Access to Local Amenity and Housing Prices*

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Abstract

This paper assesses the effects of relaxing restricted access to local schools on housing prices by comparing prices near school district boundaries before and after a school choice reform in Korea. Prior to the reform, students were randomly assigned to high schools within their school districts, where one standard deviation difference in the average college entrance exam score was associated with a 7 percent housing premium at the district borders. In 2010, the school choice reform weakened the role of school district boundaries by opening up to 60 percent of seats within a district for outside district competition through application-based lotteries. Using geo-coded data on apartment transactions and a difference-in-discontinuity design, I find that the pre-reform housing price gap decreased by 3.4 percentage points within four years after the reform, while the test score gap remained similar after the reform. Overall, the findings suggest that alleviating restrictions on access to local schools significantly decreases the housing premium, and the effects are consistent with the channel of altering parents' perception of expected school quality within their districts.

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1 Introduction

A central question in local public finance is the degree to which access to local amenities and public goods is capitalized into housing prices. Geographical boundaries that restrict the access to local services can be a source of inequality, especially when a lack of these services, such as good school quality or safe environments, leads to a greater disparity between the rich and the poor. A natural remedy to reduce this source of inequality may be weakening the role of these boundaries - for example, by decoupling residential location from school assignment. However, the existing residents of attractive areas may pose political barriers to this approach because dismantling these boundaries may decrease the housing premium attached to local amenities.

This paper assesses the effects of relaxing restricted access to local schools on housing prices by comparing prices near school district boundaries before and after a school choice reform in Korea. Prior to the reform, students were randomly assigned to high schools within their school districts. In 2010, the Ministry of Education implemented a school choice reform, allowing students to apply to any high schools outside their school districts, but within their cities. The reform was implemented in four major cities at the same time. Using geo-coded data on apartment transactions and a difference-in-discontinuity design, I identify and quantify the reform effects on housing prices and average test scores.

Despite its importance, there is limited empirical evidence on the extent to which dismantling school district boundaries would affect housing prices. Existing studies show the degree to which access to better schools is capitalized into housing prices.[1] The key common assumption behind these studies is that unobserved amenities of a neighbourhood are not correlated with the observed school quality. An influential study by Black (1999) addresses this potential endogeneity by controlling for boundary fixed effects, assuming that within a given boundary, houses differ only by the school that the child attends. Black (1999) finds that a 5 percent increase in test scores is capitalized into a 2.5 percent increase in housing prices on average.

One potential concern with the boundary discontinuity design is that other neighbourhood characteristics may change discontinuously at school district boundaries. A follow-up study by Bayer, Ferreria and McMillan (2007) uses a similar empirical design, but adds demographics of residents as control variables using restricted census block-level data. Their argument is that much of the variation in housing prices can be already explained by the demographic composition of people living there. Once they control for detailed demographics of residents, they found that households are willing to pay less than 1 percent more in housing prices for a 5 percent increase in test scores.

[1] A large body of literature measures the effects of school quality on home values using various methodologies, finding a wide range of results (Bogart and Cromwell 2000; Dee 2000 Bradbury, Mayer and Case 2001; Downes and Zabel 2002 Rosenthal 2003;Kane et al. 2003; Cellini, Ferreira and Rothstein 2010; Machin and Salvanes 2015).

A potential improvement over the existing method is to control for unobserved neighbourhood characteristics that may change discontinuously at district boundaries. Several studies use a policy change for identification, arguing that using time-series variation may eliminate this source of bias if the unobservable characteristics around the district borders are fixed over time. For example, Ries and Somerville (2010) use a redrawing of school catchment zones in Vancouver as a natural experiment that induced changes in school quality. The authors find significant effects of school quality on housing values only for the most expensive quartile of residences. Similarly, Kane et al. (2003) find systematic differences in home values along school boundaries and that prices seem to react to changes in school assignments in North Carolina. Furthermore, Machin and Salvanes (2015) use a change in admissions policy reform in Norway that created a switch from zone-based to open-enrolment to estimate the housing premium of living in a better school zone before and after the reform, and find that the premium decreased at least by 50 percent after the reform.

Since these studies rely on re-zoning or complete de-zoning of school boundaries as a natural experiment, one important caveat is that it may be difficult to find a comparison group unaffected by the policy change and to control for other nation-wide shocks that may coincide with the reform. Therefore, the central contribution of this paper is that I identify the effects of the school choice reform that partially dismantled school boundaries on housing prices by additionally exploiting control areas unaffected by the reform. I quantify the magnitude of the effects of relaxing restricted access to local schools on housing prices - a contentious and recurring topic in policy debates on the school reforms and local public finance.

I find that the school choice reform significantly reduced the housing price gap around district borders with different average test scores. I estimate that a difference in one standard deviation in the average college entrance exam score was associated with a 7 percent housing premium around the district borders prior to the reform. To assess the reform effects on the housing price gap, I estimate the change in the housing premium around the district borders before and after the policy change, and find that the reform reduced the housing price gap by 3.4 percentage points - roughly 50 percent of the pre-reform difference. Even though the school choice reform substantially closed the housing price gap, I find that the test score gap remained similar after the reform.

The reduced-form estimates imply that parents were willing to pay extra 1,5000 dollars for an apartment to send their children to schools with one standard deviation higher average test scores prior to the reform, holding everything else equal. Given that an annual income for the average household in Korea is roughly 33,000 dollars, the results suggest that the high school assignment based on district boundaries imposed a substantial financial burden on the average household desiring to send their child to a better school. After the school choice policy, which opened up to 60 percent of seats for outside district competition through application-based lotteries,

this housing premium drops by 7,300 dollars on average, which is about a fifth of the average household income. My results imply that the district-based school assignment had a substantial effect on inequality before the policy change, and the school choice reform greatly reduced that source of inequality.

A key assumption behind my empirical design is that there were no other time-varying shocks correlated with the reform that might have impacted the housing prices. For example, this assumption is violated if local governments increased property taxes on apartment values above a certain threshold around the time of the reform, potentially biasing my estimates to be upward-biased. To address this potential concern, I estimate the same empirical model on a set of placebo areas, where the average test score gap was small. I find that the housing price gap between adjacent districts with similar average test scores was statistically indistinguishable from zero before and after the reform.

The findings are consistent with a potential mechanism that parents' perception of expected school quality within their own school districts changed after the reform, increasing the demand for apartments around district borders with lower average test scores. However, the reduced-form effects may capture a change in sorting by households with higher socioeconomic backgrounds after the reform. In other words, if parents with college degrees were more likely to live in the side of the districts with higher average test scores (Bayer, Ferreria and McMillan 2007) and these parents were more likely to move to the other side after the reform, then the reduced form effects may capture both changes in parents' perception on expected school quality and sorting. Bringing detailed micro data on demographics and mobility of residents living close to the borders may help uncover the potential mechanisms behind the reduced-form findings and will be an interesting extension to this paper.

The remainder of the paper is organized as follows. Section 2 describes the institutional background for the school choice reform in Korea. I describe empirical strategy and data in Section 3. In Section 4, I present my results and their interpretation. Section 5 concludes.

2 Institutional Background

This section describes the institutional setting on the high school assignment and the school choice reform in Korea relevant for my empirical strategy.

2.1 High School Assignment

Prior to 2010, middle school students were randomly assigned to high schools within their own school districts. For example, in Seoul, each school zone contains about 8 high schools on average, and students were randomly selected into any one of high schools within their districts (see Figure 1).[2] Even though the exact school assignment mechanism was slightly different across different cities, district-based assignment was common in four major cities where the school choice reform was implemented. In 2010, the school choice policy became effective and weakened the role of the school district boundaries, allowing students to apply to any high school within their own city.

