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Essays on Economics of Immigration and Education

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Abstract

This dissertation is a collection of three empirical essays on the economics of immigration and education. In the first chapter of this dissertation, I will introduce the topics and methods, cover key literature, and summarize the main findings from all three essays.

In Chapter 2, we study the labor market consequences of opening borders by using the eastern enlargement of the European Union as a natural experiment. In our identification strategy, we use the fact that the eastern enlargement of the EU exposed construction workers in some occupations and regions differentially to the influx of foreign labor.

We find that opening borders to workers from the new EU countries decreased annual earnings of workers in vulnerable occupations relative to less vulnerable workers. This drop in earnings is economically meaningful and these workers never seem to catch up with the less vulnerable individuals after opening of the borders. Although we do find that vulnerable workers were slightly more likely to be unemployed, this does not fully explain the drop in earnings. We additionally investigate heterogeneity by age as well as adjustment mechanisms. The negative effect on earnings is driven by younger workers, who became more likely to switch to other sectors of employment and establishments of work, and older workers, who became more likely to retire.

In Chapter 3, we study the overall effects of a reform that introduced choice between public schools to the comprehensive education system in Finland. Our identification strategy exploits variation in school choice opportunities across municipalities before and after the school choice reform. The idea is that the reform was more intense in municipalities with multiple schools, as there were more opportunities to exercise choice.

We find that students from all household income groups made choices after the reform. The introduction of school choice had on average a positive effect on students' education and labor market outcomes. However, we find that the benefits of school choice were unequally distributed.

Students from higher income households benefited from school choice, as they experienced improvements in their GPA and were more likely to get a high school education. These short-term gains also translated in to improvements in long-term education outcomes. Despite that students from lower income households were as likely to exercise school choice, they did not experience improvements in short-term education outcomes and were less likely to get a higher education later in life. These results are potentially explained by students from higher income households attending schools and classes with higher average attainment after the reform. We also document heterogeneity in selection into education and occupation later in life.

In Chapter 4, I study the effects of public school choice on segregation of schools, residential segregation, and classroom-level segregation. I use the same reform and identification as Chapter 3.

I find that school choice increased segregation of schools both by ability and household characteristics. On the contrary, I find no robust evidence that choice would have had an impact on residential segregation. Lastly, my results show that students from different ability and household characteristics were less likely to meat in a classroom after the introduction of school choice. I additionally document that this results is not only driven by increased sorting to schools, but that student sorting to classrooms within the schools also increased.

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Contents

1. Introduction

This dissertation is a collection of three essays on economics of immigration and education. Essay 1 studies the eastern enlargement of the European Union by assessing its labor market effects on vulnerable native workers and their adjustment to the influx of cheap foreign labor in Finland. Essays 2 and 3 focus on a school choice reform that was introduced to the Finnish comprehensive education system in the early-1990s. Essay 2 investigates the market-level and distributional effects of choice between public schools on student education and labor market outcomes. Essay 3 explores the effects of school choice on segregation.

All three essays are empirical and contribute to the applied microeconomic literature. I use quasi-experimental methods and individual-level longitudinal administrative data from Finland for the causal interpretation of the results. Essays 1 and 2 are co-authored and Essay 3 is solo-authored work.

1.1 Motivation, Research Questions, and Related Literature

Essay 1 focuses on the enlargement of the European Union. Open borders and immigration have shaped Europe in many ways. The accession of the eastern European countries to European Union in 2004 lead to unexpectedly high immigration flows to the old member countries: for example, Dustmann et al. (2003) predicted that 5,000-13,000 immigrants would come from the Eastern European countries to Britain but according to Salt (2015) over 50,000 came already in 2004. The eastern enlargement of the EU has been speculated to be one of the key reasons behind the political turmoil in Britain, Brexit (see, for example Becker et al. 2017; Nikolka and Poutvaara 2016).

Essay 1 is joint work with Jaakko Meriläinen and titled *Labor market Effects of Open Borders: Lessons from EU Enlargement*. We study how opening borders to new, predominately low wage, EU countries and their citizens, affected the labor market outcomes of vulnerable natives in one old member country, Finland. Finland enacted a two-year transition period during which only goods and services were free to move across the borders between the new and old EU member countries in order to limit the influx of "cheap labor". Thus, this essay will also shed some light on the effectiveness of this transition period.

Essay 1 contributes to the literature on the effects of EU enlargement. Despite that the eastern enlargement of the EU boosted the EU's population almost by a 100 million, empirical evidence on its labor market effects is limited. Whereas Lemos and Jonathan (2013) find no effects on wages or employment in Britain, Blanchflower and Shadforth (2009) associates the inflow of new EU country workers to Britain with an increased fear of job loss. Åslund and Engdahl (2019) find a small negative impact on the earnings of workers near pre-existing ferry lines but no robust evidence for employment in Sweden. Evidence from Norway, a non-member country, also point to the direction that immigration from the new EU countries lead to reductions in wages (Bratsberg and Raaum 2012). However, Britain, Sweden, and a non-member country, Norway, along with Ireland, were the only countries in Europe that did not limit the movement of new EU country workers across the borders.

Beyond EU enlargement, our results contribute to the literature that studies the effects of cross-border workers and temporary migration. It has been estimated that cross-border or temporary workers potentially constitute a large portion of the increased immigration from the new EU countries to Finland after 2004.[1] The temporary nature of migration, especially in the case of cross-border workers, can lead to more adverse labor market effects in the host country than suggested by standard immigration models. For example, Dustmann and Görlach (2016) show that a temporary migrant may accept jobs and wages that an equivalent permanent immigrant would not.

The two empirical studies on the effects of cross-border workers closest to ours are Beerli et al. (2018) and Dustmann et al. (2017). Dustmann et al. study the opening of Germany's border to Czech cross-border workers in 1991, which led to a large inflow of foreign workers to German municipalities close to the border. Their results are in line with the results of Essay 1 but a key difference is that the policy experiment in Germany lasted only two years, whereas as Beerli et al., as well as Essay 1, study a more long-lasting change in open borders. Beerli et al. find that granting European cross-border workers free access to the Swiss labor market boosted high-skilled natives' wages and employment in the border regions.

Essays 2 and 3 of my dissertation contribute to the economics of education. Several countries invest sizable resources in public schooling and public spending on education has risen in most of the OECD countries in recent years (OECD 2019). School choice has been proposed as a way to improve the resource allocation and efficiency of the public education system via increased competition of schools for students (Friedman 1955; Hoxby 2006). This should ultimately lead to better student outcomes.

[1] The official estimates for the number of these workers vary between 10,000 to 30,000.

Essays 2 and 3 of my dissertation focus on studying the effects of a nationwide education reform that introduced choice between public schools in Finnish comprehensive education system. Prior to the reform, students were assigned to schools based on their residence and proximity to schools. After the reform, students could choose a school other than the assigned school, but priority was still given to students residing near the school.

The school choice reform in Finland aimed to improve student outcomes by increasing student motivation via allowing students to choose their school within the public education system (Seppänen 2003). This aim is closely related to the classical argument by Hoxby (2006) that choice between schools can improve student-school match quality, and thus improve student outcomes. However, findings of recent empirical studies from many countries paint a more nuanced picture of the effects of school choice. Empirical evidence on the effects of school choice has been mixed, ranging from small positive effects on various student outcomes (see Böhlmark and Lindahl 2015; Cullen et al. 2006; Deming et al. 2014; Hsieh and Urquiola 2006; Muralidharan and Sundararaman 2015; Sandström and Bergström 2005; Wondratschek et al. 2013, for example) to even sizeable negative impacts on test scores (Abdulkadiroglu et al. 2018).

In an international context, school choice has commonly been implemented via private or charter school vouchers (Chile, India, and the U.S.) or a combination of voucher programs and choice between public schools (Sweden). In Finland, education is entirely publicly funded and schools are not allowed to collect tuition fees. What specifically separates Finnish public education system from those in the U.S or Sweden, for example, is that the supply of schools is more restricted. School entry is always based on a need, and hence, it is fairly uncommon.

Furthermore, the reform in Finland was implemented nationwide. Large-scale school choice reforms are likely to have spillover effects on those who decide to stay in their assigned school (changing peer group), in addition to other school- and market-level changes, such as teacher resorting across schools. This also applies more generally to large-scale or national education reforms that are likely to have general equilibrium responses that are not present in small-scale programs (see Gilraine et al. 2018).

Essay 2 of my dissertation is a joint work with Liisa T. Laine and titled *Market-Level and Distributional Effects of Public School Choice*. We study the overall effects of the school choice reform in Finland on students' education and labor market outcomes. This is important, since even if students who exercise choice would benefit from making that choice, the effects can be negligible for some students (as suggested by recent theoretical studies Avery and Pathak 2015; Barseghyan et al. 2019; Epple and Romano 1998; MacLeod and Urquiola 2013, 2015, 2019; Rothstein 2006).

For the overall effects, we study if choice between public schools improved student attainment on a school market level. In our case, a school-market is a municipality. As changes in short-term student attainment can also have long-term consequences, we study whether school choice affected students' later life labor market outcomes.

We also study if the school choice reform in Finland had distributional consequences. This analysis is inspired by a controversy between aims of the school choice policies and the empirical evidence. A more recent motivation for school choice policies, especially in the U.S., has been to provide all students, regardless of their socioeconomic background, a chance to attend good quality schools (The U.S. Department of Education 2019). However, previous empirical evidence has shown socioeconomically disadvantaged students to be less likely to exercise school choice than students from higher socioeconomic status households: Not only have socioeconomically disadvantaged students found to be less likely to choose a school other than the assigned school (Walters 2018), there seems to be considerable heterogeneity in the type of choices made by the socioeconomic status of the students (Hastings and Weinstein 2008; Lucas and Mbiti 2012b).

Essay 3 of my dissertation is solo-authored and titled *The Unintended Consequences of a Public School Choice Reform on Segregation.* A common finding in the literature has been that large-scale school choice reforms increase segregation of schools (for a survey, see Epple et al. 2017). Thus, I study if the school choice reform in Finland had an impact on segregation of schools by ability and household characteristics.

In addition to segregation of schools, I study whether choice between public schools reduced residential segregation. Recent theoretical studies have shown that the ability to choose a school more freely should reduce residential segregation, as households no longer have to move in order to get their children into desired schools (Brunner et al. 2012; Epple and Romano 2003; Ferreyra 2007; Nechyba 2000, 2003a,b). This argument has also been used to motivate the implementation of school choice policies, for example, in Sweden in the case of a high school choice reform (Söderström and Uusitalo 2010).

Lastly, Essay 3 contributes to the school choice literature by studying the impacts of choice between public schools on student composition of classrooms. My aim is to understand if school choice changed the extent to which students from different ability and socioeconomic backgrounds interact with each other (actual peer group). This is important, since changes in opportunities for social interactions across different ability and socioeconomic groups can have long lasting consequences, for example on labor market outcomes, via peer or network effects (see, for example Hoxby and Avery 2013; Zimmerman 2019) To do this, I study the effects of the reform on classroom-level segregation within a municipality. As this can merely reflect school-level differences (i.e. segregation of schools), this essay additionally investigates whether the reform affected sorting of students to classrooms within schools (in addition to segregation of schools).

Essays 2 and 3 of my dissertation contribute to the literature that studies the effects of large-scale school choice policies. The conclusions of these studies are in line with the findings of my dissertation essays 2 and 3: modest to substantial increases in segregation of schools, accompanied by modest to non-existent overall student attainment gains. Hsieh and Urquiola (2006) find that the large-scale private school voucher reform in Chile increased sorting of students across schools but did not lead to overall improvements in student outcomes. Wondratschek et al. (2013); Böhlmark and Lindahl (2015) find that the comprehensive school choice reform in Sweden, that also introduced publicly funded private schools, had a small positive effect on average student attainment but according to Böhlmark et al. (2016) it increased segregation of schools by ability and household characteristics. Essays 2 and 3 contribute to the literature by studying these effects in an entirely public education system.

