The Price of Noise

A Quasi-Experimental Analysis of Rail Noise Barriers and Swiss Residential Rents

Master Thesis

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Abstract

Traffic noise is one of the leading environmental externalities, yet no study has recovered a causal price for a dB of railway noise. This thesis estimates that price using the staggered rollout of noise barriers under Switzerland's federal rail noise abatement program (BGLE). A difference-in-differences design compares rents near a barrier, before and after construction, with a control area farther away. Rents near a barrier rise by 4.4 % within 100 m relative to the control band, with smaller gains at greater distances, implying that tenants are willing to pay a substantial premium for the quiet. A counterfactual acoustic simulation quantifies the noise reduction at each individual apartment, a measure that accounts for local conditions and is more accurate than the distance proxies used elsewhere. Scaling the rent effect by the average noise reduction implies a noise depreciation index of 1.0 % of rent per dB, or about CHF 17 a month. Contrary to standard urban models, this value is not captured by the owners of the shielded buildings: their asking rents do not rise in absolute terms, while rents in the surrounding control area fall, so the gain goes to tenants instead. The resulting price gives future noise-abatement measures a monetary benchmark to be judged against.

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1 Introduction

Traffic noise is a major, unpriced environmental health burden in Europe, with at least 1.6 million healthy life years lost to it annually (World Health Organization Regional Office for Europe, 2018). Governments spend heavily to reduce it. Switzerland alone committed CHF 1.9 billion to railway noise abatement via the Bundesgesetz über die Lärmsanierung der Eisenbahnen (BGLE), building over a thousand noise barriers between 2000 and 2016 (Swiss Confederation, 2000).

Allocating capital on this scale requires deliberate planning. The BGLE evaluated measures based on the number of protected people and decibels (dB) removed. However, this index ignores the monetary benefit: what a dB is actually worth to residents. This thesis provides a causal estimate of that price for railway noise in the Swiss rental market.

To price a non-market good like the quiet, economic studies rely on hedonic pricing, where rent reveals tenants' willingness to pay for specific apartment features (Rosen, 1974). The difficulty is that apartments near railways differ from those farther away in more ways than just noise, such as proximity to a station or building style. Thus, comparing rents across locations mixes the price of quiet with unobserved spatial variables, introducing bias of an unknown direction.

Recent work avoids this by using quasi-experiments at fixed locations. For this, noise barriers create an ideal setting: traffic flows and accessibility remain unchanged, isolating the sound reduction. A price per dB needs two things: a causal price effect and a measured decibel change. Moretti and Wheeler (2025) have both for highway barriers. The railway literature, however, has never combined the two. Diao et al. (2016) and Lee and Pang (2022) have credible designs but lack the data on noise reductions. Ahlfeldt et al. (2019) have the noise data but lack a quasi-experiment: comparing homes along above-ground and underground track brings the confounding back. Therefore, no causal price estimate for a dB of railway noise exists.

Switzerland is well suited to close this gap. Its dense rail network runs through most towns, placing much of the housing stock near active tracks. Additionally, the BGLE supplies the needed quasi-experiment: a federal rule based on noise levels and population exposure decided where to construct a barrier, with construction rolled out in stages across space and time. I combine this rollout with online rental listings from 2000 to 2025 (IAZI, 2026b) and a custom acoustic simulation from the Federal Office for the Environment that recomputes noise levels as if no barriers existed (FOEN, 2026). The difference yields the dB removed by the noise barrier at each individual apartment, calculated by an acoustic simulation rather than simply being estimated by distance to the track. Following Moretti and Wheeler (2025), I use a difference-in-differences (DiD) design that compares rental units shielded by a barrier to a control group 300 to 500 m away. Rents in both groups are observed 5 years before and after construction.

I find that tenants pay a substantial premium for quiet. Within 100 m of a completed barrier, rents rise 4.4 % above the control band, averaging 3.3 % across the first 300 m treated zone. This gap appears after construction and persists over the following five years. To calculate the noise depreciation index (NDI) I divide the barrier effect by the noise reduction. This yields an NDI of 1.0 % of rent per dB, or about CHF 17 monthly.

The mechanism driving this effect is unexpected. Both the open and the closed city predict that rents behind a new barrier rise. A temporal design shows they do not: asking rents behind barriers remain flat, while control band rents fall. The fall matches the closed city, where local demand shifts toward the quieter units. The flat rents behind the barrier match neither model, and Swiss rent law is a possible explanation: it restricts rent increases, so owners cannot capture the gain. The publicly financed gains therefore remain with tenants, while unaffected owners must lower rents to compete.

This thesis contributes to the literature in three ways. First, it gives the first causal estimate of what a one-dB reduction of railway noise is worth, using the BGLE's rollout in Switzerland. Second, it estimates that price with a new kind of noise data: the dB a barrier removes at each address, which accounts for local conditions and is more accurate than the distance proxies used elsewhere. Third, it shows that the barrier's value benefits tenants rather than owners, which neither the open nor the closed city model fully predicts, while nearby unshielded areas see lower rents as demand shifts.

The rest of the thesis is organized as follows. Section 2 reviews the hedonic noise literature and urban capitalization models. Section 3 describes the data and sample construction, while Section 4 sets out the empirical design and price conversion. Section 5 reports the results and Section 6 interprets them. Section 7 concludes.

2 Literature

Traffic noise ranks among the leading environmental health risks in Europe. The World Health Organization estimates that at least 100 million people in the EU are exposed to harmful traffic noise levels, resulting in the loss of at least 1.6 million healthy life years annually in Western Europe alone (World Health Organization Regional Office for Europe, 2018).

Chronic exposure has been linked to a wide range of health outcomes. The risk of cardiovascular disease rises with traffic noise exposure (Fu et al., 2023; Münzel et al., 2021), sleep is disturbed (Basner and McGuire, 2018; Beutel et al., 2020), and rates of depression and anxiety are higher among those strongly annoyed by it (Gong et al., 2022; Beutel et al., 2020).

From an economic perspective, these health burdens are unpriced. Traffic noise is a textbook negative externality: a by-product of transport that falls on third parties and is not internalized by those generating it (Moretti and Wheeler, 2025). Because these externalities are large and uncompensated, it is important to establish their magnitude so that the costs of abatement policies can be evaluated against their benefits.

Three broad approaches to valuing noise reduction exist: health-based monetization of disease burden, stated preference surveys, and revealed preference via hedonic property prices. In economics, the hedonic approach is preferred because it reads willingness to pay off actual choices rather than from hypothetical survey responses. Unlike the health-based approach, the resulting estimate is not limited to endpoints that can be explicitly monetized: it captures the full disutility from noise, including annoyance, sleep loss, and perceived health risks (Nelson, 2008).

Applied to the housing market, this hedonic framework treats a property's price as a bundle of its individual characteristics. This allows researchers to isolate the implicit market price and marginal willingness to pay for specific attributes, such as an additional room or exposure to traffic noise. While the general concept of characteristic-based utility traces back to consumer theory (Lancaster, 1966), it was ultimately Rosen (1974) who provided a microeconomic framework.

Early empirical applications of the hedonic framework relied on cross-sectional variation, which comes with identification challenges. Emerson (1972) provided one of the first major applications, finding aircraft noise around Minneapolis-St. Paul capitalized into nearby house prices, and Gamble et al. (1974) extended the approach to road traffic across four US communities. The most comprehensive synthesis of hedonic transportation noise studies remains Nelson (2008), which surveys the North American airport and road-traffic literature and documents a noise depreciation index of roughly 0.5 % per decibel for road traffic and 0.7 % per decibel for aircraft noise. Expressing the discount per decibel is what makes the estimates portable, since projects that deliver different reductions can then be compared on a common scale. The estimates are remarkably stable across decades and study sites, suggesting that the order of magnitude is well established.

However, the stability of these estimates does not imply causal identification, for at least two reasons. First, properties near transport infrastructure differ along dimensions that are hard to observe, e.g. air pollution or building style, which depress prices in the same locations, while access to amenities such as nearby stations works in the opposite direction. But even a rich set of controls can only absorb parts of this spatial variation. Therefore, the noise coefficient has an unclear direction of bias. Second, even with perfect observables, the hedonic coefficient identifies the marginal renter's willingness to pay rather than a population average, because households sort into locations on the basis of their noise tolerance. The quasi-experimental designs developed below address the first problem directly. They cannot address the second, since no market price reveals more than the marginal renter's valuation. I return to this in Section 6.2.

To overcome spatial confounding, recent literature has shifted toward quasi-experimental research designs. A first wave of these studies exploits shocks to aircraft noise, where new airports or revised approach corridors change exposure across nearby properties for reasons unrelated to local housing markets (Boes and Nüesch, 2011; Sugasawa et al., 2024). Noise barriers along highways and train tracks offer a similarly clean shock. Because the underlying transport infrastructure continues to operate normally, with traffic volume and accessibility unchanged, a barrier isolates the jump in noise from the usual confounders.

Applied to road noise, this logic yields causal estimates. Lindgren (2021) uses the staggered rollout of a Swedish road noise mitigation program. He finds property price increases of 10–12 % near treated streets but recovers no per-decibel estimate. Moretti and Wheeler (2025) combine causal identification with barrier-specific dB reduction estimates. Their main specification is a spatial DiD model estimated over rectangular buffers on the barrier side of the highway, with a 0–500 m treatment and a 500–1500 m control band. They extend this design with a triple-difference method that adds barriers proposed but never built as an additional control. They find a 6.8 % house price increase within 100 m of a completed road barrier in Florida and an implied NDI of roughly 0.9–1.0 % per decibel. Their design is confined to owner-occupied housing.

The effect on prices from such a noise reduction depends on where the households bidding for the quieter units come from. Urban economics distinguishes between an open and a closed city. In the open city of Roback (1982), people move in freely, so rents rise where the barrier acts until they absorb the full value of the quiet. Rents further away are untouched, and the gain goes to the owners of the shielded buildings. In a closed city the local population is fixed, so the bid-rent curve pivots rather than shifts up (Brueckner, 1987): demand moves toward the shielded units, rents rise there and fall further away. Utility is then free to adjust rather than pinned by an outside option, so part of the utility gain can stay with renters (Glaeser, 2008).

The two cases are therefore told apart by the control group, not the treated one. Moretti and Wheeler (2025) run exactly this check, and their flat control band puts Florida in the open-city case. Which case applies depends on how readily local supply and population respond and on how large the shielded area is relative to the market around it.

For the impact of rail noise, the evidence is thin. The literature has produced credible causal designs and dB-level variation, but never in the same study. Diao et al. (2016) exploit the closure of the KTM line in Singapore, a shock fully exogenous to local housing markets, and find a 13.7 % price increase within 400 m of the former tracks. Lee and Pang (2022) come closest to the present design, applying DiD to rail noise barriers in Singapore and finding a 0.6 % price increase after barriers are announced and a further 1.2 % after installation, with larger gains for initially noisier units. The one rail study with dB-level variation, Ahlfeldt et al. (2019), exploits the historical evolution of Berlin's metro system over the twentieth century. By comparing property values along above-ground and underground track sections, they separate accessibility from the noise effect. They find that the negative elasticity of noise increased substantially as incomes rose. However, because their identification relies on comparing different track types across a city network, the estimate is subject to unobserved spatial confounding from neighborhood characteristics. Therefore, no rail study to date recovers a causal per-decibel estimate of the rail noise penalty.

Switzerland provides an ideal setting to close the gap, combining widespread rail exposure with a centralized infrastructure shock. Its rail network is among the densest in Europe and runs through the center of most towns. A large share of the housing stock sits close to active track, and a large stock of barriers exists to study. More important for identification, most barriers were not built in response to local demand: the Bundesgesetz über die Lärmsanierung der Eisenbahnen (BGLE) financed a centrally managed rollout in which barriers were allocated by a cost-effectiveness mechanism, driven by noise exposure and the number of people exposed rather than by local rents (Swiss Confederation, 2000). Placement is therefore plausibly independent of local housing market trends.

Swiss evidence to date pins down the sign and magnitude of the rail discount but remains correlational. Baranzini and Ramirez (2005) fit a cross-sectional hedonic model to Geneva rents and report a discount of about 0.7 % per decibel across all noise sources. The national ZKB/FOEN study (ZKB and FOEN, 2011) reports a rail-specific discount near 0.26 % per decibel above the threshold, larger than its estimates for road traffic or planes. Both read the noise coefficient off a single cross-section and cannot separate noise from the spatial confounders discussed above.

To overcome these cross-sectional limitations, this paper exploits the staggered rollout of barriers under the BGLE as a quasi-experiment, combining it with Swiss rental listings and a modeled measure of rail noise. The barrier rollout identifies the causal effect of quietness on rents, while the noise model converts that effect into an implicit price per decibel. By doing so, this study provides the first causal estimate of the per-decibel price of rail noise.

