The willingness to pay for better elementary education. Applying a spatial difference in difference model in Amsterdam

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ARTICLE INFO

Keywords:
Elementary education
School quality
Amsterdam
Spatial difference in difference
Capitalization of education

ABSTRACT

This paper studies the causal effect of the quality of elementary education on housing prices in Amsterdam between 2011 and 2019. A policy change from neighborhood to distance-based school admission rules in Amsterdam in 2015 is the empirical context for our research. We embedded a spatial difference in difference model with neighborhood boundary discontinuities in a hedonic pricing function to measure the causal effect of school quality on housing prices. Using data on CITO test scores, housing transactions and neighborhood characteristics, we find that a standard deviation increase in school quality raises housing prices with 2.3%, which corresponds with a mean increase of $8,590$ euros. Our results confirm that the willingness to pay for elementary education in Amsterdam is not different from other countries studied in the literature and is a potential segregating force.

1. Introduction

Residence-based school assignment policies affect the behavior of households regarding residential choice. The quality of an elementary school is important to (future) parents, as quality of education is causally related to future school career and income (Card, 1999; Currie and Thomas, 1999; Heyneman and Loxley, 1983). Residence-based school assignment policies increase demand for residential property in preferable school assignment zones (Black, 1999). Studies in multiple countries confirm the hypothesis that a high-quality school positively affects the value of residential property in the catchment zone (Black and Machin, 2011). When prices rise, selection by mortgage, an effect where only wealthier parents can live in the catchment area of good schools, leads to residential and educational segregation (Gibbons and Machin, 2008). The question is to what extent this force drives neighborhood sorting and segregation in neighborhoods and schools. Black (1999) and Bayer et al. (2007) find that school district boundaries coincide with segregation, where more affluent households live in better school districts. The parent's willingness to pay for school performance is an important parameter in the estimation of the model explaining this behavior. This paper estimates the willingness to pay for elementary school performance in Amsterdam. It uses a local reform in the school assignment policy in 2015 in a spatial difference in difference model to estimate the willingness to pay via capitalization in property values. The willingness to pay for school performance is important to the municipality, as its policies target inequity, ambiguity, non-transparency and segregation in neighborhoods (Rekenkamer Amsterdam, 2014).

This paper tests the hypothesis that school quality raises housing prices in the context of Amsterdam. Amsterdam knows no strict school assignment zones, which complicates the identification strategy. Hence, this paper exploits a policy reform in 2015, the *Nieuw Stedelijk Toelatingsbeleid Basisonderwijs Amsterdam*, introducing centralized priority assignment for households based on the walking distance between the home address and school. This empirical context allows for identification of the effect in a quasi-experimental set up as the policy created an empirical treated and control group.

This paper applies a spatial difference in difference model (SDiD) with neighborhood boundary discontinuities embedded in a hedonic pricing function to estimate the causal effect of elementary school quality on housing prices in Amsterdam in the period 2011-2019. This paper uses data on central test scores (CITO toets[1]) between 2009 and 2019, data on housing transactions in Amsterdam in the period 2011 to 2019 and census block data from 2015. The contribution to the literature is a first estimate of parental willingness to pay for elementary education in Amsterdam, a city without strict school catchment zones, and in the Netherlands in general. Correcting for time trend, housing and neighborhood characteristics, and neighborhood fixed effects, we find that a standard deviation increase in school quality causes a 2.3% increase in housing prices. The size of this effect is of the same order of magnitude as the results of studies in other countries and is hence not completely mitigated by the absence of strict school districts. This estimate is an important parameter in explaining residential and educational segregation and is hence of importance to policy makers.

Education behaves as a public good in many ways when evaluating its excludability and rivalry. Hence, the government organizes and finances education in most countries to secure provision and accessibility. As such, there is no direct market for public education. Nevertheless, residence- based school assignment induces an indirect market for elementary education which relies on spatial sorting. Tiebout (1956) introduced the most well-known model for spatial sorting in which heterogeneous individuals can sort themselves over different locations with heterogeneous properties. The locality of elementary education and parental preference for schools in the proximity induce spatial sorting as described in the Tiebout model.

[1] The official name is "Centrale Eindtoets basisonderwijs", but the test is still widely referred to as CITO test, which is the former name.

Although the government fully funds elementary education in the Netherlands, the quality differs per school. Parents have an incentive to enroll their children in better schools, as studies find a causal relationship between good education and income and employment (Card, 1999; Chetty et al., 2014; Currie and Thomas, 1999). Additionally, peers affect the performance of pupils and their future income, which generally is a positive effect at well performing schools (Abdulkadiroğlu et al., 2020; Holme, 2002; Rothstein, 2006). The literature finds that parents themselves value proximity, peers, and the output of schools, which are mostly test scores (Abdulkadiroğlu et al., 2020; Black, 1999; Figlio and Lucas, 2004; Hastings et al., 2005; Hayes et al., 1996; Rothstein, 2006). The preference for better schools increases with income, parental education level, and most importantly, children (Barrow, 2002; Hastings et al., 2005).

The literature searched for empirical methods to estimate the willingness to pay for elementary school performance in different countries and time periods. Early empirical research relies on housing transaction data and traditional hedonic pricing methods controlling for housing characteristics and neighborhood amenities. The difficulty in the estimation of the willingness to pay for school performance lies in the unobserved heterogeneity between different neighborhoods or school catchment zones and reverse causality. Better schools lie in better neighborhoods and children of wealthier parents perform better in school (Card, 1999; Currie and Thomas, 1999), which leads to omitted variable bias. Reverse causality is the result of local taxes and initiatives to increase school performance (Black and Machin, 2011). Wealthier neighborhoods with higher property values yield higher taxes, which in turn can lead to better school performance. Due to omitted variables and reverse causality, the school performance variable is not exogenous in the traditional hedonic pricing function and estimates of the effect on house prices in these functions are upwardly biased.

