated negatives. The only week that reaches conventional significance is week +19**,** when prices are 16 555 SEK m² lower than in the control area (95 % CI: –27 697 to –5 413; $p = 0.004$). From week +20 onwards the point estimates revert to values statistically indistinguishable from zero, indicating that the negative shock is temporary. A plausible mechanism is that severely damaged dwellings entered the market with a lag of a few months, depressing prices briefly until remediation was complete.

Figure 3: Event-Study of flood impact on apartments only



Restricting the sample to apartments yields the same flat pre-trend. No post-event coefficient is significantly negative, but week +49 exhibits a significant positive jump of +19 582 SEK m² (95 % CI: 7 857 to 31 308; $p = 0.001$). A plausible explanation is that, around one year after the flood, some housing associations had completed insurance-financed refurbishments or that a batch of premium apartments happened to be transacted at that time, either of which could have driven the observed price increase.

7.3 Sensitivity & robustness checks

This section tests how robust my main DiD results are. I do this in two ways. For apartments, I split the data by floor level, because floodwater primarily damages the lower storeys. Second, I gradually tighten the treatment definition for both apartments and all residential properties by increasing the cut-off on the Valueguard flood score, which reflects the severity of impact at each address from the August 2021 cloudburst.

Table 6: Sensitivity Analysis – DiD Estimates by flood score, all residential properties, 75 radius vs. exact match

Flood Score	75m radius	Exact match
Cutoff ≥15	–855.241*	–844.551*
	(417.083)	(406.472)
Cutoff ≥20	–1354.1930**	–1352.900***
	(410.1800)	(395.761)
Cutoff ≥80	–1680.932*	–1679.181*
	(653.403)	(680.365)
Cutoff ≥100	–2700.404***	–2785.771***
	(624.501)	(622.669)

Notes: The dependent variable is the property's sale price in SEK/m^2. Significance levels: * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. Standard errors (cluster-robust at the postal-code level) are in parentheses. Control variables are livingArea and rooms. All regressions include event-time-bin fixed effects (103 bins).

Table 6 investigates the stability of the post-flood price effect when the treatment definition is progressively tightened. I re-estimate the baseline DiD model four times, each time increasing the minimum Valueguard flood score that qualifies a property as treated to 15, 20, 80 and 100, respectively. The control group, dwellings with score 0 remains unchanged, and all covariates and event-time-bin fixed effects are held constant.

In every specification the treatment coefficient is negative and statistically significant, demonstrating that the price penalty is not an artefact of any single threshold choice. Moreover, the magnitude of the discount rises monotonically with the cut-off value: properties with a score ≥15 sell, on average, for approximately 845-855 SEK/m^2 less than the control group, whereas those with a score of ≥100 at a discount of roughly 2700-2786 SEK/m^2. This smooth trajectory indicates that market participants interpret the flood score as a continuous measure of expected damage and rationally capitalise higher risk into proportionally larger price reductions, rather than responding to an arbitrary binary classification.

Table 7: Sensitivity Analysis – DiD Estimates by flood score and floor level, apartments only, 75 radius vs. exact match

Check	Group	75m radius	Exact match
Flood score	Cutoff ≥15	–1066.333*	–1054.205*
		(481.694)	(447.414)
Flood score	Cutoff ≥20	–1475.4030**	–1476.3700***
		(472.650)	(433.642)
Flood score	Cutoff ≥80	–2033.204**	–2136.605**
		(701.650)	(684.440)
Flood score	Cutoff ≥100	–2984.418***	–3082.129***
		(740.203)	(710.072)
By floor	Ground floor	–8087.987*	–8128.356*
		(3263.182)	(3180.729)
By floor	1st floor	–1287.732	–1236.168
		(714.264)	(629.728)
By floor	2nd floor	–1813.599*	–1879.993*
		(831.482)	(869.009)
By floor	3rd floor or higher	–1375.136*	–1368.860*
		(562.597)	(622.669)

Notes: The dependent variable is the property's sale price in SEK/m^2. Significance levels: * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. Standard errors (cluster-robust at the postal-code level) are in parentheses. Control variables are livingArea, rooms, rentSqm and floor level. All regressions include event-time-bin fixed effects (103 bins).