3 Data

The empirical analysis combines various data sources covering all of Switzerland: online apartment rental listings and hedonic attributes (IAZI, 2026b), the railway network and noise barrier inventory (FOT, 2026b,a; SBB, 2021), and modeled railway noise exposure (FOEN, 2021, 2026). The unit of observation is the individual rental listing. Because apartments are only listed irregularly, the data form a repeated cross-section covering 2000–2025 rather than a panel.

The remainder of the section presents each source and its limitations in turn, describes the spatial join that produces the working sample, and reports descriptive statistics.

3.1 Data Sources

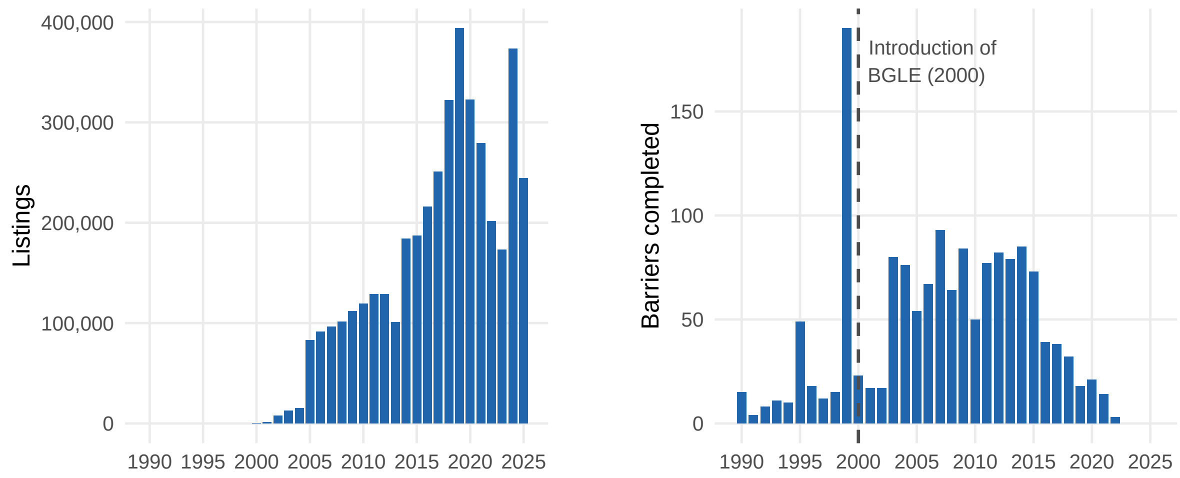
Online Rental Listings. While the majority of hedonic noise studies rely on transaction sales prices (Lindgren, 2021; Moretti and Wheeler, 2025), this analysis uses online rental listings. The hedonic model is theoretically grounded in rental rates (Rosen, 1974), and rents reflect the current flow value of quietness directly. The rental market covers roughly 60 % of Swiss households, making rents the more representative price signal for the population affected by rail noise. Asking rents are preferable to observed rents paid by sitting tenants, whose contract rents are shielded from market pressure and do not adjust to current willingness to pay.

Asking rents come closer to this ideal, though Swiss law also nominally limits rent increases between tenancies. The ceiling is the abuse test of Art. 269–269a OR: a rent counts as abusive only if it yields an excessive return on invested capital, and if it is outside the rents customary in the area. A rise can also be justified by cost increases or additional services of the landlord (Swiss Confederation, 1911). Enforcement of this rule appears weak; only 0.3 % of new leases are legally challenged (Hauck et al., 2025). Asking rents are therefore only loosely bound, and sale prices, negotiated free of these constraints, are not bound at all. That is a genuine advantage of transaction data. Buyers, given the large financial stake, might also conduct thorough due diligence and thus price noise more accurately than renters. Pope (2008), however, shows that even buyers struggle to understand the dynamics of noise: they can substantially underprice noise exposure, and the implicit noise penalty sharpens considerably only after formal disclosure requirements are introduced. Transaction-based hedonic estimates are therefore not immune to information problems.

The asking rents in this paper are online rental listings assembled by IAZI (2026b) and cover the Swiss market over the period 2000 to 2025. The outcome variable is log(grossRent). Coverage is national, with sparser listings in the early 2000s; the number of listings grew with the adoption of online platforms and reached more than 100,000 per year in the 2020s (Figure 1 (a)). The data are provided under a personal data agreement with IAZI.

Listings are not a random sample of the housing stock. An apartment enters the data only when it is listed, so the sample is a flow of units coming to market rather than the stock. Turnover rises after barrier construction, and it does so in the treated and the control rings (Figure A1, Appendix A). Therefore, it should not bias the pooled estimator.

Figure 1: Rental Market and Noise Barrier Coverage, 2000–2025



(a) Rental listings per year

(b) Noise barriers completed per year

Note: Panel (a) shows annual listing counts in the rental market; coverage is sparser before 2005 but spans the full BGLE construction period (2000–2016), providing the temporal variation needed for identification. Panel (b) reports newly completed barriers per year pooling the FOT and SBB inventories; the dashed line marks the introduction of the BGLE program (2000). Sources: IAZI (2026b), FOT (2026a), SBB (2021).

Noise barriers. The primary source is a Federal Office of Transport (FOT) GeoPackage, which records precise barrier geometry, construction year, and planning year (FOT, 2026a). A secondary dataset from the Swiss Federal Railways (SBB) fills the gaps (SBB, 2021). Combining the two is necessary because each layer has a weakness: the FOT layer traces the actual barrier along its full length but is missing some barriers, whereas the SBB layer covers more barriers but records only the start and end points of each barrier. The SBB endpoint representation introduces large inaccuracies for barriers placed on curves, where a straight line between endpoints misrepresents the true position. The merge logic is therefore FOT first, with SBB used only to add barriers not already in FOT. Figure 1 (b) reports the newly built barriers per year. In total, 1,565 barriers are included in the data, of which 95 % come from the FOT GeoPackage and the remaining 5 % are added from the SBB dataset. Around 64 % (1,003 barriers) were built between 2000 and 2016 under the Bundesgesetz über die Lärmsanierung der Eisenbahnen (BGLE), which funded rail noise abatement with CHF 1.9 billion (Swiss Confederation, 2000). Barrier placement followed statutory noise thresholds recorded in the FOT's noise exposure cadastre, not local housing market conditions (FOT, 2023). This supports treating the program as a source of noise abatement variation that is plausibly unrelated to local housing market trends.

Additionally, I use the track geometry from FOT (2026b). I exclude tramways and other urban rail since they are not within the scope of this analysis. The track geometry is used to construct the shadow boxes outlined in Section 3.2.

Hedonic Controls. To ensure the estimated barrier coefficients isolate this infrastructure shock rather than shifts in housing size or quality, the model conditions on structural and locational controls drawn from the online listings (IAZI, 2026b). The structural hedonics are the number of rooms (roomNb), living surface (surfaceLiving), floor level (floor), and construction year (buildYear). I zero-impute and flag the missing floor and build year, so no listing is dropped in the regression.

Additionally, I add two micro-location controls, both defined in Appendix A. The first is IAZI's internal amenity score with all noise components removed, so that it proxies location quality without absorbing the noise treatment this thesis estimates (IAZI, 2026a). It is time-invariant and based on 2022 data, which works well for its largely static components but not for the accessibility ones, which will have shifted over the sample period. I therefore add the public-transport accessibility class (ARE, 2026), a time-varying metric of how well an area is served by public transport, matched to the annual edition nearest each listing. This matters because a discrete jump in accessibility can coincide with a change in rail traffic and therefore noise. Conditioning on these characteristics isolates the baseline structural and locational value of the unit.

Noise Exposure. To analyze the economic impact of noise mitigation, I must first establish a metric that captures noise over a full day. Analogous to the Swiss Noise Abatement Ordinance, this thesis measures noise using the day-night weighted equivalent continuous sound level, Lr_{dn}. This metric summarizes noise immissions over a 24-hour cycle while applying a penalty to nighttime disturbances:

$$Lr_{\text{dn}} = 10 \cdot \log_{10}\left(\frac{16}{24}\, 10^{Lr_{\text{day}}/10} + \frac{8}{24}\, 10^{(Lr_{\text{night}}+10)/10}\right) \tag{1}$$

where Lr_{day} and Lr_{night} represent the equivalent sound levels in dB(A) for the day window (06:00 to 22:00) and night window (22:00 to 06:00), respectively. The +10 dB(A) night penalty directly reflects the heightened psychological annoyance and physiological health risks associated with sleep disruption, formalizing a weighting standard common across European environmental guidelines (Swiss Confederation, 1986; European Parliament and

Council, 2002; Baranzini and Ramirez, 2005). For readability, I refer to Lr_{dn} simply as decibels (dB) from here on; every noise level and reduction reported below is measured on this day-night weighted scale.

To operationalize this metric across the housing stock, I use apartment-level data modeled by the Federal Office for the Environment (FOEN) for the year 2021 (FOEN, 2021). The SonBASE model computes traffic noise in two steps (Catillaz and Fischer, 2025). First, railway source emissions are established using the sonRAIL model combined with the federal emission cadastre. Second, a three-dimensional propagation model simulates how sound travels from the tracks to individual building facades, accounting for local terrain, ground cover, micro-level building geometry, and physical obstacles. The resulting values provide the most detailed representation of real-world acoustic exposure across Switzerland, which is summarized by distance rings in Table 1.

Table 1: Railway Noise by Distance Ring: with vs. without Barriers

Ring	N	Avg no-barrier (dB)	Avg with-barrier (dB)	Avg diff	Median diff	% quieter
0–100 m	111,934	58.0	52.5	5.4	5.2	88.4
100–200 m	90,606	46.8	43.9	2.9	2.2	79.5
200–300 m	66,714	41.6	40.0	1.5	0.6	62.9
300–500 m	97,291	37.4	36.4	1.0	0.1	47.2

Note: Average railway noise by distance ring. A positive difference means the barrier lowers the noise level. % quieter is the share of units in the ring with a noise reduction of more than 0.2 dB (the margin of error). Source: FOEN (2026).

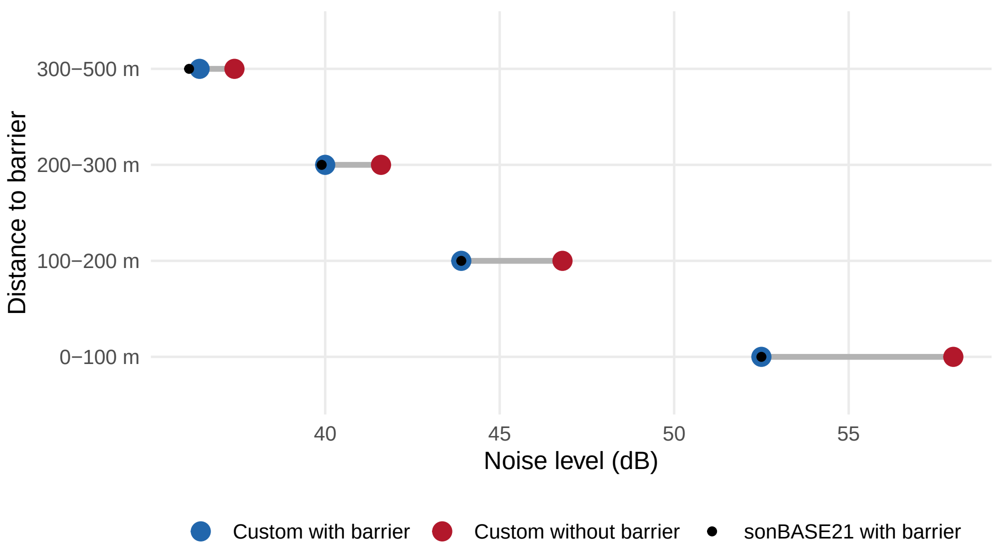
Because noise barriers enter the sonBASE propagation model strictly as physical obstacles along the sound path, their marginal impact can be cleanly isolated from other spatial confounders. To achieve this, I leverage a custom counterfactual simulation generously provided by the FOEN specifically for this research project. By re-running the acoustic propagation step with all noise barriers computationally removed from the model, they simulate a realistic counterfactual scenario. This allows me to calculate the apartment-specific, barrier-induced noise reduction:

$$\Delta \text{dB}_i = Lr_{\text{dn},i}^{\text{no-barrier}} - Lr_{\text{dn},i}^{\text{barrier}} \tag{2}$$

Using an upgraded propagation model relative to the standard public sonBASE21 release, this custom dataset provides treatment intensity for 99.9 % of treated and control units. Figure 2 validates this approach by comparing the standard public sonBASE21 baseline and the custom data with the barriers included. The estimates match tightly on average, indicating that the updated propagation model has no substantial effect on the mean values. The noise impact of the barrier is captured by the horizontal distance between the scenario with and without the barrier. As expected, the noise level and the reduction narrow with increased distance to the barrier.

