Defying Attendance Boundary Policies and the Limits to Combating School Segregation*

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Abstract

A common policy to affect student composition is to redraw school attendance boundaries. Yet redrawing only works if households comply and enroll in the designated school. Employing a novel dataset with unprecedented detail, we exploit changes over time in schools' geographic attendance boundaries to provide causal estimates of how school characteristics affect compliance with the assigned school. Households defy reassignments to schools with children from less resourceful families by enrolling in other public schools. The response to changes in school composition has a strong social gradient: resourceful households respond more to changes in school composition. We apply a boundary discontinuity design to characterize non-compliance through private school enrollment and residential relocation in the long term and once again document a strong social gradient. Our findings imply that attendance boundary policies have limited scope for desegregating schools.

1 Introduction

Policies aimed at reducing segregation in residential areas or educational institutions are common in many countries. Equalization of schools can, for instance, be

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achieved by physically moving people around, as with the Moving To Opportunity program, or changing the assignment of pupils to schools.[1] Assignment by school attendance boundaries (SABs), which geographically delineate school enrollment, is the most common system to assign pupils to primary schools; also known as catchment zones in the US and school districts elsewhere.[2] Local authorities can redraw geographic attendance boundaries of schools and thereby manipulate the composition of households who are eligible to enroll in certain schools. However, households may have other options than the designated school and therefore can choose not to comply. We document that Danish households exploit at least three different options to avoid enrollment into certain schools: they relocate, choose a private school, and make use of opportunities to enroll in other public schools. Along all these three margins of opting out, we find a social gradient. Well-off families react strongly to differences in school composition while marginalized families do not. The responses are large and affect the resulting peer composition of public primary schools. Redistricting will therefore not lead to the intended equalizing of student compositions across schools.

To identify household responses to school characteristics, we exploit a new dataset with geographic information on school boundaries and household residential locations for the universe of Danish children during the years 2008-2015. Attendance boundaries change over time, providing plausibly exogenous variation in school assignment. We use these changes to compare households who were originally designated the same school prior to a change in boundaries but different schools after. During our observed period, a total of 191 schools, 17 percent of all schools, have parts of their district reassigned to another school. We analyze changes to schools' socioeconomic status (SES). When the new designated school has lower average SES, the enrollment in the new school drops: a one standard deviation drop in average SES implies that the compliance rate falls by 20 percent. The behavioral responses to changes in SES are not symmetric: on the one hand, a large, significant drop follows a fall in school SES in compliance, on the other hand,

[1]In the US laws against segregation of schools has been mandated since the Brown vs. Board case of 1954 in the US Supreme Court, see Baum-Snow and Lutz (2011). Non-discriminatory laws for housing in the US were enacted in the Civil Rights Act of 1968, see Yinger (1986). Chetty et al. (2016) investigate long-term effects of neighborhoods and reinvestigates the Moving to Opportunity program.

[2]See Monarrez (2017) on US school systems. Almost all Danish municipalities allocate students by districts. We use the term SAB and district interchangeably throughout.

there is only weak evidence of higher compliance when being assigned to a school with higher SES.

We provide evidence of a strong social gradient: the response of the highest quartile is 2.5 times that of the lowest. These differences imply that the distribution of students who end up in the school depends on the initial school SES. A fall in school SES of respectively 1, 2 and 3 standard deviations compared to the originally intended school implies a drop in average SES of pupils arriving in the new school of respectively 4, 11 and 25 percent, where we assume the SES for shifted households is drawn from the population SES distribution.

We find that changes to the ethnic composition are also of importance for enrollment. The responses closely mirror those estimated using SES. Due to a high correlation between ethnic shares and average SES at the school level, we argue that, in a Danish context, we cannot meaningfully separate ethnicity and socioeconomic factors. Furthermore, we conjecture that households themselves might struggle to disentangle the socioeconomic composition from ethnicity. We are therefore unable to disentangle preferences from statistical discrimination. We finally analyze the importance of a public school-value added measure and find only weak effects on enrollment decisions.

The most common option that households use to avoid the new designated school is access to other public schools - an option provided free-of-charge through an opaque, decentralized process, which may be unfair to some households. When we identify the effects of changes to school districts, we only measure responses in the short term. Options for enrolling in a private school or relocating may, however, be limited in the short term.[3] Therefore, we turn to an auxiliary strategy to investigate long-term responses: *boundary discontinuity design* (BDD). This approach analyzes discontinuities in enrollment and relocation for households living near the administrative border between schools. By comparing school borders, BDD captures long-term responses beyond the immediate reaction to changes in school boundaries. Using BDD, we show increased differences in non-compliance through

[3]Using changes, we find that households do not respond along the private school margin in the short term. We speculate that the low substitution to private schools may be due to a low supply of private schools combined with the fact that they often work through waiting lists, requiring households to apply years in advance; these two facts would narrow the feasible set of private schools. Likewise, relocating quickly can be expensive and infeasible (e.g. due to commuting and financial constraints) for households.

both enrollment in private and other public schools when differences in SES increases. Private schools account for approximately one third of non-compliance. Again, we document a strong social gradient. The marginal propensity to choose a private school is approximately five times larger for high-SES households compared to low-SES households. We also demonstrate that high-SES households are much more likely to move out of districts with low socioeconomic status before school age of children.

Our findings generalize to other contexts beyond Denmark for two important reasons. First, Denmark is a relatively homogeneous country with low inequality and high social cohesion. Therefore, less cohesive societies should find larger responses to changes in attendance boundaries. Second, in our sample there is *not* a positive association between school SES and school funding.[4] In a context where school finance is positively associated with school-SES, such as the U.S., there would be added incentive to sort and our estimated behavioral response would, therefore, reflect a lower bound.

Our results imply that policies that redraw school boundaries lead to systematic defiance. Forcing students into poorer districts means that a higher number of resourceful students never arrive in the designated school or possibly abandon the public school system entirely. We note that policies aimed at redistributing skills and/or opportunities by altering the structure of social interactions operate under the assumption that more resourceful peers increase one's own chances and performance (Sacerdote, 2011). Therefore, these policies, which Durlauf (1996b) refer to as 'associational redistribution', are likely to face similar behavioral responses to those that we document. Consequently, associational redistribution policies must consider the willingness to participate and the outside options of those affected.

The investigation of school assignment and compliance dates back to Coleman et al. (1966), who defined the relocation of white people from urban to suburban areas as "white flight". Subsequent work has sought to measure out-group avoidance in school enrollment (Rossell, 1975; Saporito and Sohoni, 2007; Rangvid, 2009; Bifulco et al., 2009; Riedel et al., 2010; Baum-Snow and Lutz, 2011). Our findings are consistent with their findings in that households tend to avoid schools where

[4]We compare public schools within the same administrative unit, i.e. a Danish municipality, where one funding scheme applies to all schools. If anything, schools with lower SES are compensated to have more resources.

the ethnic composition of students differ from themselves. Papers in this literature, however, generally lack clear identification strategies to handle residential sorting. One exception is Baum-Snow and Lutz (2011), who identify responses in public school enrollment to desegregation using variation in timing of court orders.

There are several alternatives to SABs for allocating children to schools. One approach is matching mechanisms studied in a large and expanding literature, following the seminal work of Abdulkadiroğlu and Sönmez (2003).[5] Researchers have used applicant priorities over schools from truth-revealing assignment mechanism to estimate preferences for schools (Hastings et al., 2009; Burgess et al., 2015; Borghans et al., 2015; Abdulkadiroğlu et al., 2017).[6] The general findings relevant for our analysis are that households prefer schools that are closer to home, schools with better test performance and schools with higher average socioeconomic status (or a proxy thereof). The preferences for quality and socioeconomic composition are generally found to be increasing in households' own socioeconomic background. Our results closely mirror these findings, even though we investigate a completely different institutional setting. We complement the literature by considering new options for enrollment, as we consider private school enrollment and location decisions and show these to be relevant. By doing so, we demonstrate the importance of assignment loopholes and institutions outside the public school system, notably private schools and residential relocation, for student sorting; factors often overlooked in the school choice literature.

Our spatial identification approach is inspired by the hedonic pricing literature. A large literature employs a BDD approach to identify the effect of school characteristics on house prices Black 1999; Bayer et al. 2007; Fack and Grenet 2010; Black and Devereux 2011; Gibbons et al. 2013; Imberman and Lovenheim 2016. These studies find evidence that neighborhood composition as well as school composition and test scores affect prices. Imberman and Lovenheim (2016) show how the publicity of school performance information impacts house prices; they find no effects from school value added when controlling for peer characteristics. Our paper con-

[5]The innovation in matching mechanisms is that they remove gains to strategically manipulate the assignment by submitting false preferences. These mechanisms, often known as strategy-proof, provide incentives for submission of ranking over schools that does not violate ones' true preferences.

[6]A research literature on school preferences use surveys but it has largely been dismissed due to possible bias in reporting and a failure to account for different choice sets available to parents (Burgess et al., 2011).

tributes to the understanding of BDD by showing that using difference-in-difference yields similar estimates.

The paper proceeds as follows. Section 2 gives an overview of the institutional context of primary schools in Denmark. We present our identification strategies in Section 3. We describe our data in Section 4. We perform our main empirical analysis exploiting changes to school districts over time in Section 5; this is followed by our auxiliary approach using border comparisons in Section 6. Finally, Section 7 concludes.

2 Institutional background

We begin by describing the Danish primary school system and the options available to households with school-age children. Danish children usually start primary school in the summer of the year they turn six. The first year is grade 0, which has been mandatory since 2009. Primary school runs until grade 9 although it is not uncommon for schools to specialize in grade 0 through 6.

Municipalities who decide on the level of funding administer public schools. Public schools are free and parent co-payment is forbidden by law. The law governing Danish municipalities seeks to ensure Tiebout-competition on taxes and services but with extensive transfers between municipalities to combat inequality in funds attributable to differences in population composition. Local tax revenue, therefore, does not completely determine available funds for schooling.

Each municipality decides on the number of schools and the district boundaries.[7] Every residential address is associated with exactly one school, which we refer to as the *district* school. Children have a right to be admitted to the district school associated with their place of residence and once a child is enrolled she is not affected by future district changes (disregarding mergers and closures).[8] The municipal council is free to choose its priorities when constructing the districts. Changes in districts are common and some municipalities use redistricting as a way to manipulate student body characteristics, especially in large urban areas such as the Copenhagen

[7]Each district has one school; thus there is no distinction between catchment areas and school districts in Denmark.

[8]In recent years some municipalities have merged smaller schools into one organizational unit to lower costs.

metropolitan area, Aarhus and Odense.[9] Parents have a right to have their child admitted to other schools than the district school if the desired school has enough capacity. This is usually defined by a cap on class size and the total number of pupils in a school.[10] Children are free to change school during the school year. This creates the possibility that an initially oversubscribed school may become accessible for outside-district children if a family moves away.[11]

Private schools receive funds from the government covering on average 75 percent of their costs, while parents cover the rest. This is similar to voucher schools in the US education system. Danish private schools are free to set their own price, and a typical monthly fee will be around 130-270 euros a month per child with discounts for siblings.[12] These schools are free to choose who to admit and thus have no SAB. Popular private schools have waiting lists and parents can sign their children up for these very early in the life of their child. Parents, therefore, cannot be sure to exploit private schools as outside options as this is contingent on being on a waiting list and being admitted. Anecdotal evidence suggests that private schools, especially in urban areas, tend to be vastly oversubscribed.

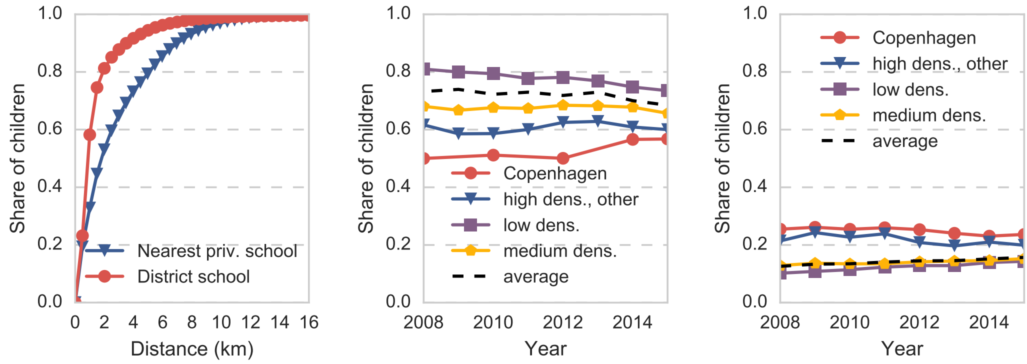
The supply of private schools in Denmark is evenly distributed geographically and most areas have a private school nearby. The distribution of distance to nearest private school in Figure 1a shows that around 42 pct. of children aged 7 have a private school within 2 km and 85 pct. have one within 10 km. The overall enrollment in private schools increased from around 12.5 pct. in 2007 to 16.4 in

[9]We interviewed the responsible administrators in the municipality of Copenhagen and consulted administrative texts to verify this to be the case.