1.2 Research Methods

The identification strategy we use in Essay 1 of my dissertation uses three sources of variation, together with detailed administrative data, to study the labor market effects of the enlargement of the EU. Our identification strategy follows closely Bratsberg and Raaum (2012) who study the impacts of immigration on natives using license requirements in the Norwegian construction sector. We use similar variation between construction sector occupations in Finland. Specifically, the identification strategy will compare painters, plumbers, builders, and carpenters, who were more vulnerable to foreign workers, to individuals in a less vulnerable occupation, electricians.[2] In addition to the occupational variation, and departing from the identification used by Bratsberg and Raaum, we exploit the exogenous shock of opening the borders and regional variation in the use of foreign workers within the construction sector. One of the advantages of this identification strategy is that it overcomes the problems related to endogenous self-selection of migrants into a booming sector and region.

We follow vulnerable native workers in exposed regions over the years, instead of studying exposed occupations and regions. This allows us to study adjustment mechanism of the natives in detail. We also avoid problems related to selective attrition and entry of natives that could mask the true impacts of immigration, as shown by Bratsberg and Raaum in the Norwegian construction sector. However, this choice also means that we only identify the effects for a small part of the population, and not the overall (wage and other) effects of the enlargement of the EU. It is nevertheless important to identify those groups that might be vulnerable to increased immigration.

[2]Electricians were not as vulnerable to workers from the eastern European countries, for example, because of the lack of formal training of foreign workers and stricter regulations of the industry.

The identification strategy in Essay 2 exploits municipal-level variation in the intensity of the reform and over time created by the reform. Since the data available to us does not have information on the true intensity of the reform, such as application rates to schools other than the assigned school, we use pre-reform variation in school choice possibilities across municipalities. We use the average number of schools in a municipality prior to the reform. The idea is that the more schools there are in a given municipality, the more choice opportunities there exist.

Our identification strategy is called difference-in-differences with continuous treatment intensity. Similar identification strategies have been used, for example by Acemoglu et al. (2004), Card (1992), Cooper et al. (2011), Duflo (2001), Foged and Peri (2016), Gaynor et al. (2013), and Lucas and Mbiti (2012a). This empirical strategy thus compares changes in outcomes before and after a treatment between units that received different levels of treatment.

In Essay 3, I use the same identification strategy as in Essay 2. I measure school, residential, and classroom-level segregation at municipal-level. I use 3 different indices that take a different approach in measuring segregation. This will give me a broader understanding of segregation, as these indices do not individually fulfill all properties a good segregation index should have (for these properties and discussion, see Allen and Vignoles 2007).

My first measure is the coefficient of determination, R^2. This has previously been used by Söderström and Uusitalo (2010). It measures how much of the variation in student-level ability and household characteristics do the schools, residential locations, or classrooms explain within a municipality. In addition, I use own-group overexposure index, originally introduced by Åslund and Nordström Skans (2009). This index measures whether students are exposed in excess to their peers from the same ability or household characteristics in schools, residential locations, or classrooms, in comparison to what the distribution at municipal-level implies. Lastly, I use the most commonly used segregation index, the dissimilarity index, that was first introduced by Duncan and Duncan (1955). This index can be interpreted as the percentage of the students that should be reshuffled across the schools, residential locations, or classrooms in order to achieve an equal distribution.

1.3 Summary of the Results

1.3.1 Essay 1: Labor Market Effects of Open Borders: Lessons from EU Enlargement

We find that opening the borders to new, and predominately low wage, EU country citizens had a negative impact on the annual earnings of workers in vulnerable occupations in heavily exposed regions in the construction sector in Finland relative to the electricians and control regions. This is in line with canonical models of immigration and labor market (see Altonji and Card 1991). This effect was mainly driven by younger workers, who became more likely to switch to other sectors of employment or establishments of work, and older workers, who became more likely to retire. Furthermore, we discover that the EU enlargement had distributional consequences: part of these effects can potentially be explained by complementarity of electricians to foreigners in exposed occupations (for a discussion on complementarities, see Borjas 1995).[3]

We additionally find that earnings were negatively affected even during the transition period. This suggests that the restrictions on labor market movement did not work. Most plausible explanation for this is that companies in Finland circumvented labor movement restrictions by hiring foreign workers via rental work companies or by using subcontractors from one of the new EU member countries to perform work in Finland. This was considered a service, and thus legal during the transition period. The use of posted workers and subcontractors remained common even after the labor movement restrictions were removed, and industries that use posted workers have continuously been linked to, for example, tax evasion and the black market (Hirvonen et al. 2010).

1.3.2 Essay 2: Market-Level and Distributional Effects of Public School Choice

We find that students from all socioeconomic groups made choices after the reform and that the average school market-level effects of school choice were positive both on the short and long term. On average, municipalities with more school choice opportunities experienced an increase in student attainment and high school graduation rate after the reform. The probability to attain higher education later in life also increased. These findings suggest that the school choice reform enhanced student performance, which is in line with Friedman's original motivation for school choice.

In addition to the positive average school market-level effects, this essay finds that the school choice reform had distributional consequences. We find that the positive average effects are driven by students from high-income households. Students from low-income households were unaffected by the reform on the short-term but their later life labor market outcomes deteriorated, as they were less likely to attain higher education. This is despite that we find that students from all socioeconomic backgrounds made choices after the reform.

[3] If electricians are complements to the workers in vulnerable construction occupations, then an influx of cheap labor into these occupations in exposed regions should increase the demand for electricians in these regions. This, consequently, can result in an upward pressure in the wages of electricians in Helsinki.

We devote the last part of Essay 2 to investigating mechanisms that could potentially explain the treatment effect heterogeneity. Our findings suggest that there might be heterogeneity in the type of choices made by students from different socioeconomic backgrounds. Students from high-income households attended schools and classrooms with higher average GPA after the reform. Later in life, student from high-income households were not only more likely to attain higher education but they ended up with an education with higher earning potential. On the contrary, low-income students ended up in an occupation with lower earning potential after the reform.

1.3.3 Essay 3: The Unintended Consequences of a Public School Choice Reform on Segregation

I find that segregation of schools increased significantly by ability and household characteristics in municipalities with more choice opportunities after the reform. The effects in segregation of schools are consistently measured both in sign and in magnitude with all three segregation indices. Increasing segregation of schools is line with previous empirical findings from several countries that have implemented large-scale school choice policies (Böhlmark et al. 2016; Hsieh and Urquiola 2006; Ladd 2002; Söderström and Uusitalo 2010).

Contrary to the theoretical predictions and previous empirical findings (Brunner et al. 2012), I do not find robust evidence that choice between public schools would have decreased residential segregation in Finland. This is perhaps not that surprising, as the school choice reform in Finland did not abolish all ties to residence-based student selection system. However, other potential changes to neighborhood composition and data limitation, as I am only able to measure residential segregation using the location decisions of the students' households, may play a role. Thus, further research is needed to better understand how choice between public schools impacted residential segregation in Finland.

I additionally find that students from different ability and socioeconomic groups were less likely to meet in a classroom after the reform, as classroom-level segregation increased in municipalities with more choice opportunities. My findings suggest that this increase in classroom-level segregation does not simply reflect increasing school-level differences, but that the reform also increased sorting of students to classrooms within schools by ability and household characteristics.

1.4 Conclusions

The results of Essay 1 suggest that the EU enlargement created both winners and losers. Although it is likely that the overall effects of the EU enlargement were positive, this essay highlights that not everyone benefited from increased immigration. In fact, we show that individuals in vulnerable positions never catch up after the opening of the borders with the individuals that were better shielded to begin with. This could have repercussion, for example, for political outcomes (as suggested by the findings of Viskanic 2017, in the case of the Brexit vote). In order to better understand the overall effects of the enlargement of the European Union, further research on the impacts of opening borders on firms and other sectors are needed. Also, evidence from the new EU countries should be collected to fully understand the impacts of integration.

The findings of Essay 2 highlight that school choice reforms can enhance student attainment on average, but that these reforms can also have unintended distributional consequences. The effects found are economically significant and surprisingly large in the Finnish context. Since Finnish schools have relatively small quality differences in international terms (OECD 2013), students should expect to gain only little from making a choice. Furthermore, the reform in Finland did not abolish all ties to residence-based student selection system. Thus, the effects of choice between schools in a public education system can potentially be larger in countries with greater quality differences between schools or more drastic school choice policies.

The results of Essay 3 show that the school choice reform significantly changed students' learning environment. Students from different backgrounds were less likely to meet in a classroom after the reform. This suggests that school choice can decrease the number of opportunities for social interactions between students from different socioeconomic and ability groups. However, it is important to note that in international context these segregation levels are still modest (OECD 2017).

Due to data limitations, Essays 2 and 3 are unable to investigate whether our distributional results are explained by heterogeneous preferences for schools or other reasons, such as asymmetric information or other type of constraints faced by socioeconomically disadvantaged students. This is left for future research.

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2. Labor Market Effects of Open Borders: Lessons from EU Enlargement[1]

2.1 Introduction

In recent years, few things have shaped Western countries economically, politically, and socially the way that open borders and immigration have. A prominent example is the European Union, the expansion of which has been speculated to be one of the key reasons for phenomena such as Brexit.[2] With merely less than ten years after the union's latest expansion to Croatia, and with Serbia and Montenegro anticipated to join by 2025, it is even more important to understand the societal impacts of the enlargement process. At the heart of this—and international integration in general—lies the following problem: who are hurt by it and how?

This paper revisits the eastern enlargement of the European Union by assessing its labor market consequences on vulnerable native workers and their adjustment to the influx of cheap foreign labor. In May 2004, eight eastern European (hereafter A8) and two Mediterranean countries joined the union, boosting its population by about a hundred million people. The newly joined eastern European countries had predominantly low wage levels. What happened in the labor markets of the old member countries after opening their borders to the new EU citizens? Despite considerable debate, empirical evidence

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[2]Becker et al. (2017) provide a descriptive assessment of the determinants of Brexit. They show that exposure to the EU enlargement predicts support for Brexit to at least some extent. Similarly, Nikolka and Poutvaara (2016) link the presence of immigrants from Eastern European EU countries with an increased propensity to vote for "leave" in the Brexit referendum.

has been limited. To begin filling this gap, we study Finland which experienced a large inflow of East European workers especially from Estonia. Our focus is on the reduced-form effects of the EU enlargement. This is because Finland enacted a two-year transition period during which only goods and services were free to move across its borders. Many workers came to Finland as so-called posted workers who were employed by firms in their home countries and whose services were rented to companies abroad.[3] As posted workers were initially not required to register in Finland and the new rules were easy to circumvent, we do not know exactly how many posted workers came and where they worked.[4]

However, we do know that the use of foreign workers became common, particularly in the construction sector. What is more, individuals in different construction-sector occupations were differentially exposed to the foreign workforce due to skill requirements and regulatory differences. The majority of those who came to Finland worked as painters, plumbers, carpenters, and builders. On the contrary, electricians were not as vulnerable to workers from the A8, for example, because of the foreign workers' lack of formal training. This is the first source of variation that we use in this paper. Our empirical approach thus owes a debt to Bratsberg and Raaum (2012), who study the impacts of immigration on natives in Norway, hinging on licence requirements in the Norwegian construction sector. We additionally exploit the exogenous shock of opening borders, and the fact that most of the foreign workforce stayed in the capital city region. This happened at least partially because ferries from Estonia only run to the ports of the Finnish capital city, Helsinki. As we have both occupational and regional variation—besides the apparent time variation—our case offers natural groundings for a triple-differences approach.

While we cannot study the overall (wage and other) effects of open borders, our setting allows us to show that natives who worked in vulnerable occupations in the exposed region prior to the EU enlargement experienced a decrease in their annual earnings relative to less vulnerable electricians and non-exposed control regions. On average, this decrease was about 1,700 euros annually. Earnings were negatively affected even during the transition period, for which we find a decrease of about 1,400 euros in annual earnings. The persistence of the negative effect is striking: even ten years after the border was opened, the exposed workers had not been able to close the wage gap.[5]

[3]The use of posted workers is in no way specific to Finland. For example, the European Parliament approximates that there were more than two million posted workers in the EU in the year 2016 (see a fact sheet available at `https://www.europarl.europa.eu/factsheets/en/sheet/37/posting-of-workers`; accessed November 13, 2019). Only the UK, Ireland, and Sweden allowed free movement of labor starting from May 1st 2004. The use of posted workers before the end of the transition period was commonplace in other EU countries.