The noise layers are cross-sections based on 2021 traffic flows and emission values, so I treat dB and ΔdB as time-invariant measures of exposure and treatment intensity. This is not restrictive for the DiD estimates, where treatment is defined by distance to the barrier and construction timing rather than by modeled noise. ΔdB enters only the conversion of the estimated rent effect into a price per dB, and Section 4.3 takes up what the 2021 cross-section implies for that conversion.

Figure 2: Barrier Effect on Railway Noise by Distance Ring



Note: Average rail noise (dB) by distance ring, comparing custom data with the barriers (blue) and without the barriers (red), to the standard sonBASE21 measure (black). Source: FOEN (2021, 2026).

3.2 Sample Construction and Spatial Join

The pipeline that produces the working file rent_master proceeds in the following steps. Listings are retained if rent $\in (0, 10{,}000]$ CHF/month, rooms $\in (0, 10]$, surface $\in (0, 500]\,\mathrm{m}^2$, floor $\in [0, 25]$ (or missing), and coordinates are valid. All spatial layers are then re-projected to EPSG:2056.

Each listing is matched to the noise data via the federal building identifier (EGID). For listings with a known floor, the match is restricted to apartments covering that floor, and noise values are averaged across matching apartments. Around 92 % of listings obtain a noise value via EGID. The remaining listings whose building identifier is missing fall back to the spatially nearest house. Listings with missing floor information use a building-wide mean. Hedonic controls from IAZI (2026b) are merged on the listing identifier.

A 500 m distance box is drawn on the quiet side of each noise barrier, following the barrier geometry. Each box is divided into four distance rings measured from the barrier (0–100, 100–200, 200–300, and 300–500 m). The inner three are defined as the treated and the outer ring (300–500 m) serves as the spatial control as seen in Figure 3. A listing is flagged behind_barrier = 1 if it falls inside the box, recorded with its distance ring, and assigned to its protecting barrier, whose construction year sets the cohort g = barrier_year_built.

Figure 3: Spatial Sampling Design

Note: Setup behind a representative noise barrier. red: treatment zone (0–300 m); blue: control zone (300–500 m). Source: FOT (2026a), FOT (2026b), IAZI (2026b).

This box setup is identical to Lee and Pang (2022) in Singapore. The treated/control boundary lies where the modeled rail-noise reduction flattens out to 1 dB, as seen in panel (2) of Figure 8. Therefore, the noise effect on the 300–500 m control ring is negligible. To better understand how the barrier's effect varies with proximity, I divide the treatment band into three rings at 100 m each, which resembles Moretti and Wheeler (2025). I use fewer treatment bins and a smaller control band because Swiss rail barriers sit in much denser urban environments with smaller neighborhoods and lower noise levels than the Florida highways in their setting. Additionally, robustness checks redraw the boundary with a finer 25 m binning.

The resulting file rent_master is used for all analyses on the listing level. It contains rent, noise data, distance to track, distance ring and barrier flags, cohort timing, municipality identifier, and controls.

The 500 m shadow box assumes barrier effects are local and that the 300–500 m control band is itself unaffected. Barriers may nonetheless shift rents beyond their box, or within the control band through spillovers, a concern I return to in Section 4.2. The design also assumes no anticipation: barriers are typically announced years before construction, so rents may adjust before the barrier is built. I address this in an anticipation test (Figure A4, Appendix B).

3.3 Descriptive Statistics

Table 2 reports means and standard deviations for apartment characteristics in the treatment and the control ring. The final column reports the p-value from a test of equal means. The sample contains 269,629 listings in the treatment ring and 97,449 in the control ring. By construction, the two rings differ sharply in proximity to the track and in rail-noise exposure. Treated apartments lie, on average, 136 m from the nearest track, against 350 m for controls, and are exposed to 46.5 dB of modeled rail noise, against 36.1 dB. Street noise differs by less than 1 dB across the two groups (53.8 versus 53.0 dB), so the contrast is specific to rail. Along the remaining dimensions the rings are close, though control apartments are slightly larger, 83 against 80 m^2, which also lifts their mean gross rent to CHF 1,809 against CHF 1,730. Scaled by size that gap nearly disappears: rent per m^2 is CHF 26.06 in the treatment ring against CHF 26.37 in the control ring, the one difference in the table that the test of equal means does not reject. The build-year and listing-year distributions are close as well. Because the sample is large, the p-values flag every other gap as statistically significant even though they are economically negligible.

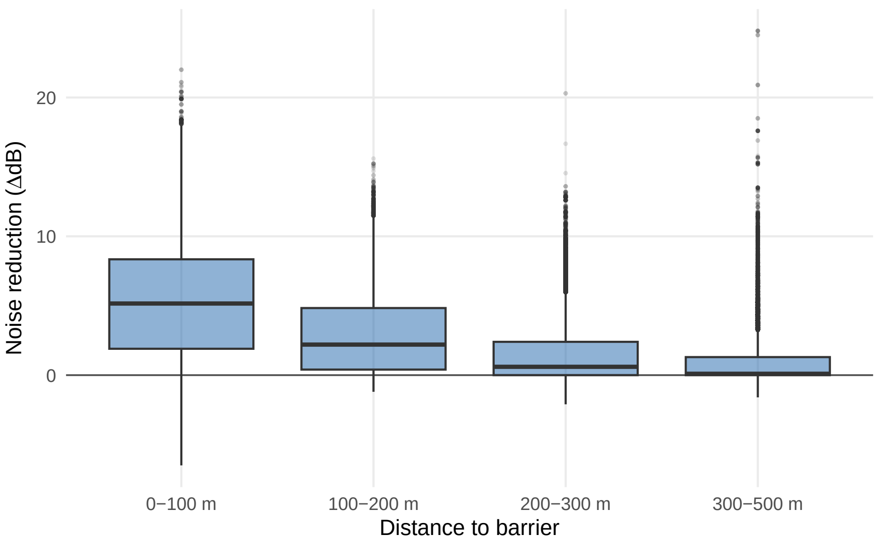
Table 2: Summary Statistics by Treatment Status

	Treatment		Control		Diff.
	Mean	S.D.	Mean	S.D.	p-value
Gross Rent (CHF)	1729.54	819.42	1808.58	830.76	0.00
Rent per m^2 (CHF)	26.06	54.10	26.37	52.16	0.16
Railway Noise (dB)	46.50	8.35	36.10	7.41	0.00
Street Noise (dB)	53.75	7.24	52.99	7.52	0.00
Distance to Track (m)	135.63	81.67	349.73	113.01	0.00
Rooms	3.32	1.20	3.37	1.19	0.00
Living Surface (m^2)	79.99	35.35	82.62	35.93	0.00
Build Year	1982.51	41.16	1980.12	47.54	0.00
Listing Year	2017.32	5.75	2017.38	5.72	0.01
N	269,629		97,449		

Note: The treatment group are units 0–300 m from a noise barrier, while the control is defined as 300–500 m from a barrier. Diff. is the p-value of a two-sided t-test of equal means.

To verify whether the barriers actually provide the intended noise reduction, Figure 2 illustrates the average day-night railway noise across the different distance rings. At first glance, the shifts in the dumbbell plot suggest that the changes in noise are closely linked to distance, with monotonic spatial decay on average. However, the box-plots in Figure 4 reveal that this average pattern hides a lot of heterogeneity, resulting in a large overlap in noise reduction between the different rings.

Figure 4: Distribution of Noise Reduction by Distance Ring

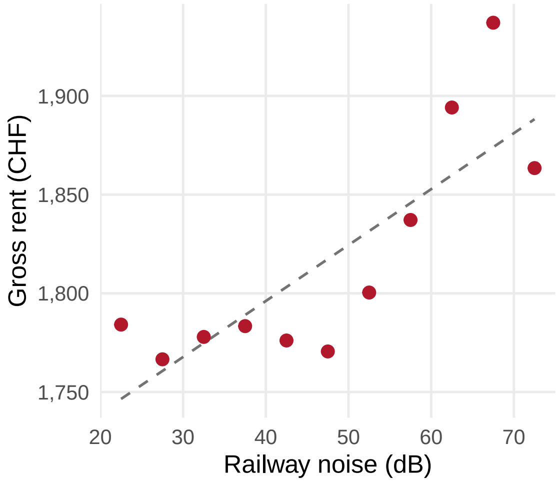


Note: Distribution of the units noise reduction ΔdB by distance ring as defined in section 3.1. Positive values indicate the barrier lowers noise level. Source: FOEN (2026).

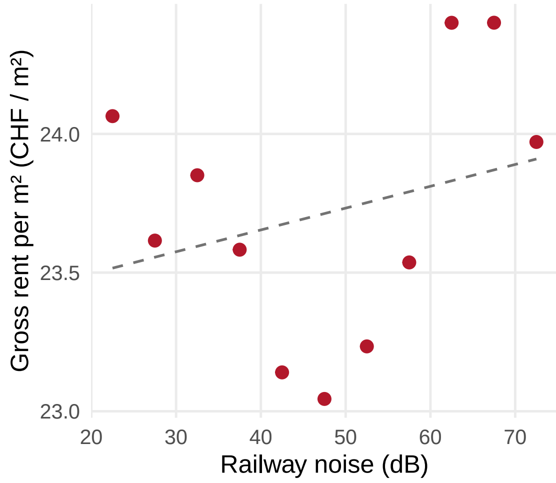
For apartments directly adjacent to the tracks (0–100 m), the median reduction is over 5 dB, with several units enjoying reductions well over 15 dB. Yet, even within this closest band, many properties experience almost zero benefit, and a few even see a slight increase in noise levels. As expected, this mitigation fades systematically toward the 300–500 m control band, where the distribution collapses toward zero. This variation highlights that while proximity to the barrier is the primary driver of quietness on average, the real-world performance of a barrier is highly dependent on site-specific factors. This large variation motivates the use of precise, modeled noise data, unlike much of the existing literature that relies only on distance-based estimates.

Figure 5 plots rent against railway noise in levels (panel (a)) and per square meter (panel (b)). In both, the relationship is positive: apartments in noisier locations rent for more, not less. This is opposite to the hedonic prediction of a noise discount, but unsurprising. Rail noise is highest in central, well-connected places where rents are high for other reasons. The raw correlation therefore mixes noise with location and is uninformative about the causal price of noise. Notably, comparing the two panels reveals a difference in effect size driven by apartment size. While gross rents (panel a) rise steeply with higher noise levels, much of this steepness flattens out when rents are scaled per square meter (panel b). This shift indicates that smaller apartments are disproportionately located closer to the noisy track corridors. Consequently, failing to control for apartment size would severely distort the unadjusted levels, underlining why conditioning on structural characteristics is essential in this empirical design.

Figure 5: Rent and Railway Noise



(a) Gross rent (CHF)

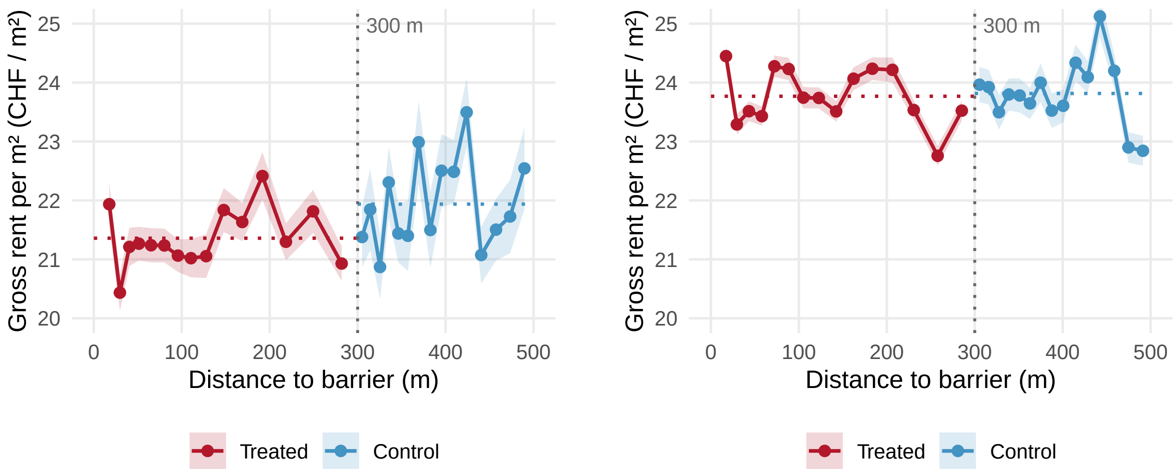


(b) Gross rent per m^2 (CHF/m^2)

Note: Binscatter of rent against railway noise (day-night dB): panel (a) gross rent (CHF), panel (b) gross rent per m^2 (CHF/m^2). Bins are 5 dB wide and dashed line plots OLS fit. Sample covers all rental listings in Switzerland with modeled railway noise above 20 dB and rent per m^2 in $[3, 100]$. Sources: IAZI (2026b), FOEN (2021).