[10]The municipality can delegate to the school principal the authority to suspend the right of outside-district children to be admitted to a certain class or year in a school. Generally, a school class must not exceed 28 at the beginning of the school year, although under special circumstances the municipality council can allow classes to reach a maximum of 30. The municipality can decide on a separate class size limit for which pupils from outside the school district can no longer enter. If a school receives more applications than its capacity the children outside the district should be admitted according to objective criteria. The Danish Ministry of education recommends distance and sibling preference as such criteria. See guidelines on the Danish school choice system https://www.uvm.dk/folkeskolen/fag-timetal-og-overgange/skolestart-og-boernehaveklassen/frit-skolevalg

[11]No centralized mechanism exists for the transitions between schools and the chance of admission depends on the timing of the request to move. It is, therefore, possible for parents to increase the opportunities for admission by repeatedly contacting the desired school. We cannot follow this process in our data.

[12]We have been unable to locate a central registry of prices, and our estimates are therefore based on data collected on the webpage of the private schools association; `https://privateskoler.dk/skolerne/liste-over-skolerne`.



(a) Distance to school (b) Share in district school (c) Share in private school

Figure 1: School distance and enrollment

The figures depict various statistics for distance and enrollment. Figure 1a shows the cumulative distribution of distance to district school and nearest private school. Figures 1b and 1c plots the annual share of children enrolled respectively in the district school and in a private school. The sample consist of all children at the age 7 between 2008 and 2015. For enrollment, the density measures are: low density, less than 1000 per sq. km; medium density, between 1000 and 5000 per sq. km, and; high density, more than 5000 per sq. km.

2016. Figures 1b and 1c shows a breakdown of school enrollment in the district school and in private school by population density. As is evident from these figures, urban density is an important determinant of enrollment. Enrollment in the district school is around 75 percent nationally but as 50 percent for the Copenhagen area, which is reflected in a corresponding larger private school enrollment compared to the national average.

3 Methods

In this section, we present our approach to identify households' compliance behavior as a function of their district school. We begin by defining the options available for the households to opt-out of the school assignment. We then proceed to discuss challenges to the identification of behavioral effects and our econometric approaches. We finish of with an empirical example of how our strategies are implemented in practice.

Options of households: A household with a child ready to enroll in primary school has several options when deciding on the school in which to enroll. The household may simply choose to enroll the child in the district school, thereby complying with the assignment mechanism. We describe this by a binary variable, denoted $comply$. This option is guaranteed by law and therefore always available. If the district school is deemed unattractive households may choose different ways of opting out. We group these responses into three binary variables: move to another district ($move$); enrolling in another public school ($othpub$) and enrolling in a private school ($priv$). Summing gives us the following identity:

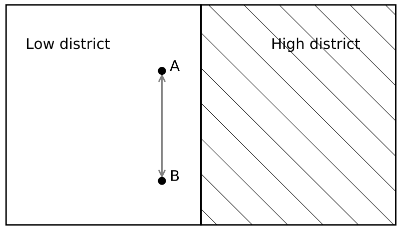
$$comply = 1 - (move + othpub + priv), \tag{1}$$

where it is implicit that we regard movers as one, regardless of whether they end up attending the district school in their new location. Each of the elements in (1) correspond to dependent variables and we use this identity to decompose responses.

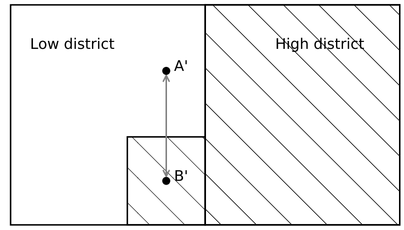
To identify household responses, we take a reduced-form approach. This implies that we do not model preferences and costs explicitly. Instead, a child's school enrollment can be seen as revealed preferences of the parents, i.e. opting into the district school or not. In order for a child to enroll in a private school a number of conditions have to be met: 1) households must prefer the private school over the associated district school; 2) the child must meet admission criteria, and; 3) the household must be able to afford enrollment. Conditions 1 and 2 also apply when choosing other public schools. As a consequence we can be sure that whenever we observe a child enrolled in a non-district school, this school must be preferred to the district school.[13] We do, however, not observe those that would want to escape the district school but are unable to, either due to financial constraints, oversubscription or inability to transport oneself. Whenever we refer to our estimates as reflecting preferences, it, therefore, comes with the caveat that preferences cannot properly be separated from costs and constraints.

In order to identify household responses to school characteristics, we need to address the fact that households do not choose their residential location randomly. This implies that there is sorting by households characteristics across the school boundaries. Some of these characteristics are observable and possible to control for.

[13] There may in practice be cases where a non-conforming child is urged on by authorities to move to another public school. We do not expect these cases to influence our results.



(a) Comparison before change in attendance boundaries



(b) Comparison after change in attendance boundaries

Figure 2: Illustration of main identification strategy

The two figures visualizes the main approach of identifying household responses to variation in school districts. Both figures illustrate two bordering school districts where the left-most district exhibit a 'low' measure of district school characteristic. From Figure 2a we see that before the redrawing of boundaries both household A and B are in the 'low' district. After the change, the area where household B lives is reassigned from the 'low' to the 'high' school, see Figure 2b. Our method is to measure the differences in the actions of households A' and B' to differences between households A and B.

Yet, other characteristics are not observed and may pose a threat to the identification of behavioral responses to school characteristics. To address these concerns, we rely on two approaches which we describe below.

3.1 Main strategy: Differences-in-Differences (DiD)

Our main approach exploits quasi-exogenous redrawing of school boundaries in a Differences-in-Differences (DiD) setup. We use the fact that the redrawn school boundaries imply a different assignment of households to schools. Our method is to compare households, who used to live within the same attendance boundary but after reassignment differ in school association. We measure the enrollment difference between these two groups before and after the reassignment to schools.

We explain the method with an example. Imagine two households, A and B, at year -1. As depicted in Figure 2a the two households have chosen to locate in the same SAB. The two households may differ and therefore may make different choices with regards to school enrollment. At year 0 the municipality chooses to redraw the district borders as depicted in Figure 2b. This will not affect household A and B as they have already enrolled their child. But their neighbors A' and B' have younger children and are therefore affected by the redistricting. If the change is unexpected we may assume that, in absence of the redistricting, the behavior of household A would have been comparable to household A' and likewise for B to B' (except for a common trend). Under this assumption, we can identify the behavioral change in outcomes as $E[y_{B'} - y_{A'}] - E[y_B - y_A]$, which is a standard difference in difference estimator.

We note that by defining our measure of interest as a function of school characteristics we are implicitly assuming a time profile of mechanisms linking school characteristics to household behavior. We assume that school characteristics appear *earlier* in a causal chain than other factors such as real estate prices and allocated school resources. An example; assume household respond to a wealth effect from rising prices due to a boundary change. In order for our estimates of marginal effects to be valid, we need to assume that this wealth effect is due to the change in school characteristics. In this setting, the wealth effect is, therefore, a mechanism for the causal link between school characteristics and household school choice.

We limit our analysis to addresses being shifted between active schools and thus we do not use school closures. We do this in order to avoid a common pattern in rural areas where private schools close (and then local communities reopen the school as a private school in the same location).[14] Therefore, we do not want to attribute the household response from school closures to school characteristics.

A drawback of the DiD-approach is that the estimates only capture short-term responses after redrawing boundaries. Households may, however, be restricted in their options in the short term. If a nearby private school is oversubscribed it will not be an option for parents who want to enroll their child with short notice. Likewise, moving can be a lengthy and costly process and parents may therefore not react immediately. We therefore complement our main approach with an auxiliary strategy to investigate long-term responses to district school characteristics.

3.2 Auxiliary strategy: border discontinuity design (BDD)

We move on to describe our auxiliary approach where we compare neighbors associated with different schools. We rely on Boundary Discontinuity Design (BDD), a method first proposed by Black (1999), to evaluate how housing prices reflect

[14] An example of this dynamic can be seen here (in Danish): `https://www.folkeskolen.dk/18759/kommune-tjener-paa-at-foraeldre-aabner-friskole`.

school characteristics. Figure 3 provides an example of our approach. Household A and household C live very close to each other but on either side of an attendance boundary. The local school is not the only consideration when choosing a location of residence, other factors play a role such as access to labor markets, local amenities and location of relatives. Assume that these local factors do not depend on the characteristics of the associated school. We can then compare A and C to elicit when and how households choose to opt-out of the district school as a function of district school characteristics. By our assumptions, we attribute any discontinuous difference in probability of opting out to a difference in school characteristics.[15]

The BDD approach is static in the sense that we do not exploit inter-temporal variation in boundaries and location. This is, therefore, best seen as a description of long-term (i.e. steady state) behavior. As noted we can therefore expect the options by which households can opt-out to be less restricted. Compared to our main approach using changes in boundaries over time, the BDD design puts fewer restrictions on which data points can be used for computing responses and therefore increases the statistical power which allows us to estimate heterogeneity in responses. This increased power, however, is a function of more restrictive assumptions; household A and C should indeed only differ in that they are assigned to two different district schools.

If this assumption is not valid we see two general narratives by which our estimates might be biased. Assume that households who value school quality for unobserved reasons anticipate the schools' characteristics and never locate within an attendance boundary for a low-quality school. By comparing households across boundaries, the households on the side with lower quality will exhibit lower preferences for good schools than those households who chose to locate on the high side. The missing counter-factual households will lead to an underestimation of behavioral responses in the BDD-approach. This effect may, however, be counteracted. There is ample evidence that house prices correlate positively with school quality. See Gibbons et al. (2013) for a brief review. Bjerre-Nielsen and Gandil (2018a) provide evidence of such dynamics in a Danish context. Assume that some households have strong preferences for private school. These households can get a house for

[15]The boundary discontinuity design is very similar to regression discontinuity design. The difference is that in geographic space we have two running variables, a Northern and an Eastern coordinate. As the coordinates have the same scale they are easily projected onto a line as the Euclidian distance to the border. In other words, two running variables are collapsed into one.

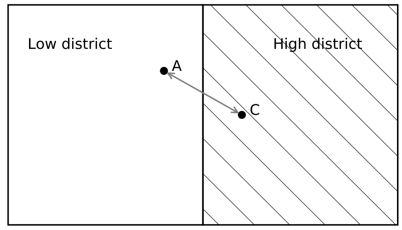


Figure 3: Illustration of auxiliary approach: Boundary Discontinuity Design

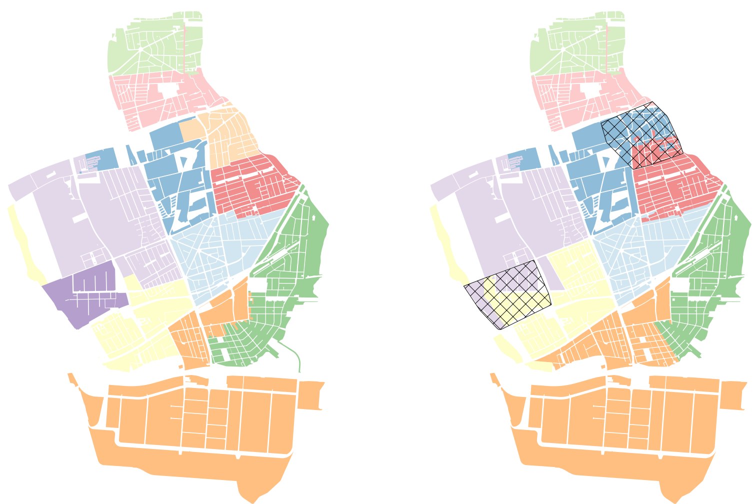
The figure displays our auxiliary approach which compares households' opt-out responses in two bordering school districts. In this figure, the left-most district has a 'low' measure of school characteristic compared to the right-most. In the figure, household A lives on the 'low' side while household C lives on the 'high' side. This approach exploits the discontinuous difference in assigned school characteristics at the boundary to see how they relate to enrollment choices.

a lower price in the less attractive school district. But in the counter-factual case, where these households were living in a better district, they would still choose private school. This would have the inverse effect, that is, we would overestimate the behavioral response when comparing households across borders. We can therefore not sign the possible bias.

