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Expensive pieces of information: school renovation as market signals in housing markets

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Abstract

This paper examines heterogeneous capitalization effects of school renovations on residential property prices in Berlin. House prices in cities reflect object and local amenity qualities, however, information asymmetries about both qualities persist. Given that school is a decisive amenity for families to choose homes, we hypothesize that investments in physical school infrastructure can serve as a signal to the value of the neighbourhood. Then, access to information about school investments can lead to different price effect, with migrant families with less access to other pieces of information valuing this visible investment more than residents. Using a new dataset on school renovations in Berlin, we analyze whether capitalization patterns vary systematically with the socioeconomic and ethnic composition of neighborhoods. Employing a difference-in-differences approach with the Sun and Abraham estimator, we find a positive average treatment effect of approximately 4% on nearby residential property prices, with effects emerging gradually over a period of two to three years, consistent with slow information diffusion. The effects are substantially stronger in low-income and high-migrant-share neighborhoods, offsetting existing price discounts and highlighting how visible infrastructure improvements function as salient neighborhood quality signals in areas characterized by greater informational constraints. Differentiating between ethnic origin groups points to considerable heterogeneity in capitalization effects, consistent with differences in local information access and neighborhood embeddedness across groups. The findings contribute to the understanding of how urban infrastructure investments interact with demographic composition and informational constraints to shape housing market dynamics, with implications for the design of equitable urban policies.

JEL: R21, R31, I22, H41, J15, I28

Keywords: School Infrastructure, Housing Capitalization, Information asymmetry, Local networks and integration, Difference-in-differences

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1 Introduction

Local amenities significantly affect housing prices in surrounding neighborhoods, and there is ample evidence that schools constitute one such amenity for families. At the same time, housing markets are characterized by persistent information asymmetries. Even in the age of digital data abundance, residents and newcomers differ in their access to local knowledge, where migrants and foreigners often face informational disadvantages when evaluating neighborhood quality and therefore tend to pay higher prices than locals. These information gaps partly explain why migration processes tend to follow established pathways: lacking access to formal and informal local knowledge, newcomers rely on co-ethnic networks as a primary source of residential guidance, reinforcing spatial clustering in neighborhoods where such networks are already present.

In this context, school renovations may play an important role as visible and easily observable signals of neighborhood quality upgrades. By improving the physical quality and capacity of local schools, investments in school infrastructure can affect nearby housing prices through three distinct channels. First, and most directly, expansions in school capacity increase the supply of accessible school places, raising the attractiveness of the surrounding neighborhood and generating additional residential demand. Second, visible improvements to school buildings serve as a signal of school quality to prospective buyers who lack direct information about educational outcomes, making infrastructure upgrades an observable and easily interpretable proxy for local educational provision. Third, and more broadly, public investment in school infrastructure signals that the city is committed to upgrading the neighborhood, functioning as a credible quality indicator for the area as a whole. While the first channel operates through straightforward demand effects, the second and third are inherently informational: they work precisely because they reduce uncertainty about neighborhood quality in contexts where such information is otherwise difficult to obtain. While average capitalization effects are well documented (Black, 1999; Nguyen-Hoang and Yinger, 2011; Cellini et al., 2010; Neilson and Zimmerman, 2014), these effects are unlikely to be spatially homogeneous. Instead, price responses may differ systematically across neighborhoods depending on their socioeconomic and demographic composition.

A key mechanism underlying such heterogeneity is information asymmetry in housing markets. Buyers typically differ in their access to local knowledge about school quality, peer composition, and neighborhood trajectories, and this gap has measurable price consequences: non-local and informationally disadvantaged buyers consistently pay higher prices for comparable properties than locally embedded residents (Kandlbinder, 2018). Particularly non-residents/migrants have less access to formal an informal pieces of information. In this context, visible school infrastructure improvements - such as renovated buildings, expanded facilities, or modernized classrooms - can act as salient and easily observable signals of neighborhood quality. The extent to which these signals are incorporated into housing prices is therefore likely to vary across neighborhoods with different income levels and migration compositions, reflecting differences in information access and market dynamics.

This paper examines how school renovations are capitalized into housing prices in Berlin, with a particular focus on heterogeneity by migration background and income. We build on Dietl et al. (2025), who document average capitalization effects of school renovations using spatial panel models, by shifting attention to who benefits most from these investments and why effects differ across neighborhoods. This leads us to two related research questions. First, does the capitalization of school renovations into residential property prices vary systematically with the socioeconomic and demographic composition of neighborhoods? Second, are price effects strongest in neighborhoods where migrant shares are high and access to local information networks is most limited? Our central hypothesis is that price responses are stronger in settings where buyers face greater information constraints, making visible infrastructure investments particularly informative in the housing market.

Berlin provides a well-suited context for studying these mechanisms due to its pronounced socioeconomic and demographic heterogeneity. Official statistics indicate that 41.7% of the city's residents have a migration background,[1] with substantial spatial variation across districts and neighborhoods (Amt für Statistik Berlin-Brandenburg, 2025). Turkish-origin residents, who account for roughly 29% of the migrant population, largely arrived during the 1960s and 1970s through guest worker programs and exhibit long-standing settlement patterns and dense local networks (Schönwälder and Söhn, 2009). In contrast, migrants from Eastern Europe - around 13 % of migrants - arrived predominantly after EU enlargement in 2004 and tend to have shorter residence durations and weaker local information networks (Glorius et al., 2013; Nowicka, 2018). While these differences are consistent with heterogeneous information frictions, they may also reflect variation in socioeconomic composition, residential sorting, and housing demand across migrant groups.

What also makes Berlin a valuable subject of study is that we can exploit the Berliner Schulbauoffensive, a large-scale public investment program launched in 2017 that allocates approximately €5.5 billion to school renovations, extensions, and new construction through 2027 (Land Berlin, 2024a). Using a difference-in-differences design with the Sun–Abraham estimator, we analyze condominium transaction prices between 2013 and 2022 within a 300-meter radius of renovated schools, linked to detailed renovation data and neighborhood characteristics.

We document four main findings. First, school renovations increase nearby housing prices by around 4 % on average, with effects emerging only after a delay of approximately two to three years, consistent with gradual information diffusion. Second, capitalization effects are substantially larger in neighborhoods with higher migrant shares, largely offsetting pre-

[1] According to the Amt für Statistik Berlin-Brandenburg (2025), "migration background" includes foreign citizens, naturalized individuals, and persons with at least one parent not born with German citizenship. These official statistics are therefore not directly comparable to the migration measures used in this paper, which are based on microm origin-group classifications derived from estimated linguistic origin.

existing price discounts associated with migration concentration. Third, renovation effects are particularly strong in areas with higher shares of Eastern European migrants compared to neighborhoods with higher Turkish-origin shares - a pattern consistent with differences in informational access and local network structures across migrant groups, reflecting the distinct migration histories outlined above: while Turkish-origin residents arrived predominantly during the 1960s and 1970s and have since developed dense co-ethnic networks and neighborhood embeddedness, Eastern European migrants whose settlement in Berlin accelerated primarily after EU enlargement in 2004, have had less time to accumulate local knowledge and social capital. Fourth, effects are significantly larger in low-income neighborhoods than in high-income areas, suggesting that school infrastructure investments may reduce spatial disparities in housing market outcomes. These findings contribute to the literature by showing that visible investments in school infrastructure generate economically meaningful housing market responses in settings characterized by persistent information asymmetries.

The remainder of the paper proceeds as follows. Section 2 reviews the related literature on school capitalization, information frictions, and migration. Section 3 describes the data and institutional background. Section 4 outlines the empirical framework. Section 5 presents the results and robustness checks. Section 6 concludes.

2 Review of Prior Research

Research on school capitalization shows that school quality is a key residential amenity whose capitalization into housing prices depends critically on information and sorting. The dominant strand of this literature measures school quality through educational outcomes — most commonly standardized test scores. Boundary discontinuity designs document sizable effects at attendance-zone borders, with property values rising by about 2.5% per 0.1 standard deviation gain in test scores (Black, 1999), while meta-analyses consistently report housing premia of 2–5% per standard deviation improvement (Nguyen-Hoang and Yinger, 2011). More recent work confirm these patterns across a range of institutional settings and estimation strategies (Collins and Kaplan, 2017; Bayer et al., 2007). Crucially, however, this literature focuses almost exclusively on school quality as measured by student performance, not on the physical condition of school buildings. Studies explicitly examining school infrastructure investments are comparatively rare, and include Cellini et al. (2010) for California school bonds, Neilson and Zimmerman (2014) for school construction in New Haven, Conlin and Thompson (2017) for U.S. school renovations, and Lafortune and Schönholzer (2022) for Los Angeles facility investments. Our paper contributes to this smaller but growing strand by focusing on building renovations as observable neighborhood quality signals, rather than on educational outcomes. A key reason why capitalization effects vary is that school quality information is imperfect and unevenly distributed: structural and reduced-form studies document large parental information gaps and show that credible quality signals, such as ratings, inspections, or performance data, shift school choices and investments, particularly among less-informed or disadvantaged households (Hastings and Weinstein, 2008; Bergman et al., 2021; Greaves et al., 2023).