Figure 6 plots mean rent per square meter by distance to the barrier, split by pre-construction (panel a) and post-construction periods (panel b). While both gradients are noisy and non-monotonic, a clear shift occurs. Before the barriers are built (panel a), rents exhibit a discount closer to the tracks, peaking at roughly CHF 23 per m^2 near 400 m. Post-construction (panel b), this pattern disappears, with comparable rents in both groups. They peak around CHF 25 per m^2 near 200 m and 400 m. The overall upward shift in rent reflects mainly the price trends over time as observations behind barriers are later.

Figure 6: Rent per m^2 by Distance to Noise Barrier, Before and After Barrier Construction



(a) Before barrier built

(b) After barrier built

Note: Binned unconditional means (15 equal-count bins per group) of gross rent per m^2 (CHF/m^2) with 95 % confidence intervals, split by whether the listing was observed before (panel (a)) or after (panel (b)) the protecting barrier was built. Treatment zone (0–300 m): red; control zone (300–500 m): blue. Dotted horizontal lines are the group means. Sources: IAZI (2026b).

Both figures highlight the same underlying difficulty: apartments near noisy rail corridors or designated barrier sites differ systematically from quieter, more distant units in ways that a simple comparison cannot disentangle. Much like the cross-sectional noise correlations, these raw trends are subject to spatial confounding. For instance, the apparent leveling of the gradient after the barriers are built mixes up the localized benefits of the barrier with a macro price trend over time, as well as structural sorting by apartment size. To cleanly separate these confounders from the true price of quietness, this paper employs a DiD strategy that exploits the staggered construction of barriers over time, as described in Section 4.

4 Methodology

Section 4.1 introduces the hedonic framework. Following this, the empirical strategy is split into two main parts. Section 4.2 identifies the causal effect of rail noise barriers on rental prices using a staggered DiD design. Section 4.3 translates these price changes into a per-dB capitalization rate.

4.1 Hedonic Pricing Framework

The analysis rests on the hedonic framework of Rosen (1974), where a rental apartment is viewed as a bundle of attributes. The acoustic environment is one such attribute. Because a noise barrier lowers rail noise without altering other apartment characteristics, it isolates the value of the acoustic environment. The rental market's price response to a new barrier thus recovers the implicit price of quietness, reflecting tenants' marginal willingness to pay to avoid rail noise.

I estimate log gross asking rents in a semi-logarithmic specification, so that coefficients measure proportional rent effects and map directly into the percent-per-decibel form in which the literature reports the NDI. Because the estimated effects are small, I report the log-rent coefficients directly as approximate percentage effects. The estimated barrier parameters reflect the equilibrium price adjustments resulting from renter and landlord responses to the altered noise environment. Scaled by the actual decibel reduction achieved across the barrier zone, these estimates yield the marginal implicit price of quietness.

4.2 Causal Effect of Noise Barriers

My primary design adapts the spatial DiD design of Moretti and Wheeler (2025) from owner-occupied transactions with road-noise in Florida to Swiss rentals affected by rail noise, comparing treated rings with an outer control band. The sample is restricted to listings within the 500 m distance box of each barrier, divided into treated rings (0–100, 100–200, 200–300 m) and a control band (300–500 m). Within each box, event time is defined as $\tau = t - g$ where g = construction year, and, following Moretti and Wheeler (2025), listings are kept within a ±5-year window around completion. Alongside this spatial design, I add a temporal design that drops the control band and identifies the effect from the staggered rollout alone, using not-yet-treated listings as controls. This lets me check whether the effect spills over into the control band.

Spatial specification. To implement this spatial design, I estimate

$$\begin{aligned}\log(\text{grossRent})_{it} = &\sum_{j \le 300\,\text{m}} \mathbf{1}\{\text{dist} = j\}\,\mathbf{1}\{\tau \ge 0\}\,\beta_j \\ &+\ \gamma_{b(i)d(i)} + \eta_{b(i)\tau} + x_{it}'\zeta + \varepsilon_{it},\end{aligned} \tag{3}$$

where dist indexes the distance ring, τ is event time, and b the protecting barrier. The coefficients β_j give the post-construction change in log rent in treated ring j, relative to the 300–500 m control band. Barrier × ring fixed effects $\gamma_{b(i)d(i)}$ absorb the permanent rent gradient within each box. Barrier × event-time fixed effects $\eta_{b(i)\tau}$ absorb shocks common to a barrier's neighborhood at a given event time. The vector x_{it} holds hedonic controls. Identification comes from within-barrier comparisons across rings and event time.

Temporal specification. Because the spatial design uses the 300–500 m band as its baseline, it cannot tell whether the premium comes from treated rents rising or control rents falling. This temporal framework serves to distinguish between a pure localized amenity effect and broader general equilibrium adjustments. If a newly built barrier motivates tenants to move into the newly quieted zone, housing demand further away could decrease, lowering rents in the 300–500 m control band. Moretti and Wheeler (2025) highlight this possibility of demand spillovers and evaluate it by checking whether absolute price trends in their control group remain flat.

To isolate these dynamics, my temporal design drops the control band entirely and restricts the sample strictly to the treated rings (0–300 m) across all corridors, identifying the effect solely from the staggered rollout. Listings observed before construction ($\tau < 0$) serve as not-yet-treated controls for listings observed after it ($\tau \ge 0$). The specification is:

$$\begin{aligned}\log(\text{grossRent})_{it} = &\sum_{j \le 300\,\text{m}} \mathbf{1}\{\text{dist} = j\}\,\mathbf{1}\{\tau \ge 0\}\,\beta_j \\ &+\ \gamma_{b(i)d(i)} + \lambda_t + x_{it}'\zeta + \varepsilon_{it}.\end{aligned} \tag{4}$$

This is very similar to the spatial design but changes the control group. It keeps the barrier × ring fixed effects $\gamma_{b(i)d(i)}$ to control for permanent local price gradients, but replaces the barrier × event-time effects with calendar-year fixed effects λ_t. This adjustment is necessary because barrier × event-time effects would absorb the post-construction indicator within the restricted sample, leaving the temporal treatment effect unidentified.

When treatment timing is staggered and treatment effects are heterogeneous, this standard TWFE estimator can be biased because already-treated units are implicitly used as controls (de Chaisemartin and D'Haultfœuille, 2020). The recent development of robust estimators avoids these forbidden comparisons by using clean, not-yet-treated control groups (Callaway and Sant'Anna, 2021) or by saturating the model with cohort-time interactions within an extended TWFE framework (Sun and Abraham, 2021; Wooldridge, 2025). Both can evaluate whether heterogeneous treatment effects introduce meaningful bias into the baseline parameters. However, the first approach is data-intensive. In my repeated cross-section, the volume of listings is often spread thinly across individual cohort-years, complicating the implementation. I therefore use the TWFE on the pooled treated band and report the comparison in Section 5.2.

Controls. Because the listings form a repeated cross-section rather than a panel, no unit fixed effect is available, and the control vector x_{it} must absorb quality differences across apartments directly. Both specifications contain the same structural and micro-location controls described in Section 3.1: rooms, living surface, floor level, construction year, the noise-free micro-location, and the time-varying public transport accessibility class (A). Differences in macro-location are absorbed by the fixed effects.

Identifying assumptions. Callaway and Sant'Anna (2021) formalize staggered difference-in-differences designs around three main conditions: irreversibility of treatment (once treated, a unit remains treated), limited anticipation (untreated potential outcomes are unaffected by treatment before some known anticipation horizon), and parallel trends (absent treatment, treated and comparison units would have followed the same path).

First, irreversibility holds by construction, since noise barriers, once built, are not removed within my sample period. Second, the absence of anticipation implies that price responses occur after construction, not at legal approval, which I test using an approval-year anticipation test (Figure A4). Third, parallel trends require that rents in treated rings would have evolved identically to the control band absent construction, a hypothesis I test via pre-trend event study coefficients (Figure 7). I assess these three in Section 5.4, after the estimates are reported.

Additionally, as in Moretti and Wheeler (2025), I check whether the barrier leaves the control band unaffected. Estimating the temporal specification on that band alone shows whether rents there move after construction (Table A3, Appendix C). The check decides how $\hat{\beta}_j$ is read: as the absolute gain near the barrier if the control band is flat, or as the contrast between the two bands if it is not. Section 5.2 reports it.

4.3 The Implicit Price per Decibel

The DiD framework in Section 4.2 recovers the percentage rent effect of a newly built barrier. This subsection converts the estimated rent effect into a noise depreciation index (NDI): the change in rent associated with a 1 dB change in noise. Following Wald (1940), I estimate this with a ratio of two treated-versus-control comparisons:

$$\widehat{\text{price}}_j = \frac{\hat{\beta}_j}{\overline{\Delta \text{dB}}_j - \overline{\Delta \text{dB}}_{\text{ctrl}}}, \tag{5}$$

where $\hat{\beta}_j$ is the spatial DiD coefficient for ring j from Eq. 3, and $\overline{\Delta \text{dB}}_j - \overline{\Delta \text{dB}}_{\text{ctrl}}$ is the difference in mean noise reduction between j and the 300–500 m control ring (Section 3.1). Both parts of the ratio are therefore the same treated-versus-control contrast: $\hat{\beta}_j$ measures rents in ring j against the control band, so the denominator has to measure noise the same way rather than using the reduction in ring j alone. Using the spatial rather than the temporal $\hat{\beta}_j$ keeps that alignment. Because the noise reduction ΔdB is modeled, I treat the denominator as fixed. Sampling uncertainty is therefore attributed only to the regression coefficient, and the intervals reported for the implicit price are narrower than the true uncertainty in the ratio by an unknown amount.

Following Moretti and Wheeler (2025), I restrict the headline NDI calculation to the 0–100 m distance ring, motivated by two structural factors. First, the price response is concave in the size of the reduction, and only close to the barrier is the reduction large enough to register. Second, restricting to the ring immediately adjacent to the barrier matches the wider hedonic transport-noise literature. The Swiss estimates restrict the NDI calculation to units with an exposure above the 50 dB statutory threshold (Baranzini and Ramirez, 2005; ZKB and FOEN, 2011).

Eq. 5 divides the whole rent effect by the whole noise reduction, which assumes the response is proportional to the reduction. What it recovers is therefore an average price over the range from zero to $\overline{\Delta \text{dB}}_j$, not the price of the last decibel removed. In my estimate, the 4.39 dB denominator is a relative difference. It is the treated-minus-control differential, not the absolute noise removed. The barrier actually removes 5.4 dB in the 0–100 m ring (Table 1). That 5.4 dB figure is the proper comparison to the 7.15 dB evaluated by Moretti and Wheeler (2025). Because they find the price effect is concave, the average value of a decibel depends on the total noise reduction. Therefore, the two estimates represent different points on this curve rather than direct replications.

The resulting estimate is converted into a percentage expression, matching the reporting standard across the literature (Nelson, 2008; Moretti and Wheeler, 2025). To provide a tangible policy magnitude, I additionally translate this into a Swiss franc figure evaluated at the sample's baseline mean gross rent.

As noted in Section 3.1, the counterfactual is simulated on 2021 traffic flows, while the barriers in the sample were built since 2000. Traffic volumes and emissions have changed over this period, though not drastically, so the noise level at a given apartment was somewhat different at the time of construction. Given small changes in the level, the barrier would have had a similar effect, and the modeled $\overline{\Delta \mathrm{dB}}_j$ remains a reasonable measure of treatment intensity.

5 Results

This section reports the estimates from the spatial and temporal specifications, together with the robustness tests behind them. It then translates the rent effect into the implicit price per decibel. Finally, I show that the conditions the design rests on hold.

5.1 Spatial Specification

The main specification (Eq. 3) compares the treated rings with the 300–500 m control band of the same barrier, before and after construction. Table 3 reports the coefficients. Column (1) pools the treated rings: once a barrier is completed, rents within 300 m stand about 3.3 % above the control band, significant at the 1 % level. Column (2) splits the band into the three distance rings. The effect is strongest in the ring adjacent to the barrier, at about 4.4 %, and falls to 2.9 % and 2.3 % in the two outer rings. All three are significant, and the decreasing effect matches the acoustic logic of the design. Both columns are estimated on 75,670 listings.

