Attendance Boundary Policies and the Limits to Combating School Segregation*

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Abstract

What is the efficacy of redrawing school attendance boundaries as a desegregation policy? To provide causal evidence on this question we employ novel data with unprecedented detail on the universe of Danish children and exploit changes in attendance boundaries over time. Households defy reassignments to schools with lower socioeconomic status. There is a strong social gradient in defiance, as resourceful households are more sensitive to the student composition of schools. We simulate the efficacy of desegregation policies and find that in areas with large levels of segregation, behavioral responses of households almost completely offset the intended effects of boundary changes.

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1 Introduction

Across the developed world, policymakers redraw school attendance boundaries (SABs) in order to alter the socioeconomic composition. By redrawing boundaries the local authorities manipulate the set of households who are eligible to enroll in certain schools. However, households may have other options than the designated school and can, therefore, choose not to comply with the policy. Such outside options pose a threat to the efficacy of boundary changes as a policy tool to decrease segregation. To assess the magnitude of such inefficacies, one needs estimates of how household decisions are affected by attributes of the affiliated school. The main obstacle to obtaining well-identified estimates of such behavioral responses can be attributed to two distinct challenges: a lack of comprehensive panel data and a lack of exogenous variation in school affiliation.

To overcome the first obstacle, we exploit a new data set with precise geographic information on both school boundaries and household residential locations for the universe of Danish children during the years 2008-2015. To overcome the second obstacle we exploit changes in attendance boundaries over time. Due to the boundary changes, some households will experience a change in school affiliation while others will not. By comparing households that are reassigned to those who are not, we can identify the effect of the school composition on choices of enrollment. The number of boundary changes is large. During our observed period, a total of 191 schools, 17 percent of all schools, have parts of their attendance reassigned to another school. The number of changes coupled with the comprehensiveness of our data allows us to fully investigate heterogeneity in household responses.

We find large and immediate effects on households that are transferred to schools with a lower average socioeconomic index. A one standard deviation drop in the average socioeconomic status (SES) implies an average fall in enrollment of 20 percent. We find qualitatively the same results when we replace school-SES with the ethnic composition of students.[1] When average school-SES is replaced by a publicly available measure of school value-added we find no systematic responses.

[1] We are unable to separate the effect schools' average socioeconomic status from ethnic composition as the two measures are highly correlated.

We also provide evidence of a strong social gradient: the enrollment decision of the highest SES quartile is 2.5 times as sensitive to the student composition as that of the lowest.

Households primarily avoid a new designated school by enrolling their children in other public schools - an option provided free-of-charge in Denmark. It is not clear, that all households have equal access to this option, as the admission process is opaque and decentralized. In the first year after the boundary change, we find little evidence that households choose a private school or move out of the attendance zone as a means of avoidance. When we investigate the responses of households with younger children, who have more time for planning enrollment, we find indications of increased responses along the private school margin. Though these latter estimates are imprecise, we conjecture that the lack of effect found in our main sample of school-age children may be due to the element of surprise of a boundary change and a lack of supply as private schools are oversubscribed.[2] Using a boundary-discontinuity design we find clear suggestive evidence of private schools being an important exit margin. While strong assumptions are required for a causal interpretation of the estimates from the boundary-discontinuity design, they suggest that behavior along additional margins, while important in the long term, may not be fully captured by our main research design.

Our results imply that there are inefficacies to changing boundaries as a means to change the socioeconomic composition of schools. To quantify these inefficacies we embed our results in a simulation. For a range of levels of residential segregation, we assign parcels from an affluent to a less affluent school attendance zone and quantify the expected responses. A transfer between two schools separated by a standard deviation of school-SES leads to 75 percent of the expected increase in school-SES of the less affluent school. For larger levels of residential socioeconomic segregation, the transfer of parcels completely fails to raise school-SES. In other words, in communities where increased desegregation may be most desirable, changing attendance boundaries is not a viable policy tool.

[2]Private schools often have waiting lists, requiring households to apply years in advance; the limited supply 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.

Our findings are informative of other contexts than that of the Danish primary school system. Firstly, allocation via attendance zones is the dominant way of allocating children to schools across the developed world.[3] However, surprisingly little empirical evidence exists on the behavioral responses to changes in attendance boundaries, especially in a European context.[4] Our findings are therefore directly relevant for administrators in school systems with attendance zone allocation. Our results show that the structure of the market for primary education matters a great deal; households exploit the options available to them. Thus, the more similar the market, the more directly transferable are the estimates to a given context. Many countries have variations on the primary school structure found in Denmark. A prime candidate for comparison is France with a combination of a degree of choice in public schools and prevalence of relatively cheap private schools, see Fack and Grenet (2010); Hiller and Tercieux (2013).[5]

Secondly, our results are also relevant for other systems of student allocation. Controlled school choice is a recent attempt at incorporating political aims, e.g. desegregation (Cantillon, 2017) by using quotas (Kamada and Kojima, 2015; Goto et al., 2017). Such policies therefore represent an alternative to changing attendance boundaries. As quotas and similar measures will lead to some forced allocation our findings are also informative for the efficacy of such policies as applicants may exploit outside options such as private schools instead.

Lastly, 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). Our results show that reallocation leads to systematic defiance. Trying to

[3] Some school systems that use attendance zones either exclusively or in combination with school choice include United States (Monarrez, 2017), France (Fack and Grenet, 2010; Hiller and Tercieux, 2013), Spain (Calsamiglia, 2014) and Scotland (Manlove, 2012).

[4] Well-identified behavioral responses to school composition are mostly estimated in a context of court-ordered desegregation in the US (Baum-Snow and Lutz, 2011). A court ordered desegregation is an arguably very different context from that which school authorities face when designing attendance boundaries.

[5] Unlike in the US, school funding does not differ between schools within municipalities. Thus we conjecture that our estimated effects may be larger if educational resources were positively correlated with the socioeconomic composition of students.

force students into low SES schools, means that a higher number of resourceful students never arrive in the designated school (or possibly abandon the public school system entirely). As such, our results imply that there may be limits to scaling up *Moving to Opportunity*-type policies (see Bergman et al. (2019) as an example). At some point socioeconomic strong households will leave, thereby negating the possible peer effects.

Related Literature 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; Clotfelter, 1976; Saporito and Sohoni, 2007; Rangvid, 2009; Bifulco et al., 2009; Riedel et al., 2010; Baum-Snow and Lutz, 2011) and have emphasized outside options of private schools (Clotfelter, 1976) and relocation (Baum-Snow and Lutz, 2011). Our findings are consistent with these findings in that households tend to avoid schools where the ethnic or socioeconomic 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. Though the context of court-ordered desegregation differs markedly from our context, we find it reassuring that the findings are similar.

An alternative to attendance zones for allocating children to schools is matching mechanisms studied in a large and expanding literature, following the seminal work of Abdulkadiroğlu and Sönmez (2003).[6] 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., 2019).[7] The general findings relevant for our analysis are that

[6]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 a ranked list of schools that does not violate ones' true preferences.

[7]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

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.

A large literature employs a *border discontinuity design* 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 prices reflect neighborhood composition as well as school composition and test scores. 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. The lack of effect from value-added mirrors our findings. We add to these studies by showing that the real estate markets only reflect a part of the behavioral responses to school composition. Not accounting for other margins of defiance leads to an overestimation of the efficacy of redistricting.

2 Institutional background

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. Municipalities run the public primary schools and decide on the level of funding on a municipal level. Public schools are free and parent co-payment is forbidden by law. There are 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 attendance zones (or school districts) associated with these schools. Every residential address is associated with exactly one school, which we refer to as the *district* school.[8] Children

(Burgess et al., 2011).

[8]These attendance zones are called districts. Each district has one school; thus there is no distinction between attendance zones and school districts in Denmark.

have a right to enroll in their district school and, once enrolled, a child is not affected by future district changes (with the exception of mergers and closures).[9] The municipal council is free to choose its priorities when constructing the attendance zones. Changes in attendance zones are common and are often used as a way to manipulate student body characteristics, especially in large urban areas such as the Copenhagen metropolitan area, Aarhus and Odense.[10]

If parents do not want to comply with school assignment they have two options: other public schools with spare capacity or a private school. Parents can enroll their child in other public schools than the district school free of charge if the desired school has "sufficient capacity". This is usually defined by a cap on class size and the total number of pupils in a school.[11] The process is decentralized and with practically no reporting requirements. Thus, little is known about the fairness and legality of the process by which children from outside the attendance boundary are prioritized for admission. Children are free to change school during the school year which creates the possibility that an initially oversubscribed school may become accessible for outside-district children if a family moves away.[12]

The other margin by which parents may defy school assignments is private school. Private primary schools are prevalent in Denmark. Around 42 percent of children aged 7 have a private school within 2 km of their home and 85 percent

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

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

[11] 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 attendance zone can no longer enter. If a school receives more applications than its capacity, then the children outside the attendance zone should be admitted according to objective criteria. The Danish Ministry of education recommends distance and sibling preference as a criterion. See guidelines on the Danish school choice system https://www.uvm.dk/folkeskolen/fag-timetal-og-overgange/skolestart-og-boernehaveklassen/frit-skolevalg.

[12] The chance of admission therefore 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.

have one within 10 km, see Figure B.1a in the appendix for full distribution. Private schools are non-profit and receive funds from the government covering on average 75 percent of the average cost of a pupil in public school. Parents cover additional costs. Danish private schools are free to set their own price, and parents typically pay 130-270 euros a month per child with possible discounts for siblings.[13] Private schools choose themselves who to admit and thus have no attendance zones. Popular private schools have waiting lists and parents can sign up their children up early in the life of their child. Anecdotal evidence suggests that private schools, especially in urban areas, tend to be vastly oversubscribed. Parents, therefore, cannot be sure to exploit private schools as an alternative to the district schools, as enrollment is contingent on being on a waiting list and being admitted. The overall enrollment in private schools increased from around 12.5 pct. in 2007 to 16.4 in 2016. The increase was mainly driven by rural areas, see Figures B.1b and B.1c in the appendix.

