Market-Level Effects of a Large-Scale Public School Choice Reform*

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PRELIMINARY AND INCOMPLETE

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Abstract

We study market-level and distributional effects of a large-scale public school choice reform on education and labor market outcomes. Our identification strategy exploits variation in school choice possibilities across municipalities and over time generated by a nationwide reform that introduced school choice in Finland in the 1990s. Students from all socio-economic backgrounds make choices but the benefits of the choice reform are unequally distributed. School choice benefits higher SES students by improving their GPA and education outcomes. The education and labor market outcomes of lower SES students deteriorate because of displacement and selection effects in their education and occupational choices.

Keywords: school choice, educational reforms, educational and labor market outcomes, segregation

JEL Codes: I20, I28, C21, C26

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1 Introduction

School choice programs aim at improving student allocation and outcomes. These programs may have unintended distributional consequences on large scale. For instance, even if the students exercising choice benefit from school choice, the total effects on a market-level may be negligible or negative because of treatment effect heterogeneity, spillover effects to students not exercising choice, changes in the school network, or changes in parental preferences (Avery and Pathak, 2015; Epple and Romano, 1998; MacLeod and Urquiola, 2018).

This paper studies market-level and distributional effects of a large-scale public school choice reform in Finland on education and labor market outcomes. Market-level analysis is necessary in the evaluation of the total effects of school choice reforms because it takes into account the effects of the reform on all students in the market. At the same time, evaluating distributional effects is important from a policy perspective especially in terms of resource allocation in a publicly funded system. Despite of the obvious importance of the topic, there is very little empirical evidence on the market-level or the distributional effects of large-scale choice reforms on students' performance and sorting across schools. We contribute to this gap in the literature and provide empirical evidence in a pure public school setting. To our knowledge, this is the first paper to do so.

We focus on a nationwide school choice reform in Finland in the 1990s, which changed the financial incentives of schools and provided municipalities incentives to allow students to choose among public schools within their municipality.[1] Our identification strategy exploits municipal-level variation in school choice possibilities across municipalities and over time provided by the reform. By using this strategy for our identification allows us to capture the local market-level effects of school choice including the effects on those who choose another school, the effects on those who stay in their neighborhood school (including changes in the peer group, for example) as well as other school market-level changes.

In addition to market-level analysis, we use our rich longitudinal register data to study whether the school choice reform had distributional effects with respect to students' socio-economic background. This is important for policy evaluation: previous literature suggests that school choice programs may affect students differentially depending on their background. To understand the possible distributional effects better, we investigate the potential channels of school choice that could drive or explain our results. Lastly, we study whether the reform affected segregation of

[1] We focus on students entering lower secondary school for data reasons, but also because it is the most common time for school choice in Finland.

schools with respect to grades and parental background.

Our paper has four main findings. First, we find that the reform increased choice considerably, especially in larger cities. This is striking in the light of the results from international PISA studies showing only small differences between Finnish schools in learning outcomes or test scores (OECD, 2013). We also find that students from all socio-economic groups make choices. Our third finding is that school choice increased average educational attainment but the positive average effects are driven by the positive effect of school choice on students from high and middle income families. Students with higher socio-economic status (SES) benefit from more choice possibilities, as the reform increased their GPA and education and labor market outcomes. Students from low SES groups do not benefit form choice on the short-term, but on the long-term, they are less likely to have higher education. Fourth, we further explain these effects by displacement and selection into education and occupation in addition to finding evidence of increased segregation of schools. Taken together, our results imply that public school choice programs can result in an unequal distribution of its benefits and increased inequality.

We make three contributions to the previous literature on school choice. Previous empirical papers on school choice mostly focus on estimating the effects only on those who exercise choice (see, for example, Cullen et al., 2006; Deming et al., 2014) or consider spillover and/or market-level effects when studying the effects of private school vouchers or combined effects of choice and private school vouchers (Muralidharan and Sundararaman, 2015; Hsieh and Urquiola, 2006). We focus instead on the market-level effects in a pure public school setting.

Our second contribution is that we study the effects of a large-scale public school choice reform, whereas previous papers related to public school choice focus on a more local school choice reform (Lavy, 2010, 2015). The effects of a local school choice reform may differ from a state-wide or national choice reform.[2] One of the main strengths of our identification strategy is that we can account for general equilibrium effects within local school markets and evaluate important policy questions regarding school market-level effects of choice reforms. Lastly, as far as we are aware of, this is the first study to provide evidence on distributional effects of school choice using a quasi-experimental research design.

The following section describes the Finnish comprehensive school and the public school choice reform. Section 3 lays out our identification strategy. Section 4 describes the data. Our results in

[2]This applies more generally to large-scale or national education reforms. See a recent working paper by Gilraine et al. (2018).

are presented Section 5, and the last section concludes.

2 School Choice Reform in Finland

Finnish comprehensive school consists of primary school and lower secondary school. Primary school start in the year when the student turns seven and lasts for six years. Lower secondary school (intermediate level) takes three additional years. Comprehensive school is compulsory and almost all students complete their comprehensive school education: for example, the completion rate in 2013 was around 99.7 percent. (EDUFI, 2017)

Municipalities are required to provide comprehensive school education to all its comprehensive school aged residents by law. The vast majority of schools are operated by municipalities (over 95 percent). Some schools are operated by state government (less than 1 percent), some schools in larger cities are private (around 2 percent), and the rest of the schools are operated by joint municipal authorities. (Kumpulainen, 2010)

School market is regulated as follows. Opening new schools is needs-based and thus uncommon. Schools are not allowed to collect tuition fees or to make profit. Moreover, all schools have the same basic curriculum guidelines. Importantly, education is publicly funded using the same principles and governmental subsidies regardless of the schools ownership status. Therefore, compared to the education system in the US, for instance, the Finnish education system can be interpreted as being fully public despite of the differing ownership status of schools.

Our empirical analysis focus on the comprehensive school choice reform implemented in the mid-1990s. The reform allowed students to apply to other schools than the school they are assigned. Before the school choice reform, students were assigned into the closest school based on their residential address and distance to schools. We call this school hereafter a neighborhood school. Each school had its own catchment area from which students were eligible to attend the school. Crossing of the catchment area boundary was very uncommon and required specific reasons, such as specific medical conditions. The school catchment areas were set by municipalities, and this process was also regulated by several laws set by the government and other guidelines set by the provincial authorities.

Before the school choice reform, the government subsidies for comprehensive level school funding were based on the expected education costs tied to the school catchment areas. Municipalities designed their catchment areas to maximize the funding for comprehensive schooling in their municipality. These expected education costs gave the basis for the government subsidies for compre-

hensive level schooling in each municipality.

Public school choice was introduced through several law and policy changes.[3] These law and policy changes were part of the government decentralization process which had a goal of enabling municipalities to organize the provision of the municipal-level services more freely.

The main part the reform made the allocation of government subsidies for comprehensive schooling simpler and more transparent. After the reform, government subsidies were paid based on realized number of students attending the school. Other school market regulations were also relaxed. First, municipalities were allowed to design their school network more freely. Second, high demand schools were allowed to expand.[4] The changes in the financing and regulations on school networks created strong incentives for schools to accept students outside of the catchment area boundaries. After the reform, the law still required municipalities to assign each student to a neighborhood school. These students also had a priority in this school if the school was oversubscribed. However, the reform made it possible for the students to apply to other than their neighborhood school.[5]

Otherwise the implementation of school choice varies between municipalities. The application processes and the acceptance rates to other than the neighborhood schools vary between municipalities. Some municipalities require families to state their preferences of schools in a centralized application process. Other municipalities require students to contact the desired schools directly. There is also variation in whether schools actually take applicants outside their catchment areas and how this decision is made. In some municipalities, the decisions are made by a municipal-level institution but in other municipalities, the decision is made by school principals. (Seppänen, 2006) According to law, if the school is oversubscribed, student selection cannot be based on previous grades or family background. However, for example having a sibling in the school, can be used to prioritize applicants.

As a part of the reform, also the curriculum guidelines were updated to give schools more flexibility in curriculum design and possibility to specialize. In fact, specialization was one of the main political motivations for school choice because it was thought to provide inspiration and increase motivation for students. (Seppänen, 2006) Later this led into the introduction and popularization of special classes that offer extra teaching hours in for example sports or natural

[3]By school choice by a student we refer to the complete school choice decision making process. Because students are young, the choice is usually made together with parents (or a parent).

[4]There are no other restrictions on the expansions of schools except capacity constraints.

[5]By school choice of a student we refer to the complete school choice decision making process that includes the role of parents too. Because of the young age of the students, school choice is usually made together with (or by) parents and parental preferences may play a role in the decision making process.

sciences. Students from the school's own catchment area do not have a priority in these classes, and instead, schools can admit students from all over the municipality to these classes. These classes became very popular after the reform. Starting from 1999, a law change allowed schools to even use aptitude tests to select students into these special classes. Today, most of the comprehensive schools in Helsinki, for example, offer a special class or focus on a certain topic.

The most important funding related law changes took place in 1993, which is why we consider it to be the first reform year. Some trial schools without catchment areas existed in Helsinki in 1993 (Koskinen, 1994), while school choice spread to other municipalities from 1993 onward.

3 Identification Strategy and Model Specifications

3.1 First Stage and Reduced Form

We examine the market-level effects of a large-scale public school choice reform on education and labor market outcomes. We do this by exploiting variation in school choice possibilities across municipalities and over time generated by the school choice reform.

Our identification strategy is based on difference-in-differences with continuous treatment intensity.[6] The key idea behind this strategy is to compare changes in outcomes before and after a treatment between units across different levels of treatment intensity. Ideally, we would use variation across municipalities in their potential level of school choice activity in the post-reform period. Because this is not something we get to observe in the data, we use instead the number of schools in the municipality as a proxy for the school choice activity in the post-reform period. The motivation for our choice for the school choice activity measure is that the reform was more intense in municipalities with multiple schools as there were more schools for students to choose from and, consequently, more possibilities to exercise school choice.[7]

We begin by estimating the effects of the treatment intensity measure on realized school choice and student outcomes. We then use the treatment intensity measure as an instrument for realized school choice to scale the estimated reduced form effects.

The specification for our first stage (FS) model on the differential effects of the reform across municipalities with varying number of schools on realized school choice is given by

$$S_{imc} = \alpha_c^{FS} + \gamma_m^{FS} + \beta^{FS} N_m \times Post_c + \mu_c^{FS} X_i + \epsilon_{imc}^{FS}, \tag{1}$$

[6] Similar identification strategy has been used, for example by Card (1992).

[7] We use the average number of schools in a municipality before the reform to avoid endogeneity bias.

where S_{imc} is the measure for realized school choice of student i in municipality m, and cohort c , α_c^{FS} is a cohort fixed effect (cohort being the year in which student started seventh grade), and γ_m^{FS} is a municipality fixed effect. The coefficient of interest is given by β^{FS}. It captures the differential effect of the reform, $Post_c$, at a higher level of the treatment intensity measure, N_m. Equation (1) also includes student-level controls X_i, whose coefficients are allowed to vary across cohorts.

We estimate all models with three different control variable specifications. Specification 1 includes cohort and municipality fixed effects only. Specification 2 includes cohort and municipality fixed effects, gender and native language of the student, in addition to controls for the parental background: earnings, education and employment of a parent (either single parent or both parents), family income, and an indicator for a single parent. Specification 3 includes the variables included in Specification 2 in addition to the cohort-specific individual and parental background controls.

Our reduced form (RF) model on the differential effects of the reform across municipalities with different number of schools on student outcomes is given by

$$y_{imc} = \alpha_t^{RF} + \gamma_m^{RF} + \beta^{RF} N_m \times Post_c + \mu_c^{RF} X_i + \epsilon_{imc}^{RF}, \tag{2}$$

where y_{imc} is a student outcome, such as *GPA at the end of ninth grade*. The rest of the reduced form model is defined analogously to the first stage model (equation (1)).

The key identifying assumption in our difference-in-differences strategy is that without the reform the realized school choice and student outcomes would have evolved the same way across municipalities with varying numbers of schools over the time (the parallel trends assumption). We test the parallel trends assumption underlying the differences-in-differences design using the following event-study specification that includes interactions between each cohort and the treatment intensity variable:

$$\begin{aligned} y_{imc} = \alpha_t^{RF} + \gamma_m^{RF} + & \sum_{c=1988}^{1991} \beta_c^{RF} N_m \times \mathrm{D(cohort{=}c)} \\ & + \sum_{c=1993}^{2004} \beta_c^{RF} N_m \times \mathrm{D(cohort{=}c)} + \mu_c^{RF} X_i + \epsilon_{imc}^{RF}, \end{aligned} \tag{3}$$

where D(cohort=1988), refers to a dummy variable that equals 1 if the student started seventh grade in the year 1988, and 0 otherwise. The other cohort dummies are defined analogously. The coefficients of interest are β_c^{RF}'s ($c = 1988, 1989, ..., 1991, 1993, ..., 2004$), as they capture the

interaction between our treatment intensity variable and cohort. We normalize these coefficients relative to the last cohort before the reform (1992). Rest of the model is defined analogously to equations (1) and (2). The idea behind this test is that before the reform the relationship between the treatment intensity variable and the outcome should be stable, meaning all the β^{RF}'s in equation (3) before the reform should be zero. In addition to testing the parallel trends - assumption, equation (3) allows us to study the dynamics of the effects of the reform. It is quite likely that the school choice became more prevalent over time, and thus the effects become stronger for the younger cohorts.

3.2 IV-estimation

We scale the differential impacts of the reform on student outcomes across municipalities by the differential impacts of the reform on realized school choice with an instrumental variable approach. We use the interaction between the reform and the number of schools in the municipality as an instrument for the share of students that attend a non-neighborhood school.

We estimate a 2SLS model where $N_m \times Post_c$ is used as an instrument for realized school choice measured by the share of students attending a non-neighborhood school.

Our first stage model is the same as equation (1). The IV estimates are obtained from the following equation

$$Y_{imc} = \alpha_c^{2SLS} + \gamma_m^{2SLS} + \beta^{2SLS}\hat{\mathrm{S}}_{imc} + \mu_c^{2SLS}X_i + \epsilon_{imc}^{2SLS}, \tag{4}$$

where $\hat{\mathrm{S}}_{ims}$ is realized choice predicted by the intensity of the reform from our first stage given by equation (1), and β^{2SLS} is the coefficient of interest that measures the effect of a percentage point increase in the share of students attending a non-neighborhood school in a municipality. Rest of the variables in the equation are defined as in equation (1).

We make some additional points of our IV strategy. First, we use our proxy for the non-neighborhood school attendance as our endogenous variable. This choice does not introduce bias to our IV estimates as long as any measurement error in the proxy is uncorrelated with the instrument, conditional on the controls included in the model.[8] Second, realized school choice, as measured by non-neighborhood school attendance, may not fully capture all of the channels through which the effects of the school choice reform operate. This may be the case, for instance, if schools respond

[8] We discuss our school choice approximation measures in Section 4.2 in more detail.

to the threat of students switching schools in which case student outcomes might be affected even if there is no realized school choice. Even if this is the case, the IV estimates nevertheless provide a useful scaling for the reduced form estimates.