Black (1999) proposed an innovative method to reduce bias in the estimation of the hedonic price function. The paper introduces a regression discontinuity design, which selects housing transactions in a bandwidth on two sides along an admission boundary in suburbs of Boston between 1993 and 1995. The assumption is that houses in the bandwidth share the same neighborhood amenities except for school quality as the bandwidth of 150 meters on both sides of the boundary is sufficiently small (Black, 1999). Black (1999) finds an effect of 2.1% on property values for a standard deviation change from the mean school performance, which is measured with test scores. Fack and Grenet (2010) use a similar method for estimations in Paris between 1997 and 2004, but match data points to similar observations on the other side of the boundary for comparison. Fack and Grenet (2010) find a 1.4% to 2.4% effect on housing prices for a standard deviation change in public school performance, depending on the chosen performance variables. A high number of private schools mitigates this effect (Fack and Grenet, 2010). Other studies using a regression discontinuity design find an effect of between 2% to 4% (Clapp et al., 2008; Davidoff and Leigh, 2008; Gibbons and Machin, 2003). Although the regression discontinuity design is successful in eliminating much bias, the method is only applicable to areas with strict catchment zone boundaries.

We find an example of the upward bias when we compare the papers of Kane et al. (2003, 2006). The early study replicates the work of Black (1999) and finds an effect of 18% to 25% in North Carolina. In 2006, Kane et al. revisit North Carolina and use a difference in difference model with extra control variables to correct for unobserved neighborhood characteristics resulting in estimations of 9% to 12%. Additionally, Kane et al. (2006) stress the difficulty of disentangling the effects of school performance and household sorting along the boundaries of catchment zones.

Bayer et al. (2007) approach the question with a sorting model, which deals with the problem of correlation between school performance and unobserved neighborhood quality. The sorting model enables the authors to include individual preferences in the model. This is an important factor as preferences are heterogeneous and households sort into neighborhoods where the amenities satisfy their specific preferences (Bayer and McMillan, 2012; Tiebout, 1956). The paper finds a 1% effect on property prices for a standard deviation change in school quality from the mean and shows that the willingness to pay for school performance of households is heterogeneous. The authors argue that their estimate is lower because of a smaller upward bias due to unobserved neighborhood characteristics as a result of the sorting model. Yinger (2015) revisits the sorting model using data from Cleveland and finds an elasticity of 0.75 between house sales prices and school performance.

Quasi-experimental studies exploit changes in policy reforms for identification. These policy reforms entail court orders (Kane et al., 2006), new information sources on school quality (Figlio and Lucas, 2004; Kane et al., 2003), school reforms (Dee, 2000), loosening of school assignment regulations (Machin and Salvanes, 2016; Reback, 2005), and redrawing of school district boundaries (Bogart and Cromwell, 2000; Dhar and Ross, 2012; Ries and Somerville, 2010). Quasi-experimental set ups can be successful if the policy reform is unanticipated, as this entails that no sorting took place. Therefore, a quasi-experimental set up can eliminate within-neighborhood endogeneity problems. Most of the above-mentioned studies find an effect between 3% and 12% for a standard deviation change in school performance (Bogart and Cromwell, 2000; Dee, 2000; Dhar and Ross, 2012; Figlio and Lucas, 2004; Kane et al., 2006; Machin and Salvanes, 2016; Reback, 2005), and Nguyen-Hoang and Yinger (2011) find that the mean effect of the significant studies in the field, including all different methods, lies around 4%. There are a few exceptions, as the previously mentioned paper by Kane et al. (2003) finds an effect of 18% to 25% percent, likely due to the upward bias. Furthermore, Ries and Somerville (2010) only find a significant effect for houses likely to be bought by wealthier households.

The next section, section 2, outlines the supply of elementary education in Amsterdam. Section 3 presents the method we used to measure a causal effect. Section 4 gives a summary of the data and description of key variables. Section 5 presents the results of our model and discusses the findings in the context of the literature.

2. Elementary education in Amsterdam

This section outlines the elementary education system present in the Netherlands, explains the policy change in Amsterdam and discusses the CITO tests.

2.1. Elementary education in the Netherlands and a new central admissions policy in Amsterdam

Central in this paper is the *Nieuw Stedelijk Toelatingsbeleid Basisonderwijs Amsterdam*, which is the the name of the new central elementary school admission policy in Amsterdam introduced in 2015 (Breed Bestuurlijk Overleg Amsterdam, 2014). Children in the Netherlands attend elementary school between the age of four and twelve. Elementary education is mandatory and predominantly public. Private elementary education is not popular in the Netherlands and almost all pupils attend a publicly funded school (Rekenkamer Amsterdam, 2014). Before 2015 there was no central admission or placement regulation. Neighborhoods and schools independently organised registrations and placement, which resulted in a non-transparent situation for parents and the municipality. Parents competed for places at the best schools. Most common were priority regulations based on four-digit postal code (PC4) areas (Breed Bestuurlijk Overleg Amsterdam, 2014). Situations where parents could not register their children at the nearest school because of neighborhood boundaries occurred frequently (Breed Bestuurlijk Overleg Amsterdam, 2014). Moreover, the situation stimulated segregation and led to an unequal distribution of disadvantaged pupils (Rekenkamer Amsterdam, 2014). Hence, the pupil population of individual schools became more homogeneous.