Salient school quality information can influence residential choices and housing price capitalization. Experimental and quasi-experimental studies document large effects of school quality disclosure on neighborhood choice and housing prices, with price increases of 4.2 % in high-performing districts and dynamics unfolding over several years (Davis et al., 2020; Lu et al., 2023). These results highlight an informational capitalization channel highly relevant for school renovations as localized quality signals, with locals typically possessing superior neighborhood information (Garmaise and Moskowitz, 2004; Kurlat and Stroebel, 2015). Qualitative evidence from Berlin further suggests that schools function as symbolic drivers of neighborhood valuation, making them particularly salient signals for prospective buyers (Kadıoğlu, 2021).

School infrastructure investments may coincide with neighborhood upgrading and sorting. Evidence from U.S. programs documents property value increases of 6–7 % alongside substantial displacement pressures (Hussain, 2023; Biasi et al., 2024). For the Berlin context specifically, Dietl et al. (2025) document positive average capitalization effects of school renovations on nearby residential property prices using spatial panel models, finding that effects attenuate with distance and are strongest within a 300-meter radius. Building on these findings, the present paper shifts attention from the spatial propagation of amenity effects to heterogeneous responses across resident groups, employing a difference-in-differences framework that allows us to trace the timing of capitalization and its systematic variation with neighborhood socioeconomic and demographic composition. While we do not directly measure displacement, observed price responses may reflect both amenity gains and sorting dynamics.

While information frictions are well documented, their systematic variation across locals and migrants and implications for housing price capitalization remains less understood. Research on immigrant integration suggests that local knowledge accumulates gradually, improving residential outcomes over time (Iceland and Scopilliti, 2008). Research on educational choice shows that migrants often lack institutional knowledge about school systems, with informational gaps varying across origin groups - particularly in tracked systems such as Germany's (Tjaden, 2020; Zimmermann, 2024). Consistent with this, less-informed parents are disadvantaged in school choice markets, and immigrant parents frequently lack local system knowledge, creating an uneven playing field (Kloosterman and Troyan, 2021; Bunar and Ambrose, 2016).

In Berlin, Turkish-origin communities display strong settlement persistence in districts such as Kreuzberg and Neukölln, dating to 1960s–1970s guestworker recruitment (Schönwälder and Söhn, 2009). These dense co-ethnic networks facilitate within-group information flows but also segment information relative to natives and other migrants. Consistent with this, Zimmermann (2024) documents that knowledge gaps about the German tracked school system vary substantially across migrant origin groups, pointing to systematically uneven informational barriers in educational decision-making among immigrant households.

Ethnic networks shape information flows and residential sorting. Co-ethnic ties reduce information costs and steer location choices (Massey, 1990), and migrant outcomes depend critically on destination network size (Munshi, 2003). For our purposes, this implies a testable prediction: households embedded in dense, long-established co-ethnic networks - such as Turkish-origin residents in Berlin - have greater access to informal neighborhood knowledge and are therefore less reliant on visible infrastructure signals when forming housing valuations. Conversely, more recently arrived groups with weaker local networks - such as Eastern European migrants - face higher informational costs and should respond more strongly to salient quality signals such as school renovations.

Direct effects of immigration on housing prices provide important context. Sá (2015) show that immigrant inflows into Spanish provinces raise house prices through both direct demand and induced native relocation, while Braun et al. (2025) provide causal evidence from Denmark that refugee inflows increase rental and house prices at the municipal level. These demand pressures interact with school quality perceptions: immigration can raise housing demand but also trigger native avoidance if immigrant concentration signals lower school quality (Rogne et al., 2025; Boustan et al., 2024), even when actual peer effects are neutral or positive after accounting for sorting (Figlio et al., 2024). Accordingly, capitalization of school renovations likely depends not only on average demand shifts but also on informational access and local demographic composition.

In order to quantify the effects appropriately, we rely on a recent strand of econometric literature: Recent work shows that two-way fixed-effects (TWFE) event-study estimators can be biased under staggered adoption with heterogeneous effects, as already treated units serve as controls for later-treated units, distorting dynamic estimates (Sun and Abraham, 2021). Sun and Abraham (2021) propose a cohort-specific event-study estimator using only not-yet-treated or never-treated controls, yielding consistent dynamic treatment effects.

Related work develops clean-control estimators and documents misleading TWFE weights under heterogeneity (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021). We rely on this framework, which delivers an event-study representation while avoiding contaminated comparisons.

These considerations are central for the Berlin school renovation program, where renovations occur at different times across schools and effects are likely dynamic as information diffuses and households adjust residential choices gradually. Accordingly, our baseline specifications employ the Sun–Abraham estimator to recover dynamic treatment effects while allowing for heterogeneous responses across household types and migration backgrounds.

3 Data

We use three datasets: transaction-level condominium sales from the Berlin Committee of Valuation Experts (Gutachterausschuss für Grundstückswerte, GAA) (Gutachterausschuss für Grundstückswerte in Berlin, 2023), neighborhood socioeconomic characteristics from the RWI–Geo–Grid (RWI – Leibniz-Institut für Wirtschaftsforschung, 2023), and school renovation records from the Berliner Schulbauoffensive (Senatsverwaltung für Bildung, Jugend und Familie, 2024). The transaction-level data covers all notarized condominium sales between 1984 and 2022. For the period 2013–2022, this yields 142,959 geocoded condominium transactions. Following Dietl et al. (2025), we clean the data with regard to outliers and missing values and restrict the estimation sample to transactions located within 300 meters of a public school and observed within an event-time window of $-5 \leq t \leq 5$ years around renovation completion (with staggered timing across schools). This spatial and temporal linkage reduces the analysis sample to 22,854 transactions.

Neighborhood characteristics, a nationwide 1-km^2 raster dataset, cover the period 2009–2021. Raster-cell averages are assigned to each transaction and school location based on geographic overlap. We use purchasing power per capita and the number of households to capture local income levels and residential density, and we construct income-tercile indicators and child-density measures from these variables (Dietl et al., 2025).

As this paper explicitly analyzes migration-related neighborhood characteristics, we additionally account for both the overall migrant share and the origin composition of local migrant populations, in particular the shares of residents with Turkish and Eastern European backgrounds. We focus on these two groups because they represent quantitatively large migrant communities in Berlin, while also differing substantially in migration history, duration of residence, and likely access to local information networks. These measures allow us to study heterogeneous capitalization effects of school renovations across neighborhoods that differ not only in income but also in demographic composition.

The Berliner Schulbauoffensive provides detailed records on renovation projects across all public schools in Berlin. In Berlin, residential location is particularly salient for primary-school access because enrollment is generally tied to the local catchment area (Einschulungsbereich), and attending a different school is typically only possible conditional on available capacity. This institutional feature implies that improvements to nearby primary-school infrastructure are more likely to be capitalized into local house prices, as the relevant school option is geographically anchored. We therefore use school-district (catchment-area) fixed effects as a fine-grained spatial control that absorbs persistent within-city differences in school environments and neighborhood sorting beyond administrative districts. A limitation is that this mechanism is weaker for secondary schools, where admission follows a choice-based allocation procedure. Hence, catchment-area fixed effects primarily strengthen identification for primary-school-related exposure while serving as a general small-area location control for other school types.

Table 3.1 reports summary statistics for property, locational, and neighborhood characteristics, as well as migration and origin-group variables. Table 3.2 summarizes the analyzed public schools and renovation projects. These data sets provide a unified and consistent basis for analyzing heterogeneous treatment effects by migration share and origin composition.

Tables 3.1 and 3.2 present summary statistics for our analysis sample of 142,959 condominium transactions observed in Berlin from 2013 to 2022. The average transaction price is €274,016, with substantial variation (SD: €204,836).

Our sample neighborhoods[2] also show substantial variation in the share of residents classified into analyzed migrant origin groups in the RWI–Geo–Grid data. On average, these groups account for 11.0% of residents, with considerable variation across neighborhoods (SD: 7.5 percentage points; range: 1.3–36.3%) (cf. Figure 3.1).[3] Figure 3.1 illustrates the spatial clustering of migrant origin groups across Berlin neighborhoods, particularly in the city's inner districts. Following the origin-group classification provided in the socioeconomic data set,[4] we distinguish between three broad migrant origin groups. The largest group consists of residents of Turkish origin (29.1% of all migrants). A second group comprises residents classified into Eastern European origin groups (12.9%), including migrants associated with Poland, Russia, Ukraine, Belarus, the Baltic states, Romania, Bulgaria, the Czech Republic, Slovakia, and Hungary. The remaining 57.3% comprise all other origin groups, including Southern European, Balkan, Middle Eastern/North African, Sub-Saharan African, Asian, and ethnic German repatriate backgrounds, which serve as the reference category in the empirical analysis. The large variation in neighborhood migration composition allows us to test whether school renovation effects differ by the origin composition of the local migrant population.