4 Data and variables

4.1 Data

We use combined register data from two sources. The first data source is Centralized Secondary School Application and Admissions Data containing all ninth graders in Finland in 1991-2007. These students entered the lower secondary school (grade seven) between 1988 and 2004. This means that we have data starting from five years before the reform. These data include information on the comprehensive school attended, upper secondary schools applied to, upper secondary school place received (either in academic track, i.e. high school, or vocational track, or no place received), and grades at the end of ninth grade.

Second data source is the Finnish Longitudinal Employer-Employee Data (FLEED). These data contain everyone aged 16 to 65 and runs from 1988 to 2015. The data contains individual level information on the highest education level achieved, earnings and other labor market outcomes, and parental background information. These data are combined with grid database of students' yearly residential locations. Our final sample consists of all Finnish students who are 16 years old at the end of the compulsory education (or turned 16 during this year), and who were not repeating their ninth grade, or doing tenth grade.[9]

4.2 Proxy for school choice and the intensity of the reform

Our data have the following limitations. We do not have information about the applications or the original neighborhood school of the students because neither are systematically collected in Finland. We also do not know when and how municipalities implemented school choice. Our data contain only information on which comprehensive school student attended. Also, we only know the student addresses on a 1km by 1km grid each year for all municipalities.

In this subsection, we discuss how we exploit the information we have to construct different school choice proxies. All our proxies for school choice are based on the students' yearly residential

[9]Our sample consists only of students living in mainland Finland because there is missing information of the students who live in Åland Islands. Only 0.5 percent of population lives in Åland Island. Our sample period also coincides with a couple of municipal mergers. In this case, we merge these municipalities from the first sample year onwards.

location and the comprehensive school attendance information in our data. We use the proxy as our first stage outcome variable to see if the number of schools is related to the changes in realized school choice.

We call our main first stage outcome variable *non-neighborhood school*. It gives the share of students attending other school than the most common school in the area that year.[10]

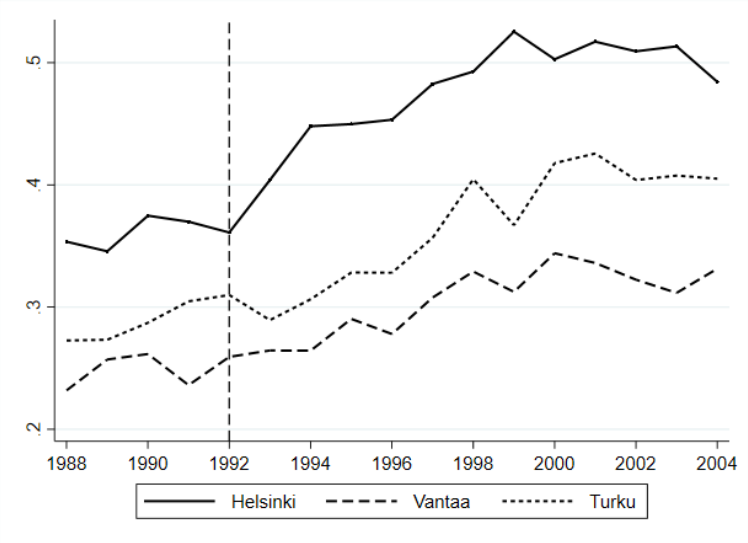


FIGURE 1: The development of the share of students attending other than the most common school in Helsinki, Turku, and Vantaa

Figure 1 shows the development of this proxy for school choice (*non-neighborhood school*) in three bigger cities of Finland: Helsinki, Turku, and Vantaa. It shows that school choice increased 10-15pps in Helsinki and Turku. In Vantaa, a city with a more restricted school choice policies, the increase in school choice after the reform was more modest than in Helsinki and Turku.

The results in Figure 1 are also in line with the information from the early municipal reports. According to the reports, the amount of students exercising choice was considerable already from the first years of the reform: around third of the lower secondary school students (those entering seventh grade) applied, and around 80 percent of them were accepted into other than their neighborhood school already in 1994 in Helsinki. (Koskinen, 1994) Moreover, in the beginning of 2000's, about half of the students in Helsinki applied to other than their neighborhood school in seventh grade. (Seppänen, 2006) The corresponding numbers are a bit lower in other large municipalities.[11]

[10]The precise definition of the variable is that the dummy takes value 100, if student attends other than the most common school of a particular 1km by 1km grid that year, otherwise the variable gets value zero. We choose the value 100 for scaling purposes and to ease the interpretation of our results.

[11]Unfortunately these numbers are not available on yearly basis.

Figure 1 shows that approximately 35 percent of the students would have attended other than their neighborhood school in Helsinki in 1988. Some schools in Helsinki area had special language classes before the school choice reform which would be captured by our measure as choice activity. Because Helsinki is more densely populated than other cities, some students living in the same grid area may be assigned to different neighborhood schools. In sum, this suggests that main our approximation measure may slightly over-estimate the number of students attending other than their assigned neighborhood school (compared to the figures from the municipality reports). Another limitation of this first approximation measure is that it may also become less accurate over the years, as school choice becomes more prevalent and the most common school of the region could, at least in theory, be other than the actual neighborhood school.

Because of these two limitations, we use two other complementary measures to our main school choice proxy described above. The first alternative measure takes a different approach in approximating the neighborhood school. In this measure, a school is defined as a neighborhood school if at least 30 percent of the region's students attend it.[12] We call this alternative proxy for school choice *other than neighborhood school*. Our second alternative proxy for school choice is *the mobility index*, M_i, and it measures the mobility of students in 1km by 1km grid each year. The measure varies between 1 and 0, where value 1 means that the student is very mobile and 0 that the student attends the same school as everyone else in the grid.[13] Comparing all the three approximation measures above show similar patterns in the development of choice.

Our differences-in-differences identification strategy uses the average number of comprehensive schools that have grades seven to nine in a municipality between 1988 and 1992. We use this variable to capture the pre-reform variation in school choice possibilities and the potential intensity of the reform.

Table 1 shows that the number of schools remained fairly constant before and after the reform. The number of students is higher in municipalities with more than ten schools and lower in municipalities with less than ten schools before the reform in post-reform part of the sample. The most striking difference between municipalities is in the number of schools, and hence in school choice possibilities: on average there are only about 1.2 schools in smaller municipalities (with less than ten schools before the reform) whereas in municipalities with more than ten schools there are on

[12] More precisely, the measure takes a value 100 if the student attends other than a school that at least 30 percent of the students of the 1km by 1km grid attend that year, otherwise 0.

[13] Mobility index measure is calculated as $M_i = 1 - \Delta_j$, where Δ_j is the share of students attending the same school, j, as student i and live in the same grid as student i.

TABLE 1: Municipal-level descriptive statistics.

	More than 10 schools		Less than 10 schools	
	1988-1992	1993-2004	1988-1992	1993-2004
	(1)	(2)	(3)	(4)
Number of schools	20.3	21.3	1.21	1.26
	(13.4)	(13.7)	(1.20)	(1.21)
School size	76.5	73.9	52.0	51.5
	(8.16)	(7.87)	(38.7)	(37.3)
Cohort size	1778.9	1816.2	106.3	103.0
	(913.9)	(1011.5)	(123.0)	(117.2)

Notes: These are the means of municipal-level variables calculated separately for municipalities that have less than ten or more than ten schools before the reform and calculated separately for all cohorts before and after the reform. Standard errors are clustered at the municipality level and shown in parentheses.

average more than 20 schools to choose from.

This discrepancy is also illustrated by Figure 2. There are close to 280 municipalities with less than two schools and only around ten municipalities have more than 10 schools.[14]

[14]In Finland, some municipalities do not have students for each cohort. This is shown in our data as zero.

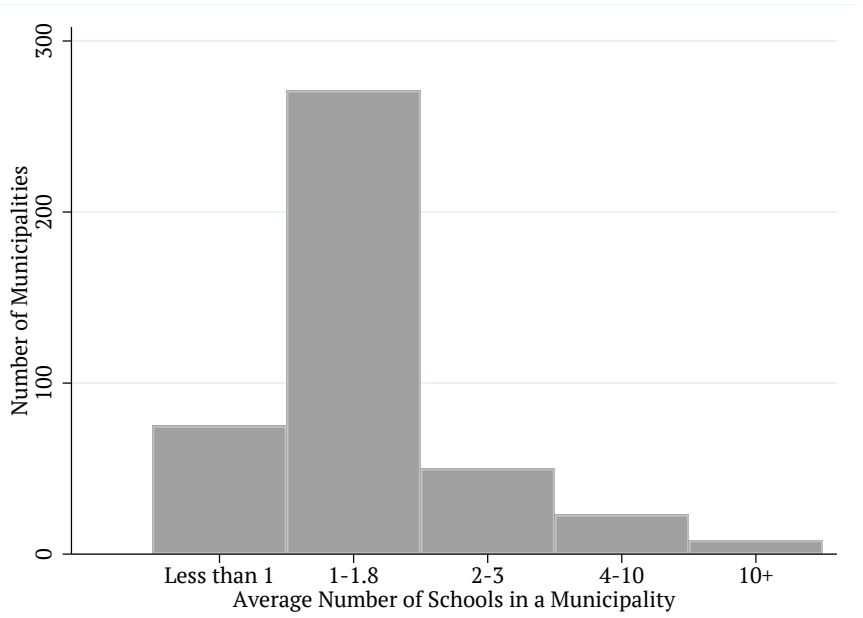


FIGURE 2: Distribution of the average number of schools within a municipality before the reform.

4.3 HETEROGENEITY WITH RESPECT TO FAMILY BACKGROUND AND DISTRIBUTIONAL EFFECTS OF THE REFORM

We divide our sample into quartiles each year using family income. Family income includes all work and entrepreneur related earnings of the family in addition to the benefits, such as maternity leave, social or unemployment benefits, received by the family.[15] The upper and bottom quartiles represent the students from high and the low income families, respectively. We combine the two middle quartiles and this group represents the students from middle income family.

Table 2 summarizes our main family background and student characteristics, with respect to family income, before and after reform, and between small (less than ten schools before the reform) and large (more than ten schools before the reform) municipalities. [16] For the full set of descriptive statistics of our outcome and control variables, see Appendix.

Table 2 shows striking differences in parental characteristics between the socio-economic groups (high, medium, low income). Compared to the students form high income families, students from a low income family are more likely to have one or both parents without a (upper) secondary or higher education and unemployed. They are also more likely to be from a single parent family.[17]

[15] Family income does not include capital income because this information is not available to us.

[16] All of these family background characteristics are measured the year the student turns 14.

[17] This is a proxy for single parent, as the parent who the child lives with might have remarried and hence this dummy also proxies whether the parents of the student have divorced or not.

TABLE 2: Descriptive statistics of control and outcome variables

	More than 10 schools		Less than 10 schools	
	1988-1992	1993-2004	1988-1992	1993-2004
	(1)	(2)	(3)	(4)
Average family income (in 2015 euros)				
Average	63,200	72,672	49,554	57,487
	(40,783)	(71,453)	(29,915)	(47,749)
Low Income	24,384	26,438	24,823	26,953
	(9,318)	(12,665)	(8,894)	(12,051)
Middle Income	50,409	58,191	48,944	56,636
	(7,614)	(16,844)	(7,559)	(16,496)
High Income	94,210	114,092	84,456	98,708
	(48,109)	(101,044)	(45,769)	(84,605)
Parent secondary or higher educated				
Average	0.84	0.90	0.81	0.91
	(0.37)	(0.30)	(0.39)	(0.29)
Low Income	0.67	0.76	0.70	0.83
	(0.47)	(0.43)	(0.46)	(0.38)
Middle Income	0.81	0.90	0.82	0.93
	(0.40)	(0.30)	(0.38)	(0.26)
High Income	0.94	0.97	0.84	0.97
	(0.24)	(0.16)	(0.23)	(0.16)
Unemployed parent				
Average	0.09	0.15	0.10	0.16
	(0.28)	(0.36)	(0.31)	(0.36)
Low Income	0.22	0.40	0.19	0.33
	(0.42)	(0.49)	(0.39)	(0.47)
Middle Income	0.09	0.13	0.09	0.12
	(0.29)	(0.34)	(0.29)	(0.33)
High Income	0.02	0.03	0.02	0.03
	(0.14)	(0.17)	(0.15)	(0.16)
Single parent				
Average	0.28	0.35	0.20	0.26
	(0.45)	(0.48)	(0.40)	(0.44)
Low Income	0.59	0.65	0.34	0.41
	(0.49)	(0.48)	(0.47)	(0.49)
Middle Income	0.26	0.33	0.16	0.22
	(0.44)	(0.47)	(0.36)	(0.41)
High Income	0.16	0.20	0.11	0.16
	(0.36)	(0.40)	(0.32)	(0.36)
Student has a foreign native language				
Average	0.006	0.026	0.002	0.007
	(0.079)	(0.160)	(0.045)	(0.081)
Low Income	0.026	0.093	0.005	0.018
	(0.160)	(0.291)	(0.073)	(0.133)
Middle Income	0.002	0.014	0.001	0.003
	(0.048)	(0.115)	(0.026)	(0.052)
High Income	0.002	0.005	0.001	0.002
	(0.043)	(0.071)	(0.030)	(0.043)

Notes: These are the means calculated over all individuals who live in municipalities with less than ten or more than ten schools before the reform. The means are calculated separately for all cohorts before and after the reform. *Average family income* is an average of the sum of all work and entrepreneur related earnings of the family in addition to the benefits, such as maternity leave, social or unemployment benefits, received by the family. *Student has a foreign language* is defined as a student whose native language is not either Finnish or Swedish. Standard errors are in parentheses.

The share of single parent families also differs between small and large municipalities and over the years.

The characteristics of the students themselves differ also by family income: there are more foreign native language speakers (i.e. not Finnish or Swedish as native language) in students who come from low income families, especially after the reform in larger municipalities (almost 10 percent) (Table 2). On average, a student from low income families receive lower grades at the end of the comprehensive school, are less likely to have a high school and higher education, and they also earn less in 2015 than their peers from high and middle income families (Table A1 in Appendix).