The schools and municipality initially intended to tackle non-transparency and segregation with a central admission system. However, after pilots between 2010 and 2015 the municipality found that reducing segregation could not be unified with the other goals of the policy as parents could buy property in the vicinity of schools (Breed Bestuurlijk Overleg Amsterdam, 2014). Transparency and unambiguity in the registration of pupils formed the motivation to reform the admission policy in 2015 (Breed Bestuurlijk Overleg Amsterdam, 2014). Furthermore, the municipality hoped that new regulations would lead to a pupil population that mirrored the population of the neighborhood (Rekenkamer Amsterdam, 2014). Amsterdam introduced the *Nieuw Stedelijk Toelatingsbeleid Basisonderwijs Amsterdam* in 2015. The policy aimed at ensuring that 75% of the pupils would be admitted at their first choice and 90% at their top eight schools (Breed Bestuurlijk Overleg Amsterdam, 2014). To reach these goals, the municipality introduced a central admission system with several priority regulations which would be equal for all pupils in Amsterdam. Priority placement remained similar to before 2015 with respect to family at school and priority positions due to a preschool registration. The important addition was that pupils receive a priority position at the eight closest schools from their home address (Breed Bestuurlijk Overleg Amsterdam, 2014). Neighborhood boundaries no longer play a role, as the distance to a school is measured in walking distance. Parents can fill in their preferences when their child is two years old and can find information on which schools qualify for priority placement online. The municipality places children in three enrollment rounds during the year. When demand for a school exceeds the capacity, the municipality assigns the pupils randomly to another school on their preference list. Pilots showed that the measures were sufficient to reach the enrollment targets (Rekenkamer Amsterdam, 2014; Breed Bestuurlijk Overleg Amsterdam, 2014). This paper hypothesizes that the high chance of acceptance at one of the closest eight schools has risen demand for houses close to well-performing schools, which in turn increases prices.

2.2. The CITO test

Pupils in the Netherlands take the central tests for elementary schools (CITO) around the age of twelve in their final year of elementary school. The test is not mandatory, but most schools participate, as the test score is an important indicator for the future school career. The test is graded on a scale from 500 to 550 and the results are scaled to have a constant mean over time. An elementary school hands pupils an official recommendation for the level of high school, which is partly based on the CITO score. In the Netherlands, the level of high schools varies from very practical to theoretical scientific education. A higher CITO score, leads to a recommendation for a higher level of education, which in turn leads to higher future income (Card, 1999; Currie and Thomas, 1999; Heyneman and Loxley, 1983). Hence, the average CITO score on a school is an indicator of future success for parents. Hayes et al. (1996) and Hastings et al. (2005) confirm the hypothesis that parents value good grades in their research on the preference of parents for school characteristics. Hence this study uses CITO scores as indicator for school quality.

3. Estimating the capitalization of elementary education

The main issue in the literature is the identification and isolation of a causal effect. Early research relied on a traditional hedonic pricing model to estimate the relation between school quality and property prices. The standard hedonic pricing model assumes that the price of heterogeneous goods is determined by a vector of its characteristics (Rosen, 1974). For residential property, these characteristics consist of the features of the house and the neighborhood characteristics. The hedonic pricing method combines observed transaction prices and characteristics of many heterogeneous houses to estimate coefficients for all characteristics. The coefficients reflect the marginal willingness to pay for the specific characteristic. Including school performance in the vector of characteristics enables estimation of the willingness to pay for better school performance. A traditional hedonic pricing function is of the form:

$$\ln(p_{isjt}) = \alpha + \beta z_s + \gamma X_i + \epsilon N_j + \zeta Y_t + u_{isjt}, \quad (1)$$

where $\ln(p)$ denotes the natural logarithm of the price of housing transaction i in the attendance zone of school s in neighborhood j in year t. α is a constant, z the school performance indicator of school district s, X a vector of the characteristics of house i, N a vector of neighborhood characteristics of neighborhood j, Y a vector of year dummies and u_{isjt} is an i.i.d. error term. The coefficient β should reflect the marginal willingness to pay of parents for better school performance. However, as Black (1999) points out in her research, this estimate is upwardly biased due to the inclusion in the error term of unobserved neighborhood characteristics correlated with school performance which positively affect the house price. Therefore, β is not an estimate of a causal effect. Fack and Grenet (2010) studied the severity of the bias and find that a traditional hedonic pricing function overestimates the effect by 20%. Black (1999) proposed a regression discontinuity design to lower bias. Selecting observations in a bandwidth along school attendance boundaries lowers bias caused by unobserved neighborhood characteristics under the assumption that these characteristics vary continuously over space and are not interrupted by the attendance boundaries (Bayer et al., 2007; Black, 1999; Fack and Grenet, 2010). Comparing sales on both sides of the boundaries hence creates a spatial discontinuity in school performance, which solves the endogeneity issue in the traditional hedonic pricing function. Fack and Grenet (2010) find some limitations in the method of Black (1999). The regression discontinuity design assumes constant neighborhood fixed effects over time, similar willingness to pay for housing characteristics in different areas and no correlation between unobserved neighborhood characteristics across a boundary (Fack and Grenet, 2010).