Neighborhoods also vary considerably in socioeconomic status. Purchasing power per capita averages €34,076 (SD: €7,674). We classify neighborhoods into three income groups of equal size: low-income (<€28,529 per capita), middle-income (€28,529–35,826 per capita), and high-income (>€35,826 per capita). The unemployment rate averages 14.6% (SD: 5.3 percentage points), higher than the German average and varying substantially across neighborhoods (range: 0–25.7%). Single-person households make up 56.1% of all households on average (SD: 17.9 percentage points), typical for a large European city.

School renovations vary considerably in scale, school type, and timing. Renovation projects range from minor upgrades to large-scale modernization programs, with project costs averaging €9.2 million and ranging from almost zero to €77.8 million. The renovation sample includes 324 schools completed between 2013 and 2022, while additional projects were either completed before the observation period or remained ongoing or planned after 2022. The treated schools

[2]Berlin comprises twelve administrative districts: Mitte, Friedrichshain-Kreuzberg, Pankow, Charlottenburg-Wilmersdorf, Spandau, Steglitz-Zehlendorf, Tempelhof-Schöneberg, Neukölln, Treptow-Köpenick, Marzahn-Hellersdorf, Lichtenberg, and Reinickendorf

[3]The migration measures in the RWI–Geo–Grid are based on aggregated microm origin-group classifications derived from estimated linguistic origin and are therefore not directly comparable to official statistics on migration background.

[4]The migration variables in the RWI–Geo–Grid are based on aggregated origin and language groups provided by microm and rely on an "estimated linguistic origin" classification derived from names rather than official nationality records.

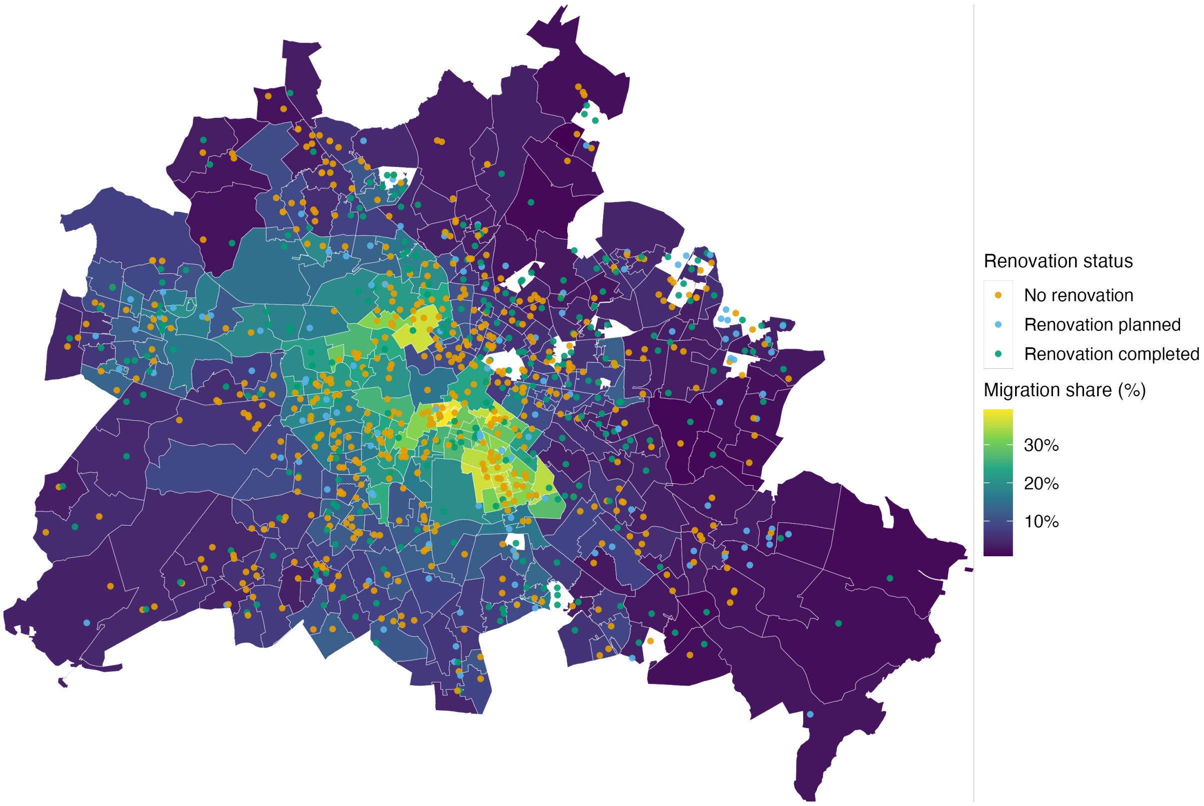

Figure 3.1: Spatial distribution of School districs and migration-related classification in Berlin.

also differ substantially by educational function within the German tracked school system. Primary schools (*Grundschulen*), which provide elementary education, account for 34.3% of the analysis sample, followed by secondary schools (22.2%), academic-track high schools (*Gymnasien*, 18.8%), special-needs schools (14.8%), and vocational schools (7.1%). This variation in timing, project scale, and institutional function generates substantial heterogeneity in treatment exposure across Berlin neighborhoods.

Table 3.1: Summary Statistics: Property and Transaction Characteristics

Variable	Mean	Median	SD	Min	Max
Transaction Characteristics					
Transaction price (€)	274,016	216,476	204,836	50,000	1,500,000
Price per m^2 (€/m^2)	3,679	3,388	1,829	0	21,655
Log(price)	12.29	12.29	0.68	10.82	14.22
Structural Characteristics					
Floor area (m^2)	72.49	66.00	30.85	20.00	296.82
Year built	1,955	1,959	46	1,775	2,025
Floor level	2.25	2.00	1.91	−1	25
Balcony (dummy)	0.65	1.00	0.48	0	1
Elevator (dummy)	0.45	0.00	0.50	0	1
Parking (dummy)	0.08	0.00	0.26	0	1
Locational Characteristics					
Distance to water (m)	721	636	493	5	2,540
Distance to tram (m)	3,331	2,129	3,516	9	21,728
Distance to bus (m)	194	165	133	5	1,001
Distance to restaurants (m)	169	118	164	0	2,070
Distance to nearest school (m)	323	279	206	0	3,228
Neighborhood Characteristics (1-km^2 grid)					
Purchasing power per capita (€)	34,076	31,752	7,674	0	65,935
Log(purchasing power)	10.41	10.37	0.27	−4.61	11.10
Number of households	5,692	5,117	3,371	−1	13,591
Log(households)	8.40	8.54	0.82	0.00	9.52
Share of children (0–17 years)	0.24	0.24	0.03	−1.00	0.48
Low-income area (dummy)	0.33	0.00	0.47	0	1
High-income area (dummy)	0.33	0.00	0.47	0	1
High-children area (dummy)	0.50	0.00	0.50	0	1
Share with migration background (%)	11.01	8.50	7.47	1.27	36.30
Turkish origin (% of migrants)	29.13	24.02	17.71	0.00	79.79
Eastern European origin (% of migrants)	12.89	12.97	4.22	0.00	34.09
Other origins (% of migrants, ref.)	57.34	59.83	15.22	17.44	94.13
Unemployment rate (%)	14.56	15.09	5.25	0.00	25.74
Share of single-person households (%)	56.13	62.11	17.91	0.00	87.22

Note: Summary statistics for 142,959 condominium transactions, observed 2013–2022. Income terciles based on purchasing power per capita: low-income (<28,529 €/capita), middle-income (28,529–35,826 €/capita, reference), and high-income (>35,826 €/capita). High-children areas are neighborhoods above the median share of children aged 0–17. Migration background includes all residents not of German origin. Origin shares sum to 100% within migrant population. Turkish origin: Turkey. Eastern European origin: Poland, Russia, Ukraine, Belarus, Estonia, Latvia, Lithuania, Romania, Bulgaria, Moldova, Czech Republic, Slovakia, Hungary. Other origins (reference): Southern Europe (Italy, Greece, Spain, Portugal), Balkans (Serbia, Croatia, Bosnia-Herzegovina, Montenegro, North Macedonia, Slovenia, Kosovo), late repatriates (ethnic Germans from former Soviet Union), Middle East/North Africa (Syria, Iraq, Lebanon, Jordan, Egypt, Morocco, Tunisia, Algeria, Iran, Afghanistan), Sub-Saharan Africa (Nigeria, Ghana, Cameroon, Kenya, Somalia, Eritrea, Ethiopia), Asia (China, Vietnam, Thailand, India, Pakistan, Korea, Japan, Philippines), and other countries (Americas, Oceania). All monetary values in 2022 euros.