5 Results

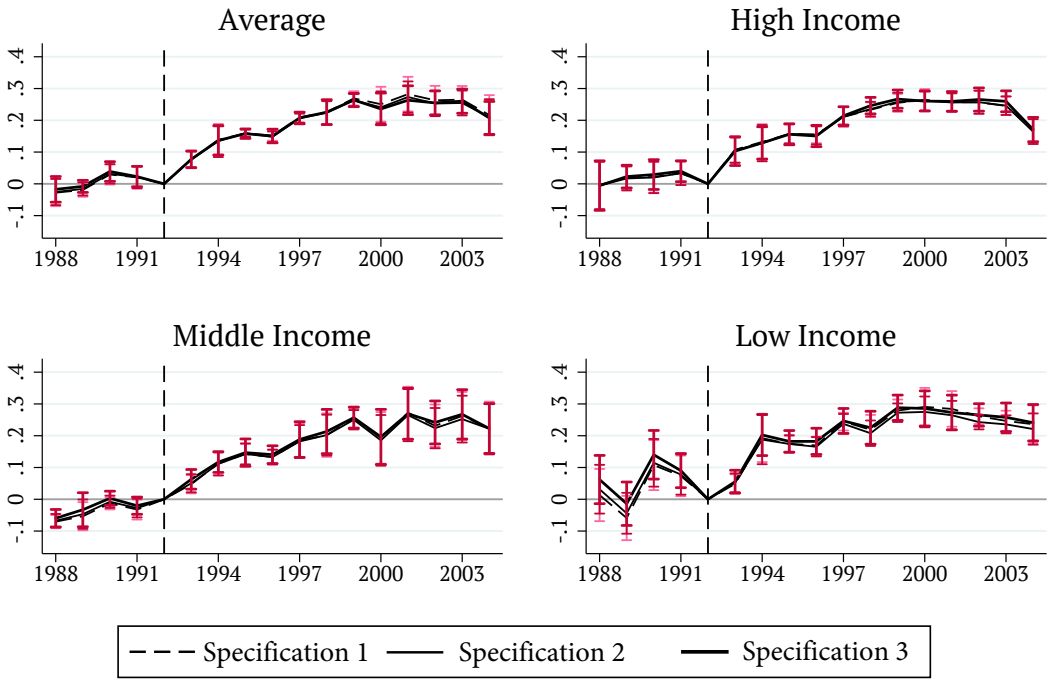
5.1 First Stage

We begin by showing that our instrument is related to changes in realized school choice. Figure 3 plots our event-study estimates (equation 3) for our main school choice proxy, *non-neighborhood school*. We find that school choice increased gradually after the reform. Figure 3 also shows no evidence of differential pre-trends. Figure 3 contains the three specifications and shows that the results are robust in all the specifications.[18] Also, students from all socio-economic backgrounds make choices: there are no significant differences between students from the low, middle, and high income families. Our first stage results using equation (1) are summarized in Table 3.

As a robustness check, we estimate event-study plots using other two alternative proxies. The results are in our Appendix. They are in line with the results using our main proxy: the other two approximation measures also show no evidence of differential pre-trends and show an upward trend in school choice after the reform. These are confirmed by the first stage results using equation (1) summarized in Table 3.

We quantify these results as follows. Compare a low treatment intensity municipality with less than two schools to a high treatment intensity municipality with more than 50 schools. This is a rough comparison of an average municipality to the capital city, Helsinki. This means that our estimates shows a 10 percentage point (which is approximately 0.3 standard deviations) increase in the probability of attending a non-neighborhood school. In other words, the reform increased the probability of school choice by 10pps. Figure 3 shows that this estimate is even greater after 2000's,

[18] For clarity, the rest of the event-study plots Specification 3. Instead, the tables summarizing our results have all the three specifications.



Specification 1: Cohort and municipality fixed effects.
Specification 2: Specification 1 + controls for gender, language, and parental background.
Specification 3: Specification 2 + cohort-specific individual and parental background

FIGURE 3: The share of students attending non-neighborhood school, by family income.

about 15pps. This shows that our instrument captures the gradually increasing school choice after the reform.

TABLE 3: First stage results.

Share attending non-neighborhood school			
	(1)	(2)	(3)
Average	0.212***	0.205***	0.200***
	(0.0232)	(0.0208)	(0.0193)
Low Income	0.197***	0.179***	0.174***
	(0.0356)	(0.0293)	(0.0280)
Middle Income	0.226***	0.219***	0.219***
	(0.0205)	(0.0194)	(0.0188)
High Income	0.192***	0.192***	0.193***
	(0.0191)	(0.0182)	(0.0168)
Share attending other than neighborhood school			
	(1)	(2)	(3)
Average	0.283***	0.275***	0.273***
	(0.0217)	(0.0191)	(0.0173)
Low Income	0.271***	0.251***	0.250***
	(0.0321)	(0.0249)	(0.0237)
Middle Income	0.311***	0.304***	0.306***
	(0.0158)	(0.0147)	(0.0155)
High Income	0.248***	0.249***	0.250***
	(0.0199)	(0.0192)	(0.0170)
Mobility Index (M_i)			
	(1)	(2)	(3)
Average	0.00226***	0.00219***	0.00214***
	(0.000445)	(0.000434)	(0.000419)
Low Income	0.00216***	0.00197***	0.00197***
	(0.000553)	(0.000493)	(0.000486)
Middle Income	0.00249***	0.00243***	0.00240***
	(0.000452)	(0.000446)	(0.000433)
High Income	0.00190***	0.00188***	0.00184***
	(0.000364)	(0.000359)	(0.000341)
FE	Yes	Yes	Yes
Background	No	Yes	Yes
Interactions	No	No	Yes

Notes: FE refers to cohort and municipality fixed effects. Background refers to individual and parental level controls. Interactions refers to interactions between the cohort and background level controls. Standard errors are clustered at the municipality level and are shown in parentheses.
* p<0.05, ** p<0.01, *** p<0.001.

5.2 Education and Labor Market Outcomes

We begin by studying the impacts of school choice on short-term education outcomes: *GPA* and the probability of graduating with a upper secondary school degree. Then we examine the long-term effects of school choice and study how school choice affects the probability of graduating from college.

Our first education outcome is *GPA*, which is defined by the average of theoretical subjects at the end of the comprehensive school and ranges from 10 (outstanding) to 4 (failed).[19] *GPA* is an important outcome because student applies to upper secondary schools (academic or vocational track) with it.

Figure 4 plots the event-study estimates of equation (3) for *GPA*. We find that that school choice has on average a positive effect on *GPA*. Quantifying the reduced form estimates using the same logic as in Section 5.1, our estimates show approximately an increase of 0.08 standard deviations in *GPA*.[20] Figure 4 also shows that the effect is gradually increasing after the reform. These findings are also confirmed by Table 4 which summarizes the reduced form for all our short-term education outcomes using equation 2.

Strikingly, the breakdown of these average market-level effects by family income reveals that the positive average effect on *GPA* is driven by a positive effect on students from high and middle income families. Students from high income families have approximately 0.1 standard deviations higher GPA. There are no significant effects on GPA for students from low income families.

[19] The grades are given by teachers. There is no standardized country-wide testing in Finland. We standardize this to have a mean of zero and standard deviation of one. We drop around 10 municipalities from our *GPA* for year 1994 because of the high share of missing values.

[20] Recall, we compare a low treatment intensity municipality to a high treatment intensity municipality, that is Helsinki with more than 50 schools vs. an average municipality in Finland with less than two schools.

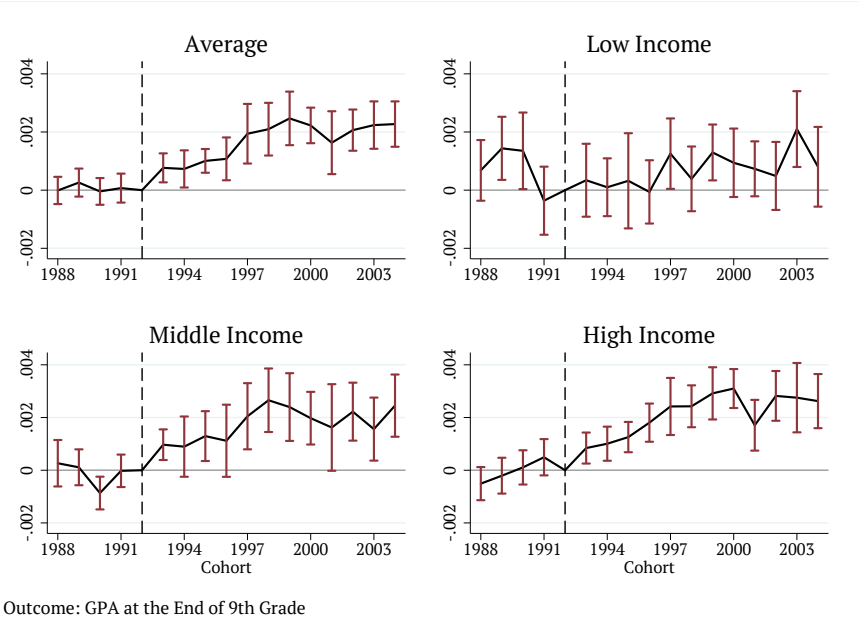


FIGURE 4: GPA, by family income.

We then examine how school choice affects the probability of graduating with a upper secondary school degree. We study two outcomes: *high school graduate* (academic track)[21] and *at least a secondary level education after 10 years*[22].

[21] *High school graduate* takes value 1 if the student has completed high school and matriculation examination by the end of 2015, otherwise zero. Academic track high school degree is usually used for college/university applications.

[22] Variable *at least a secondary level education after 10 years* takes value 1, if the student has completed an upper secondary level education at least 10 years after the completion of compulsory education, otherwise 0. This measure contains degrees from either academic or vocational track of the upper secondary school and takes into account if the student had a delay in graduating. This outcome is only available for cohorts who started seventh grade between 1988 and 2002.

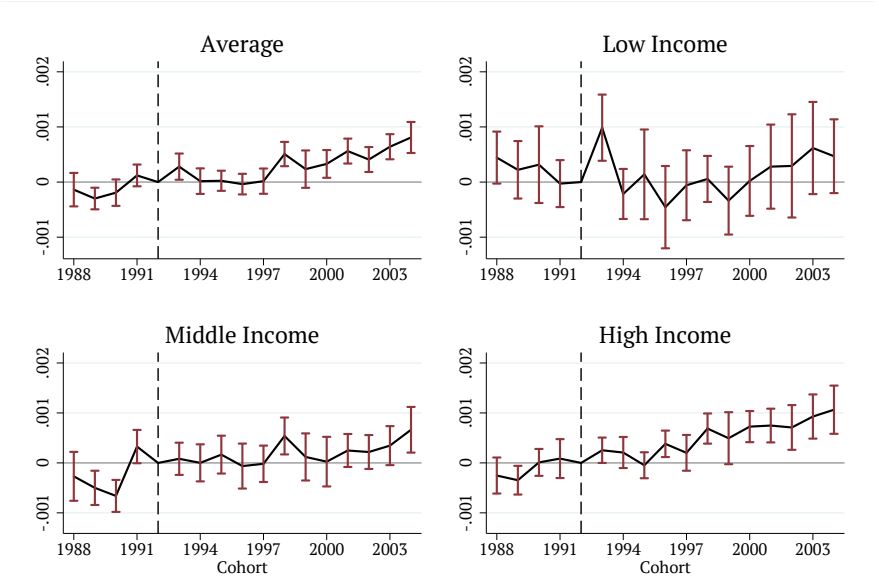


FIGURE 5: Probability of graduating from high school, by family income.

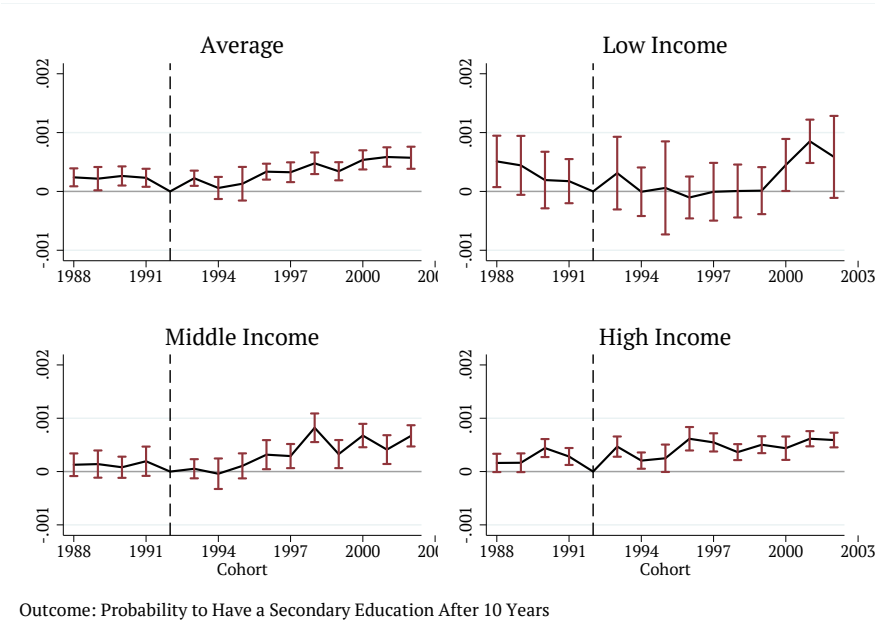


FIGURE 6: Probability to have at least a secondary school education 10 years after comprehensive school, by family income.

We find that on average, school choice increased the probability of graduating from high school and the probability to have at least a secondary schooling 10 years after graduating from comprehensive school. Figure 5 shows around 2pps (or 0.04 standard deviations) increase in high school graduation. Figure 6 shows 1pps (or 0.03 standard deviations) increase in the probability to have

at least a secondary schooling ten years after graduating from comprehensive school. Similarly to the GPA the effects of school choice on the probability of graduating with a upper secondary school degree are gradually increasing after the reform.

Again, we find that the positive average effect is driven by a positive effect on students from high and middle income families: students from high income families are about 3pps more likely to graduate from high school and 1pps more likely to have a secondary school education ten years after graduating from comprehensive school. Similarly to our GPA results, we find no significant effects on either of these outcomes for students from low income families.

In sum, we find a positive effect of school choice on our first education outcomes. Importantly, we find that the positive effect is driven by a positive effect on students from high and middle income families. These heterogeneous effects are striking, because they arise despite the fact that our first stage results show that students from all backgrounds make choices.

We scale the effects of the reform by using the IV approach described in Section 3.2. For the interpretation of our IV estimates, we consider an increase of 10pps in realized school choice activity measured by our choice approximation, attending a *non-neighborhood school*. 10pps is by how much the attendance at non-neighborhood school increased on average after the reform, weighted by the number of schools.[23] Our IV results are very similar in magnitude to our reduced form estimates. On average, GPA increases 0.08 standard deviations, high school graduation around 2pps (or 0.04 standard deviations) and the probability of having at least a secondary school education 10 years after comprehensive school by 1pps (about 0.03 standard deviations).

[23]Figure 1 shows around 10pps increase in the share of students attending a non-neighborhood school after the reform in Turku and around 15pps in Helsinki.

TABLE 4: Effect of school choice on GPA and upper secondary school degree.

