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Hörnig, Lukas; Schäfer, Max

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Editors

Prof. Dr. Thomas K. Bauer RUB, Department of Economics, Empirical Economics Phone: +49 (0) 234/3 22 83 41, e-mail: thomas.bauer@rub.de Prof. Dr. Ludger Linnemann Technische Universität Dortmund, Department of Business and Economics Economics – Applied Economics Phone: +49 (0) 231/755-3102, e-mail: Ludger.Linnemann@tu-dortmund.de Prof. Dr. Volker Clausen University of Duisburg-Essen, Department of Economics International Economics Phone: +49 (0) 201/1 83-3655, e-mail: vclausen@vwl.uni-due.de Prof. Dr. Ronald Bachmann, Prof. Dr. Almut Balleer, Prof. Dr. Manuel Frondel, Prof. Dr. Ansgar Wübker RWI, Phone: +49 (0) 201/81 49-213, e-mail: presse@rwi-essen.de

Editorial Office

Sabine Weiler

RWI, Phone: +49 (0) 201/81 49-213, e-mail: sabine.weiler@rwi-essen.de

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Lukas Hörnig and Max Schäfer

The Value of School Choice Opportunities



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Abstract

In this paper, we study the house price effects of local school choice opportunities among public primary schools using a rare and large-scale reform that abolished binding catchment areas in North Rhine-Westphalia, the largest German state with 18 million inhabitants. To estimate the reform's effect on valuations of houses, we compare houses with different local school choice sets, before and after the reform. We find that gaining access to a school within 2,000 meters and with a higher transition rate to the academic track (relative to the initial neighborhood school) increases house prices by 1.5 percent. This effect is larger when the more attractive school is closer and diminishes as distance grows. The full reform effect materializes roughly five years after reform onset.

JEL-Codes: I28, R21, R3

Keywords: School choice; housing demand; real estate markets

November 2024

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1 Introduction

School choice is often part of parents' decisions about where to live, and parents are willing to pay a premium for houses in high-quality school districts (Black, 1999). When school admissions depend on binding school catchment areas (hereafter SCAs), the demand for houses in the SCAs of desired schools tends to increase, leading to higher house prices. This can restrict access to better schools for families with fewer financial resources, thus impeding social mobility.

To make high-quality education more accessible to a wider range of students, policymakers have introduced many different programs that have expanded the set of schools from which parents can choose (Abdulkadiroğlu and Andersson, 2023). Coming from a catchment area system that assigns students to a designated neighborhood school based on where they live, these school choice programs can broadly take two forms. The expansion of choice schools, such as charter, magnet, private or denominational schools, allows parents to opt out of their neighborhood school, while SCAs are maintained for traditional public schools. Another possibility is to abolish the catchment area regime itself, enabling parents to choose from several public schools (*i.e.*, open enrollment policies). In the US, being offered a choice school, such as a charter school, has been shown to be valued by parents (Andreyeva and Patrick, 2017; Zheng, 2022). Few studies have examined open enrollment policies and they generally document that the removal of SCAs weakens the house price premium of zoned school quality (Machin and Salvanes, 2016; Brunner et al., 2012; Reback, 2005). However, there is little evidence on how parents value the school choice options among traditional public schools that become available when catchment areas are removed.

In this paper, we analyze whether parents value improved school choice opportunities for public primary schools using a school choice reform that abolished binding catchment areas. This reform changed primary school access from a catchment area system to an open enrollment system in North Rhine-Westphalia (NRW), the largest German state with around 18 million inhabitants. Before the reform took effect in the 2008/09 school year, students had to attend the neighborhood school in whose catchment area they lived. After the reform, students were free to choose their primary school. Since students retained the right to attend their neighborhood school (the closest school in terms of distance), the reform provided parents and children with a choice, effectively depending on the set of alternative schools that were available locally. In particular, we examine how gaining access to attractive primary schools affects house prices, with school attractiveness measured by each primary school's transition rate to the academic track. The transition from universal primary school to different secondary school tracks at the age of ten is an important step in the lives of students and an important predictor of both their educational and labor market career (Dustmann, 2004; Bellenberg and Forell, 2012). If households live in the catchment area of an unattractive school, but the reform allows them to choose a more desirable primary school that is outside their former catchment area but in the vicinity, parents may pay a house price premium for having this choice.

To learn how house prices respond to public school choice options that arose from the removal of binding SCA boundaries in the 2008/09 school year, we combine all house listings from Germany's largest housing advertising platform (immobilienscout24.de) with administrative data for the universe of primary schools in NRW from 2007 to 2015. All of our data is geocoded and we create school choice sets for each house, containing all the schools within 2,000 meters (or five minutes drive time) from its location. Within these choice sets, we call a school more attractive if it sends more students to the academic secondary school track compared to the neighborhood school. We focus on this measure because it captures both the causal effect of schools on learning as well as the quality of peers, with peer quality in particular being of high importance to parents (Abdulkadiroğlu et al., 2020; Imberman and Lovenheim, 2016). We define a house as having improved school choice (*treated*) if there is a more attractive school in the local choice set. The treatment group is compared to houses without a more attractive or with no school in the choice set (control houses) before and after the school choice reform. Using a difference-in-differences approach, we compare the evolution of house prices for houses that have a more attractive school in their local choice set to the evolution of prices for houses in the control group. Since treatment variation stems from distance (or drive time) to alternative schools outside the SCA, we can hold house price levels fixed in comparisons between treated and control houses at the level of the previously binding SCAs. We then compare houses in the same SCA where, due to arbitrary locations within the SCA, one house has improved school choice after the reform and another does not.

We find that access to a more attractive school increases house prices by 1.5 percent,

equivalent to an increase of 24 EUR per square meter (evaluated at the median house price). The reform effect takes roughly five years to fully materialize and then stabilizes at just above 2 percent. In addition, we find that improved school choice for public schools has a stronger impact in neighborhoods that are particularly populated by families, consistent with Greaves and Hussain (2024), who argue that school amenities are more highly valued where more parents live. We further document how far improved school choice is capitalized into house prices by looking at the precise distances between homes and schools. We estimate a spatial decay function and find that having a more attractive alternative school within 500 meters increases house prices by approximately 3.5 percent after the reform. This effect diminishes with distance and becomes close to zero beyond 2,250 meters. Finally, we find that a one standard deviation increase in the academic track rate of the best accessible alternative school increases house prices by 0.8 percent.

This paper relates to a large literature that documents how differences in local school quality are reflected in house prices. Much of the evidence comes from settings where place of residence determines assignment to schools (Black and Machin, 2011). In a seminal paper, Black (1999) compares houses in close proximity but on either side of SCA boundaries (and hence access to different schools), and finds a house price premium of 2.5 percent for a one standard deviation increase in average school test scores. Her boundary approach has since been replicated in various contexts, and the literature on zoned school quality typically finds that house prices increase on average between two and four percent in response to a one standard deviation increase in test scores (Black and Machin, 2011; Nguyen-Hoang and Yinger, 2011).

While studies applying the boundary design show that school quality is reflected in house prices, more recent work highlights how school choice programs reduce the capitalization effect of zoned school quality (Zheng, 2022; Machin and Salvanes, 2016; Brunner et al., 2012). If school admission is not strictly determined by catchment area assignment (*e.g.*, in the presence of choice schools or open enrollment policies), households can live elsewhere and maintain access to desired schools, weakening the capitalization effect of the neighborhood school. This is shown for the expansion of school choice through charter schools (Zheng, 2022), private schools (Fack and Grenet, 2010) and other choice schools (Schwartz et al., 2014). There are similar effects under open enrollment policies. Brunner et al. (2012) study intra-district choice in selected US states and find that house prices and population density increase in districts where more students attend out-of-district schools. Machin and Salvanes (2016) show that the capitalization effect of local school quality falls by roughly 50 percent after the introduction of open enrollment for high schools in Oslo in 1997.¹ Taken together, the existing literature establishes a causal relationship between zoned school quality and house prices and shows that school choice programs reduce this capitalization effect.

As our main contribution to this literature, we offer a complementary but distinct view on the valuation of school choice that explicitly considers local choice opportunities among public schools. To this end, we examine the house price response to highly localized school choice sets for traditional public schools after the removal of binding catchment areas in NRW, Germany. Our spatially granular data allows us to analyze choice where it is actually prevalent, whereas previous studies had to rely either on more aggregated variation at the district or catchment area level or were not able to control for catchment area fixed effects. Furthermore, the precise spatial nature of our data enables us to investigate the accessibility of schools in the choice set, a previously understudied dimension. This allows us to present new findings on how far in distance school choice is capitalized in house prices.

Our paper is also related to the literature on the effects of choice schools on house prices. Access to choice schools (such as charter, magnet, denominational or private schools) may be valued by parents as an amenity if there are otherwise binding catchment areas for traditional public schools. Andreyeva and Patrick (2017) show that having priority access to entering charter schools increases house prices by six to eight percent. Similarly, Zheng (2022) finds a house price effect of around five percent three to four years after a charter school enters within five miles.

We add to this literature by estimating the effect of school choice among traditional public schools. This provides a new perspective to school choice through charter school expansion in the US (*e.g.*, Andreyeva and Patrick, 2017; Zheng, 2022) by broadening the object of study from the narrow type of charter schools. This is important because policymakers may want to consider the potentially different housing market consequences of different school choice programs (*e.g.*, charter expansion and/or changes in

¹Other studies that examine the diminished importance of SCA boundaries following open enrollment policies include Chung (2015) and Reback (2005). Bettinger and Bogart (2001) find an opposite pattern for Michigan's "School of Choice" legislation, suggesting that student flows and institutional restrictions on choice can limit house price responses.

catchment area assignment). In fact, the effect of charter school entry of around five to seven percent (Andreyeva and Patrick, 2017; Zheng, 2022) is much larger than our estimate of 1.5 percent for improved choice for a traditional public school. To understand why our estimate of school choice among public schools is smaller compared to choice of charters, we consider different potential explanations. For instance, charter schools have an incentive to locate in areas where local schools are underperforming, presumably because charter schools can then attract students more easily. The entry of a charter into these areas may mark a more pronounced improvement in local schooling options than allowing choice between already existing schools, which may also explain the larger effects of charter entry on house prices. In addition, charter schools may be seen as providing a more distinct choice relative to traditional public schools, since charters typically have different management principles, are held more accountable for student outcomes and may offer specialized curricula.² This highlights the importance of looking at choice between traditional public schools, as charter schools are a specific and less common feature of school systems.