Our identification method cannot rely on a regression discontinuity design, although the bandwidth is applicable, as Amsterdam has no strict school catchments zones. Alternatively, this paper exploits the policy reform in 2015 in the municipality of Amsterdam, which entailed the introduction of a central admission system with priority regulations for pupils in the vicinity of the schools. The important consequence of the new policy is that households close to a neighborhood border can now freely register their children with priority on schools in the vicinity on the other side of the border. Hence, individual households on different sides of a PC4 boundary experience different consequences from the policy change in 2015. Households in PC4 areas with relatively weak schools benefit if they can register their children at better schools in other areas. Depending on residential location and the supply of schools in the vicinity, other households do not experience such benefit. Hence, the policy change historically created a treated and control group. To tackle the bias due to unobserved heterogeneity in neighborhood amenities, this paper selects observations in a bandwidth along the PC4 boundaries similar to the method of Black (1999). We chose for a 100-meter buffer on both sides of the boundary to select enough observations. We compare the treatment effect of each observation to the mean effect in the buffer to select observations which benefit relative to the other observations in their buffer for the treatment group. This paper does not exploit a sharp discontinuity in space, but a temporal and spatial change. To correct for unobserved heterogeneity between neighborhoods, the equation includes fixed effects for three-digit postal codes and neighborhood characteristics. We chose PC3 over PC4 fixed effects because the treatment effect is closely related to the PC4 neighborhood of the observation. Hence, it dampens too much useful information in the model.

This paper applies a SDiD, which fits the empirical context of Amsterdam. A SDiD is a quasi-experimental method which can be applied when historical events create a treated and control (non-treated) group based on their geographical location. The difference in difference model compares the average effect over time of an explanatory variable of interest on a treated group and a control group using panel data. In this context the treatment group consists of the housing transactions in a buffer along the neighborhood border which experienced benefit from the policy change relative to the rest of the buffer. The control group consists of households who did not experience benefit relative to the mean of the buffer. The observations that actually received treatment are the observations in the treatment group in or after 2015. The houses in the same buffer are likely to experience the same parallel trend, as the bandwidth is sufficiently narrow to reduce the omitted variable bias due to unobserved heterogeneity. In contrast to regular hedonic pricing methods, SDiD can find causal effects. This method addresses reverse causality, as the quasi-experimental set up can identify an effect directly caused by the treatment. We base treatment on the change in school quality relative to the local buffer mean. Hence, we measure a different concept than possible reverse causality via local school taxes. This paper proposes a different hedonic pricing function from the traditional form as described in the paragraph above:

$$\ln(p_{ijt}) = \alpha + \beta X_i + \gamma N_j + \epsilon F_j \cdot Y_t + \delta_0 D_i + \delta_1 T_i + u_{ijt} \quad (2)$$

where $\ln(p)$ denotes the natural logarithm of the price of housing transaction i in neighborhood j in year t. α is the intercept, X a vector of housing characteristics and N a vector of neighborhood characteristics. F is a vector of three-digit postal code (PC3) neighborhood dummies to include neighborhood fixed effects in the model. Y is a set of year dummies, which interacts with the PC3 dummies to include an estimate of the fixed effects for each observed year. New in the equation is the dummy D, which indicates if observation i is in the treated group. We add a second dummy T, which is the product of D with a treatment year dummy equal to zero if from before 2015 (period 1) and equal to 1 for years after 2015 (period 2). The estimate δ_0 shows the effect of being in the treatment group on $\ln(p)$, while δ_1 yields the effect of experiencing treatment. The sign and magnitude of δ_0 and δ_1 will tell if a causal relation exists. We expect δ_0 to be negative, as the treatment group is a deprived group when compared to the mean in the buffer. If this deprivation

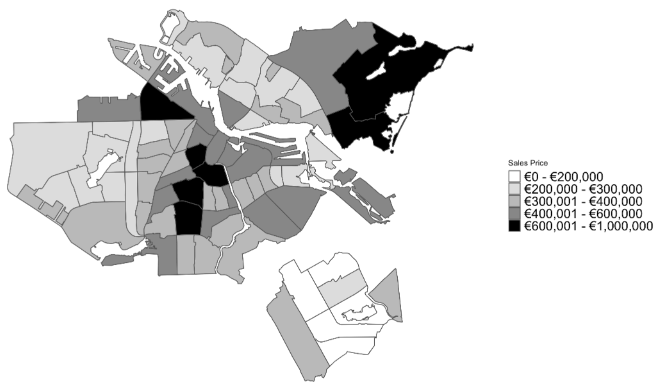


Figure 1: Mean house sale price per 4-digit postal code in the period 2011-2019.

Figure 2: The spatial distribution of the sampled observations within 100 meters of 4-digit postal code borders.

is not sufficiently large, a non-significant δ_0 lies in the line of expectation as well. As the treatment in or after 2015 affects the observations positively, we expect δ_1 to be positive. If δ_0 and δ_1 have similar sign and magnitude, there is no significant difference in the treatment group before and after treatment.

4. Data and summary Statistics

To estimate the willingness to pay for better education in Amsterdam we collected data on housing transactions between 2011 and 2019, CITO scores between 2009 and 2019 and neighborhood census block data from 2015. This paper uses housing transaction data on Amsterdam from the NVM, a Dutch real estate agents association covering around 75% of the national housing transactions (NVM, 2021). We retrieved CITO scores from 2009 to 2019 for schools in Amsterdam from an online database of the executive education branch of the government (DUO), which keeps track of the performance of all schools (Dienst Uitvoering Onderwijs, 2019). For data on PC4 boundaries, this paper uses spatial data provided by the municipality of Amsterdam (Gemeente Amsterdam, 2021). We retrieved data on neighborhood characteristics from CBS, The Dutch central agency for statistics (CBS, 2021).