	GPA					
	(1)		(2)		(3)	
	RF	IV	RF	IV	RF	IV
Average	0.00061**	0.00287***	0.00107***	0.00525***	0.00166***	0.00831***
	(0.000214)	(0.000828)	(0.000231)	(0.000820)	(0.000348)	(0.001107)
		[84]		[98]		[108]
Low Income	-0.00047	-0.00233	-0.00019	-0.00102	0.00016	0.00088
	(0.000540)	(0.002343)	(0.000261)	(0.001369)	(0.000267)	(0.001570)
		[38]		[45]		[46]
Middle Income	0.00172***	0.00774***	0.00161***	0.00746***	0.00187***	0.00870***
	(0.000474)	(0.001504)	(0.000432)	(0.001447)	(0.000460)	(0.001492)
		[102]		[106]		[112]
High Income	0.00219***	0.01139***	0.00216***	0.01119***	0.00216***	0.01119***
	(0.000405)	(0.001372)	(0.000387)	(0.001306)	(0.000375)	(0.001362)
		[105]		[115]		[142]
	Probability to graduate from high school					
	(1)		(2)		(3)	
	RF	IV	RF	IV	RF	IV
Average	-0.00011	-0.00054	0.00006	0.00027	0.00043***	0.00216***
	(0.000091)	(0.000388)	(0.000083)	(0.000432)	(0.000057)	(0.000257)
		[84]		[98]		[109]
Low Income	-0.00033	-0.00172	-0.00034	-0.00191	-0.00002	-0.00009
	(0.000375)	(0.001639)	(0.000241)	(0.001100)	(0.000215)	(0.001241)
		[32]		[38]		[39]
Middle Income	0.00035***	0.00157***	0.00024***	0.00113***	0.00042***	0.00196***
	(0.000085)	(0.000339)	(0.000071)	(0.000310)	(0.000088)	(0.000324)
		[121]		[127]		[136]
High Income	0.00063***	0.00336***	0.00058***	0.00308***	0.00063***	0.00342***
	(0.000094)	(0.000442)	(0.000086)	(0.000401)	(0.000089)	(0.000399)
		[102]		[111]		[134]
	At least an upper secondary level education after 10 years					
	(1)		(2)		(3)	
	RF	IV	RF	IV	RF	IV
Average	-0.00000	-0.00001	0.00009	0.00046	0.00017***	0.00090**
	(0.000065)	(0.000319)	(0.000049)	(0.000277)	(0.000043)	(0.000276)
		[107]		[127]		[137]
Low Income	-0.00023	-0.00119	-0.00016	-0.00092	-0.00003	0.00016
	(0.000179)	(0.000786)	(0.000105)	(0.000562)	(0.000105)	(0.000622)
		[37]		[45]		[45]
Middle Income	0.00024***	0.00113***	0.00025***	0.00122***	0.00026***	0.00125***
	(0.000043)	(0.000217)	(0.000045)	(0.000206)	(0.000044)	(0.000216)
		[155]		[163]		[168]
High Income	0.00027***	0.00146***	0.00025***	0.00134***	0.00025***	0.00133***
	(0.000048)	(0.000349)	(0.000046)	(0.000325)	(0.000059)	(0.000397)
		[130]		[142]		[165]
FE	Yes		Yes		Yes	
Background	No		Yes		Yes	
Interactions	No		No		Yes	

Notes: Outcome variables are: *GPA* is the average of theoretical subjects at the end of the comprehensive school, a dummy *high school graduate* takes value 1 if the student has completed high school and matriculation examination by the end of 2015, otherwise 0, RF refers to reduced form estimations and IV to instrumental variable estimations. FE refers to cohort and municipality fixed effects. Background refers to individual and parental level controls. Interactions refers to interactions between the cohort and background level controls. Standard errors are clustered at the municipality level and shown in parentheses. First stage F-tests in square brackets.
* p<0.05, ** p<0.01, *** p<0.001.

Next, we study whether school choice affected the probability of obtaining a college degree (or higher) and earnings.[24] We first study whether school choice affected the probability of having *at least a Bachelor level degree in 2015*.[25] Figure 7 shows the event-study plot of the probability of having *at least a Bachelor level degree in 2015* using equation 3. We find that the probability to have a higher degree education increased by 1pps (or 0.02 standard deviations) after the reform.

Similarly to the results of our education outcomes above, the positive average effect of the probability of having a college degree (or higher) is driven by a positive effect for students from high income families. Both the reduced form and IV estimates for this outcome in Table 5 are positive and significant for students from high income families. Our IV estimates also show that students from low income families are less likely to have a higher education. Quantifying the reduced form and IV estimates, using the same logic as above, our results show that students from high income families are 2pps more likely to have at least a college degree (or higher) education after the reform. Our IV estimates also show that students from low income families 2pps less likely to have a higher education after the reform.

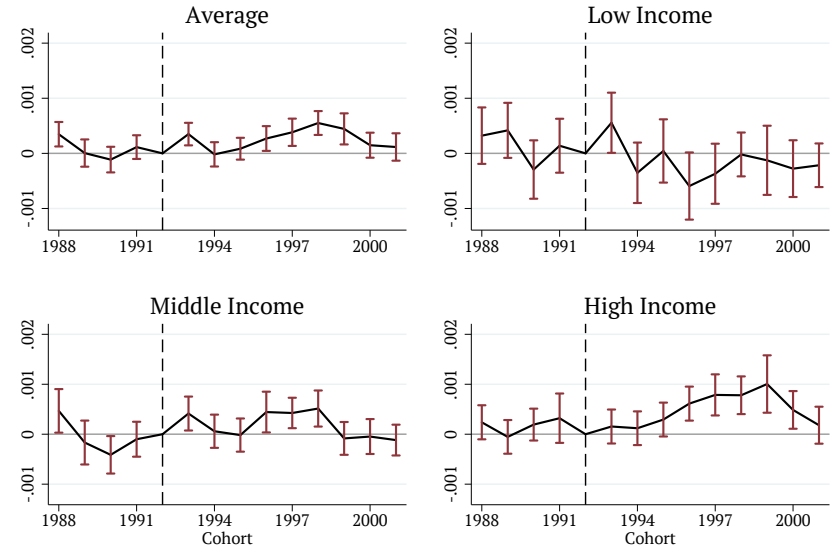
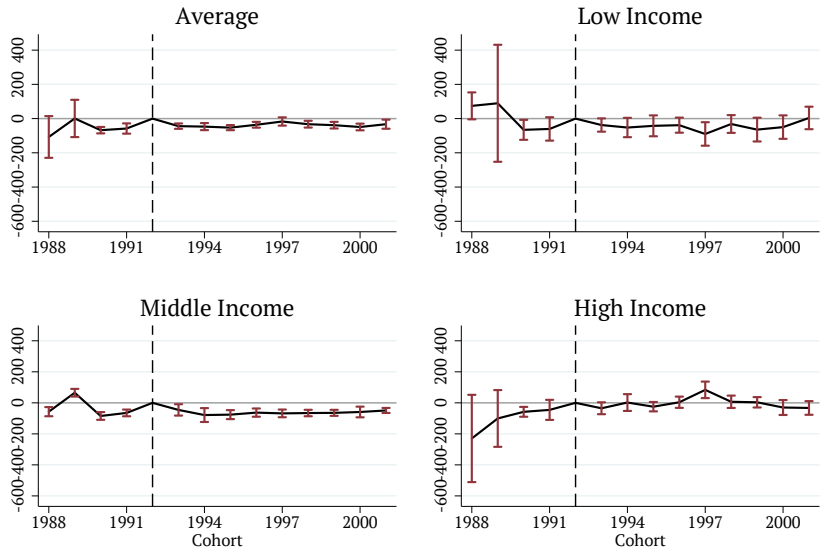


FIGURE 7: Probability of having at least a Bachelor level degree in 2015 or before, by family income.

[24] These outcome variables are only available for cohorts who started seventh grade between 1988 and 2001.

[25] *At least a Bachelor level degree in 2015* takes value 1 if the student has a degree that is equivalent to a Bachelor, or higher, in 2015, otherwise 0.



Outcome: Earnings in 2015

FIGURE 8: Earnings in 2015, by family income.

TABLE 5: Effect of school choice on academic degree and earnings.

	Earnings in 2015					
	(1)		(2)		(3)	
	RF	IV	RF	IV	RF	IV
Average	-68.23**	-347.04***	-64.52*	-338.76**	7.46	40.09
	(25.15)	(98.63)	(25.31)	(106.66)	(14.63)	(79.01)
		[115]		[137]		[145]
Low Income	-62.91	-341.19*	-54.59	-328.74	-48.71	-303.36
	(40.94)	(173.14)	(37.55)	(185.53)	(40.11)	(213.03)
		[32]		[39]		[40]
Middle Income	-39.89***	-192.60***	-39.08***	-195.21***	-35.62***	-177.55***
	(9.80)	(57.81)	(9.49)	(57.51)	(8.48)	(50.78)
		[172]		[179]		[177]
High Income	11.01	60.00	5.98	32.46	85.48**	466.18**
	(32.80)	(182.56)	(33.29)	(182.52)	(29.94)	(165.85)
		[146]		[162]		[187]
	At least a Bachelor level degree in 2015					
	(1)		(2)		(3)	
	RF	IV	RF	IV	RF	IV
Average	-0.00029*	-0.00146**	-0.00017	-0.00088	0.00019**	0.00100*
	(0.000132)	(0.000565)	(0.000118)	(0.000568)	(0.000071)	(0.000390)
		[115]		[137]		[145]
Low Income	-0.00057	-0.00307*	-0.00053**	-0.00317***	-0.00026	-0.00164*
	(0.000335)	(0.001335)	(0.000193)	(0.000816)	(0.000148)	(0.000780)
		[32]		[39]		[40]
Middle Income	0.00011	0.00051	0.00007	0.00034	0.00022**	0.00109**
	(0.000088)	(0.000433)	(0.000087)	(0.000439)	(0.000083)	(0.000407)
		[172]		[179]		[177]
High Income	0.00036***	0.00197***	0.00030**	0.00161**	0.00035***	0.00192**
	(0.000093)	(0.000566)	(0.000091)	(0.000543)	(0.000099)	(0.000588)
		[146]		[162]		[187]
FE	Yes		Yes		Yes	
Background	No		Yes		Yes	
Interactions	No		No		Yes	

Notes: Our outcome variables are: *Earnings in 2015* defined by the sum of work and entrepreneurial related earnings and capital income of the student in 2015 and *At least a Bachelor level degree in 2015* defined by taking value 1 if the student has a degree that is equivalent to a Bachelor, or higher, in 2015, otherwise 0. RF refers to reduced form estimations and IV to instrumental variable estimations. FE refers to cohort and municipality fixed effects. Background refers to individual and parental level controls. Interactions refers to interactions between the cohort and background level controls. Standard errors are clustered at the municipality level and shown in parentheses. First stage F-tests in square brackets.
* p<0.05, ** p<0.01, *** p<0.001.

We then study the impacts of school choice on *Earnings in 2015*.[26] The reduced form and IV estimates for the average effect on earnings in 2015 are positive but insignificant (Figure 8 and Table 5).

The breakdown of these average results by family income shows that the effects are positive for students from high income families and negative but insignificant for students from low income families, whereas for students from middle income families the reduced form estimate is negative and significant. However, the estimates on earnings should be interpreted with caution as Figure 8 shows some evidence of the differential pre-trends. Furthermore, some students are quite young and studying when we observe them in 2015 and may thus have zero earnings (and no higher education). The share of zero earners is highest among the students from low income families and most of the high income family students, who have zero earnings, are still studying. This is illustrated in Figure A1 in our Appendix.

These results show that the positive market-level effects of the reform are unevenly distributed. This is surprising because students from all backgrounds make choices. The short-term benefits for students from high income families may translate into better chances of receiving a higher education later in life. One potential explanations is that students from high and middle income families are displacing low income students from the best (upper secondary) schools due to higher grades or, alternatively, better motivation.[27] This conclusion remains speculative at this stage. The next section will study potential channels of school choice that that aim at explain the underlying mechanisms of these effects.

5.3 Channels of School Choice

In this section, we study the potential channels of school choice to explain the distributional effects of the previous section. We begin by studying whether school choice changed the peer composition in comprehensive schools. We then continue by studying if school choice affected selection into education and occupations.

We measure the changes in peer or school quality using *average GPA of the comprehensive school*. We use the "leave-one-out principle": that is by leaving student's own GPA out of the average GPA calculation.[28] We find that the reform had on average a positive effect on the average

[26] *Earnings in 2015* is the sum of work and entrepreneurial related earnings and capital income of the student in 2015.

[27] There are likely to be only certain amount of seats available each year for the best upper secondary schools.

[28] We leave out around 10 municipalities in year 1994 in estimation due to high share of missing GPA values in these municipalities.

GPA of the comprehensive school (Figure 9 and Table 6): average GPA of the comprehensive school increases by 3pps (or 0.15 standard deviations) after the reform according to our reduced form and IV estimates.[29]

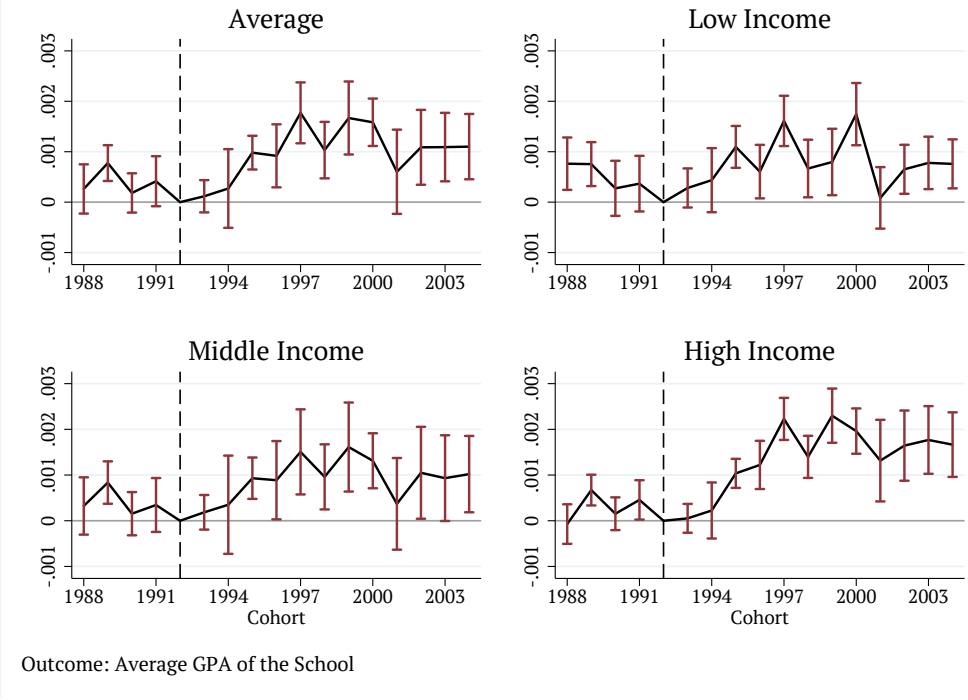


FIGURE 9: Reduced form results for average GPA of the comprehensive school

The results also show that students from high income families enter comprehensive schools with higher average GPA than students from low income families. Average GPA of the the comprehensive school increases more for students from high and middle income families, 6pps, whereas the effect is also positive but smaller, 2pps, for students from low income families after the reform.