[4]Similar difficulties apply to studies that try to assess the effects of illegal migration (see, e.g., Hanson 2006).

[5]Studies on temporary labor market shocks have made similar remarks. For instance, Autor et al. (2016) show that local labor market in the United States that experience a negative China trade shock have depressed wages and labor force participation rates for at least a decade after the shock took place.

We also find evidence that the EU enlargement increased the number of months in unemployment for those affected, but the effect is too small to fully explain the earnings losses. Nevertheless, our main results are in line with the canonical models of immigration and the labor market, which predict a reduction in wages and employment if immigrants are substitutes for native workers (see Altonji and Card 1991 for an early example).[6]

The impact on earnings is economically significant from the exposed workers' perspective, roughly equal to one month's salary. On the other hand, the effects that we detect are fairly small in contrast to the number of foreign workers that potentially came to Finland after the EU enlargement in 2004. Based on estimates from different sources, we approximate that there was an inflow of foreign workforce about the size of—or even larger than—the native population in the vulnerable construction sector occupations in the exposed region prior to the enlargement of the EU. Therefore, our results indicate a close-to-zero elasticity of annual earnings with respect to the increase in labor supply induced by cross-border workers. However, given the difficulty of approximating the change in the labor force, one should take the elasticities with a grain of salt.

Going further, we document that the negative effect on earnings is mainly driven by young (less than 30 years old) and old (over 50 years old) workers. A particular aspect of our extensive administrative data is that we can also study the adjustment mechanisms in detail. We provide suggestive evidence that the EU enlargement made young workers more likely to start working in other establishments or commuting zones. Moreover, we find that exposure to the EU enlargement influenced older workers' retirement choices. They became more likely to retire, and less likely to take up part-time retirement.

Finally, we illustrate that open borders created both winners and losers in the Finnish labor market which reflects the conventional wisdom on asymmetric effects of international integration. In our case, this could be due to the fact that native electricians can be complements to immigrant workers in other construction sector occupations (see Borjas 1995 for a theoretical treatise, and Kugler and Yuksel 2008 and Foged and Peri 2016 for some empirical evidence on complementaries). In theory, an influx of cheaper workforce could then increase the demand for electricians. In line with such an argument, we present indicative evidence that the EU enlargement affected electricians' labor market outcomes positively. To understand whether our main results are driven by this, we complement our findings with a difference-in-differences analysis that compares vulnerable construction sector workers in Helsinki with those in less

[6]The temporary nature of migration, especially in the case of cross-border workers, can mean more adverse labor market effects in the host country than suggested by standard immigration models. For example, Dustmann and Görlach (2016) show that a temporary migrant may accept jobs and wages that an equivalent permanent immigrant would not.

exposed regions. The findings from this analysis are in tally with the conclusions drawn from the triple-differences results.

The consequences of immigration have been the subject of a vast literature (see Okkerse 2008, Dustmann et al. 2008, Dustmann et al. 2016, and National Academies of Sciences, Engineering, and Medicine 2017 for reviews). Much of this research centers around permanent immigration and its impacts on labor markets.[7] Our study contributes to a specific strand of the migration literature studying the effects of temporary migration and cross-border workers. Dustmann and Görlach (2016) provide a review of temporary migration literature.

Only a few studies have exploited opening borders for cross-border workers as a quasi-experiment. The closest to our study is the work by Dustmann et al. (2017) and Beerli et al. (2018). Dustmann et al. (2017) show that opening Germany's border to Czech cross-border workers in 1991 led to a large inflow of foreign labor to municipalities close to the border. Their results are in line with ours. On average, the wages of the natives declined, and this was driven by younger workers. They also find a significant reduction in the employment prospects of older workers (reduced hiring), whereas we find that older workers respond by retiring. A key difference between their study and ours is that the policy experiment in Germany lasted only two years, whereas we look at a more permanent change.[8] Another study that considers a more long-lasting change in open border policies is that of Beerli et al. (2018). They find that granting European cross-border workers (who were mainly high-skilled) free access to the Swiss labor market boosted high-skilled natives' wages and employment in the border regions. According to their evidence, this is due to the impact of opening the border on firms: the size, production, and innovation performance of firms increased, and new firms were established.

We add to this strand of research by studying one of the most notable cases of economic integration—the enlargement of the European Union. Work on the impacts of EU enlargement on the member countries is scarce. In particular, we are short of causal evidence using quasi-experimental approaches and suitable data. Lemos and Jonathan (2013) study how a large inflow of A8 migrants to the British labor market shaped

[7]Older studies in particular often lack a credible identification strategy, which means that one should be careful with causal interpretations. More recent work has exploited different types policy changes or historical events as sources of exogenous variation. For example, Foged and Peri (2016) identify the effects of immigration on natives' outcomes using randomized refugee placement in Denmark. Glitz (2012) studies the effects of allowing ethnic Germans in eastern European countries to obtain German citizenship in the aftermath of the fall of the iron curtain. Another example from a historical setting is Edo (2019) who illustrates that an unexpected inflow of Algerian immigrants to France resulting from Algerian independence in 1962 led to a temporary wage decrease.

[8]Much of the immigration literature explores one-time shocks in the number of immigrants. A prominent example is the Mariel boatlift and its impact on the Miami labor market. Card (1990) does not find any impacts in his seminal study. Borjas (2017) revisits the Mariel boatlift and finds that the wages of high school dropouts in Miami dropped dramatically after a large number of Cubans came to Florida.

natives' wages. They find no evidence that this resulted in lower wages or higher unemployment. They speculate that rising minimum wages might hide the negative wage effects given that the A8 workers were competing primarily with low-skilled natives. On the contrary, Blanchflower and Shadforth (2009) associate the inflow of A8 workers to Britain with an increased fear of losing one's job. This might have contributed in turn to lower wage inflation through weakened bargaining power. Becker and Fetzer (2018) document that the labor market shock that the UK faced in the aftermath of the eastern enlargement was considerable: more than one million people migrated to the country from Eastern Europe after the year 2004. They find that places where the Eastern European immigrants settled had limited prior immigration. These areas subsequently saw smaller wage growth at the lower end of the wage distribution. Åslund and Engdahl (2019) exploit variation stemming from the eastern enlargement of the EU in combination with transport links to Sweden from new member states. They document a negative effect on earnings of workers in areas close to pre-existing ferry lines, but they do not find robust evidence that the eastern enlargement affected employment or wages. Whereas the UK and Sweden did not enact a transition period for labor movement across the border, Finland was among the majority of EU countries that first allowed free movement of goods and services, and opened their borders to free labor movement only later.[9] Our example highlights that opening borders can matter for the labor market outcomes of natives even when the cross-border movement of labor is regulated.

The remaining parts of the paper are organized as follows. Section 2.2 discusses the eastern enlargement of the EU and the use of foreign labor force in Finland. We introduce our data and identification strategy in Section 2.3, where we also conduct some preliminary analysis. We proceed to more detailed regression analysis in Section 2.4, and assess the sensitivity of these results in 2.5. Section 2.6 concludes the study.

2.2 Background

In May 2004, eight eastern European countries (Czechia, Estonia, Hungary, Latvia, Lithuania, Poland, Slovakia, and Slovenia), and two Mediterranean countries (Malta and Cyprus) joined the European Union. Over a hundred million people became new EU citizens. The substantial increase in the population of the union also meant changes

[9] The investigation of Bratsberg and Raaum (2012) also covers the first two years of the enlargement of the EU, although Norway is not a member of the union. Around time of the eastern enlargement of the EU, Norway adopted a liberal transition period and allowed workers from the A8 countries to seek employment and reside in the country for up to 6 months (Dølvik and Eldring 2008). Bratsberg and Raaum (2012) find that immigration had a negative impact on wages in the construction sector. This reduction in wages resulted in lower consumer prices.

in the labor markets. This section discusses the use of foreign labor in Finland before and after the eastern enlargement.[10]

2.2.1 Transition Period: Free Movement of Goods and Services

To avoid an uncontrollable influx of cheap labor from the A8, most of the EU15 countries implemented a transition period before opening the borders for free movement of workers.[11] Finland implemented a two-year transition period during which only goods and services were free to move between the countries but workers were not. Finnish companies were allowed to hire an employee from a A8 country only if no suitable worker could be found from Finland. This practice was also used prior to the EU enlargement, and even today in hiring workers from outside the EEA or Switzerland.

Nonetheless, firms could circumvent these restrictions by hiring a worker via a rental work company in an A8 country, such as Estonia, or by using a subcontractor from one of the A8 countries to perform work in Finland. This was considered a service, and thus doing so was legal during the transition period. Workers employed in one EU country and sent by their employer to work in another EU country are usually called posted workers. Hiring a posted worker was faster and more flexible than employing someone from the local labor market; a posted worker could arrive within days instead of weeks, and only work for a short period of time.[12] Thus, the transition period in Finland paved the way for increased use of foreign rental work and subcontractors, especially in the construction sector. For example, the Finnish Construction Trade Union randomly inspected constructions cites and found that 18 percent of the workers in Uusimaa (the capital city region) were posted workers in 2006 (the last year of the transition period) (Hirvonen et al. 2010). Two years later in 2008, the Occupational Safety and Health Division at the regional state administrative agency for Uusimaa found that the share of foreign workers had increased to 27 percent (Hirvonen et al. 2010). These foreigners were

[10] Romania and Bulgaria became EU countries in the year 2007, and Croatia joined in 2013. These expansions had very little if any impact on the Finnish labor market.

[11] The newly joined eastern European countries, the A8, had predominantly low wage levels. For example, OECD estimates suggest that the average annual wage in Estonia was only 6,201 euros (in 2018 euros) in 2004, whereas it was almost 30,000 euros in Finland. Evaluating how many foreign workers would come was *ex ante* an extremely difficult task. For example, Dustmann et al. (2003) estimated that 13,000 people would arrive in the UK from the A8 should Germany not implement a transition period for free labor movement. Such a policy was implemented in Germany, and over 50,000 people arrived to the UK already in the first year (Salt 2015).

[12] See Eurofond (2010) for an overview of the use of posted workers across the EU.

mainly rental workers or workers of subcontractors, and only a minority worked for the main contractor.[13]

Regulations regarding posted worker pay and taxation proved hard to enforce. In theory, posted workers should have received wages according to the collective bargaining agreements of the industry. In addition, they should have registered and paid taxes if they worked ten consecutive days in Finland (Hirvonen et al. 2010).[14] But in practice, many posted workers were paid less than the going wage.[15] Similarly, the registration requirements could easily be dodged by working only for short periods at a time. While companies were also obliged to report the use of posted workers, the low probability of getting caught combined with small fines resulted in low reporting rates (Hirvonen et al. 2010).[16] Hence, we do not know exactly how many posted workers came to Finland, what they were paid, and where they actually worked (region and establishment). However, we do have indirect evidence according to which the inflow of people from the A8 countries to Finland both as registered (living and working in Finland) and as posted workers increased significantly after the enlargement of the EU.

2.2.2 Foreign Labor in Finland before and after 2004

Most of the workers who arrived in Finland after the eastern enlargement of the EU were Estonians. According to administrative data from Statistics Finland, the number of registered Estonians who were directly employed and lived in Finland increased steeply after the transition period (Panel A of Figure 2.1) from around 10,000 in 2003 to over 40,000 in 2015.[17] The numbers also increased for other A8 countries, but the growth was more moderate. Most of the registered A8 workers stayed in the Helsinki commuting zone (Panel B of Figure 2.1), and they were employed in the construction, cleaning, and

[13]See CFCI surveys for statistics, available at `https://www.rakennusteollisuus.fi/Tietoa-alasta/Talous-tilastot-ja-suhdanteet/Tyovoimakyselyt/`; accessed November 13, 2019).