In our empirical approach, we can control for time-constant unobserved differences across catchment areas that can exist due to prior sorting. Studies of zoned school quality typically make comparisons across catchment area boundaries and Bayer et al. (2007) find that the capitalization effect of school quality is halved when neighborhood characteristics are controlled for (indicating sorting across boundaries). Some studies use variation in SCA rezoning over time to account for prior sorting (Bogart and Cromwell, 2000; Ries and Somerville, 2010). Nevertheless, the rezoning of catchment areas may follow demographic trends that at the same time systematically affect the housing market. Our empirical approach differs in that we use a school choice reform that removed SCAs altogether, which allows us to control for prior sorting by including SCA fixed effects. Machin and Salvanes (2016), who study the introduction of open enrollment for high schools in Oslo in 1997, use catchment area fixed effects (sometimes together with the boundary design) to show that the capitalization effect of local school quality falls by roughly 50 percent after the introduction of open enrollment. While their approach accounts for prior sorting on observed and unobserved time-constant factors (as does ours), they rely on variation at the level of SCAs. In contrast, we

²Moreover, changes in school admission policies (for existing schools) may be less salient than the introduction (*e.g.*, construction) of new schools, which may drive effects in the housing market.

leverage variation in the Euclidean distance from houses within the same (previously binding) catchment area to alternative schools that may or may not be accessible after the reform introduced free choice.

We contribute to the existing literature in several ways. First, we provide a distinct perspective on how local school choice for public schools, created by the removal of binding catchment areas, affects house prices. By exploiting precise distances between houses and schools, we document that a more attractive alternative school is valued up to around 2,250 meters on average. Second, we add an estimate of the improved choice for traditional public schools upon the removal of binding catchment areas. Previous research has shown how school choice in the form of access to charter schools is valued in the housing market. However, a charter school is a very specific type of school and the effects of choice among traditional public schools may be of broader interest to policymakers. Finally, the spatial granularity of our data enables us to take a novel empirical approach to estimate the effect of school choice on house prices. Instead of relying solely on variation across catchment areas, we consider potentially different school choice sets for locations within the same catchment area. By leveraging precise distances between houses and schools outside their designated catchment area, we can compare houses within the same formerly binding catchment area but with different school choice sets.

The rest of the paper proceeds as follows. In Section 2, we describe the institutional setting. In Section 3, we introduce our data. The empirical strategy is discussed in Section 4 and we present results in Section 5. We conclude in Section 6.

2 Institutional Setting

2.1 School System of North Rhine-Westphalia

Germany's education system is decentralized, but all states follow a general structure of primary and secondary education. In North Rhine-Westphalia (NRW), the length of primary education is four years. There is essentially one type of school for primary education, the *Grundschule*, which is attended by all students except those with special needs.³ With the mid-term report in the fourth grade, parents receive a recommenda-

³We exclude primary schools that exclusively cater to students with special needs from the analysis because special needs primary schools have their own, separate catchment areas. In the 2007/08 school

tion from the primary school teacher regarding their child's further education. This recommendation was binding until 2010, and since then is only intended to help parents make an informed decision.

Secondary education consists of several types of schools, the most common being the academic track (attended by around 39 percent of students in 2007, the prereform year), the intermediate track (29 percent), the basic track (15 percent) and the comprehensive track (17 percent). Secondary education can last up to nine years, with compulsory schooling comprising ten years.⁴ Each type of school serves different educational purposes. The *Hauptschule* (basic track) provides the most fundamental education, qualifying graduates for certain vocational training programs. The *Realschule* (intermediate track) prepares students for vocational education, with the possibility to continue in another school to qualify for university education. *Gymnasiums* (academic track) directly prepare students for university education. Finally, the *Gesamtschule* (comprehensive track) is a mixture of the different school types, allowing students to earn any of the three diplomas, with the potential to qualify for university depending on the student's performance.

Due to the early separation of students into different types of schools after the fourth grade, primary school is of great importance. This is particularly true because, while it is possible to change tracks, upward mobility is rare (Bellenberg and Forell, 2012). The academic track is the main route to the highest school-leaving degree, *Abitur*. Evidence for Germany suggests that assignment to the academic track is highly correlated with university enrollment and higher earnings later in life (Dustmann, 2004). Although the *Abitur* can be obtained in the comprehensive track, parents may still value the academic track more because of the peer group, the opportunity to learn more languages, and the higher social and academic prestige.

2.2 The School Catchment Area Reform

Drawing school catchment areas is the predominant assignment mechanism for primary schools in Germany (Breuing, 2014). In NRW, SCAs were in place until the 2008/09 school year. Until then, school authorities (the *Schulträger*) defined catchment

year, around 5 percent of primary school students in the state attended primary schools that only taught students with special needs (Ministerium für Schule und Weiterbildung des Landes Nordrhein-Westfalen, 2008).

⁴For *Gymnasium* students, compulsory schooling ends after 9th grade.

areas and children had to attend the neighborhood school in whose catchment area they lived. Parents had to comply with the assignment, but could make a reasoned request for their child to attend a public school outside their designated SCA. The reasons for such exemption requests were usually related to factors such as the availability of childcare facilities in other SCAs, rather than the quality or student composition of the schools themselves (Makles and Schneider, 2012).

The assignment system in NRW, even with binding catchment areas, offered some school choice through the unique presence of denominational schools. Denominational schools are public primary schools run mainly by the Catholic or Protestant churches, but offer the same curriculum as state primary schools. Parents can choose to send their child to a denominational school instead of their assigned state school. However, if the denominational school is located outside the child's school district, the child must attend the denominational school's respective religious education classes in order to attend the school. Denominational schools are unevenly distributed across NRW, being the exceptional school type in most municipalities and the typical school type in others. We use this variation in Section 5.2 to show that in locations with denominational schools nearby—and thus with some choice option even before the reform—the effect of the reform on house prices is close to zero and statistically insignificant.

By the school year 2008/09, parents in NRW were free to choose any primary school for their children.⁵ After the change of state government in 2005, when the Social Democrats did not lead the NRW government for the first time since 1966, the state parliament passed the school choice reform in 2006. The motivation to remove binding SCAs was to promote competition between schools and to respect parents' preferences regarding the primary school their child would attend. After the reform came into effect in the 2008/09 school year, parents were free to choose any primary school in NRW. However, every child still had the right to attend the primary school closest to his or her home (see §46 *Schulgesetz NRW*), and oversubscribed schools could deny access to children with a closer neighborhood school. These rules limit the extent of free school choice, when many students favor few primary schools. However, the reform coincided with a period of demographic change characterized by a declining number

⁵While the reform applies to all of NRW there were 15 (out of 396) municipalities that had voluntarily implemented the reform a year earlier. These municipalities are Arnsberg, Düsseldorf, Euskirchen, Hagen, Hamm, Herscheid, Iserlohn, Korchenbroich, Netphen, Oelde, Porta Westfalica, Rheda, Siegburg, Tönisvorst and Werdohl. We exclude these municipalities from our estimation sample.

of school-age children (see Figure A1). Declining student numbers should reduce the pressure on over-subscribed schools, and thus limited choice due to oversubscribed schools should be less of a concern in our setting.

The SCA reform facilitated school choice beyond denominational schools and justified exemption requests both legally and practically. While school choice existed to some extent before the reform (Riedel et al., 2010), more parents exercised school choice after the reform came into effect (Groos, 2015; Makles and Schneider, 2012). For example, using data on all children entering primary school in Mülheim an der Ruhr, a large city in NRW, Groos (2015) reports that around 10 percent of children did not attend their assigned neighborhood school before the removal of SCAs. By the 2008/09 school year, this figure had increased to 14.6 percent, and by 2011–2013, about 25 percent of children were opting out of their neighborhood school. Similarly, Makles and Schneider (2012) compare the share of students selecting non-neighborhood schools in Wuppertal, another large city in NRW, between the school years 2007/08 (pre-reform) and 2008/09 (post-reform). In the first year of free choice, 20.8 percent of students chose a public school outside their former SCA, up from 14.9 percent before the reform.

Proximity to school and the school-level academic track rate are strong determinants of primary school choice in NRW (Schneider et al., 2012; Makles and Schneider, 2012; Riedel et al., 2010). To these existing findings, we add evidence from school-level administrative data (presented in the next section) on the number of students in the final year of primary school. Our descriptive results show that schools in the bottom quartile of the pre-reform distribution of academic track rates lose substantially more students than schools in the top quartile after the school choice reform takes effect (see Figure A1). Although these results are descriptive, they suggest that parents are responding to the reform by choosing schools based on schools' academic track rates.

3 Data

House Listings Data

For our house-level analyses we use the RWI-GEO-RED data (RWI and Immobilien-Scout24, 2023). This data set contains all listings of the online platform immobilien-

scout24.de, which is the market leader in Germany for online listings of apartments and houses. The data includes extensive information on object characteristics and, depending on the type (for sale/for rent), the asking price or rent. Listings are geocoded at their address. For a more detailed description of the data, see Schaffner (2020).

We focus on house sales data for several reasons. First, although most people in Germany are renters, house prices may better reflect forward-looking behavior. Home buyers, unlike renters, consider not only the current price but also the potential future resale value of the house. Ahlfeldt and Maennig (2015) show this difference in behavior between homeowners and renters in the context of airport noise. Second, we study the effects of a school choice reform, and as Greaves and Hussain (2024) argue, schooling amenities should be more highly valued by parents with school-age children. In Figure B1, we show that the decision to become a homeowner coincides with the arrival of children. Households increasingly buy (rather than rent) at a time when schooling amenities may be more salient and important, and thus motivating our choice to study prices rather than rents. Therefore, we consider house sales data, using the logarithmic price per square meter as our outcome variable. Panel B of Table A1 shows the extensive set of object characteristics used as control variables.

Since listing data are more prone to duplicates or unrealistic outliers than administrative transaction data, we follow Ahlfeldt et al. (2023) and Klick and Schaffner (2021) to clean the data. After restricting the full data set to observations in NRW between 2007 and the first half of the year 2015, the sample consists of 1,858,544 listings. Restricting to having no missing values in coordinates, floor space, and price reduces the sample to 1,639,464 listings. We then remove outliers following Ahlfeldt et al. (2023): prices per square meter must be above 250 EUR, living space must range between 30 and 500 square meters, and land area must be below 25,000 square meters. Furthermore, the price per square meter must be between 20 and 500 percent of the county median, resulting in a sample of 1,597,464 listings. In addition to Ahlfeldt et al. (2023), we trim the highest one percent of (non-zero) monthly rental income, and restrict listings to a maximum of nine rooms and four bathrooms. Finally, we exclude entries with missing values in merged data sets on neighborhood characteristics, reducing the sample to 1,444,792 listings. After removing observations from municipalities that adopted the reform early, our final estimation sample consists of 1,359,662 listings.