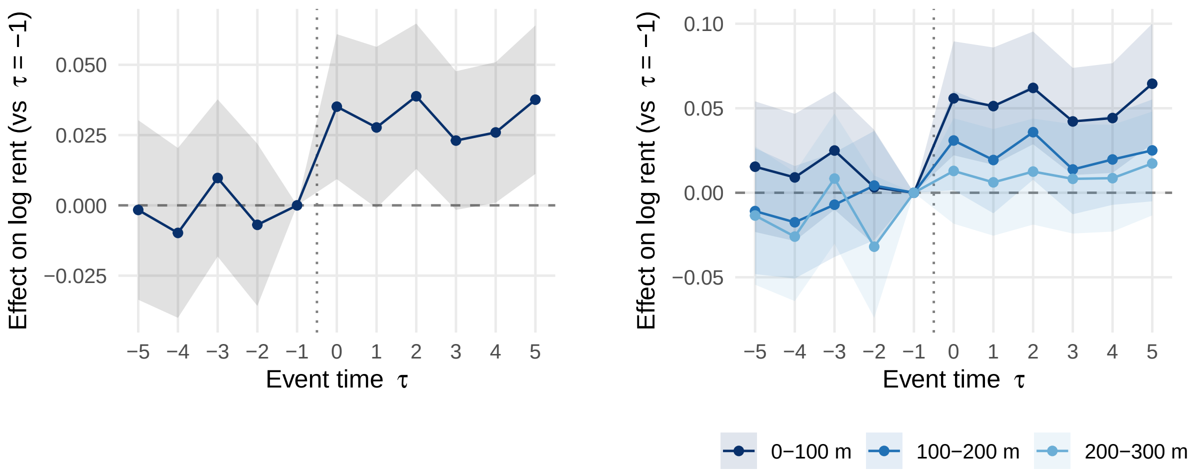
Table 3: Spatial Specification: Effect of Barrier Construction on Log Rent

Dependent Variable:	Log gross rent	
	Pooled	By distance ring
Model:	(1)	(2)
Variables		
Post × Treated (0–300 m) = 1	0.0329***	
	(0.0101)	
Post × Ring = 0–100 m		0.0439***
		(0.0125)
Post × Ring = 100–200 m		0.0291***
		(0.0108)
Post × Ring = 200–300 m		0.0230**
		(0.0109)
Fixed-effects		
Barrier-Ring	Yes	Yes
Barrier-Event time	Yes	Yes
Fit statistics		
Observations	75,670	75,670
R^2	0.79504	0.79508

Note: Coefficients from Eq. 3 on a ±5-year window around barrier completion, pooled over the treated band and split by distance ring. SEs clustered on barrier in parentheses. FE: barrier × ring and barrier × event-time. Outcome is log rent, so coefficients are approximately proportional effects. Stars: *** p<0.01, ** p<0.05, * p<0.1.

Figure 7 traces the same effect over event time, pooled across the treated band (panel (a)) and separately by distance ring (panel (b)). The gap opens at $\tau = 0$, reaching about 3.5 % in the year of completion, and then stays in a band of 2.3 to 3.9 %. The response is immediate rather than gradual, and it does not decay over the five years after completion. The ring-specific series in panel (b) follow the same path and differ mainly in level, with the 0–100 m ring highest throughout, so the timing of the response is common to the whole treated band while its size is not. The coefficients before construction are flat, which Section 5.4 takes up.

Figure 7: Event Study: Effect of Barrier Construction on Log Rent



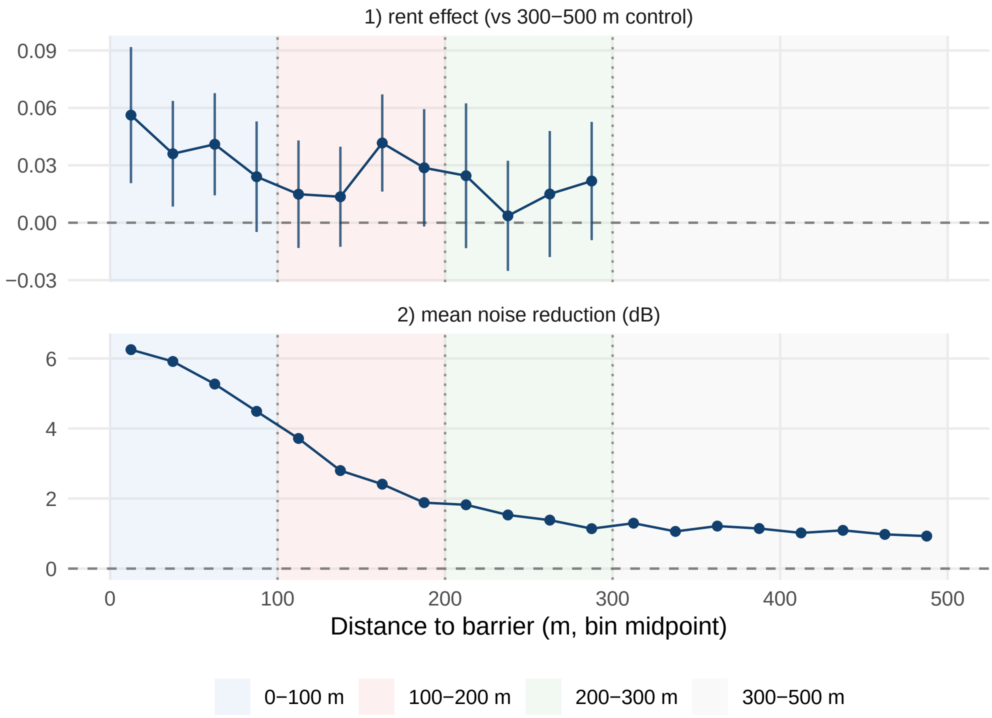
(a) Pooled (b) By distance ring

Note: Coefficients from Eq. 3 interacting event time τ with treated status (panel (a)) or distance ring (panel (b)), relative to $\tau = -1$. Dashed vertical line marks barrier completion. Shaded bands are 95 % confidence intervals from SEs clustered on barrier. FE: barrier $\times$ ring and barrier $\times$ event-time.

These results are robust to different estimations. Re-estimating the same specification on $\log\left(\frac{\text{rent}}{\text{m}^2}\right)$ gives a pooled effect of 2.9 % and leaves the ring pattern intact (Figure A5, Appendix B), so it does not come from smaller apartments sorting closer to the track. Dropping one barrier at a time moves each ring effect by only a fraction of its confidence interval (Figure A6, Appendix B), so the effect is not pushed by a single unusual site either.

The three rings suggest an orderly decline with distance, but that reading depends on the ring definition. Figure 8 re-estimates the same specification in 25 m bins. Panel (1) shows that the rent effect is not a smooth function of distance: it moves up and down across the treated band rather than falling steadily, and it does not line up with the smooth decay of ΔdB in panel (2). Small sample sizes cannot explain this variation, since each bin still holds over 3,200 listings from nearly 300 barriers. While a regional split partially accounts for the pattern (see Figure A7, Appendix B), it does not explain it entirely. Therefore, the estimates must be interpreted as a band average, not at a specific distance.

Figure 8: Rent Effect and Noise Reduction by Distance to Barrier, 25 m Bins



Note: Panel (1) plots the rent effect against the 300–500 m control band by distance to the barrier, estimated in 25 m bins instead of the 100 m rings used in Table 3. 95 % CIs from SEs clustered on barrier. Panel (2) plots the mean ΔdB the barrier removes over the same bins.

5.2 Temporal Specification

The temporal specification of Eq. 4 isolates the movement in the treated rings by dropping the control band and identifying from the staggered rollout alone, with listings observed before construction serving as not-yet-treated controls.

The treated rings do not move. The pooled estimate in Table 4 is 0.05 % with a standard error of 0.51 %. The ring-specific estimates in column (2) are likewise small and insignificant, although their point estimates still fall with distance to the barrier as in Table 3. Rents in the treated rings therefore do not rise in absolute terms after a barrier is completed.

Because treatment timing is staggered, plain TWFE can be biased by forbidden comparisons whenever treatment effects differ across cohorts or evolve over event time. Table A2 in Appendix B reports the Sun and Abraham (2021) and Wooldridge (2025) estimates alongside it. Both stay close to zero and insignificant, and the cohort-specific estimates behind them are individually small, so the result survives more robust estimators.

Table 4: Temporal Specification: Effect of Barrier Construction on Log Rent

Dependent Variable:	Log gross rent	
	Pooled	By distance ring
Model:	(1)	(2)
Variables		
Post	0.0005	
	(0.0051)	
Post x 0–100 m		0.0058
		(0.0070)
Post x 100–200 m		-0.0020
		(0.0070)
Post x 200–300 m		-0.0059
		(0.0084)
Fixed-effects		
Barrier-Ring	Yes	Yes
Year	Yes	Yes
Fit statistics		
Observations	57,401	57,401
R^2	0.77444	0.77446

Note: Coefficients from Eq. 4 on the treated rings alone, with listings observed before completion serving as not-yet-treated controls, on a ±5-year window. SEs clustered on barrier in parentheses. FE: barrier × ring and calendar year. Outcome is log rent, so coefficients are approximately proportional effects. Stars: *** p<0.01, ** p<0.05, * p<0.1.

The control band, by contrast, falls. Estimating Eq. 4 on the 300–500 m band alone puts rents there 2.4 % lower after construction, significant at the 5 % level (Table A3, Appendix C). Because the treated rings are flat while the control band falls, the spatial premium is the control band moving away from the treated rings rather than the treated rings moving up. Additionally, the two estimates imply a contrast between the methods. Differencing the two gives a temporal contrast of 2.4 %, against the 3.3 % the main spatial specification estimates. Table A4 in Appendix C traces what separates the two designs.

5.3 The Implicit Price of Noise

The spatial estimates can now be converted into an implicit price per decibel. Applying Eq. 5 to them gives Table 5. In the headline 0–100 m ring the barrier removes 4.4 dB relative to the control band and raises rents by 4.4 %, with a standard error of 1.3 %. That implies an NDI of 1.0 % per dB, or CHF 16.74 per month per dB evaluated at the ring's mean gross rent, with a 95 % interval of 0.4 to 1.6 % per dB.

Table 5: Implicit price of quietness

Band	$\hat{\beta}$ (log pts)	SE	ΔdB	ΔΔdB	%/dB	CHF/dB
0–100 m	4.39	1.25	5.49	4.39	1.01	16.74
100–200 m	2.91	1.08	2.79	1.69	1.73	29.52
200–300 m	2.30	1.09	1.51	0.41	5.76	97.32
300–500 m	—	—	1.10	—	—	—

Note: Coefficients from Eq. 3, converted into a price per dB by Eq. 5. $\hat{\beta}$ and SE are in log points, with SEs clustered on barrier. ΔdB is the band's own average sonBASE noise reduction; ΔΔdB nets out the 300–500 m control band, which is the contrast $\hat{\beta}$ identifies and the denominator of the price, so the control band carries a raw reduction only. ΔΔdB is treated as fixed, so the price carries only the sampling error in $\hat{\beta}$. Price is the implied rent change per dB, evaluated at the band's mean rent.

The outer rings show that the restriction to the first ring is not only a convention. ΔdB falls with distance faster than the rent effect does, so the ratio inflates quickly: it reaches an implausible 5.8 % per dB at 200–300 m, where the barrier removes only 0.41 dB.

5.4 Identifying Assumptions

Section 4.2 set out three conditions on which the design rests. These conditions are assessed here. The first is irreversibility, and it holds by design: no barrier in the sample is removed once it is built.

The second condition is parallel trends. Absent construction, rents in the treated rings would have followed the control band. Before construction ($\tau < 0$) the coefficients in Figure 7 fluctuate narrowly around zero, with confidence intervals that cover zero and no visible trend, which supports the assumption.

The third condition concerns anticipation. Barriers are legally approved several years before they are built, so the market could in principle react to the approval rather than to the completion. The median barrier is completed three years after approval. Re-dating τ to the approval year lowers the pooled effect, and the event study then stays flat until $\tau = 3$, the median lag, before rising. Rents therefore respond to construction rather than to the announcement of it (Figures A3 and A4, Appendix B).

6 Discussion

The two designs in Section 5 measure the same contrast but attribute it differently. This section works out why, how the estimates should be read as a result, and their implications.

6.1 Where the Effect Comes From

Comparing the two designs shows whether the treated rings rose or the control band fell. The spatial design finds that a barrier raises rents within 100 m by 4.4 % relative to the control band, and rents within 300 m by about 3.3 %. The temporal design, which drops the control band and identifies from the staggered rollout alone, finds no movement in the treated rings at all. Run on the control band by itself, it finds rents there 2.5 % lower after construction. The gap that the spatial design measures is therefore mainly the control band falling away from the treated rings, not the treated rings rising.