Table 7 limits the analysis to apartments and explores two sources of heterogeneity: first, the stability of the post-flood price effect when the treatment definition is progressively tightened (just like Table 6), and second, the apartment's height above ground.

I begin by repeating the threshold exercise from the full-sample analysis, this time within the apartment sector. As the minimum score that qualifies a dwelling as treated increases from 15 to 100, the post-event discount grows in a smooth and orderly fashion. Apartments in buildings that recorded a score of ≥15 sell for about 1054-1066 SEK/m^2 less than comparable controls, while those in buildings with scores of ≥100 trade at a markdown close to 2984-3082 SEK/m^2. The pattern confirms that buyers capitalise the observed degree of damage: the more severely a building was inundated, the larger the price reduction that follows.

The second part of Table 7 partitions the sample by storey to capture the vertical distribution of losses. As expected from the physics of surface flooding, ground-floor units bear the brunt of the shock; their prices fall by 8088-8128 SEK/m^2. The effect diminishes with altitude yet remains economically meaningful: first-floor flats show a modest and statistically weak discount, second-floor units a stronger and significant one at about 1 800 SEK/m^2, and apartments located three or more floors above ground still lose around 1 370 SEK/m^2.

Two conclusions follow. First, the price response mirrors the physical path of the water, which enters at street level and attenuates quickly with height. Second, even units that never came into direct contact with floodwater suffer a measurable penalty, suggesting that purchasers also take indirect costs into account, such as damage to common areas, lift outages, future special assessments and the reputational stigma attached to a flooded building. Taken together, the results indicate that the housing market internalises both the severity of the flood at the building level and the likelihood of water ingress at the unit level, reinforcing the robustness of the main findings.

8. Discussion

8.1 Methodological reflections

A difference-in-differences design was chosen because the 2021 flood provides a natural quasi-experiment: it generated an exogenous shock affecting some properties (the treated group) but not others (the control group), with both groups observed before and after the event. This framework allows me to net out time trends common to both groups and isolate the flood's impact. Unlike cross-sectional hedonic models, DiD helps control for unobserved, time-invariant differences between flooded and non-flooded locations, making causal interpretation more credible. However, this approach relies on key assumptions. The most critical is the parallel trends assumption: absent the flood, housing prices in the treatment and control groups would have followed the same path. I partially address this by visually and statistically checking pre-flood trends (finding no systematic divergence), but ultimately parallel trends cannot be fully tested. If the treated areas were on a different trajectory for other reasons (such as local economic changes or neighborhood development), my estimates could be biased. Another concern is confounding due to unobserved factors that coincide with the flood. For instance, if the flood-damaged areas differ systematically (in building quality or demographic mix) in ways that also affect prices, those differences may violate the DiD identifying assumption. I mitigate this by focusing on apartments (more homogeneous housing stock than single-family homes) and including rich covariates where possible, but unmeasured confounders remain a limitation.

Spatial spillovers are also a potential issue: the flood might influence prices of nearby but not directly flooded properties, blurring the treated/control distinction. Buyers may revise beliefs about risk even for adjacent blocks (an "information shock"), causing secondary price effects. Conversely, flood mitigation efforts or insurance claims might improve neighborhood resilience and offset some damage. My model implicitly assumes such spillovers are minimal or captured in common trends, but this may not hold perfectly. In future work one might explicitly model spatial interactions. More generally, DiD in this context is not randomized and cannot account for all forms of endogeneity. For example, if flood-damaged owners drop out of the market (selling to investors or dropping listings), selection biases could emerge. Still, compared to simple cross-sectional comparisons, the DiD framework is a strong design for this natural experiment. Despite its limitations, I believe it credibly estimates the causal flood effect given the exogenous nature of the storm event.

8.2 Data quality and limitations

My analysis relies on third-party transaction data (from Valueguard and Booli) and flood-risk metrics, which have known constraints. The Valueguard and Booli data may suffer from reporting lags or missing sales, and they provide limited information on property attributes (e.g. condition or exact layout) that could confound prices. Geocoding errors are another concern, if some apartments are mislocated by the data providers, the mapping to flood-risk zones could be imprecise. I attempted to validate coordinates against official registers, but minor mismatches can occur. The "flood-score" used to classify exposure is itself a proxy, likely based on modelled inundation or expert assessment. Such scores are uncertain and aggregate, potentially misclassifying some properties. For instance, a building near but slightly above the modelled floodplain might be deemed exposed, or vice versa, which would attenuate estimated effects. Finally, my control variables (e.g. living area, number of rooms, floors) capture many but not all aspects of dwelling quality. Unobserved factors like recent renovations or unique amenities might bias my estimates if they correlate with flood risk. In summary, data imperfections, common in urban impact studies, should warrant some caution regarding the interpretation with regard to precise magnitudes, even as the finding (a price drop) is robust.