To handle missing values in some object characteristics, we introduce a dummy

| | Unique | Mean | SD | Min | Median | Max |
|-----------------------|--------|-------|-------|-----|--------|-----|
| Number Fourth Graders | 134 | 55.08 | 23.12 | 1.0 | 51.0 | 226 |
| Academic Track Rate | 1146 | 0.38 | 0.15 | 0.0 | 0.4 | 1 |
| Share German Pupils | 822 | 0.89 | 0.12 | 0.3 | 0.9 | 1 |
| Share Female Pupils | 759 | 0.49 | 0.08 | 0.0 | 0.5 | 1 |
| Denominational School | 2 | 0.36 | 0.48 | 0.0 | 0.0 | 1 |
| Urban | 2 | 0.75 | 0.43 | 0.0 | 1.0 | 1 |
| School Closure within | 2 | 0.08 | 0.27 | 0.0 | 0.0 | 1 |
| 2,000 meters | | | | | | |
| N. of Observations | 3129 | | | | | |

Table 1: Summary Statistics for Primary Schools in 2007

Notes: This table presents summary statistics for all primary schools in 2007 in our sample. We report the number of unique values, mean, standard deviation, minimum, median and maximum value for the full sample. *Denominational School* and *Urban* are indicators taking the value one if the school is a denomination school (mainly Catholic or Protestant) or if the school is located in an urban county (following the administrative RegioStaR 2 definition (BMVI, 2018)) and zero otherwise. *School Closure* takes the value one if another school within 2,000 meters closed between 2007 and 2015, and zero otherwise. The unit of observation are primary schools. All variables regarding the student body refer to the schools' fourth graders.

variable for missing values and replace the missing values with zeros, following the approach of Klick and Schaffner (2021). Because sellers have to provide a lot of information, categories such as basement availability or rental income often have missing data. These likely indicate that there is no basement or that the property is not rented, but this cannot be confirmed. We follow the above approach to avoid being overly restrictive in constructing the sample.

School Data

We obtain transition rates from primary to secondary schools for the universe of primary schools in NRW from the State Statistical Office of NRW (IT.NRW). This data set includes the number of students in the fourth grade, their composition by sex and by German citizenship, and the transition rates to the different types of secondary schools between the school years 2005/06 and 2015/16.

Table 1 describes the school data for the last pre-reform year 2007. In the 2007 cross-section, there are more than 3,000 primary schools and the average number of fourth graders per school is 55. Almost 40 percent of fourth graders leave primary school to attend an academic track secondary school. A significant proportion of the schools, 36 percent, are denominational, mainly Catholic or Protestant. As NRW is a predominantly urban state, three out of four schools are located in an urban area.

As shapefiles of school catchment boundaries are not available for the whole of NRW, we approximate the school catchment areas ourselves based on the location of the schools. We define the closest school in terms of Euclidean distance within the municipality as the designated SCA school, *i.e.* the neighborhood school.⁶ In addition, to display information at the SCA or school level, we find the nearest school for the centers of all $100m \times 100m$ grids in NRW, and then combine all grid cells assigned to a school into the corresponding SCA.

We validate the SCA assignment procedure by comparing the approximated with the real catchment areas for the city of Mülheim an der Ruhr, for which we know the exact SCAs as of 2008. Figure 1 shows the constructed SCAs in red and contrasts them with the real SCAs in black. Blue and orange highlight the correctly and incorrectly assigned residential and mixed areas, respectively. While we document a large overlap between constructed and real SCAs, there is also a sizable amount of misclassification. We correctly assign 81.4 percent of residential and mixed areas, and 89.5 percent in the scenario excluding areas close to the constructed boundaries (see Figure A2). In terms of observed houses, we assign 73.8 percent of all listings in Mülheim to the correct SCA (see Figure A3). If we exclude areas close to the constructed boundaries, we correctly assign 80.0 percent of the listings. Assuming that this rate is similar for the other municipalities, there is a considerable measurement error. However, as long as this measurement error is classical, our estimate will be biased towards zero and we would be underestimating the true effect of the reform. In Table A2, we provide suggestive evidence that correctly and incorrectly specified objects do not differ much in their observed characteristics for the sample of listings in Mülheim an der Ruhr prior to the reform.

Figure 2 presents the resulting SCAs for the whole of NRW, shaded according to the average asking prices for houses. The structure of the SCAs is as expected: rural areas have larger SCAs due to lower population density. The map also shows that urban areas are more expensive on average. Notably, some urban SCAs still fall into the lowest price bracket. Hence, we see variation in house prices both between and within municipalities. The SCAs outlined in red are excluded from our analysis because they

⁶We consider two modifications of this procedure as robustness checks. First, we use the drive time instead of Euclidean distance to assign the neighborhood school. Second, we exclude houses that are close to our approximated SCA boundaries, because locations that are close to the approximated boundaries have a higher probability of being misassigned to the wrong neighborhood school.



Figure 1: Real and Constructed SCAs in Mülheim an d. Ruhr Notes: The map shows real and constructed SCA boundaries together with correctly and falsely assigned residential and mixed areas. Source: © OpenStreetMap CARTO. Herausgeber: Stadt Mülheim an der Ruhr Amt für Geodaten, Kataster und Wohnbauförderung.

correspond to municipalities that implemented the reform a year earlier.

Socio-demographic Neighborhood Characteristics

To control for the socio-demographic composition of neighborhoods, we use information from the RWI-GEO-GRID data (RWI and microm, 2023). The RWI-GEO-GRID covers Germany's populated areas at the $1km \times 1km$ grid level and includes approximately 225,000 grid cells for the period 2005 and 2009–2017. It is hosted by the Research Data Center Ruhr at the RWI – Leibniz-Institute for Economic Research and, like the RWI-GEO-RED, available for non-commercial research as a Scientific Use File. Information on the socio-economic and demographic composition of the grid cells was originally collected by Micromarketing-Systeme und Consult GmbH (microm), a commercial micro- and geomarketing provider. A more detailed description of the data can be found in Breidenbach and Eilers (2018). We merge each house with the RWI-GEO-GRID data and enrich the house data with local neighborhood characteristics such as the share of households with children, population density, the unemployment



Figure 2: Average house prices per m² at SCA level in 2007

Notes: The map illustrates the approximated SCAs and shows the respective average listing prices of houses in 2007 for NRW. Note that we perform the empirical analysis at the level of houses, not at the level of catchment areas. Red borders indicate SCAs located in early adopter municipalities which are excluded from the analysis.

rate, and average income per capita (see Table A1).

4 Empirical Strategy

4.1 Reform Bite and Treatment Group Assignment

We aim to estimate the effect of having access to a *more attractive* school on house prices. Although the reform removed binding school catchment areas across NRW, we argue that it only affected school choice in certain locations, *i.e.* the reform had different *bite*. We construct a binary bite measure that indicates whether local school choice opportunities improved as a result of the reform. Given the bite indicator, our empirical approach resembles a difference-in-differences setup with repeated cross-sections of house listings. That is, we compare the evolution of house prices in a treatment group with the evolution of prices in a control group.

Treatment group assignment is a function of the attractiveness of the neighborhood school and both the attractiveness and accessibility of the alternative schools. The attractiveness of a school is measured by the share of students who continue in the academic track after leaving primary school. We first assign to each house the academic track rate of the school in its pre-reform SCA, *i.e.* the neighborhood school.⁷ For each house, we then construct a set of alternative schools within a radius of 2,000 meters. Houses are included in the treatment group if an alternative school has a higher academic track rate than the neighborhood school (both measured in 2007, before the reform) and is located within 2,000 meters distance. Conversely, houses are included in the control group if there is no school with a higher academic track rate (compared to the neighborhood school) within 2,000 meters. Homes without an alternative school within the 2,000 meters are included in the control group. We end up with around 610,000 houses in the treatment and around 750,000 houses in the control group (see Table 2). That is, almost 610,000 houses are located in a (former) catchment area where at least one school with a higher academic track rate (compared to the neighborhood school) is accessible within 2,000 meters. As a robustness check, we repeat our analyses using drive time instead of Euclidean distance, with a threshold of five minutes.

With this way of assigning locations to the treatment and control group we try to mimic the primary school choice of parents. First, distance is one of the most important factors in school choice (Burgess et al., 2015). Second, for the reform under study, Schneider et al. (2012) and Makles and Schneider (2012) show that academic track rates are important determinants of school choice.

Both the 2,000 meters and the five minute thresholds are certainly somewhat arbitrary. If parents value more attractive schools further away than these thresholds, then our control group is partially treated (Butts, 2023). Our approach of defining a set of feasible schools by geographical constraints is in line with previous work. For example, Burgess et al. (2015) construct "high-probability feasible choice sets" of primary schools in England based on actual student enrollment. They find that the closest 80 percent of students live on average 2,450 meters away from their school. This figure is 1,995 meters in metropolitan areas and 4,689 meters in rural areas. As NRW is a state with a largely urban character, we consider the choice of the 2,000 meters

⁷As administrative SCA boundaries are not available for all of NRW, we approximate the SCAs as described in Section 3.

threshold to be reasonable. Later in the paper, we relax the assumption of the correct distance cutoff and estimate a treatment effect function for 250 meters intervals.

The abolition of binding SCAs changed the opportunities for school choice in some places, but not in others. Before the reform, access to (more attractive) schools in neighboring catchment areas was largely restricted. The end of binding SCAs then provided some houses (about 500,000 houses in the treatment period sample) with alternative schooling options (here the reform bite). However, houses that either already had the (locally) most attractive neighborhood schools before the reform or did not gain access to other nearby schools became potentially less valuable (around 640,000 houses in the treatment period sample). Thus, we exploit variation in the assignment of treatment both between and within catchment areas. In other words, two houses in a given (formerly binding) catchment area may have different treatment statuses due to different sets of alternative schools.

Table 2 shows summary statistics of key variables by treatment status. The average price per square meter for houses with access to a more attractive school relative to the neighborhood school is 100 EUR (6.5 percent) higher than for houses without such access. This difference is probably due to the different share of houses located in urban areas. Houses in the treatment group are mostly located in urban areas (86 percent), while the urban-rural distribution in the control group is more even (69 percent urban). Correspondingly, the average distance to the neighborhood school is 400 meters shorter in the treatment group. To ensure that the estimated effect of the reform does not simply capture the different house price trends between urban and rural areas, we control for county-specific trends and repeat our analysis separately for the urban and rural subsample, where we find quantitatively similar effects (see Table A4). Moreover, the summary statistics show that SCA schools in the treatment group have, on average, a 9.2 percentage points lower academic track rate. This difference is partly mechanical, as we define the treatment group as having at least one school with a higher transition rate outside its corresponding SCA.