[14]Prior to 2007, posted workers were only obliged to register and pay taxes to Finland if they stayed longer than six months (Section 11 of L 1535/1992 and HE 158/2006). After June 2006, posted workers from A8 countries were obliged to register at a local employment office (L 418/2006), though legislation on this was valid only until April 2009.

[15]In an extreme case, Estonian workers were paid less than 200 euros per month (see an article in *Helsingin Sanomat* available at `https://www.hs.fi/kotimaa/art-2000004306132.html`; accessed May 6, 2019). Nevertheless, Estonian posted workers were on average paid more than what they would have earned working in the Estonian construction sector (see an article in *Helsingin Sanomat* available at `https://www.hs.fi/kotimaa/art-2000004271870.html`; accessed May 6, 2019).

[16]In the year 2007, it became the responsibility of the contractee to ensure that the posted workers are paid according to the collective bargaining agreements of the sector and have access to occupational health care (L 1233/2006). Failing to do so, in case of an inspection by the Regional State Administrative Agency, resulted in fines up to 16,000 euros in 2007-2012. The fine was later increased to 60,000 euros (HE 18/2006 and L 469/2012). Foreign companies that operate in Finland temporarily do not have to report the use of posted workers (HE 158/2006).

[17]Besides the geographical proximity of the countries, this could be explained at least partially by the similarity of the Estonian and Finnish languages.

health care sectors (Panel C of Figure 2.1). The numbers for registered workers may give us some hints as to where the posted workers were employed during the transition period.

There is also evidence that the number of posted workers coming from Estonia increased rapidly around the time of the eastern enlargement. As these workers did not live in Finland permanently, they had to travel between the countries. Figure 2.2 shows that the number of ferry passengers from Estonia roughly doubled after 2004, according to annual border interview surveys. Most of the passengers in the border interview surveys stated that their main destination was in the capital city area.[18] Passengers who reported that the purpose of their visit to Finland was work constitute a large fraction of this increase.

The Ministry of Economic Affairs and Employment in Finland has estimated that about 10,000 Estonians travel from Estonia to Finland for work annually, and this figure has been relatively stable since 2009. According to Estonian authorities and experts, the number is slightly higher: about 15,000-20,000 workers. This number is also merely an approximation, as the cross-border workers are not properly registered in Estonia either.[19]

2.2.3 Foreign Labor in the Construction Sector

The construction sector is of particular interest to us. It provides us with a natural setting for analyzing worker-level consequences of EU enlargement for three reasons. First, the use of A8 workers both as direct employees (registered labor) and as posted workers (unaccounted labor) in the construction sector increased considerably after the enlargement of the EU. Second, occupations *within the sector* differed in their exposure to the A8 workers. Third, most of the A8 workers stayed in Helsinki, meaning that workers in different local labor markets were differentially exposed to the foreign workforce.

This is perhaps not so surprising. *Ex ante*, the Helsinki commuting zone had more A8 workers than other commuting zones. Immigrants' past geographic distribution tends to heavily influence immigrants' current location choices (Altonji and Card 1991; Card 2001). Another reason is the geographical proximity of the Helsinki region to Estonia. Ferries run frequently between Tallinn and Helsinki, but there is no ferry connection from Estonia to any other city in Finland. Cross-border workers typically end up working in border regions (Dølvik and Eldring 2008; Dustmann et al. 2017; Beerli et al. 2018).

Unaccounted Foreign Workforce The estimated number of unaccounted foreign person-years in the construction sector almost tripled after the enlargement of the EU

[18] Most passengers from Estonia arrived in Helsinki by ferry from Tallinn. The ferry ride is frequent, takes at least 1.5 hours, and only runs from Tallinn to (various ports in) Helsinki. No other city in Finland has ports with ferries to Estonia. According to Dølvik and Eldring (2008), many Estonians traveled to Finland biweekly.

[19] See an article by *Yleisradio* at `https://yle.fi/uutiset/3-9758938` (accessed May 4, 2019).

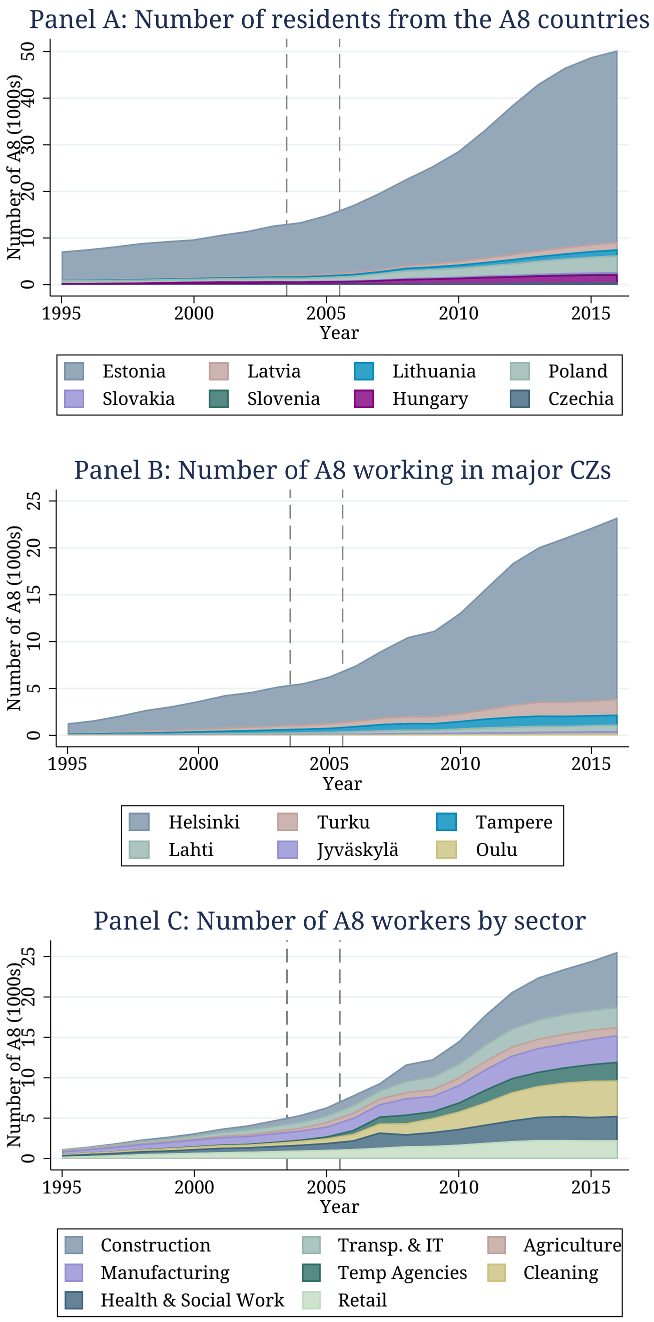

Figure 2.1. Number of residents from the A8 countries.

Notes: Vertical lines mark the transition period. Areas from top to bottom in the graphs correspond to labels in the legend from left to right. Panel C only includes sectors with at least 2,000 workers. *Source*: Authors' own calculations based on administrative data.

(see Figure 2.3). These estimates come from Hirvonen et al. (2010). They were calculated by comparing the estimated person-years needed for each year's building and other construction production to the actual person-years of Finnish construction sector workers obtained from a special labor force survey conducted by the Statistics Finland.[20] These estimates thus approximate the number of posted workers.

Further evidence is provided by the Confederation of Finnish Construction Industries (CFCI), an employer-side union that has conducted random inspections on construction sites of its member companies from 2007 onward. We visualize the development in the use of foreign workers in construction sites in 2007 and 2011 in Figure 2.4. In 2011, in the construction sites in Uusimaa province (which includes the Helsinki commuting zone) the share of foreign workers was above 30 percent, whereas the shares in other parts of the country were still quite modest.[21] These foreign workers were mostly rental workers or workers of subcontractors.[22]

Development of Registered Labor Figure 2.5 illustrates the development of the share of A8 in five important industries within the construction sector—building, carpentry, painting, HPAC (heating, piping, and air conditioning), and electricity—in Helsinki and five other major commuting zones (Turku, Tampere, Lahti, Jyväskylä, and Oulu).[23] We scale the absolute numbers by the size of the industry in 2000. Other commuting zones had smaller numbers of registered foreign workers both before and after the eastern enlargement of the EU.[24]

Two important notions arise from the figure. First, we see a clear increase in the share of A8 workers after the enlargement of the EU and especially after the transition period. Helsinki received many more A8 workers than the other major commuting zones. Second, the figure also shows that there is variation between the industries within the construction sector in terms of exposure to A8 workers. One reason for the differences between these industries within the construction sector is simply that different tasks require differentially demanding sets of skills or certifications.[25]

[20]These calculations assume that the latter data contain the hours worked by the natives and registered foreigners, whereas the former data also contain those of the posted workers.

[21]These shares are likely to be larger in reality, as CFCI only inspected its member companies. According to Hirvonen et al. (2010), non-member companies use even more posted workers.

[22]See annual CFCI surveys available at `https://www.rakennusteollisuus.fi/Tietoa-alasta/Talous-tilastot-ja-suhdanteet/Tyovoimakyselyt/`; accessed November 13, 2019.

[23]The administrative data that we use in this paper do not contain detailed information on occupations on a yearly basis. Thus, we resort to illustrating the development of the industries (NACE Rev. 1 classification) here. According to our administrative data, the most common occupations (ISCO-88 classification) in these industries in the year 2000 were builders and carpenters in building and carpenting, painters in painting, and plumbers in HPAC, and electricians in the electricity industry (see Appendix Figure A3). The administrative data is described in more detail in Appendix 2.B.1.

[24]Figure A5 in Appendix 2.A plots the share of A8 for all industries with at least 2,000 workers in the year 2000.

[25]This is reflected in expert opinions in an article in *Rakentaja-lehti* (June 12, 2015), a construction sector magazine:

In Helsinki, the building industry had more than four times more A8 workers than the electricity industry. Even though foreigners are not prohibited from working as electricians, there are a few obstacles. For one, workers are required to have a formal education or sufficient experience and be aware of all the current safety requirements in order to perform electrical work in Finland (Section 8 of L 410/1996). Second, each construction site requires a licensed supervisor who is responsible for all electrical work, and all electrical work needs to be reported to the authorities (Section 8 of L 410/1996; Finnish Safety and Chemicals Agency 2019). Lastly, insurance does not cover potential electrical work damages if the work is performed by unskilled workers.[26] Therefore, most electricians in Finland have formal training. This might deter foreigners, especially temporary workers, from working as electricians in Finland. In contrast, no such obstacles exist for workers in the other four industries shown in Figure 2.5.

"In general, it seems that a former pig farmer from Põltsamaa can become a professional builder during a one and a half hour long ferry trip."

– CEO Jari Syrjälä from LVI-TU, the trade union for HPAC workers

"There are very few industries in which one can do jobs that affect people's health without having proper certifications. For example, everything related to electricity is regulated and things are working well."

– Chief Specialist Petri Mero from Finance Finland

[26] See an article by an insurance provider, *If*, at `https://www.if.fi/henkiloasiakkaat/vakuutukset/kotivakuutus/muuttaminen/asunnon-remontti-ja-vakuuttaminen` (accessed August 14, 2019).

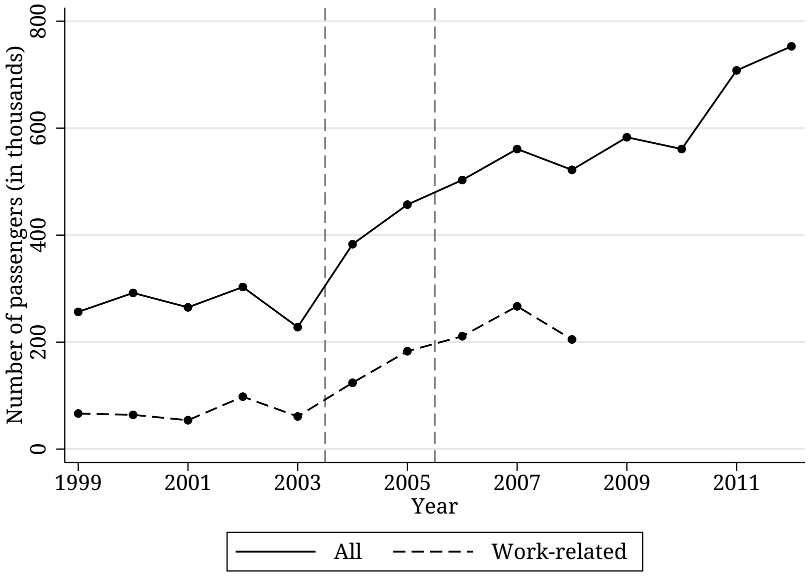

Figure 2.2. Estonian travelers to Finland (in thousands).