3.3 Empirical example

In this final sub-section, we explain with a brief example, how we identify behavioral responses in a real institutional setting. The context is Hvidovre, a municipality in the Copenhagen metropolitan area, with around fifty thousand residents. The two maps in Figure 4 illustrates how the SABs were redrawn between 2011 and 2012 where two public schools closed down.[16] To allocate the children living within the attendance boundary of the now-closed schools, other boundaries needed to be changed. This implies two kinds of variation. Firstly, households who thought their children would attend a school which by then was closed had to attend another school. These are the households with children who in 2012 lived in the hatched areas of Figure 4b. Secondly, other households were reassigned to new schools that they had not expected although their originally designated school did not close. An

[16]The two closed schools are Sønderkærskolen, the orange district in the North-East, and Enghøjskolen, the Western dark purple district.



(a) School districts, 2011 (b) School districts, 2012

Figure 4: District changes in the municipality of Hvidovre

The figures depict the school districts in the municipality of Hvidovre in the autumn of 2011 and 2012. The hatched areas in 2012 show the convex hull of two closed schools. In order to enroll students who would live in districts of the now-closed schools, a range of other changes was made. Only the latter changes are used for identification. See section 4 for a description of the district data. Some areas differ from the official documentation. These areas are mostly not populated but some measurement error occurs. The map is constructed by merging addresses on to official geodata. In the analysis, we use addresses directly to bypass mismeasurement of geographical entities. The district polygons are only used to measure distances to borders.

example of this is the change from the green to the orange for some areas in the South. We only use this latter source of variation in our Difference-in-Difference analysis. In the auxiliary approach, BDD, we exploit discontinuities in school characteristics at boundaries in both Figures 4a and 4b. Importantly we only compare within municipalities, which ensure equal levels of taxation and overall school funding.

4 Data and measurement

Our sample is based on Danish registry data for year 2008-2015.[17] From Statistics Denmark we obtain detailed information on household income, education ethnicity, educational enrollment and test scores. We link these records to a detailed geographical information on over 95 percent of households in Denmark.[18] We link this data with school district data obtained from records in the CPR-vej-register. These are reported by the municipalities themselves and are not verified by Statistics Denmark. We clean the district data and merge them unto the place of residence of households in the registers.[19] We calculate the distance to the boundary from the centroid of geographical polygons, in which the household lives.

Our data is sampled from all children who are observed at age 5 and enrolled in primary school at age 7 during our sample period (irrespective of siblings, preschool institution choice etc.). We focus on 7-year-old children as this age captures the earliest point in time by which we expect all children to have enrolled in primary school (some parents defer enrollment until their children turn 7).

We use data on outcomes, i.e. enrollment of the child and residential location of the household, for the year the child turns 7. We require observations at age 5 in order to measure school characteristics and household covariates before the children possibly experience changes to their school district and/or enroll in primary school.

For ease of interpretation, we construct a socioeconomic index (henceforth, SES). We define this as the first component from a principal component analysis (PCA) on income rank, an employment dummy and dummies for long-cycle education. We then rank the resulting indicator such that it is uniformly distributed and bounded on the unit-interval. Our socioeconomic index increases with income, employment and high cycle education as expected. Appendix A.1 describes this SES-index in detail. Ethnicity is not part of the PCA analysis and is investigated

[17]The data goes further back but the data quality on addresses, and thus geographic data, suffers from a break in 2007 when Denmark implemented a large reform of municipalities.

[18]We have constructed a set of polygons such that k-anonymity of the households is maintained, see Bjerre-Nielsen and Gandil (2018b) for details. Software is available at GitHub; `https://github.com/abjer/private_spatial_dk`

[19]For manipulation of data we have made extensive use of open-source Python libraries. Among others we have used Pandas, Scipy, Scikit-learn and NetworkX for data structuring, see McKinney (2010); Jones et al. (2001–); Pedregosa et al. (2012); Hagberg et al. (2008); GeoPandas and Shapely for GIS-data manipulation, see Gillies et al. (2007–); Matplotlib, Statsmodels for respectively plotting and regression models, see Hunter (2007); Seabold and Perktold (2010).

	N	mean	median	std	binary
Socioeconomic status [SES]	578,903	0.50	0.50	0.29	N
Employment parents, min. [EMP]	581,457	0.74	1.00	0.44	N
Income rank parents, max. [INC]	581,449	0.62	0.67		Y
Non-western [NW]	581,457	0.13	0.00		Y
High cycle educ. parents, max. [HCU]	578,903	0.19	0.00		Y
No educ. after prim. school, min. [NE]	578,903	0.07	0.00		Y
Number of parents	581,457	1.82	2.00		Y
Housing contract: rental	581,457	0.21	0.00		Y
Housing contract: coop.	581,457	0.04	0.00		Y

Table 1: Descriptive statistics for children and their households

The table presents the mean, std. deviation and count of observations for variables that we employ in the analysis as covariates for matching, for modelling or in to compute the SES index (see Appendix A.1).

separately. We measure ethnic background with a dummy for being a non-Western immigrant, descendant or child of descendants (up to the third generation).[20]

We focus on three school measures: ethnic composition measured as the share of non-Western immigrants (abbreviated NW); average socioeconomic status (SES), and; school value added (SVA). We measure both NW and SES in the data. We calculate averages of all students enrolled in a school for each year. For SVA, however, we make use of official measures calculated by the Danish Ministry of Education. SVA is calculated using a version of the empirical Bayes estimator. The outcome is (uncentered) grades in the final exam in grade 9, which corresponds of the final year of secondary school. The measure is calculated every year for new cohorts. The controls do not include pre-school test scores and therefore may suffer considerably from omitted variable bias. Furthermore, urbanization is not taken into account and we suspect the presence of substantial unobserved sorting. The measure is volatile with a year-on-year correlation in subject-institution value added of 0.3. We use SVA measured on grade averages as our measure. Importantly, these measures are available publicly on the Ministry website and are therefore plausibly part of the information set of parents. This also allows us to ignore measurement errors of the SVA in our estimations, as we take these as given from the point of view of households.

[20]In order to be a descendant both parents must be non-Danish. Same goes for children of descendants. Thus, one Danish parent is sufficient to be of Danish descent.

	# obs.	Mean	Std. err.
Non-Western share	9332	0.09	0.13
SES index (average)	9332	0.47	0.10
School Value Added	3714	0.04	0.34

Table 2: School descriptives

The table presents a descriptive statistics for schools, where each school is represented once per year.

An important factor for the choice of school is the geographical distance (see Abdulkadiroğlu et al. (2017) for an example.) We calculate Euclidean distances from the place of residence (centroid of resident-polygon) when the child is five years old to the original and the new school and take the difference. When a district changes, the distance changes differently for each household depending on the place of residence. Therefore, contrary to our other school measures we have more variation in distance changes than in our other school measures, which are the same for all households in the district. Descriptive statistics for schools are presented in Table 2.

5 Main approach: Changes in attendance boundaries

We now move on to the analysis of the behavioral responses to local school characteristics.

The administrative procedures of changing attendance boundaries differ between municipalities. The changes are usually announced no more than a year before they occur, usually in the spring before the school years beginning in August.[21] Proposals for changes are usually made by administrative staff at city hall and are subject to the confirmation by the city council. The changes most often occur due to changing demographics, which induce shifts in the demand for primary schooling.[22] For all residential locations, we record changes in formal school

[21]We have interviewed responsible authorities in the municipality of Copenhagen as well as gone through public documents from other municipalities to understand the process.

[22]When a proposal of a change is made citizens and schools may voice concerns, which sometimes turns into heightened local political tension. Anecdotal evidence suggests this is mostly due to the closing of schools as opposed to tiny changes around borders. Therefore, enacted changes in this context might not be completely random as some areas are likely more difficult politically to manipulate due to a politically strong citizenry. In order to ensure exogeneity, we focus on transfers between existing schools. We exclude mergers and closings of schools by requiring that both schools involved in an exchange exist before and after.

affiliation and the year the change occurs. We restrict our attention to addresses, which experience a single change or no change in our data. For each address, which experiences a change in affiliation, we calculate the time span in years between the current year and the year of the change. We record the outcome of children the year they turn 7. We then find the address of these children at age 5 and merge it onto the attendance boundary data. A temporal difference to the SAB-change of zero implies that the change occurs between the ages of 6 and 7 of the child. A distance of 1 means that the change occurs at the ages of 5 and 6. We exclude all attendance boundaries wherein no address is shifted at any point in our data.

5.1 Positive and negative shocks

We estimate household responses to changes in school affiliation in a difference-in-difference framework. To capture the effect from characteristic changes separate from "pure" effect from a surprise change we construct a treatment indicator for all addresses, which are shifted. We categorize the school changes into three groups depending on the change in district school characteristics; those experiencing a positive, negative and a "negligible" change. The latter category is taken as reference. We estimate the responses using OLS specifications of the following kind:

$$\begin{aligned} Y_{iast} &= \alpha_T T_a + \alpha_- T_a^- + \alpha_+ T_a^+ \\ &\quad + \sum_{k=-4, k\neq -1}^{4} \Big[(\beta_T^k T_a + \beta_-^k T_a^- + \beta_+^k T_a^+) \times K_{iat} \Big] + \mu_{st} + \varepsilon_{iast}, \end{aligned} \tag{2}$$

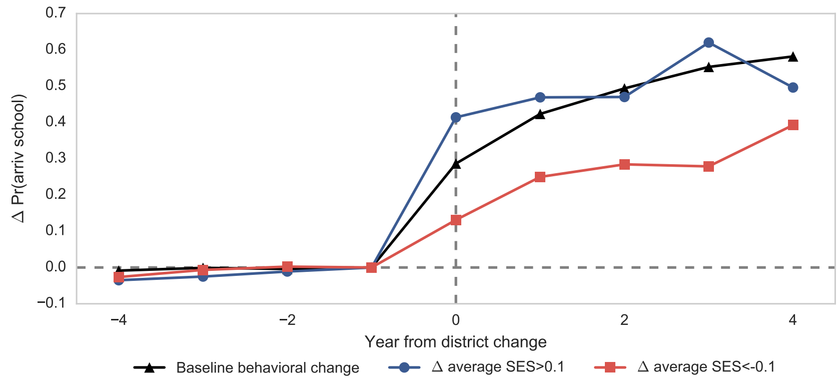
where Y_{its} is the outcome of interest for child i aged 7 at time t living at address a at the age of 5 in original SAB s. Define the dummy variables for the positive and negative treatment as well as a general treatment indicator, respectively denoted T_a^- and T_a^+ and T_a. K_{iat} is a set of dummies for the time gap between the change of association and the year the child outcome is measured. We center our estimates at the year before the association change. We are interested in comparing differences across addresses within the same year which share the old school association and implement this by including a fixed effect for the original SAB s at time t. The effects of interest are the coefficient on the interactions; β_T^k, β_-^k and β_+^k, where coefficients for $k < 0$ serve as placebo tests.

The specification in Equation (2) implies that β_-^k and β_+^k are interaction terms. In other words, the parameters describe how the mean effect of a change in SAB is affected by the change in characteristics. As mentioned we have multiple measures of school characteristics. In this section, we present our graphic results from using average SES as our school measure. We perform the same analysis for changes to share of non-Western heritage and school value-added in Appendix B.