Figure 3 shows the evolution of average house prices over time by treatment status. Average asking prices are consistently higher in the treatment group. About three years after the reform, the price difference seems to increase. In order to identify the house price effect of the reform, we need to make several assumptions, which are outlined below.

| | Control Group | | Treatment Group | | | |
|--------------------------------------|---------------|-----------|-----------------|-----------|----------------|------------------|
| | Mean | Std. Dev. | Mean | Std. Dev. | Δ Means | Std. Error |
| Price per m ² | 1604 | 665 | 1707 | 641 | 103*** | 1.1 |
| Academic Track Rate of SCA School | 0.44 | 0.14 | 0.35 | 0.13 | -0.092*** | 0.000 23 |
| Distance to SCA School | 1039 | 964 | 633 | 423 | -405*** | 1.2 |
| After Reform | 0.8 | 0.4 | 0.79 | 0.41 | -0.012^{***} | $7	imes 10^{-4}$ |
| Urban | 0.69 | 0.46 | 0.86 | 0.34 | 0.18*** | 0.000 69 |
| N. of Observations | 74 | 19 629 | 61 | 0 033 | | |

Table 2: Listings Summary Statistics by Treatment Group Status Notes: The unit of observation are housing listings. The transition rate corresponds to the transition rate of the neighborhood school to the academic track in 2007. + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

4.2 Identification Strategy

In our difference-in-differences approach, we compare changes in house prices of the treatment group with those of the control group before and after the NRW school choice reform in 2008/09. Treatment group status is determined by local schooling opportunities, and we use variation in local school choice sets of houses sharing the same 2007 catchment areas to identify the effect of the school choice reform on house prices. The variation in local school choice sets arises from differences in the Euclidean distance between houses and alternative primary schools outside the shared SCA. While under the catchment area regime households were likely to sort into SCAs taking into account the designated neighborhood school (Bayer et al., 2007), treatment assignment depends on the Euclidean distance to primary schools that are *not* the neighborhood school. As the catchment area system restricted access to these other schools, we consider the systematic sorting of households within SCAs with respect to alternative schools to be a minor concern.

We leverage variation in the Euclidean distance between houses and alternative schools to construct the local set of schools available to households. Because their exact location within the catchment area varies, houses in the same catchment area may have different alternative primary schools in their local choice set (*i.e.*, all primary schools within 2,000 meters). We use the local choice set to define the treatment group status and assign houses to the treatment group if they have an alternative school with a higher academic track rate (compared to the neighborhood school) within 2,000 meters)



Figure 3: Average Asking Prices for Treated and Control Houses over Time *Notes*: The figure shows the average asking price per square meter of house listings by treatment status over the time from 2007 to 2015. Objects are treated if there is at least one school within 2,000 meters with a higher transition rate to the academic track than the neighborhood school. The vertical line indicates the time when the reform came into effect.

distance. We provide an example and visualization of the treatment group assignment in the Appendix (see Figure A5 and the corresponding note). The school choice reform that came into effect in the 2008/09 school year provides us with variation over time and we can compare the treatment group with the control group before and after the reform.

Identifying the effect of the reform on house prices requires, at its core, the parallel trends assumption inherent in difference-in-differences settings. We require that, in the absence of the school choice reform, house prices for homes with an alternative school with a higher academic track rate within 2,000 meters (treated) would have followed the same trend as house prices for homes with no such school or any school within 2,000 meters (control). This assumption would be violated if there were a third factor correlated with both the treatment and the change in house prices over time. For example, locations in more densely populated areas may have more schools in close proximity and therefore be more likely to be treated (*i.e.*, have better school choice), while at the same time more densely populated areas have experienced greater increases in house prices over time. We address this concern in several ways.

We begin by controlling for population at the grid cell level and include SCA

fixed effects that account for time-invariant differences in house prices by density. We therefore assume that the parallel trends assumption holds conditionally. That is, we allow the evolution of untreated potential outcomes to depend on the level of neighborhood characteristics measured before the reform and we hold time-invariant SCA characteristics constant. Next, in our preferred specification, we include county-specific trends in house prices to allow for differential price evolution in local housing markets over time. Additionally, we conduct our empirical analysis separately for urban and rural subsamples (following the administrative RegioStaR 2 definition (BMVI, 2018)) and we find similar house price effects of the reform in the two subsamples. Finally, we assess the plausibility of the conditional parallel trends assumption descriptively by running an event-type specification of the empirical model. In Figure 4 we show that the pre-treatment coefficients are not statistically different from zero. We now discuss the other identification assumptions in turn.

First, the stable unit treatment value assumption (SUTVA) rules out spatial spillovers and general equilibrium effects induced by the reform. In our setting, the SUTVA assumption could be violated by general equilibrium effects because we are comparing houses in the same housing market. For example, if it becomes more attractive to live in the part of an SCA that is close to many desired schools, the demand for houses in that part may increase, while the demand for houses in parts of the SCA without access to more attractive alternative schools may decrease. In this case, we would overestimate the typical average treatment effect on houses that gained improved school choice through the reform (*i.e.*, the ATT). What we are identifying is the impact of the reform on treated locations relative to non-treated locations, with the impact being driven by house price changes in both groups. We believe that this treatment effect is of interest because it reflects the difference in house prices in locations with and without improved school choice as a result of the reform.

Second, we have to assume that the effects of the 2008/09 reform will not materialize in earlier periods. As the reform was already passed in 2006, there is theoretically room for anticipation (Ding et al., 2024). Although we cannot rule out anticipatory behavior, we believe that it is not critical in our setting. There is evidence that the share of children exercising school choice gradually increased after the SCA reform came into effect and leveled off about three to four years after the reform (Groos, 2015). This suggests that it takes time for parents to learn how to "play by the new rules" and take advantage of the choice opportunities. Therefore, we expect no house price effect before the reform, but we expect an increase in the house price premium for improved choice in the first years after the reform comes into effect. We observe such a pattern in our event-type regression shown in Figure 4, where we do not detect any pre-trends.

Third, we assume that our distance cutoff of 2,000 meters (and five minutes drive time) is correct. That is, parents do not value having access to a more attractive school beyond 2,000 meters, otherwise our control group would be partially treated (Butts, 2023). Later, we relax the correct distance cutoff assumption and estimate a treatment effect function for 250 meters intervals.⁸ This exercise provides evidence that our choice of cutoff indeed proves useful, as the treatment effect approaches zero after about 2,000 to 2,250 meters distance.

Our identification strategy relies on comparing treated and untreated houses before and after the removal of mandatory SCAs in 2008/09. The reform we study took effect at a single point in time⁹ and our treatment is binary. Thus, the setting we study avoids many design aspects that are prone to produce misleading estimates under heterogeneous treatment effects due to inappropriate comparisons (de Chaisemartin and D'Haultfœuille, 2020). However, adjustment for continuous time-varying control variables can be problematic (Caetano and Callaway, 2024; Sant'Anna and Zhao, 2020; de Chaisemartin and D'Haultfœuille, 2020). We necessarily adjust for contemporaneously observed house characteristics in the hedonic price function.

Finally, the use of listing data from an online platform warrants some caveats with respect to identifying the effects of the reform on house prices. First, the listing data cover only a selected sample of the housing universe. However, immobilienscout24.de covers an estimated 70 percent of all housing transactions (Bundeskartellamt, 2016), making it the market leader among online marketplaces for residential and commercial real estate. While this is a large share of the market, there could be an endogenous selection of houses that appear on online platforms. This could potentially affect the external validity of our results.

⁸Relaxing the correct cutoff assumption then only requires that the true distance cutoff (beyond which the treatment effect is zero) lies between zero and some distance value without the need to specify the true (unknown) cutoff (Butts, 2023). To allow for this relaxation, we must assume that the counterfactual untreated trend must be constant across the distance intervals. This is a more stringent assumption than in the case where the distance cutoff is correctly determined and parallel trends need to hold only on average (Butts, 2023).

⁹There were 15 out of 396 municipalities in NRW that voluntarily adopted the reform in the 2007/08 school year. We exclude house listings from these early-treated municipalities in all of our analyses.

4.3 Estimation

To estimate the effect of gaining access to a more attractive school, we use the standard hedonic regression model with *log* house prices per square meter *y* (Black and Machin, 2011). Our units of observation are houses *i* with assigned pre-reform catchment areas *c* (see Section 3). In addition, houses are mapped onto a $1km \times 1km$ grid referenced by *r* to control for pre-reform grid-level characteristics subsumed in $W_{r,2005}$. Time is biannual, and we consider school semesters *t* as our time periods. Starting with the winter semester 2008/09 the reform indicator $After_t$ becomes one, and before that is zero. The coefficient of interest is τ , which governs the interaction between the treatment group indicator and the reform indicator.

The estimation equation reads

$$y_{i,r,c,t} = \beta \ Bite_i + \phi_t + \sigma_c + \tau \ Bite_i \times After_t + \eta' \ X_{i,t} + \gamma' \ W_{r,2005} + \epsilon_{i,r,c,t}$$
(1)

where ϕ_t and σ_c are period and catchment area fixed effects, respectively. The vector $X_{i,t}$ contains the set of house characteristics as listed in Panel A of Table A1. We cluster the standard errors at the level of SCAs as of 2007.

In presenting our main results we gradually build up the specification outlined in the estimation equation above. First, we include SCA fixed effects in the regression model. The rationale is that households may have sorted across SCAs to access different schools long before the reform.¹⁰ Such patterns of selection may affect house or neighborhood characteristics (Bayer et al., 2007). For example, families that respond to differences in school characteristics by sorting into attractive SCAs may also place more weight on house maintenance (Machin and Salvanes, 2016). House maintenance is largely unobserved by the researcher and may affect the evolution of house prices. To keep unobserved characteristics at the level of (previously binding) SCAs constant, we include SCA fixed effects. In addition, we control for regional characteristics observed before the reform at the level of $1km \times 1km$ grids (see Panel C of Table A1 for summary statistics).

Finally, we control for school closures that occurred within a 2,000 meters radius

¹⁰For example, Roy (2022) presents evidence of neighborhood sorting after zones of choice were introduced in certain Los Angeles high school markets. However, Greaves (2023) shows that neighborhood sorting is not widespread in England and that house price effects are driven by a small share of affluent households.

of the house. Between 2007 and 2015, around 15 percent of primary schools in NRW were closed or were merged with other schools. School closures occurred at a time of demographic change, when there were fewer children to educate. The reform that abolished the SCAs therefore provided political support for school closures, as parents were able to "vote with their feet". To control for local school closures, we include an indicator that turns (and stays) one when a school closed within 2,000 meters of the house.¹¹

5 Results

5.1 Main Results

Table 3 reports the estimated effect of the reform on the asking prices of house listings that gain improved school choice. Column (1) reports results for the hedonic house price model, controlling only for house characteristics and year fixed effects. We gradually include SCA fixed effects, regional control variables and county-specific time trends (columns (2) through (4)). The estimate of τ , the coefficient of interest, is 0.015 in our preferred specification (column (4)) and ranges from 0.022 to 0.033 in less specified models. The estimates are statistically significant at the 0.1 percent level. Based on model (4), our results indicate that gaining access to a more attractive school increases house prices by 1.5 percent. Consequently, parents value improved schooling opportunities at about 24 EUR per square meter (evaluated at the median house price in our sample). We replicate these results in Table A3, where treatment group assignment is constructed using drive time, and find qualitatively the same result. Quantitatively, the results also differ little, the corresponding estimate for column (4) is 0.016.

Five years after the reform took effect, the estimate for the house price effect of the reform rises to slightly above two percent. See Figure 4, where we plot the event-type pattern of treatment effect estimates for the set of control variables corresponding to column (4) in Table 3. We have two additional remarks to make.