8.3 Comparison to existing literature

My results echo, but also refine, the pattern reported in earlier flood-pricing studies. Within Sweden, the few available papers suggest that markets have so far reacted only weakly to flood information. Berggreen Clausen (2016) finds no lasting discount for Lake Vänern flood-plain homes, and Fredriksson (2021) records a very small per-metre gradient that disappears within a year after new SMHI hazard maps are released. By contrast, the 2021 cloudburst generated an immediate drop of about 6-7% in exposed apartment prices. This implies that a salient, realised disaster can trigger a stronger repricing than a map-based information shock alone.

The European evidence is mixed but generally consistent with a negative short-run effect. Votsis and Perrels (2016) document 6-13% discounts in Finnish cities when high-resolution flood-risk maps are published, and Aus dem Moore et al. (2022) show a significant markdown in German districts that flooded after the 2021 Ahr Valley disaster, while districts that were mapped as risky but spared remained unaffected. Along Hungary's Danube and Tisza rivers, Békés et al. (2016) estimate roughly a 2% price reduction per ten-percentage-point rise in expected flood depth, and Skouralis et al. (2024) report an average 8% discount for English homes rated at moderate to severe flood risk. My estimate sits comfortably inside this European range, though it is closer to the lower end, perhaps because Swedish buyer exposure is softened by bundled insurance and generous post-disaster compensation.

United States quasi-experiments show broadly similar dynamics. Bin and Landry (2013) observe a 5-9% discount in North Carolina after Hurricane Floyd, Troy and Romm (2004) find a roughly 4% drop in California following mandatory hazard-disclosure laws, and Harrison et al. (2001) report a comparable markdown in Florida once higher flood-insurance premiums were announced. Those magnitudes match the 6-7% effect identified here, reinforcing the idea that policy or event salience, not just actuarial risk, drives much of the initial price adjustment.

Finally, the belief-heterogeneity literature points to behavioural channels that help reconcile seemingly divergent findings. Bakkensen and Barrage (2022) demonstrate that United States coastal prices can deviate from fundamentals when buyers underweight low-probability hazards, and Giglio et al. (2021) show that long-run climate uncertainty is capitalised into ground-lease rates only where salience is high. By exploiting a sudden and vivid flood shock, this thesis finds a more pronounced price discount than map-based Swedish studies, consistent with behavioral insights showing that when risk becomes tangible, markets reprice far more forcefully.

Taken together, the weight of earlier evidence indicates that property markets typically penalise flood exposure in the short run, with the exact magnitude shaped by institutional context (for example insurance design), the nature of the shock (information versus physical damage), and behavioural salience. Against that backdrop, the Gävleborg flood's 6-7% markdown appears both plausible and economically meaningful, situating Sweden firmly within the broader international pattern of post-disaster price adjustments.

8.4 Alternative methods and future research

Although the difference-in-differences design fits the sudden and local nature of the 2021 cloudburst, several complementary strategies could strengthen or nuance the causal picture. One logical extension is to combine DiD with matching estimators. By first balancing flooded and non-flooded apartments on observable attributes through propensity-score or coarsened-exact matching, then applying the DiD estimator, future work could reduce the bias that survives fixed effects. Bin and Landry (2013) use this two-step approach to sharpen causal inference after Hurricane Floyd. Matching would be especially valuable if richer micro-level data on building quality or household characteristics become available.

A second avenue is spatial econometrics. Flood shocks do not respect neighbourhood boundaries, and price signals may spill over to adjacent but physically dry streets. Spatial-lag or spatial-error specifications, grounded in the autocorrelation framework developed by Dubin (1988), allow researchers to estimate how much of the observed discount reflects direct damage compared with informational contagion across space. Such models can also quantify whether high-scoring blocks depress prices in nearby low-scoring ones, a pattern that a standard DiD might attribute solely to the treated group.