3 Data and measurement

Our sample is based on Danish registry data for the years 2008-2015.[14] From Statistics Denmark we obtain detailed information on household income, education, ethnicity, and educational enrollment. We link these records to detailed geographical information on over 95 percent of households in Denmark.[15] The median polygon has a size of 0.08 square kilometers with larger precision in dense urban areas. We obtain attendance zone data from records in the CPR-vej-register. These are reported by the municipalities themselves and are not verified by Statistics Den-

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

[14]The data go 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.

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

mark.[16]

Our sample contains children at age 5 who live within an observed attendance zone *and* are enrolled in primary school at age 7 during our sample period.[17] We require observations at age 5 in order to measure school characteristics and household covariates before the children possibly experience changes to their school attendance zone and/or enroll in primary school. We use data on outcomes, i.e. enrollment of the child and residential location of the household, for the year the child turns 7. In section 5.3 we also exploit a sample of younger children to investigate delayed responses.

We are interested in the heterogeneity of behavioral responses with respect to socioeconomic background. For ease of interpretation, we construct a household socioeconomic index (henceforth, SES). We define this index as the first component from a principal component analysis (PCA) on income rank, an employment dummy, and dummies for long-cycle education among the parents. We then rescale the PCA index such that each household is measured by its quantile rank; as a consequence, the resulting distribution is uniformly distributed and bounded on the unit-interval. As expected, our SES-index increases with income, employment, and long-cycle education. Appendix A describes this SES-index in detail. We measure ethnic background with a dummy for being a non-Western immigrant, descendant or child of descendants (up to the third generation).[18] Table 1 presents descriptive statistics for our sample of children.

To characterize schools we primarily use the average socioeconomic status (SES) among the children enrolled. We refer to this measure as school-SES. We complement this measure with two additional measures; ethnic composition measured as

[16]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 for plotting. For all regression we use StataCorp (2017).

[17]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. Note that some parents defer enrollment until their children turn 7 years old.

[18]In order to be a non-Western descendant *both* parents must be non-Danish. The same goes for children of descendants. Thus, one Western parent is sufficient to be of Western descent.

Table 1: Descriptive statistics for children and schools

	Count	Mean	Median	Std.
A. Children				
Socioeconomic status [SES]	578,903	0.50	0.50	0.29
Non-western [NW]	581,457	0.13		
B. Public schools				
Socioeconomic status, average	9,332	0.47	0.46	0.10
Non-Western, share	9,332	0.09	0.05	0.13
School Value-added	3,714	0.04	0.00	0.34
C. Private schools				
Socioeconomic status, average	3,413	0.49	0.51	0.16
Non-Western, share	3,412	0.13	0.04	0.25

The table presents a descriptive statistics at the level of children and schools. The children are measured at year 7 while schools are represented once per year.

the share of non-Western immigrants (abbreviated NW), and, school value-added (SVA). As with school-SES, we measure NW-share as the share 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 available on the ministry website. SVA is calculated using a mixed-effect model which we have serious methodological concerns with.[19] Even though these measures might be biased they are a salient part of the information set of households. We, therefore, use this measure of value-added even though we doubt its validity.

An important factor for the choice of school is the geographical distance (see Abdulkadiroğlu et al. (2019) for an example). We calculate the Euclidean distance between home and schools from the centroid of the geographical polygon, in which the household lives. Descriptive statistics for public and private schools are presented in Table 1.

[19]The outcome is (uncentered) grades in the final exam in grade 9, which corresponds to 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 the SVA measured on grade averages across disciplines.

Changes in attendance boundaries The administrative procedures of changing attendance boundaries differ between Danish municipalities. The changes are usually announced no more than a year before they occur, usually in the spring before the beginning of the school year in August.[20] Proposals for changes are usually made by administrative staff at city hall and are subject to confirmation by the city council. The changes most often occur due to changing demographics which induce shifts in the demand for primary schooling.[21] For all residential locations, we record changes in formal school 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 that 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 attendance boundary change of zero implies that the change occurs between the ages of 6 and 7 of the child. A distance in years of 1 means that the change occurs between the ages of 5 and 6. We exclude all attendance boundaries wherein no address is shifted at any point in our sample.

We calculate changes in Euclidean distances following a change in boundaries from the place of residence when the child is five years old to the original and the new reassigned school. When a boundary changes, the distance changes differently for each household depending on the place of residence. Therefore, we have more variation in distance changes than in our other school measures, which are the same for all households in the attendance zone.

To illustrate the variation that we use to identify behavioral responses we consider Hvidovre. This municipality is located in the Copenhagen metropolitan area

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

[21] When a proposal of a change is made, citizens and schools may voice concerns, which sometimes turns into heightened, local political tension. 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. Anecdotal evidence suggests this is mostly due to the closing of schools as opposed to tiny changes around boundaries. 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.

with approximately fifty-thousand residents. The two maps in Figure 1 illustrates how the attendance boundaries were redrawn between 2011 and 2012 when two public schools closed down.[22] To allocate the children living within the attendance boundary of the now-closed schools, other boundaries were 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 that in 2012 lived in the areas surrounded by dotted lines in Figure 1b. Secondly, other households were reassigned to new schools that they had not expected although their originally designated school did not close. These areas are shown in solid lines in Figure 1b. As a source of variation to estimate the behavioral responses, we only use boundary changes for addresses outside the attendance zones of the closed schools.

4 Margins of compliance and household sorting

A Danish household with a child ready to enroll in primary school has several options. The household may simply choose to enroll the child in the district school, thereby complying with the default option. We describe this by a binary variable, denoted *Comply*. This option is guaranteed by law and therefore always available. When households do not enroll in the district school we group their choices into three mutually exclusive categories: enroll in another public school (*Public*); enroll in a private school (*Private*) or move to another attendance zone (*Move*). Summing gives us the following identity:

$$Comply = 1 - (Public + Private + Move), \tag{1}$$

where it is implicit that we regard a move as a single action, regardless of whether the child ends up attending the assigned school in their new location. Each of

[22]The two closed schools are Sønderkærskolen, in the North-East, and Enghøjskolen, the Western part.

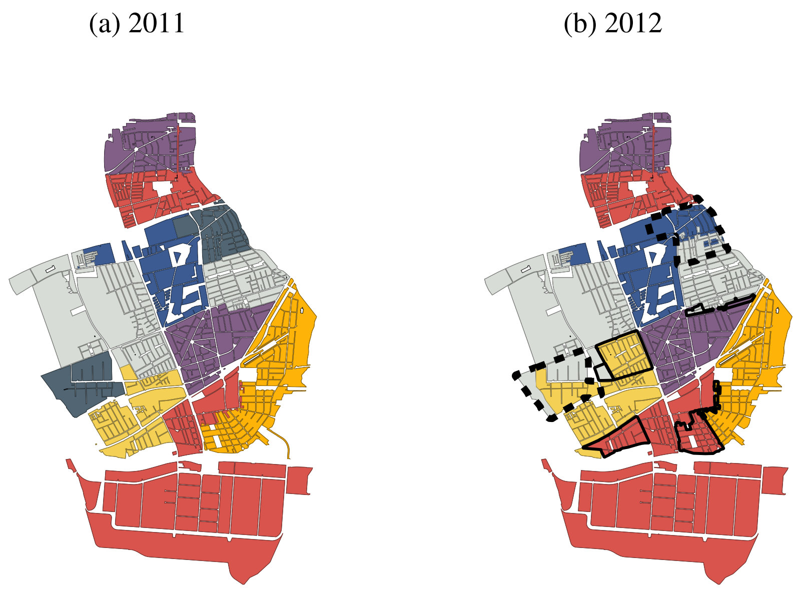


Figure 1: Attendance boundary changes in the municipality of Hvidovre

The figures depict the attendance zones in the municipality of Hvidovre in the autumn of 2011 and 2012. The hatched areas in 2012 show the area of two closed schools. In order to enroll students who would live in districts of the now-closed schools, Hvidovre also changed the other boundaries. Only the latter changes are used for identification. See section 3 for a description of the attendance zone data. Some areas differ from the official documentation. These areas are mostly not populated but some measurement error occurs. In the analysis, we use addresses directly to bypass mismeasurement of geographical entities. The district polygons are only used to measure distances to boundaries.

the elements in (1) correspond to a dependent variable and we use this identity to decompose responses along the three margins of defiance.