¹¹Including post-treatment characteristics can, in principle, lead to a "bad control" problem. If increased school choice options cause certain schools to close because parental demand for them decreased after the reform, then the overall effect of the reform on house prices also captures the effect of school closures. To make progress on our research question, we want to explicitly rule out this mechanism. Therefore, we control for school closures in our preferred specification. Excluding information on surrounding school closures does not change our main results.

| | (1) | (2) | (3) | (4) |
|---------------------|-----------|-----------|-----------|-----------|
| Bite | 0.071*** | -0.007 | -0.010+ | -0.004 |
| | (0.009) | (0.005) | (0.005) | (0.005) |
| Bite \times After | 0.033*** | 0.022*** | 0.023*** | 0.015*** |
| | (0.005) | (0.003) | (0.003) | (0.003) |
| N. Obs. | 1 359 662 | 1 359 662 | 1 359 662 | 1 359 662 |
| R ² Adj. | 0.255 | 0.551 | 0.553 | 0.557 |
| SCA FE | No | Yes | Yes | Yes |
| Regional Controls | No | No | Yes | Yes |
| County Time Trend | No | No | No | Yes |

Notes: Each column shows in the second row the estimate of τ from equation 1, which captures the effect of gaining access to a more attractive alternative school on the logarithm of the asking price in EUR per square meter. Bite is an indicator variable that takes the value one if a house has a more attractive school than its neighborhood school within 2,000 meters, and zero otherwise. After is an indicator variable that takes the value one if a house listing was posted on the platform after the reform abolishing binding catchment areas took effect, and zero otherwise. Control variables are included as reported at the bottom of the table. Object characteristics (see the list of characteristics in Table A1) and year fixed effects are included in all specifications. The unit of observation is the house for sale listing. Standard errors are

clustered at the SCA level. + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table 3: Effect of Gaining Access to More Attractive School on House Prices

First, the event-type graph allows visual inspection of parallel trends in the pretreatment period. The estimates of the pre-treatment coefficients in the two pre-reform periods are not statistically different from zero, and they are estimated relatively precisely. Since our house data start in 2007, we can only report these two pretreatment coefficients. However, they provide suggestive evidence of the plausibility of the (untestable) common trends assumption.

Second, the dynamic estimates on house prices in Figure 4 show a reasonable pattern of how the reform effect evolves over time. The effect increases up to five years after the reform and then remains constant at slightly above two percent thereafter. The five-year time lag for the full effect to capitalize, we find, is similar to other results in the literature. Machin and Salvanes (2016) find a much larger decline in the house price premium five years after the open enrollment reform in Oslo in 1997/98, compared to the short-run effect after only one year. Similarly, Zheng (2022) shows that capitalization effects of school quality level off four to five years after charter school entry.

The reform effect on house prices we find is 1.5 percent, rising to around 2 percent after five years. This estimate is lower than the house price valuations of charter



Figure 4: Effect of Gaining Access to More Attractive School on House Prices by Time Period

Notes: The figure reports the estimated effect of having a more attractive school than the neighborhood school within 2,000 meters on the logarithm of the asking price in EUR per square meter for each time period relative to the second semester of the 2007/08 school year. As indicated by the dashed vertical line, the reform abolishing binding catchment areas took effect starting with the school year 2008/09. All control variables, *i.e.* time and SCA fixed effects, county specific time trends, regional control variables and object characteristics (see the list of characteristics in Table A1) are included, mirroring the fourth specification of Table 3. The unit of observation is the house for sale listing. Standard errors are clustered at the SCA level. Confidence intervals refer to the 95 percent confidence level.

school entry in the US, which are around 5–7 percent (Andreyeva and Patrick, 2017; Zheng, 2022). One potential explanation for the difference is that charter schools are more likely to enter school districts where they bring a distinct improvement in school quality. Charters have incentives to select districts where they can quickly attract students, which may be easier when local public schools are perceived as less attractive. Descriptive evidence supports this, showing that charter schools tend to locate in areas with low-performing public schools (Ferreyra and Kosenok, 2018; Singleton, 2019). As a result, the house price effects of charter entry would reflect this distinct improvement. In contrast, our estimates reflect the value of having access to alternative schools with higher academic track rates than the neighborhood school, even if these differences are small. Additionally, charter schools are usually managed differently than traditional public schools, held to higher accountability standards for student outcomes and may offer specialized curricula. Charter schools may therefore be viewed as providing a more distinct choice between different schooling experiences, whereas choosing between traditional public schools may not offer the same level of (horizontal) differentiation.

Heterogeneity by Euclidean Distance

Up to this point, we have assumed that school choice is relevant only if alternative schools are located within 2,000 meters (or five minutes drive time). As noted in Section 4.2, we now relax this cutoff assumption and estimate a treatment effect function for 250 meters intervals. This approach allows us to more formally test how far the treatment effect extends in space and to assess the appropriateness of our choice of the 2,000 meters cutoff. It also allows us to explore the heterogeneity by distance to the nearest more attractive school, which a binary treatment might otherwise mask (Butts, 2023). As primary school children (typically aged six to ten years in our setting) are relatively young, distance to school is particularly important to parents. This leads to two hypotheses. First, we expect the reform effect to be close to zero after a distance of around 2,000 meters (the maximum distance that most students travel to primary schools in metropolitan areas in England (Burgess et al., 2015)). Second, we expect that the closest alternative schools with higher academic track rates have the largest effect on house prices because they are less costly to reach.

To test these hypotheses, we run a related regression to Equation 1, but we replace the binary bite measure with indicators for 250-meters-spaced distance bins indicating the distance to the nearest more attractive primary school. Figure 5 shows the resulting coefficients, with houses that do not have a more attractive school within 4,000 meters distance as the reference group. We see that the positive house price effect of gaining access to a more attractive school is mainly driven by the first three distance bins, although the coefficients remain positive for all but one bin up to 2,250 meters. The farthest distance bin that is still statistically significantly positive is the 2,000 to 2,250 meters bin. Therefore, our threshold of 2,000 meters seems to be appropriate. Furthermore, Figure 5 shows the expected spatial decay of the treatment effect: the pattern suggests that more attractive alternative schools are valued more when they are less costly to reach.



Figure 5: Effect of Gaining Access to More Attractive School on House Prices by Distance

Notes: The figure reports the coefficients of the interaction terms between indicator variables for bins of 250 meters distances to the nearest more attractive school and the post-reform indicator. The reference group includes all houses whose distance to the nearest more attractive school is greater than 4,000 meters. The dependent variable is the logarithm of the asking price in EUR per square meter. Because only a small number of houses have a more attractive school within 0 to 500 meters distance, we pool them into one larger bin. Figure A7 shows the distribution of these bins. All control variables, *i.e.* time and SCA fixed effects, county specific time trends, regional control variables and object characteristics (see the list of characteristics in Table A1) are included, mirroring the fourth specification of Table 3. The unit of observation is the house for sale listing. Standard errors are clustered at the SCA level. Confidence intervals refer to the 95 percent confidence level.

Heterogeneity by Academic Track Rate

In this subsection, we estimate the empirical model specified in Equation 1 using the absolute academic track rate of the most attractive school in the choice set (measured before the reform). In line with our empirical approach, we treat houses with no alternative school in the choice set as having a choice of an alternative school with an academic track rate of zero.

Table 4 presents the results with this continuous treatment. The estimate of our preferred specification is 0.055 and significant at the 0.1 percent level (see column 4). The estimate reflects the situation where a primary school with an academic track rate of 100 percent within 2,000 meters becomes available after the school choice reform (compared to locations where the most attractive alternative school has an academic track rate of zero or where the local choice set is empty). For a one standard deviation

| | (1) | (2) | (3) | (4) |
|------------------------|-----------|-----------|-----------|-----------|
| Alternative Track Rate | 0.369*** | -0.039*** | -0.040*** | -0.011 |
| | (0.017) | (0.011) | (0.011) | (0.010) |
| Alternative Track | 0.100*** | 0.090*** | 0.093*** | 0.055*** |
| Rate \times After | (0.010) | (0.007) | (0.007) | (0.007) |
| N. Obs. | 1 367 016 | 1 367 016 | 1 367 016 | 1 367 016 |
| R ² Adj. | 0.313 | 0.551 | 0.554 | 0.557 |
| SCA FE | No | Yes | Yes | Yes |
| Regional Controls | No | No | Yes | Yes |
| County Time Trend | No | No | No | Yes |

Table 4: Effect of Gaining Access to Most Attractive Alternative School' Transition Rate on House Prices

Notes: Each column shows the corresponding regression specification of the main results in Table 3 where we replace the bite of the reform, *i.e.* gaining choice, with the academic track rate of the best alternative school within 2,000 meters. We treat houses with no alternative school within 2,000 meters as having a choice of an alternative school with an academic track rate of zero. After is an indicator variable that takes the value one if a house listing was posted on the platform after the reform abolishing binding catchment areas took effect, and zero otherwise. Control variables are included as reported at the bottom of the table. The dependent variable is the log of the listing price in EUR per square meter. Object characteristics (see the list of characteristics in Table A1) and year fixed effects are included in all specifications. The unit of observation is the house for sale listing. Standard errors are clustered at the SCA level. + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

increase in the academic track rate of an alternative school within 2,000 meters, house prices would increase by 0.8 percent (0.055×0.15).

By comparison, our estimate of a 0.8 percent increase in house prices for a one standard deviation increase in the academic track rate of the best nearby alternative school is relatively small. Typical estimates for zoned school quality are found to be around 2–4 percent for a one standard deviation increase in average test scores (Black and Machin, 2011). This difference does not seem unusual as we are studying the effect of having an improved school *choice*, which contrasts to the literature on zoned school quality where admission is deterministic. When catchment areas are binding, residential location determines access to particular levels of school quality, whereas after the free choice reform, admission to schools that are not the neighborhood school involves a degree of uncertainty.¹² As practical constraints on school choice may persist, our estimate would represent a lower bound on the true effect of free school choice on

¹²We note that oversubscription may not be as much of an issue in this analysis because the number of primary school children has gradually decreased over our study period. That is, in the school year 2007/08 there were 721,113 students attending primary school in NRW, whereas there were 617,310 in 2014/15, a decrease of 14.4 percent (Ministerium für Schule und Weiterbildung des Landes Nordrhein-Westfalen, 2008, 2015).

house prices.

Aside limitations to choice, there may be reasons why parents wish to send their child(ren) to the neighborhood school, even if the transition rate is lower than that of a nearby option. For example, after-school care slots (which were rolled-out in the 2000s and 2010s and were initially rationed) may be allocated to children who live closest, making the neighborhood school become the preferred school for some parents. Access to an alternative school may then be less valuable if location-based rules determine access to certain school features for the neighborhood school.¹³

To explore how the effect of having a choice of alternative schools varies by their academic track rates, we compare houses within 2,000 meters of alternative primary schools that are in different quintiles of the academic track rate distribution to houses with no alternative school nearby, before and after the reform. Figure A8 reports the results of this exercise. Having the choice of an alternative school whose academic track rate is in the highest quintile raises house prices by four percent compared to houses with no alternative school nearby. Although the reform effect decreases as the most attractive alternative school falls into lower quintiles of the academic track rate distribution (with coefficients from quintiles one to four not statistically distinct), it remains positive and significant, suggesting that a feasible choice is preferred to no alternative school within 2,000 meters.