Table 3.2: Summary Statistics: Schools and Treatment Characteristics

Variable	N	Share/Value
Panel A: School Universe		
Total public schools in Berlin	945	100.0%
Renovation Timeline		
Renovations completed before 2013	267	28.3%
Renovations completed 2013–2022 (analysis sample)	324	34.3%
Renovations ongoing or planned after 2022	152	16.1%
No renovations (within observed period)	202	21.4%
Total schools with renovations	743	78.6%
Panel B: School Types (Analysis Sample, N=324)		
Grundschule (Primary school)	111	34.3%
Sekundarschule (Secondary school)	72	22.2%
Gymnasium	61	18.8%
Förderschule (Special-needs school)	48	14.8%
Berufsschule (Vocational school)	23	7.1%
Andere (Other)	9	2.8%
Panel C: Transaction-Level Exposure		
Total transactions (Berlin, 2013–2022)	142,959	100.0%
Treatment Sample (within 300m, $-5 \leq t \leq 5$)		
Treated transactions	22,854	16.0%
Before renovation ($t < 0$)	15,189	66.5%
After renovation ($t \geq 0$)	7,665	33.5%

Note: Panel A reports all 945 public schools in Berlin with renovation timeline. The analysis sample consists of 324 schools that completed renovations between 2013 and 2022, representing 34.3% of all schools and capturing both pre-BSO baseline activity (2013–2016) and BSO rollout phase (2017–2022). Panel F reports transaction-level exposure. Treatment defined as properties within 300m of renovated schools observed in event-time window $-5 \leq t \leq 5$ years around completion. School types and project types calculated relative to analysis sample (N=324). Before/after timing calculated for treated transactions only.

An overview of the dependent and explanatory variables used in the empirical analysis, including their temporal coverage, sources, and spatial levels, is provided in Appendix Table A.1.

4 Empirical Analysis

We estimate the heterogeneous effects of school renovations on nearby housing prices, focusing on variations by migration share, income segregation, and origin groups in Berlin. The DiD approach allows us to examine temporal dynamics and heterogeneity in treatment effects precisely, particularly in the context of informational asymmetries between native Germans and migrant populations. We take advantage of the staggered treatment timing in schools, where treatment is defined as the year of completion of the renovation and handover to use.

4.1 Difference-in-Differences Framework

By employing a DiD design we can identify the causal impact of school renovations on housing prices. A DiD modell compares the changes in outcomes for treated units (here: properties near renovated schools) with the changes in untreated units over time, isolating the effect of treatment under key assumptions (Lechner, 2011; Wooldridge, 2010). The approach leverages: district fixed effects (α_i) to control for time-invariant local heterogeneity, such as persistent urban gradients, and year fixed effects (λ_t) to account for city-wide shocks affecting all observations equally in a specific period. Identification relies on (i) the assumption of a parallel trend between treated and control groups absent treatment, (ii) no anticipation effects before completion, and (iii) no confounding spillovers from treated to control units (Angrist and Pischke, 2009; Bertrand et al., 2004).

This method is particularly suitable for our study because it allows us to trace the timing of capitalization around renovation completion, distinguishing pre-treatment discounts, construction disruptions, and post-completion gains. Thus, DiD here enables a dynamic analysis of how effects vary by neighborhood characteristics like migration share and income levels. For instance, renovations may signal stronger quality improvements in high-migration areas due to informational asymmetries, where migrants rely more on visible infrastructure cues than on school reputation (which may be less accessible due to language or network barriers). This aligns with prior research applying difference-in-differences designs to study school infrastructure impacts on housing markets, including Neilson and Zimmerman (2014), Conlin and Thompson (2017), and Lafortune and Schönholzer (2022).

We use proximity as proxy for treatment exposure: for each transaction, we identify the earliest completion year of any school renovation within a 300-meter radius.[5]

Properties located within 300 meters of a school renovation enter the treatment group once the respective renovation is completed, while properties not yet exposed to a completed renovation serve as controls. Our endogenous variable is the log transaction price per square meter of a transacted property ($Y_{it} = \log P_{it}$), allowing coefficients to be interpreted as elasticities. Controls (X'_{it}) include hedonic attributes (e.g., size, age, property-related amenities like balcony or elevator) and accessibility measures (e.g., distances to transit, parks, restaurants).

[5]This choice builds on findings from Dietl et al. (2025), where spatial panel models tested radii of 300m, 500m, 750m, and 1000m. The results showed the strongest effects at 300m. Effects became much weaker with greater distance and often not significant beyond 300m. This pattern fits with how local amenities work in dense cities like Berlin, where impacts from upgrades decrease quickly due to other nearby factors (Brueckner, 2013; Rossi-Hansberg et al., 2010. For example, a 300m radius matches a short walking distance of about 3-4 minutes, which is typical for direct neighborhood effects in urban housing markets (Glaeser and Gottlieb, 2009). The average Euclidean distance from transactions in our dataset to the nearest school is approximately 500m (calculated based on 945 public schools across Berlin's 891 km^2 area). Choosing 300m thus focuses on direct neighborhood effects and reduces weakening from far-away properties. We also applied the models for larger radii, which showed weaker or non-significant effects. This supports our focus on close areas to avoid mixing with wider market trends.

To capture dynamics, we use an event-study specification:

$$Y_{it} = \alpha_i + \lambda_t + \sum_{k=-K, k\neq -1}^{K} \beta_k \mathbf{1}\{\tau_{it} = k\} \times G_i + X_{it}'\gamma + \varepsilon_{it}, \tag{1}$$

where $\tau_{it} = t - T_i$ is the relative time to the event year T_i, G_i indicates potential treatment, and $k = -1$ (the year before completion) is omitted as the reference category. Pre-event coefficients (β_k for $k < 0$) test for parallel trends and anticipation effects; post-event coefficients trace capitalization paths. Units that were never treated receive a placebo event year (median among treated), and we restrict the analysis to a symmetric -5 to $+5$ year window for balanced support.

4.2 Addressing Staggered Treatment with the Sun and Abraham Estimator

Standard two-way fixed effects (TWFE) DiD can produce biased estimates in staggered treatment settings, where units are treated at different times. As shown by Goodman-Bacon, 2021, TWFE decomposes into a weighted average of treatment effects, but weights can be negative when early-treated units serve as controls for later-treated ones, leading to under- or overestimation if effects are heterogeneous over time or across cohorts. In our case, with renovations completing variably from 2017 onward, this risks bias, especially given heterogeneity by migration and income contexts.

To address this issue, we use the interaction-weighted estimator proposed by Sun and Abraham, 2021, which aggregates cohort-specific average treatment effects on the treated (ATT) without negative weights. It first estimates event-study coefficients separately for each treatment cohort (grouped by completion year) and then weights them by cohort size to form overall dynamic effects. The estimator is:

$$\hat{\beta}_k = \sum_c \hat{w}_{c,k} \hat{\beta}_{c,k}, \tag{2}$$

where $\hat{\beta}_{c,k}$ is the cohort-c-specific effect at relative time k, and $\hat{w}_{c,k}$ are positive weights proportional to cohort exposure at k. This ensures unbiased aggregation under parallel trends within cohorts.

This estimator is able to handle varying treatment timing robustly, avoiding the negative weighting pitfalls of TWFE (Sun and Abraham, 2021). It is particularly apt for infrastructure studies with phased rollouts, as in the case of Berlin's renovations, where completion dates depend on administrative priorities rather than endogenous factors. Related methodological work by De Chaisemartin and d'Haultfoeuille, 2020 highlights limitations of two-way fixed effects estimators under heterogeneous treatment effects and informs later applications in urban economics.

Standard errors are clustered at the school district level to account for spatial correlation. This setup extends our prior spatial focus by adding temporal granularity and heterogeneity analysis, providing policy insights on equity in educational investments.

5 Empirical Results

We estimate two complementary models using the Sun and Abraham, 2021 estimator to examine how school renovations affect nearby housing prices across different neighborhood contexts. Model 1 investigates the overall dynamic effects and examines heterogeneity by migration share, income levels, and unemployment rates. Model 2 extends the analysis to explore heterogeneity by migrant origin composition, testing whether effects differ depending on the specific ethnic composition of the local migrant population. This two-step approach allows us to first establish whether migration-related heterogeneity exists, and then to examine whether this heterogeneity reflects differences in information networks and integration patterns across migrant origin groups. Core results of model 1 und model 2 are reported in Table 5.1.

5.1 Dynamic Effects and Heterogeneity by Migration and Income

The average treatment effect on the treated (ATT) indicates that school renovations increase housing prices by 4.22% ($p<0.05$) on average across all treated properties within a 300-meter radius of renovated schools.[6] This positive capitalization effect demonstrates that physical improvements to school infrastructure function as neighborhood amenity shocks that are valued by homebuyers and are reflected in transaction prices. The magnitude of 4.22% is economically substantial and consistent with school facilities serving as localized public goods that enhance the desirability of surrounding residential locations (Neilson and Zimmerman, 2014; Cellini et al., 2010). Unlike effects driven solely by changes in school performance or reputation, this finding isolates the impact of visible infrastructure upgrades - improved building facades, modernized classrooms, expanded capacity, and enhanced facilities - that signal investment in neighborhood quality and educational resources to prospective buyers. We note, however, that visible infrastructure improvements may also correlate with actual improvements in educational quality: modernized facilities can provide better equipment and technology, while capacity expansions may attract younger or more motivated teachers.