4.1. Residential property transactions

The sample size of the NVM housing transaction data is 81,100, which are roughly 75% of the observed transactions in Amsterdam between 2011 and 2019. The mean sales price of the houses is 373,480 euros with a price of 4,175 euros per square meter. Figure 1 shows the spatial distribution of house prices per PC4 neighborhood. For the analysis, we subsetted the data based on geographical location. The data included the coordinates of the transaction addresses which point them precisely on the map of Amsterdam. We only considered observations in a 100-meter bandwidth along PC4 boundaries, which reduced the sample size to 26,468. We matched observations contained by multiple buffers to the buffer of the closest boundary[2]. We displayed the sampled observations within a 100-meter bandwidth of a boundary in figure 2. Subsampling concentrated the observations in the center of Amsterdam as the number of PC4 neighborhoods borders increases there, which is visible in figure. Table 1 displays the means and standard deviations of the different samples. The subsample contains more expensive houses as the mean price per square meter is 4,461 versus 4,175 euros in the full sample. Furthermore, the subsample contains older houses of which a higher share is monumental. This can be explained by the concentration of observations in the historical centre of the city. The observations in the subsample contain households which are part of a higher income group, have better school quality and less kids than households in the full sample. However, these differences are small. Subsetting slightly altered the sample means, but we did not observe any alarming changes in table 1.

[2]Measured in Euclidean distance from the observation to the closest point on the boundary.

4.2. PC4 neighborhoods and neighborhood characteristics

PC4 neighborhoods are administrative neighborhoods defined by four-digit postal codes. Amsterdam counts 80 unique PC4 neighborhoods. PC4 school boundaries coincide with other legislative boundaries, which is a frequent concern when using a spatial discontinuity (Black, 1999; Fack and Grenet, 2010). However, this paper explicitly uses the cut off year 2015, which is not related to another administrative policy change on a PC4 level in Amsterdam to the best of our knowledge. PC4 boundaries are consistent over time. The data consists of eighty polygons that span the area of every individual PC4 neighborhood. Reducing the boundaries of the polygons to line strings, split at every boundary intersection, allows for the construction of buffers along every unique border. This entails that a buffer contains one boundary between only two neighborhoods[3]. The buffers have a bandwidth of 100 meters (figure 2) from the boundary which selects a sufficiently large sample size with comparable sample means to

[3]We split the line strings (borders) at every intersection with another string (border). Hence the remaining strings entail the one to one border of only two adjacent neighborhoods. We used these unique borders to construct buffers. Hence, we only selected the observations on both sides of a one to one border.

Table 1
Summary statistics of full sample, buffer sample, treated group and control group

Variables	Full sample		<100 m		Treated		Control	
	Mean	s.d.	Mean	s.d.	Mean	s.d.	Mean	s.d.
House characteristics								
Price	373,480	281,488	402,898	286,140	399,408	267,276	406,079	302,282
Size	88.93	42.42	90.2	44.4	89.48	42.9	90.9	43.8
Price per m^2	4,175	1,690	4,461	1,654	4,464	1,548	4,459	1,639
Age of building (percent)								
≤ 1905	15.7		20.3		21.3		19.3	
1906 - 1944	33.1		33.8		34.7		32.9	
1945 - 1970	12.5		7.4		7.9		6.9	
1971 - 1990	11.7		9.5		8.9		10.1	
≥ 1991	24.9		26.6		25.1		28.0	
Unknown	2.1		2.4		2.1		2.8	
Rooms (percent)								
One	3.2		3.2		2.5		3.8	
Two	21.1		22.6		22.5		22.5	
Three	39.3		40.5		42.2		38.9	
Four	21.2		19.5		19.0		19.9	
≥ Five	15.2		14.2		13.8		14.9	
Number of floors (percent)								
One	68.9		69.5		69.7		69.4	
≥ Two	31.1		30.5		30.3		30.6	
Parking (percent)	14.4		17.2		16.9		17.5	
Garage (percent)	10.1		12.3		12.3		12.3	
Balcony (percent)	52.3		52.0		53.0		51.1	
Monumental (percent)	3.1		4.3		3.7		4.9	
Neighborhood characteristics								
Number of households per block	7,851	2,812	7,905	2,783	7,943	2,782	7,870	2,783
Number of individuals per block	14,009	4,881	13,782	4,731	13,811	4,789	13,757	4,678
Households with kids (percent)	24.0		22,7		22,1		23,2	
Income (percent)								
low	52.1		49.6		48.8		50.2	
average	41.5		40.8		40.6		41.0	
high	6.5		9.6		10.6		8.7	
Immigrant background (percent)	47.6		45.2		45.2		45.2	
School quality								
s_0	534.9	3.9	535.3	3.8	534.7	3.8	535.8	3.8
s_1	534.8	3.7	535.1	3.5	535.8	3.3	534.4	3.6
N	81,100		26,468		12,620		13,848	

the full sample (table 1).

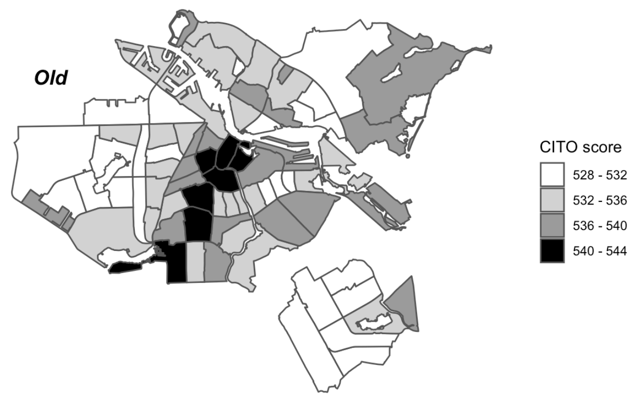
Census block data on PC4 level was unavailable for the full time period of this research. Hence, this paper uses census block data of the CBS from 2015 to account for heterogeneity in neighborhood characteristics (CBS, 2021). Although we acknowledge that neighborhoods experience gentrification or other changes over time, taking the year in the middle of the observed time span ensures that there are not many structural changes in the characteristics of the neighborhood. Furthermore, we include the previously described fixed effects on a three-digit postal code level to account for unobserved heterogeneity between neighborhoods.