Section 2 set out two benchmarks, the open and the closed city, which differ in their predictions about what happens to the control band. An open city predicts that the treated rings rise and the control band stays flat, the pattern Moretti and Wheeler (2025) document in Florida. A closed city predicts that the treated rings rise and the control band falls because the households bidding for the newly quiet units have to come from the same city. The Swiss data match the closed city on the control band and neither case on the treated rings.

The null in the treated rings could be an artifact of the design. The temporal specification cannot use the full fixed effects of the spatial one, so a true effect could be hidden. Appendix C shows that even under the most generous reading, the coefficient on the treated rings would rise only from 0.05 % to 0.7 %, still indistinguishable from zero. The reduced fixed effects do bias the estimate downward, just not enough to make a difference.

Swiss rent law offers a possible explanation for this result. Generally, asking rents can be negotiated freely, but there is regulation that limits the pricing power of the landlord (Section 3). Nothing restricts keeping the rent flat or even reducing it. The constraint is therefore asymmetric: rents near the barrier cannot climb as fast as landlords would like, while rents further out can fall as much as the market requires. In the short term, this limits price rises in the treated area, which is the pattern I find. Rent rises in the treated rings might then be deferred, arriving only years after the barrier is built. This thesis cannot tell whether they are: the temporal specification estimates a single post-construction coefficient rather than a path over event time, and it follows barriers for at most five years after completion.

Neither where the effect comes from nor the possibility of later capitalization disturbs the estimated price. What a household pays for quiet is the difference in rent between a quiet apartment and an otherwise identical but noisy one, and the spatial design measures exactly that difference within the same neighborhood and the same year. Which ring this effect comes from does not change what the household gives up to avoid noise, so the relative contrast is the right object for the question this thesis asks. It does matter for who gains from the barriers, the subject of Section 6.4.

6.2 Interpreting the Implicit Price

Having established that the premium is the right contrast, I now turn to what the price built from it contains. The NDI of 1.0 % per dB divides the estimated rent effect by the modeled noise reduction and so attributes the entire value of the barrier to the change in decibels. That is the premise the design rests on: a barrier alters the acoustic environment and leaves the rest of the apartment untouched (Section 4.1). It holds only approximately. What the ratio prices is the barrier as installed rather than the quiet on its own, and two factors make the resulting figure a lower bound.

The first is that a barrier does not only deliver quiet. It is also a wall, and it takes some of the view and some of the light from the units immediately behind it. That cost enters the same coefficient with the opposite sign, so $\hat{\beta}_j$ prices the acoustic gain net of the lost outlook. The value of quietness on its own is therefore larger than the figures in Table 5 would suggest. This should be largest in the 0–100 m ring, which is where the headline figure comes from. The two cannot be separated here, since both are strongest for the lower floors nearest the barrier.

The second factor is whose valuation the price reflects. Every household in this sample has chosen to live within 500 m of a railway. What a hedonic coefficient recovers is the willingness to pay of the marginal, not of the average household. People who are strongly bothered by rail noise tend not to live there, so the figure here reflects a relatively noise-tolerant population and sits below what an average household would pay for the same quiet. That is the right population for assessing the BGLE, since these are the people the barriers protect, but as a general price for quiet, the figure is a lower bound.

6.3 Comparison with the Literature

The estimates can now be compared to the existing literature. The rent effect and the implicit price are worth comparing separately, since they are not equally comparable across studies.

The rent effect from the spatial model sits inside the published range. The 3.3 % premium pooled over the treated band, and 4.4 % in the ring adjacent to the barrier, is larger than the 1.8 % Lee and Pang (2022) find on the installation of rail barriers in Singapore and below the 6.8 % Moretti and Wheeler (2025) report within 100 m of a Florida road barrier, though the latter is measured on sale prices rather than rents. It is far below the 13.7 % Diao et al. (2016) report within 400 m of a closed rail line. That ordering is to be expected: smaller noise reductions create smaller price changes.

The implicit price of noise lands close to the only other causal estimate available. At 1.0 % per dB it sits at the top of the 0.9 to 1.0 % range Moretti and Wheeler (2025) obtain for Florida road barriers. This is worth noting because the two studies share a methodological design but are in very different settings. The closeness should not be pressed too hard, though. While both figures are averages rather than marginal prices, they are evaluated at different doses, 7.15 dB in Florida against the 5.4 dB a Swiss barrier actually removes in the 0–100 m ring, and the concavity they document implies a smaller average price at the smaller dose. Two numbers this similar at doses this different might just be a coincidence rather than a replication. The Swiss cross-sectional benchmarks are further off. Baranzini and Ramirez (2005) report 0.7 % per decibel across all noise sources in Geneva, and ZKB and FOEN (2011) report 0.26 % for rail specifically, roughly a quarter of the estimate here.

The gap to ZKB and FOEN (2011) is the one that is interesting, since it prices rail noise in the Swiss rental market and so comes closest to measuring the same thing. Two differences separate the estimates. The first is identification. Their coefficient comes from a single cross-section, so it includes all unobserved attributes of living near a track. This thesis changes exposure in a fixed area. The second is the exposure at which a decibel is priced. They measure above a statutory threshold as well, but impose one linear coefficient on everything above it, so their estimate is assumed not to depend on the noise level. The same report notes that raising that threshold by 5 dB can raise the estimated reduction per decibel by a factor of 1.2 to 2, so a larger figure here, from the ring closest to the track, is what that sensitivity plausibly implies.

6.4 Incidence and Policy Implications

The results also carry distributional consequences that the program's own accounting never priced. The barriers in this sample were federally financed, under a program that spent CHF 1.9 billion on rail noise abatement (Swiss Confederation, 2000). The noise model confirms the acoustic benefit for shielded households. This thesis shows who gains financially from it. Both benchmarks in Section 6.1 predict that rents next to a barrier rise, so its value accrues to the owners there; they differ only in what happens further out. In this setting, rents next to the barrier do not rise at all. The value of the barrier is not collected where the barrier was built.

The economic benefit stays with tenants instead. The temporal null shows that asking rents in the treated rings do not adjust to reflect the quiet, so households move into the newly quiet apartments at a price set before the barrier was built. Under the open-city reading, they would have paid the full value of the improvement, and the publicly financed benefit would have passed to owners (Glaeser, 2008). Instead, it is held by whoever lives there, as a gain in tenant utility rather than in land value. Tenants in the control band gain as well: their rents fall relative to the national trend, which an open city would not have produced.

The cost of this shift falls on owners in the control band. They received no barrier while their tenants' outside option improved, so they had to offer lower asking rents to compete with the now-quieter units nearer the track. A federally financed improvement to one set of buildings is therefore costing the owners of another set a few hundred meters away. This outcome is plausibly driven in part by Swiss rent law. Landlords are limited in their ability to raise rents, but not to lower them, as outlined in Section 6.1. The program's cost-effectiveness ranking counted only the people it protected, so a transfer from owners to tenants outside the protected ring never entered the calculation.

Whatever the incidence, the price itself is portable. Estimated from the Swiss rental market, the CHF 17 per dB per month converts the modeled rail noise reduction of any further measure, whether another barrier, rail grinding, or rolling-stock retrofits, into a rent equivalent that can be set against its cost. Used within the limits of the setting it came from, it prices a cost the transport sector does not pay. Only in monetary terms can that cost be weighed against the spending meant to reduce it.

6.5 Limitations

Three limitations are relevant for these estimates: what the premium measures, how well it converts into a price per decibel, and how that price carries over to other situations. The premium is a difference between rings rather than an absolute change in the treated rings, so it does not support a verdict on whether the BGLE was worth its cost. That needs the absolute gain to the corridor, which this design does not identify. Section 6.1 sets out why the relative contrast is nonetheless the right object for the price this thesis estimates.

In this thesis, treatment intensity is assigned by horizontal distance alone. As Appendix B sets out, a distance ring pools apartments the barrier treats very differently, since it shields lower floors far more than upper ones and buildings shield one another. This does not bias $\hat{\beta}_j$, which is an average effect over the ring, but it matters for the conversion in Section 4.3: that average is divided by the ring's mean noise reduction, and because the price response is concave in the reduction, an average over the ring sits below the effect at its mean. The implicit price is again more likely understated than overstated.

Finally, barriers were built where rail noise exceeded the statutory thresholds, so external validity within Switzerland is good but not complete. The estimates describe the areas the program targeted, which is most of the network above the threshold, but they do not necessarily carry over to quieter stretches, to road or aircraft noise, or to more elastic housing markets in other countries. The threshold restriction matters more than it may appear, because decibels are a logarithmic scale and the annoyance they cause does not rise in step with them, so a price estimated just above the statutory limit should not be carried far from that level in either direction. Applied where it was estimated, on Swiss rail exposure near the statutory limit, it prices exactly the margin the BGLE was built to address.

7 Conclusion

This thesis asks what households pay to avoid rail noise. The existing literature offered credible causal designs or decibel-level variation, but never both in the same study, so no causal price per decibel of rail noise existed. I close that gap with a natural experiment combined with two data sources. The staggered rollout of noise barriers under the BGLE allocated barriers by a federal cost-effectiveness rule rather than by local housing market conditions, which makes for a strong quasi-experimental setup (Swiss Confederation, 2000). I combine it with national rental listings covering 2000 to 2025 (IAZI, 2026b) and a counterfactual acoustic simulation that computes exposure at every apartment with all barriers removed (FOEN, 2026). With this setup I can first identify the causal rent effect of a barrier, and then convert that effect into a price per decibel.

Barriers affect the relative prices in the observed area. In the spatial DiD design, rents within 300 m of a completed barrier rise 3.3 % above the 300–500 m control band, and 4.4 % in the 100 m ring immediately behind it. The gap opens in the year of completion and does not fade over the five years I observe afterwards. It survives estimation on rent per square meter, dropping any single barrier, and estimators robust to staggered timing. The pattern behind the 3.3 % is closer to the closed city prediction than to the open one. Rents in the control band fall by 2.4 % after construction, and that is what happens when the households bidding for the newly quiet units come from the same local market rather than from outside it (Brueckner, 1987). The match is not complete, though, since a closed city also predicts a rise in the treated rings, which the temporal design does not find.

Converted at the noise reduction, the barrier implies a noise depreciation index of 1.0 % per dB. At the mean rent of the ring it comes from, that is about CHF 17 per month. The figure is estimated where it is best identified, in the 0–100 m ring, the only ring in which exposure was reliably above the statutory threshold before construction. It sits at the top of the 0.9 to 1.0 % range Moretti and Wheeler (2025) report for Florida road barriers, the only other causal estimate available, though the two are evaluated at different noise reductions and should not be read as one replicating the other. It is above the Swiss cross-sectional figures, which put the rail discount at 0.26 % per decibel (ZKB and FOEN, 2011) and the discount across all noise sources in Geneva at 0.7 % (Baranzini and Ramirez, 2005). The result is an estimate from the Swiss rental market that converts the modeled reduction of any further measure into a rent equivalent that can be set against its cost.

The price I measure is a relative one. It is what a household gives up to sit behind a barrier rather than in a comparable spot without one. It does not measure the absolute gain to the corridor. So it can rank a further measure against what that measure costs, but it cannot say whether the CHF 1.9 billion the BGLE spent was a good investment. It is also an average within a ring. Treatment intensity is defined by distance alone, so a ring pools apartments the barrier treats very differently: it shields lower floors far more than upper ones, and buildings shield one another. Dividing the rent effect by the average reduction in the ring assumes the response is proportional to the amount of quiet delivered. Dropping that assumption and using apartment-level reduction as the dose itself is the natural next step for future work.

Who ends up holding the value created by the noise barriers is the most surprising result of this thesis. Both the open and the closed city predict that landlords next to a new barrier can charge more. In this setting I find no evidence of that: asking rents in the treated rings do not move in absolute terms after construction. A publicly financed improvement is therefore not capitalized where it was built. The benefit stays with the households who move into the quieter apartments, as a gain in tenant utility rather than in land value. The cost of the adjustment falls on owners a few hundred meters away. They received no shielding from the barrier and have to lower their asking rents to compete with the newly quiet units nearer the track. Swiss tenancy law might partially explain this: landlords face some limitations for rent increases, which might limit their ability to capture the full benefit of barriers. The policy therefore benefits the residents the BGLE was written to protect, rather than the landowners.

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Appendix

A Data

This section defines the two micro-location controls used in the hedonic specification and collects the additional descriptive figures referenced in Section 3.