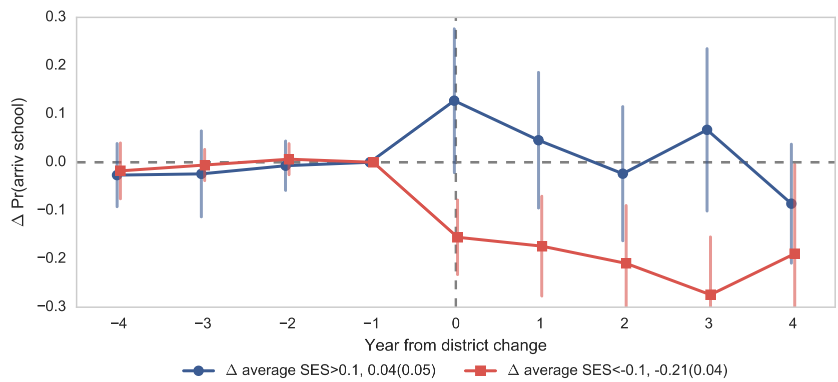
We begin by estimating our model for enrolling in the assigned school after a change in school SAB.[23] The black line in Figure (5a) depicts the change in the probability of enrolling in the new school as a function of time from the SAB change. The probability is compared to the households from the same original SAB, but who were not transferred. If redrawing attendance boundaries is an effective instrument, the probability of enrolling in a newly assigned school should rise discontinuously at the time of change. This is clearly the case. The probability of enrolling increases by almost 30 percentage points the first year and continues to rise to around 50 percentage point, which is well within the rage of common enrollment rates. The red and blue lines depict the enrollment when the change in average SES is numerically larger than 0.1, representing a standard deviation of the school level SES distribution. If households experience a positive change in school-level SES of more than a standard deviation, the compliance rate rises by around 10 percentage points in the first year, which however is insignificant as seen figure 5b. Four years after the change compliance rate converges to the baseline, which is most likely due to the sorting over time as incoming families with children know the new school association in later years. If, on the other hand, the average SES *falls* by a standard deviation the average compliance over the years is 21 percentage points lower than average enrollment rate and does not seem to converge over time. This implies that there are lasting falls in compliance for areas where the newly assigned school has a weaker socioeconomic composition. Together with flat pre-trends, this provides evidence of a causal relationship between school characteristics and compliance with the school assignment mechanism.

Larger changes cause larger responses The change of +/- 0.1 in average SES is somewhat arbitrary. We may expect that larger changes will lead to larger re-

[23]For the non-shifted addresses within a SAB, we assign arrival school as the arrival school of those that are shifted. A few SAB experience exchanges between multiple other SABs. In these cases, we assign the closest possible arrival school to the non-shifted addresses.



(a) Change in probability or enrolling in new school



(b) Excess change in probability or enrolling in new school as a function of change in SES

Figure 5: Compliance response to change in SAB by school characteristic change

Figure 5a display changes in estimated compliance rates based on the model in (2). The black lines depict the estimated β_T^ks, while the blue and red line depict $\beta_T^k+\beta_-^k$ and $\beta_T^k+\beta_+^k$ respectively. Figure 5b displays the interaction terms, β_-^k and β_+^k, along with 95-percent confidence intervals. The parameters represent the difference in the likelihood of enrolling in the new district school, when the average SES at a school level changes, relative to the average arrival probability following a district change. The dependent variable is binary and equals one if the child is enrolled in the district school at age 7 based on the district at age 7 for the address at age 5. The y-axis denotes the excess probability of enrolling relative to baseline. The model is estimated with "origin-SAB"-year fixed effects. Standard errors are clustered on origin SAB level. Results are centered at the year before the SAB-change. Estimates from a simple before-after-DID are reported in the legends of figure 5b.

sponses.[24] To investigate this aspect, we collapse our regression into the following:

$$\begin{aligned} Y_{iast} &= \alpha_T T_a + \alpha_- T_a^- + \alpha_+ T_a^+ \\ &\quad + \left(\beta_T T_a + \beta_- T_a^- + \beta_+ T_a^+\right) \times Post_{at} + \mu_{st} + \varepsilon_{iast}, \end{aligned} \tag{3}$$

[24]This would be the case if the marginal response to school quality was constant.

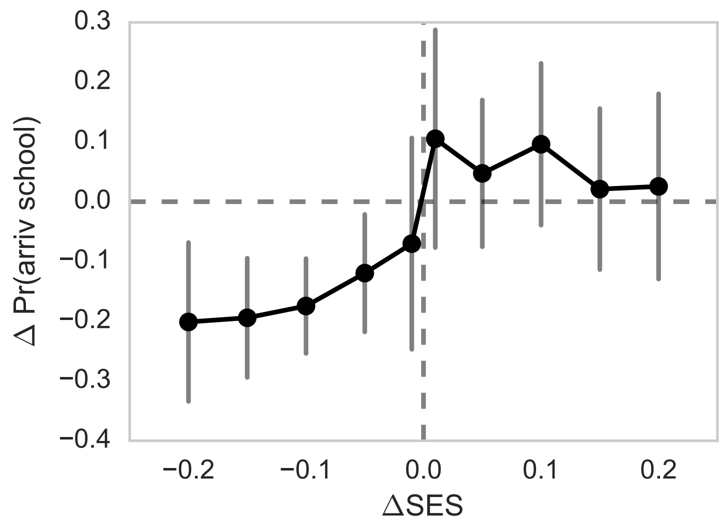


Figure 6: DID-estimate as a function of change size in average SES

The figure displays the interaction terms, β_{-} and β_{+}, along with 95-percent confidence intervals for models with different thresholds for defining a positive or negative change. The threshold is given on the x-axis and the corresponding DiD-estimate is shown on the y-axis. The positive and negative changes are estimated jointly, which implies that estimates with the same distance from 0 on the x-axis stem from the same estimation. The models are estimated with "origin-SAB"-year fixed effects. Standard errors are clustered on origin SAB level.

where $Post_{at} = 1$ if the household is observed after the SAB-change. This is a classic two-period DiD-estimator with heterogeneity in the treatment intensity. By letting the limit for which we categorize the change to be positive or negative vary, we can elicit response size as a function of the school characteristics change. We estimate this model for addresses which are within two years of the change (-2 to 2) and those who do not experience a change. The result for overall compliance is shown in Figure 6. For a fall in average SES the absolute response is monotonically increasing. Thus, larger falls in SES entail lower compliance. When average SES rises, however, we do not observe the same functional relationship - the estimates are small, stable and insignificant. In other words, households respond to a lowering of socioeconomic status of schools but do react to to the same degree when school SES increases. This asymmetry is also found when using other NW-share as school measure.

5.2 Continuous differences in school characteristics

In the previous sub-section, we have shown that changes in compliance are a function of changes in average SES and that non-compliance occur primarily through enrolment in other public schools. To further quantify the responses, we now employ a two-period difference-in-difference model with *continuous* treatment. This model allows us to compute heterogeneous effects in socioeconomic status and control for changes in distance from home to school.

Let $\Delta Q_{ss'} = Q_{s'} - Q_s$ be the difference in a school characteristic between schools s' and s recorded the year before address a experience the change.

$$\begin{aligned} Y_{iass't} = {} & \alpha_0 T_a + \beta_0 \Delta Q_{ss'} + \alpha_1 T_a \times Post_{at} + \beta_1 \Delta Q_{ss'} \times Post_{at} \\ & + \mu_{st} + \varepsilon_{iass't}, \end{aligned} \tag{4}$$

where change in characteristic, $\Delta Q_{ss'}$, equals zero for those addresses which do not experience a change (that is $s = s'$). Our central parameter, β_1, is therefore once again interpreted as an interaction term. This approach yields a number of advantages. Firstly, by including the changes in characteristics directly we encompass the finding above that larger changes are associated with larger responses. This allows us to interpret our results as average marginal effects, which can be used for prediction. Secondly, we can control for other changes in regard to school assignment occurring simultaneously, most notably changes in distance to the district school. We may also employ multiple school characteristics at a time, though we will discuss the somewhat subtle changes in interpretation when multiple measures are included.

Interpreting the results using the specification in (4), we are implicitly assuming symmetric effects, which we have shown may not be present. We however see this as a justifiable simplification for now. When we estimate models of the type specified in (4) we limit our data to SABs where at least one address experiences a change and include only observations two years prior and two years after that change.

We begin by estimating (4) for overall compliance with one school characteristic at a time. The partial results are shown in columns 1-4 in Table 3. Enrollment into the newly assigned school generally increases by around 30 percentage points among those who are actually reassigned, as seen by the value of the parameter on $T \times Post$. The coefficient on distance is significantly negative for all specifications implying that the further a child must travel to the district school the lower the

	(1)	(2)	(3)	(4)	(5)
T $\times$ Post	0.33***	0.33***	0.33***	0.29***	0.30***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Δ Dist $\times$ Post	-0.03*	-0.03*	-0.03*	-0.08*	-0.06*
	(0.01)	(0.01)	(0.01)	(0.04)	(0.03)
Δ SES $\times$ Post		0.70***			0.58
		(0.14)			(0.37)
Δ NW $\times$ Post			-0.40***		-0.14
			(0.10)		(0.27)
Δ SVA $\times$ Post				0.08	0.09*
				(0.05)	(0.05)
N	53,426	53,426	53,426	48,355	48,355

Table 3: Compliance as a function of SAB change

Columns 1-3 display regression results for the model presented in Equation (4) for one school characteristic at a time and with compliance as dependent variable. Column 4 display the result of an estimation using all characteristics at a time. The models are estimated with "origin-SAB"-year fixed effects. Standard errors are in parentheses and clustered on origin SAB level. † $p < .1$, * $p < .05$, ** $p < 0.01$, ***$p < 0.001$.

compliance. This is intuitive as travel time likely is associated with a decrease in utility for households.

The interaction terms on SES and NW-share respectively are highly significant. A standard deviation increase of average SES entails an increase in compliance of around $(0.7 \times 0.1) \times 100 \approx 7$ percentage points. Conversely, an increase of ten percentage points in the NW-share decreases the compliance by 4 percentage points. School value added has no discernible effect on compliance.[25]

Interpretation of partial effects Column 5 of Table 3 include all school characteristics in a simple regression. While the coefficient on SVA changes little, we see substantial changes in the parameters on average SES and NW-share. These changes are due to a large negative correlation of 0.84 between changes in SES and changes in NW-share as evidenced in Table 4. This correlation makes it extremely difficult to separate out partial effects of changes in socioeconomic and ethnic composition. A subtle issue is whether households can make this distinction themselves. It may be the case, that parents simply use the Non-Western as a proxy for school SES.

As described earlier, we constructed our SES-index from a principal component

[25]The coefficient on distance, however, doubles when SVA is included, due to the correlation between the official SVA and distance.

	Δ Dist	Δ SES	Δ NW	Δ SVA
Δ Dist	1			
Δ SES	0.0115	1		
Δ NW	0.0815***	-0.836***	1	
Δ SVA	0.0868***	-0.128***	0.189***	1

Table 4: Correlation in school characteristics changes

The table presents the Pearson correlation coefficients for the changes in school measures following a change in SAB weighted by the number of households experiencing the change.† $p < .1$, * $p < .05$, ** $p < 0.01$, ***$p < 0.001$

analysis. This is likely not a perfect measure of SES. If we are willing to assume the NW-share is really just another proxy for (unobserved) socioeconomic composition, we can employ an insight developed by Lubotsky and Wittenberg (2006) by which we can combine the two estimates to yield a coefficient on a "true" SES-index. If we assume that ΔNW correlates negatively with the unobserved index, then the "true" parameter is given by $0.58 - 0.93 \times (-0.14) = 0.71$, which is almost equal to the parameter value of 0.7 on SES in column 2 of Table 3.[26] This back-of-the-envelope calculation leads us to conclude that the NW-share and our constructed SES index may essentially measure the same underlying socioeconomic conditions and cannot meaningfully be separated in our data.[27] We, therefore, proceed to show results

[26]Formally, simplifying notation, we assume that the true SES, SES^*, is approximated by our index, SES, and the Non-Western share, NW, in the following way:

$$y = \beta SES^* + \varepsilon$$
$$SES^* = SES + u_1$$
$$SES^* = -\rho NW + u_2,$$

where we set the coefficient on SES to one and therefore set the scale of the true SES. Then Lubotsky and Wittenberg (2006) show that β may be approximated by simultaneously regressing y on SES and NW and adding up the coefficients according to their covariances as $\beta = \beta_{SES} + \frac{cov(y,NW)}{cov(y,SES)}\beta_{NW}$. With covariance, $cov(y, SES) = 0.000275$ and $cov(y, NW) = -0.000257$ we obtain the values in the main text.