2.2 School Choice Reform

The school choice reform in Korea was introduced to allow students to apply to specific schools both within and outside their school districts. The reform was announced in the first quarter of 2009 and became effective starting in 2010. Although the Ministry of Education hinted the possibility of implementing the reform in 2008, they did not give any further details, such as the probability of admission, until 2009, so this policy change came as a shock for affected families. Reforms similar to the one in Seoul were adopted in three other major cities: Pusan, Daegu, and Daejun. In Seoul, students can have up to a 40 percent chance of getting into a school in different districts, while the probability is different in other cities.[3] In Pusan, the probability of getting into schools outside districts is 40 percent, while the probability is 60 percent in Daejun and 10 percent in Daegu. I include in Appendix A more details on the school choice reform in 2009. After the reform, the majority of students apply to high schools within their own or adjacent districts, primarily because of the commuting costs and of the fact that the admission probability still partially depends on a student's residential location.[4]

[2]Students can apply to specialized (language, arts, and science) or "elite" high schools, which require entrance exams and are not part of the centralized school system.

[3]The school assignment after the reform is based on a modified version of the Boston Mechanism, in which students are matched to schools based on their ranked preferences, and lotteries are used to break ties. The matching can take multiple stages and other factors, such as students' residential location and religion, can be used to assign unmatched students to remaining schools. More details on this mechanism can be found in Appendix A and in Hwang (2017).

[4]Although accounting for the school choice mechanism and how students ranked their school preferences in the empirical analysis would be important for mapping parents' expected school quality into housing prices, it requires additional data on students' applications and their outcomes. Due to the data availability, this paper captures the reduced-form effects of the reform on housing prices, as explained in Section 3.2. Uncovering potential channels behind these effects through incorporating parents' beliefs on their expected school quality would be an interesting extension to this paper.

3 Empirical Strategy

This section explains my empirical model and data used to estimate the effects of the school choice reform on housing prices. Identifying the effect of relaxing restricted access to local schools on housing prices is challenging in part because the quality of local schools may be correlated with other amenities or neighborhood characteristics, making it difficult to isolate the main effect from other confounders (Black 1999; Bayer, Ferreria and McMillan 2007). Furthermore, other shocks that can affect housing prices may coincide with the reform, making it difficult to distinguish the reform effects from contemporaneous changes in property values. I overcome the first challenge by comparing housing prices within and close to school district boundaries before and after the policy change. To address the second challenge, I apply the same difference-in-discontinuity method with boundary fixed effects to a set of apartment transactions near boundaries relatively unaffected by the reform because the school quality gap prior to the reform was very small.

3.1 Estimating the Effect of Access to Local Schools

The estimation of the hedonic price function suffers the identification problem that unobserved neighborhood amenities, valued by consumers but unobserved by econometricians, are likely to be correlated with the observed school quality. If we assume that such unobserved amenities are fixed at the boundary level, then we can eliminate this bias by controlling for boundary fixed effects (Black 1999). The intuition behind this strategy is that houses near the school district boundary share similar amenities and neighborhoods, so controlling for boundary fixed effects and zooming in on areas near the borders are sufficient to eliminate omitted variables correlated with school quality that are fixed at the boundary-level.

One can estimate the following regression discontinuity model to estimate the effects of being located in a better school district on housing prices:

$$Y_{idb} = f(r_{idb}) + \theta D_{idb} + X_{idb}\beta + \Phi_b + \epsilon_{idb} \tag{1}$$

where Y_{idb} is an outcome of interest for an apartment transaction i at a boundary b of a district d. r_{idb} is an apartment's distance (in miles) relative to a boundary b of a district d, and $f()$ is some smooth function. $D_{idb} = 1[r_{idb} > 0]$ is a dummy equal to one if i is located at the side of the boundary b, where the average test score of district d is higher than the average test score of district d' on the opposite side of the boundary. X_{idb} is a vector of apartment $i's$ characteristics at boundary b of district d. Φ_b is a vector of boundary fixed effects and ϵ_{ibd} is an unobserved error term. The

estimate of θ captures the effect of the difference in the mean school quality on the difference in the mean housing price on opposite sides of school district boundaries, controlling for observable housing characteristics.

3.2 Estimating the Effect of the School Choice Reform

The key question is how much the school choice reform that weakened the role of school district boundaries affected the housing price gap between two school districts with different average test scores. I estimate the reform effects on the main outcomes using the following difference-in-discontinuity model:

$$Y_{idbt} = f(r_{idb}) + f(r_{idb}) \times Post_t + \theta_1 D_{idb} + \theta_2 D_{idb} \times Post_t + X_{idbt}\beta + \Phi_b + \alpha_t + \epsilon_{idbt} \qquad (2)$$

where $Post_t$ is equal to 1 if it is after the reform in 2009, α_t are year fixed effects, and the other variables are as defined in equation 1. The estimate of θ_1 captures the housing premium associated with higher school quality before the reform, and the estimate of θ_2 captures the difference in the housing premium before and after the reform. When estimating this model, I impose the following restrictions. First, I limit the distance from either side of the border to be less than or equal to 0.3 miles and do a robustness check using 0.1 miles (Black 1999; Bayer, Ferreria and McMillan 2007).[5] Second, I cut my sample to have a minimum average test score difference of a half standard deviations between two adjacent districts. The intuition is that the school choice reform that weakened the role of boundaries would have an effect on housing prices if there was a noticeable school quality difference prior to the reform. This second restriction leads to a sample of apartment transactions around district borders, where the difference in average test scores between two adjacent districts is about one and a half standard deviations on average prior to the reform. I do a robustness test where I impose a different level of restrictions on the difference in average test scores and find qualitatively similar results (see Appendix B).

The main identifying assumption, in addition to what I discussed in Section 3.1, is that other time-varying shocks that can affect housing prices are not correlated with the reform. For example, this assumption is violated if the local governments imposed higher taxes on property values above a certain threshold around the time of the reform, thereby leading the estimate of θ_2 to be potentially upward-biased. To address this concern, I estimate the same model in equation 2 for

[5] Restricting the distance from either side of the border is equivalent to choosing an optimal bandwidth in a regression discontinuity design, where the potential trade-off is accuracy and precision. I can increase the distance on both sides to include more observations, which may increase efficiency but may introduce a potential bias from unobservables correlated with school quality that are not fixed at the boundary-level.

a subset of the sample where I impose the opposite of the second restriction: I cut my sample to have a maximum average test score difference of a quarter standard deviations between two adjacent districts. The intuition is that a set of apartment transactions around district borders where the difference in average test scores was small serve as a control group, where I expect to find both θ_1 and θ_2 to be close to zero. On the other hand, if there were other shocks that coincided the reform, then the estimate of θ_2 would not be economically and statistically different from zero. Therefore, I can directly test my main assumption by leveraging on this control group.

I hold the definition of D_{idb} fixed throughout the sample period, 2007 to 2013. Average test scores can change over time, leading to changes in the test score gap between two adjacent districts. However, the difference in average test scores never changed to the point where the higher school quality side became the lower school quality side during the sample period. Furthermore, the average test score gap between two adjacent districts changed marginally after the reform. Therefore, holding the definition of D_{idb} fixed over time does not affect my overall results.

3.3 Data

I combine two data sets for empirical analysis. The first data set consists of all apartment sale transactions in South Korea from 2007 to 2013. This data set includes residential addresses, transaction dates and prices, and other characteristics of the properties, such as the size, the age of the building, and the floor level. I obtained this data set from the Real Estate Bureau, Department of Land and Transportation. I geo-coded every apartment transaction based on its physical address using the Google API service. Each apartment was uniquely matched to the closest boundary and I calculated its distance from the boundary by using its geocode and spatial software programs.[6]

The second data set consists of school-level information on college entrance exam test scores from 2009 to 2013.[7] The school-level data set contains information on other school characteristics, such as the college entrance rate, class size, and school amenities (i.e., number of libraries). This data set is owned by the Ministry of Education and the Korean Institute of Curriculum Evaluation (KICE), and I obtained it through the Education Data Service System (EDSS).

I merge the apartment transaction data with the school-level test score data by averaging school-level test scores at the district-level and matching two data sets at the district-level. The final data set contains information on apartment transactions and their associated district-level school quality.