Micro-Location Score. The micro-location control micro_score is derived from IAZI's amenity scores, which are supplied per location identifier (IAZI, 2026a). IAZI's own total is a weighted sum of the nine sub-scores listed in Table A1. Three of them price noise exposure directly, so retaining them would absorb part of the treatment this thesis estimates. I therefore recompute the index over the six remaining components,

$$\begin{aligned} \text{TotalScore}_i &= \sum_{j \in \mathcal{R} \cup \mathcal{N}} w_j \, s_{ij}, \\ \text{micro\_score}_i &= \text{TotalScore}_i \; - \sum_{j \in \mathcal{N}} w_j \, s_{ij} \; = \sum_{j \in \mathcal{R}} w_j \, s_{ij}, \end{aligned} \tag{6}$$

where s_{ij} is location i's score on sub-score j and w_j the weight IAZI attaches to it, $\mathcal{N} = \{\text{road, rail, aircraft}\}$ collects the three noise components and $\mathcal{R}$ the six retained ones. The data supply the products $w_j s_{ij}$ rather than the two factors separately, so the construction never requires the individual weights: removing the noise terms from the total leaves the weighted sum over $\mathcal{R}$ whatever the w_j happen to be.

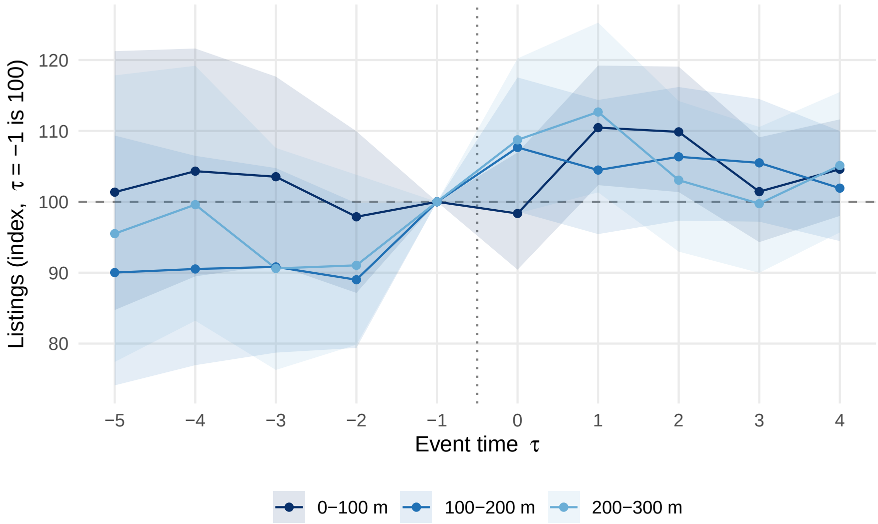
Table A1: Components of the IAZI Micro-Location Score

Sub-score	Captures	In micro_score
Slope	Terrain gradient at the location	Yes
Orientation	Compass orientation of the plot	Yes
Centrality	Proximity to the local center	Yes
Sunshine	Exposure to direct sunlight	Yes
Public transport	Quality of public-transport access	Yes
Rail access	Quality of rail access	Yes
Road noise	Modelled road noise exposure, dB	No
Rail noise	Modelled railway noise exposure, dB	No
Aircraft noise	Modelled aircraft noise exposure, dB	No

Note: The nine sub-scores enter as IAZI's weighted components and their sum is TotalScore. The three noise components are removed by Eq. 6. Source: IAZI (2026a).

Public-Transport Accessibility. The second micro-location control is the ARE public-transport accessibility class (ÖV-Güteklasse) (ARE, 2026). It partitions Swiss territory into polygons ranging from A (very good access) to D (limited access); listings falling outside every polygon are coded 0 and kept as an explicit category rather than dropped, so no listing is lost to the regression. The classification is published annually and each listing is matched to the vintage nearest its listing year over 2012–2026. It is therefore the only time-varying location control in the model, and it enters as a set of dummies with class 0 as the reference.

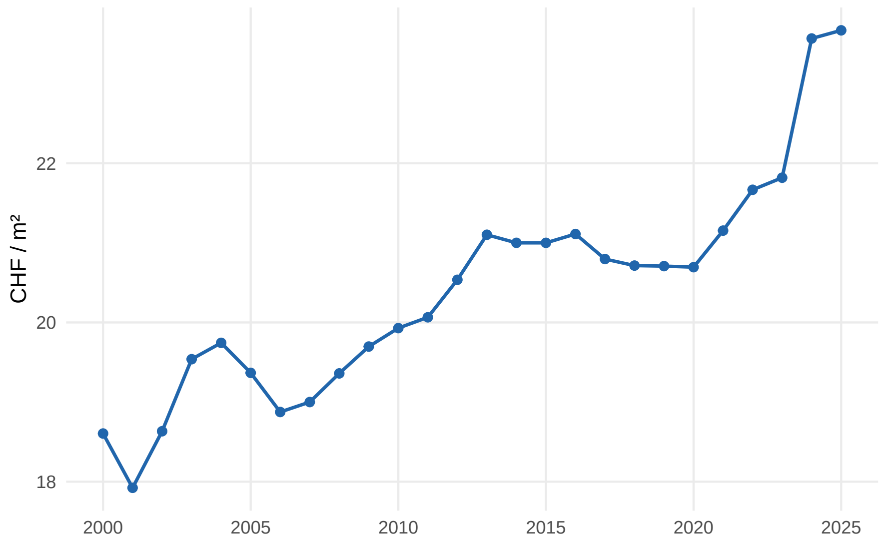
Figure A1: Listing Counts by Ring Around Barrier Completion



Note: Poisson event study on barrier × ring × τ listing counts, with barrier and calendar-year fixed effects; SE clustered on barrier. Index: $\tau = -1$ of the same ring = 100. Shaded area is the 95 % confidence interval. Fixed effects net out each barrier's baseline listing volume and platform-wide growth over time, isolating the within-barrier dynamic effect.

Listing Counts by Ring. Figure A1 asks whether the sample itself responds to treatment, since an apartment enters the data only when it is advertised. Counts in all rings rise after completion and fall back around $\tau = 3$, with the rise slightly larger in the 100–200 m ring. The common part of that response is differenced out by Eq. 3, which compares rings within the same barrier and event time, so only the differential across rings can move $\hat{\beta}_j$. Where that differential shifts the composition of listings along observable characteristics, the hedonic controls absorb it. Selection on unobservables is not absorbed and remains a caveat on the estimates.

Figure A2: Median Rent per m^2, 2000–2025



Note: Annual median of gross rent divided by living surface (CHF/m^2), computed on all Swiss rental listings in the data. Observations with rent per m^2 outside $[3, 100]$ excluded. Source: IAZI (2026b).

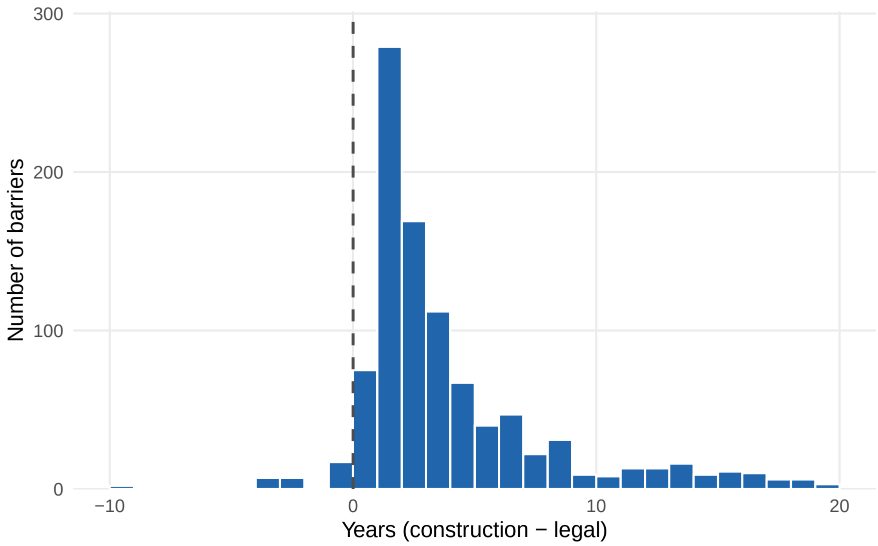
Median Rent per Square Meter. Figure A2 shows the market-wide movement in the rent level over the sample period. The median climbs from just under CHF 18 per m^2 in 2001 to about CHF 23.5 in 2025, and it does so unevenly: a dip in the mid-2000s, a plateau between 2013 and 2020, and a sharp acceleration after 2021. Barriers are completed at different dates, so treated and control listings are drawn from different parts of this path. The barrier $\times$ event-time fixed effects in Eq. 3 remove this bias from the estimates.

B Robustness

This section collects the robustness tests referenced in Sections 3 through 6.

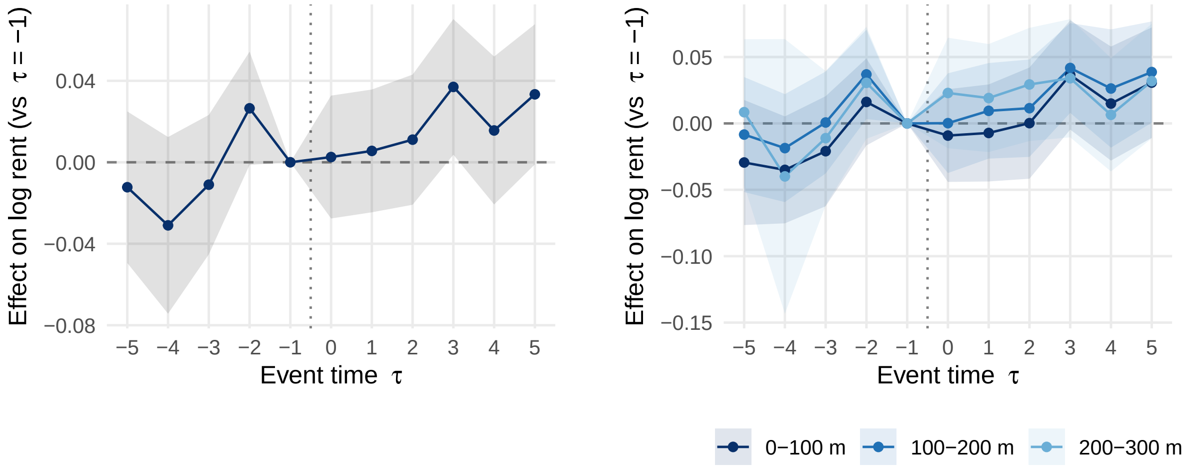
Anticipation. Barriers are legally approved several years before they are built, so the market could in principle react to approval rather than to completion. The two figures below date the lag and trace the estimated event study.

Figure A3: Years Between Legal Approval and Construction



Note: Distribution of the duration between legal approval and barrier construction, one observation per barrier, for barriers completed between 2000 and 2020. Positive values mean construction followed legal approval. The few negative observations are either due to retrospective approval or a data error. Source: FOT (2026a).

Figure A4: Event Study: Legal Approval Year as Event Time

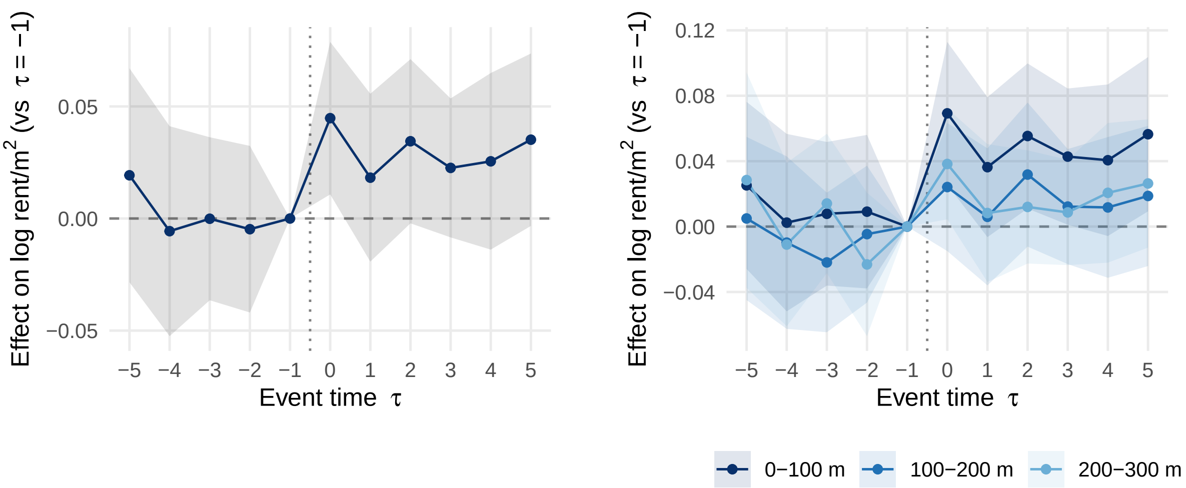


(a) Pooled (0–300 m vs. 300–500 m) (b) By distance ring

Note: Same specification as Figure 7, with τ dated off legalapproval instead of barrierconstruction. Dashed vertical line marks legal approval. Shaded bands are 95 % confidence intervals from SEs clustered on barrier.