Notes: Vertical lines mark the transition period. *Source*: Border Interview Surveys. Available online at `http://www.stat.fi/til/rajat/index_en.htm` (accessed May 12, 2019).

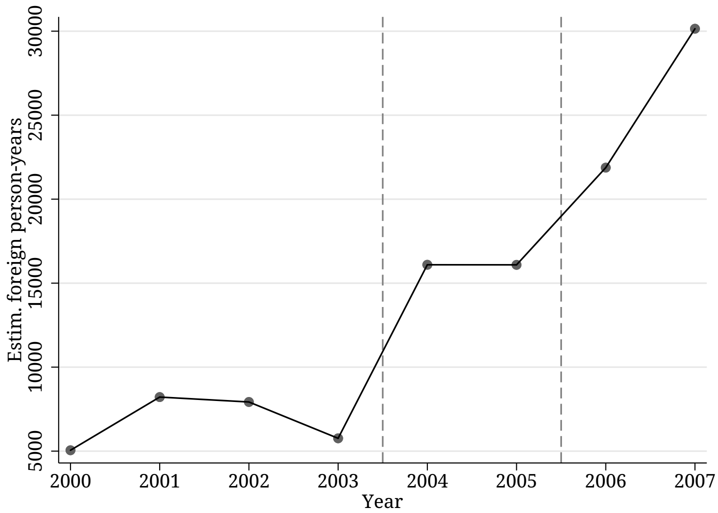

Figure 2.3. Estimated number of foreign person-years outside the labor force survey.

Notes: Vertical lines mark the transition period. *Source*: Hirvonen et al. (2010).

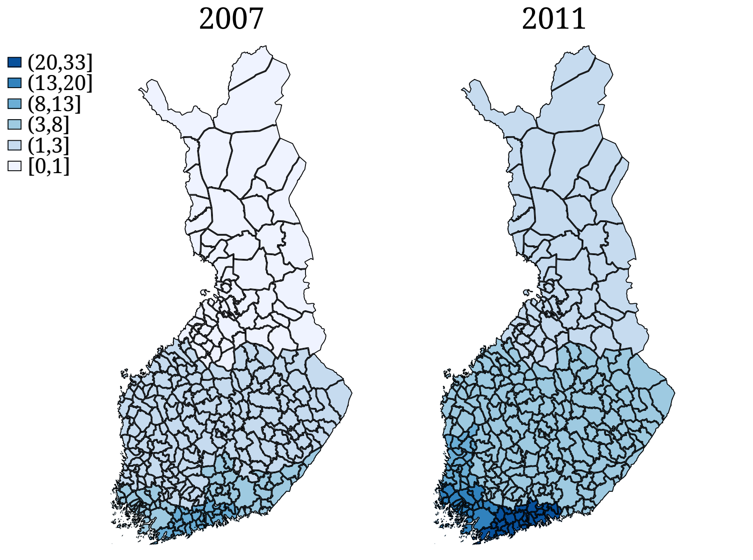

Figure 2.4. Share of foreigners in CFCI's member companies' construction sites by county in 2007 and 2011.

Source: CFCI.

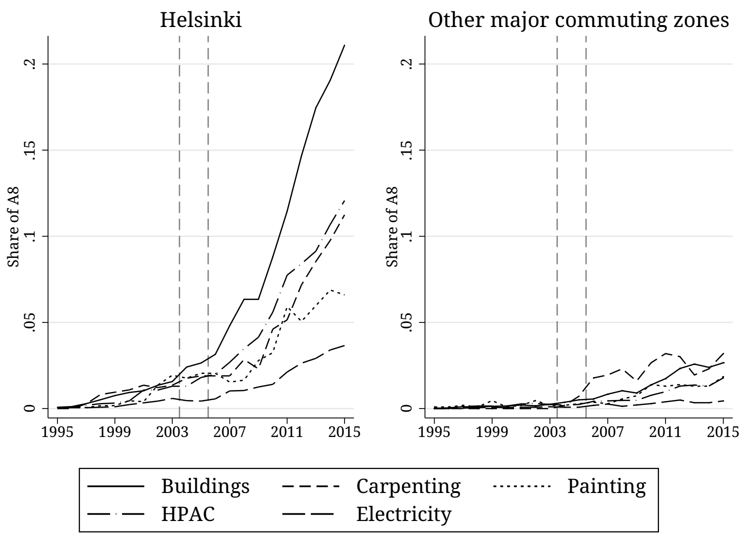

Figure 2.5. Development of the share of A8 workers.

Notes: The figures show the development of the share of A8 workers in affected construction industries in Helsinki and five next biggest commuting zones between 1995 and 2015, relative to the number of workers in 2000. Vertical lines mark the transition period. *Source*: Authors' own calculations based on administrative data.

2.3 Data and Empirical Strategy

This section introduces our data and empirical approach. We use detailed longitudinal administrative registries of individuals to build a data set that covers all workers in the five most common construction occupations (painter, plumber, carpenter, builder, and electrician) in six major commuting zones in Finland.

Our empirical approach hinges upon differential exposure of occupations and regions to the EU enlargement. Our data allow us to follow the exposed individuals over years, as opposed to only following exposed regions. Thus, our identification does not suffer from natives' selective out- and in-migration to these regions.

2.3.1 Data

We use individual-level longitudinal administrative data from Statistics Finland. Importantly, these data contain a number of individual-level characteristics and labor market outcomes. In Appendix 2.B.1, we discuss our data and variables in more detail.

Dependent Variables The administrative data provide us with an opportunity to study an extensive set of outcome variables. First, we look at two fundamental labor market outcomes: work-related earnings, and months of unemployment. Second, we study natives' labor market adjustment outcomes. We focus on switching to a non-construction sector, changing to work in another firm, or moving (i.e., starting to work in another commuting zone). Third and finally, we investigate retirement-related outcomes: full retirement, part-time retirement, and disability pension.

Constructing the Data Set We start by taking all natives in our administrative data who worked in the construction sector in the year 2000. We fix everything to the year 2000 in order avoid any anticipatory movements of natives that might affect the composition of individuals in each region. The year 2000 is chosen because we only have the occupation codes for years 1995, 2000, and annually from 2004 onwards. In addition, the occupation codes from 2004 onwards are too crude to separate some of the construction sector workers into sub-occupation categories (such as electricians or builders).

We focus on individuals who worked in one of the five most important construction sector jobs, which covered about 85% of the sector's workers in the year 2000: carpenter, builder, painter, plumber, or electrician (see also Appendix Figure A1). An obvious limitation is that we do not take into account unemployed individuals with these occupations in 2000, as unemployed individuals do not have an occupation or industry code in our data.[27]

[27]In the study by Dustmann et al. (2017), the employment response to a cross-border worker shock is largely driven by smaller inflows of natives into employment.

A fundamental challenge for estimating the impacts of EU enlargement causally is finding a suitable control group. Helsinki commuting zone is by far the largest commuting zone of Finland. To construct treatment and control groups that would be as comparable as possible before the treatment, we focus our attention only to those individuals who, in the year 2000, worked in one of the six major commuting zones (CZs): Helsinki, Turku, Tampere, Lahti, Jyväskylä, or Oulu. These commuting zones are likely to have an active construction sector with more similar infrastructure and building projects as Helsinki than the smaller commuting zones.[28] Moreover, major construction projects in smaller commuting zones might not be performed solely by commuting zone's own workers due to a smaller number of construction workers and companies in these areas.[29]

Altogether, Helsinki and the other five major commuting zones cover 64 municipalities out of 311, and about half of the working age population. It is worth mentioning that this commuting zone is an approximation, as the municipality of the establishment might not be the true municipality in which the work is performed. Our data lack information on the actual construction sites, which can change on a yearly, even monthly or weekly basis.[30]

Our final sample consists of 30,013 unique individuals. We follow these individuals and their labor market outcomes for the years 1998–2016. Our panel is unbalanced. Some individuals drop out of our data if they move abroad or die during the sample period. Furthermore, some younger individuals (in the year 2000) might be too young (and thus, outside of the labor force) to appear in the data before 2000.[31] We provide summary statistics using the data from this base year in Appendix Table B1.

2.3.2 Empirical Set-Up and Preliminary Analysis

The eastern enlargement of the EU, given its nature, provides us with three sources of variation that we use in our empirical setting: occupational, regional, and time variation. More specifically, we use the triple-differences approach, in which we exploit the differential exposure to foreign workers across certain occupations and regions before and after the eastern enlargement of the EU. We thus follow treated individuals, rather than regions or occupations, over the years; see also Foged and Peri (2016) for a similar strategy. This strategy compares individuals in vulnerable occupations (builders, painters, carpenters, and plumbers) with individuals in a non-vulnerable

[28]See a map of all the commuting zones in Finland at `https://www.stat.fi/meta/luokitukset/tyossakayntial/001-2008/index.html` (accessed February 25, 2020).

[29]Despite these caveats, our estimation results and qualitative conclusions do not change fundamentally even if we included all the commuting zones in our analyses.

[30]The potential measurement error caused by this approximation may bias our treatment effect towards zero.

[31]In the first year of our sample, the year 1998, we have 111 individuals fewer in our data than in 2000. In the year 2016, we lose 4,363 individuals.

occupation, electrician, in a high-exposure treatment region, the Helsinki commuting zone, to individuals in these same occupations in low-exposure control regions, other major commuting zones (Turku, Tampere, Lahti, Jyväskylä, and Oulu). So our setting is very similar to that in Bratsberg and Raaum (2012), with the exception that we study an exogenous shock to immigration stemming from opening borders, and also exploit regional variation in the use of foreign labor.[32] The treatment and control regions are shown in Figure 2.6.

The advantage of using the triple-differences strategy is that we can control for systematic differences and changes across our treated and control regions in the construction sector. Similarly, changes unrelated to the enlargement of the EU that affect the occupations differently are controlled for. By using variation within the construction sector, we also get rid of the endogenous self-selection of migrants into a booming sector and region. In contrast, a standard difference-in-differences specification, in which we compare vulnerable occupations across high- and low-exposure regions or vulnerable occupations to non-vulnerable occupations within the Helsinki commuting zone, would likely suffer from the above-mentioned problems.

In an ideal case, we would start with a first-stage specification that links occupations and regions to the share of foreign workers each year. This is not something we observe in our data. As we discuss in Section 2.2, we do not have information about foreign workers who work in the construction sector as posted workers or for a subcontractor. Our data only cover the foreigners who reside and are employed in Finland. Furthermore, we do not have yearly information on occupations. Thus, it is impossible to follow occupation-specific use of foreign workers over the years. With the data at hand, we are able to use a reduced-form approach that allows us to identify the overall effect of the eastern enlargement of the EU.

First Glance at the Differences As a first step, we analyze on differences in means in Table 2.1. The table reports descriptive statistics on our main labor market outcomes for (i) vulnerable and non-vulnerable (control) occupations in (ii) high- and low-exposure (Helsinki and control, respectively) commuting zones, (iii) six years before and 13 years after the enlargement of the EU.

Let us first consider the earnings outcome in Panel A. The treated individuals earned, on average, 24,325 euros before the treatment and 23,976 after it—the difference being 349 euros. Similarly, the earnings of electricians in Helsinki commuting zone increased by 584 euros. Subtracting this difference from the first one gives us the conventional difference-in-difference estimate for the Helsinki commuting zone: $DD^{Helsinki} = -349 - 584 = -933$ euros.

[32]Another key difference to Bratsberg and Raaum (2012) is that our control group consists only of electricians as opposed to electricians and plumbers.