The propensities for different margins of non-compliance differ along household-SES in the cross-section, as evidenced in Figure 2. Whereas low-SES households tend to send their children to other public schools or to move, private school is the preferred outside option among high-SES households. As low-SES households have higher aggregate rates of defiance in the cross-section than high-SES households, the former might be expected to be more likely to defy reassignment policies than the latter. However, this interpretation overlooks residential sorting as a poten-

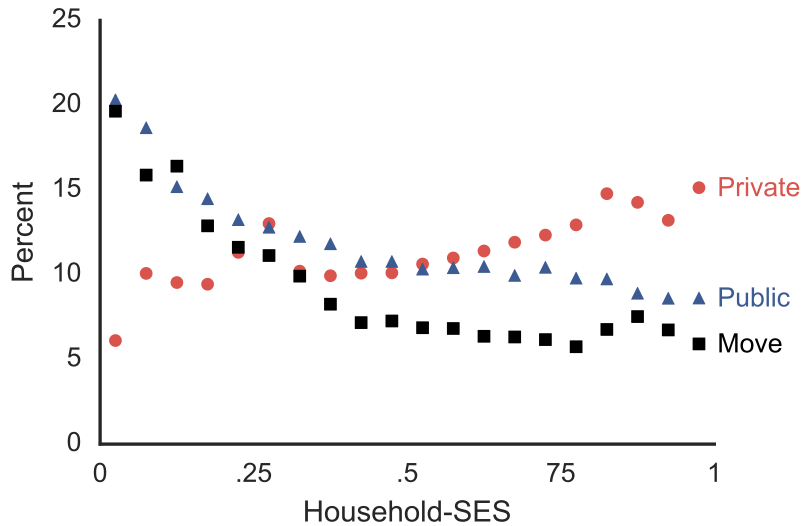


Figure 2: Margins of defiance by household-SES

The figure shows simple means of three component of Equation (1) within vintiles of household-SES within the cross-sectional sample.

tially important factor in explaining non-compliance. If high-SES households are able to locate within attendance zones of more desirable schools, they will have less incentive to opt out of assignment. As such, an initial residential location decision may serve as a substitute to the other margins presented in figure 2.[23]

To investigate whether residential location decisions serve as a substitute to private school and other public schools we construct graphs in the spirit of the Boundary Discontinuity Design originally introduced by Black (1999). We follow Bayer et al. (2007) and compare households in two adjacent school attendance zones, where we define the "left" school as the school with lower school-SES and vice versa for the "right" school. We construct bins of distances such that the addresses located in the lower-SES school attendance zone have negative distances to the

[23] Importantly, in absence of boundary changes, locating within an attendance zone guarantees enrollment in the associated school. Locating within the attendance zone of a desirable school may, therefore, be a less risky choice than getting on the waiting list of a private school or bet on admission to a non-associated school. This location decision is presumably what drives the positive effects of measures of school quality on house prices found across institutional settings.

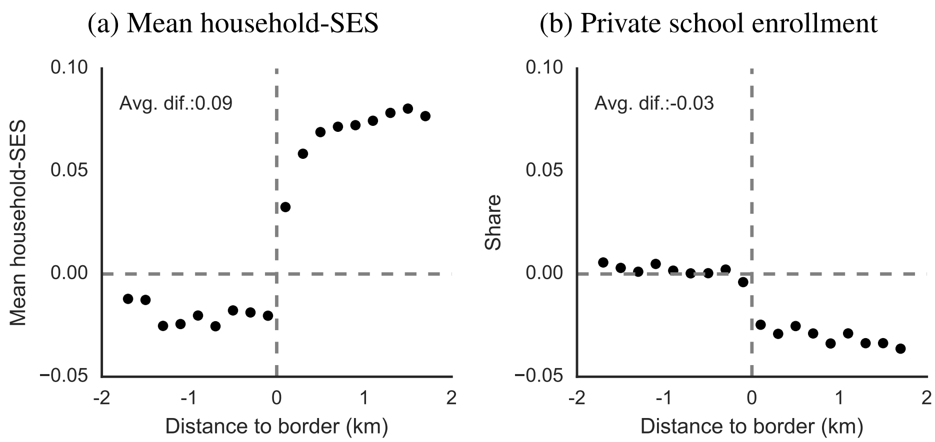


Figure 3: Differences at boundaries between low- and high-SES schools

The figures depict the boundary discontinuity estimates for household-SES and private school enrollment. The dependent variables are displayed in the figure titles. Negative distance to boundary signifies that the household is situated in the district of the two adjacent districts with the lower value of the school characteristic. The models are estimated with fixed effects at the boundary-year level.

boundary and addresses in the high-SES attendance zone have positive distances. We then run regress the outcomes on binned distances and boundary-year fixed effects.[24]

Figure 3a shows a clear discontinuity in household-SES at the boundary.The average difference in household-SES between two zones is 9 points, roughly equivalent to one standard deviation of school-SES. If boundaries were drawn at random this would be a clear indication that high-SES schools attract high-SES households. Naturally, causality also flows the other way; high-SES households send their children to the local school creating a mechanical link between household-SES and school-SES and boundaries may reflect initial residential segregation. However, if parents do not value school-SES, we would expect private school enrollment to be higher in the area where more high-SES households locate in accordance with the cross-sectional evidence in Figure 2. However, the opposite is the case as evidenced by the discontinuous drop of three points in private school enrollment at

[24]By including the boundary-year fixed effects, we compare only within boundary regions in the same year. We center our results on a the left side, i.e. the side with the lower school-SES.

the boundary in Figure 3b. In other words, even though high-SES households disproportionally relocate to the right side of the boundary, we observe *lower* private school enrollment. This provides evidence that relocating to high-SES districts may serve as a substitute to private school.

We provide further evidence of sorting and enrollment around the boundary discontinuity in Appendix B. For instance Figures B.3b and B.3d provide indications that families who reside in low-SES zones are more likely both to use other public schools and to move away.[25] Furthermore, there is evidence that the decrease in private school use and moving propensity within the affluent attendance zones is driven by highest SES group whose boundary response is respectively 5 and 3.5 times greater than that of lowest SES, see Figures B.3c and B.3d. Finally, we show that the magnitude of non-compliance increases with difference school-SES and is evident for all margins in (1) see Figure B.4. In other words, the larger the difference between schools, the larger differences in behavior we observe at the boundary.

However, due to the simultaneity between residential segregation and school segregation, the Boundary Discontinuity Design does not provide estimates of the relevant policy-counterfactuals of changing school affiliations.[26] Rather than providing estimates of the causal effects of school characteristics on enrollment choice, the discontinuities in Figure 3 show that residential sorting is inherently interconnected with school segregation. Estimating responses to desegregation policies without conditioning on initial residential segregation is, therefore, not a viable strategy. In the next section we therefore exploit changes in the geographical shape of attendance zones over time to estimate the causal effects of this exact policy while allowing us to condition on household sorting.

[25] We attribute the large spike in use of other public schools around the boundary as driven by preferences for school proximity - living closer to the boundary implies higher propensity to substitute to the neighboring schools.

[26] If Figure 3a would be interpreted as balancing tests, the Boundary Discontinuity Design would clearly not be valid. However, if households sort on school characteristics, the household characteristics would be a function of school characteristics and, therefore, a function of the "treatment". In the latter case, controlling for household-SES would constitute a "bad control". We observe heterogeneity across household SES in responses to school characteristics.

5 Behavioral responses to changes in boundaries

To obtain causal estimates of behavioral responses to reassignment policies, we exploit the changes to attendance boundaries that occur regularly in Danish municipalities. Conceptualizing these boundary changes as natural experiments, we compare households that are affected by boundary changes to those that are not. Unaware of future changes, two households can relocate to the same attendance zone but end up allocated to two different schools. If the timing of the changes in boundaries is sufficiently random, as we argue below, we can attribute changes in differences in behavior over time to changes in school affiliation. In other words, the boundary changes allow us to estimate the behavioral effects in a difference-in-difference design.

To understand where policies for redrawing boundaries were enacted, we examine whether characteristics for municipalities and schools are predictive of the propensity to redraw boundaries, see Table 2. From column (1) we see that municipalities with denser populations, i.e. urban areas, are more likely to change school boundaries. Within municipalities the likelihood of reassignment for schools is governed mainly by variation in SES within the school as well as the mean and standard deviation of population density within attendance zone, see column (2). We complement this macro-level analysis of treatment by examining treatment exposure for children in Subsection 5.3. This checks our assumption of randomness by examining differences in children SES within school conditioned on treatment by boundary changes, see Table 6.

5.1 Responses to positive and negative shocks

We estimate household responses to changes in school affiliation in a difference-in-difference framework. We begin the difference-in-difference analysis by investigating the responses over time. To capture the effect from characteristic changes separate from the "pure" effect from a surprise change we construct a treatment indicator for all addresses, which are shifted. We categorize the changes in school SES resulting from boundary changes into three treatment groups: positive, with higher school-SES in the new district school compared with the old; negative, which have

Table 2: Treatment propensity and characteristics for municipalities and schools

	(1) Municipal level	(2) District level
SES (mean)	0.556	0.101
	(0.443)	(0.160)
SES (std. dev.)	1.690	1.378
	(2.138)	(0.493)
Pop. density (mean)	0.0810	0.0197
	(0.0359)	(0.00979)
Pop. density (std. dev.)	-0.104	0.166
	(0.110)	(0.0403)
N	83	1047
F.E.	-	Municipal
Clusters	-	Municipal

This table contains models of the treatment indicator as outcome explained. The explanatory variables are descriptive statistics for municipalities and schools.

lower school-SES; and, "negligible" change. The latter category is taken as reference. We estimate the responses using Ordinary Least Squares (OLS) specifications of the following form:

$$\begin{aligned} Y_{iast} &= \alpha_T T_a + \alpha_- T_a^- + \alpha_+ T_a^+ \\ &\quad + \sum_{\tau=-4,\tau\neq-1}^{4} \left[(\beta_T^\tau T_a + \beta_-^\tau T_a^- + \beta_+^\tau T_a^+) \cdot \mathbf{1}(t_a = \tau) \right] + \mu_{st} + \varepsilon_{iast}, \end{aligned} \tag{2}$$

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 attendance zone, s.

We include a fixed effect for all combinations of original attendance zones and year thereby ensuring that we compare households who sought to locate within the same attendance zone. As we only compare addresses within the same original attendance zone and therefore the same municipality, we ensure equal levels of taxation and overall school funding. Let t_a be the year in which boundary a is changed. We focus on the three following indicator variables, corresponding to our three treatment categories: T_a if treated, T_a^+ for positive treatment and T_a^- for negative treatment. We center our estimates in the year before the change of boundaries.