[6] I omitted boundaries in my sample if the two school districts were divided in any other ways, such as by large rivers and parks (Black 1999).

[7] Due to the data availability, I do not observe the school quality information for years prior to 2009. Since I hold the definition of D_{idb} in equation 2 fixed throughout the sample period, this data constraint does not affect my overall results. Furthermore, the level of the average test score is stable across years, 2009 to 2013.

3.4 Variable Definitions

The main dependent variable is the apartment sales price. When estimating the effects of school quality on home values, the implied assumption is that the average sales (transacted) price reflects the prevailing home value in that area, controlling for observable apartment characteristics. There are several potential concerns when using transaction prices as a measure of home value. If the number of transactions is low on the side of districts with lower average test scores, then the estimate on the effects of school quality and the reform would be imprecise. As shown in Table 1, the number of transactions on both sides is similar. Another issue could be that families may want to rent an apartment rather than purchasing it.[8] However, if sales prices are highly positively correlated with rental prices, then this is less of a concern because a sales price is a good proxy for a rental price.

The main independent variable is the school quality, measured as the average college entrance test score at the district-level. One can think of school quality essentially in two dimensions: (1) the overall performance level of students measured by the average standardized test score at the school and (2) the additional value that the school contributes. If parents are actually paying for the value-added of a school, then we should measure quality as the change in student performance from the change in time-varying school inputs. However, in my setting, parents can access information only on the average test score of a school. Therefore, I use the average test score as a perceived measure of school quality.[9]

Other relevant variables related to apartment characteristics are the number of beds, the age of the building, and the floor level. Since the apartment transaction data do not have information on the number of beds, I impute the variable based on the size of the unit and the guideline commonly used by real estate agents in Korea.[10] The age of the building is defined as the difference in the year of the observation and the year it was built.

[8] Since data on apartment rents is available from 2010, I cannot use this data set to analyze the effects of the school choice reform on rental prices.

[9] Many empirical findings suggest parents consider average test scores as a signal for quality. Hayes and Taylor (1996) use data from the Dallas school district and find that parents do not value changes in school expenditures, but value changes in school test scores. Using data from Chicago, Downes and Zabel (2002) find that parents value average test scores and not measures of value added. Furthermore, Brasington and Haurin (2006) compare 37 different measures of school quality and conclude that test scores have a significant effect on housing prices. Thus, existing empirical evidence supports that average test score is a good proxy for school quality.

[10] My main results are quantitatively similar if I use the size of the unit instead of the imputed number of beds as a control variable. The number of beds is a useful variable to include, as it is commonly used as a control variable in other studies (Black 1999; Bayer, Ferreria and McMillan 2007)

3.5 Descriptive Statistics

I summarized the main variables, such as the apartment transaction price, age, floor, number of beds, and test scores in Table 1. Note that the sample is restricted to areas within 0.3 miles of the district borders and the sample period is from 2007 to 2013. As shown in Columns (3) and (4), transaction prices are higher on the side of the boundaries with higher average test scores among the treated areas, where the difference in test scores is about one and a half standard deviations on average. However, other apartment characteristics, such as the number of beds, age of the building, and floor level are similar on either side of the boundaries. Columns (5) and (6) show that the housing price gap is small on average in the control areas. Other housing characteristics are similar on both sides.

4 Results

This section shows the results from the estimation of the difference-in-discontinuity model in Section 3, and presents additional tests supporting the interpretations of the results.

4.1 Reform Effects on Test Score Gap

Panel A of Figure 3 plots the residualized test scores within 0.3 miles of the school district boundaries prior to the reform in 2009, controlling for boundary fixed effects.[11] Test scores are school-level average college entrance exam scores, standardized in each year and averaged at the district-level. Each dot indicates a residualized test score averaged over each bin, where the bin size is 0.06 miles (0.1 kilometers). The negative distances indicate the lower test score side of the boundary. Note that the test scores are normalized to be zero at the -0.06 mile bin. The difference in the test score gap is about one and a half standard deviations. Panel B of Figure 3 plots the residualized test scores within 0.3 miles of the district boundaries after the reform in 2009. The test score gap decreased by a little, and the change is economically insignificant.

Table 2 presents the difference-in-discontinuity estimation results on the test scores within 0.3 and 0.1 miles of the boundary. I winsorize (bottom- and top-code) the main outcome at the fifth and ninety-fifth percentiles. Column (1) shows that the regression discontinuity coefficient is 1.51, implying that the pre-reform test score gap between two adjacent districts was about one and a half standard deviations on average. The difference-in-continuity coefficient is -0.127, implying

[11] Since the average test score is at the district-level, whether or not controlling for boundary fixed effects does not quantitatively affect the estimates of the residuals.

that the reform decreased the gap by 0.13 standard deviation point, which is less than 10 percent of the pre-reform gap between two adjacent districts. Column (3) shows results from a similar estimation, within 0.1 miles of the boundary. The coefficient estimates are similar to the ones in Column (1). Even-numbered columns replace a dummy $Post_t$ with year fixed effects. Overall, the results suggest that the reform had economically small effects on the test score gap between two adjacent districts whose pre-reform test score gap was about 1.5 standard deviations on average.

4.2 Reform Effects on Housing Price Gap

Panel A of Figure 4 plots the residualized log(housing prices) within 0.3 miles of the school district boundaries prior to the reform in 2009, controlling for observable apartment characteristics and boundary fixed effects. Housing price is the transaction price for a given apartment unit. Each dot indicates a residualized price averaged over each bin, where the bin size is 0.06 miles (0.1 kilometers). The negative distances indicate the lower test score side of the boundary. Note that the housing prices are normalized to be zero at the -0.06 mile bin. The housing premium associated with one and a half standard deviations higher average test scores was about 10 percent prior to the reform. Panel B of this figure plots the residualized log(housing prices) within 0.3 miles of the school district boundaries after the reform in 2009. The housing premium decreased significantly after the reform, as visually shown in the graph.

Table 3 presents the difference-in-discontinuity estimation results on housing prices within 0.3 and 0.1 miles of the boundary. I winsorize (bottom- and top-code) the main outcome at the fifth and ninety-fifth percentiles. Column (1) shows that the regression discontinuity coefficient is 0.099, implying that the pre-reform housing price gap between two adjacent districts affected by the reform was about 10 percent on average. The difference-in-continuity coefficient is -0.049, implying that the reform closed the gap by 5 percentage points - roughly 50 percent of the pre-reform gap between two adjacent districts. Column (3) shows results from a similar estimation, within 0.1 miles of the boundary. The coefficient estimates are similar to the ones in Column (1). Even-numbered columns replace a dummy $Post_t$ with year fixed effects. Assuming that the effects scale down linearly, this result implies that one standard deviation increase in the average test score is associated with roughly 7 percent housing premium prior to the reform, and the reform reduced the premium by roughly 3.4 percentage points.

Overall, the results suggest that the school choice reform that weakened the role of school district boundaries significantly reduced the housing price gap between two adjacent districts with different average test scores, while the test score gap did not change much. Furthermore, these findings are consistent with the channel that parents' perception of their expected school quality

within their districts changed after the reform, increasing the demand for apartments located at the side of adjacent boundaries with lower average test scores. However, other subsequent changes in those areas, such as sorting by parents with higher socioeconomic backgrounds or improvements in various amenities, could have contributed to the decrease in the housing price gap after the reform. Therefore, the reduced-form effects based on the difference-in-discontinuity estimates quantify the total impacts of the reform on housing prices. Uncovering potential mechanisms requires further analyses in the affected areas using additional micro-level data on detailed demographics and mobility of residents living close to the boundaries.

4.3 Placebo Test Using Control Areas

One potential concern with my analysis is that there might have been other time-varying shocks that coincided with the school choice reform. For example, local governments might have imposed higher taxes on property values above a certain threshold around the time of the reform, which could lead my estimates on the change in the housing price gap to be upward-biased. To address this concern, I estimate the same model in equation 2 for a subset of the sample with a maximum average test score difference of a quarter standard deviations between two adjacent districts. The intuition is that a set of apartment transactions around district borders where the difference in average test scores was small serve as a control group, where I expect to find the housing price gap to be zero both before and after the reform.