[27]This is naturally a function of the Danish context. The correlations may differ between countries and over time. In the present case, we think of the correlation as being policy-invariant, though this may not be true in the long term. Interestingly, we also find a positive correlation between the average distance change and the change in school value added of 0.09. This correlation most likely reflects that the SVA, as constructed by the Danish Ministry of Education, does not take urbanization into account. We speculate that a high SVA is due to unobserved sorting which correlates with the location decision of households. We see this as problematic for a measure of value added. This issue is reflected in column 4 of Table 3 where the coefficient on distance drops to zero when we include SVA. We however have more confidence in our distance measure than the SVA provided by the ministry.

using our SES index and abstain from estimating regressions with more than one school characteristic at a time in what follows.

Margins of response We have previously defined how household may defy the assignment mechanisms through different choices. Because we investigate surprise changes in SABs, we suspect that an important margin is to enroll into school in the original SAB. We therefore further decompose the "other public school"-margin, such that the original district school is measured separately. We therefore modify equation (1) to encompass all responses:

$$comply = 1 - third - original - priv - move, \tag{5}$$

where $comply$ takes the value of one if the child enrolls in the assigned district school, $original$ denotes the departure school and $third$ denotes a public school different from original school and the newly assigned school. If the family moved out of the district between age 5 and 7 of the child, $move$ takes the value of one.

We decompose non-compliance rate by estimating models corresponding to column 2 in Table 3 using each component from equation (2). The results are displayed in Table 5. Column 1 reproduce column 2 of Table 3. The remaining columns approximately sum to (minus) the first column. It is evident that changes in compliance stem from the publicly provided option of choosing other public schools, measured by the outcomes, "Original" and "Third". When school-SES increases, the majority of the increase in compliance stems from a diminishing propensity to choose other public schools, as seen by the estimate of -0.79 on SES in the column denoted by "Third" in Table 5. An additional source of increased compliance is the decrease in private school enrollment, though this effect is only significant at a ten-percent confidence-level. This increase in compliance is however attenuated by an increase in the propensity to stay in the original district school. These results are consistent with our first difference-in-difference analysis where we categorized shocks as positive and negative, see Figure B.1. We show below that this response along the $original$-margin stems mostly from low SES households staying behind.

Household heterogeneity in responses - SES As mentioned in section 3 preferences and constraint likely differ at the household-level. This would lead to heterogeneity in responses to changes in SABs. To elicit this, we interact the model

	Comply	Original	Third	Private	Move
T $\times$ Post	0.33***	-0.13***	-0.18***	-0.03*	0.01
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Δ Dist $\times$ Post	-0.03*	0.01	0.04***	-0.02†	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Δ SES $\times$ Post	0.70***	0.38*	-0.79***	-0.19†	-0.10
	(0.14)	(0.15)	(0.12)	(0.11)	(0.09)
N	53,426	53,426	53,426	53,426	53,426

Table 5: Responses along different margins

Columns display regression results for the model presented in Equation (4) using SES as a measure of schools with different dependent variables, displayed in the columns title. The models are estimated with "origin-district"-year fixed effects. Standard errors are in parentheses and clustered on origin district level. † $p < .1$, * $p < .05$, ** $p < 0.01$, ***$p < 0.001$

presented in Equation (4) with household-level characteristics. We begin by fully interacting the model with SES quartile of the household. The effect on overall compliance along with different margins of response is shown in Table 6. The basic reaction to an attendance boundary change along with distance changes exhibits no heterogeneity, as evidenced by the first eight rows. The results, however, indicate a high degree of heterogeneity in responses to changes in school SES. In column 1 the coefficient on average SES is monotonically increasing in own SES. In other words, the higher the socioeconomic status of the household, the larger the expected response. The response from a change in school SES is around 2.5 times larger for the highest quartile compared to the lowest quartile.[28].

The low effect for households in the first SES quartile is explained by a large tendency to stay behind in the old SAB, a choice higher SES-households are unlikely to make, being much more likely to comply with the assignment when average SES increases. We find weak and insignificant evidence that private school may also be a margin of response which higher SES households exploit (though not the highest SES quartile.)

To give a sense of the overall magnitude of changes in cohort size and average SES following an attendance boundary change, we perform a back-of-the-envelope calculation using the results in Table 6. We assume that the group of children to be transferred has a measure of one and is uniformly distributed in four quartiles on the

[28]Calculated as $\frac{0.53+0.36}{0.36} \approx 2.48$

	Comply	Original	Third	Private	Move
T × Post	0.33***	-0.09*	-0.20***	-0.03	-0.01
	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
T × Post × SE Q2	-0.01	-0.02	0.02	-0.02	0.03
	(0.02)	(0.04)	(0.03)	(0.03)	(0.03)
T × Post × SE Q3	-0.01	-0.07†	0.02	0.03	0.03
	(0.02)	(0.04)	(0.03)	(0.02)	(0.02)
T × Post × SE Q4	-0.00	-0.04	-0.00	0.03	0.02
	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)
Δ Dist × Post	-0.03**	-0.01	0.04***	-0.01	0.01
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Δ Dist × Post × SE Q2	0.00	0.01	-0.00	-0.00	-0.01
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Δ Dist × Post × SE Q3	0.00	0.02*	0.00	-0.01†	-0.01
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Δ Dist × Post × SE Q4	-0.01	0.02	-0.00	-0.01	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Δ SES × Post	0.36*	0.65**	-0.67**	-0.16	-0.18
	(0.15)	(0.24)	(0.25)	(0.17)	(0.14)
Δ SES × Post × SE Q2	0.23†	-0.34	0.00	-0.02	0.12
	(0.13)	(0.25)	(0.19)	(0.20)	(0.17)
Δ SES × Post × SE Q3	0.37**	-0.29	-0.21	-0.03	0.16
	(0.14)	(0.23)	(0.27)	(0.19)	(0.18)
Δ SES × Post × SE Q4	0.53**	-0.29	-0.29	-0.01	0.06
	(0.20)	(0.27)	(0.28)	(0.20)	(0.16)
N	47,498	47,498	47,498	47,498	47,498

Table 6: Heterogeneity in responses along different margins, interacted with househols SES

Columns display regression results for the model presented in Equation (4) using SES as a measure of schools with different dependent variables, displayed in the columns title. Characteristics are interacted with household SES quartile. The first quartile is baseline.The models are estimated with “origin-SAB”-year fixed effects. Standard errors are in parentheses and clustered on origin SAB level. $\dagger\, p < .1$, $*\, p < .05$, $**\, p < 0.01$, $***p < 0.001$

unit interval along the SES dimension.[29] First, assume that this group is transferred to a new school but experience no change in school SES, formally $\Delta SES = 0$. The group will have a mass of around 0.33 and an SES of 0.5 equal to the reference group. If school SES falls by 1 std. the mass falls further to 0.27, i.e. a decrease in compliance rate by 20 pct. Not only does the mass fall but the average SES of enrolling households are now 0.48, i.e. a drop of 4 pct.[30] Although changes in SES have a linear effect on compliance rate the impact on SES is non-linear. A drop in school SES of resp. 2 and 3 std. entail a drop in SES of complying households to resp. 0.45 and 0.38, i.e. a drop of resp. 11 and 25 pct. This implies that not only does the size of the group fall, but the socioeconomic composition will be markedly different from the group which was intended to enroll in the new school. Both of these factors should be taken into account when policy-makers are considering redrawing attendance boundaries.

Household heterogeneity in responses - Ethnicity We round off the analysis of attendance boundary changes by investigating heterogeneity in responses across ethnic groups. We interact our basic model with an indicator for whether the child in the household is of Non-Western descent. The results of this exercise are displayed in Table 7. Compared to the basic estimation in column 3 of Table 3, the parameter of the reference group changes from -0.4 to -0.55, which imply that Western/Danish households react more to changes in the Non-Western share than average. Conversely, Non-Western households react to a far lesser degree, as can be seen by adding up the appropriate coefficients in the "Comply"-column (-0.55 + 0.49 = -0.06).

Concluding attendance boundary changes

We have documented that household compliance to school assignment is a function of the school characteristics, with a stark social gradient. A consistent finding is that other public schools make out the primary way by which households avoid the reassignment to a new school. The most important means of non-compliance is therefore publicly provided.

[29]For each quartile we assign an SES in the middle of the interval.
[30]This is calculated using Equation 10.

	Comply	Original	Third	Private	Move
T × Post	0.34***	-0.15***	-0.18***	-0.02	0.01
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)
Δ Dist × Post	-0.03*	0.01	0.04***	-0.02†	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Δ NW × Post	-0.55***	-0.18†	0.61***	-0.03	0.16†
	(0.12)	(0.10)	(0.09)	(0.09)	(0.09)
T × Post × NW	-0.02	0.10**	-0.02	-0.05†	-0.01
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Dist × Post × NW	0.00	-0.05*	0.01	0.02†	0.02
	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)
Δ NW × Post × NW	0.49***	-0.26†	-0.17	0.06	-0.11
	(0.13)	(0.15)	(0.16)	(0.11)	(0.12)
N	53,426	53,426	53,426	53,426	53,426

Table 7: Heterogeneity in responses along different margins, interacted with households' SES

Columns display regression results for the model presented in Equation (4) using SES as a measure of schools with different dependent variables, displayed in the columns title. Characteristics are interacted with household Non-Western dummy. The models are estimated with "origin-SAB"-year fixed effects. Standard errors are in parentheses and clustered on origin SAB level. $\dagger\ p < .1$, $*\ p < .05$, $**\ p < 0.01$, $***p < 0.001$

We find very small effects of changes on private school enrollment. We conjecture that the lack of response along this margin may be due to the surprise effect of the redistricting. If households have not foreseen the change, as we indeed assume, then a private school may not be an option due to over-subscription. Thus, responses along this margin is probably larger in the long run, which we cannot capture using the changes in attendance boundaries. The same kind of reasoning may apply to the decision to move out of the district, which may entail significant costs. To investigate whether results may differ in the long term we turn to our auxiliary approach.

6 Auxiliary approach: Cross border comparisons

Our aim in this section is to the uncover responses, which may not have been feasible for households during the short window used to measure the impact of district changes in the previous section. We once again investigate all three options for opting-out of the school district (i.e. non-compliance); relocating, enrolling in either private and exploiting the public loophole by enrolling in another public school.

As noted in the previous section, the school-SES and the NW-share are highly correlated in a Danish context. In this setup, we observe a correlation of school borders' differences in average socioeconomic status and share with a non-Western heritage of -0.75, see Appendix Table C1. We, therefore, focus on socioeconomic status and note that replacing SES with NW-share will yield the same conclusions.

We begin our analysis with reduced form estimation of border differences without covariates. We follow the approach presented by Bayer et al. (2007) and construct bins of distances. We define a distance to be negative if the household is associated with the school with the lowest value of the two neighbor schools on a given school characteristic. We implement regressions of the following kind:

$$Y_{ibt} = \sum_{d=D^-}^{D^+} \gamma^d \mathbf{1}(dist_{ibt} = d) + \mu_{bt} + \varepsilon_{ibt}, \tag{6}$$

where Y_{ibt} is our binary dependent variable for child in household i at border b at time t, $dist$ is the signed distance to the border and μ_{bt} is a border-year fixed effect. By including the border-year-fixed effect we are comparing only within border regions in the same year. As we are interested in the difference across borders, we center our results on a left side dummy (i.e. the lower side).[31]

To provide central estimates to compare with the previous section we collapse Equation (6) to the following:

$$Y_{ibt} = \gamma^+ \mathbf{1}(dist_{ibt} > 0) + \mu_{bt} + \varepsilon_{ibt}, \tag{7}$$

where γ^+ denotes the value of being on the high side of the boundary. We estimate the marginal effect of school characteristics on compliance as equation (7), but replacing the dummy for being o a high side with the SES of the district school. Due to the inclusion of border-year-fixed effects the only variation used is from crossing the border. We report these estimates in the figures as well.

In our samples, we use all children age 5 located residing less than 2 kilometers from a border. Each child may be located within multiple border regions. We cluster standard errors to accommodate this. Outcomes are observed at age 7, ie. where all children should be enrolled in a primary school.

[31]We include a dummy for being on the higher value border side as well as interactions of high side with distance. The low side is therefore the reference category.

Socioeconomic status We estimate Equation (6) for the probability of choosing a non-district public school, private school and moving away, again using school average SES as a measure of schools. The resulting γ^d from the estimations are plotted in Figure 7.[32] We start by noting that the probability of non-compliance increases as one approaches the border from either side, as seen in Figure 7a. This can be ascribed to the decreased distance to the neighboring school closer to the border.[33] Despite this pattern there is a clear discontinuity at the border taking the form of a vertical shift in non-compliance. In other words, close to the border of two attendance zones, being on the side with lower average student SES is associated with around 13 percentage points lager non-compliance. We interpret this as evidence that socioeconomic compositions of the student body matter for household compliance rates.