4.3. Elementary schools and school quality

Amsterdam had 221 unique active elementary schools in the period between 2011 and 2019 of which between 203 and 206 were active at the same time. As mentioned previously, we use CITO scores as indicator for school quality. The mean yearly CITO score of schools in Amsterdam ranges between 532.7 and 536.8 with a full sample mean of 534.4 and standard deviation of 4.525. The CITO test has a constant mean over time, but there exist differences on a local level. Furthermore, the mean in the data is not weighed for the number of pupils per school. Generally, we computed the quality of a school in year t as the mean of the CITO scores in year t, $t-1$ and $t-2$. When no observation of year t is available, we excluded the school from the sample as this could mean that the school was not active in that year. When years $t-1$ or $t-2$ were unavailable, we did not take them into account when computing quality score, as this reflects the lack of information that parents had when choosing a school. We plotted the schools on the map using geocoding to extract the coordinates of the addresses from the internet, which we used to compute the distance between observations and schools.

Figure 3 shows the distribution of the quality of schools over houses in Amsterdam calculated on a PC4 level (Old) and based on distance (New). The plot shows that the distribution of school quality became more homogeneous after the admission policy reform.

Unfortunately for our research purposes, the CITO test is not mandatory by law, which results in not every school administering the test. Problematic in the data was that opening or closure of schools during the observed time period and not administering the test both lead to non-available observations in the set. Therefore, we did not take schools with non-observed scores into account. The unavailability of CITO scores for some schools is a limitation in the computation of the school quality scores. Although these schools do not administer the CITO test, they are part of the choice set of parents. Moreover, the popularity of the CITO test decreased after 2016. Between 2016 and 2019 the number of schools participating in the test fell from 199 (97%) to 145 (71%) in 2019. Nevertheless, the CITO test remains a widely used indicator for school quality in recent literature (Forster and Van de Werfhorst, 2020; Van Tetering et al., 2018).



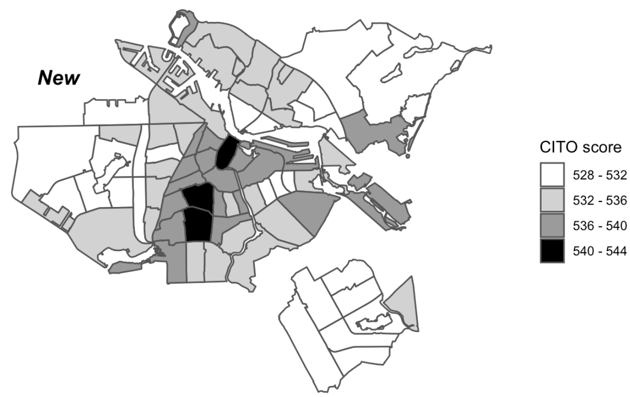


Figure 3: Mean test scores per 4-digit postal code in the "old" situation with school placement based on postal code and "new" situation with admission based on distance in the period 2011-2019.

4.4. The computation of the available school quality for households

We added two school quality variables to the observations which we computed differently. First, we computed a quality variable (s_0) based on the schools in the PC4 neighborhood of the observation. For every transaction i in year t we computed s_0 as mean of the quality of the available schools in PC4 neighborhood j in year t. For observations in a neighborhood with no schools we computed s_0 as the mean of the quality of the closest three schools measured in Euclidean distance. This reflects the choice set of parents, as the unavailability of schools in the neighborhood forces parents to apply elsewhere. s_0 reflects the situation in period 1, before 2015, when school admissions were mostly organized on a neighborhood level.

Second, we computed a quality variable which reflects the situation in period 2, after 2015. The second variable (s_1) is the mean of the school quality of the closest three schools, ignoring PC4 boundaries, measured in Euclidean distance. After 2015, the municipality and schools organized admissions centrally and priority placement is available based on the walking distance to the eight closest schools. Hence, s_1 reflects the choice set of parents after 2015. The choice for the *three* closest schools in the computation of s_1 follows from the strong parental preference for schools in the vicinity.

The difference between s_0 and s_1, Δs, reflects the effect of the policy on the available school quality for each observation. We computed the mean s_0 and s_1 for each buffer in every year and compared each observation to the mean Δs of the buffer. If the difference between the Δs of the observation and the Δs of the buffer is positive, the observation is treated. In other words, observations which benefited relatively more from the policy than average for the buffer in that specific year are treated. We dummy coded the treatment (D) with 0 for negative or no difference and 1 for a positive difference and hence local positive effect on the quality of the available schools for an observation. We then multiplied D with a treatment year dummy which equals 1 if the observation is from the treatment period, period 2, to get the treated dummy T. T equals 1 for every observation which experienced a positive effect relative to their buffer and is from period 2. Table 4 shows the differences in characteristics between the treated and control group. Houses in the control group enjoy higher school quality in the PC4 area (s_0), while the households in the treated group enjoy higher school quality in the vicinity (s_1). This proves the hypothesis that houses in the treated group are slightly deprived when it comes to quality of education before 2015. However, the differences are small and the mean price per square meter over the full period is almost equal.