Temporal Dynamics and Information Diffusion

The event-study estimates show that price effects emerge only after renovation completion, indicating a delayed capitalization process (see Figure 5.1). Post-treatment effects begin to emerge immediately after renovation completion at $\tau = 0$ (2.21%, insignificant) and $\tau = +1$ (2.39%, insignificant), but remain statistically insignificant in the first year. Effects become significant at $\tau = +2$ (5.99%, $p<0.05$), two years after completion, and peak at $\tau = +3$ (7.96%,

[6] Percentage effects are calculated as $[\exp(\beta) - 1] \times 100$, where β is the log-linear regression coefficient. This exact transformation accounts for the non-linear relationship between log-transformed prices and percentage changes.

p<0.05), three years after renovation handover. After this peak, effects stabilize around 5%, with $\tau = +4$ (4.79%, insignificant) and $\tau = +5$ (5.45%, insignificant).
This 2-3 year delay between renovation completion and maximum price capitalization indicates that informational asymmetries play an important role in urban housing markets. While delayed adjustment is not uncommon in housing markets due to search frictions, low transaction frequency, and gradual residential mobility, the pronounced heterogeneity across migration and income contexts suggests that the observed dynamics are unlikely to reflect purely mechanical market adjustment. Instead, the stronger responses in neighborhoods characterized by greater informational constraints are consistent with an information-diffusion mechanism. Markets require time to recognize and capitalize on infrastructure upgrades, consistent with gradual information diffusion processes documented by Neilson and Zimmerman, 2014 for school construction projects. The pattern suggests that visible infrastructure improvements do not immediately translate into price effects, but rather that their quality signals must first be observed, interpreted, and incorporated into buyer expectations before full capitalization occurs. There is no evidence of anticipation effects in the years immediately preceding the completion of the renovation, since the coefficients for $\tau = -4$ (-3.12%, insignificant), $\tau = -3$ (-0.01%, insignificant) and $\tau = -2$ (-2.09%, insignificant) are close to zero and statistically insignificant.[7] This absence of anticipation effects supports the causal interpretation of our estimates, as buyers do not appear to adjust prices in anticipation of upcoming renovations before completion.

Areas with higher migration shares exhibit systematically lower baseline housing prices. The elasticity of housing prices with respect to the foreign-born share is -0.36 (p<0.001): a 1% increase in the migration share is associated with a 0.36% decrease on average, c.p. in property prices in control areas. This negative association is consistent with prior literature documenting price discounts in high-migration neighborhoods (Sá, 2015; Rogne et al., 2025). These discounts may reflect multiple mechanisms, including income differentials and differences in educational backgrounds across migrant groups, for which our specifications account only partially.
However, the interaction between treatment and log migration share is large and positive (0.35, p<0.01), indicating that the initial price discount associated with higher migration share is almost fully offset after renovation. Specifically, in treated areas, the net elasticity is near zero, meaning that a 1% increase in the migration share is associated with virtually no change in property prices after school renovation. This means that school renovations generate substantially stronger price effects in high-migration neighborhoods than in low-migration areas. The treatment effect completely compensates for the pre-existing price discount associated with migration concentration.[8]

[7] The exception is $\tau = -5$; see Section 5.3.2 for discussion.

[8] The full offset of the pre-existing price discount warrants careful interpretation. Beyond informational mechanisms, this result may partly reflect the correlation of high migrant shares with other neighborhood characteristics, such as lower baseline price levels, greater scope for valuation gains, or systematically larger renovation projects in disadvantaged areas, that independently amplify capitalization effects.

The stronger capitalization observed in neighborhoods with high migrant shares is consistent with stronger informational effects, although correlated neighborhood characteristics such as income differences, residential sorting, or lower initial price levels may also contribute to this pattern. Migrants may rely more heavily on visible infrastructure signals such as school building quality due to limited access to soft information about school reputation, which often circulates through native social networks and may be less accessible due to language barriers or social segregation (Zimmermann, 2024; Tjaden, 2020). In addition, improvements in school infrastructure can alter neighborhood perceptions by counteracting negative signals associated with disadvantaged or high-migration areas. This interpretation relates to broader research on the housing market showing that visible neighborhood conditions and prominent local signals can shape housing demand and property values through mechanisms of reputation and stigma (Hartley, 2014; Schuetz et al., 2008).

Income Heterogeneity: Stronger Effects in Disadvantaged Neighborhoods

Heterogeneity by neighborhood income points in a similar direction. Relative to middle-income areas, the interaction between low-income areas and treatment is associated with a 14.66% price increase ($p<0.05$), indicating substantially stronger capitalization in economically disadvantaged neighborhoods. Several mechanisms may contribute to this pattern. In addition to potentially stronger informational effects, low-income areas may differ systematically in educational attainment, housing demand, outside schooling options, and baseline neighborhood quality, implying greater scope for valuation changes following visible infrastructure improvements.

In contrast, the interaction between high-income areas and treatment yields no statistically significant effects, suggesting more limited price gains in wealthier contexts. Higher-income households typically have better access to information, greater choice options for school choice, and stronger social networks, reducing the marginal value of information of visible infrastructure upgrades. School buildings in high-income neighborhoods may also already be in better structural condition prior to renovation, leaving less room for visible quality improvements. In low-income neighborhoods, by contrast, school renovations may represent a comparatively rare and highly salient improvement in local public infrastructure, generating stronger capitalization effects.

Table 5.1: Core Results: Migration Level and Origin Composition Heterogeneity

	Model 1 **Migration Share**	Model 2 **Origin Composition**
Average Treatment Effect on Treated		
ATT	0.041301* (0.018503)	0.043086* (0.018547)
Event study: relative year τ (reference: $\tau = -1$)		
$\tau = -5$ (treated)	−0.088990* (0.039061)	−0.090828* (0.038610)
$\tau = -4$	−0.031685 (0.026393)	−0.032940 (0.026233)
$\tau = -3$	−0.000091 (0.022364)	−0.000697 (0.022412)
$\tau = -2$	−0.021058 (0.014242)	−0.020994 (0.014244)
$\tau = 0$	0.021790 (0.015234)	0.022635 (0.015073)
$\tau = +1$	0.023552 (0.019700)	0.024945 (0.019721)
$\tau = +2$	0.058175* (0.022684)	0.060462** (0.023108)
$\tau = +3$	0.076492* (0.029990)	0.079065** (0.030204)
$\tau = +4$	0.046773 (0.032891)	0.048407 (0.032884)
$\tau = +5$	0.053110 (0.043317)	0.057765 (0.042826)
Migration heterogeneity (Model 1)		
log(Migration share)	−0.362910*** (0.096287)	
log(Migration share) × Treated	0.359178** (0.120421)	
Origin composition (Model 2)		
Turkish share (among migrants)		−0.014414*** (0.003583)
Turkish share × Treated		0.015260*** (0.004532)
Eastern European share (among migrants)		−0.118384** (0.045019)
Eastern European share × Treated		0.124456** (0.046859)
Socioeconomic controls		
Singles share	−0.008289 (0.009044)	0.050537** (0.016866)
Singles share × Treated	0.013007 (0.009649)	−0.045694** (0.016078)
log(Unemployment rate)	−0.237361 (0.180817)	−0.119249 (0.072267)
log(Unemployment rate) × Treated	0.126099 (0.147207)	—
log(Households)	−0.009499 (0.027082)	−0.006039 (0.028307)
High share of children × Treated	−0.055381 (0.036591)	−0.054188 (0.035708)
Income heterogeneity (ref: middle income)		
Low income × Treated	0.136697* (0.058417)	0.146788* (0.057403)
High income × Treated	0.008278 (0.048744)	0.003041 (0.051445)
Distance decay		
Distance to school × Treated	0.000048 (0.000099)	0.000045 (0.000098)
Year FEs	Yes	Yes
School District FEs	Yes	Yes
Observations	22 837	22 837
R^2 (adj.)	0.808	0.808
R^2 (within)	0.702	0.703
RMSE (log)	0.276	0.276

Notes: Dep. var.: log(transaction price). Event-study terms: $\mathbf{1}\{\tau\} \times \mathbf{1}\{\text{treated}\}$; ref. $\tau = -1$. Model 2 origin shares sum to 100%; "other origins" = reference category. Both models: school district and year FEs; SEs clustered at school district level. — not included. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, · $p < 0.10$.

Control variables show broadly plausible signs throughout. Hedonic attributes perform as expected, with floor area, building age, balcony, elevator, and floor level all positively and significantly associated with transaction prices. Full coefficient estimates are provided in Table A.2 in the Appendix. Distance variables are generally insignificant, consistent with the dense urban structure of Berlin where accessibility varies little within narrow treatment radii. Regarding school type heterogeneity, renovation effects are negative and significant only for secondary schools (-7.24%, $p<0.05$), while all other school types show insignificant interactions with treatment. This pattern is consistent with the institutional argument in Dietl et al. (2025): capitalization effects are strongest for primary schools, where catchment-area enrollment rules create a direct spatial link between school quality and residential location, while secondary school effects are diluted by choice-based admission procedures.