Once again, we see that the public option is the main source of non-compliance. Figure 7b shows that choosing another public school makes out over half the total non-compliance. This is in line with the Difference-in-Difference results. In the previous section, we found no clear evidence that households use private school as a means by which to defy the assignment mechanism. In Figure 7c, however, we see that differences in private school enrollment make out a sizable portion of the total difference in compliance rate. The difference in private school enrollment of around three percentage points accounts for a quarter of the total non-compliance. An equal portion of the non-compliance is explained by households moving out of the district before the child turns seven, as seen in Figure 7d.

When we rescale the average difference in non-compliance with the average difference in school SES the estimate is -1.16 which is numerically higher than our baseline estimate of -0.7 from our Difference-in-Difference approach, see Table 3. However, subtracting the effect from private school and moving, one gets the public option exclusively, whit an estimate of -0.64 - much closer to the effect iden-

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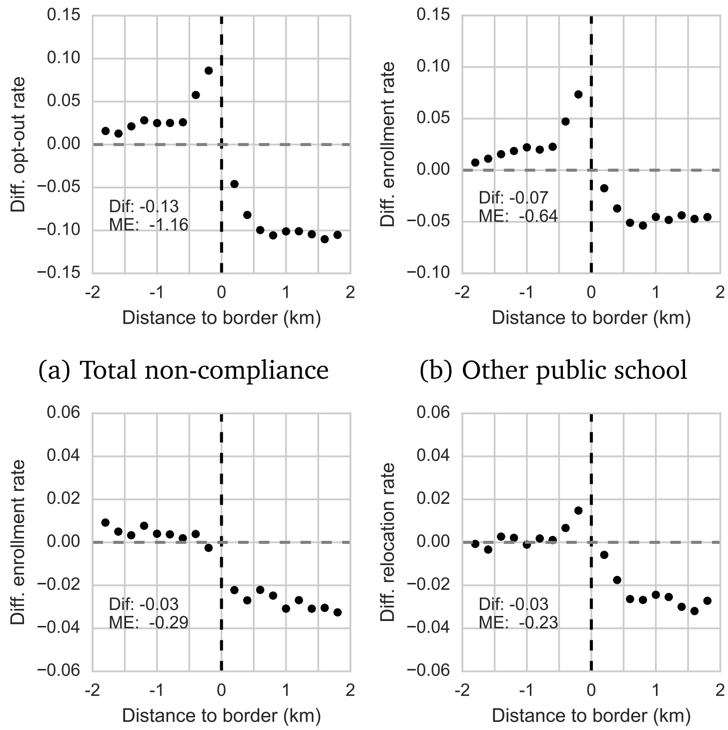
[32] Appendix Figure C.2 presents estimates for all three measures, SES, NW-share and SVA (as provided by the ministry).

[33] We note that the rising tendency to not attend the district school closer to the border is fully explained by enrollment in other public schools and moving with no such pattern for private schools. Therefore, the pattern is likely the product of commuting distances. As one approaches the border the distance to the neighbor school will decrease. It may, therefore, be more *convenient* for households to choose the neighbor school. If school characteristics are not important we should thus observe a bell shape around the border. The same logic can account for the rise in movement propensity when approaching the border from either side in Figure 7d.



(a) Total non-compliance

(b) Other public school

(c) Private school

(d) Move before age 7

Figure 7: Boundary difference in opt-out for low/high SES schools

The figures depict the estimated parameters of a BDD-model estimated for different dependent variables and average SES as school characteristic according to the model presented in equation (6). The dependent variable in Figure 7b is a dummy which takes the value of one if the child is enrolled in a non-district public school school. The dependent variable in Figure 7c is a dummy indicating that the child is enrolled a private school school. The dependent variable in Figure 7d is a dummy indicating that the household has moved before before the child turns seven years old. Negative distance to border signifies that the household is situated in the district of the two bordering districts with the lower value of the school characteristic. The models are estimated with fixed effects at the border-year level. The mean difference is estimated in OLS and displayed in the lower-left corner of the figures. The corresponding rescaled estimate, denoted ME, is estimated with OLS by replacing the indicator for being on the right side with the average SES of schools on either side. All marginal effect estimates are significant at $p < 0.001$; see Table C2 for details.

tified off the changes in attendance boundaries. This implies that the long-term estimates using the Boundary Discontinuity Design are in line with the Difference-in-Difference estimate though the former is subjective to more restrictive identifying assumptions.[34]

The results displayed in Figure 7 are averages over all borders. However, an issue is that differences between the neighboring district schools are of different magnitudes. If the school quality measures actually cause the observed differences, we would expect the differences in opting-out use to be monotonically increasing in the size of differences. To investigate this, we estimate the model from Equation (7) across the distribution of border-SES-differences for opting out, decomposed into the three elements; another public school, private school and moving away. The results of this exercise are displayed in Appendix Figure C.3. The decomposition show that all three sources of non-compliance generally are important and that differences in behavior are almost linear in differences in school characteristics, which imply constant marginal effects. [35]

Response heterogeneity by socioeconomic status We finish our boundary discontinuity analysis of the importance of school SES by investigating heterogeneity in responses. We repeat the analysis for each quartile of the household SES-distribution.[36] The results of this exercise is presented in Figure 8. Figure 8a shows a clear discontinuity in compliance for all quartiles. However, the implied marginal effect of the highest quartile, -1.47, is more than twice the size of the estimate for the lowest quartile of -0.67. Thus, high SES households are much more sensitive to the socioeconomic composition in the district school when choosing whether to comply with the school assignment. Figure 8b shows the difference in enrollment into non-district public schools. A clear discontinuity for all quartiles is evident, but all exhibit marginal effects around -0.5. The heterogeneity is therefore not explained by the public school option in this setting. Figure 8c shows private schools

[34]We perform a number of robustness tests including adjusting for covariates and exploit heterogeneous effects. The results are robust to a variety of specifications.

[35]Note the close resemblance to the Wald-estimator. Again, subject to an exclusion restriction the discontinuity estimates may be rescaled by the difference in school SES to obtain an IV estimate of the marginal effect of average school SES on compliance rates. The very linear relationship in Appendix Figure C.3 implies a constant marginal effect.

[36]This approach, therefore, compares *within* SES quartile *within* border-year and is equivalent to fully interacting the border-year fixed effect with SES-quartile.

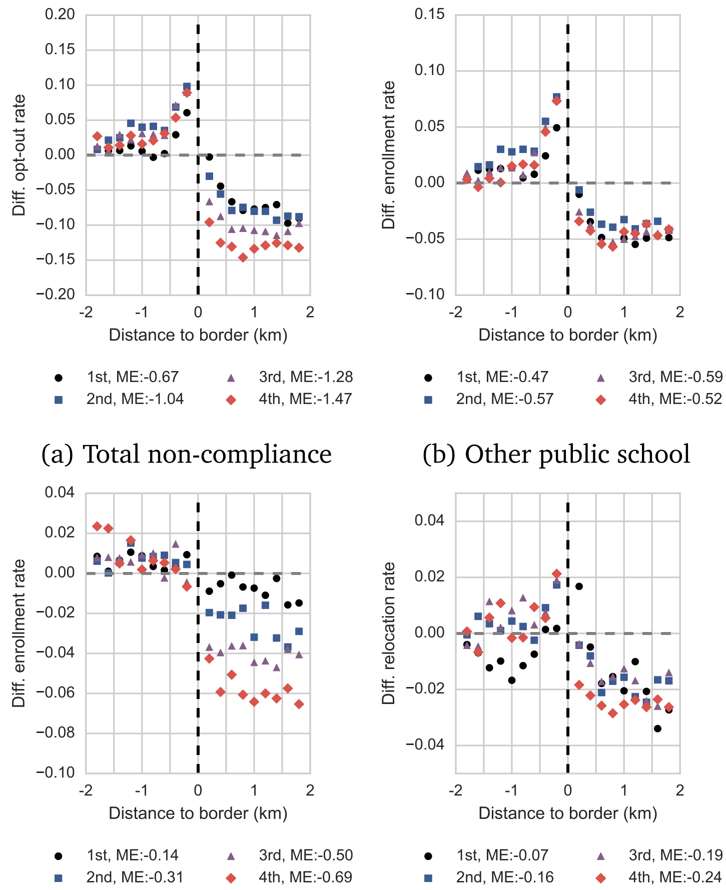
enrollment is more heterogeneous across SES quartiles - the discontinuity is monotonically increasing in household SES. The ratio of the marginal effect of the highest to the lowest quartile is almost 5.[37] In other words, faced with lower average SES in the district school a high-SES household is much more likely to enroll the child in private school than a low-SES household. Figure 8d shows the same pattern for the relocation of households as for private school enrollment, though the ratio of highest to lowest quartile is only 3.4. In other words, high SES-households are much more likely to move away when faced with a low-SES district school, though they are relatively more sensitive along the private school margin compared to low-SES households. In this framework we cannot investigate whether this socioeconomic gradient is due to preferences or constraints, but we conjecture that both likely play a role. Regardless of the source of heterogeneity in behavior, our findings imply that redrawing of attendance boundaries likely will lead to less between school homogeneity than a prediction without consideration of behavioral effects would suggest.

School value-added We investigate the importance of school value-added in Appendix C. Figure C.2 shows the discontinuities in responses to differences in school value-added; the responses are weaker than the responses for SES in Figure 7. An overview of estimated marginal effects associated with the differences is found in Table C3. Figure C.4 shows decomposition of non-compliance as a function of school value-added; it is evident that increased school value-added is associated with higher compliance, though the effects are less clear than those for other school characteristics.

7 Conclusion

Policy-makers who aim to balance school composition can manipulate the school boundaries and choose which children are supposed to enroll where. But the efficacy of this strategy, as with most other public interventions, is threatened by individuals' behavioral responses. We have documented that parents react to redistricting by opting out of their assigned school. They do this by moving, choosing other public schools and private schools. Households with high socioeconomic status drive the responses, which implies that the "leakage" occurs in the top of the

[37] From Figure 8c we calculate the ratio $(-0.69)/(-0.14) = 4.9$



(a) Total non-compliance

(b) Other public school

(c) Private school

(d) Move before age 7

Figure 8: Boundary difference in opt-out for low/high SES schools - by household SES

The figures depict the estimated parameters of a BDD-model estimated for different dependent variables and average SES as school characteristic according to the model presented in equation (6). The model is estimated separately for each quartile of household SES. The dependent variable in Figure 8b is a dummy which takes the value of one if the child is enrolled in another public school. The dependent variable in Figure 8c is a dummy indicating that the child is enrolled in a private school. Negative distance to border signifies that the household is situated in the district of the two bordering districts with the lower value of the school characteristic. The models are estimated with fixed effects at the border-year level. The mean difference is estimated in OLS and displayed in the lower-left corner of the figures. The corresponding rescaled estimate is estimated with OLS by replacing the indicator for being on the right side with the average SES of schools on either side. For parameter coefficients and tests from the estimation of marginal effects see Table C2.

distribution. Consequently, if policy-makers want to minimize variance in student compositions they must do so under the constraint that parents have an outside option.

Interpreted more generally these findings imply that there are limits to the possible manipulation of peer groups when individuals have an outside option. In other words, there is limited potential for what Durlauf (1996a) refer to as associational redistribution. Consequently, our results matter not only for drawing boundaries between schools. They are also relevant in a broader sense for designing groups within schools and organizations.

Our results indicate some possible venues for further analysis. Our model is limited by only investigating enrollment decisions and relocation partially; it would be interesting to model the choice of residence and school simultaneously. One possible theoretical analysis would be to investigate the extent to which limiting the outside options of households for enrollment in private schools affects the compliance of parents in the public district school system.

A peculiar facet of the Danish system is the high (but not full) degree of public financing of private schools and the possibility of enrolling in non-district public schools. The latter is in practice a very opaque process. It is likely that this process makes it relatively easier for highly sophisticated (and most likely educated) parents to exploit the system to the detriment of less sophisticated parents. The private school funding and the implementation of choice mechanisms make it possible for households to segregate in the educational system without the usually associated residential segregation. This may be beneficial if there are benefits to desegregation outside primary school education. Nevertheless, the implied decoupling between educational and residential segregation also diminishes the efficacy of redistricting as a policy tool to increase equality of opportunity.