Table 2
Results: The impact of treatment on the natural logarithm of house prices

Model	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Treatment group (D)	-0.234***	0.030***	0.018***	-0.001	0.000	0.006
	(0.009)	(0.009)	(0.005)	(0.004)	(0.004)	(0.004)
Treatment group post 2015 (T)	0.392***	-0.022*	0.004	0.015***	0.013**	0.013**
	(0.010)	(0.011)	(0.006)	(0.006)	(0.005)	(0.005)
Time trend	No	Yes	Yes	Yes	Yes	Yes
Housing characteristics	No	No	Yes	Yes	Yes	Yes
PC3 Dummies	No	No	No	Yes	Yes	Yes
PC3 and time trend interaction	No	No	No	No	Yes	Yes
Neighborhood variables	No	No	No	No	No	Yes
N	26,468	26,468	26,468	26,468	26,468	26,468
R^2	0.056	0.211	0.797	0.849	0.852	0.875

Standard errors in parentheses; * p<0.1, ** p<0.05, ***p<0.01

5. The impact of quality schooling on residential property prices

In this section, we run the spatial difference in difference model as described in section 3 to estimate the treatment effect on the house prices. We consider multiple models with different control variables.

5.1. Estimating the spatial difference in difference coefficient

The SDiD model builds on the distinction between the treated and control group. As previously described in section 4, the coefficients of the dummies for treatment (D), δ_0, and treatment post 2015 (T), δ_1, are the studied coefficients that could indicate a causal relation. δ_1 is the SDiD estimate which displays the difference between the treated and control group after treatment. We displayed the results of the analysis in table 2. Column 1 in table 2 shows the results of a naive model which simply explains the difference in house prices with only D and T as explanatory variables. We can derive the percentage change from the coefficients by raising e to the power of the coefficient. The coefficient for D, δ_0, is -0.234, which is strongly negative and corresponds with 20.9% lower house prices. δ_1 is strongly positive and corresponds with 48.0% higher house prices. Both coefficients are significant at a $p < 0.01$ level. The estimates are very high when compared to the findings in the literature, which can be explained by the absence of control variables and correction for time trend. Moreover, the dummies explain little variance as the model has an R^2 of 0.056

The second model, in column 2, adds year dummies, which are important explanatory variables in a model for property prices, to correct for a time trend. The sign of δ_0 flipped and is significant at $p < 0.01$, while δ_1 has a negative coefficient with low significance at $p < 0.1$. However, it is likely that there are more structural differences between the treatment and control group. This is probable when we compare the summary statistics of the two subsamples regarding housing characteristics in table 1. In the third model we add a set of control variables for housing characteristics to the hedonic pricing regression. These control variables further mitigate the difference between the treated and control group. δ_0 is significant and positive (0.030), while δ_1 loses significance.

To control for heterogeneity between neighborhoods, we add neighborhood fixed effects on a PC3 level in our fourth model, which amplifies the difference between group T and the other observations. δ_1 equals 0.018 and is positive and significant, while δ_0 loses significance. In the fifth model, we interact these fixed effects with the time trend to allow for different neighborhood fixed effects per year. δ_0 is not significant in this model, while δ_1 is 0.013, which is positive and significant at a $p < 0.05$ level. In a sixth and final model we added neighborhood variables and it hence includes all control variables. The results of model 6 are similar to the results of model 5, as δ_0 increases but remains insignificant.

As discussed in the literature review, the main issue when estimating the willingness to pay for higher quality education is the unobserved heterogeneity between neighborhoods and observations. The buffer sampling, time trend, housing characteristics, neighborhood fixed effects and neighborhood variables reduce the bias in the model and hence our preferred model is model six. Furthermore, the addition of these control variables converged the estimate to a probable value when comparing it to the findings in the literature. Hence, our study finds an insignificant coefficient of 0.006 for δ_0 and a coefficient of 0.013 for δ_1 which is significant at a $p < 0.05$ level. The preferred model has an R^2 of 0.875. We use the estimates of the preferred sixth model in column 6 to calculate the magnitude of the effect.

5.2. What is the magnitude of the effect?

The findings in table 2 report the difference between the treated and control group. More specifically, δ_0 gives the difference between the full treated and control group, while δ_1 gives the difference between these two groups after treatment. In model 6, δ_0 is insignificant, which means that there is no significant difference between the treated and control group before treatment in 2015. δ_1 is positive and significant, which shows that there is a difference between the treated group and control group after treatment. The willingness to pay for houses in the treatment group increased by 1.3%. To calculate the magnitude of the willingness to pay for education we need to compare the change in quality of education for the treated and control group. The mean of the treatment effect on the available quality of education for the treated group equals 1.300, while the effect on the control group equals -1.185. The difference in the quality of available education hence equals 2.485 points on a CITO test. Hence, we can conclude that households are willing to pay a 1.3% premium for a 2.485 increase in CITO scores at the available schools. To compare the findings to the literature we computed the premium for a standard deviation increase in school quality. As stated previously, the mean CITO score between 2009 and 2019 equals 534.4 with a standard deviation of 4.525. Therefore, the willingness to pay for a standard deviation increase on school quality equals 2.3%. We find the magnitude of the effect when we relate this premium to the mean house price of $373,480$ euros. A 2.3% premium for a standard deviation increase in school quality is equal to an extra $8,590$ euros on the mean house price.