Panel A of Figure 5 plots the residualized test scores within 0.3 miles of the school district boundaries prior to the reform in 2009, controlling for boundary fixed effects. The difference in the test score gap was about a quarter standard deviations on average prior to the reform. Panel B of Figure 3 plots the residualized test scores within 0.3 miles of the district boundaries after the reform in 2009. The test score gap increased by a little, and the change is economically insignificant.

Table 4 presents the difference-in-discontinuity estimation results on the test scores within 0.3 and 0.1 miles of the boundary. I winsorize (bottom- and top-code) the main outcome at the fifth and ninety-fifth percentiles. Column (1) shows that the regression discontinuity coefficient is 0.23, implying that the pre-reform test score gap between two adjacent districts affected by the reform was less than a quarter standard deviations on average. The difference-in-continuity coefficient is 0.093, implying that the reform increased the gap by 0.09 standard deviation point, which is economically insignificant. Column (3) shows results from a similar estimation, within 0.1 miles of the boundary. The coefficient estimates are similar to the ones in Column (1).

Panel A of Figure 6 plots the residualized log(housing prices) within 0.3 miles of the school district boundaries prior to the reform in 2009, controlling for observable apartment characteristics

and boundary fixed effects. Panel B of this figure plots the residualized log(housing prices) within 0.3 miles of the school district boundaries after the reform in 2009. Both graphs show that the housing gap was small between these adjacent districts with the small test score gap, and the reform did not seem to affect the gap after the reform.

Table 5 presents the difference-in-discontinuity estimation results on housing prices within 0.3 and 0.1 miles of the boundary. I winsorize (bottom- and top-code) the main outcome at the fifth and ninety-fifth percentiles. Column (1) shows that the regression discontinuity coefficient is 0.035, implying that the pre-reform housing price gap between two adjacent districts affected by the reform was about 3.5 percent on average, although this estimate is not statistically different from zero even at the 10 percent significance level. The difference-in-continuity coefficient is -0.017, implying that the reform closed the gap by 1.7 percentage points, which is not statistically different from zero. Column (3) shows results from a similar estimation, within 0.1 miles of the boundary. The coefficient estimates are similar to the ones in Column (1). Therefore, the reform seemed to have no effect on the housing price gap between two adjacent districts with similar average test scores, and these results support the assumption that it was unlikely that there were other shocks that coincided with the reform that might have affected the housing price gap.

4.4 Robustness Checks and Internal Validity

To strengthen the internal validity of my results, I run several robustness checks. First, I use a different level of winsorizing the main outcomes and find quantitatively similar results. Second, I re-run all the analyses with a different definition of the treated areas, and find qualitatively similar results. The results from these robustness tests are included in Appendix B.

4.5 Economic Interpretation

The results imply that parents were willing to pay extra 1,5000 dollars for an apartment to send their children to schools with one standard deviation higher average test scores prior to the reform, everything else equal. Given that an annual income for the average household in Korea is roughly 33,000 dollars in 2010, these estimates suggest that the high school assignment based on district boundaries imposed a huge financial burden on the average household desiring to send their child to a better school.[12] After the school choice policy, which opened up to 60 percent of seats for

[12] According to the OECD estimates, an annual average income in South Korea was about 32,500 dollars in 2010. More details on the historical average annual incomes in South Korea can be found at https://data.oecd.org/earnwage/average-wages.htm.

outside district competition, this housing premium drops by 7,300 dollars on average, which is about a fifth of the average household income.

The findings are consistent with a potential mechanism that parents' perception of expected school quality within their own school districts changed after the reform, increasing the demand for apartments around district borders with lower average test scores. However, the reduced-form effects may capture a change in sorting by households with higher socioeconomic backgrounds after the reform. In other words, if parents with college degrees were more likely to live in the side of the districts with higher average test scores (Bayer, Ferreria and McMillan 2007) and these parents were more likely to move to the other side after the reform, then the reduced form effects may capture both changes in parents' perception on expected school quality and changes in sorting.

5 Conclusion

In this paper, I quantify the effects of relaxing restricted access to local schools on housing prices by comparing property values near school district borders before and after a school choice reform. Prior to the reform, the difference of one standard deviations in average college entrance exam scores was associated with a 7 percent housing premium at the district boundaries. In 2009, the school choice reform weakened the restrictions imposed by school district boundaries and opened up to 60 percent of seats within a district for outside district competition through application-based lotteries. I find that the pre-reform housing price gap decreased by 3.4 percentage points within four years after the reform. Even though the school choice reform significantly reduced the housing price gap, I find that the test score gap did not change much after the reform.

Overall, the findings suggest that alleviating restrictions on access to local schools significantly decreases the housing premium, and the effects are consistent with the channel of altering parents' perception of expected school quality in their districts. Utilizing data on high school applications and lottery outcomes would help me more accurately pin down parents' school preferences and substitution patterns. Understanding potential mechanisms behind the reduced-form effects using additional micro-data will be an interesting extension of this paper that may shed further light on the distributional consequences of the school choice reform.

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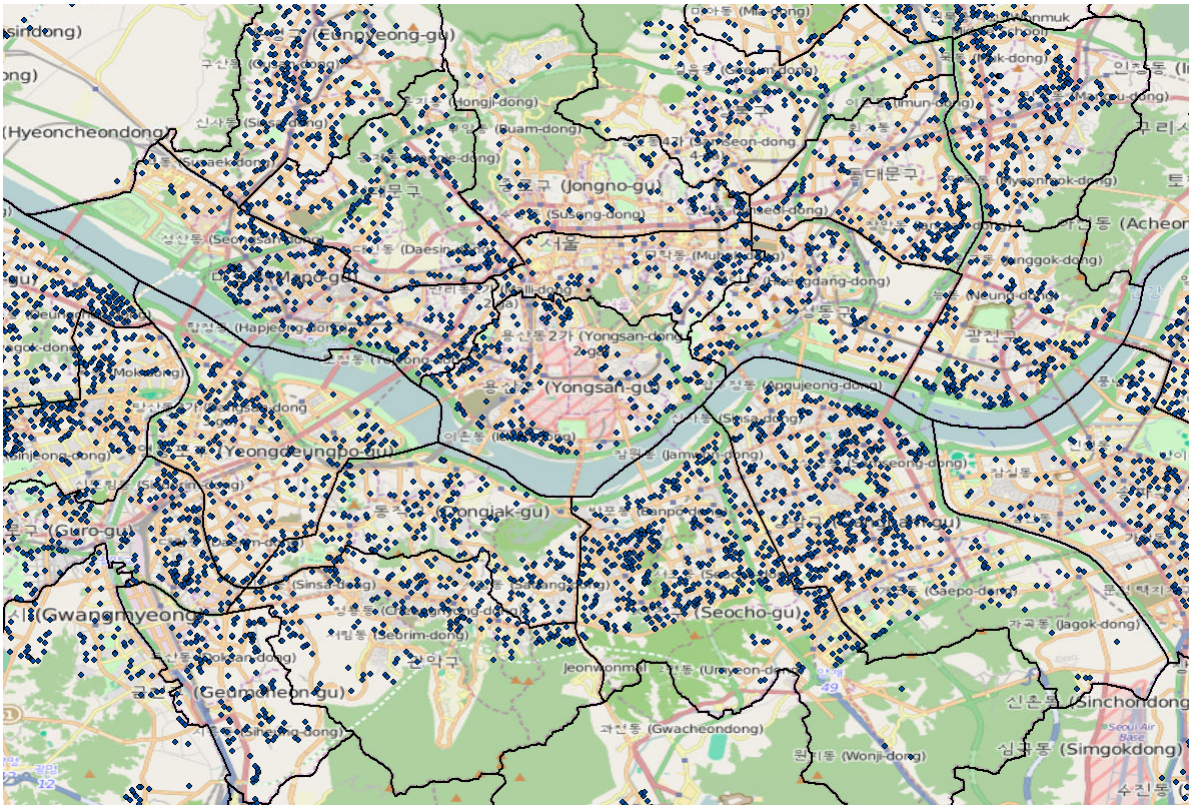
Figure 1: School District Boundaries in Seoul