We are interested in comparing addresses which were both within the same year and within the same attendance boundary prior to the change. We implement this comparison by including a fixed effect for the original attendance boundary s at time t. We are mainly interested in the coefficients on the interactions, β_T^τ, β_-^τ; and, β_+^τ, as they are associated with the treatment groups. The coefficients for $\tau < 0$, i.e. prior to the change, serve to assess parallel pre-trends.[27]

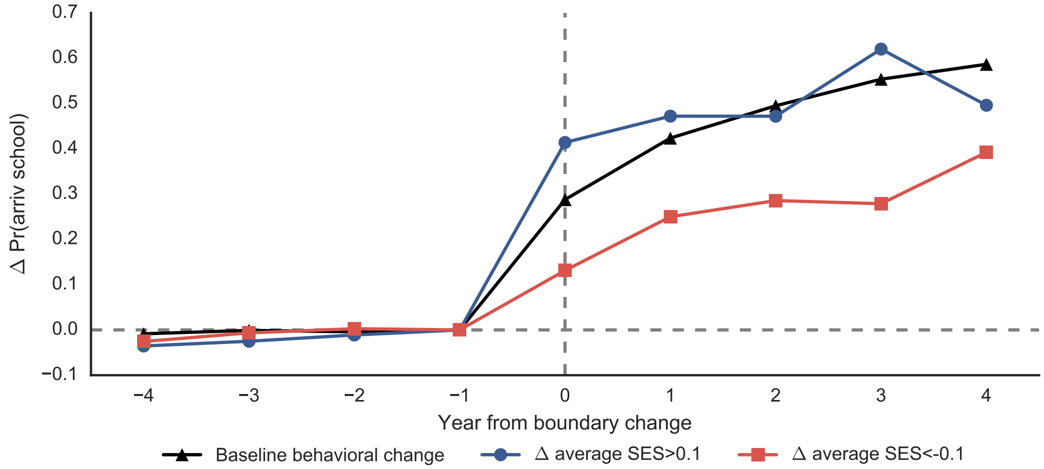
Although we have multiple measures of school characteristics, we focus only on school-SES in this subsection. In Appendix B we repeat this analysis using share with non-Western heritage and school value-added, see Figure B.6.

We estimate our model for enrollment in the assigned school after a change in the attendance boundary.[28] The black line in Figure (4a) depicts the change in the probability of enrolling in the new school as a function of time from the boundary change. We compare the enrollment probability to the households from the same original attendance zone 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 points, which is well within the range of common enrollment rates.

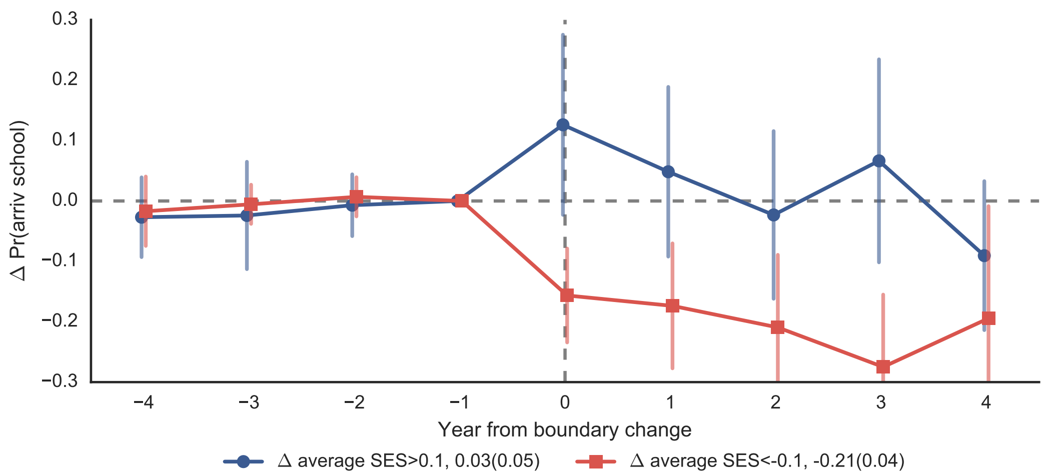
The red and blue lines depict the enrollment propensity when the change in average SES is numerically larger than 0.1, representing a standard deviation of the school level SES distribution. If households experience a positive change in school-level SES of more than a standard deviation, the compliance rate rises by around 10 percentage points in the first year which, however, is insignificant as seen figure 4b. 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. However, if the average school-SES *falls* by

[27] The specification in Equation (2) implies that β_-^τ and β_+^τ are interaction terms describing the intensity of treatment. In other words, the parameters describe how the mean effect of a change in SAB is affected by the change in characteristics.

[28]For the non-shifted addresses within a SAB, we assign arrival school as the arrival school of those that are shifted. A few SABs 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 4: Compliance response to a change in SAB by school characteristic change

Figure 4a display changes in estimated compliance rates based on the model in (2). The black lines depict estimated coefficients of β_T^τ, while the blue and red line depict coefficients of $\beta_T^\tau + \beta_-^\tau$ and $\beta_T^\tau + \beta_+^\tau$ respectively. Figure 4b displays the interaction terms, β_-^τ and β_+^τ, 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-attendance-zone"-year fixed effects. Standard errors are clustered on 'origin-attendance-zone"-level. Results are centered at the year before the boundary change. Estimates from a simple before-after-DID are reported in the legends of figure 4b.

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.

As we observe completely flat pre-trends, this provides evidence of a causal relationship between school characteristics and compliance with the school assignment mechanism.

5.2 Estimating marginal effects

To measure how households respond to boundary changes we decompose the compliance rate into the possible margins of response. We simplify the estimation by collapsing the time dimension and employ a two-period difference-in-difference model with *continuous* treatment.

Let $\Delta Q_{ss'} = Q_{s'} - Q_s$ be the change in school characteristic when reassigned from schools s to s'. We record $\Delta Q_{ss'}$ in the year before address a is transferred to the attendance zone of school s'. We estimate models of the following form:

$$\begin{aligned} Y_{iass't} = {} & \alpha_0 T_a + \beta_0 \Delta Q_{ss'} + \alpha_1 T_a \times Post_{at} + \beta_1 \Delta Q_{ss'} \times Post_{at} \\ & + \mu_{st} + \varepsilon_{iass't}, \end{aligned} \tag{3}$$

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. First, by including the changes as a continuous measure in the regression we obtain estimates of marginal effects, which can be used for prediction. Second, we can control for other changes in regard to school assignment occurring simultaneously, most notably changes in distance to the district school. Finally, we can include multiple school characteristics simultaneously in the model, though we will discuss the somewhat subtle changes in interpretation when multiple measures are included. When we estimate models of the type specified in (3) 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 Equation (3) 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 on average 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 negative and statistically significant for all specifications implying that the further a child must travel to the district school the lower the 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 are highly significant. A standard deviation increase of average school-SES entails an increase in compliance of around $(0.7 \times 0.1) \times 100 \approx 7$ percentage points, which corresponds to roughly 20 percent of the baseline first-year increase in enrollment of 33 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.[29]

Interpretation of partial effects Column 5 of Table 3 includes all school characteristics in a single regression. While the coefficient on value-added changes little, we see substantial changes in the parameters on school-SES and NW-share. These changes in coefficients are due to a large, negative correlation (=-0.84) between changes in school-SES and changes in the NW-share. 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 share with Non-Western heritage as a proxy for school SES.

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

[29]The coefficient on distance, however, doubles when SVA is included, due to a large correlation between the SVA and distance. This is most likely due to urbanization being an omitted variable in the model for measuring SVA.

Table 3: Compliance as a function of SAB change

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

Columns 1-3 display regression results for the model presented in Equation (3) 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-attendance-zone"-year fixed effects. Standard errors are in parentheses and clustered on 'origin-attendance-zone"-level.

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 combined 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.[30] 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.[31]

[30] 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:

$$\begin{aligned} y &= \beta SES^* + \varepsilon \\ SES^* &= SES + u_1 \\ SES^* &= -\rho NW + u_2, \end{aligned}$$

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.

[31] This is naturally a product of the Danish context. The correlations may differ between countries

We, therefore, proceed to focus mainly on the 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 households may defy the assignment mechanisms through different choice margins. Because we investigate unexpected changes to boundaries, we suspect that an important margin is to enroll in the school of the original attendance zone. We, therefore, expand the public school margin, such that the original district school is measured separately. Equation (1) is therefore modified to encompass four means of non-compliance:

$$Comply = 1 - Original - Public - Private - Move, \tag{4}$$

where *Comply* takes the value of one if the child enrolls in the assigned district school, *Original* denotes the departure school and *Public* now denotes a public school different from original school *and* the newly assigned school. *Private* denotes private school enrollment and if the family moved out of the district between ages 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 (4) as dependent variable.

The results are displayed in the top panel of Table 4. Column 1 reproduces column 2 of Table 3. In accordance with Equation (4) the remaining columns approximately sum to (minus) the first column. Changes in compliance primarily stem from the publicly provided option of choosing other public schools, measured by the estimates in the columns *Original* and *Public*. 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 *Public* column in the top panel of Table 4. An additional source of increased compliance is the decrease in private school enrollment, seen in the *Private* column, though this effect is only statistically significant at a ten-percent confidence-level. The increase

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.

in compliance is, however, attenuated by an increase in the propensity to stay in the original district school. We show below that this response along the *Original*-margin stems mostly from low SES households staying behind.

Positive and negative changes in school-SES We previously found that the response to boundary changes depend on whether households are transferred to stronger or weaker attendance zones in terms of school-SES. To further elaborate on this heterogeneity we interact our continuous measure of change in school-SES with an indicator with the sign of this change. The results are shown in the middle panel of Table 4. Consistent with the previous findings in Figure 4, we find that the responses are larger for negative shocks. For overall compliance, the responsiveness to school-SES is 80 percent larger when households are faced with a negative change compared to a positive change in school-SES. This difference in sensitivity is driven by the *Public* and *Move* margins, though the latter is insignificant. This implies that a policy of moving children out of poor schools is more effective than moving children into poor schools.

Household heterogeneity in responses - SES Preferences and constraints likely differ at the household-level. This would lead to heterogeneity in responses to changes in SABs. To elicit this, we interact the model presented in Equation (3) with household-level characteristics. We begin by interacting the model with the SES quartile of the household. The effect on overall compliance along with different margins of response is shown in the lower part of Table 4. The basic reaction to an attendance boundary change given zero change in school-SES along with responses to changes in distance exhibit no heterogeneity, as evidenced by the first eight rows.