Next, we study whether school choice affected selection into education and occupation. For selection into education, we use *log of average earnings of the education group in 2015*.[30] For selection into occupation, we use *log of average earnings of the occupation in 2015*.[31] We calculate both of these outcomes using the leave-one-out principle described above. These outcomes reflect the earnings potential of the education and occupation choice made by the student.[32]

[29] Our reduced form estimates are quantified by comparing a high treatment intensity municipality (i.e. Helsinki) to a low treatment intensity municipality (i.e. average municipality). Our IV estimates are quantified by an increase of 10pps in realized school choice.

[30] The variable is defined as the logarithm of the average earnings of everyone with the same level and field of education in Finland in 2015.

[31] This outcome is calculated as the logarithm of average earnings of everyone with the same occupation code in Finland in 2015 measured using the total FLEED sample.

[32] Unfortunately the data available to us is only for cohorts who started seventh grade between 1988 and 2001.

Table 6: Estimates for channels of school choice.

	Average GPA of the school					
	(1)		(2)		(3)	
	RF	IV	RF	IV	RF	IV
Average	0.00071***	0.00340***	0.00072***	0.00354***	0.00069***	0.00351***
	(0.000205)	(0.000773)	(0.000191)	(0.000770)	(0.000185)	(0.000768)
		[84]		[97]		[106]
Low Income	0.00029	0.00149	0.00028	0.00160	0.00036*	0.00213*
	(0.000160)	(0.000894)	(0.000158)	(0.000966)	(0.000145)	(0.000960)
		[33]		[40]		[40]
Middle Income	0.00060*	0.00267**	0.00056*	0.00260*	0.00059*	0.00273**
	(0.000259)	(0.001009)	(0.000253)	(0.001030)	(0.000253)	(0.001023)
		[118]		[123]		[131]
High Income	0.00122***	0.00646***	0.00121***	0.00636***	0.00116***	0.00612***
	(0.000207)	(0.000815)	(0.000202)	(0.000813)	(0.000197)	(0.000817)
		[100]		[109]		[129]
	Log of average earnings of the education group in 2015					
	(1)		(2)		(3)	
	RF	IV	RF	IV	RF	IV
Average	-0.00004	-0.00020	0.00008	0.00043	0.00036***	0.00194***
	(0.000082)	(0.000402)	(0.000069)	(0.000388)	(0.000066)	(0.000318)
		[115]		[137]		[145]
Low Income	-0.00039	-0.00212*	-0.00032**	-0.00195***	-0.00010	-0.00061
	(0.000249)	(0.001025)	(0.000112)	(0.000528)	(0.000093)	(0.000572)
		[32]		[39]		[40]
Middle Income	0.00034***	0.00163***	0.00033***	0.00162***	0.00040***	0.00197***
	(0.000086)	(0.000350)	(0.000087)	(0.000368)	(0.000105)	(0.000432)
		[172]		[179]		[177]
High Income	0.00062***	0.00335***	0.00054***	0.00295***	0.00056***	0.00303***
	(0.000087)	(0.000587)	(0.000083)	(0.000554)	(0.000101)	(0.000663)
		[146]		[162]		[187]
	Log of average earnings of the occupation in 2015					
	(1)		(2)		(3)	
	RF	IV	RF	IV	RF	IV
Average	-0.00061***	-0.00295***	-0.00054***	-0.00268***	-0.00021***	-0.00108***
	(0.000126)	(0.000431)	(0.000120)	(0.000442)	(0.000038)	(0.000192)
		[162]		[191]		[206]
Low Income	-0.00086***	-0.00419***	-0.00077***	-0.00402***	-0.00063***	-0.00342***
	(0.000201)	(0.000630)	(0.000159)	(0.000584)	(0.000132)	(0.000569)
		[66]		[90]		[94]
Middle Income	-0.00031***	-0.00146**	-0.00031***	-0.00153***	-0.00029***	-0.00143**
	(0.000085)	(0.000451)	(0.000075)	(0.000419)	(0.000087)	(0.000481)
		[223]		[228]		[216]
High Income	0.00002	0.00011	-0.00003	-0.00017	0.00011	0.00055
	(0.000122)	(0.000626)	(0.000112)	(0.000556)	(0.000082)	(0.000446)
		[155]		[167]		[199]
FE	Yes		Yes		Yes	
Background	No		Yes		Yes	
Interactions	No		No		Yes	

Notes: RF refers to reduced form estimations and IV to instrumental variable estimations. FE refers to cohort and municipality fixed effects. Background refers to individual and parental level controls. Interactions refers to interactions between the cohort and background level controls. Standard errors are clustered at the municipality level and shown in parentheses. First stage F-tests in square brackets.
* p<0.05, ** p<0.01, *** p<0.001.

We make a buzzling finding: on average the students end up with an education with higher earnings potential after the reform and they have an occupation with lower earnings potential after the reform (Figures **??** and 11). According to our reduced form and IV estimates, in Table **??**, earnings of the education group increase on average by 2 percent whereas the earnings of the occupation decrease on average by 1 percent if we quantify the estimates with the same logic as above.

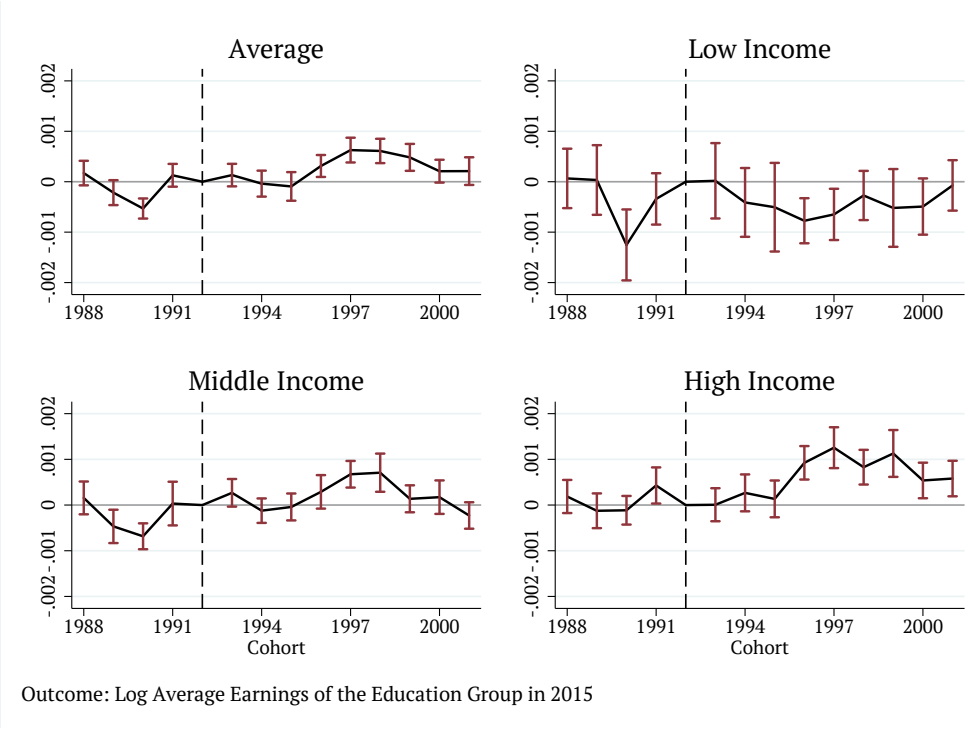


FIGURE 10: Reduced form results for log average earnings of the education group in 2015.

The breakdown of these results with respect to family income shows that students from high income families end up with an education of 3 percent higher earnings potential, whereas for students from low income families the effect is negative but insignificant. On the other hand, students from low income families get an occupation with 3 percent lower earnings potential after the reform. The effect is also negative and significant for students from middle income families, 1 percent lower earnings potential, and positive but insignificant for students from high income families.

Summarizing these results together with the results from the previous section, we find that the benefits of school choice are unequally distributed: students from high income families enter

Additionally, the latter outcome is available only for those students who are employed in 2015.

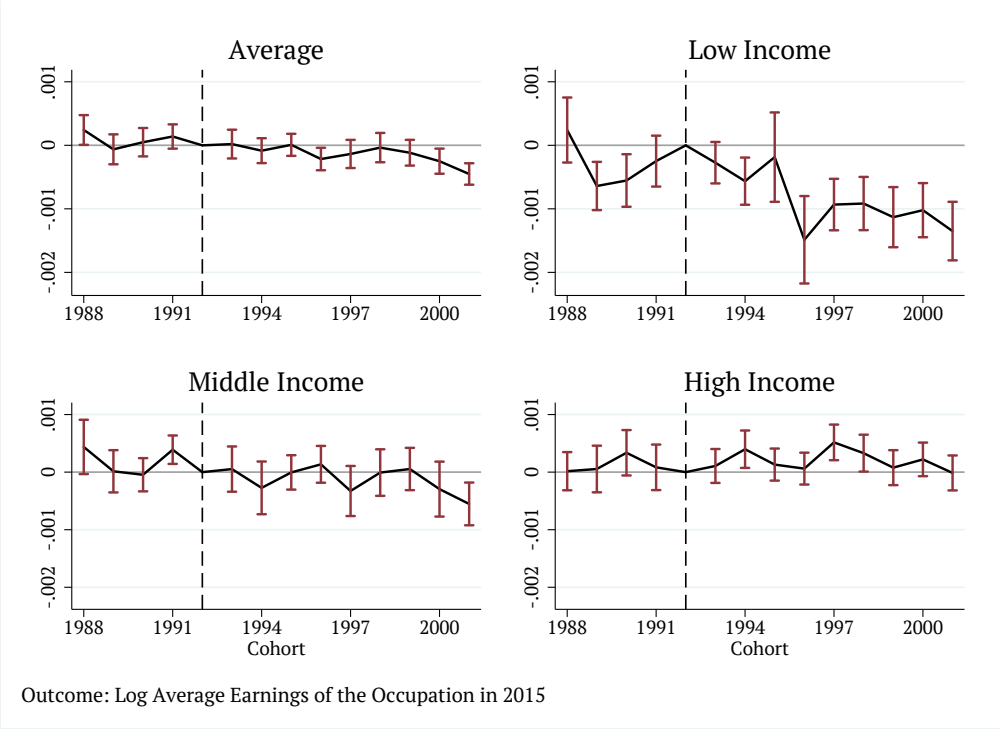


FIGURE 11: Reduced form results for log average earnings of the occupation in 2015.

better comprehensive schools and, may as a result, receive better grades after the reform. They are also more likely to graduate from high school than their less well-off peers after the reform. These short-term gains translate, not only into better chances of getting a higher education later in life, but students from high income families end up with an education with higher earnings potential. On the other hand, even if students from low income families do not see negative effects on the short-term, they are less likely to get a higher education and end up with an occupation with lower earnings potential. This is surprising, given that we see that students from all backgrounds make choices after the reform.

One crucial feature of school choice is its tendency to re-sort students across schools according to ability or family background (see Epple and Romano, 1998; Hsieh and Urquiola, 2006). For example, if high demand schools have an incentive to cream-skim, this may result in increasing segregation of schools with respect to ability (or family background). To investigate this, we use two segregation measures, *segregation of schools with respect to GPA* and *segregation of schools with respect to residual GPA*, as our segregation outcomes. As earlier, GPA refers to the student's GPA at the end of the ninth grade. We get the residual GPA by running a regression with mother's and father's income and education level (3 levels: no education after comprehensive

TABLE 7: Segregation of schools.

GPA		Residual GPA	
RF	IV	RF	IV
0.000826***	0.00275***	0.000381*	0.00127***
(0.000185)	(0.000310)	(0.000147)	(0.000272)
First stage F-statistic		22	
Number of observations		7242	

Notes: The unit of observation is a municipality. We control for cohort and municipal level fixed effects. RF refers to reduced form estimations and IV to instrumental variable estimations. Standard errors are in parentheses.
* p<0.05, ** p<0.01, *** p<0.001.

school, some education, and higher education) and using the residual from these regressions as our outcome variable. The motivation for the residual GPA is the high correlation of GPA and parental background characteristics, such as education. In order to have a more "pure" measure of ability, we use the residual GPA.

Our segregation index of choice is a coefficient of determination, R^2. The index can take values from 0 to 1, where 0 implies that schools of the municipality do not explain GPAs of the students, and are thus not segregated. Municipalities without schools take value 0. We adjust our segregation index to measure segregation from randomness, and this procedure is described in Appendix. After the adjustment, the index will take values between -1 and 1.

Our segregation measure is a municipal-level measure that we obtain from regressions where we explain GPA or residual GPA of the students by the schools of the municipality. Thus, our segregation index answers the question how well the schools of the municipality explain the student-level variation in GPA. Another useful feature of this index is that it can be used in the case of a continuous variable, such as GPA, unlike many other widely used segregation indices that measure segregation only with respect to a dummy variable. Comparing different segregation indices across years and municipalities is not straightforward. A more thorough investigation with a comparison of several segregation indices may be interesting, but is left for future research.

We next turn to our segregation results, shown in Figures 12 and 13. The figures show the effects on segregation of schools with respect to GPA of the students at the end of the ninth grade and segregation of schools with respect to residual GPA, respectively. The reduced form and IV results are summarized in Table 7.

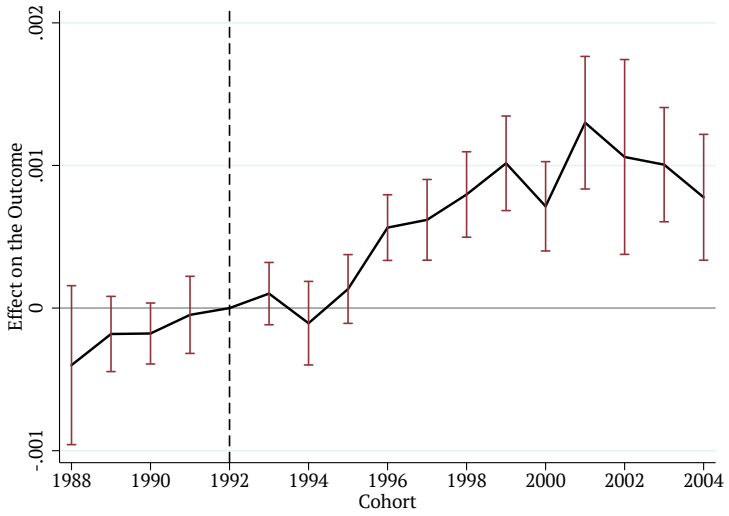


FIGURE 12: Segregation of Schools with Respect to GPA.