One concern is the possibility that earnings of the treated and control occupations are subject to systematically different changes that have nothing to do with the enlargement of the EU. Having untreated workers in vulnerable occupations in another region allows us to net out this potential earnings trend that might be different from the electricians' earnings trend. We can compute a respective difference-in-differences estimate for the control commuting zones: $DD^{Control} = -88 + 663 = 575$ euros. The difference between $DD^{Helsinki}$ and $DD^{Control}$ gives us the triple-difference estimate which reveals a negative impact of the EU enlargement on natives' wages: $DDD = -933 - 575 = -1,508$ euros ($p = 0.054$).

Going through similar calculations for months of unemployment shows that the EU enlargement had a small positive impact ($DDD = 0.128$, Panel B of Table 2.1). This triple-difference is statistically significant at 5% level. For the rest of the outcomes, these differences in means are in Appendix Table B2. We show that the EU enlargement had (i) a small positive effect on moving to a non-construction sector occupation ($DDD = 0.004$); (ii) a small positive effect on starting to work at another establishment ($DDD = 0.033$); (iii) a small positive effect on moving to another commuting zone ($DDD = 0.035$); (iv) a small positive effect on retiring ($DDD = 0.018$); (v) a negative effect on part-time retirement ($DDD = -0.006$); and (vi) no effect on being on disability pension ($DDD = 0.000$).

Table 2.1 also shows that electricians were differentially affected in the control and treatment regions. The positive effect on earnings in the treated region suggests that electricians may have benefited from an inflow of foreign workers to complementary occupations. We explore this further in Section 2.5. The differential earnings responses of electricians in the control and treated regions could also be explained by differences in the composition of workers. For example, electricians in the control region were older than electricians in the treatment region (see Appendix Table B1).

2.3.3 Regression Framework

The comparisons in Table 2.1 are, of course, quite crude. For example, they do not account for potential differences in the characteristics of workers residing in different regions.[33] To obtain formal estimates and to conduct statistical inference, we start with a parametric triple-differences specification which takes the following form:

$$\begin{aligned} Y_{iomt} = &\beta \text{Vulnerable}_o \times \text{Helsinki}_m \times \text{After}_t \\ &+ \lambda_i + \lambda_{mt} + \lambda_{ot} + \epsilon_{iomt}. \end{aligned} \tag{2.1}$$

[33]Summary statistics by region are available in Appendix Table B1.

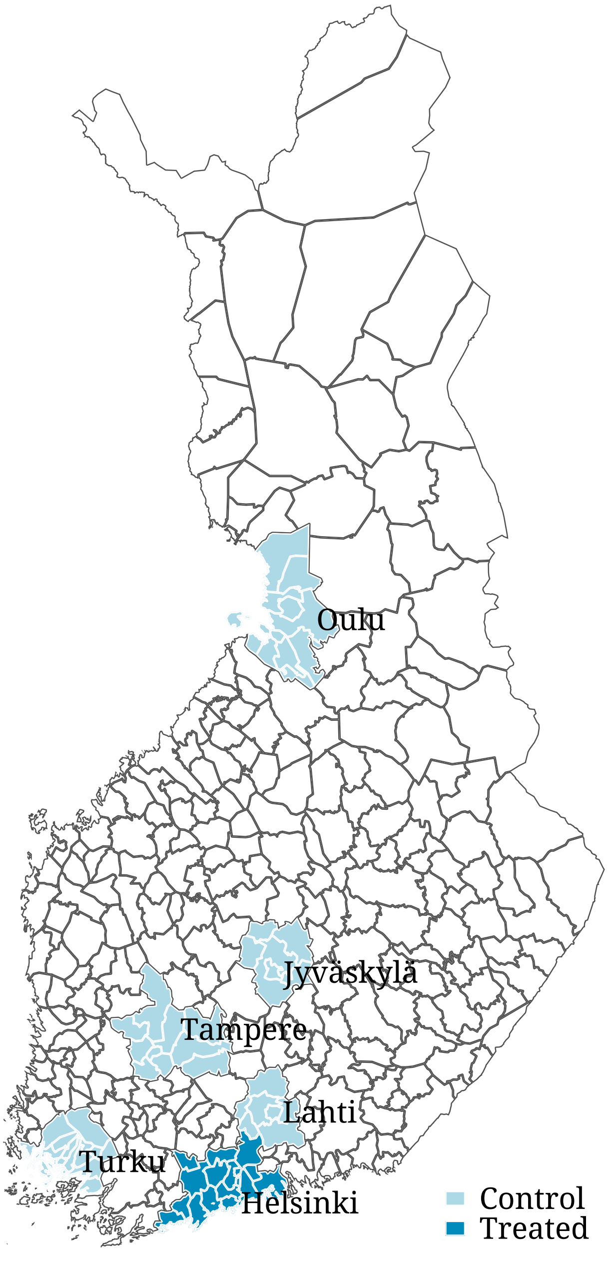

Figure 2.6. Treatment and control regions.

Table 2.1. EU enlargement and labor market outcomes: differences in means.

	Helsinki CZ			Control CZs		
	Vulnerable (1)	Control (2)	Difference (3)	Vulnerable (4)	Control (5)	Difference (6)
Panel A: Earnings						
Before	24.325	31.432	-7.106***	22.652	29.647	-6.995***
	(15.740)	(15.144)	(1.175)	(14.626)	(14.119)	(0.800)
	[74,809]	[16,154]	[90,963]	[72,543]	[15,823]	[88,366]
After	23.976	32.015	-8.039***	22.564	28.984	-6.420***
	(21.705)	(21.330)	(0.736)	(19.865)	(20.524)	(0.701)
	[151,752]	[33,352]	[185,104]	[148,540]	[32,518]	[181058]
Difference	-0.349	0.584	*DD*	-0.088	-0.663	*DD*
	(0.227)	(0.653)	-0.933	(0.246)	(0.554)	0.575
	[226,561]	[49,506]	(0.575)	[221,083]	[48,341]	(0.522)
			DDD = -1.508* (0.766)			
Panel B: Months of unemployment						
Before	0.841	0.533	0.308***	1.047	0.667	0.380***
	(2.285)	(1.799)	(0.075)	(2.398)	(1.872)	(0.063)
	[74,809]	[16,154]	[90,963]	[72,543]	[15,823]	[88,366]
After	1.122	0.723	0.399***	1.275	0.932	0.343***
	(2.925)	(2.367)	(0.061)	(3.002)	(2.541)	(0.048)
	[151,752]	[33,352]	[185,104]	[148,540]	[32,518]	[181,058]
Difference	0.281***	0.189***	*DD*	0.228***	0.265***	*DD*
	(0.014)	(0.029)	0.091***	(0.030)	(0.046)	-0.037
	[226,561]	[49,506]	(0.021)	[221,083]	[48,341]	(0.043)
			DDD = 0.128*** (0.048)			

Notes: The table reports region- and occupation-level averages before and after the enlargement of the EU in 2004. Vulnerable occupations are builders, painters, carpenters, and plumbers. Control refers to electricians. Earnings are measured in thousands of 2015 euros. Standard errors clustered at the municipality level and reported in parentheses. Number of observations are shown in brackets.
* p<0.10, ** p<0.05, *** p<0.01

Here Y_{iomt} refers to the worker-level outcome of an individual i in occupation o, commuting zone m, and year t. Occupation and commuting zone are fixed to the (pre-treatment) year 2000. Our treated individuals are those who were builders, carpenters, painters, and plumbers (vulnerable occupations) in the year 2000. The dummy Vulnerable_o takes the value 1 for them. We compare workers in these occupations with electricians for whom $\text{Vulnerable}_o = 0$. The treatment dummy Helsinki_m takes the value 1 if the individual worked in the Helsinki region in the year 2000 (high-exposure commuting zone), and 0 otherwise. The indicator variable After_t takes the value 1 after the enlargement of the EU in 2004, and is equal to zero before that. β is the coefficient of interest. It captures the effect of EU enlargement on individuals in vulnerable occupations in the treated region. We include individual fixed effects (λ_i) to capture time-invariant differences between individuals, and also net out commuting zone-time (λ_{mt}), and occupation-time (λ_{ot}) fixed effects. We report parametric regression results for the transition period (2004-2006), a medium period (eight years), and a long period (thirteen years).

We also estimate a non-parametric, dynamic event study model. Such a specification has multiple advantages. First, it can be used to study potential anticipatory movements before the actual enlargement of the EU in 2004. For example, firms in Finland might have reacted to the announcement that the European Union would admit eight eastern European countries.[34] Second, the specification is also useful in studying the dynamic effects of the EU enlargement. The inflow of workers from the A8 countries was likely gradual and increased over the years as the use of foreign, especially posted and subcontractor, workers became more common in Finland. Moreover, the employment restrictions of the initial transition period were lifted in 2006. Lastly, these non-parametric results can also inform us about pre-treatment differences in trends (parallel trends assumption).[35] We expect the effects of the enlargement of the EU to exhibit no trend and be close to zero before 2004. However, the effects might be non-zero a few years prior to 2004 because of potential anticipatory movements.

[34]The A8 countries voted on joining the European Union in 2003. Right after the decision was taken, many rental work companies were founded in Estonia (see an article in *Helsingin Sanomat* available at `https://www.hs.fi/kotimaa/art-2000004214683.html`; accessed May 6, 2019). The loophole in the transition period regarding rental work "services" was widely recognized even before the enlargement in May 2004 (see an article in *Helsingin Sanomat* available at `https://www.hs.fi/kotimaa/art-2000004214638.html`; accessed May 6, 2019).

[35]To be more specific, we can examine whether the difference between individuals employed in exposed occupations and electricians in our treated and non-treated regions evolved similarly before the enlargement of the EU.

The event study design is specified as follows:

$$Y_{iomt} = \sum_{t \neq 2000} \beta_t \text{Vulnerable}_o \times \text{Helsinki}_m \times \lambda_t \\ + \lambda_i + \lambda_{mt} + \lambda_{ot} + \zeta_{iomt}. \tag{2.2}$$

The coefficients of interest are β_t (where $t = 1998, 1999, 2001, 2002, ..., 2016$). They capture the year-specific effects of the EU enlargement. The fixed effects are specified as before.[36]

Identifying assumptions The triple-differences approach identifies the causal effect of the EU enlargement if the difference in (unobserved) trends across vulnerable occupations and electricians is similar across Helsinki (treatment) and other (control) regions. This is what we know as the parallel trends (or paths) assumption.

In addition to the parallel trends assumption, we assume treatment effect homogeneity. This is because we have a setting in which everyone is treated, and we compare individuals with a high treatment intensity to individuals with a low treatment intensity (Fricke 2017). This is a restrictive assumption. In our case, it means that the treatment response to one additional foreign worker should be the same across the treated and control regions and occupations.[37]

2.4 Regression Results

In this section, we present the results from our regression analysis. We start by examining the impacts on two main labor market outcomes, earnings and unemployment. We then explore the following adjustment mechanisms: working in other than the construction sector, changing jobs, moving to work in another commuting zone, and retirement decisions. Furthermore, we assess the heterogeneity of the effects by age and occupation. We conclude the analysis by conducting back-of-the-envelope calculations to back up the effect of 1,000 additional workers using our reduced form estimates and other available information.

[36] Appendix 2.C contains a number of robustness checks. First, note that our main analysis pools together all occupations that were more vulnerable to the foreign workforce. We report findings from a specification in which we allow the effects to vary by occupation. There appears to be very little heterogeneity across occupations, if none at all. Second, we estimate an alternative specification that nets out commuting-zone-specific occupation fixed effects rather than the individual fixed effects. This analysis results in similar conclusions regarding the effects of EU enlargement. Third, we illustrate that our findings are not predominantly driven by workers located in any specific municipality. Our leave-one-municipality-out estimates are stable.

[37] Fricke (2017) proposes a less restrictive assumption under which the difference-in-differences (or in our case, triple-difference) estimator identifies an absolute lower bound: ordered treatment effects. That is, both the treated and control groups respond to the treatment similarly, and that one group is treated more and the response to this treatment is stronger.