Yet, we find a high degree of heterogeneity in responses to changes in school-SES. In the *Comply* column the coefficient on school-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

Table 4: Responses along different margins

	Comply	Original	Public	Private	Move
	– Constant homogeneous responses –				
T × Post	0.33	-0.13	-0.18	-0.03	0.01
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Δ Dist × Post	-0.03	0.01	0.04	-0.02	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Δ SES × 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
	– Positive and negative changes in school-SES –				
T × Post	0.35	-0.12	-0.20	-0.03	0.00
	(0.03)	(0.03)	(0.03)	(0.02)	(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^+ SES × Post	0.49	0.39	-0.62	-0.20	-0.06
	(0.30)	(0.32)	(0.25)	(0.15)	(0.20)
Δ^- SES × Post	0.88	0.40	-0.95	-0.18	-0.15
	(0.23)	(0.24)	(0.19)	(0.21)	(0.14)
N	53,426	53,426	53,426	53,426	53,426
	– Household heterogeneity in responses –				
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

Columns display regression results for the model presented in Equation (3) using SES as a measure of schools with margins of response displayed in the columns title. The top panel are models estimated with homogeneous compliance both in sign of change and household-SES. The middle panel are models with positive and negative schocks while the lower panel shows heterogeneity in in responses for household-SES quartiles where the first quartile is baseline. The models are estimated with 'origin-attendance-zone"-year fixed effects. Standard errors are in parentheses and clustered on origin 'origin-attendance-zone"-level.

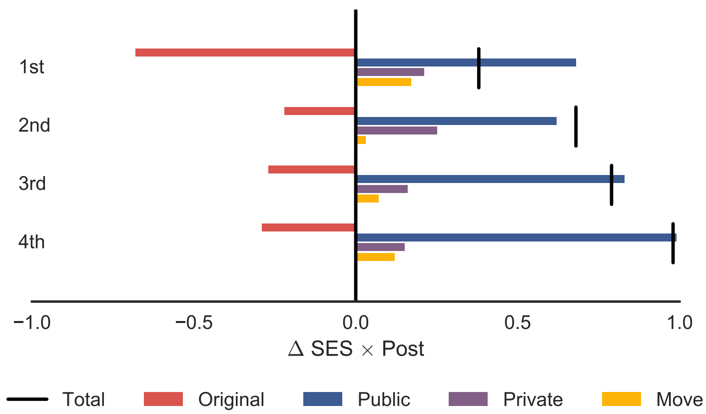


Figure 5: Responses at each choise margin by SES quartile

The figure displays responses to school-SES for quartiles of household SES. The estimates and standard errors can be read off Table 4.

is around 2.5 times larger for the highest quartile compared to the lowest quartile.[32] For ease of interpretation we display the magnitudes for each quartile separately in Figure 5. As can be seen in this figure, the relatively low estimate for households in the first SES quartile is explained by a large tendency to stay behind in the "original" school.

5.3 Further heterogeneity in responses and robustness

In this section, we present further results. First, we focus on younger children, as their parents have a longer time span to plan enrollment and thus may be less constrained in their outside options. We then investigate whether responses differ depending on the birth order of the child. We briefly investigate heterogeneity in responses depending on household ethnicity and lastly check for endogenous sorting following treatment.

Time to enrollment If households want to defy assignment to a specific school (or to opt-in) time may be an important determinant for the options available. For

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

example, the small response along the private school margin found above may be due to parents being unable to get their children on the waiting list. If this is the case, responses along this margin should be larger for younger children who would enroll later. To investigate this, we construct two new samples containing children who were 3 and 4 years old respectively at the time of the boundary change and reestimate the model in the middle panel of Table 4. We once again split the treatment according to whether the shock is positive or negative. The results of this exercise is illustrated in Figure 6 where the rightmost points correspond to the estimates reported in the middle panel of Table 4. The estimates for younger children tend to be imprecise but the point estimates suggest that parents act along different margins depending on the time before children reach schooling age. The difference-in-difference plots with multiple periods before and after also support this conclusion as the pre-trends seem flat, see Figure B.7c.

If parents are faced with a fall in school-SES, the overall compliance sensitivity is more or less independent of the age of the child at the time of boundary change as evidenced by the red points in the top-left panel of Figure 6. However this masks differences in the sensitivity on the different margins, whereby households can defy assignment. The more time to adjust the more likely are children to enroll in private school, but only for negative shocks. This difference according to the sign of change is intuitive, as opting *out* of private school is always an option, while opting *in* is not. If given less time to adjust, on the other hand, households are more likely to enroll their child in another public school, than the one they were assigned to. Though the effects are insignificant, these patterns provide indications that the short time-span in our main estimations constrain parents and therefore explain the lack of response along the private school margin for 5-year-olds. We note that higher use of private school is consistent with the boundary discontinuity analysis which established strong discontinuites in private school at the boundary, see Section 4.

The birth order of children Municipalities generally allow younger siblings to enroll in the school of the older sibling, even if attendance boundaries change. This implies that households with older children have strictly more options than those without. Yet, households with older children will face logistical challenges if the

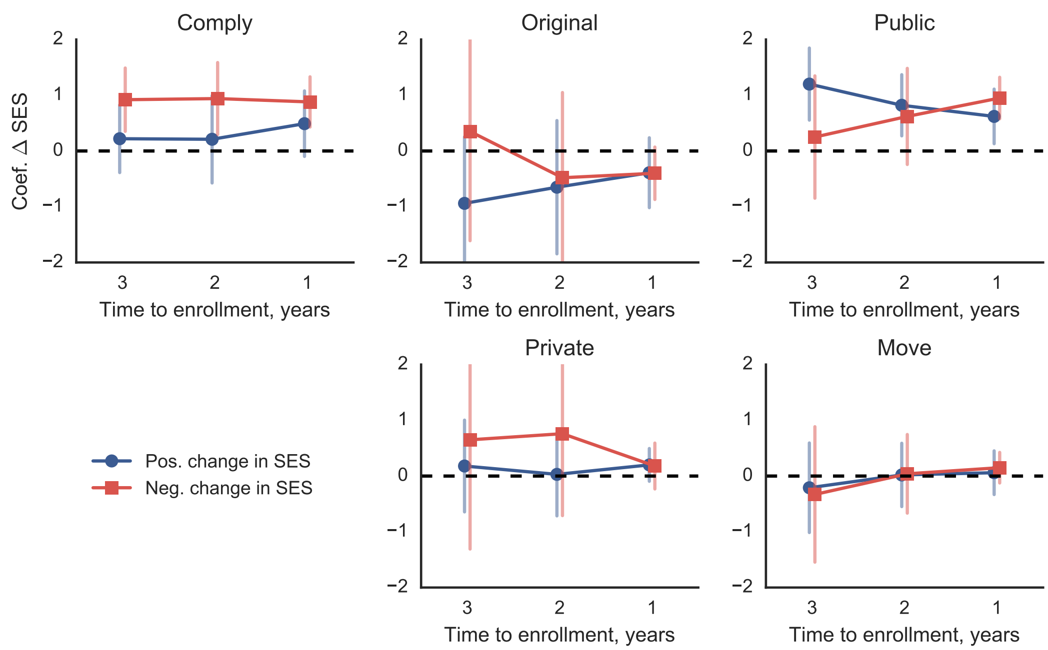


Figure 6: DID-estimates as a function of time to enrollment

The figure displays the estimated sensitivity to changes in school-SES for children at different ages. The younger the child, the longer time to exploit margins. The estimates at 1 year, ie. 5-year-olds, are the same estimates as reported in the middle panel of Table (4). For ease of interpretation, we flip the sign for the margins, such that the sum of effects for a given age sum to the estimate on "Comply". The models are estimated with 'origin-attendance-zone"-year fixed effects. Standard errors are in parentheses and clustered on 'origin-attendance-zone"-level level

younger child enrolls in another school. As a consequence, we should expect households with older children to be less inclined to comply with the assignment and instead enroll their child in the "original" school but the logistical constraints may make these households less likely to exploit other options of defiance.

To investigate this heterogeneity, we split our sample according to whether the child is the oldest child or not and reestimate the models in the middle panel of Table (4) for the two samples separately. The results are displayed in Table 5. The results confirm our expectation that children stay in the original school, which is likely where their older sibling(s) enrolled. While the probability of enrollment in the "original" school falls by 19 percentage points after a boundary change for those without siblings, the corresponding fall for those with older siblings is 6 percentage

Table 5: Results, split by being an older child

	Comply	Original	Public	Private	Move
	– No older siblings –				
T $\times$ Post	0.39	-0.19	-0.20	-0.01	0.01
	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)
Δ Dist $\times$ Post	-0.03	0.01	0.04	-0.02	0.00
	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
Δ^{+} SES $\times$ Post	0.42	0.33	-0.26	-0.29	-0.20
	(0.43)	(0.43)	(0.27)	(0.34)	(0.35)
Δ^{-} SES $\times$ Post	0.79	0.43	-1.11	-0.07	-0.04
	(0.26)	(0.31)	(0.21)	(0.27)	(0.22)
N	22.352	22.352	22.352	22.352	22.352
	– Have older sibling –				
T $\times$ Post	0.31	-0.06	-0.20	-0.04	-0.01
	(0.04)	(0.04)	(0.04)	(0.03)	(0.02)
Δ Dist $\times$ Post	-0.03	0.01	0.04	-0.02	0.01
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Δ^{+} SES $\times$ Post	0.52	0.50	-0.92	-0.11	0.01
	(0.28)	(0.38)	(0.54)	(0.21)	(0.16)
Δ^{-} SES $\times$ Post	0.97	0.29	-0.84	-0.21	-0.21
	(0.28)	(0.40)	(0.31)	(0.29)	(0.14)
N	24.692	24.692	24.692	24.692	24.692

The table show the results of splitting the sample according to whether the child is an older child or not. The models are estimated with ‘origin-attendance-zone”-level-year fixed effects. Standard errors are in parentheses and clustered on ‘origin-attendance-zone”-level.

points and insignificant.