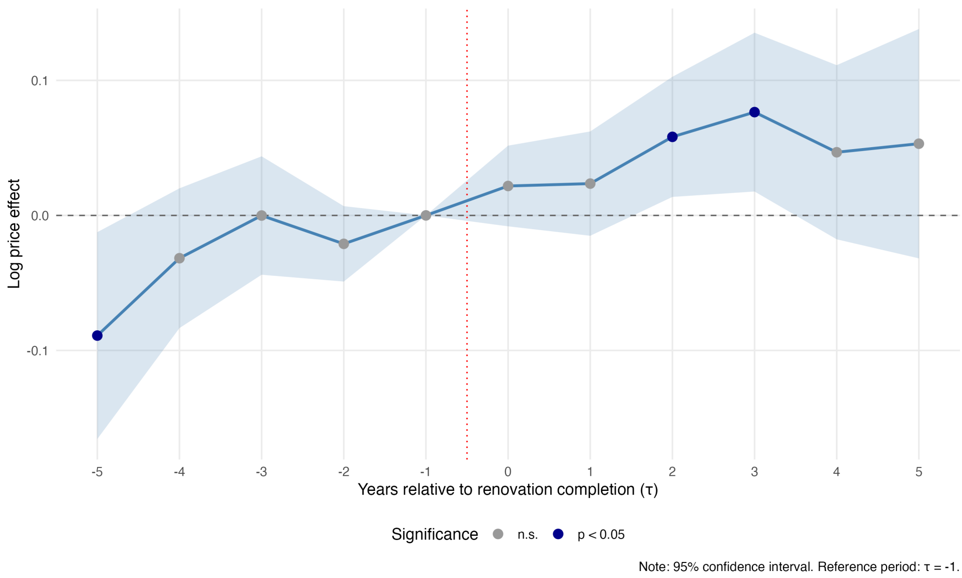

Figure 5.1: Event-study estimates of the dynamic effects of school renovations by migration share (Model 1). Estimates are based on a staggered difference-in-differences design using the Sun and Abraham (2021) estimator. The omitted reference period is the year prior to renovation completion ($\tau = -1$).

5.2 Heterogeneity by Migrant Origins

Average Treatment Effects and Temporal Dynamics

Model 2 (cf. Table 5.1) extends the analysis to examine whether renovation effects vary systematically by the ethnic composition of the local migrant population. The average treatment effect on the treated (ATT) remains stable at 4.3% ($p<0.05$), nearly identical to Model 1, indicating that the overall magnitude of capitalization is robust to the inclusion of origin-specific heterogeneity. Temporal dynamics likewise mirror Model 1, with post-treatment effects peaking at $\tau = +3$ (7.9%, $p<0.01$), confirming the same gradual adjustment pattern documented earlier: renovation effects emerge after completion, strengthen over the subsequent two to three years as information diffuses through the market, and then stabilize.

There are substantial baseline price differences associated with neighborhood ethnic composition. Relative to areas with predominantly other migrant origins (the reference category, comprising Southern Europeans, Balkans residents, late repatriates, Middle Eastern, Sub-Saharan African, and Asian migrants), neighborhoods with higher concentrations of Turkish-origin residents exhibit modest negative price: a one-percentage-point increase in the Turkish share among migrants is associated with 1.4% lower housing prices ($p<0.001$). This pattern may reflect multiple mechanisms, including socioeconomic differences, residential sorting, stigma effects, and other unobserved neighborhood characteristics associated with long-standing Turkish settlement patterns in districts such as Kreuzberg and Neukölln.

Substantially larger baseline discounts are observed for Eastern European migrant concentrations. A one-percentage-point increase in the Eastern European share among migrants is associated with 11.8% lower prices ($p<0.01$).[9] This pronounced discount likely reflects multiple mechanisms: Eastern European migration to Berlin accelerated primarily after 2004 EU enlargement, resulting in newer, less established communities with shorter average residence durations, potentially lower incomes, and fewer accumulated local networks compared to the long-established Turkish community dating from the 1960s–1970s *Gastarbeiter* programs (Glorius et al., 2013; Nowicka, 2018).

Differential Renovation Effects by Migrant Origin

The interaction terms between migrant origin shares and school renovation reveal striking heterogeneity in price capitalization, providing indirect evidence that informational asymmetries vary systematically across migrant groups. In neighborhoods with higher Turkish-origin concentrations, school renovations generate a positive interaction effect of +1.5% per percentage point increase in Turkish share ($p<0.001$). This effect fully offsets the baseline discount, indicating that renovations eliminate the negative price association in Turkish-majority areas.

However, the renovation premium is substantially amplified in neighborhoods with higher Eastern European concentrations. The interaction between Eastern European share and treatment yields +12.4% per percentage point ($p<0.01$) - more than eight times larger than the Turkish interaction effect.[10] This pronounced differential suggests that Eastern European residents, as newer and potentially less-informed migrants, rely more heavily on visible infrastructure signals

[9]This pronounced baseline discount likely reflects multiple mechanisms and should be interpreted with caution. The Eastern European origin group in Berlin is compositionally heterogeneous, encompassing both refugee populations from the 1990s Balkan conflicts and more recent EU labor migrants from Romania and Bulgaria, who differ substantially in socioeconomic status, residential location, and housing market integration. The large coefficient may therefore partly reflect correlated unobserved neighborhood characteristics rather than a pure origin-group effect.

[10]A contributing factor may be the spatial sorting of Eastern European migrants into outer districts such as Marzahn-Hellersdorf, where lower baseline price levels and greater scope for valuation gains may amplify renovation effects relative to the more centrally located Turkish-origin communities in Kreuzberg and Neukölln. The differential interaction effects may therefore partly reflect differences in baseline neighborhood price levels and location in addition to informational mechanisms.

such as school building quality due to limited access to informal information networks about school reputation, neighborhood trajectories, or local amenities. In contrast, the Turkish community's longer settlement history, denser co-ethnic networks, and greater institutional familiarity may reduce dependence on visible quality signals, as information about school quality circulates through established social ties (Zimmermann, 2024; Tjaden, 2020).

These findings align with broader evidence on migrant information gaps in education systems. Recent work documents that non-EU migrants in Germany face substantially larger knowledge deficits about the tracked school system than EU migrants, with Turkish-origin families particularly disadvantaged relative to other groups (Zimmermann, 2024). However, our results reveal an additional dimension: even conditional on facing information barriers, *newer* migrant groups (Eastern Europeans) exhibit stronger behavioral responses to visible infrastructure improvements than *established* migrant groups (Turks), likely because recent arrivals have not yet accumulated the local knowledge and social capital that established communities use to assess school quality.

This heterogeneity underscores that school renovations do not operate uniformly as amenity shocks but rather function as neighborhood quality signals whose salience varies systematically with residents' informational access and integration levels. In Berlin's socially and demographically diverse urban context, infrastructure investments interact with neighborhood composition to produce differentiated housing market responses. The stronger capitalization effects observed in neighborhoods with higher shares of residents classified into migrant origin groups are consistent with the interpretation that visible school infrastructure improvements become particularly relevant where access to local information is more limited.

Control variables remain fully consistent with Model 1, with hedonic attributes, distance variables, and school type interactions showing identical patterns (cf. Table A.2 in the Appendix·.

5.3 Robustness checks

To assess the credibility of our main findings, we conduct a series of robustness checks that examine potential threats to identification and test the sensitivity of our estimates to alternative specifications. These tests address five key questions: (i) Was renovation timing systematically targeted toward certain neighborhood types (endogeneity)? (ii) Did treated and control units follow similar price trends prior to renovation (parallel trends)? (iii) Do the observed effects reflect expectations rather than actual renovation completion (placebo tests)[11]? (iv) Do the results depend on the choice of the event-time window (window specification)? and (v) Are the conclusions robust to alternative clustering assumptions?

[11] Placebo tests assess whether estimated treatment effects also appear when treatment is artificially assigned to periods before the actual intervention. Statistically insignificant placebo effects support the interpretation that the main results are not driven by pre-existing trends or anticipation effects.

5.3.1 Endogeneity and Selection Mechanism

A central concern for identification is whether school renovations were systematically targeted toward neighborhoods with specific socioeconomic or demographic characteristics. Institutional evidence from the Berliner Schulbauoffensive (BSO) suggests that this is unlikely.

The Berliner Schulbauoffensive is explicitly designed to address two citywide infrastructure challenges: (i) the elimination of a substantial backlog in school building maintenance and (ii) the expansion of school capacity in response to rapidly increasing student numbers. According to official documentation, renovation and construction priorities are driven by the physical condition of school buildings, capacity constraints, and organizational feasibility, rather than neighborhood socioeconomic characteristics such as income composition or migrant shares (Land Berlin, 2024a; Land Berlin, 2024b).[12]

A key institutional feature of the BSO is the systematic and standardized assessment of renovation needs. Beginning in 2015, all Berlin districts were required to conduct comprehensive building scans of school facilities using uniform criteria. These scans document structural deficiencies, safety issues, and maintenance backlogs and are centrally compiled, validated, and updated by the Senate Department for Education in cooperation with the districts and the state-owned real estate agency (BIM). The resulting assessments form the empirical basis for prioritizing renovation projects and allocating funds (Scheeres, 2016).

Project selection and implementation are coordinated across multiple actors - including the Senate Departments for Education and for Urban Development, the Berlin districts, and the public housing company HOWOGE - within formal governance structures such as the Taskforce Schulbau and steering committees. Measures are grouped into predefined "BSO tranches" based on project type (e.g., renovation, expansion, modular construction), administrative responsibility, scale, and implementation horizon. Importantly, these classifications are organizational and technical in nature and do not incorporate explicit social or demographic targeting criteria.