2.4.1 Main Results

Labor Market Outcomes We start by presenting the results for earnings (Table 2.2, column 1). Annual earnings of the treated individuals drop by around 1,300 euros during the transition period, and by around 1,700 euros during the medium (until 2010, or after eight years) and long run (until 2016, or after thirteen years) relative to less vulnerable electricians and regions. This corresponds to around 7 percent of their earnings in 2000—almost one month's salary. Panel A of Figure 2.7 plots the regression results using the event study specification. After the enlargement of the EU, we see a gradually strengthening negative effect on earnings for the treated individuals. The negative impact on earnings stabilizes around the end of the transition period, when the treated individuals earn about 2,000 euros less per year.[38]

Treated individuals also face more unemployment months after the enlargement of the EU (column 2 in Table 2.2 and Panel B in Figure 2.7). During the transition period, they spend three days more as unemployed (0.092×30 days). During the medium and long run, the effect amounts to around four or five days. The effect on unemployment months is small and, thus, cannot entirely explain the drop in earnings.[39] We stress that the effect on unemployment should be interpreted with caution, as the pre-treatment estimates are not that different to the estimates after enlargement in the event-study specification.

What explains the decrease in earnings if not changes in employment status? One possibility is simply that the EU enlargement dampens earnings growth in the affected region and occupations. The drop in earnings could also be explained by native adjustment. We turn to these next.

Adjustment Mechanisms It appears that the workers might be anticipating the incoming foreigners before the EU enlargement, and switching from construction to other sectors of employment (Panel C of Figure 2.7). However, the effect is not persistent in the long-run, as can be seen in the parametric estimates (Table 2.2, column 3). The point estimate using a 13-year period after the enlargement is zero. The point estimates are positive for changing jobs (column 4 of Table 2.2 and Panel D of Figure 2.7) but not statistically significant at any conventional levels. Finally, we see a positive impact on the probability to move to work in another commuting zone

[38]We report results using logarithm of earnings in Appendix Figure C8 and Table C6. These show a decrease of about 6 to 9 percent in annual earnings.

[39]We have also looked into the probability of having at least one month of unemployment, being primarily unemployed, and being primarily in employment during the year. For the treated individuals, the probability to have at least one month of unemployment increased by 2-3 percentage points and the probability of being employed decreased by around 3 percentage points after the EU enlargement. The effect on being primarily unemployed is positive but small and statistically insignificant. These results can be found in Appendix Figure C9 and Table C6.

(column 5 of Table 2.2 and Panel E of Figure 2.7), suggesting that the treated individuals in Helsinki might have moved to regions with fewer foreigners. However, this effect is not statistically significant and the event-study estimates after the enlargement are not that different from the pre-treatment estimates.

We detect evidence that the EU enlargement might influence retirement behavior. The parametric estimates (Table 2.2, column 6) show that workers affected by the EU enlargement become about 2 percentage points more likely to retire. On the other hand, there is a clear negative impact on part-time retirement (column 7) of around 5-7 percentage points, depending on the time window. We find no effect on being on disability pension (column 8). The event study specification results in Panel F to H of Figure 2.7 confirm these findings. There is a gradually strengthening positive effect on full retirement after the enlargement of the EU. Similarly, there is a negative effect that gets stronger until 2010 for part-time retirement, after which the effect gradually tails off. For full retirement and disability pension, the event-study plots reveal that there is a potential violation of the parallel trends assumption: a positive anticipatory effect that seems to start as early as 2001. This means that these estimates should be interpreted with caution.

Discussion We have tentative evidence that the decrease in earnings could stem at least partially from workers starting to work in other firms and in other commuting zones that potentially have lower wages.[40] Part of the dip in earnings can also be explained by increased probability of retiring. The decreased use of part-time retirement is also an interesting finding. It could reflect a reduction in the bargaining power of the workers. In a competitive industry where a worker can easily be replaced by a newcomer, it might be harder to negotiate for part-time retirement. The workers seem to retire fully instead.

The effects on unemployment months or the adjustment mechanisms cannot fully explain the instantaneous and economically meaningful drop in earnings of the treated individuals relative to electricians and other regions. Although construction sector wages are collectively bargained in Finland, they are flexible to some extent. These agreements stipulate the minimum wage, and part of the wages are always negotiated locally. Furthermore, electricians have a different collective bargaining agreement from painters or builders. Another explanation for the drop in earnings is a change in working hours, perhaps due to less overtime work or not taking up any additional jobs. These may have evolved differently between the occupations and regions as a result of the EU enlargement. Unfortunately, we do not have data on individual wage agreements or hours worked.

[40] Workers in exposed occupations earned about 26,000 euros annually in the Helsinki commuting zone in the year 2000, whereas the average earnings in our control regions were about 2,000 euros less.

Our findings thus far lead to the following conclusions. First, the drop in earnings suggests that the posted workers were direct substitutes to natives in vulnerable occupations. Second, as this drop is persistent, the natives were potentially unable to properly shield themselves through skill upgrading or changing occupations as seen by Foged and Peri (2016) in Denmark after an influx of refugees. We cannot rule this possibility out for sure, as we are unable to investigate occupational change. Third, the suggestive evidence of natives being more likely to move as a response to increased immigration is in contrast with previous findings from the United States. For example, Card (2001) finds that native mobility rates are insensitive to immigration inflows.

Table 2.2. Effects of EU enlargement.

	Earnings	Months of unemployment	Non-construction	Job change	Moved	Retired	Part-time retirement	Disability pension
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Transition period	-1.342**	0.092*	-0.003	0.022	0.028	0.015*	-0.006***	0.006
	(0.543)	(0.052)	(0.012)	(0.038)	(0.034)	(0.009)	(0.001)	(0.007)
N	267,868	267,868	236,556	205,813	226,967	267,868	267,868	267,868
Until 2010	-1.695***	0.131***	-0.006	0.033	0.034	0.020**	-0.007***	0.004
	(0.622)	(0.040)	(0.011)	(0.033)	(0.035)	(0.010)	(0.002)	(0.006)
N	383,363	383,363	320,391	288,347	310,858	383,363	383,363	383,363
Until 2016	-1.729**	0.152***	-0.000	0.028	0.036	0.023*	-0.005***	0.000
	(0.786)	(0.046)	(0.013)	(0.029)	(0.034)	(0.013)	(0.002)	(0.005)
N	545,491	545,491	421,732	387,306	412,212	545,491	545,491	545,491

Notes: Earnings refers to annual work-related earnings measured in 2015 euros. Non-construction refers to working in a sector other than construction. Job change refers to change in establishment. Moved refers to a change in commuting zone of work. Standard errors clustered at the municipality level are reported in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

2.4.2 Heterogeneous Effects by Age

Next, we turn to heterogeneous effects of the EU enlargement. Heterogeneous effects, if there are any, can also help us reconcile the main findings. Younger and older workers are likely to face differential labor market prospects and thus differ in their adjustment margins, as suggested by Dustmann et al. (2017). The degree to which foreigners substitute for natives may differ between the age groups as well. We group individuals into three groups based on their age in the year 2000: under 30-year-olds, 30-50-year-olds, and over-50-year-olds.

Labor Market Outcomes The sub-sample analysis suggests that the negative effect on earnings is mainly driven by younger (under 30-years-old) and older (over 50-years-old) workers. The parametric DDD estimates for young workers reveal a decrease of around 1,700 euros ($p < 0.05$) in earnings during the transition period and the medium run (until

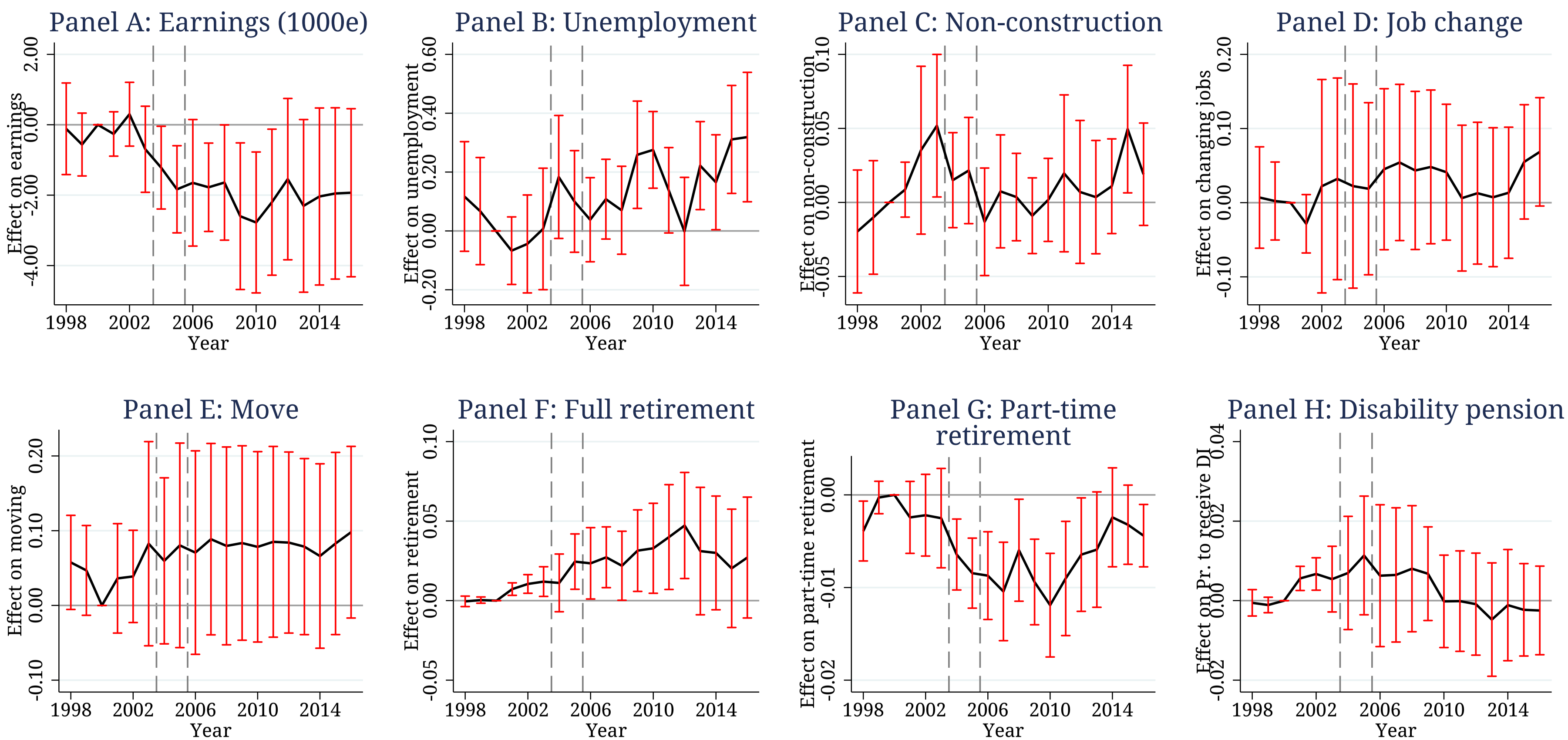

Figure 2.7. Effects of EU enlargement: event study specification.

Notes: Figure shows point estimates and their 95% confidence intervals constructed using standard errors clustered at the municipality level. Gray dashed lines mark the transition period.

2010) (column 1 of Panel A in Table 2.3). The point estimate using a 13-year window is also negative, although statistically insignificant. For older workers, the estimates are in the same ballpark but not significant at any conventional levels. These findings are corroborated by the patterns we see in an event-study specification (see Appendix Figure C5).

The effect on months of unemployment seems to be driven by the 30-50-years-old workers, although the point estimates in column (2) of Table 2.2 are positive and similar in size for all groups and time periods (see also Panel B of Figure C5 in Appendix 2.C). The estimates are too small to be economically meaningful. This suggests that the negative impact on earnings does not, or at least not entirely, stem from changes in employment status.