Households with older children are in general more responsive to school-SES, than those households, where the oldest child who is about to enroll. Yet, the differences between the two groups of households are insignificant for both positive and negative shocks. The larger responsiveness stems primarily from a larger tendency to exploit private schools and to move out of the district, though both estimates remain insignificant. Households do, however, not seem to exploit the margin of staying behind, when school-SES changes.

When it comes to enrolling in other public schools, it may be more inconvenient for households to choose another school, if an older child is not already enrolled. Correspondingly, we find that households without older children are thirty percent more sensitive along this margin, when experiencing a negative shock to school-SES. However, when the shock to school-SES is positive, households with other siblings are more responsive. This implies that households with older children exploit the opportunity of a higher school-SES when afforded to them, even though the older sibling may be enrolled in another school.

Comparing treatment and control groups In our analysis of boundary changes, we leverage the difference-in-difference method. The validity of the methods requires both that the treatment and control groups have approximately identical trends and that the control and treatment groups do not change composition over time, i.e. there is no dynamic selection. We examine differences in the two groups by measuring whether children with different levels of household-SES vary in their exposure to boundary changes and whether the average household-SES changes over time in response to these changes. Table 6 presents estimates from regression models using Eq. (3) where we limit our sample to two years prior and after boundary changes. From column (1) in the table, we see that being exposed to a boundary change cannot predict household-SES. These conclusions hold regardless of whether we include changes in distances at the household level, see column (2). Neither is the sign of change in school-SES predictive of household-SES before reallocation nor does it lead to changes in the average SES after changes, see columns (3)-(6). We note that the model with a continuous measure of SES finds a weak relationship between change in school-SES and household-SES, see columns (5)-(6). The results of Table 6 lead us to conclude that our results are not driven by changes to household composition within attendance zones, neither initially, nor over time.

Household heterogeneity in responses - Ethnicity We finish our analysis of attendance boundary changes by investigating heterogeneity in responses across ethnic groups. We find that Non-Western households has a small, negligible response to changes in the share of Non-Western, see Table B1 in Appendix B1. However, the response by native and Western descendants is stronger than in the the homogeneous from Table 4.

Table 6: Selection in socioeconomic status related to boundary changes.

	Household SES					
	(1)	(2)	(3)	(4)	(5)	(6)
T	0.01	0.01	-0.00	0.00	0.01	0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
T × Post	-0.01	-0.01	0.00	0.00	-0.01	-0.01
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
(Δ SES > 0.1)			0.06	0.05		
			(0.04)	(0.04)		
(Δ SES > 0.1) × Post			-0.06	-0.06		
			(0.04)	(0.04)		
(Δ SES < -0.1)			-0.01	-0.02		
			(0.03)	(0.03)		
(Δ SES < -0.1) × Post			-0.01	-0.01		
			(0.03)	(0.03)		
Δ SES					0.18	0.17
					(0.10)	(0.10)
Δ SES × Post					-0.11	-0.11
					(0.09)	(0.09)
Δ Dist		0.01		0.01		0.01
		(0.00)		(0.00)		(0.00)
Δ Dist × Post		-0.01		-0.01		-0.01
		(0.01)		(0.01)		(0.01)
N	53.406	53.187	53.406	53.187	53.406	53.187

This table contains models of household-SES as the dependent variable using Eq. (3). The models capture whether exposure to and quality of boundary changes are related to either the composition of children in the sample before the change occurred or whether an the change in school-SES changed the compositions in our sample. The models are estimated at the child level. Odd models numbers have distance excluded as variables while even numbered models include them. Columns (1) and (2) estimate the average household-SES given treatment status before and after the change. Columns (3)-(6) predict household-SES when including changes in school-SES induced by boundary changes (where we let untreated units have zero difference). Models (3)-(4) use discrete changes in school SES computed as two thresholds of whether change in school SES is higher or lower than of 0.1, while models (5)-(6) use continuous changes in school SES. The models are estimated with "origin-attendance-zone"-year fixed effects. Standard errors are in parentheses and clustered on 'origin-attendance-zone"-level.

6 Implications for policy

To evaluate the implications of our findings for the efficacy of boundary changes as a desegregation policy we embed our causal estimates in a simple simulation model. We model an environment with two public schools. We do not distinguish between the margins of responses and therefore assume a single outside option, distinct from the two schools, which we assume is available to all households. The total population of households has unit mass and the attendance zones have equal sizes with a mass of one half. Households are divided into SES-quartiles where households receive an SES-index in the middle of their quartile interval. To investigate the importance of initial levels of segregation, we construct distributions of neighboring school attendance zones by computing a range of feasible combinations of the four quartiles in the two districts. For each distribution of households, we calculate the mean SES of the families in the attendance zone. By construction, the absolute difference in our measure of mean attendance zone SES can take values between 0 and 0.5, where the former represents full desegregation and the latter represent full segregation.[33]

To compute school-SES from residential-SES we estimate baseline compliance in linear probability models for each SES-quartile, with compliance on the left side and residential SES on the right. The linear probability models are estimated using all available data from the years 2008-2015. We use these models to predict baseline compliance levels and in turn compute the baseline difference in school-SES.

We model a transfer of 10 percent of the parcels in the high-SES attendance zone to the low-SES attendance zone and calculate the resulting SES-change in the low-SES school. We assume that households observe the difference in baseline school-SES across the two districts and that the transferred parcels are representative of the district as a whole. Based on the baseline differences in school-SES, we construct three scenarios. First, we assume limited compliance which is inde-

[33] The maximal difference follows from the fact that the most segregated case is where the bottom half of households are all in one attendance zone with mean SES = 0.25; accordingly the two top quartiles reside in the other attendance zone which therefore has a mean SES of 0.75. The difference in residential-SES between the two zones are therefore 0.5.

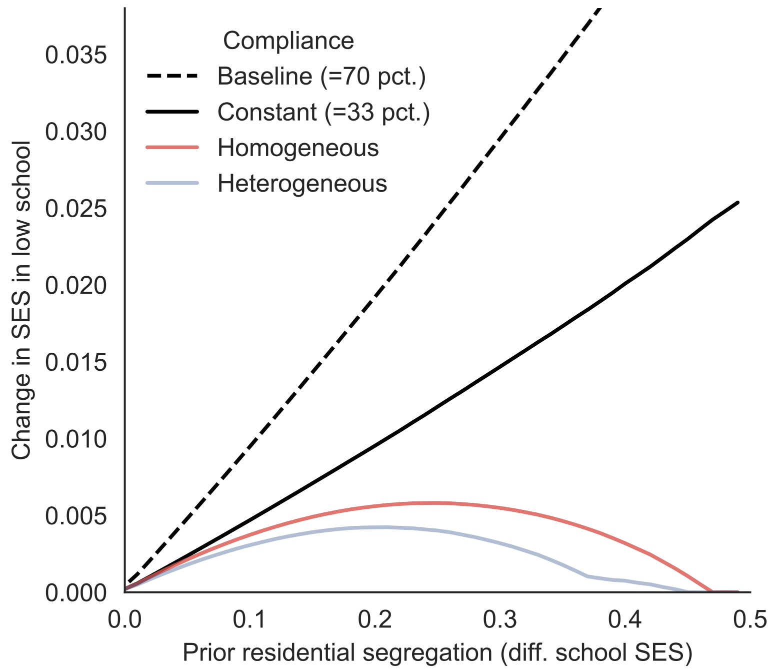


Figure 7: Simulated policy efficiency with and without behavioral responses

The figure displays the result on school SES from a change in an attendance boundary. Each location on the x-axis represents a simulated environments with a given level of residential segregation. We refer to the main text for more details.

pendent of school-SES. Second, we assume that the response to school differences for compliance rates is homogeneous across household-SES, using the estimates of the top left column in Table 4. Third, we assume heterogeneity in compliance rates based on the estimates in the bottom left column of Table 4. We restrict predicted compliance by truncating values to reside within the unit interval.[34]

The result of this exercise is shown in Figure 7. Naturally, the more initial residential segregation, the larger is the incentive to transfer parts of the affluent attendance zone to the less fortunate one. This explains the high slope of the baseline-compliance scenario in Figure 7 (in solid black). This scenario is computed using

[34]Though compliance is estimated in a linear probability model we restrict the predicted compliance to be between zero and one. The assumption of baseline compliance, therefore, is important as it limits the degree of response in either direction.

the average national compliance of approximately 70 pct. However, our analysis shows that the compliance rate after reassignment is on average 33 pct., which is substantially lower the national compliance rate. By construction, the achieved increase in school-SES is a fixed fraction of the intended increase.

Assuming homogeneous behavioral responses to the change in school-SES, the slope is still positive for the first half of the range of residential segregation. In other words, the policy succeeds in changing the social composition of the low SES-school. However, at sufficiently high levels of residential segregation between attendance zones, the expected gain is reduced. This is due to the difference in baseline school-SES. When this difference becomes large, households exploit the outside option. At the extreme end of the segregation scale, the expected gain is zero.

In the scenario with heterogeneous responses, the inefficacy of redrawing boundaries becomes even more pronounced. As in the case with homogeneous responses, household compliance fall with the baseline difference between the two schools. The high-SES households, however, opt-out at smaller changes in school-SES and the mass of complying students become socioeconomically weaker, further negating the expected gains to school-SES in the challenged school. In the words of Okun (1975), the policy of changing attendance zone boundaries is akin to a leaky bucket; the policy does not achieve the intended desegregation, as households exploit the availability of outside options. Where the degree of segregation is large, i.e. where desegregation may be most desirable, a policy of boundary changes is almost completely ineffective. The implication is that boundary changes are only effective at increasing school-SES in already desegregated areas.