Heterogeneity by Urbanity

The regional distribution of the treatment group indicator differs between urban and rural areas (see Figure A4). To show that our results are not driven by the unequal development of house prices in urban and rural areas, we repeat our analysis for urban and rural areas separately. Table A4 shows the estimates for the urban and rural subsamples (following the RegioStaR 2 definition (BMVI, 2018)). The coefficient of the interaction term is 0.014 in the urban subsample and is highly significant. For rural locations, the estimate is 0.016 and significant at the five percent level. Thus, we are confident that our main estimate is not driven by differences between urban and rural regions.

¹³Also, parental commuting patterns and idiosyncratic preferences may influence school choice.

5.2 Robustness

Valuation of School Choice in Neighborhoods with more Families

Any amenity derived from local schools should be more highly valued in neighborhoods where more parents with school-age children live (Greaves and Hussain, 2024). Households with children approaching or attending primary school are arguably those most directly affected by the 2008/09 school choice reform. While households with older or no children may in principle also value local school amenities (*e.g.*, due to improved infrastructure near schools), we expect these groups to be more agnostic about the rules governing school choice than parents with young children.¹⁴ In addition, family formation is associated with the decision to become a homeowner, as discussed in Section 3 (see also Figure B1). Therefore, we expect to find larger reform effects in neighborhoods where families with children are more likely to live, as households tend to buy homes around the arrival of children, when school amenities are salient, and households with young children value school amenities more than non-parents.

To test whether improved school choice is indeed more highly valued in neighborhoods with a higher proportion of households with children, we run regressions on two different subsamples. We split the housing data at the median of the local share of families with children into a high and a low family share neighborhood. We use the proportion of families with children living in $1km \times 1km$ grid cells, our definition of neighborhood, which we observe in 2005 (before the reform).

Panel A of Table A5 in the Appendix shows the estimates of the reform effect for houses in neighborhoods with high and low shares of households with children in the underlying $1km \times 1km$ grids. The reform effect in neighborhoods with a higher share of families with children is larger than in the full sample (1.9 and 1.5 percent, respectively) and is statistically significant at the 0.1 percent level. Conversely, the effect is smaller in neighborhoods with a lower share of family households (1.1 percent). We do not consider the positive reform effect in neighborhoods with low proportions of households with children to be a cause of concern for our identification, as households with children still live in these neighborhoods. Overall, we find that school choice is more highly valued in neighborhoods where households with children are more likely

¹⁴Though households without children may not value school access, they are affected by the reform through changes in local house prices.

to live than in neighborhoods with fewer families. We take these results as evidence that school choice is indeed more highly valued by the group of households for which the strongest response was expected, namely parents with school-age children.

Testing School Choice Effects in Presence of Existing Denominational Schools

Next, we turn to the presence of denominational schools. Denominational schools are public schools that allow students to apply from outside the catchment area of their designated neighborhood school (for institutional information on denominational schools, see Section 2.2). Even under mandatory catchment areas denominational schools provided parents with a school choice option. If a denominational school was nearby, this choice option existed even before the school choice reform. Therefore, we expect the reform to have a smaller effect in locations where denominational schools were locally available. We test this hypothesis by estimating our empirical model on the subsample of our data where houses are located within 2,000 meters of at least one denominational school.

We present the results in Table A6 in the Appendix. For houses located within 2,000 meters of a denominational school, we find a reform effect of 0.002 (column (4)), which is statistically not different from zero. In contrast, the reform's effect on house prices for the full sample is 0.015 and highly significant. We interpret this as suggestive evidence that the effect of the reform is muted in areas that already had some level of school choice prior to the reform. This further suggests that our estimate based on the overall sample reflects the impact of newly gained school choice on house prices.

Alternative Definition of Catchment Area

We repeat our main analysis for the sample of house listings that lie in the "core" of our approximated catchment areas. Because we assign house locations to their neighborhood school based on the shortest Euclidean distance (see Section 3), houses that are similarly close to two different schools run the risk of being erroneously assigned to the wrong neighborhood school. To guard against this potential (attenuation) bias, we exclude house listings where the second closest school is less than 100 meters further away relative to the listing's closest school. By restricting our sample to houses in core catchment areas, we remove 17 percent of the observations (see Figure A2 for an illustration). If there is an attenuation bias due to measurement error, we expect the

estimates of the reform on house prices to increase for the restricted sample.

In Table A7 in the Appendix, we document comparable results for the same regression models as in our main analysis, when using the core catchment area sample. In both the core and the full sample, the estimate from our preferred specification is 0.015. Any difference in the other specifications is in the third digit after the leading zero and and is not statistically significant, suggesting a negligible influence of the measurement error in the full sample.

6 Conclusion

Houses located in catchment areas of high-quality schools have a price premium (Black, 1999), which under strict catchment area rules may limit access for families with fewer financial resources. To make high-quality education more accessible, policymakers have introduced programs to increase school choice (Abdulkadiroğlu and Andersson, 2023). In principle, school choice programs such as charter schools or open enrollment policies weaken the house price premium of zoned school quality (Zheng, 2022; Brunner et al., 2012). What is less understood is the valuation that improved choice among traditional public schools has. In this paper, we use a rare and large-scale reform that abolished binding catchment areas for primary schools in Germany's largest state, North Rhine-Westphalia (NRW), to estimate the value of gaining access to an attractive alternative school. We construct a measure of accessible and attractive public school choice based on the distance to schools and the school's transition rate to the academic track. We find that access to a primary school within 2,000 meters and with a higher academic track rate (compared to the initial neighborhood school) increases house prices by 1.5 percent.

In our empirical approach, we exploit the spatially granular nature of our data, which allows us to present an additional set of results. We find that more attractive schools increase house prices the closer they are located. Beyond about 2,250 meters, more attractive alternative schools are no longer valued. Consistent with Greaves and Hussain (2024), who argue that any amenity derived from local schools should be valued more highly in neighborhoods where more parents with school-age children live, we find larger house price premiums in neighborhoods with more families. Furthermore, we use a specific institutional feature in the NRW school system, de-

nominational schools, to support that our results are driven by increased choice. Even under mandatory catchment areas, denominational schools provided parents with a school choice option. We show that the reform has no statistically significant impact on house prices if a denominational school is nearby, *i.e.*, school choice already existed before the reform.

Our results contribute to the literature on the valuation of school choice options by providing an estimate of choice between traditional public schools. Previous work has established that access to charter schools is highly valued, with house price effects of around five to seven percent after charter school entry (Andreyeva and Patrick, 2017; Zheng, 2022). We find that gaining access to a more attractive traditional public school increases house prices by up to two percent five years after the removal of catchment areas. This advances our understanding of the value households place on school choice beyond specific school types, such as charter schools in the US. In addition, we find that choice options for traditional public schools are valued up to around 2,250 meters. If parents value attractive public school choice options only up to such a distance threshold, this also provides information about the potential competitive effects that expanded school choice could stimulate (Hoxby, 2003). In areas with low school density, competition between public schools through choice may be limited if parents do not consider more attractive alternative schools that are further away. Our results suggest that the house price premium of school choice is indeed concentrated in locations with nearby alternative schools.

Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work the authors used ChatGPT and DeepL Write in order to improve the language style. After using these tools, the authors reviewed and edited the content as needed and take full responsibility for the content of the publication.

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Appendix A





Notes: The figure shows the total number of students in the fourth and final year in primary schools that are in the bottom and top quartiles of the pre-reform academic track rate distribution. Before the reform, student numbers in both parts of the distribution evolved similarly, while schools in the bottom quartile of the pre-reform academic track rate distribution lost more students than schools in the corresponding top quartile after the school choice reform took effect in the 2008/09 school year.



Figure A2: Real and Constructed SCAs in Mülheim an d. Ruhr (Core Areas)

Notes: The map shows constructed SCA boundaries together with correctly and falsely assigned residential and mixed areas. Areas close to the constructed boundaries are excluded, which improves the intersection of constructed and correct SCAs: around 89.5 percent of the areas are correctly assigned. Source: © OpenStreetMap CARTO. Herausgeber: Stadt Mülheim an der Ruhr Amt für Geodaten, Kataster und Wohnbauförderung.



Figure A3: Spatial Distribution of Listings in Mülheim an der Ruhr

Notes: This map illustrates the number of house listings at the $100m \times 100m$ grid level for the city of Mülheim an der Ruhr. Black dots indicate primary school locations, black and red lines refer to real and constructed school catchment areas. Source: © OpenStreetMap CARTO. Herausgeber: Stadt Mülheim an der Ruhr Amt für Geodaten, Kataster und Wohnbauförderung.



Figure A4: Bite at School Level

Notes: The map shows the approximate SCAs and the bite measure at school level. Red borders indicate SCAs located in early adopter municipalities, which are excluded from the analysis. The regional distribution of the treatment group indicator (bite) differs between urban and rural areas. For illustrative purposes, we show the bite at the SCA level in this Figure, *i.e.* we define the bite analogous as in Section 4 but for schools instead of listings. Note that our analyses are at the listings level, where we use variation in the bite indicator within SCAs. The spatial distribution shows that the bite indicator is more likely to be zero in rural areas. This is an intuitive result because rural areas feature fewer schools and, therefore, less school choice. However, we observe variation in both urban and rural areas that we can exploit. In the paper, we repeat our main analysis separately for urban and rural areas to ensure that the results are not driven by regional differences. In both subsamples, we find qualitatively the same result (see Table A4).