Another challenge is the possible endogeneity of exposure scores. If the Valueguard flood metric correlates with unobserved amenities or building standards, the estimated discount may conflate risk and quality effects. Exogenous variation in rainfall intensity, or in upstream hydrological features that affect run-off without influencing local housing demand, could serve as instruments in the spirit of the regulatory shocks used by Chay and Greenstone (2005) for air-quality valuation. Implementing such an IV strategy would require high-resolution precipitation or watershed data combined with transaction records, a feasible but data-intensive task.

Where flood-risk maps draw sharp hazard boundaries, a regression-discontinuity design can deliver quasi-experimental identification. Homes located just inside versus just outside the mapped floodplain should be similar on most unobservables, echoing Black's (1999) school-district boundary logic. If Swedish municipalities produce detailed pluvial hazard maps after the Gävle event, exploiting those borders could provide compelling evidence about buyers' willingness to pay for small differences in stated risk.

Longer-horizon and distributional analyses would also deepen our understanding of market dynamics. Extending the weekly event-study window could reveal whether the current 6-7 % discount fades, stabilises, or grows over several years, paralleling the temporal patterns documented by Votsis and Perrels (2016) in Finland and by Aus dem Moore et al. (2022) in Germany. In addition, future research could examine market liquidity indicators, such as transaction volume, time on market, and bid–ask spreads to assess whether the flood affected not only prices but also the ease with which properties could be sold. While being outside of this scope of this thesis, a preliminary analysis based on the number of sales before and after the flood has also been made (see Appendix Figure 1A). Quantile regressions or group-specific DiD models could test whether lower-quality buildings, financially constrained buyers, or different floor levels suffer larger price impacts. Evidence from Bakkensen and Barrage (2022) shows that belief heterogeneity drives substantial variation in coastal risk pricing, while Giglio et al. (2021) demonstrate that long-run climate uncertainty is capitalised unevenly across investment horizons. Similar heterogeneity may exist within the Swedish housing market and could inform targeted adaptation or disclosure policies.

Pursuing these methodological extensions would not only test the robustness of the present findings but also shed light on how real-estate markets process increasingly frequent climate shocks, thereby helping policymakers design more effective risk-communication and land-use strategies.

8.5 Implications for climate risk and urban planning

My findings suggest a few potential lessons for policy, though they should be interpreted with due caution. First, they highlight the possible benefits of clearer flood-risk information in the Swedish housing market. Today, buyers receive limited formal disclosure about property-level exposure, yet industry commentators note that highly exposed homes can be more difficult to insure or finance. The "climate-resilience certificate" proposed by Research Institutes of Sweden (RISE) (2023), modelled on the existing energy certificate, could in principle give buyers, insurers, and lenders a clearer view of long-term risk. The large insurance payouts after the 2021 Gävleborg flood (over SEK 1 billion) illustrate the financial stakes involved. Voluntary or mandatory flood-risk labelling at the point of sale might therefore enhance market transparency and reduce unpleasant surprises for households and banks.

Second, the analysis indicates that market prices do respond to realised flood events, although the effect is modest. If climate change increases the frequency of such events, even relatively small price adjustments could matter for Sweden's highly leveraged households. A downturn in collateral values may translate into higher credit risk for lenders. Local planners may therefore wish to review permitting in high-risk areas, and financial supervisors could consider whether existing stress tests adequately capture climate-related shocks to real-estate prices.

Finally, the fact that price effects fade over time does not mean the underlying risk disappears; rather, it may signal that public attention wanes. The period immediately after a disaster could offer a window in which investments in drainage, targeted buy-out programmes, or awareness campaigns are most likely to gain support. Improved disclosure through public flood maps, updated building codes, or insurance pricing that more closely reflects risk could help align individual decisions with long-term adaptation needs.

Overall, while this study focuses on a single event and has several limitations, it tentatively underscores the value of timely and transparent climate-risk communication and of forward-looking planning to reduce future costs for homeowners and for the wider economy.

In sum, my findings underscore the economic value of proactive climate risk communication and planning, without it, markets may only learn of flood hazards the hard way, at the expense of homeowners and the broader economy.