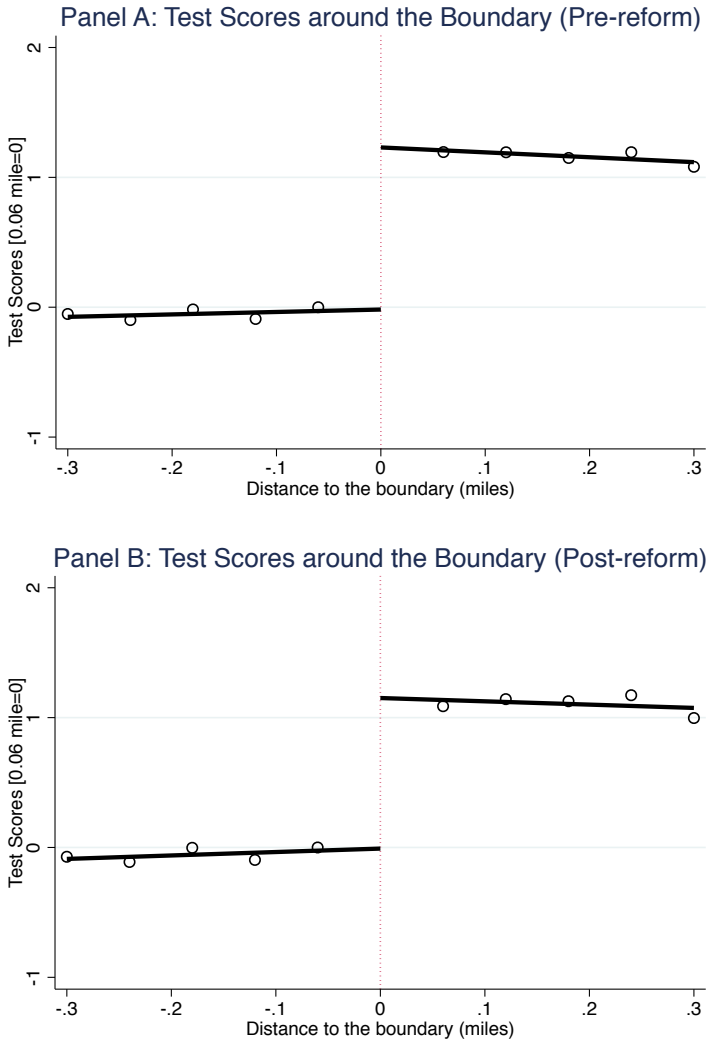
Notes: This figure shows school districts and their corresponding boundaries in Seoul. There are about 200 high schools and 26 school districts, so each district contains about 8 high schools on average. The blue dots indicate all-male high schools, the red dots indicate all-female high schools, and the green dots indicate co-ed high schools.

Figure 2: Geo-coded Apartment Sales in Seoul



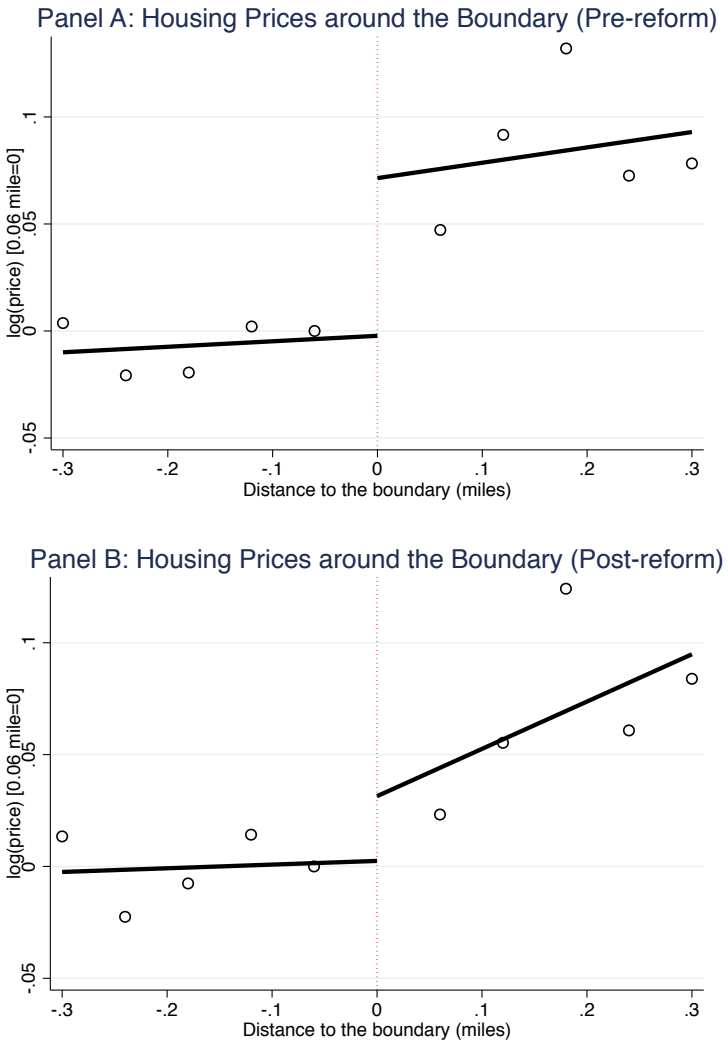
Notes: This figure shows (repeated) apartment sales in Seoul from 2007 to 2013. Each dot indicates a geo-coded location of the transaction. The black solid lines indicate the school district boundaries, as shown in Figure 1. Note that the spatial distribution of apartment sales is smooth within school districts and across the entire city, and there were no transactions in mountainous areas (green) or rivers (blue).

Figure 3: Reform Effects on Test Score Gap (Affected Areas)



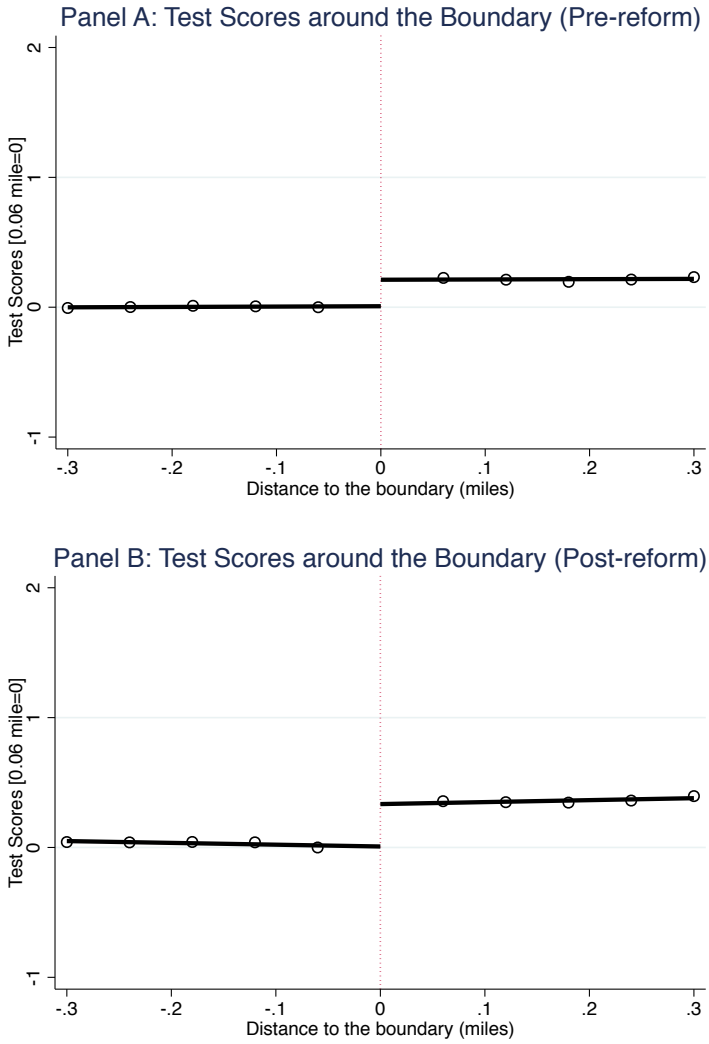
Notes: Panel A of this figure plots the residualized test scores within 0.3 miles of the school district boundaries prior to the reform in 2009, controlling for boundary fixed effects. Test scores are school-level average college entrance exam scores, standardized in each year and averaged at the district-level. A difference of one in test scores indicates a 1 standard deviation difference in average college entrance exam scores between two adjacent districts around the border. Each dot indicates a residualized test score averaged over each bin, where the bin size is 0.06 miles (0.1 kilometers). The negative distances indicate the lower test score side of the boundary. Note that the test scores are normalized to be zero at the -0.06 mile bin. Panel B of this figure plots the residualized test scores within 0.3 miles of the boundaries after the reform in 2009.