Figure A5: Illustration of Local School Choice Sets

Notes: The two panels at the top show local school choice sets defined as Euclidean distance (in red) for two house locations in the same catchment area (grey line). The two panels at the bottom show local school choice sets based on a five-minute drive time distance. Consider as an example two houses from the same 2007 catchment area, where one house is located North and one house is located South of the (same) neighborhood school. In the past when catchment areas were binding, households may have sorted into this specific catchment area having taken into account the characteristics of the neighborhood school. The two depicted house locations have different Euclidean distances to the surrounding primary schools. We leverage variation in the distances to alternative schools across these two house locations to assign their treatment group status. In the example, the two locations have different local school choice sets as these contain different alternative schools (dashed lines indicate the geographic boundaries of the choice sets). The Northern house in fact has no schools within the Euclidean or drive time distance cutoff and would hence be assigned to the control group. The local choice set of the Southern house contains primary schools and the house's treatment status depends on how these alternative schools compare to the neighborhood school in terms of their academic track rate. We take out variation in house prices across catchment areas through catchment area fixed effects and therefore leverage variation in house prices within catchment area across the treated and control group. The depicted house locations are fictitious.

| | Unique (#) | Mean | SD | Min | Median | Max |
|---|-------------|----------|--------|-------|----------|----------|
| Panel A: Outcome Variables | | | | | | |
| Price EUR/m ² | 212 225 | 1663.7 | 684.7 | 250.0 | 1596.0 | 18750.0 |
| log(Price EUR/m ²) | 212 224 | 7.3 | 0.4 | 5.5 | 7.4 | 9.8 |
| Panel B: Object Characteristics | | | | | | |
| Furnishing: Missing | 2 | 0.7 | 0.4 | 0.0 | 1.0 | 1.0 |
| Furnishing: Normal | 2 | 0.1 | 0.3 | 0.0 | 0.0 | 1.0 |
| Furnishing: Sophisticated | 2 | 0.1 | 0.4 | 0.0 | 0.0 | 1.0 |
| Furnishing: Deluxe | 2 | 0.0 | 0.1 | 0.0 | 0.0 | 1.0 |
| Number rooms | 98 | 5.3 | 1.5 | 0.0 | 5.0 | 9.0 |
| Missing number rooms | 2 | 0.0 | 0.0 | 0.0 | 0.0 | 1.0 |
| Number bathrooms | 5 | 0.9 | 1.0 | 0.0 | 1.0 | 4.0 |
| Missing number bathrooms | 2 | 0.4 | 0.5 | 0.0 | 0.0 | 1.0 |
| Cellar | 2 | 0.3 | 0.4 | 0.0 | 0.0 | 1.0 |
| Missing cellar | 2 | 0.4 | 0.5 | 0.0 | 0.0 | 1.0 |
| Parking lot | 2 | 0.1 | 0.4 | 0.0 | 0.0 | 1.0 |
| Missing parking lot | 2 | 0.8 | 0.4 | 0.0 | 1.0 | 1.0 |
| Built 1940-1960 | 2 | 0.1 | 0.2 | 0.0 | 0.0 | 1.0 |
| Built 1960-1980 | 2 | 0.2 | 0.4 | 0.0 | 0.0 | 1.0 |
| Built 1980-2000 | 2 | 0.2 | 0.4 | 0.0 | 0.0 | 1.0 |
| Built after 2000 | 2 | 0.3 | 0.4 | 0.0 | 0.0 | 1.0 |
| Missing year of constr. | 2 | 0.2 | 0.4 | 0.0 | 0.0 | 1.0 |
| Plot size | 12095 | 601.7 | 583.8 | 0.0 | 500.0 | 5000.0 |
| Missing plot size | 2 | 0.1 | 0.3 | 0.0 | 0.0 | 1.0 |
| Monthly rental income | 6000 | 21.0 | 337.6 | 0.0 | 0.0 | 28 000.0 |
| Missing monthly rental income | 2 | 1.0 | 0.1 | 0.0 | 1.0 | 1.0 |
| Number floors | 37 | 1.0 | 1.2 | 0.0 | 0.0 | 42.0 |
| Missing number floors | 2 | 0.5 | 0.5 | 0.0 | 1.0 | 1.0 |
| Condition: Missing | 2 | 0.4 | 0.5 | 0.0 | 0.0 | 1.0 |
| Condition: Needs renovation; dilapidated | 2 | 0.1 | 0.2 | 0.0 | 0.0 | 1.0 |
| House type: Missing | 2 | 0.1 | 0.3 | 0.0 | 0.0 | 1.0 |
| House type: Single-family house | 2 | 0.5 | 0.5 | 0.0 | 1.0 | 1.0 |
| House type: Terraced; 2-family | 2 | 0.2 | 0.4 | 0.0 | 0.0 | 1.0 |
| House type: Special property | 2 | 0.1 | 0.3 | 0.0 | 0.0 | 1.0 |
| School closure within 2,000m | 2 | 0.0 | 0.2 | 0.0 | 0.0 | 1.0 |
| Panel C: Neighborhood (1km x 1km grid) Char | acteristics | | | | | |
| Population ₂₀₀₅ | 6308 | 1785.9 | 1777.7 | 0.0 | 1239.0 | 25669.0 |
| Unemployment share ₂₀₀₅ | 2505 | 6.7 | 4.5 | 0.0 | 6.0 | 38.0 |
| Kids per household ₂₀₀₅ | 9598 | 0.3 | 0.1 | 0.0 | 0.3 | 3.6 |
| Income pc_{2005} | 126 925 | 19 154.6 | 4955.5 | 576.5 | 18 509.6 | 64316.8 |
| Panel D: Bite Variables | | | | | | |
| Bite (Better within 2,000m) | 2 | 0.5 | 0.5 | 0.0 | 0.0 | 1.0 |
| Bite (Better within 5 min) | 2 | 0.4 | 0.5 | 0.0 | 0.0 | 1.0 |
| N of Observations: 1250.662 | | | | | | |
| IN. OF ODSERVATIONS: 1 339 662 | | | | | | |

Table A1: Summary Statistics of Control Variables

Notes: The unit of observation are house listings. Reference categories are left out. Subscripts for indicate that we do not use time variation of grid characteristics, because this might lead to a bad control problem.

| | Incorrectly assigned (N=311) | | Correctly assigned (N=1578) | | | |
|--|------------------------------|-----------|-----------------------------|-----------|----------------|------------|
| | Mean | Std. Dev. | Mean | Std. Dev. | Diff. in Means | Std. Error |
| Price EUR/m ² | 2079.01 | 729.81 | 2069.35 | 597.90 | -9.66 | 43.71 |
| Bite (Better within 2,000m) | 0.75 | 0.43 | 0.76 | 0.42 | 0.01 | 0.03 |
| Furnishing: Missing | 0.82 | 0.39 | 0.89 | 0.31 | 0.07** | 0.02 |
| Furnishing: Normal | 0.07 | 0.25 | 0.06 | 0.23 | -0.01 | 0.02 |
| Furnishing: Sophisticated | 0.10 | 0.31 | 0.05 | 0.21 | -0.06^{**} | 0.02 |
| Furnishing: Deluxe | 0.01 | 0.08 | 0.00 | 0.05 | 0.00 | 0.00 |
| Number rooms | 5.00 | 1.97 | 5.24 | 1.84 | 0.25* | 0.12 |
| Missing number rooms | 0.00 | 0.06 | 0.00 | 0.06 | 0.00 | 0.00 |
| Number bathrooms | 1.27 | 1.01 | 1.31 | 0.98 | 0.03 | 0.06 |
| Missing number bathrooms | 0.26 | 0.44 | 0.25 | 0.44 | 0.00 | 0.03 |
| Cellar | 0.16 | 0.37 | 0.18 | 0.38 | 0.02 | 0.02 |
| Missing cellar | 0.79 | 0.40 | 0.76 | 0.43 | -0.03 | 0.03 |
| Parking lot | 0.65 | 0.48 | 0.62 | 0.49 | -0.02 | 0.03 |
| Missing parking lot | 0.35 | 0.48 | 0.36 | 0.48 | 0.01 | 0.03 |
| Built 1940-1960 | 0.12 | 0.32 | 0.13 | 0.34 | 0.02 | 0.02 |
| Built 1960-1980 | 0.25 | 0.43 | 0.23 | 0.42 | -0.02 | 0.03 |
| Built 1980-2000 | 0.10 | 0.31 | 0.09 | 0.29 | -0.01 | 0.02 |
| Built 2000-2020 | 0.22 | 0.42 | 0.27 | 0.44 | 0.05+ | 0.03 |
| Missing year of constr. | 0.17 | 0.38 | 0.13 | 0.34 | -0.04+ | 0.02 |
| Plot size | 452.45 | 634.17 | 430.85 | 424.92 | -21.60 | 37.24 |
| Missing plot size | 0.23 | 0.42 | 0.21 | 0.41 | -0.01 | 0.03 |
| Monthly rental income | 83.42 | 488.60 | 49.12 | 396.57 | -34.31 | 29.23 |
| Missing monthly rental income | 0.96 | 0.21 | 0.97 | 0.16 | 0.02 | 0.01 |
| Number floors | 1.38 | 1.15 | 1.43 | 1.14 | 0.05 | 0.07 |
| Missing number floors | 0.38 | 0.49 | 0.35 | 0.48 | -0.03 | 0.03 |
| Condition: Missing | 0.08 | 0.27 | 0.17 | 0.37 | 0.09*** | 0.02 |
| Condition: Needs renovation; dilapidated | 0.06 | 0.24 | 0.08 | 0.27 | 0.02 | 0.01 |
| House type: Missing | 0.12 | 0.33 | 0.11 | 0.31 | -0.02 | 0.02 |
| House type: Single-family house | 0.23 | 0.42 | 0.38 | 0.48 | 0.14*** | 0.03 |
| House type: Terraced; 2-family | 0.53 | 0.50 | 0.43 | 0.50 | -0.09** | 0.03 |
| House type: Special property | 0.09 | 0.28 | 0.06 | 0.23 | -0.03+ | 0.02 |
| School closure within 2000m | 0.31 | 0.46 | 0.29 | 0.46 | -0.02 | 0.03 |

Table A2: Listings Summary Statistics by Incorrect and Correct SCA Assignment

Notes: Balancing test. The table shows summary statistics of characteristics of listings, which are observed before the reform in 2008/09 for the city of Mülheim an der Ruhr. (We consider listings observed before the 2008/09 school year because, after the reform, the location and other characteristics of listings may respond to the reform. For example, objects on the border of former catchment areas (with more choice options after the reform) may be listed more often or be different from objects listed before the reform. Note that our hedonic house price regression controls for potential compositional changes by including a rich set of object characteristics.) Of these listings, around 16.5 percent (N=311) are assigned to a wrong SCA. The mean difference of object characteristics incorrectly and correctly assigned listings is reported in the latter columns. We find that some object characteristics are on average statistically different between the two samples (6 and 9 out of 32 at the 5 percent and 10 percent level, respectively). The most important characteristics (i.e., listing price, treatment group indicator and school closures nearby) are not statistically different at the 10 percent level. Taken together, this balancing test suggests that incorrectly and correctly assigned listings are only moderately different in their observed characteristics. The measurement error arising from the approximation of SCAs therefore is likely to be to a large extent "classical". However, we acknowledge that the balancing test is performed for listings from a single city only. + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001



Figure A6: Effect of Gaining Access to More Attractive School on House Prices by Time Period (using Drive Time)

Notes: The figure reports the estimated effect of having a more attractive school than the neighborhood school within five minutes drive time on the logarithm of the asking price in EUR per square meter for each year relative to the second semester of the 2007/08 school year. Starting with the school year 2008/09 on, the reform abolishing binding catchment areas took effect. All control variables, *i.e.* time and SCA fixed effects, county specific time trends, regional control variables and object characteristics (see the list of characteristics in Table A1) are included, mirroring the fourth specification of Table 3. The unit of observation is the house for sale listing. Standard errors are clustered at the SCA level. Confidence intervals refer to the 95 percent confidence level.