5.3. Discussion of the results

Households are willing to pay 2.3% extra for houses with access to a standard deviation better schools. In an overview of the literature, Nguyen-Hoang and Yinger (2011) estimated the mean effect of all significant studies in the field at 4%. The findings of quasi-experimental studies span an effect of 3% to 12%. Our findings are lower than the average of the findings in the literature. There are different hypotheses that can explain this finding. A first hypothesis is that the lower findings relate to the fact that Amsterdam does not know a strict elementary school admission system with catchment zones. The policy change used in this study is a priority regulation, which only ensures a higher probability of enrollment for households in the vicinity. Studies in the United States, France and the United Kingdom uses strict catchment zones, which completely ensure enrollment. Therefore, you could expect the effect to be smaller in Amsterdam.

However, as explained in the literature review, the main issue in estimating the willingness to pay for better education is the unobserved heterogeneity between observations. The literature found that the correlation between school quality and other preferable neighborhood characteristics is strong and induces an upward bias on the results. Authors in the sample of Nguyen-Hoang and Yinger (2011) reflect critically on their findings and blame weaknesses in their methodology for high estimates. When we consider the most innovative studies, we see a pattern that differs from the mean of the literature. Using a regression discontinuity design, Black (1999) found an effect of 2.1%, which is much closer to our estimate. Clapp et al. (2008), Davidoff and Leigh (2008) and Gibbons and Machin (2003) find similar results using a regression discontinuity design. Fack and Grenet (2010) matched comparable observations in their regression discontinuity analysis and found an effect between 1.4% and 2.4%. Bayer et al. (2007) included individual household preferences in their sorting model and found an effect of 1%. In short, stronger methodologies, which implement more control variables and have more reliable identification strategies find lower estimates. Hence, our second, and preferred, hypothesis is that our study has a lower upward bias due to improvements in design. This study combines the sufficiently small buffers of Black (1999) and Fack and Grenet (2010), which reduce the bias induced by unobserved neighborhood heterogeneity, with a SDiD for finding a causal effect. Furthermore, the model controls for time trend, neighborhood fixed effects per year, and housing and neighborhood characteristics. Hence, the identification strategy is stronger than in studies applying a SDiD on policy changes in entire neighborhoods, which is a probable explanation for the low estimates. Furthermore, the quality of education is an important explanatory variable for future success and hence it is likely that Dutch parents have an equally high willingness to pay for education when compared to other countries. Hence, we argue that the willingness to pay for education is of the same order of magnitude as in other countries.

The economic implications of our findings, explained in the previous paragraph on the magnitude of the effect, are easier to interpret than the social consequences of the estimated effect and the policy. As seen in figure 3, the distribution of the quality education over the neighborhoods became more homogeneous after the new policy. However, houses that gained access to better schools became more expensive and we conclude that this effect is prevalent for all houses in Amsterdam. This could possibly induce residential segregation due to sorting and income inequality. Parents with a higher preference and willingness to pay for education will sort themselves into locations offering high quality education. Moreover, low-income parents will not be able to acquire property in the vicinity of high-quality schools due to the premium on the price. The magnitude of these effects will depend on the development of the willingness to pay for better education.

We would recommend further research in the context of Amsterdam to find more evidence for our conclusion. A sorting model, similar to the method used by Bayer et al. (2007), in the context of Amsterdam could further mitigate the upward bias as it includes individual household preferences and will probably find a lower and more accurate estimate. Furthermore, including these preferences will shine more light on the present segregating forces described in the previous paragraph. To further improve the method of this study we have some minor recommendations for future research. First, the use of Euclidean distance instead of walking distance when computing the distance between observations and schools is a limitation in this method. Natural bodies and constructions limit physical movement, as for example the rivers, canals and infrastructure in Amsterdam do (Diao et al., 2017). Although, their effect will be small as most neighborhood borders do not have such boundaries, implementing network distance, as used by Diao et al. (2017), would be a preferable measure in future research. Second, the decreasing popularity of the CITO test demands different methods for computing school quality for future research. This would enable future methods to use all available schools, including those which do not administer the CITO test. Moreover, this could entail testing the theory that test scores are correlated with other school quality indicators in Amsterdam.

6. Conclusion

The quality of elementary education has a significant effect on housing prices in Amsterdam over the period 2011 to 2019. Applying a spatial difference in difference model on a new admission policy in Amsterdam, we find that a standard deviation increase in school quality, measured in test scores, raises housing prices with 2.3%. This corresponds with a mean housing price increase of $8,590$ euros. The SDiD model includes control variables for time trend, neighborhood and housing characteristics and neighborhood fixed effects per year. Furthermore, we measured the treatment effect relative to the other observations in a small bandwidth buffer along a neighborhood boundary. We selected the treated observations based on who benefited from the policy relative to the observations in the neighborhood with similar neighborhood characteristics. In conclusion, this method controls for both observed and unobserved heterogeneity between observations and neighborhoods and hence limits the upward bias which occurs frequently in this field of research. The estimated effect of 2.3% is similar to the findings of the studies which the literature acknowledges most for reducing bias in the estimation of the effect. Furthermore, the findings in Amsterdam are similar to findings in the United States, the United Kingdom and France, which is interesting as Amsterdam knows no strict school district boundaries. The willingness to pay for better education can have social implications. Educational and residential segregation will depend on the development of the parental willingness to pay for education and its capitalization in the housing prices in Amsterdam.

Acknowledgements

First, I wish to thank my supervisor Thomas de Graaff for his support and feedback on my work. Even though we have not met in person due to the pandemic, our online meetings were very helpful. Second, I wish to thank the NVM who provided data on the Amsterdam housing market free of charge. Third, I wish to thank my roommate João for his semester long performance as "rubber duck". Although he is not an economist, his presence was very helpful for organizing my thoughts.

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