Figure 4: Reform Effects on Housing Price Gap (Affected Areas)



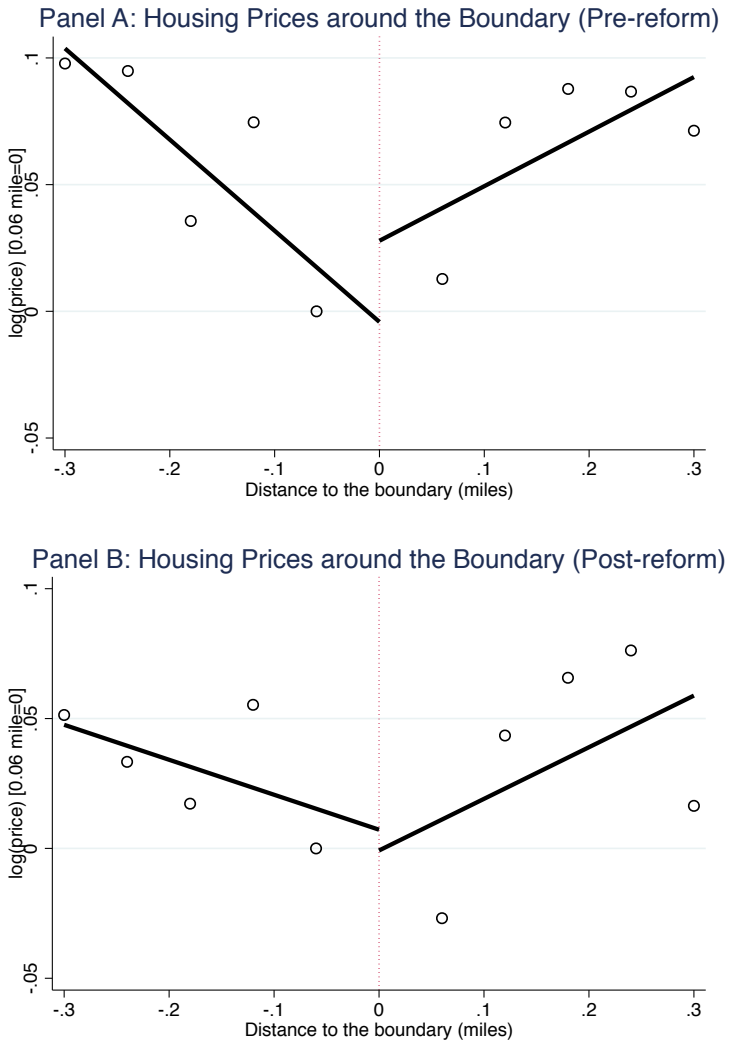
Notes: Panel A of this figure plots the residualized log(housing prices) within 0.3 miles of the school district boundaries prior to the reform in 2009, controlling for observable apartment characteristics and boundary fixed effects. Housing price is the transaction price for a given apartment unit. Each dot indicates a residualized price averaged over each bin, where the bin size is 0.06 miles (0.1 kilometers). The negative distances indicate the lower test score side of the boundary. Note that the housing prices are normalized to be zero at the -0.06 mile bin. Panel B of this figure plots the residualized log(housing prices) within 0.3 miles of the school district boundaries prior to the reform in 2009.

Figure 5: Reform Effects on Test Score Gap (Placebo Areas)



Notes: Panel A of this figure plots the residualized test scores within 0.3 miles of the school district boundaries prior to the reform in 2009, controlling for boundary fixed effects. Test scores are school-level average college entrance exam scores, standardized in each year and averaged at the district-level. A difference of one in test scores indicates a 1 standard deviation difference in average college entrance exam scores between two adjacent districts around the border. Each dot indicates a residualized test score averaged over each bin, where the bin size is 0.06 miles (0.1 kilometers). The negative distances indicate the lower test score side of the boundary. Note that the test scores are normalized to be zero at the -0.06 mile bin. Panel B of this figure plots the residualized test scores within 0.3 miles of the boundaries after the reform in 2009.

Figure 6: Reform Effects on Housing Price Gap (Placebo Areas)



Notes: Panel A of this figure plots the residualized log(housing prices) within 0.3 miles of the school district boundaries prior to the reform in 2009, controlling for observable apartment characteristics and boundary fixed effects. Housing price is the transaction price for a given apartment unit. Each dot indicates a residualized price averaged over each bin, where the bin size is 0.06 miles (0.1 kilometers). The negative distances indicate the lower test score side of the boundary. Note that the housing prices are normalized to be zero at the -0.06 mile bin. Panel B of this figure plots the residualized log(housing prices) within 0.3 miles of the school district boundaries prior to the reform in 2009.

Table 1: Descriptive Statistics

	Full Sample		Treated Areas		Control Areas	
	(1)	(2)	(3)	(4)	(5)	(6)
	Treated	Control	High Test Score	Low Test Score	High Test Score	Low Test Score
log(Price)	12.19	12.27	12.23	12.15	12.28	12.24
	(0.658)	(0.784)	(0.696)	(0.638)	(0.812)	(0.743)
Number of Beds	3.884	3.837	3.882	3.866	3.813	3.861
	(0.368)	(0.468)	(0.375)	(0.402)	(0.497)	(0.422)
Age of Building	13.16	13.34	13.66	12.95	14.23	13.09
	(8.810)	(8.987)	(9.303)	(9.043)	(9.593)	(8.976)
Floor Level	7.050	6.860	7.092	6.875	6.706	7.001
	(3.706)	(4.144)	(3.558)	(3.890)	(4.144)	(4.126)
Test Score	0.253	0.595	0.936	-0.547	0.787	0.422
	(0.929)	(0.790)	(0.607)	(0.580)	(0.784)	(0.843)
Observations	14147	13431	3507	4069	3261	3361

Notes: Sample years include 2007-2013. Price is the transaction price of a given apartment unit. Test scores are school-level average college entrance exam scores, standardized in each year and averaged at the district-level. Age is the difference in the current year and the year in which the apartment was built. Treated and control areas are defined in Section 3.