Figure A7: Histogram of Distance Bins to Next More Attractive School *Notes*: Number of listings within distance bins reported in Figure 5 and in the control group.



Figure A8: Effect on House Prices by Academic Track Rate of Most Attractive Alternative School

Notes: The figure reports the interaction terms of the post-reform indicators and indicators for quintiles of the academic track rate of the best alternative school within 2,000 meters. We treat houses with no alternative school within 2,000 meters as having a choice of an alternative school with an academic track rate of zero. All control variables, *i.e.* time and SCA fixed effects, county specific time trends, regional control variables and object characteristics (see the list of characteristics in Table A1) are included, mirroring the fourth specification of Table 3. The unit of observation is the house for sale listing. Standard errors are clustered at the SCA level. Confidence intervals refer to the 95 percent confidence level.

| | (1) | (2) | (3) | (4) |
|--------------------------|-----------|-----------|-----------|-----------|
| Bite | 0.040*** | -0.023*** | -0.022*** | -0.019*** |
| | (0.008) | (0.004) | (0.004) | (0.004) |
| Bite \times After | 0.022*** | 0.019*** | 0.020*** | 0.016*** |
| | (0.005) | (0.003) | (0.003) | (0.003) |
| N. Obs. | 1 367 016 | 1 367 016 | 1 367 016 | 1 367 016 |
| R ² Adj. | 0.245 | 0.551 | 0.553 | 0.557 |
| SCA FE | No | Yes | Yes | Yes |
| Regional Controls | No | No | Yes | Yes |
| County Time Trend | No | No | No | Yes |

Table A3: Effect of Gaining Access to More Attractive School on House Prices (drive time specification)

Notes: Each column shows in the second row the estimate of τ from equation 1, which captures the effect of gaining access to a more attractive alternative school on the logarithm of the asking price in EUR per square meter. Bite is an indicator variable that takes the value one if a house has a more attractive school than its neighborhood school within five minutes drive time, and zero otherwise. After is an indicator variable that takes the value one if a house listing was posted on the platform after the reform abolishing binding catchment areas took effect, and zero otherwise. Control variables are included as reported at the bottom of the table. Object characteristics (see the list of characteristics in Table A1) and year fixed effects are included in all specifications. The unit of observation is the house for sale listing. Standard errors are clustered at the SCA level. + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

| | (1) | (2) | (3) | (4) |
|------------------------|-------------|-----------|-----------|-----------|
| Panel A: Listings in U | Irban Areas | | | |
| Bite | 0.038*** | -0.005 | -0.007 | -0.006 |
| | (0.010) | (0.006) | (0.006) | (0.006) |
| Bite \times After | 0.014** | 0.013*** | 0.015*** | 0.014*** |
| | (0.005) | (0.004) | (0.004) | (0.004) |
| N. Obs. | 1 040 189 | 1 040 189 | 1 040 189 | 1 040 189 |
| R2 Adj. | 0.254 | 0.522 | 0.525 | 0.529 |
| Panel B: Listings in R | ural Areas | | | |
| Bite | 0.057*** | 0.005 | 0.002 | 0.003 |
| | (0.013) | (0.011) | (0.011) | (0.011) |
| Bite \times After | 0.031*** | 0.019* | 0.018* | 0.016* |
| | (0.009) | (0.007) | (0.008) | (0.008) |
| N. Obs. | 319 473 | 319 473 | 319 473 | 319 473 |
| R ² Adj. | 0.317 | 0.478 | 0.480 | 0.481 |
| SCA FE | No | Yes | Yes | Yes |
| Regional Controls | No | No | Yes | Yes |
| County Time Trend | No | No | No | Yes |
| | | | | |

Table A4: Effect of Gaining Access to More Attractive School on House Prices by Urbanity

Notes: Each column in the second row of the respective panel reports the estimate of τ from equation 1, which captures the effect of gaining access to a more attractive alternative school on the logarithm of the asking price in EUR per square meter, for the urban and rural subsamples, respectively, according to the RegioStar2 definition. Bite is an indicator variable that takes the value one if a house has a more attractive school than its neighborhood school within 2,000 meters, and zero otherwise. After is an indicator variable that takes the value one if a house listing was posted on the platform after the reform abolishing binding catchment areas took effect, and zero otherwise. Control variables are included as reported at the bottom of the table. Object characteristics (see the list of characteristics in Table A1) and year fixed effects are included in all specifications. The unit of observation is the house for sale listing. Standard errors are clustered at the SCA level. + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

| | (1) | (2) | (3) | (4) | | |
|--|--------------|-------------|-------------|----------|--|--|
| Panel A: Listings in C | Grids with a | ibove Medii | an Share of | Families | | |
| Bite | 0.060*** | -0.018** | -0.021** | -0.017* | | |
| | (0.010) | (0.007) | (0.007) | (0.007) | | |
| Bite \times After | 0.038*** | 0.024*** | 0.025*** | 0.019*** | | |
| | (0.007) | (0.005) | (0.005) | (0.004) | | |
| N. Obs. | 680 481 | 680 481 | 680 481 | 680 481 | | |
| R2 Adj. | 0.307 | 0.550 | 0.551 | 0.554 | | |
| Panel B: Listings in Grids with below Median Share of Families | | | | | | |
| Bite | 0.041*** | 0.000 | 0.003 | 0.004 | | |
| | (0.012) | (0.008) | (0.007) | (0.007) | | |
| Bite \times After | 0.018** | 0.014** | 0.013** | 0.011* | | |
| | (0.007) | (0.005) | (0.005) | (0.004) | | |
| N. Obs. | 677 733 | 677 733 | 677 733 | 677 733 | | |
| R ² Adj. | 0.235 | 0.543 | 0.546 | 0.550 | | |
| SCA FE | No | Yes | Yes | Yes | | |
| Regional Controls | No | No | Yes | Yes | | |
| County Time Trend | No | No | No | Yes | | |

Table A5: Effect of Gaining Access to More Attractive School on House Prices by Share of Families in Neighborhood

Notes: Each column in the second row of each panel reports the estimate of τ from equation 1, which captures the effect of gaining access to a more attractive alternative school on the logarithm of the asking price in EUR per square meter, separately for listings in neighborhoods with an above and below median share of families in panels A and B, respectively. Bite is an indicator variable that takes the value one if a house has a more attractive school than its neighborhood school within 2,000 meters, and zero otherwise. After is an indicator variable that takes the value one if a house listing was posted on the platform after the reform abolishing binding catchment areas took effect, and zero otherwise. Control variables are included as reported at the bottom of the table. Object characteristics (see the list of characteristics in Table A1) and year fixed effects are included in all specifications. The unit of observation is the house for sale listing. Standard errors are clustered at the SCA level. + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

| | (1) | (2) | (3) | (4) |
|--------------------------|---------|---------|---------|---------|
| Bite | 0.007 | 0.004 | 0.002 | 0.002 |
| | (0.016) | (0.010) | (0.009) | (0.009) |
| Bite \times After | -0.001 | 0.000 | 0.002 | 0.002 |
| | (0.009) | (0.006) | (0.006) | (0.005) |
| N. Obs. | 515751 | 515751 | 515751 | 515 751 |
| R ² Adj. | 0.257 | 0.553 | 0.555 | 0.560 |
| SCA FE | No | Yes | Yes | Yes |
| Regional Controls | No | No | Yes | Yes |
| County Time Trend | No | No | No | Yes |

Table A6: Effect of Gaining Access to More Attractive School on House Prices in the Presence of Pre-Existing Choice via Denominational Schools

Notes: Each column shows in the second row the estimate of τ from equation 1, which captures the effect of gaining access to a more attractive alternative school on the logarithm of the asking price in EUR per square meter. The sample is limited to houses that have a denominational school within 2,000 meters, so parents had some school choice even before the reform that abolished SCAs. Bite is an indicator variable that takes the value one if a house has a more attractive school than its neighborhood school within 2,000 meters, and zero otherwise. After is an indicator variable that takes the value one if a house listing was posted on the platform after the reform abolishing binding catchment areas took effect, and zero otherwise. Control variables are included as reported at the bottom of the table. Object characteristics (see the list of characteristics in Table A1) and year fixed effects are included in all specifications. The unit of observation is the house for sale listing. Standard errors are clustered at the SCA level. + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

| | (1) | (2) | (3) | (4) |
|---------------------|-----------|-----------|-----------|----------|
| Bite | 0.079*** | -0.011* | -0.011* | -0.006 |
| | (0.009) | (0.005) | (0.005) | (0.005) |
| Bite \times After | 0.030*** | 0.023*** | 0.021*** | 0.015*** |
| | (0.005) | (0.004) | (0.004) | (0.003) |
| N. Obs. | 1 127 044 | 1 127 044 | 1 127 044 | 1127044 |
| R ² Adj. | 0.262 | 0.555 | 0.557 | 0.560 |
| SCA FE | No | Yes | Yes | Yes |
| Regional Controls | No | No | Yes | Yes |
| County Time Trend | No | No | No | Yes |

Table A7: Effect of Gaining Access to More Attractive School on House Prices with Core SCA

Notes: Each column shows in the second row the estimate of τ from equation 1, which captures the effect of gaining access to a more attractive alternative school on the logarithm of the asking price in EUR per square meter. In this sample, we exclude house listings where the second closest school is less than 100 meters further away relative to the listing's closest school.Bite is an indicator variable that takes the value one if a house has a more attractive school than its neighborhood school within 2,000 meters, and zero otherwise. After is an indicator variable that takes the value one if a house listing was posted on the platform after the reform abolishing binding catchment areas took effect, and zero otherwise. Control variables are included as reported at the bottom of the table. Object characteristics (see the list of characteristics in Table A1) and year fixed effects are included in all specifications. The unit of observation is the house for sale listing. Standard errors are clustered at the SCA level. + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Appendix B





Notes: The left graph shows the homeownership rate of households who eventually have children relative to the time of arrival of children in the household (*i.e.*, the event). The right graph shows homeownership rate of households who never (light blue) and households who eventually have children (dark blue) in their observed panel lifetime. The figure shows descriptively that households that eventually have children become home owners specifically after the arrival of children. The homeownership rate roughly doubles from around 26 percent one year before the arrival of a child to 51 percent six years after (when the first child reaches the school entry age). Looking at the homeownership rate by the age of the household head, we further document that households that never have children have a much more linearly increasing homeownership rate profile compared to households that have children at some point (whose homeownership profile is much steeper between the ages of 20 and 45). The underlying household samples are taken from the SOEP-Core v36 (EU edition) (Goebel et al., 2019). The sample consists of observations from 1984 to 2019 and is restricted to households with non-missing information in their homeownership and household type information. Households who ever have children are couples with children of any age co-residing and single parents. Households without children are 1-person-households and couples without children. We exclude multi-generation and 'other' households. Underlying the left graph, we observe 4,102 unique households amounting to 51,252 observations. For the right graph, we have a total of 47,764 unique households with 325,932 observations. We use household survey weights.