FIGURE 13: Segregation of Schools with Respect to residual GPA.

Our results suggest that segregation of schools with respect to GPA increased after the reform in municipalities with more school choice. The effect is quite substantial (more than 4 standard deviations) when we compare a municipality with few schools to municipality with more than 50 schools in reduced form estimation. The IV estimation shows a little more modest estimate and increase of about 3 standard deviations.

The results for the effects for segregation of schools with respect to residual GPA are also positive and about half of the size of the estimates of segregation with respect to GPA. This suggests that half of the effect on segregation of schools with respect to GPA is explained by students sorting

across schools with respect to parental background after the reform.

6 Conclusions

We study the market-level and distributional effects of a large-scale public school choice reform that introduced municipal-level school choice in Finland. Our identification strategy is based on differences-in-differences with continuous treatment intensity: we use municipal-level variation in school choice possibilities across municipalities and time to capture the intensity of the reform. We also use this variation as an instrument for realized choice. The advantage of our identification strategy is that it captures the effects of the reform at school market level. This helps us to account for local (municipal-level) general equilibrium effects of the reform.

We find that the reform increased choice considerably and students from all socio-economic groups make choices. Despite this, we find that the effects of the reform on various education and labor outcomes are heterogeneous with respect to family background. We find that higher SES students benefit from more choice possibilities, as the reform increased their GPA and education outcomes more. Students from low SES groups do not benefit from the reform on the short-term but are less likely to have a higher education on the long-term. Our examination of potential channels of school choice suggest that these effects could be explained by displacement and selection into education. We also find evidence of increased sorting of students with respect to family background and ability.

Our findings suggest the following policy conclusions. First, our results emphasize the importance of school counseling and decision support for choices. In Finland, there is no publicly available information on student attainment such as average test scores or grades. Moreover, Finnish schools have consistently ranked as one of the World's least segregated in terms of student outcomes in international PISA comparisons, suggesting small (quality) differences between schools. (OECD, 2013) This means making a choice based on average attainment or grades of students is difficult. Indeed, previous survey studies (see for example Seppänen (2006)) have found that choices are made based on perceived differences between schools (such as reputation of the school or area it serves, facilities or special classes offered by the school). For example, schools could provide more information about choice possibilities and support the decision making of less well-off students.

Second, because a school that offers special classes can use aptitude tests to select their students, this may result in sorting by ability. This type of cream-skimming by popular schools could be avoided by restricting the use of aptitude tests or previous grades in student admittance to special

classes.[33] Similarly, the municipal-level variation in policies for reimbursing school transportation can affect propensity of school choice. This would also be an important extension to our study.

Our results suggest that public school choice programs can result in an unequal distribution of benefits and increasing inequality. Our results show surprisingly large effects in Finland, despite small quality differences between Finnish schools in international comparisons. Consequently, these effects can potentially be even greater in countries with greater quality differences between schools and more diverse population.

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A Appendix

A.1 School choice enabling law changes

School choice was introduced in the Finnish education system as a result of several small and bigger law changes that were part of the government decentralization process. The aim of the decentralization process was to give municipalities more decision making power regarding the organization of public services. It also aimed at relaxing unnecessary regulations that complicated public sector service production. Also, efficiency and cost reduction were central in the midst of a big recession and it was thought that municipalities knew best how to reduce costs. (Varjo, 2007)

The first laws took effect in 1991. They did not yet introduce school choice, but made implementing school choice easier in the future. The law changes allowed students to enter schools with special educational tasks from outside their own catchment area (L 171/1991, 1991, article 7). Another law gave permission to offer comprehensive school level education in foreign language (L 261/1991, 1991, article 25) as previously schools could only offer language immersion.

The biggest and most important law reforms took effect in 1993, although, the laws did not mention school choice as such. L 705/1992 (1992) changed the way government subsidies for organizing comprehensive level schooling were paid to municipalities. Before, government subsidies were tied to the catchment areas, and in order to maximize the subsidies, each school was assigned a strict catchment area. After the the law change, these subsidies were based on realized costs and number of students.

Two other clauses from laws were also removed and took effect in 1993. The clause, that stated that students could get into other than their assigned school for a special reason only, was relaxed (L 707/1992, 1992, article 38), and the old maximum distance (5km) to school -clause was removed (L 682/1993, 1993). Essentially, the latter law change, combined with the change in government subsidies, gave municipalities more freedom to design their school network and allow students to cross the catchment area boundaries in a manner that would not affect the finances of the municipality.

Even though the laws did not formally state school choice (unless some special conditions were met) until 1999, (L 628/1998, 1998, article 28), it is mentioned in "passing" in one of the laws that took effect in 1993. It was also stated that if a student is admitted to other than the assigned neighborhood school for some other reason than the special conditions listed in article 38, then the parents are required to cover the school commuting costs. (L 707/1992, 1992, article 47) Essentially,

this means that school choice without a special reason was allowed after 1993.

In 1994, previously strict curriculum guidelines were also removed. This was one of the main contributors for the popularization of school choice. It enabled schools to specialize to attract students from the entire municipality. Guidelines for curriculum were replaced only by a broader framework that allowed schools to introduce special classes or so called weighted curricula. This meant extra classes with a focus on natural sciences, sports or mathematics, for example. In many cases, these special classes did not have a catchment area, and could take students from all over the municipality. In 1999, they were even allowed to use aptitude tests for student intake. (L 628/1998, 1998, article 28)

A.2 Descriptive Evidence

Table A1 summarizes our control variables used our regression results before and after the reform, and in small (less than ten schools) and large (more than ten schools) municipalities.

Tables A4 and A4 summarize our first stage variables and outcome variables before and after the reform, and in small (less than ten schools) and large (more than ten schools) municipalities.

Our descriptive statistics are defined as following. Earnings of a parent measures all the work-related earnings (in Euros), whereas family income is the sum of parents' (single or both) earnings and all non-work related income (such as maternity leave, social or unemployment benefits). Education of a parent is an indicator variable that can take three possible values: no formal education after comprehensive school, upper secondary schooling, or higher education. Employment of a parent indicates the employment situation of the parent and takes value 1, if the parent is primarily unemployed, otherwise 0. We also control for a single parent with a dummy that takes value 1, if student lives only with one of the parents, and 0 otherwise. This is a proxy for single parent, as the parent who the child lives with might have remarried and hence this dummy also proxies whether the parents of the student have divorced or not. All of these family background control variables are measured the year the student turns 14. In addition to these variables, we control for the gender of the student.[1]

[1]Gender does not significantly differ between socio-economic groups, small and large municipalities, or before and after the reform.

TABLE A1: Descriptive statistics of control variables

	More than 10 Schools		Less than 10 schools	
	1988-1992	1993-2004	1988-1992	1993-2004
	(1)	(2)	(3)	(4)
Average family income (in 2015 euros)				
Average	63,200	72,672	49,554	57,487
	(40,783)	(71,453)	(29,915)	(47,749)
Low Income	24,384	26,438	24,823	26,953
	(9,318)	(12,665)	(8,894)	(12,051)
Middle Income	50,409	58,191	48,944	56,636
	(7,614)	(16,844)	(7,559)	(16,496)
High Income	94,210	114,092	84,456	98,708
	(48,109)	(101,044)	(45,769)	(84,605)
Mother's earnings (in 2015 euros)				
Average	21,848	26,018	15,977	19,615
	(14,460)	(22,547)	(12,461)	(16,707)
Low Income	10,827	11,162	7,519	8,441
	(10,398)	(12,769)	(8,971)	(11,080)
Middle Income	19,242	23,023	16,608	20,309
	(10,081)	(14,200)	(9,962)	(13,477)
High Income	29,526	37,414	25,763	32,177
	(15,865)	(28,080)	(14,360)	(20,130)
Father's earnings (in 2015 euros)				
Average	33,140	38,155	22,667	27,220
	(28,125)	(61,543)	(20,523)	(42,688)
Low Income	6,012	5,779	6,064	6,159
	(9,621)	(10,398)	(9,459)	(10,621)
Middle Income	24,701	28,271	22,908	27,237
	(13,224)	(17,703)	(13,609)	(17,543)
High Income	54,262	66,863	44,437	54,133
	(30,439)	(90,907)	(25,032)	(80,746)
Mother's secondary or higher educated				
Average	0.68	0.79	0.66	0.80
	(0.47)	(0.41)	(0.47)	(0.40)
Low Income	0.52	0.61	0.54	0.69
	(0.50)	(0.49)	(0.50)	(0.46)
Middle Income	0.61	0.76	0.65	0.82
	(0.49)	(0.43)	(0.48)	(0.39)
High Income	0.82	0.90	0.83	0.91
	(0.38)	(0.29)	(0.37)	(0.29)
Father's secondary or higher educated				
Average	0.67	0.73	0.60	0.71
	(0.47)	(0.44)	(0.49)	(0.45)
Low Income	0.40	0.46	0.43	0.54
	(0.49)	(0.50)	(0.49)	(0.50)
Middle Income	0.61	0.71	0.60	0.73
	(0.49)	(0.45)	(0.49)	(0.44)
High Income	0.86	0.90	0.84	0.88
	(0.35)	(0.30)	(0.36)	(0.32)
Unemployed mother				
Average	0.04	0.08	0.06	0.10
	(0.20)	(0.28)	(0.23)	(0.30)
Low Income	0.11	0.23	0.10	0.20
	(0.31)	(0.42)	(0.30)	(0.40)
Middle Income	0.04	0.07	0.05	0.08
	(0.19)	(0.26)	(0.21)	(0.27)
High Income	0.01	0.02	0.02	0.02
	(0.12)	(0.14)	(0.13)	(0.15)
Unemployed father				
Average	0.05	0.08	0.06	0.07
	(0.22)	(0.27)	(0.23)	(0.26)
Low Income	0.15	0.26	0.12	0.19
	(0.35)	(0.44)	(0.32)	(0.39)
Middle Income	0.06	0.06	0.05	0.05
	(0.23)	(0.25)	(0.21)	(0.21)
High Income	0.01	0.01	0.01	0.00
	(0.09)	(0.09)	(0.08)	(0.07)
Single parent				
Average	0.28	0.35	0.20	0.26
	(0.45)	(0.48)	(0.40)	(0.44)
Low Income	0.59	0.65	0.34	0.41
	(0.49)	(0.48)	(0.47)	(0.49)
Middle Income	0.26	0.33	0.16	0.22
	(0.44)	(0.47)	(0.36)	(0.41)
High Income	0.16	0.20	0.11	0.16
	(0.36)	(0.40)	(0.32)	(0.36)
Foreign language speaker				
Average	0.006	0.026	0.002	0.007
	(0.079)	(0.160)	(0.045)	(0.081)
Low Income	0.026	0.093	0.005	0.018
	(0.160)	(0.291)	(0.073)	(0.133)
Middle Income	0.002	0.014	0.001	0.003
	(0.048)	(0.115)	(0.026)	(0.052)
High Income	0.002	0.005	0.001	0.002
	(0.043)	(0.071)	(0.030)	(0.043)

Notes: All values are means calculated over all individuals who live in municipalities with less than ten or more than ten schools before the reform and calculated separately for all cohorts before and after the reform. Standard errors are in parentheses.

TABLE A2: Descriptive statistics of first stage outcome variables

	More than 10 Schools		Less than 10 schools	
	1988-1992	1993-2004	1988-1992	1993-2004
	(1)	(2)	(3)	(4)
Share attending non-neighborhood school				
Average	29.94	38.25	9.39	11.56
	(45.80)	(48.60)	(29.17)	(31.97)
High Income	30.08	38.49	10.75	12.80
	(45.86)	(48.66)	(30.97)	(33.41)
Low Income	34.39	43.05	10.16	13.08
	(47.50)	(49.52)	(30.22)	(33.72)
Middle Income	28.03	35.83	8.45	10.28
	(44.92)	(47.95)	(27.82)	(30.38)
Share attending other than neighborhood school				
Average	26.76	36.60	7.77	9.53
	(44.27)	(48.17)	(26.78)	(29.36)
High Income	27.16	36.80	8.79	10.50
	(44.48)	(48.23)	(28.32)	(30.65)
Low Income	31.72	42.02	8.66	11.15
	(46.54)	(49.36)	(28.12)	(31.48)
Middle Income	24.41	33.91	6.91	8.31
	(42.95)	(47.34)	(25.36)	(27.60)
Mobility Index				
Average	0.432	0.526	0.113	0.135
	(0.315)	(0.308)	(0.225)	(0.244)
High Income	0.435	0.529	0.141	0.165
	(0.315)	(0.306)	(0.244)	(0.257)
Low Income	0.465	0.563	0.100	0.130
	(0.330)	(0.313)	(0.226)	(0.253)
Middle Income	0.415	0.506	0.108	0.126
	(0.307)	(0.305)	(0.215)	(0.232)

Notes: All values are means calculated over all individuals who live in municipalities with less than ten or more than ten schools before the reform and calculated separately for all cohorts before and after the reform. Standard errors are in parentheses.

TABLE A3: Descriptive statistics of education and labor market outcome variables

	More than 10 Schools		Less than 10 schools	
	1988-1992	1993-2004	1988-1992	1993-2004
	(1)	(2)	(3)	(4)
GPA				
Average	7.82	7.79	7.67	7.61
	(1.14)	(1.12)	(1.15)	(1.11)
Low Income	7.41	7.34	7.39	7.35
	(1.15)	(1.13)	(1.15)	(1.11)
Middle Income	7.63	7.64	7.62	7.57
	(1.13)	(1.10)	(1.13)	(1.09)
High Income	8.20	8.18	8.14	8.03
	(1.03)	(1.00)	(1.06)	(1.05)
High school graduate				
Average	0.62	0.62	0.52	0.52
	(0.48)	(0.48)	(0.50)	(0.50)
Low Income	0.44	0.42	0.39	0.39
	(0.50)	(0.49)	(0.49)	(0.49)
Middle Income	0.54	0.56	0.50	0.51
	(0.50)	(0.50)	(0.50)	(0.50)
High Income	0.80	0.81	0.74	0.73
	(0.40)	(0.39)	(0.44)	(0.44)
At least a secondary school education after 10 years				
Average	0.87	0.87	0.91	0.90
	(0.33)	(0.34)	(0.29)	(0.30)
Low Income	0.78	0.76	0.86	0.85
	(0.42)	(0.43)	(0.34)	(0.35)
Middle Income	0.86	0.86	0.91	0.91
	(0.35)	(0.35)	(0.28)	(0.29)
High Income	0.93	0.93	0.95	0.95
	(0.25)	(0.25)	(0.22)	(0.22)
At least a Bachelor level degree in 2015				
Average	0.46	0.39	0.43	0.38
	(0.50)	(0.49)	(0.50)	(0.49)
Low Income	0.30	0.23	0.32	0.27
	(0.46)	(0.42)	(0.47)	(0.45)
Middle Income	0.39	0.34	0.42	0.37
	(0.49)	(0.47)	(0.49)	(0.48)
High Income	0.61	0.53	0.62	0.54
	(0.49)	(0.50)	(0.49)	(0.50)
Earnings in 2015				
Average	38,921	24,811	35,445	24,823
	(160,701)	(33,285)	(75,291)	(23,609)
Low Income	32,712	20,457	30,753	22,138
	(225,543)	(24,195)	(33,097)	(23,192)
Middle Income	34,538	24,110	34,565	24,910
	(75,500)	(27,909)	(30,621)	(21,397)
High Income	46,550	27,969	44,124	28,066
	(192,744)	(41,844)	(155,109)	(28,539)

Notes: All values are means calculated over all individuals who live in municipalities with less than ten or more than ten schools before the reform and calculated separately for all cohorts before and after the reform. Standard errors are in parentheses.