9. Conclusion

The study shows that the cloudburst that hit Gävleborg on 18 August 2021 reduced residential property values in the affected neighbourhoods by about 1 350 to 1 475 SEK/m^2, which corresponds to a decline of 6–7 %. The estimate comes from a difference-in-differences analysis of nearly 11 000 matched transactions and remains robust when I tighten the exposure definition and separate sales by floor level.

Although the price gap narrows after the first year, the evidence suggests that Swedish housing markets do capitalise realised flood risk even under a system of bundled and universal flood insurance. For policymakers, the results point to the possible benefits of clearer flood-risk disclosure, cautious land-use planning, and climate-aware financial stress-testing in a country where household leverage is high. For researchers, the study highlights several useful extensions, such as spatial models, boundary designs, and liquidity analyses, that could further clarify how climate shocks influence real-estate markets over time. In sum, fostering resilience in the financial system under a warming climate requires an evidence-based understanding of both the physical pathways of floodwaters and the valuation adjustments they precipitate in real assets.

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Popular science summary

Intense cloudbursts are becoming more common in Sweden as the climate warms, and they can quickly turn urban streets into streams and basements into pools. On 18 August 2021 the town of Gävle set a national rainfall record when 161 millimetres fell in a single day. This thesis asks whether such an event leaves a mark on house prices. I combine almost 11 000 home sales from the property site Booli with detailed flood-exposure scores for every address, then compare the price paths of homes on the flooded streets with similar homes in nearby low-risk areas before and after the storm. This approach allows me to isolate the effect of the flood from normal movements in the housing market. The results suggest that the hardest-hit homes sold for about 6–7% less during the first year, roughly 1 400 SEK/m^2, with the steepest drop for ground-floor flats. The gap closed after about twelve months, hinting that buyers react most strongly right after a disaster and that the memory fades over time. Although Swedish household insurance still covers water damage for most properties, recent news reports indicate that banks and insurers are starting to examine climate risk more closely, so future price responses could be larger. Taken together, the study offers an early sign that the housing market already puts a value on the danger of flash floods and underscores the need for clearer risk information and smart drainage investments that can protect both household finances and broader economic stability.

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Appendix 1

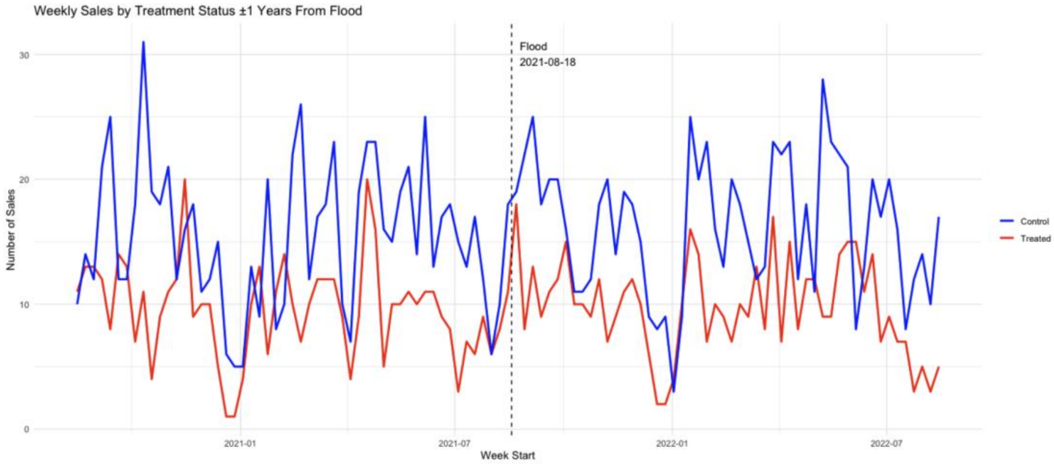
Table 8A: Transaction count by municipality

Municipality	**Count**
Bollnäs	482
Gävle	6041
Hofors	135
Hudiksvall	1582
Ljusdal	335
Nordanstig	93
Ockelbo	28
Ovanåker	66
Sandviken	1292
Söderhamn	834
Grand Total	**10888**

Table 2A: Transaction count by flood-exposure score

Score	**Count**
0	6103
8	10
10	144
12	168
16	709
20	2509
28	1
40	5
50	9
60	28
80	281
100	921
Grand Total	**10888**

Figure 1A: Weekly sales by treatment status ± 1 year from flood



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