Table 2: Reform Effects on Test Scores (Affected Areas)

	Within 0.3 miles of Boundary		Within 0.1 miles of Boundary	
	(1)	(2)	(3)	(4)
	Test Score	Test Score	Test Score	Test Score
Highside	1.510***	1.513***	1.556***	1.559***
	(0.044)	(0.044)	(0.039)	(0.039)
Highside x Post	-0.127***	-0.132***	-0.122***	-0.127***
	(0.041)	(0.041)	(0.032)	(0.032)
Boundary FEs	Yes	Yes	Yes	Yes
Year FEs	No	Yes	No	Yes
Pre-reform Highside Mean	0.953	0.953	0.894	0.894
R-squared	0.87	0.87	0.86	0.87
Observations	7582	7582	2667	2667

Notes: This table reports the reform effects on test scores based on specification (2). The dummy for $Highside_{idb}$ equals 1 for the side of the district d with the higher average test score, as explained in Section 3. The dummy for $Post_t$ equals 1 if the time period is after the end of the school choice reform year (2009). Test scores are school-level average college entrance exam scores, standardized in each year and averaged at the district-level. A difference of one in test scores indicates a 1 standard deviation difference in the average college entrance exam score between two adjacent districts around the border. The main outcome is winsorized at the fifth and ninety-fifth levels. Each time period is a year, and the sample period is from 2007 to 2013. All specifications include boundary fixed effects and even-numbered columns include year fixed effects. The standard errors are clustered at the apartment block level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 3: Reform Effects on Housing Prices (Affected Areas)

	Within 0.3 miles of Boundary		Within 0.1 miles of Boundary	
	(1)	(2)	(3)	(4)
	ln(Price)	ln(Price)	ln(Price)	ln(Price)
Highside	0.099***	0.095***	0.100***	0.096***
	(0.036)	(0.036)	(0.030)	(0.030)
Highside x Post	-0.049**	-0.048**	-0.034**	-0.032*
	(0.023)	(0.023)	(0.017)	(0.017)
Basic Control	Yes	Yes	Yes	Yes
Boundary FEs	Yes	Yes	Yes	Yes
Year FEs	No	Yes	No	Yes
Pre-reform Highside Mean	12.170	12.170	12.127	12.127
R-squared	0.82	0.83	0.81	0.82
Observations	7576	7576	2665	2665

Notes: This table reports the reform effects on housing prices based on specification (2). The dummy for $Highside_{idb}$ equals 1 if an apartment i was located in the side of the district d with the higher average test score, as explained in Section 3. The dummy for $Post_t$ equals 1 if the time period is after the end of the school choice reform year (2009). Price is a transaction price for an apartment unit. Basic control variables include apartment characteristics, such as a number of beds, floor-level, age, and age-squared. The main outcome is winsorized at the fifth and ninety-fifth levels. Each time period is a year, and the sample period is from 2007 to 2013. All specifications include boundary fixed effects and even-numbered columns include year fixed effects. The standard errors are clustered at the apartment block level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 4: Reform Effects on Test Scores (Placebo Areas)

	Within 0.3 miles of Boundary		Within 0.1 miles of Boundary	
	(1)	(2)	(3)	(4)
	Test Score	Test Score	Test Score	Test Score
Highside	0.230***	0.228***	0.202***	0.200***
	(0.020)	(0.019)	(0.016)	(0.016)
Highside x Post	0.093***	0.093***	0.086***	0.086***
	(0.019)	(0.019)	(0.014)	(0.014)
Boundary FEs	Yes	Yes	Yes	Yes
Year FEs	No	Yes	No	Yes
Pre-reform Highside Mean	0.746	0.746	0.724	0.724
R-squared	0.95	0.95	0.96	0.96
Observations	6623	6623	2262	2262

Notes: This table reports the reform effects on test scores based on specification (2). The dummy for $Highside_{idb}$ equals 1 for the side of the district d with the higher average test score, as explained in Section 3. The dummy for $Post_t$ equals 1 if the time period is after the end of the school choice reform year (2009). Test scores are school-level average college entrance exam scores, standardized in each year and averaged at the district-level. A difference of one in test scores indicates a 1 standard deviation difference in the average college entrance exam score between two adjacent districts around the border. The main outcome is winsorized at the fifth and ninety-fifth levels. Each time period is a year, and the sample period is from 2007 to 2013. All specifications include boundary fixed effects and even-numbered columns include year fixed effects. The standard errors are clustered at the apartment block level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table 5: Reform Effects on Housing Prices (Placebo Areas)

	Within 0.3 miles of Boundary		Within 0.1 miles of Boundary	
	(1)	(2)	(3)	(4)
	ln(Price)	ln(Price)	ln(Price)	ln(Price)
Highside	0.035	0.040	0.025	0.030
	(0.037)	(0.037)	(0.031)	(0.031)
Highside x Post	-0.017	-0.020	-0.012	-0.017
	(0.029)	(0.029)	(0.022)	(0.022)
Basic Control	Yes	Yes	Yes	Yes
Boundary FEs	Yes	Yes	Yes	Yes
Year FEs	No	Yes	No	Yes
Pre-reform Highside Mean	12.239	12.239	12.176	12.176
R-squared	0.86	0.87	0.86	0.87
Observations	6622	6622	2262	2262

Notes: This table reports the reform effects on housing prices based on specification (2). The dummy for $Highside_{idb}$ equals 1 if an apartment i was located in the side of the district d with the higher average test score, as explained in Section 3. The dummy for $Post_t$ equals 1 if the time period is after the end of the school choice reform year (2009). Price is a transaction price for an apartment unit. Basic control variables include apartment characteristics, such as a number of beds, floor-level, age, and age-squared. The main outcome is winsorized at the fifth and ninety-fifth levels. Each time period is a year, and the sample period is from 2007 to 2013. All specifications include boundary fixed effects and even-numbered columns include year fixed effects. The standard errors are clustered at the apartment block level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

For Online Publication

This appendix supplements my paper "Access to Local Amenity and Housing Prices" with the following sections:

- Section A provides institutional details.
- Section B shows the results from robustness tests.

A Institutional Details

In Appendix A, I provide more institutional details on the high school assignments after the school choice reform in Korea.

A.1 High School Assignment in Seoul

- First stage: applicants may choose and apply to 2 schools in the city of Seoul. 20% (central 60%) of the applicants will be matched to schools based on their order of preferences, where lotteries are used to break ties.
- Second stage: applicants may choose and apply to 2 schools within their school districts. 40% of applicants will be matched to schools based on their order of preferences, where lotteries are used to break ties.
- Third stage: 40% of the applicants who were not initially matched will be reconsidered for the third round of lotteries based on their residence, religion, and their order of preferences in stages 1 and 2.

A.2 High School Assignment in Pusan

- A lottery system is used for assignments after applicants apply to desired schools.
- For "broad area" school districts, 40% of seats will be assigned to applicants who selected the district as their first choice. Applicants who selected the district as their second choice will be matched to schools if there is any excess capacity.
- For "local area" school districts, 20% of seats will be assigned to applicants who selected the district as their first choice. Applicants who selected the district as their second choice will be matched to schools if there is any excess capacity.

A.3 High School Assignment in Daejun

- Applicants will be matched to schools based on their order of preferences (60% of the seats).
- The remaining 40% of seats will be matched through a lottery system regardless of their ranked preferences to break ties in the case of excess demand.

A.4 High School Assignment in Daegu

- First stage: Apply to 2 different desired schools in the city of Daegu. 10% of the applicants will be matched to one of their desired schools based on their order of preferences using a lottery system.
- Second stage: Apply to 2 different schools within applicable school districts near their residence. If the applicant's residence is in a rural area, the applicant may apply to "Shinmyung" high schools associated in the second school district. If there is no available school in the district, the applicant may apply to a centralized school in a different school district. 40% of the applicants will be matched to one of their desired schools based on their ranked preferences using a lottery system.
- Third stage: 50% of applicants who were not placed in the first two stages will be matched to schools based on their commuting time and ranked preferences in stages 1 and 2.

B Robustness Checks

In Appendix B, I present a set of robustness test results based on (1) a different level of winsoring the main outcome variables, and (2) a different way of defining the treated areas.

The main results are based on winsorizing the main outcome variables at the ninety-fifth percentile. Table B.1 presents the results on the reform effects on housing prices based on the specification in equation 2 by winsorizing the outcome variable at the tenth and ninetieth percentiles. The results are quantitatively similar to the main results.