TABLE A4: Descriptive statistics of education and labor market outcome variables

	More than 10 Schools		Less than 10 schools	
	1988-1992	1993-2004	1988-1992	1993-2004
	(1)	(2)	(3)	(4)
Average GPA of the comprehensive school				
Average	7.93	7.90	7.78	7.72
	(0.40)	(0.40)	(0.42)	(0.43)
Low Income	7.90	7.87	7.80	7.75
	(0.47)	(0.48)	(0.53)	(0.56)
Middle Income	7.89	7.86	7.78	7.72
	(0.39)	(0.38)	(0.39)	(0.40)
High Income	7.99	7.97	7.78	7.72
	(0.35)	(0.37)	(0.29)	(0.30)
Average earnings (in 2105 euros) of the occupation in 2015				
Average	42,635	36,660	40,282	35,866
	(21,325)	(16,192)	(18,604)	(14,381)
Low Income	38,442	33,387	37,304	33,863
	(18,583)	(13,935)	(16,510)	(12,875)
Middle Income	39,675	34,933	39,518	35,282
	(18,700)	(14,229)	(17,540)	(13,565)
High Income	47,411	40,090	45,945	39,620
	(23,875)	(18,446)	(22,148)	(17,051)
Average earnings (in 2015 euros) of the education group in 2015				
Average	30,451	26,552	29,749	26,481
	(16,236)	(14,627)	(14,367)	(13,022)
Low Income	25,350	21,878	26,616	23,794
	(13,542)	(11,447)	(12,492)	(11,139)
Middle Income	28,061	25,033	29,248	26,138
	(14,280)	(12,929)	(13,663)	(12,294)
High Income	35,369	30,831	35,320	30,814
	(17,989)	(16,741)	(16,769)	(15,700)
Segregation w.r.t. GPA				
	0.030	0.051	0.001	0.002
	(0.020)	(0.028)	(0.008)	(0.013)
Segregation w.r.t. residual GPA				
	0.017	0.027	0.001	0.002
	(0.012)	(0.015)	(0.007)	(0.011)

Notes: All values are means calculated over all individuals who live in municipalities with less than ten or more than ten schools before the reform and calculated separately for all cohorts before and after the reform. Standard errors are in parentheses.

A.3 Segregation from Randomness

Here we discuss how we construct our segregation measures.

We begin by correcting our segregation index to measure segregation from. This is important because even if students were randomly allocated to schools of the municipality, segregation of schools is unlikely to be 0. This is an issue if school sizes are small and could cause systematic bias in our segregation measures.(Carrington and Troske, 1997)

We want to know whether school choice introduced *systematic* sorting of students. We therefore use a method introduced by Carrington and Troske (1997). We adjust our segregation index by subtracting the expected random segregation from the measured segregation, and scale this to range between -1 and 1. This is

$$\hat{Z}_m = \begin{cases} \frac{Z_m - E(Z_m)}{1 - E(Z_m)}, \text{ if } Z_m \geq E(Z_m) \\ \frac{Z_m - E(Z_m)}{E(Z_m)}, \text{ if } Z_m < E(Z_m) \end{cases} \tag{A1}$$

where Z_m the municipal level segregation index, directly measured from data and $E(Z_m)$ the expected value of the segregation from randomness.

We obtain the expected value, $E(Z_m)$, by randomly allocating students to schools each year within a municipality, keeping school and cohort size fixed. We calculate the segregation of schools at municipal level for the random allocation and repeat this 500 times. The sample mean of these segregation indices is the expected value of segregation from randomness.

A more detailed measurement of segregation across years and municipalities may require the use of several different types of segregation indices. This is out of the scope of this paper.

A.4 The share of Zero-Earners

Figure A1 plots the shares of zero earners and the zero of these zero earners that still study (heterogeneity by family income).

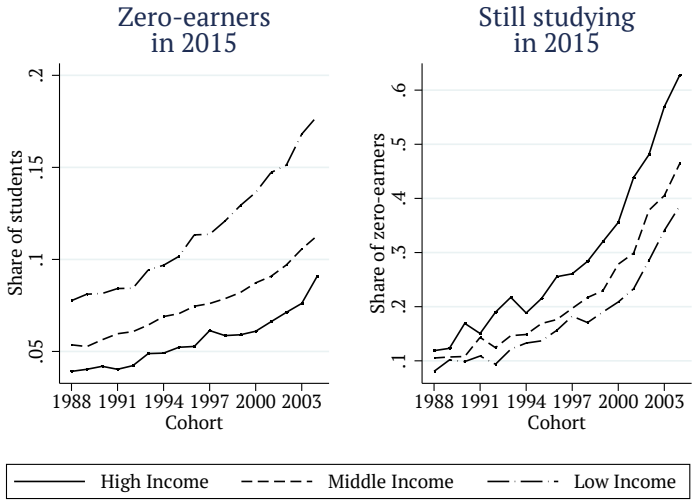


FIGURE A1: Share of students who have zero earnings by cohort and family income. Share of zero-earners still studying in 2015.

A.4.1 DID STUDENTS MAKE CHOICES AFTER THE REFORM?

Here we present some descriptive evidence on whether students make choices after the reform. Figure A2 shows the development of our second proxy for school choice, *other than neighborhood school*.

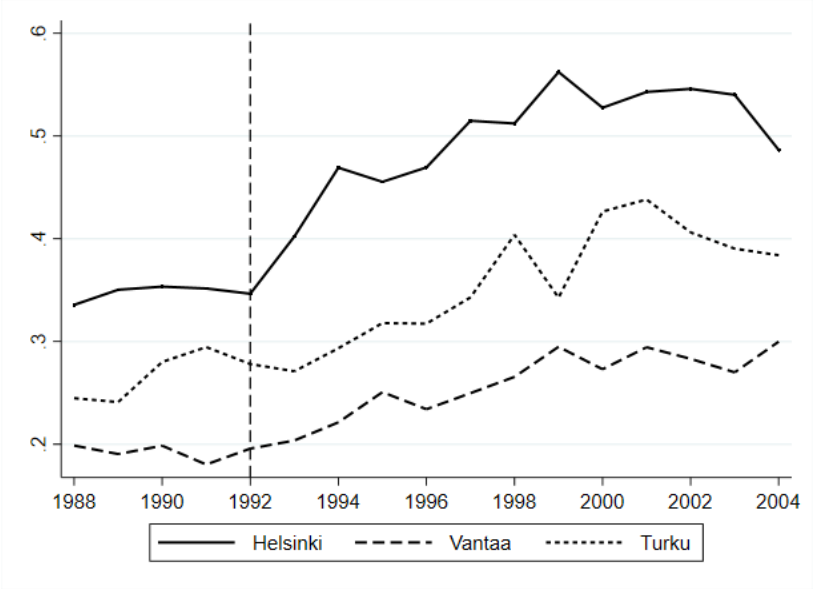
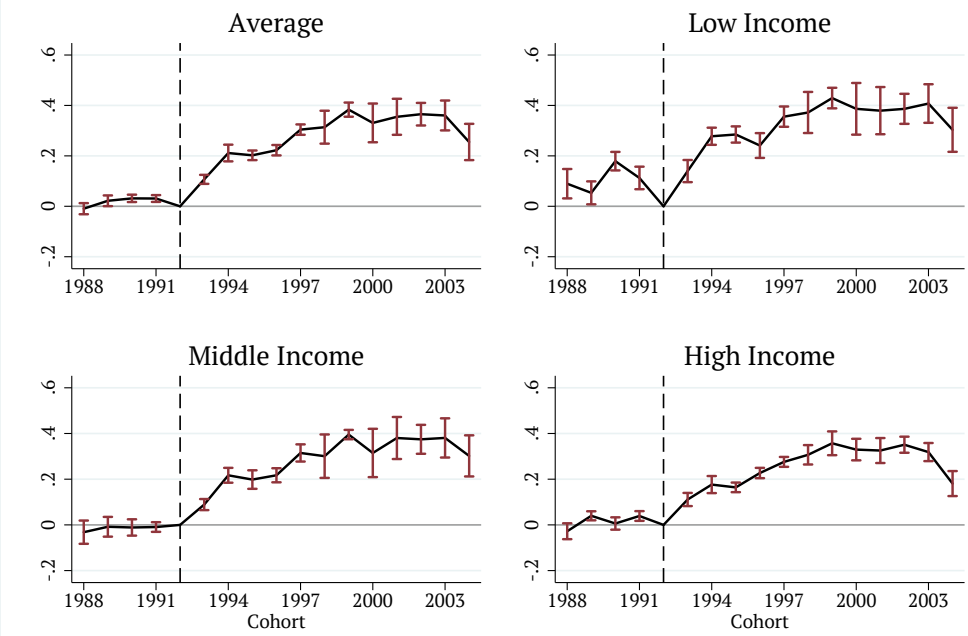


FIGURE A2: The development of the share of students attending other than the school that at least 30per cent of the students of the region attend in Helsinki, Turku and Vantaa.

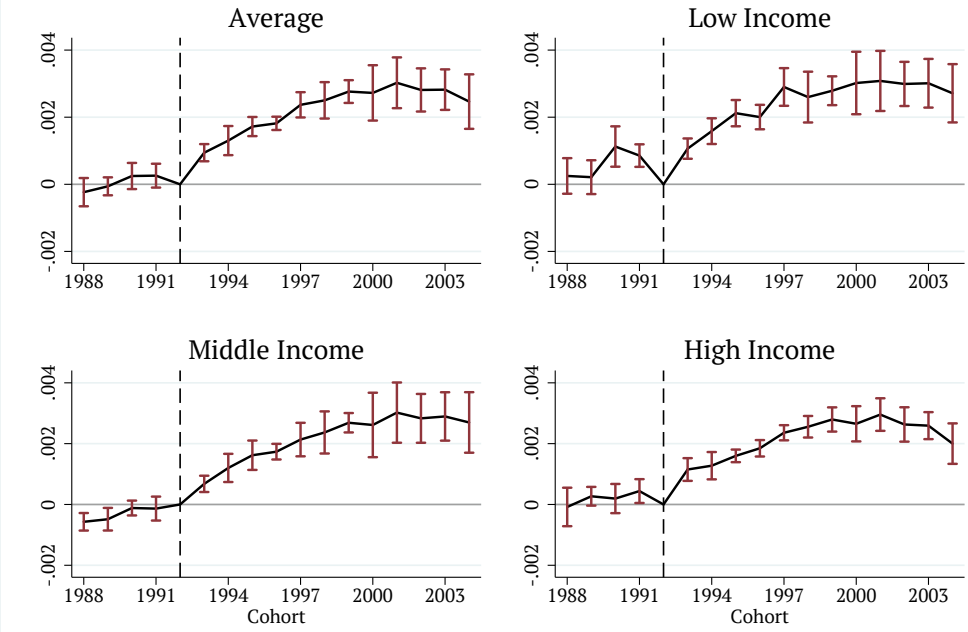
A.5 First Stage Results

This subsection shows our first stage results using the two alternative measures for school choice (Figures A3 and A4).



Outcome: Share Attending Other than the Neighborhood School

FIGURE A3: Attending other than a school that at least 30per cent of the students in the region attends, by family income.



Outcome: Mobility Index

FIGURE A4: Mobility Index, by family income.

A.6 IV Results Based on Alternative Choice Approximation

Here we present the IV estimation results for our education and labor market outcomes as well as for our channels of school choice using our alternative approximation for non-neighborhood school, *other than neighborhood school*, (Tables A5, for our education outcomes and the labor market outcome, and Tables A7 for the channels of school choice). Lastly, Table A8 presents our segregation using the alternative measure.

TABLE A5: IV Estimates for short-term education outcomes using other than neighborhood school as a proxy for choice

	GPA		
	(1)	(2)	(3)
Average	0.0021**	0.00389***	0.00607***
	(0.000649)	(0.000659)	(0.000942)
	[168]	[203]	[243]
Low Income	-0.0017	-0.00073	0.00062
	(0.001803)	(0.000999)	(0.001085)
	[87]	[124]	[132]
Middle Income	0.0056***	0.00532***	0.00616***
	(0.001252)	(0.001181)	(0.001211)
	[289]	[317]	[295]
High Income	0.0087***	0.00859***	0.00854***
	(0.001099)	(0.001042)	(0.001082)
	[166]	[180]	[242]
	High school graduate		
	(1)	(2)	(3)
Average	-0.00040	0.00020	0.00158***
	(0.000299)	(0.000316)	(0.000183)
	[171]	[210]	[251]
Low Income	-0.00125	-0.00135	-0.00006
	(0.001266)	(0.000856)	(0.000867)
	[74]	[106]	[111]
Middle Income	0.00114***	0.00081***	0.00140***
	(0.000256)	(0.000226)	(0.000248)
	[365]	[402]	[376]
High Income	0.00257***	0.00236***	0.00255***
	(0.000327)	(0.000297)	(0.000297)
	[164]	[176]	[233]
At least an upper secondary level education after 10 years			
	(1)	(2)	(3)
Average	-0.00001	0.00034	0.00065***
	(0.000235)	(0.000195)	(0.000183)
	[235]	[298]	[353]
Low Income	-0.00086	-0.00065	-0.00011
	(0.000610)	(0.000412)	(0.000433)
	[89]	[133]	[138]
Middle Income	0.00081***	0.00086***	0.00088***
	(0.000146)	(0.000144)	(0.000146)
	[550]	[618]	[536]
High Income	0.00110***	0.00101***	0.00099***
	(0.000246)	(0.000231)	(0.000281)
	[221]	[237]	[306]
Dummies	Yes	Yes	Yes
Background	No	Yes	Yes
Interactions	No	No	Yes

Notes: Standard errors in parentheses. F-tests in square brackets. Dummies refer to cohort and municipal fixed effects. Background refers to individual and parental level controls. Interactions refers to cohort-specific individual and parental background controls.
* p<0.05, ** p<0.01, *** p<0.001

TABLE A6: IV estimates for long-term education and labor market outcomes Using other than neighborhood school as a proxy for choice

	Earnings in 2015		
	(1)	(2)	3
Average	-255.90**	-248.26**	28.98
	(79.15)	(84.25)	(57.11)
	[240]	[302]	[355]
Low Income	-246.04	-230.71	-208.21
	(137.39)	(142.05)	(157.11)
	[82]	[121]	[131]
Middle Income	-138.06***	-138.60***	-125.60***
	(37.28)	(36.62)	(32.62)
	[622]	[683]	[555]
High Income	45.75	24.78	353.51**
	(138.94)	(139.23)	(125.19)
	[203]	[219]	[278]
	At least a Bachelor level degree in 2015		
	1	2	3
Average	-0.00108*	-0.00065	0.00072**
	(0.000439)	(0.000428)	(0.000279)
	[240]	[302]	[355]
Low Income	-0.00221*	-0.00223***	-0.00113
	(0.001093)	(0.000669)	(0.000578)
	[82]	[121]	[131]
Middle Income	0.00036	0.00024	0.00077**
	(0.000308)	(0.000310)	(0.000292)
	[622]	[683]	[555]
High Income	0.00150***	0.00123**	0.00145***
	(0.000424)	(0.000409)	(0.000437)
	[203]	[219]	[278]
Dummies	Yes	Yes	Yes
Background	No	Yes	Yes
Interactions	No	No	Yes

Notes: Dummies refers to cohort and municipality fixed effects. Background refers to individual and parental level controls. Interactions refers to cohort-specific individual and parental background controls. Standard errors in parentheses. F-tests in square brackets.