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A Data description

This appendix consists of an account for how we measure socioeconomic status and also contains additional descriptive output.

A.1 Construction of SES index

This sub-appendix outlines how we construct our socio-economic index. We describe our approach of reducing a set of socio-economic variables to a single socio-economic index (SES index henceforth) and we evaluate the index' performance.

We construct our SES-index by choosing the first variable resulting from a principal component analysis (PCA) based on the following variables:

- INC: We calculate the market income rank of all adults in the population. We select the highest income rank observed in a household.
- LCE: A dummy which takes the value of one if an adult in a household has completed a long cycle education.
- NE: A dummy which takes the value of one if an adult in a household has not completed in education beyond primary school or have no registered education.
- EMP: A dummy which takes the value of one if an adult in a household is employed.

We select the first component of the PCA. This leads to the following index:

$$SES = 0.62INC + 0.38LCE - 0.44NE + 0.53EMP, \tag{8}$$

where all variables have been standardized to their corresponding z-scores. This index accounts for 47 pct. of the variation in the four variables. The SES-index applied in our paper is the population ranks of SES, as such it is uniformly distributed on the unit interval.

To get a sense of the mapping between the underlying variables we calculate averages of the underlying variables in percentiles of the SES-index. The results are displayed in figure A.1. While this is a very simple index we find that this component is intuitive. In the bottom of the distribution almost all households have an uneducated parent and no parent with a high cycle education. In the top 75

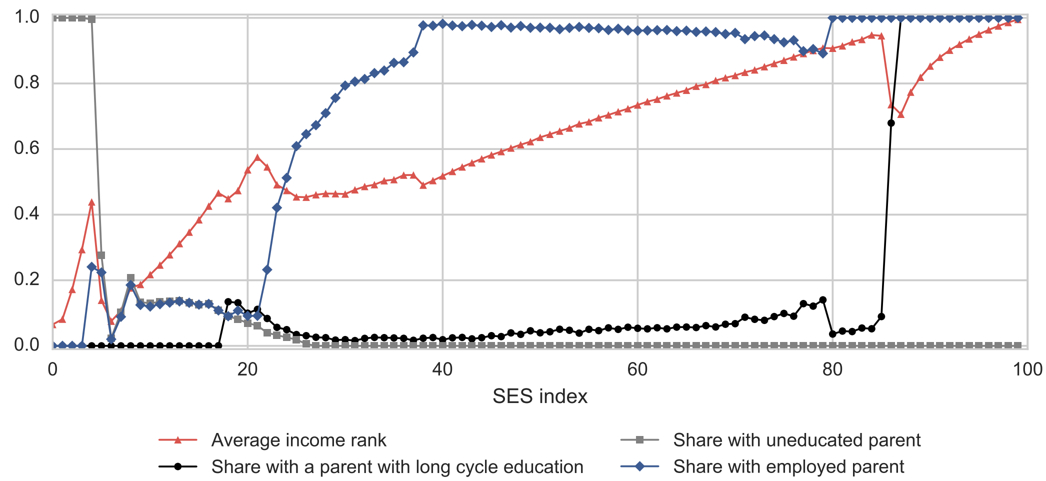
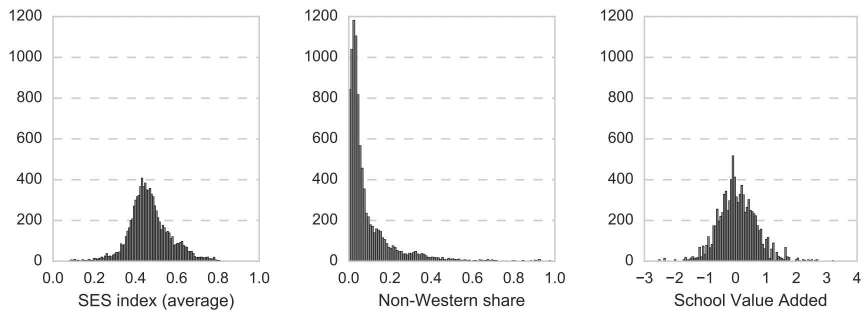


Figure A.1: Average characteristics as a function of SES-index

The figure depicts means of variables used to construct the SES-index. The SES-index is uniformly distributed on the unit interval. Each marker represent the mean of the variable in question within a percentile bin. Income rank is bounded between 0 and 1.

percent of the distribution no household contain an uneducated parent. Income and employment are both rising in the SES-index. Thus we find it safe to assume that the SES-index reflects a true underlying socioeconomic status.



(a) Average SES (b) NW-share (c) SVA

Figure A.2: Distributions of school characteristics

The figures depict the distributions of school characteristics for district schools for the years 2008-2015. Figures A.2a, A.2b and A.2c respectively display district schools' average share of non-Western descendants, average SES-index and school value added. Note these measures exclude private schools.

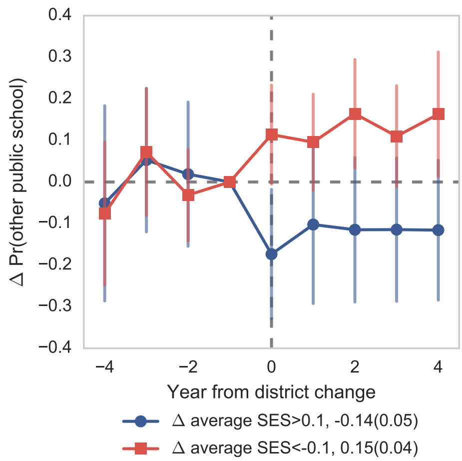
B Supplementary results for changes in district borders

In this appendix we provide additional results for the main approach. We begin with the computation of SES for complying households.

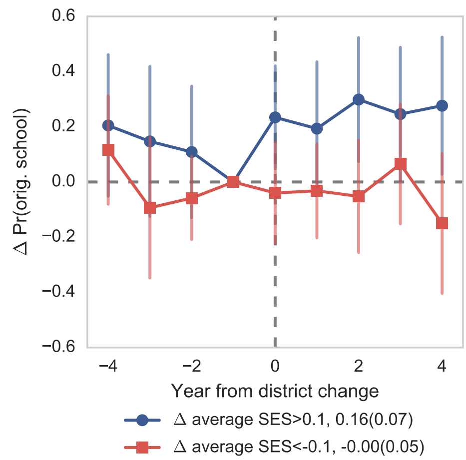
$$E[SES_i|comply_{i,ss'} = 1,\ \Delta SES] = \frac{\sum_{q\in\{1,..,4\}} w_q \left(\beta_q^{P\cdot T} + \beta_q^{P\cdot T\cdot SES}\Delta SES_{ss'}\right)\cdot \mu_q}{\sum_{q\in\{1,..,4\}} w_q \left(\beta_q^{P\cdot T} + \beta_q^{P\cdot T\cdot SES}\Delta SES\right)} \tag{9}$$

where q denotes quartile, ΔSES is the change in school SES and μ_q is the mean SES for quartile q. We can rewrite the equation into and plug in parameter estimates from Table 6 into Equation 9:

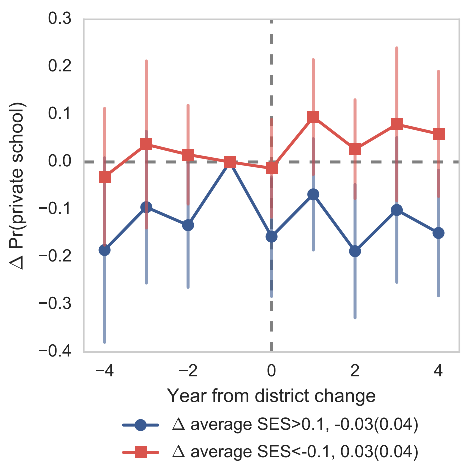
$$E[SES_i|comply_{i,ss'} = 1,\ \Delta SES_{ss'}] = \frac{0.165 + 0.388\cdot\Delta SES}{0.329 + 0.663\cdot\Delta SES} \tag{10}$$



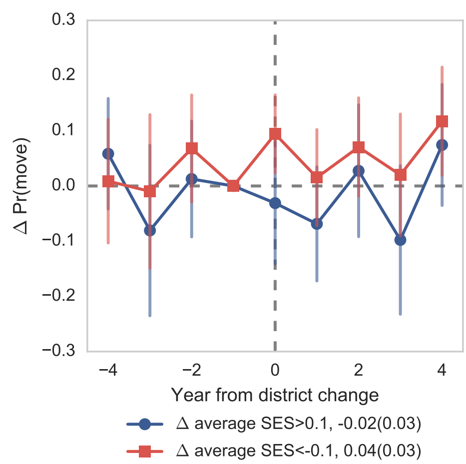
(a) Other public school



(b) Stay in original district school



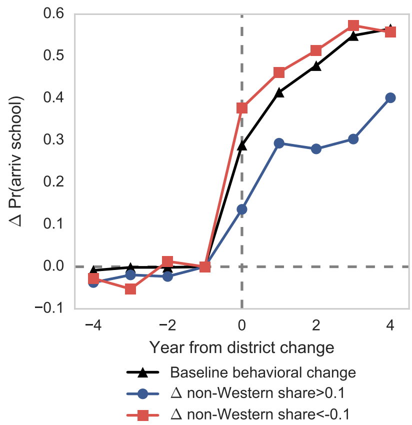
(c) Private school enrollment



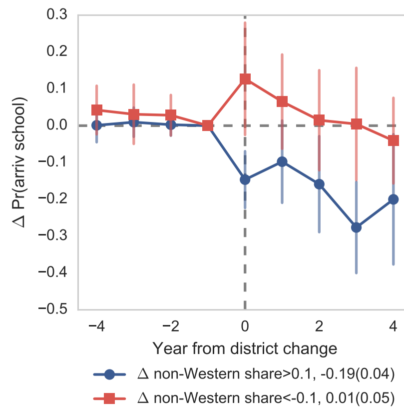
(d) Moving

Figure B.1: Response along margins to change in district by school characteristic

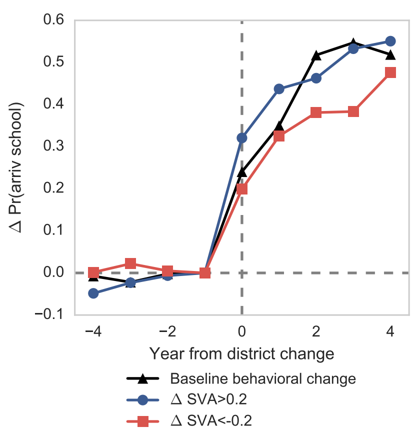
The figure display the interaction terms, β_{-}^{k} and β_{+}^{k}, along with 95-percent confidence intervals. The parameters represent the difference in likelihood of enrolling in the new district school when the average SES at a school level changes relative to the average arrival probability following a district change. The dependent variables of all figures are binary and measured at age 7 based on the district at age 7 for address at age 5. The models are estimated with "origin-district"-year fixed effects. Standard errors are clustered on origin district level. Results are centered at the year before the district change. Estimates from a simple before-after-DID are reported in the legends of figure 5b.