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, depends on the behavioral responses of the subjects of the policy. We document that parents react to redistricting by opting out of their assigned school by choosing other public schools and to some extent use private schools and move away. Households with high socioeconomic status drive the responses, which implies that the "leakage" occurs in the top tail of the distribution. Consequently, if policy-makers want to minimize segregation across schools they must do so under the constraint that parents have an outside option.

Interpreted more generally these findings imply that there are limits to minimize segregation when individuals have an outside option. In other words, there is limited potential for what Durlauf (1996) refers 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 school system.

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A Construction of SES index

This 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:

$$SES = 0.62INC + 0.38LCE - 0.44NE + 0.53EMP, \tag{5}$$

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 percent of the distribution no household contain an uneducated parent. Income and

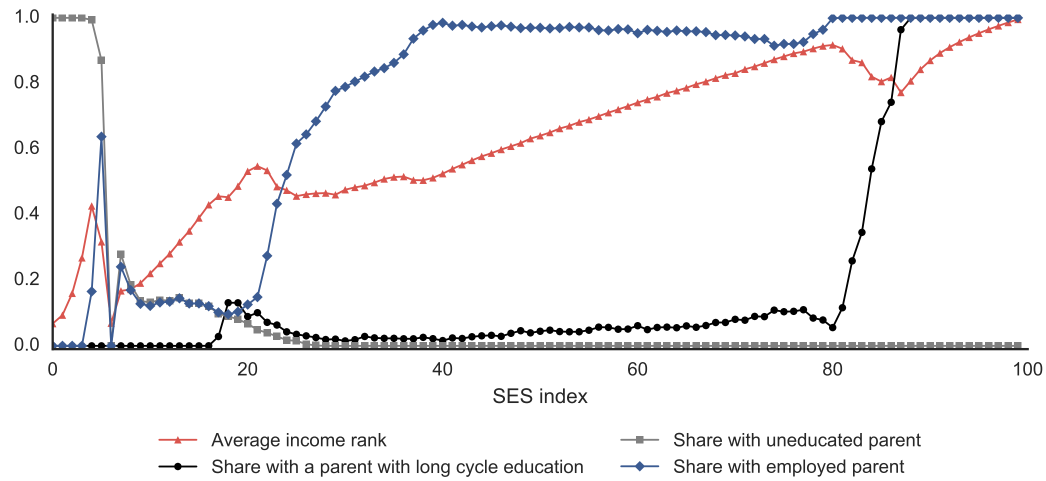
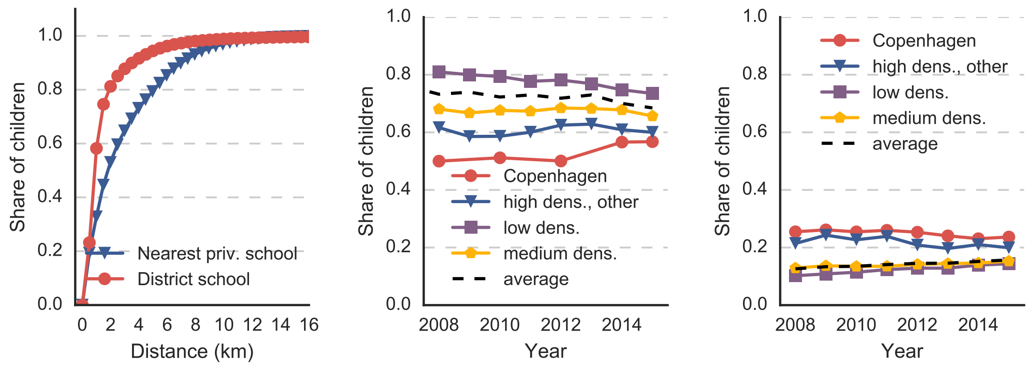


Figure A.1: Average characteristics as a function of SES-index

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.

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.

B Supplementary tables and plots



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

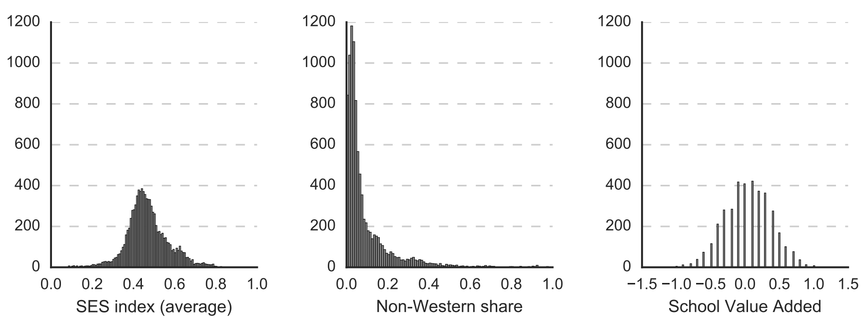
Figure B.1: School distance and enrollment

The figures depict various statistics for distance and enrollment. Figure B.1a shows the cumulative distribution of distance to district school and nearest private school. Figures B.1b and B.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.

Table B1: Responses along different margins to NW-share, interacted with own ethnicity

	Comply	Original	Public	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

Columns display regression results for the model presented in Equation (3) 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.



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

Figure B.2: Distributions of school characteristics

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

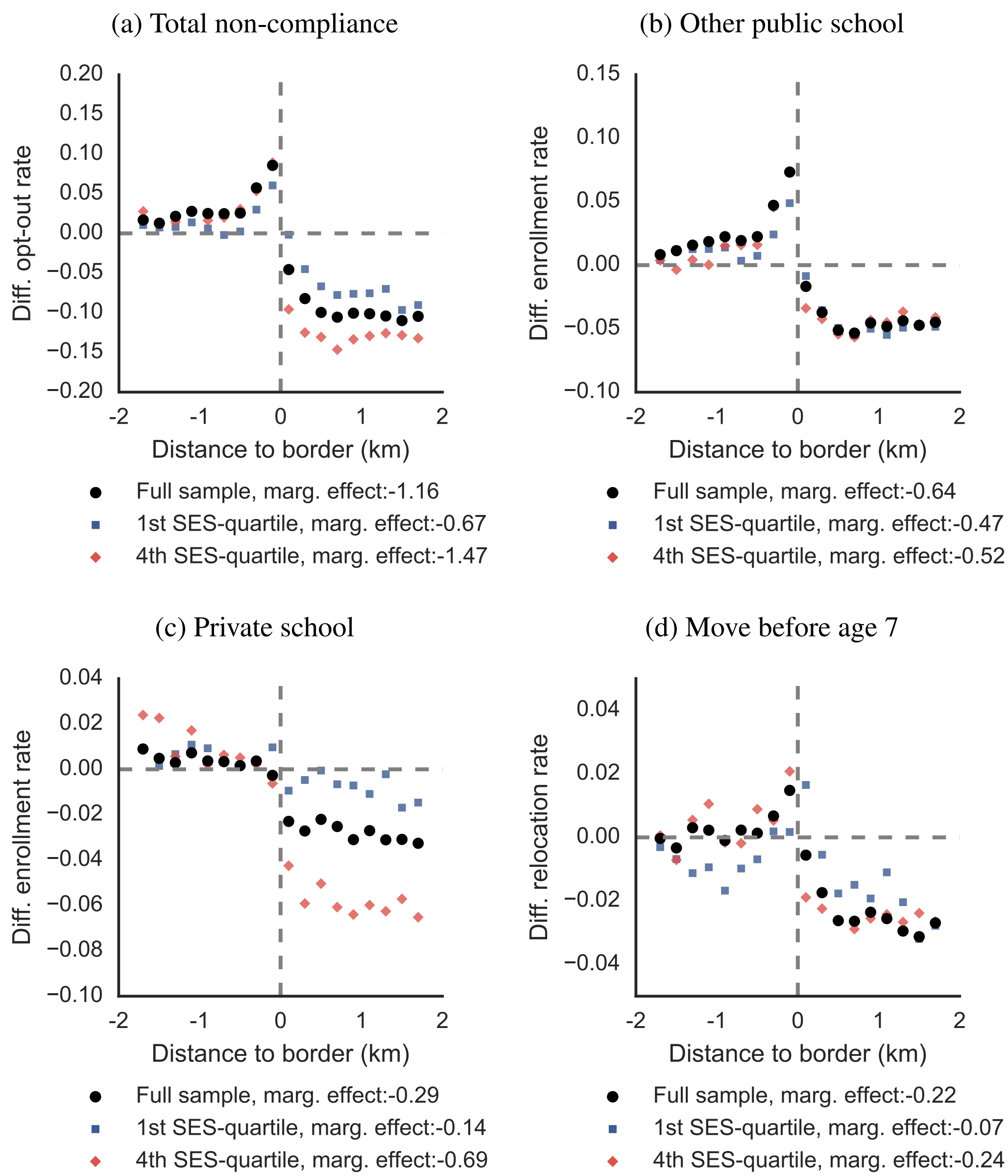


Figure B.3: Boundary difference in opt-out for low/high SES schools - by household SES