* p<0.05, ** p<0.01, *** p<0.001

TABLE A7: IV estimates for channels of school choice using other than neighborhood school as a proxy for choice

	Average GPA of the school		
	1	2	3
Average	0.0025***	0.0026***	0.0026***
	(0.000611)	(0.000599)	(0.000588)
	[170]	[207]	[243]
Low Income	0.0011	0.0011	0.0015*
	(0.000622)	(0.000658)	(0.000621)
	[77]	[110]	[115]
Middle Income	0.0019*	0.0019*	0.0019*
	(0.000771)	(0.000778)	(0.000766)
	[350]	[384]	[355]
High Income	0.0049***	0.0049***	0.0047***
	(0.000616)	(0.000612)	(0.000614)
	[160]	[172]	[224]
	Log of average earnings of the education group in 2015		
	1	2	3
Average	-0.00015	0.00031	0.00140***
	(0.000299)	(0.000279)	(0.000233)
	[240]	[302]	[355]
Low Income	-0.00153	-0.00137***	-0.00042
	(0.000830)	(0.000410)	(0.000394)
	[82]	[121]	[131]
Middle Income	0.00117***	0.00115***	0.00139***
	(0.000274)	(0.000284)	(0.000335)
	[622]	[683]	[555]
High Income	0.00256***	0.00225***	0.00230***
	(0.000433)	(0.000412)	(0.000489)
	[203]	[219]	[278]
	Log of average earnings of the occupation in 2015		
	1	2	3
Average	-0.002220***	-0.00201***	-0.00080***
	(0.000359)	(0.000363)	(0.000140)
	[313]	[382]	[446]
Low Income	-0.003131***	-0.00294***	-0.00245***
	(0.000589)	(0.000532)	(0.000471)
	[206]	[347]	[347]
Middle Income	-0.001065***	-0.00110***	-0.00103**
	(0.000312)	(0.000284)	(0.000330)
	[732]	[800]	[621]
High Income	0.000082	-0.00013	0.00043
	(0.000486)	(0.000433)	(0.000344)
	[194]	[204]	[250]
Dummies	Yes	Yes	Yes
Background	No	Yes	Yes
Interactions	No	No	Yes

Notes: Dummies refers to cohort and municipality fixed effects. Background refers to individual and parental level controls. Interactions refers to cohort-specific individual and parental background controls. Standard errors in parentheses. F-tests in square brackets.

* p<0.05, ** p<0.01, *** p<0.001.

TABLE A8: IV estimates for segregation of schools using other than neighborhood school as a proxy for choice

GPA	Residual GPA
0.00241***	0.00111*** (0.000278)
(0.000286)	
First stage F-statistic 49	
Number of observations 7242	

Notes: These are municipal-level regressions. We control for cohort and municipal level fixed effects. Standard errors in parentheses.
* p<0.05, ** p<0.01, *** p<0.001

A.7 Robustness Checks

A.7.1 Municipal-Level Controls

As our first robustness check, we check whether our effects are driven by changes in cohort size (number of students), employment and unemployment shares, and average earnings of the municipality. Average earnings, employment and unemployment shares are calculated using total population in the municipality with Finnish Longitudinal Employer-Employee Data (FLEED) between 1988 and 2004.

Bigger cities have more schools and these cities also grow during our observation period, whereas smaller municipalities experience a slight decrease in the number of students attending their schools (see Table A9). Municipalities may also have had differential unemployment and employment level trends during the observation period. This could potentially affect funding of the schools, as an example, and hence outcomes of the students.

Results are summarized in Table A9 Panel A. These results show that our main effects are not driven by changes in cohort size or other municipal level measures.

A.7.2 Year-specific County Level Dummies

We also check if our results are driven by differential county level trends, for example, because of differential exposure to Soviet Union trade and its collapse in the early 90s, or if some of the municipalities are co-operating in the supply of comprehensive school education. Our second robustness check shows that this is not the case.

We study this by including cohort-specific dummies for the county of the municipality in the regressions that also include municipal-level controls of Section A.7.1. Results are summarized in Table A9 Panel B. These results show that our main short-term effects are not driven by changes in county-level trends, but the results on the probability of having a higher education are no longer significant.

A.7.3 Results without Helsinki

In our third robustness check, we study if the results are driven by Helsinki. Helsinki is the biggest city in Finland, and has on average more than 50 schools that provide grades 7-9 during the observation period. The next biggest cities have less than half the number of schools of Helsinki.

Results are summarized in Table A9 Panel C. They show that the effects on short-term education outcomes are not driven by Helsinki, but the effects on the probability of having a higher education are no longer significant.

TABLE A9: Reduced form robustness checks results

Panel A: Municipal level controls

	Non-neighborhood school	Other than neighborhood school	GPA	High school graduate	Higher education
Average	0.192***	0.270***	0.00138***	0.000308**	0.000191
	(0.0227)	(0.0205)	(0.000407)	(0.000113)	(0.000100)
Low Income	0.177***	0.260***	-0.000170	-0.0000881	-0.000237
	(0.0336)	(0.0287)	(0.000283)	(0.000133)	(0.000182)
Middle Income	0.206***	0.297***	0.00166**	0.000339*	0.000170
	(0.0240)	(0.0220)	(0.000515)	(0.000151)	(0.000105)
High Income	0.181***	0.247***	0.00170***	0.000338***	0.000294*
	(0.0183)	(0.0170)	(0.000348)	(0.0000899)	(0.000148)

Panel B: Municipal and county level controls

	Non-neighborhood school	Other than neighborhood school	GPA	High school graduate	Higher education
Average	0.185***	0.260***	0.00104*	0.000307*	-0.0000293
	(0.0227)	(0.0195)	(0.000410)	(0.000121)	(0.0000901)
Low Income	0.175***	0.264***	-0.000542	-0.0000391	-0.000345
	(0.0324)	(0.0273)	(0.000446)	(0.000174)	(0.000189)
Middle Income	0.194***	0.285***	0.00148**	0.000423*	0.00000110
	(0.0229)	(0.0179)	(0.000514)	(0.000181)	(0.000105)
High Income	0.175***	0.228***	0.000957**	0.000120	-0.0000346
	(0.0188)	(0.0190)	(0.000367)	(0.000111)	(0.000158)

Panel C: Results without Helsinki

	Non-neighborhood school	Other than neighborhood school	GPA	High school graduate	Higher education
Average	0.320***	0.387***	0.00357***	0.000562	0.0000464
	(0.0428)	(0.0362)	(0.00105)	(0.000303)	(0.000463)
Low Income	0.330***	0.381***	-0.000535	-0.00112**	-0.000916
	(0.0469)	(0.0498)	(0.00108)	(0.000419)	(0.000522)
Middle Income	0.304***	0.377***	0.00411***	0.000758*	0.000240
	(0.0564)	(0.0419)	(0.00116)	(0.000294)	(0.000444)
High Income	0.330***	0.399***	0.00503***	0.00114*	0.000116
	(0.0392)	(0.0383)	(0.00111)	(0.000474)	(0.000627)

Notes: *Non-neighborhood school* and *other than neighborhood school* refer to approximation of the neighborhood school. All models include dummies for cohort, municipality, and cohort-specific individual level and family background controls. In Panel A, the model also includes additional municipal level controls for cohort size, average income, unemployment, and employment levels. In Panel B, the model includes the controls of Panel A and also year-specific dummy controls for the county. In Panel C, the original model is run without the students who reside in Helsinki. Standard errors in parentheses.
* p<0.05, ** p<0.01, *** p<0.001

A.7.4 Year-specific Controls for the Rural/Urban -status of the municipality

In this robustness check, we address the issue that the decentralization process that school choice reform was part of may affect our results, i.e. the decentralization process might have a direct impact on our outcomes and not only via choice. This is a potential threat to our identification only if municipalities of different sizes/composition are differentially affected by the decentralization process.

We confirm that our results of cities and rural areas having differential trends across time are unrelated to our reform. We add a categorical control variable to our specifications that tells whether a municipality is a city, city-like or completely rural. We also allow this to to have cohort-specific trends in a flexible manner: we add interaction between a cohort dummy for each year and the categorical variable.

The results are summarized in Table A10 and they show that our main results are not driven by differential trends between bigger cities and rural areas.

TABLE A10: Reduced form robustness checks result: controlling for the rural/urban -status of the municipality

	Non-neighborhood school	Other than neighborhood school	GPA	High school graduate	Higher education
Average	0.170***	0.252***	0.0017***	0.0004***	0.0002**
	(0.0119)	(0.0127)	(0.00038)	(0.00007)	(0.00007)
Low Income	0.131***	0.217***	0.0008**	0.0003*	-0.00003
	(0.0157)	(0.0157)	(0.0003)	(0.00012)	(0.00013)
Middle Income	0.186***	0.286***	0.0018***	0.0004***	0.0002*
	(0.0110)	(0.0110)	(0.00046)	(0.00010)	(0.00009)
High Income	0.173***	0.236***	0.0020***	0.0006***	0.0004***
	(0.0147)	(0.0151)	(0.00038)	(0.00010)	(0.00040)

Notes: *Non-neighborhood school* and *other than neighborhood school* refer to approximation of the neighborhood school. All models include dummies for cohort, municipality, and cohort-specific individual level and parental background controls. Standard errors in parentheses.

* p<0.05, ** p<0.01, *** p<0.001

A.7.5 DIFFERENCES-IN-DIFFERENCES WITH 0/1 TREATMENT DUMMY

In this robustness check, we study if the results change when we move from differences-in-differences with continuous treatment intensity to traditional differences-in-differences setup. A municipality with more than 1 school before the reform, will have a treatment status 1 while municipalities with 1 or less schools on average before the reform, are untreated.

This robustness check is meant to address the concerns raised by Fricke (2017) about the treatment effect homogeneity in the use of differences-in-differences with different or multiple levels of treatment intensity, i.e. comparing units that received more intensive treatment to units that received less intensive treatment. We thus check if our results, and conclusions we draw from them, still carry on when we compare treated municipalities (regardless of the intensity of the treatment) to untreated municipalities (where choice is not possible). Results in Table A11 confirm that using this specification does not change the conclusions we draw from the results.

TABLE A11: Reduced form robustness checks results: standard differences-in-differences specification

	Non-neighborhood school	Other than neighborhood school	GPA	High school graduate	Higher education
Average	3.783***	4.087***	0.0197	0.00513	0.000720
	(0.728)	(0.993)	(0.0104)	(0.00322)	(0.00331)
Low Income	3.393***	3.759***	-0.0153	-0.00923*	-0.0113*
	(0.646)	(0.836)	(0.0111)	(0.00465)	(0.00453)
Middle Income	3.690***	3.918***	0.0223	0.00688	0.00172
	(0.721)	(0.963)	(0.0114)	(0.00373)	(0.00380)
High Income	4.433***	4.918***	0.0493**	0.0120	0.00724
	(1.082)	(1.395)	(0.0171)	(0.00627)	(0.00675)

Notes: *Non-neighborhood school* and *other than neighborhood school* refer to approximation of the neighborhood school. All results include dummies for cohort, municipality, and cohort-specific individual level and family background controls. Standard errors in parentheses.

* p<0.05, ** p<0.01, *** p<0.001

A.7.6 HETEROGENEITY WITH RESPECT TO OTHER FAMILY BACKGROUND CHARACTERISTICS

In our last robustness check, we show the heterogeneity of our results using predicted GPA as an indicator for family background. We predict GPA using family income, parental education and employment status, and municipality. We then divide the sample into quartiles each year using the predicted GPA. The upper and bottom quartiles are the high and the low SES students, respectively, whereas the two middle quartiles are combined to represent the middle SES students. The results are in Table A12 and they are in line with the results where we use family income to study heterogeneity of our results.

The reason why we do not use parental education in heterogeneity estimation stems from the long observation period (17 years). As shown by Table A12, the probability of the parent to be highly educated increases significantly after the reform. Similarly, the probability of the parent to not have a formal education after comprehensive schooling decreases after the reform. Another reason is that we believe the "status" of having a higher education in year 1988 is likely to be different from the "status" it has in 2004. This would make comparison over the years, that our identification strategy utilizes, difficult.

TABLE A12: Reduced form results for heterogeneity with respect to predicted GPA

	Non-neighborhood school	Other than neighborhood school	GPA	High school graduate	Higher education
Average	0.197***	0.270***	0.00166***	0.000426***	0.000186**
	(0.0189)	(0.0171)	(0.000348)	(0.0000568)	(0.0000709)
Low SES	0.176***	0.250***	0.000433	0.000135	-0.000268**
	(0.0222)	(0.0199)	(0.000263)	(0.000100)	(0.0000954)
Middle SES	0.214***	0.294***	0.00175***	0.000347***	0.000143
	(0.0229)	(0.0179)	(0.000353)	(0.0000668)	(0.000104)
High SES	0.187***	0.254***	0.00192***	0.000543***	0.000311***
	(0.0143)	(0.0154)	(0.000386)	(0.0000810)	(0.0000933)

Notes: *Non-neighborhood school* and *other than neighborhood school* refer to approximation of the neighborhood school. All models include dummies for cohort, municipality, and cohort-specific individual level and family background controls. Socio-economic status (SES) is predicted via regressing GPA with parental and residential level controls. Predicted GPAs from this regression are used to categorize students into High, Middle and Low SES groups in a similar manner as with family income. Standard errors are in parentheses.

* p<0.05, ** p<0.01, *** p<0.001

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