Moreover, the main results are based on restricting the main analysis sample of treated areas to have a minimum difference of a half standard deviations in average test scores between two adjacent districts. I re-run all the analyses in Section 4 by imposing a stricter restriction: I cut my sample to have a minimum difference of 0.7 standard deviations in average test scores to define the treated areas. As Tables B.2 and B.3 show, the results are qualitatively similar to the main results in Tables 2 and 3. Additionally, I cut my sample to have a maximum difference of 0.2 standard deviations in average test scores to define the control areas. As Tables B.4 and B.5 show, the results are qualitatively similar to the main results in Tables 4 and 5.

Table B.1: Reform Effects on Housing Prices (different levels of winsorizing)

	Within 0.3 miles of Boundary		Within 0.1 miles of Boundary	
	(1) ln(Price)	(2) ln(Price)	(3) ln(Price)	(4) ln(Price)
Highside	0.110***	0.106***	0.104***	0.101***
	(0.033)	(0.033)	(0.028)	(0.028)
Highside x Post	-0.052**	-0.051**	-0.040**	-0.038**
	(0.021)	(0.021)	(0.016)	(0.016)
Basic Control	Yes	Yes	Yes	Yes
Boundary FEs	Yes	Yes	Yes	Yes
Year FEs	No	Yes	No	Yes
Pre-reform Highside Mean	12.164	12.164	12.141	12.141
R-squared	0.82	0.83	0.82	0.82
Observations	7576	7576	2665	2665

Notes: This table reports the reform effects on housing prices based on specification (2). The dummy for $Highside_{idb}$ equals 1 if an apartment i was located in the side of the district d with the higher average test score, as explained in Section 3. The dummy for $Post_t$ equals 1 if the time period is after the end of the school choice reform year (2009). Price is a transaction price for an apartment unit. Basic control variables include apartment characteristics, such as a number of beds, floor-level, age, and age-squared. The main outcome is winsorized at the tenth and ninetieth levels. Each time period is a year, and the sample period is from 2007 to 2013. All specifications include boundary fixed effects and even-numbered columns include year fixed effects. The standard errors are clustered at the apartment block level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table B.2: Reform Effects on Test Scores (Affected Areas)

	Within 0.3 miles of Boundary		Within 0.1 miles of Boundary	
	(1)	(2)	(3)	(4)
	Test Score	Test Score	Test Score	Test Score
Highside	1.533***	1.536***	1.591***	1.594***
	(0.049)	(0.049)	(0.042)	(0.042)
Highside x Post	-0.151***	-0.156***	-0.134***	-0.138***
	(0.048)	(0.048)	(0.038)	(0.038)
Boundary FEs	Yes	Yes	Yes	Yes
Year FEs	No	Yes	No	Yes
Pre-reform Highside Mean	0.962	0.962	0.906	0.906
R-squared	0.86	0.86	0.86	0.86
Observations	6616	6616	2289	2289

Notes: This table reports the reform effects on test scores based on specification (2). The dummy for $Highside_{idb}$ equals 1 for the side of the district d with the higher average test score, as explained in Section 3. The dummy for $Post_t$ equals 1 if the time period is after the end of the school choice reform year (2009). Test scores are school-level average college entrance exam scores, standardized in each year and averaged at the district-level. A difference of one in test scores indicates a 1 standard deviation difference in the average college entrance exam score between two adjacent districts around the border. The main outcome is winsorized at the fifth and ninety-fifth levels. Each time period is a year, and the sample period is from 2007 to 2013. All specifications include boundary fixed effects and even-numbered columns include year fixed effects. The standard errors are clustered at the apartment block level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table B.3: Reform Effects on Housing Prices (Affected Areas)

	Within 0.3 miles of Boundary		Within 0.1 miles of Boundary	
	(1)	(2)	(3)	(4)
	ln(Price)	ln(Price)	ln(Price)	ln(Price)
Highside	0.098**	0.094**	0.108***	0.106***
	(0.038)	(0.038)	(0.032)	(0.032)
Highside x Post	-0.057**	-0.057**	-0.037**	-0.036**
	(0.024)	(0.025)	(0.018)	(0.018)
Basic Control	Yes	Yes	Yes	Yes
Boundary FEs	Yes	Yes	Yes	Yes
Year FEs	No	Yes	No	Yes
Pre-reform Highside Mean	12.156	12.156	12.116	12.116
R-squared	0.83	0.83	0.81	0.81
Observations	6610	6610	2287	2287

Notes: This table reports the reform effects on housing prices based on specification (2). The dummy for $Highside_{idb}$ equals 1 if an apartment i was located in the side of the district d with the higher average test score, as explained in Section 3. The dummy for $Post_t$ equals 1 if the time period is after the end of the school choice reform year (2009). Price is a transaction price for an apartment unit. Basic control variables include apartment characteristics, such as a number of beds, floor-level, age, and age-squared. The main outcome is winsorized at the fifth and ninety-fifth levels. Each time period is a year, and the sample period is from 2007 to 2013. All specifications include boundary fixed effects and even-numbered columns include year fixed effects. The standard errors are clustered at the apartment block level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table B.4: Reform Effects on Test Scores (Placebo Areas)

	Within 0.3 miles of Boundary		Within 0.1 miles of Boundary	
	(1)	(2)	(3)	(4)
	Test Score	Test Score	Test Score	Test Score
Highside	0.190***	0.188***	0.151***	0.151***
	(0.019)	(0.019)	(0.011)	(0.011)
Highside x Post	0.091***	0.090***	0.080***	0.080***
	(0.021)	(0.021)	(0.016)	(0.016)
Boundary FEs	Yes	Yes	Yes	Yes
Year FEs	No	Yes	No	Yes
Pre-reform Highside Mean	0.839	0.839	0.786	0.786
R-squared	0.95	0.96	0.97	0.97
Observations	5439	5439	1930	1930

Notes: This table reports the reform effects on test scores based on specification (2). The dummy for $Highside_{idb}$ equals 1 for the side of the district d with the higher average test score, as explained in Section 3. The dummy for $Post_t$ equals 1 if the time period is after the end of the school choice reform year (2009). Test scores are school-level average college entrance exam scores, standardized in each year and averaged at the district-level. A difference of one in test scores indicates a 1 standard deviation difference in the average college entrance exam score between two adjacent districts around the border. The main outcome is winsorized at the fifth and ninety-fifth levels. Each time period is a year, and the sample period is from 2007 to 2013. All specifications include boundary fixed effects and even-numbered columns include year fixed effects. The standard errors are clustered at the apartment block level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

Table B.5: Reform Effects on Housing Prices (Placebo Areas)

	Within 0.3 miles of Boundary		Within 0.1 miles of Boundary	
	(1)	(2)	(3)	(4)
	ln(Price)	ln(Price)	ln(Price)	ln(Price)
Highside	0.051	0.055	0.041	0.044
	(0.040)	(0.040)	(0.034)	(0.034)
Highside x Post	-0.031	-0.033	-0.022	-0.025
	(0.032)	(0.032)	(0.025)	(0.025)
Basic Control	Yes	Yes	Yes	Yes
Boundary FEs	Yes	Yes	Yes	Yes
Year FEs	No	Yes	No	Yes
Pre-reform Highside Mean	12.322	12.322	12.205	12.205
R-squared	0.87	0.87	0.87	0.88
Observations	5438	5438	1930	1930

Notes: This table reports the reform effects on housing prices based on specification (2). The dummy for $Highside_{idb}$ equals 1 if an apartment i was located in the side of the district d with the higher average test score, as explained in Section 3. The dummy for $Post_t$ equals 1 if the time period is after the end of the school choice reform year (2009). Price is a transaction price for an apartment unit. Basic control variables include apartment characteristics, such as a number of beds, floor-level, age, and age-squared. The main outcome is winsorized at the fifth and ninety-fifth levels. Each time period is a year, and the sample period is from 2007 to 2013. All specifications include boundary fixed effects and even-numbered columns include year fixed effects. The standard errors are clustered at the apartment block level and are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.