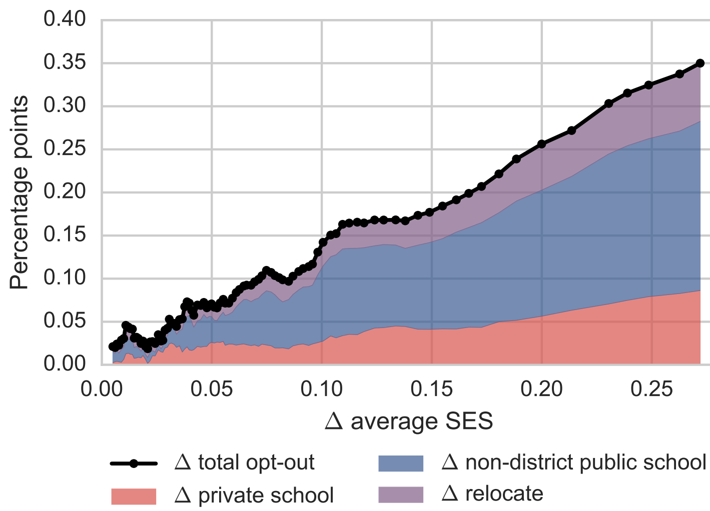
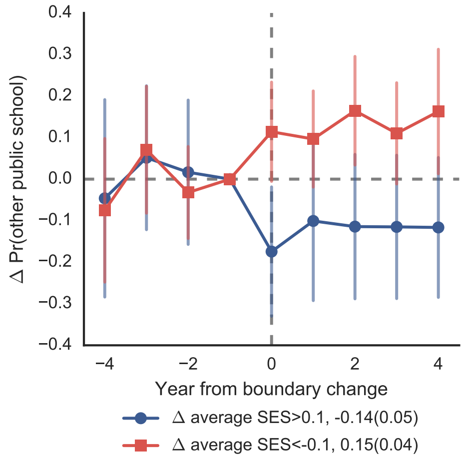
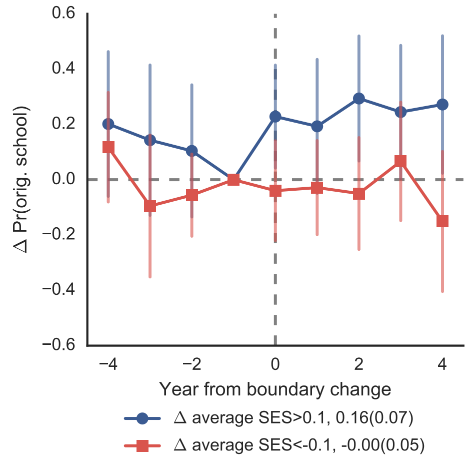


Figure B.4: Differences in opt-out rate as a function of difference in socioeconomic status

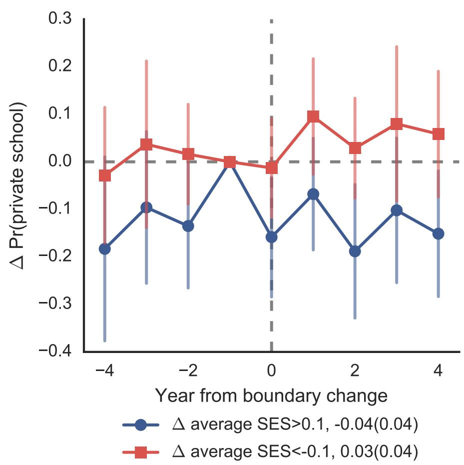
The figure depicts the estimated border differences in opting-out of the district school as a function of the border differences in average SES for adjacent district schools. We decompose the opting-out into its three subcategories, other public school or private school and moving away from the attendance zone. 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.



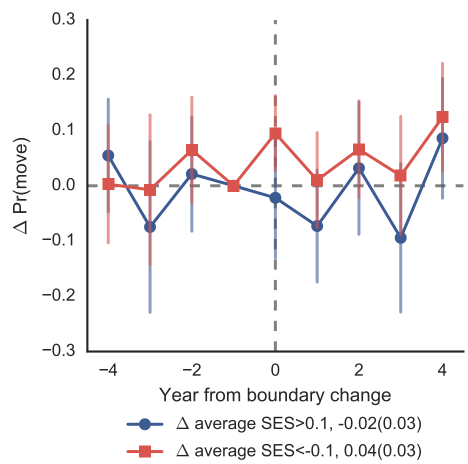
(a) Other public school



(b) Stay in original district school



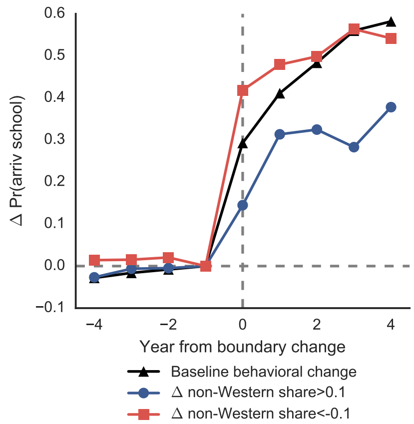
(c) Private school enrollment



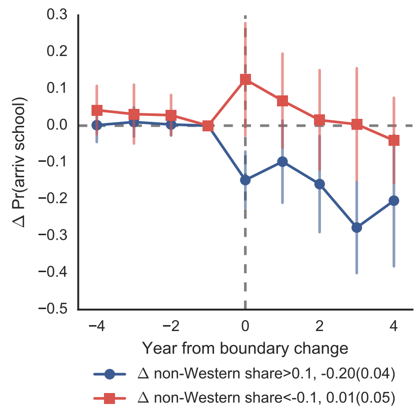
(d) Moving

Figure B.5: Response along margins to change in district by school characteristic

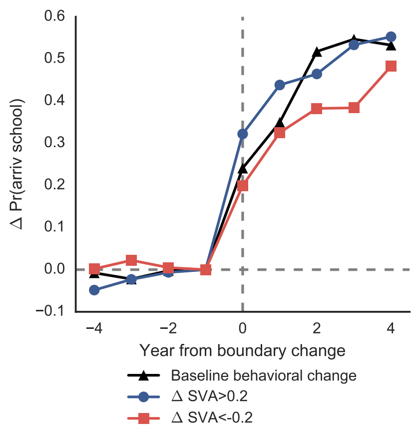
The figure display the interaction terms, β_{-}^{k} and β_{+}^{k}, along with 95-percent confidence intervals. The parameters represent the difference in likelihood of enrolling in the new district school when the average SES at a school level changes relative to the average arrival probability following 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 4b.



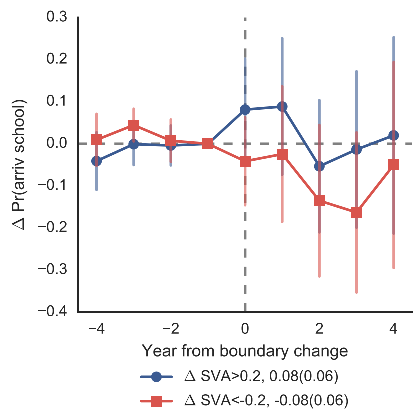
(a) NW - aggregate compliance



(b) NW-share - interaction



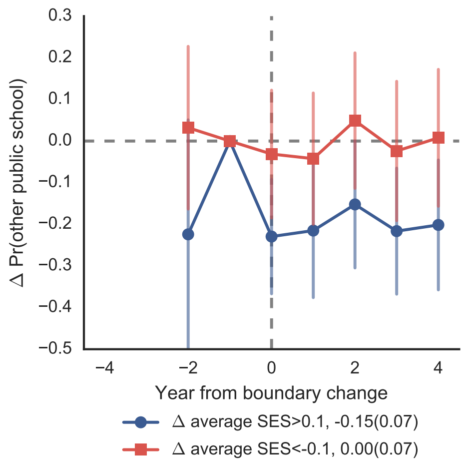
(c) SVA - aggregate compliance



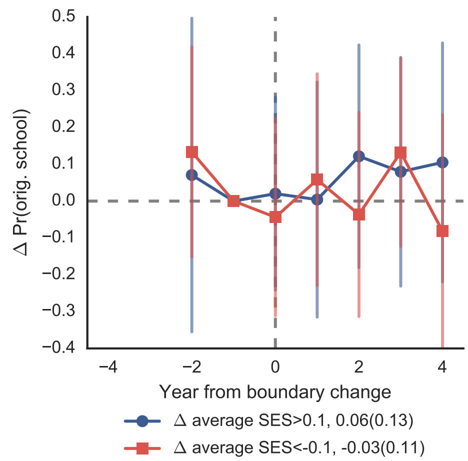
(d) SVA - interaction

Figure B.6: Compliance as a function of Non-Western share and school value-added

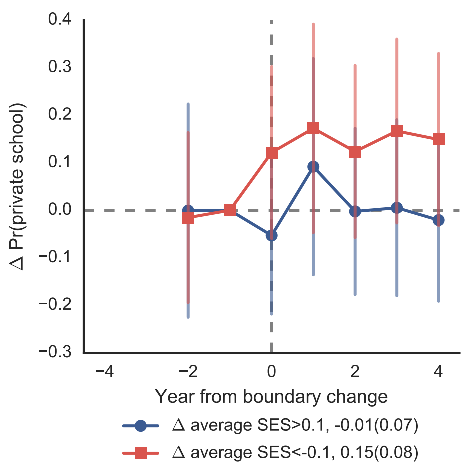
The figures on the left display changes in estimated compliance rates based on the model in (2) estimated with diffrenent measures of school characteristics. The black lines depict the estimated β_T^ks, while the blue and red line depict $\beta_T^k + \beta_-^k$ and $\beta_T^k + \beta_+^k$ respectively. 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 4b.



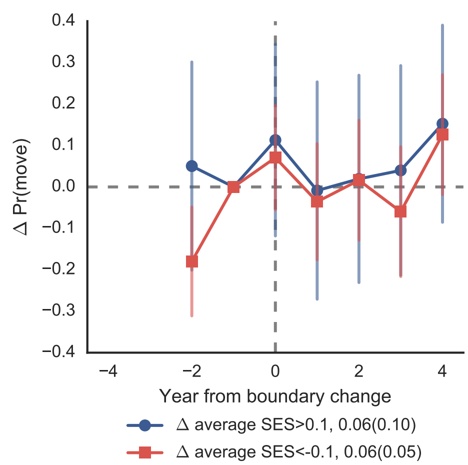
(a) Other public school



(b) Stay in original district school



(c) Private school enrollment



(d) Moving

Figure B.7: Response along margins to change in district by school characteristic when child aged 3 at time of boundary change

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 3. 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.