Taken together, the institutional design of the Berliner Schulbauoffensive implies that renovation timing is primarily determined by building condition, capacity needs, and administrative readiness, rather than by local housing market trends or neighborhood composition.

Empirically, this institutional interpretation is consistent with our timing regressions. Baseline characteristics measured prior to treatment, such as income composition, unemployment, and overall migrant share, do not systematically predict renovation timing once district fixed effects are included. This suggests that, at the relevant policy scale, renovation decisions are not driven by pre-existing socioeconomic trends.

However, renovation timing is correlated with baseline migrant composition, particularly the shares of residents with Turkish and Eastern European backgrounds. This pattern likely reflects

[12] Some degree of endogeneity nevertheless cannot be fully ruled out. In particular, rising student numbers and capacity shortages may themselves be correlated with neighborhood demographic change, migration patterns, or local housing demand. We therefore examine renovation timing empirically and account for persistent neighborhood differences through fixed effects.

the historical co-location of older school infrastructure and migrant-dense neighborhoods in Berlin rather than deliberate social targeting. Nevertheless, these correlations indicate that renovation timing is not fully orthogonal to neighborhood composition and therefore represent a potential source of endogeneity. Importantly, our identification does not rely on cross-district comparisons alone. All main specifications include district fixed effects and cohort-specific event-study estimators, such that treatment effects are identified from within-district, within-cohort variation over time. Moreover, the absence of systematic pre-trends in the years immediately preceding renovation supports the interpretation that the estimated effects are not primarily driven by differential underlying housing market dynamics.

Two limitations remain. First, while building-condition indices are systematically collected, they are not directly observable in the transaction-level data, preventing a fully structural control for physical school quality. Second, the significant joint pre-trend tests in the event-study reflect the high statistical power of staggered adoption designs rather than economically meaningful pre-treatment dynamics.

Overall, while endogenous selection into renovation cannot be ruled out entirely, the empirical results suggest that renovation timing is unlikely to be systematically driven by short-run housing price dynamics.

5.3.2 Parallel Trends

The parallel trends assumption is assessed using an event-study specification that traces price dynamics over the window $\tau \in [-5, +5]$ years relative to renovation completion. Figure 5.1 plots the estimated event-time coefficients, while Table **??** reports the full set of regression results.

Pre-treatment estimates indicate no economically meaningful price trends in the years immediately preceding renovation completion. Specifically, the coefficients for $\tau = -4$, $\tau = -3$, and $\tau = -2$ are small in magnitude and statistically insignificant, with point estimates of -3.2% ($p = 0.23$), -0.0% ($p > 0.99$), and -2.1% ($p = 0.14$), respectively. These coefficients suggest that, in the relevant pre-treatment period used for identification, treated and control locations follow similar price trajectories.

The coefficient at $\tau = -5$ is negative and statistically significant (-8.9%, $p = 0.023$). This deviation does not constitute a violation of the parallel trends assumption. Rather, it reflects the explicit prioritization criteria of the Berliner Schulbauoffensive, which sequences renovation measures according to the urgency of building conditions and available administrative and budgetary capacity (Senatsverwaltung für Bildung, Jugend und Familie, 2024), rather than according to neighborhood socioeconomic characteristics. Notably, building deterioration and capacity shortfalls are themselves correlated with demographic change and population growth - particularly in districts that received larger numbers of refugee children in 2015/2016 and 2022/2023 - such that renovation priorities driven by physical condition may nonetheless exhibit systematic patterns across neighborhood types. Importantly, this potential correlation is absorbed by our district fixed effects and cohort-specific event-study estimators. Importantly, this effect occurs well outside the immediate pre-treatment window and does not persist into the years directly preceding renovation completion.

Formal joint tests of the pre-treatment coefficients for $\tau = -4$ to $\tau = -2$ yield statistically significant Wald statistics, reflecting the high power of cohort-specific tests in staggered difference-in-differences designs. As emphasized by recent methodological work, pre-trend tests should be interpreted with caution, as their statistical significance and power depend on the number of tested coefficients, sample size, and the magnitude of economically relevant deviations from parallel trends (Roth, 2022; Rambachan and Roth, 2023). Accordingly, statistical significance alone is not sufficient to diagnose violations of parallel trends in this setting.

Crucially, the absence of systematic pre-treatment patterns in the years immediately preceding renovation, combined with the robustness evidence discussed below, supports the credibility of the parallel trends assumption in the relevant identification window.

Post-treatment coefficients display a gradual adjustment pattern. Estimated effects are small and statistically insignificant in the completion year ($\tau = 0$: 2.2%, $p = 0.15$) and the first post-treatment year ($\tau = +1$: 2.4%, $p = 0.23$), but become statistically significant from $\tau = +2$ onward (5.8%, $p = 0.01$), peaking at $\tau = +3$ (7.6%, $p = 0.01$). This delayed capitalization is consistent with gradual information diffusion and market adjustment following infrastructure upgrades (Neilson and Zimmerman, 2014).

5.3.3 Placebo Tests

To assess anticipation effects and spurious pre-trends, we conduct placebo tests that shift treatment timing two years before actual renovation completion. Under the DiD identifying assumptions, these placebo treatments should not generate significant effects if the main estimates are causal.

Table 5.2 reports the results for both baseline specifications. In both models, the placebo average treatment effects on the treated (ATT) are small in magnitude, negative in sign, and statistically insignificant. For the migration-share model, the placebo ATT amounts to −0.020 (p = 0.34), while for the migrant-composition model the estimate is −0.019 (p = 0.37). These estimates are close to zero relative to the main treatment effects and do not indicate any systematic price response prior to the actual renovation period.

Overall, the placebo results provide no evidence of anticipation effects or spurious pre-trends that could mechanically generate the main findings. Instead, they support the interpretation that the observed post-renovation price increases reflect genuine capitalization effects following school renovation completion rather than pre-existing trends or mis-timed policy responses.

Table 5.2: Placebo Tests: Shifted Treatment Timing (Two Years Earlier)

Model	ATT	Std. Error	t-value	p-value
Migration Share Model	−0.0203	0.0210	−0.97	0.335
Migrant Composition Model	−0.0188	0.0211	−0.89	0.374

Notes: This table reports placebo average treatment effects on the treated (ATT) from models in which the treatment timing is artificially shifted two years earlier than the actual school renovation completion year. Standard errors are clustered at the school district level. None of the placebo estimates are statistically significant, indicating no evidence of anticipation effects or spurious pre-treatment trends.

5.3.4 Event Window Specification

To assess the sensitivity of the estimated treatment effects to the choice of the event-time window, we re-estimate the models using symmetric windows of ±3, ±4, and ±5 years around renovation completion. Across both specifications, the estimated average treatment effects on the treated (ATT) remain stable in magnitude and sign. While statistical significance varies slightly with sample size and precision, effect sizes consistently range between 4 and 5 percent. This pattern indicates that the main results are not driven by a particular choice of event window.

Table 5.3: Event Window Sensitivity of Treatment Effects

Model	Event Window	Observations	ATT	Std. Error	p-value
Model 1 (Migration Share)	±3	15,668	0.045	0.021	0.035
Model 1 (Migration Share)	±4	19,291	0.040	0.022	0.068
Model 1 (Migration Share)	±5	22,854	0.041	0.019	0.026
Model 2 (Migrant Composition)	±3	15,668	0.049	0.022	0.024
Model 2 (Migrant Composition)	±4	19,291	0.043	0.022	0.051
Model 2 (Migrant Composition)	±5	22,854	0.043	0.019	0.021

Notes: This table reports average treatment effects on the treated (ATT) estimated using alternative symmetric event-time windows around renovation completion. All models include school-district and year fixed effects. Standard errors are clustered at the school-district level.

5.3.5 Timing Endogeneity (Targeting)

A potential concern in the evaluation of large-scale school renovation programs is that investment timing may be endogenous, for instance if renovations are systematically prioritized in socioeconomically disadvantaged or high-migration neighborhoods. To assess this issue, we examine whether the timing of renovation completion is correlated with pre-treatment neighborhood characteristics measured prior to any renovation activity.

Specifically, we regress the renovation cohort year on baseline values of migration, income composition, and unemployment, measured over the period 2013–2016, while controlling for district fixed effects. Table 5.4 reports results for overall migration and income characteristics, and Table 5.5 for migrant origin composition.

For the migration-share specification, we find no statistically significant relationship between renovation timing and baseline migrant share, unemployment, or income composition. This suggests that renovation timing is not systematically driven by pre-existing socioeconomic disadvantage or overall migration intensity.

In contrast, renovation timing is positively correlated with the baseline composition of migrant origins, particularly the shares of Turkish and Eastern European migrants. This pattern indicates that schools in neighborhoods with higher concentrations of specific migrant groups tended to be renovated earlier. Importantly, income and unemployment remain insignificant in this specification. Moreover, all main estimations include district fixed effects and cohort-specific event-study estimators, ensuring that identification relies on within-cohort variation rather than cross-district targeting. As such, while composition-related targeting cannot be ruled out, it does not undermine the causal interpretation of the estimated treatment effects.