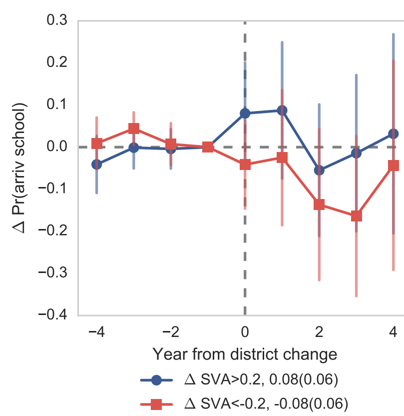
(a) NW - aggregate compliance



(b) NW-share - interaction



(c) SVA - aggregate compliance



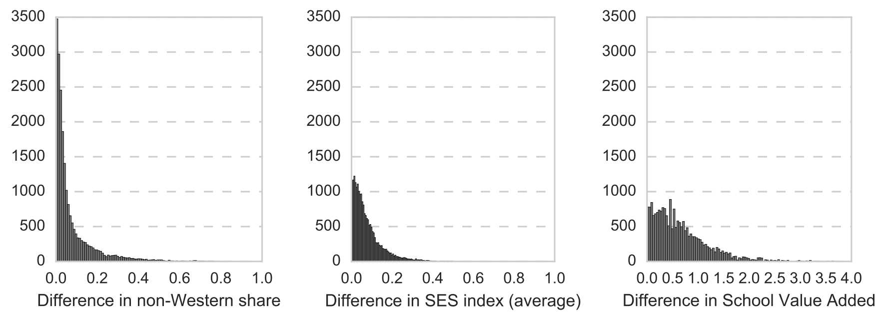
(d) SVA - interaction

Figure B.2: Compliance as a function of Non-Western share and school value-added

The figures on the left display changes in estimated compliance rates based on the model in (2) estimated with diffrenent measures of school characteristics. The black lines depict the estimated β_T^ks, while the blue and red line depict $\beta_T^k + \beta_-^k$ and $\beta_T^k + \beta_+^k$ respectively. The figures to the right display the interaction terms, β_-^k and β_+^k, along with 95-percent confidence intervals. The parameters represent the difference in likelihood of enrolling in the new district school when the school characteristic at a school level changes relative to the average arrival probability following a district change. The dependent variable is binary and equals one if the child is enrolled in the district school at age 7 based on the district at age 7 for address at age 5. The y-axis denotes the excess probability of enrolling relative to baseline. Standard errors are clustered on origin district level. Results are centered at the year before the district change. Estimates from a simple before-after-DID are reported in the legends of figure 5b.

C Supplementary results for auxiliary approach: Cross border comparison

This appendix provides supporting information with additional results for the analysis using border comparisons in Section 6.



(a) NW-share (b) Average SES (c) SVA

Figure C.1: Border differences of school characteristics

The figures depict the distributions of boundary differences in school characteristics for the years 2008-2015. Figures C.1a, C.1b and C.1c display the associated absolute differences in the school measures between neighboring districts.

	ΔSES_b	ΔEMP_b	ΔINC_b	ΔHCU_b	ΔNE_b	ΔNW_b	ΔGPA_b	ΔSVA_b
ΔSES_b	1.00	0.88	0.98	0.74	-0.83	-0.75	0.66	0.18
ΔEMP_b	0.88	1.00	0.89	0.45	-0.83	-0.84	0.58	0.15
ΔINC_b	0.98	0.89	1.00	0.66	-0.83	-0.80	0.64	0.17
ΔHCU_b	0.74	0.45	0.66	1.00	-0.45	-0.39	0.56	0.14
ΔNE_b	-0.83	-0.83	-0.83	-0.45	1.00	0.76	-0.58	-0.16
ΔNW_b	-0.75	-0.84	-0.80	-0.39	0.76	1.00	-0.51	-0.10
ΔGPA_b	0.66	0.58	0.64	0.56	-0.58	-0.51	1.00	0.23
ΔSVA_b	0.18	0.15	0.17	0.14	-0.16	-0.10	0.23	1.00

Table C1: Correlation matrix for border school district differences across borders

The table presents a correlation matrix for variables used in the analysis and the variables used to construct the socioeconomic index.

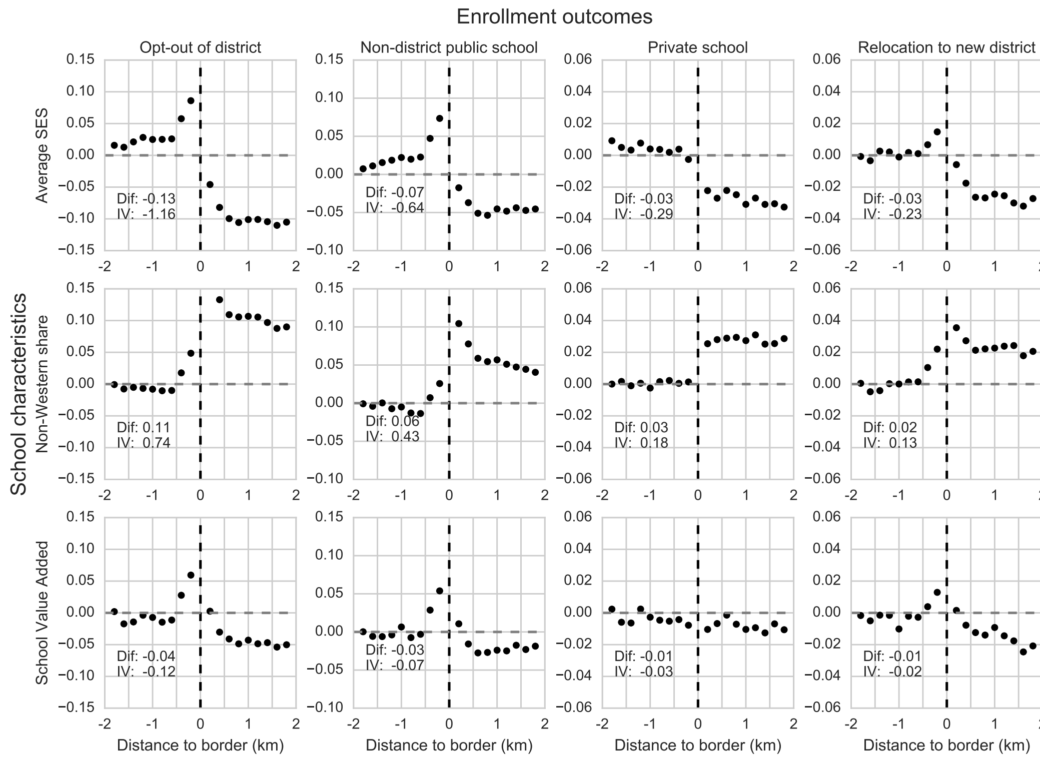


Figure C.2: Differences in opt-out rate as a function of difference in school characteristics

The figures depict the estimated parameters of a BDD-model estimated for different dependent variables and School Value Added as school characteristic according to the model presented in equation (6). The dependent variable in Figure 7b is a dummy which takes the value of one if the child is enrolled in a non-district public school school. The dependent variable in Figure 7c is a dummy indicating that the child is enrolled a private school school. The dependent variable in Figure 7d is a dummy indicating that the household has moved before before the child turns seven years old. Negative distance to border signifies that the household is situated in the district of the two bordering districts with the lower value of the school characteristic. The models are estimated with fixed effects at the border-year level. The mean difference is estimated in OLS and displayed in the lower-left corner of the figures. The corresponding rescaled estimate is estimated with OLS by replacing the indicator for being on the right side with the average SES of schools on either side.

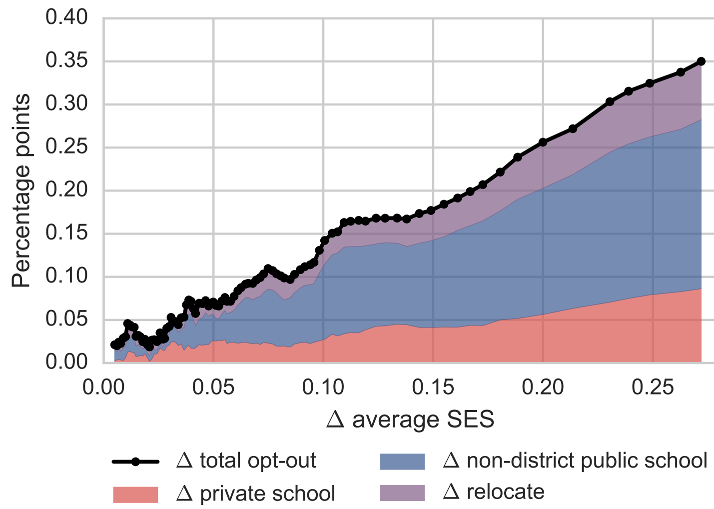


Figure C.3: Differences in opt-out rate as a function of difference in socioeconomic status

The figure depicts the estimated border differences in opting-out of the district school as a function of the border differences in average SES for adjacent district schools. The markers are the γ^+ of the model presented in (7) where we decompose the opting-out into its two subcategories, other public school or private school. The model is estimated in sliding ten percentage point windows of the ranked border difference distribution. Each marker represents the middle of the sampling interval.

	Any	Q1	Q2	Q3	Q4
Opt-out of district	-1.159^{***}	-0.674^{***}	-1.038^{***}	-1.279^{***}	-1.466^{***}
	(0.032)	(0.041)	(0.044)	(0.047)	(0.046)
Non-district public school	-0.641^{***}	-0.466^{***}	-0.567^{***}	-0.585^{***}	-0.518^{***}
	(0.029)	(0.040)	(0.033)	(0.036)	(0.036)
Private school	-0.287^{***}	-0.141^{***}	-0.305^{***}	-0.495^{***}	-0.686^{***}
	(0.014)	(0.024)	(0.024)	(0.028)	(0.033)
Relocation to new district	-0.226^{***}	-0.073^{**}	-0.164^{***}	-0.190^{***}	-0.243^{***}
	(0.019)	(0.027)	(0.025)	(0.021)	(0.024)

†: $p < .1$, $*$: $p < 0.05$, $**$: $p < 0.01$, $***$: $p < 0.001$

Table C2: IV estimates of difference in opt-out rate from difference in socioeconomic status

The table presents estimations of the marginal propensity to opt out as a function of difference in school average socioeconomic status (SES). School SES is instrumented by a dummy for being at the "high side" of the district border. Models are estimated with only one quality measure at a time. Standard errors are clustered by the connected component of a graph where an edge exists between border regions if the same household forms part both regions. This procedure creates in 480 clusters.

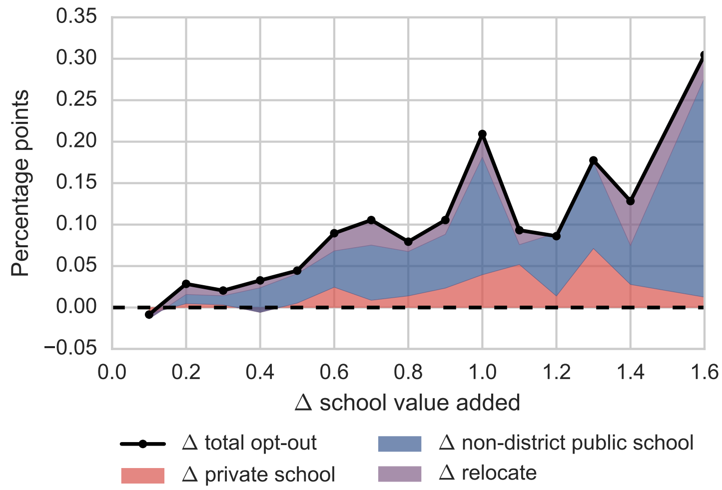


Figure C.4: Differences in opt-out rate as a function of difference in School Value Added

The figure depicts the estimated border differences in opting-out of the district school as a function of the border differences in School Value Added (SVA) for adjacent district schools. The markers are the γ^+ of the model presented in (7) where we decompose the opting-out into its two subcategories, other public school or private school. The model is estimated in sliding ten percentage point windows of the ranked border difference distribution. Each marker represents the middle of the sampling interval.

	Any	Q1	Q2	Q3	Q4
Opt-out of district	-0.118^{***}	-0.069^{***}	-0.131^{***}	-0.116^{***}	-0.131^{***}
	(0.016)	(0.020)	(0.019)	(0.021)	(0.019)
Non-district public school	-0.070^{***}	-0.059^{**}	-0.063^{***}	-0.064^{***}	-0.059^{***}
	(0.014)	(0.018)	(0.013)	(0.014)	(0.012)
Private school	-0.025^{***}	-0.004	-0.038^{***}	-0.028^{*}	-0.051^{***}
	(0.007)	(0.008)	(0.009)	(0.011)	(0.015)
Relocation to new district	-0.024^{***}	0.002	-0.029^{**}	-0.026^{**}	-0.025^{*}
	(0.006)	(0.011)	(0.010)	(0.009)	(0.010)

†: $p < .1$, $*$: $p < 0.05$, $**$: $p < 0.01$, $***$: $p < 0.001$

Table C3: IV estimates of difference in opt-out rate from difference in School Value Added

The table presents estimations of the marginal propensity to opt out as a function of difference in School Value Added (SVA). School SVA is instrumented by a dummy for being at the "high side" of the district border. Models are estimated with only one quality measure at a time. Standard errors are clustered by the connected component of a graph where an edge exists between border regions if the same household forms part both regions. This procedure creates in 480 clusters.