Figure A3 dates the gap: the median barrier is completed three years after approval, so the two events can be distinguished in event time. Re-dating event time to the approval year lowers the pooled effect from 3.3 to 2.1 % and leaves the event study flat until the median lag at $\tau = 3$ before it rises (Figure A4). Together they point the same way: rents respond to construction rather than to the announcement of it.

Figure A5: Event Study: Log Rent per m^2

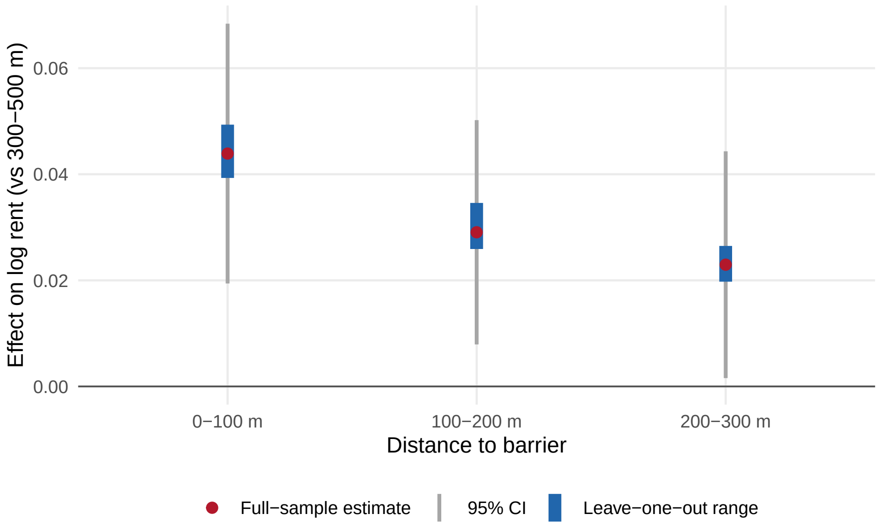


(a) Pooled (b) By distance ring

Note: Same specification as Figure 7, with log(grossRent/surfaceLiving) as the outcome. Shaded bands are 95 % confidence intervals from SEs clustered on barrier.

Rent per Square Meter. Figure A5 re-estimates the specification of Figure 7 on rent per square meter, which nets out the tendency of smaller apartments to sit closer to the track (Section 3.3). The pooled effect falls slightly to 2.9 % and the ordering across rings is unchanged, so the premium is not an artifact of size sorting.

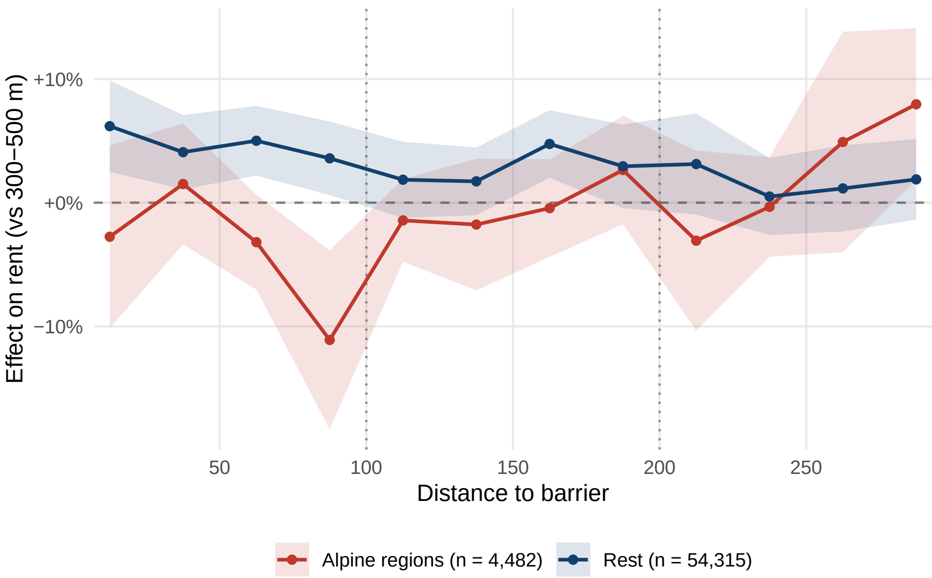
Figure A6: Leave-One-Barrier-Out Robustness of the Spatial Specification



Note: The spatial specification of Table 3 is re-estimated once per barrier, each time dropping that barrier from the sample. Red points are the full-sample estimates and thin grey lines their 95 % confidence intervals, from SEs clustered on barrier; thick blue bars span the minimum and maximum estimate across all refits.

Influence of Individual Barriers. The barriers differ widely in size and setting, so the estimate could theoretically be driven mainly by a few unusually large sites with an unusual effect rather than from the sample as a whole. Figure A6 shows it does not: dropping any single barrier moves each ring effect by only a fraction of its confidence interval.

Figure A7: Rent Effect by Distance to Barrier, Alpine Regions vs the Rest of Switzerland



Note: Panel (1) of Figure 8 re-estimated separately on the three alpine labor-market regions (Berner Oberland, Ostalpen, Westalpen) and on the rest of the country, with the same 25 m bins, the same 300–500 m control band and the same fixed effects. Dotted vertical lines mark the ring boundaries of the main specification. Shaded bands are 95 % CIs from SEs clustered on barrier.

Regional Heterogeneity and the Distance Profile. Part of the uneven profile in Figure 8 is regional. Outside the Alps the effect is positive in every bin and close to monotone; in the three alpine labor-market regions it is negative across most of the treated band and lowest near 100 m, around where the pooled series dips. That only explains part of it, since the non-alpine line still rises between 100 and 150 m. The rest is most likely measurement error.

Distance predicts the mean noise reduction well, as panel (2) of Figure 8 shows, but it says much less about any single apartment. Within the first 100 m, some apartments get louder while others get more than 15 dB quieter (Figure 4). Lower floors gain far more than upper ones, buildings shield one another, and barriers differ in height and length. A distance ring therefore averages over apartments the barrier treats very differently, and that spread within each ring is a plausible reason the rent effect does not decline smoothly. Identifying the effect from floor-level variation within a building would be a natural direction for further research.

Cohort effects and estimator choice. Because treatment timing is staggered, plain TWFE can be biased by forbidden comparisons whenever effects differ across cohorts

Table A2: Cohort-robustness of the temporal ATT: Different estimators

Estimator	ATT	SE	p	N	Controls
TWFE (plain)	-0.001	(0.005)	0.82	56,200	full
Sun-Abraham	0.000	(0.005)	0.99	56,200	full
Wooldridge ETWFE	0.006	(0.014)	0.67	51,819	reduced

Note: Cohort-robustness of the temporal (not-yet-treated) design. Outcome log(gross rent), pooled 0–300 m treated band (the three 100 m rings), barrier cohorts 2004-2020 with at least 20 listings, +-5 y window. Barrier + year FE; SE clustered on barrier (in parentheses). TWFE and Sun-Abraham use the full hedonic controls; Wooldridge ETWFE uses the reduced continuous controls, as it interacts each control with cohort x period. Stars: *** p<0.01, ** p<0.05, * p<0.1. FE here is barrier + year (not barrier x ring, as in the main not-yet-treated spec) and the sample is cohort-restricted, so the TWFE row differs slightly from the main text's pooled not-yet-treated estimate; both are statistically indistinguishable from zero. Outcome is log rent, so coefficients (ATT) are approximately proportional effects.

or evolve over event time. Table A2 re-estimates the temporal ATT with the Sun and Abraham (2021) and Wooldridge (2025) estimators: all three stay close to zero and none is significant, so the null of Section 5.2 is not an artifact of the estimator.

C Measuring the Gap between the Designs

The control band in absolute terms. Eq. 3 identifies a difference between the treated rings and the 300–500 m control band, so a positive $\hat{\beta}_j$ is equally consistent with rents rising in the treated rings, falling in the control band, or both. Section 5.2 settles the first half: the treated rings do not move. Table A3 supplies the second, estimating Eq. 4 on the control band alone and keeping the window, the controls, the fixed effects, and the clustering of the treated-band estimate so that the two are comparable. Rents in the control band fall by 2.4 % after construction.

Table A3: Temporal Specification Estimated on the Control Band Alone

Dependent Variable:	Log gross rent
Model:	(1)
Variables	
Post	-0.0240**
	(0.0094)
Fixed-effects	
Barrier-Ring	Yes
Year	Yes
Fit statistics	
Observations	19,159
R^2	0.76538

Note: Coefficients from Eq. 4 estimated on the 300–500 m control band alone, on the same ±5-year window as the treated-band columns. SEs clustered on barrier in parentheses. FE: barrier × ring and calendar year. Outcome is log rent, so coefficients are approximately proportional effects. Stars: *** p<0.01, ** p<0.05, * p<0.1.

What separates the two designs. The spatial design and the two temporal designs together measure the same treated-versus-control contrast. But they do not return the same number, and Table A4 closes the distance between them in steps. Rows (1) and (2) are the two temporal estimates and row (3) the contrast they imply; rows (4) and (5) fit that same contrast jointly on both bands, under the two sets of fixed effects the designs use. Reduced fixed effects are barrier × ring and calendar year, all that Eq. 4 can hold; standard fixed effects are barrier × ring and barrier × event-time, as in Eq. 3. Of the 0.85 percentage points separating the two designs, 0.63 come from the fixed effects and the remainder from the split estimation, so moving from the reduced to the standard set raises the measured contrast by roughly a fifth. Re-estimating row (4) on the sample of row (5) leaves it at 2.7 %, so this is the fixed effects and not the observations they drop.

Table A4: Spatial and temporal estimates of the same contrast

Specification	Estimate	SE	N
(1) Temporal, treated band (0–300 m)	0.0005	(0.0051)	57,401
(2) Temporal, control band (300–500 m)	-0.0240**	(0.0094)	19,159
(3) Temporal contrast, (1) − (2)	0.0244		
(4) Spatial, reduced FE	0.0266***	(0.0084)	76,560
(5) Spatial, standard FE	0.0329***	(0.0101)	75,670
(6) Design gap, (5) − (3)	0.0085		

Note: Pooled estimates only, all on the ±5y window with the same hedonic controls and SE clustered on barrier. Rows (1) and (2) estimate the temporal specification separately on the treated band and on the control band (FE: barrier×ring + calendar year); row (3) rebuilds the treated-versus-control contrast from them. Rows (4) and (5) fit that contrast jointly on both bands: row (4) under calendar-year fixed effects (reduced, the set the temporal design is restricted to) and row (5) under barrier×ring + barrier×event-time fixed effects (standard, the spatial specification of the main text). Of the gap in row (6), 0.0022 separates rows (3) and (4) and 0.0063 rows (4) and (5), so the larger part is the fixed effects rather than the split estimation. Stars: *** p<0.01, ** p<0.05, * p<0.1. Outcome is log rent, so entries are approximately proportional effects; kept in raw log points here since the table decomposes a difference between two designs, not a single estimate.

Rows (4) and (5) differ only in fixed effects. This invites the question whether the flat treated rings of row (1) are an artifact of the reduced set. That check cannot be run: $\mathbf{1}\{\tau \geq 0\}$ is a function of τ, so barrier × event-time fixed effects absorb it, and no specification can measure a band's own movement while holding them constant. What we can say, though, is that attributing the entire 0.63 percentage points to the treated rings lifts their coefficient from 0.05 % to roughly 0.7 %. That is the most generous reading available to that explanation, and it still leaves the treated rings indistinguishable from zero.

D Tools and Resources Used

Table A5: Software and Tools Used in This Thesis

Task	Software / Tool	Note
Data analysis	R [4.5.3] in RStudio [2026.01.1+ 403]	Data cleaning, the spatial join described in Section 3.2, the DiD estimation (`fixest`, `did`, `etwfe`), and all figures and tables.
Coding	Claude (Anthropic)	Assisted in writing, debugging, and documenting the R data pipeline and analysis scripts.
Document preparation	Overleaf (LaTeX), GitHub	Compiling, spelling and reference checks, and version control between the analysis repository and this document.
Writing and editing	Claude (Anthropic) and Gemini (Google)	Assisted in drafting and formatting the LaTeX-document, editing prose throughout.

Note: This table lists the software and tools used in the preparation of this thesis.

Declaration of Independent Work

I hereby declare that I have written this thesis without any help from others and without the use of documents and aids other than those stated above. I have mentioned all used sources and cited them correctly according to established academic citation rules. I am aware that otherwise the Senat is entitled to revoke the degree awarded on the basis of this thesis, according to article 36 paragraph 1 letter r of the University Act from 5 September 1996.

Bern, August 30, 2026

Samuel Bächli