Table 5.4: Timing Endogeneity Test: Migration Share and Income

Baseline Variable	Estimate	Std. Error	t-value	p-value
Log migrant share	0.001	0.338	0.003	0.998
Unemployment rate (log)	−0.010	0.453	−0.02	0.983
Low income	0.054	0.431	0.12	0.901
High income	−0.197	0.620	−0.32	0.751

Notes: The dependent variable is the renovation completion cohort year. Baseline variables are averaged over the pre-treatment period 2013–2016. All regressions include district fixed effects and heteroskedasticity-robust standard errors.

Table 5.5: Timing Endogeneity Test: Migrant Origin Composition

Baseline Variable	Estimate	Std. Error	t-value	p-value
Turkish share (among migrants)	0.042	0.014	3.12	0.002
Eastern European share (among migrants)	0.145	0.040	3.62	<0.001
Unemployment rate (log)	−0.398	0.401	−0.99	0.322
Low income	0.293	0.413	0.71	0.478
High income	−0.286	0.618	−0.46	0.645

Notes: The dependent variable is the renovation completion cohort year. Baseline variables are averaged over the pre-treatment period 2013–2016. All regressions include district fixed effects and heteroskedasticity-robust standard errors. Migrant composition shares are measured conditional on the migrant population.

5.3.6 Alternative Clustering

To assess the sensitivity of statistical inference to the choice of clustering level, we re-estimate the main specifications clustering standard errors at the district level instead of the school-district level. While point estimates remain unchanged, standard errors increase, as expected given the limited number of districts. Statistical significance is reduced accordingly, but the magnitude and direction of the estimated treatment effects remain stable.

Table 5.6: Robustness to Alternative Clustering Levels

Model	Clustering Level	ATT	Std. Error	t-value	p-value
Model 1 (Migration Share)	School district	0.041	0.0185	2.23	0.026
Model 1 (Migration Share)	District	0.041	0.0239	1.73	0.112
Model 2 (Migrant Composition)	School district	0.043	0.0185	2.32	0.021
Model 2 (Migrant Composition)	District	0.043	0.0241	1.79	0.102

Notes: The table reports average treatment effects on the treated (ATT) using alternative clustering levels for standard errors. Point estimates are identical across clustering schemes. Larger standard errors under district-level clustering reflect the small number of districts (12).

6 Conclusion

This paper examined whether school renovations are capitalized into nearby housing prices, whether these effects vary systematically across neighborhoods with different migration and income compositions, and whether such heterogeneity is consistent with information frictions in urban housing markets.

The results yield three main findings. First, school renovations increase nearby condominium prices by approximately 4% on average, with effects emerging only gradually over a period of two to three years. Second, capitalization effects differ substantially across neighborhoods and are significantly stronger in areas with higher migrant shares and lower income levels, where renovations largely offset pre-existing price discounts. Third, the differential capitalization effects across migrant origin groups suggest the presence of information frictions in housing markets. In particular, renovation effects are more pronounced in neighborhoods with larger shares of Eastern European migrants than in areas with predominantly Turkish-origin populations. While we cannot directly identify the underlying mechanism, the most plausible interpretation, given that we control for a rich set of socioeconomic and locational characteristics, is that visible school infrastructure improvements carry greater informational value in contexts characterized by more limited access to local information and weaker neighborhood networks. Alternative explanations, such as differences in residential preferences, housing demand dynamics, or unobserved neighborhood characteristics across migrant groups, cannot be fully ruled out.

The finding that capitalization effects are particularly pronounced in low-income neighborhoods warrants careful interpretation. Stronger price responses in disadvantaged areas may contribute to reducing spatial disparities, but rising property values can simultaneously exert gentrification pressures, potentially displacing the very residents who stand to benefit most from improved local schools.

Futhermore, these findings have important implications for urban policymakers, housing market participants, and local communities. They suggest that public investments in school infrastructure not only improve educational facilities but also shape neighborhood attractiveness and housing market dynamics in socially heterogeneous ways.

Policy responses could therefore focus on complementing school infrastructure investments with measures that improve transparency and access to local information in housing markets, particularly in neighborhoods characterized by higher informational constraints. In addition, policymakers may need to accompany neighborhood upgrading processes with housing affordability measures to mitigate potential gentrification and displacement pressures in areas experiencing strong price appreciation.

Several open questions remain and should be considered in light of the limitations of the empirical approach. While the empirical design accounts for staggered treatment timing and extensive neighborhood fixed effects, renovation timing may still partly reflect underlying demographic or infrastructure dynamics that are difficult to observe completely. Moreover, although the heterogeneous effects are consistent with information frictions, alternative mechanisms such as differences in income, educational composition, residential preferences, or neighborhood-specific demand trends may also contribute to the observed patterns.

Future research could examine whether the observed capitalization effects contribute to neighborhood change through gentrification, residential sorting, and displacement processes. It may also be valuable to analyze how school renovations affect segregation patterns, school composition, and housing market responses differently across owner-occupied and rental segments.

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A Appendix

Table A.1: Overview of Variables and Data Sources

Variable	Time Coverage	Source
Dependent variable		
Transaction price (€/m^2, log)	2013–2022	GAA Berlin
Treatment variables		
School renovation (completion year indicator)	2013–2027	Berlin School Construction Initiative
Renovation type (sanitation, extension, new build, temporary, combined)	2013–2027	Berlin School Construction Initiative
Renovation investment volume	2013–2027	Berlin School Construction Initiative
Distance to nearest renovated school (meters)	2013–2027	Own calculations (FIS-Broker)
Property characteristics		
Floor area (m^2), year built, floor level	2013–2022	GAA Berlin
Balcony, elevator, parking (dummies)	2013–2022	GAA Berlin
Accessibility and amenities		
Distance to public transport (bus, tram)	2013–2022	Own calculations (FIS-Broker)
Distance to restaurants, water, green spaces	2013–2022	Own calculations (FIS-Broker)
Neighborhood socioeconomic characteristics (1-km^2 grid)		
Purchasing power per capita (log)	2009–2021	RWI–Geo–Grid[a]
Number of households (log)	2009–2021	RWI–Geo–Grid
Share of children (0–17 years)	2009–2021	RWI–Geo–Grid
Income terciles (low / middle / high income)	2009–2021	Own construction based on RWI–Geo–Grid
High-child-density indicator	2009–2021	Own construction based on RWI–Geo–Grid
Migration and demographic composition		
Share of residents with migration background	2009–2021	RWI–Geo–Grid
Share of migrants of Turkish origin	2009–2021	RWI–Geo–Grid
Share of migrants of Eastern European origin	2009–2021	RWI–Geo–Grid
Share of other migrant origins (reference group)	2009–2021	RWI–Geo–Grid
Unemployment rate	2009–2021	RWI–Geo–Grid
Share of single-person households	2009–2021	RWI–Geo–Grid

Note: Socioeconomic and migration indicators are drawn from the 1-km^2 RWI–Geo–Grid and assigned to each transaction and school location using raster-cell averages. Derived indicators (income terciles, high-child-density areas, origin-group shares) are constructed consistently from these grid-level values. Migration origin shares sum to 100% within the migrant population.

Table A.2: Hedonic Attributes, Distances & School Types – Model 1 and Model 2

	Model 1 **Migration Share**	Model 2 **Origin Composition**
Hedonic attributes		
Floor area (m^2)	0.014147*** (0.000188)	0.014144*** (0.000189)
Year built	0.001867*** (0.000221)	0.001852*** (0.000221)
Balcony	0.023732** (0.007566)	0.023542** (0.007536)
Elevator	0.062804*** (0.016268)	0.063364*** (0.016314)
Floor level	0.022050*** (0.002986)	0.022178*** (0.002941)
Parking	−0.017241 (0.017799)	−0.018235 (0.017497)
Distance variables		
Distance to tram	−0.00000836 (0.00004700)	−0.00000852 (0.00004500)
Distance to bus	0.00001665 (0.00006900)	0.00001875 (0.00006900)
Distance to restaurant(s)	−0.00010113 (0.00009100)	−0.00010236 (0.00009100)
Distance to water	−0.00000115 (0.00003900)	−0.00000697 (0.00003900)
School type heterogeneity (ref: other)		
Gymnasium × Treated	−0.043741 (0.035773)	−0.042794 (0.035692)
Grundschule × Treated	−0.041602 (0.032758)	−0.039058 (0.032314)
Sekundarschule × Treated	−0.072358* (0.034678)	−0.071003* (0.034286)
Förderschule × Treated	−0.082006 (0.060751)	−0.079268 (0.061038)
Berufsschule × Treated	−0.026872 (0.049223)	−0.025702 (0.048942)
Andere × Treated	0.123051 (0.112837)	0.124677 (0.113010)
School District FEs	Yes	Yes
Year FEs	Yes	Yes
Observations	22 837	22 837
R^2 (adj.)	0.808	0.808
R^2 (within)	0.702	0.703
RMSE (log)	0.276	0.276

Notes: Dep. var.: log(transaction price). This table reports hedonic attributes, distance variables, and school type interactions for both models. Both models include school district and year FEs; SEs clustered at school district level. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, · $p < 0.10$.