Adjustment Mechanisms We start analyzing potential adjustment mechanisms by looking into the probability of not working in construction sector, and the probability of changing jobs. These results are found in columns (3) and (4) of Table 2.3, and Panels C and D of Figure C5 in Appendix 2.C. The estimates for the probability of not working in the construction sector do not show any clear pattern (column 3). However, there is a slight positive trend for younger workers in the event-study plots, and the point estimates in the parametric DDD (column 4) show an increase of about 5 percentage points in the probability of changing jobs during the medium and long run. These effects are statistically significant at 10% and 5% levels for the medium and long run, respectively. Lastly, there seems to be no difference between the age groups in the probability to move to work in another commuting zone (column 5 of Table 2.3 and Panel E of Figure C5 in Appendix 2.C).

Retirement Decisions of Older Workers We find suggestive evidence that some older workers adjust to changes in the labor market by retiring. Over 50-year-olds become 3-4 percentage points more likely to retire after the eastern EU enlargement, although these point estimates are statistically insignificant (column 6 of Table 2.3). On the other hand, we see a significant decrease in part-time retirement (column 7). One possibility is that instead of continuing to work part-time and being partially retired, exposed workers decide to retire fully. The point estimates obtained using the event-study specification are reported in Appendix Figure C6. They tally with our parametric estimates. However, these estimates also show a positive anticipatory effect that seems to start as early as in 2001 for full retirement and disability pension. This is a potential violation of the parallel trends assumption, and hence, these estimates should be interpreted with caution.

Discussion The effect on earnings is driven by younger and older workers. Older individuals in exposed occupations seem more likely to retire and less likely to take up part-time retirement. Younger workers respond by changing jobs. These findings imply that different age groups may have different adjustment margins (see also Dustmann et al. 2017 for similar remarks).

Our findings also show that the age groups might differ in their degree of substitutability. Younger, and thus less experienced, workers are more likely to be closer substitutes for unskilled foreign labor than older, and potentially more tenured, workers. Older workers, even though more experienced, might be forced to retire or accept lower wages as they pose a risk of disability and are generally more expensive to the employers as the collective bargaining agreements stipulate higher wages for more experienced workers.[41] This reasoning is reflected in our results, as older workers seem to be less likely to change jobs.

2.4.3 Beyond the Reduced-Form Estimates

We have shown that the eastern enlargement of the European Union had a negative impact on natives' wages in vulnerable construction occupations. Importantly, our estimation framework and the data that are available only allow us to assess reduced-form effects. These effects are virtually due to the influx of workers from the A8 countries, but what can we say about the impact of one additional such construction worker? This Section provides an overview—further details on the back-of-the envelope calculations are available in Appendix 2.D.

Relying on different information sources, we can make naïve guesses about how many came to the Finnish labor market after the enlargement of the European Union. During the transition period, the estimates vary between a conservative estimate of about 2,000 to a liberal approximation of over 20,000 posted and registered workers. Scaling the point estimates for earnings with these numbers suggest that an inflow of 1,000 foreign workers during the transition period induced a decrease of 123-743 euros in earnings, on average. Over the medium and long run, the estimates vary between 3,400 and over 30,000 workers from the A8 countries. Scaling our estimates with these numbers results in a relatively small decrease of 131-400 euros in earnings induced by the arrival of every 1,000 foreigners. Given the wide range of estimates of the number of foreign workers, the approximated effect sizes also exhibit quite a lot of variation, especially for the transition period.[42]

[41]Since 2007, the employers in construction sector have been incentivized to keep the workers at work and healthy by requiring them to pay part of the disability pension of the workers (Kyyrä et al. 2012).

[42]Even the larger estimates are not at odds with what is believed to have happened in many European countries after the eastern enlargement of the European Union. For instance, Schmieder and Weber (2019) show that in Austria, the stock of workers from the A8 countries increased fourfold between the years 2003 and 2016. This number does not include posted workers, and it is thus likely to underestimate the true amount of A8 workers who came in the country.

Table 2.3. Effects of EU enlargement by age.

	Earnings (1)	Months of unemployment (2)	Non-construction (3)	Job change (4)	Moved (5)	Retired (6)	Part-time retirement (7)	Disability pension (8)
Panel A: Under 30-year-olds								
Transition period	-1.665**	0.052	-0.024	0.027	0.013			
	(0.736)	(0.094)	(0.023)	(0.027)	(0.023)			
N	50,098	50,098	44,973	39,781	41,764			
Until 2010	-1.744**	0.120	-0.009	0.054*	0.014			
	(0.721)	(0.075)	(0.024)	(0.027)	(0.024)			
N	72,238	72,238	63,970	58,500	60,780			
Until 2016	-1.274	0.109	0.016	0.053**	0.028			
	(0.902)	(0.078)	(0.025)	(0.026)	(0.022)			
N	105,170	105,170	91,522	85,521	88,334			
Panel B: 30-50-year-olds								
Transition period	-0.409	0.074	0.002	0.029	0.035			
	(0.428)	(0.075)	(0.013)	(0.042)	(0.038)			
N	165,954	165,954	150,621	131,218	145,634			
Until 2010	-0.402	0.107*	-0.002	0.037	0.041			
	(0.453)	(0.062)	(0.012)	(0.036)	(0.038)			
N	238,217	238,217	207,794	187,478	202,837			
Until 2016	-0.308	0.180***	-0.003	0.028	0.039			
	(0.555)	(0.057)	(0.015)	(0.031)	(0.038)			
N	343,871	343,871	279,223	257,098	274,275			
Panel C: Over 50-year-olds								
Transition period	-1.112	0.066	0.005	-0.010	0.019	0.029	-0.035***	0.012
	(1.134)	(0.168)	(0.020)	(0.064)	(0.042)	(0.028)	(0.009)	(0.022)
N	51,816	51,816	40,962	34,814	39,569	51,816	51,816	51,816
Until 2010	-1.592	0.167	-0.023	-0.009	0.034	0.037	-0.035***	0.007
	(1.235)	(0.100)	(0.020)	(0.062)	(0.047)	(0.029)	(0.010)	(0.019)
N	72,908	72,908	48,627	42,369	47,241	72,908	72,908	72,908
Until 2016	-1.495	0.132	-0.023	-0.010	0.035	0.032	-0.025***	-0.002
	(1.586)	(0.090)	(0.021)	(0.058)	(0.046)	(0.025)	(0.009)	(0.014)
N	96,450	96,450	50,986	44,686	49,602	96,450	96,450	96,450

Notes: Age is determined in the year 2000. Earnings refers to annual work-related earnings measured in 2015 euros. Non-construction refers to working in a sector other than construction. Job change refers to change in establishment. Moved refers to a change in commuting zone of work. Standard errors clustered at the municipality level are reported in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

These back-of-the-envelope calculations and our empirical findings show that even though the number of foreigners kept increasing, the negative impact on earnings stabilized after the transition period. We are not able to answer empirically why the effect on earnings did not continue to get larger after the transition period, but there are at least three potential explanations for this. First, Finland entered a recession after the financial crisis of 2007-2008. This might have curbed the differential wage growth of treated and control occupations and regions.[43] Second, as can be seen from Figure 2.4, the control regions also received foreigners after the transition period. Thus, what we identify may be a lower bound for the true effects. Lastly, Estonia experienced a rapid increase in standard of living after joining the European Union (Estonian Ministry of Foreign Affairs 2014). This might have forced Finnish employers to pay Estonian posted workers more than in the beginning.

We can also think of the effect as the elasticity of annual earnings (W) with respect to the increase in labor supply induced by both permanent immigration and cross-border workers from the A8 countries (N/M), $\partial W/\partial(M/N)$. Of course, given the uncertainty about how many workers came to Finland after the eastern enlargement, the approximations of elasticities are very crude. The average annual earnings of workers in vulnerable occupations in the exposed region was about 26,000 euros in the year 2000. Thus, the impact on earnings corresponds to a decrease of about 7 percent when considering the period until 2016. Taking the conservative estimate of 3,400 new workers, the labor supply increased by around 17 percent after the EU enlargement. This would imply that $\partial W/\partial(M/N) \approx -0.238$. This elasticity is smaller than what Bratsberg and Raaum (2012) document in the Norwegian construction sector. They find an elasticity of -0.663 using data on daily wages. Aydemir and Borjas (2007) also report similar elasticities in Canada, the U.S., and Mexico. However, most approximations that we have at hand suggest that there were more cross-border workers. Should the EU enlargement have doubled the labor force in vulnerable occupations in the exposed region, as suggested by estimates from several sources, the elasticity would be close to zero.

The conclusion that the overall impacts of low-skilled foreign workers on low-skilled native workers' wages and employment are possibly small or non-existent is not unique. For example, Card (1990) and Peri and Yasenov (2019) document no effects of Cuban immigrants on the Miami labor market in the aftermath of Mariel boatlift. Similarly, Clemens et al. (2018) find that excluding the Mexican *braceros* from the U.S. labor market did not induce any changes in natives' labor market outcomes, on average.

[43]For example, the wage growth of electricians might have dropped more in comparison because of potentially higher locally agreed benefits and bonuses that were easier to cut. The individuals in treated occupations and regions might already have received wages at the minimum level required by the collective bargaining agreements without any locally agreed bonuses to cut.

2.5 Sensitivity Analysis

We have now shown that immigration negatively affected vulnerable construction-sector workers *relative* to less vulnerable electricians and non-exposed regions. It is possible that the EU enlargement had distributional consequences, and it could have created both winners and losers. For example, native electricians can be complements to foreign workers in other construction sector occupations. An influx of cheap labor into these complementary occupations could then increase the demand for electricians (Borjas 1995). Consequently, electricians in Helsinki commuting zone could experience an upward pressure on their wages. To understand whether our findings could be driven by electricians being the winners of the economic integration process, we estimate a standard difference-in-differences specification. We exploit only regional variation to study whether electricians in the highly exposed region were also affected by the EU enlargement.

If this is the case, our triple-differences approach might over- or underestimate the effects on vulnerable workers. To avoid this pitfall, we complement our findings in the previous section with results from a standard difference-in-differences specification by comparing individuals in vulnerable occupations in treated and control regions.

2.5.1 Complementarity of Electricians

Empirical Specification We use a difference-in-differences (DD) specification to study whether the enlargement of the EU also affected individuals in the control occupation, electricians, in Helsinki region (in 2000). Our baseline DD specification, *for electricians only*, is specified as follows:

$$Y_{imt} = \gamma \text{Helsinki}_m \times \text{After}_t + \lambda_i + \lambda_t + \mu_{imt}. \tag{2.3}$$

Here γ is the coefficient of interest that captures the effect of the enlargement of the European Union on working in the Helsinki commuting zone (in 2000) relative to other major commuting zones. The treatment variables are defined analogously to Equation 2.1. We also control of individual and time fixed effects.

Estimation Results The parametric DD point estimates reported in column (1) of Table 2.4 show that there is potentially a positive and gradually increasing effect on earnings. Electricians in the treated region earn around 400 to 1,400 euros more after the enlargement of the EU, but these effects are not statistically significant. There is evidence of a slight negative trend in months of unemployment after the enlargement of the EU, although the point estimates are small and insignificant (column 2 of Table 2.4).

In Appendix 2.E, we report results from a corresponding event study specification that delivers similar findings.

The regression results for working in the non-construction sector and retirement are slightly more concerning. We see that electricians in Helsinki become about 2-3 percentage points more likely to move away from the construction sector (column 3 in Table 2.4). The sign of this effect is perhaps the opposite from what one might expect. There is no obvious *ex post* interpretation to this finding. The most common sectors among those individuals who no longer worked in construction were facility support activities and transportation. However, the data suggest that they may have carried on working in similar jobs. The electricians who switched to other sectors were most likely to still to work as electricians or electrical mechanics during the years for which we observe their occupation.

Furthermore, electricians in Helsinki seem less likely to fully retire after the enlargement of the EU, although these estimates are small and only marginally significant when we consider the full time span (column 6). They also become marginally more likely to take up part-time retirement (column 7).

In sum, we find weakly suggestive evidence that on average more skilled electricians (in the treated region) are complements to foreign workers in the exposed occupations. Although most of the effects that we detect for electricians are not statistically significant at any conventional levels, the confidence intervals are wide. For annual earnings, the upper bound of the 95% confidence interval varies from 1,500 to 3,000 euros. Similarly, the lower bound of the confidence interval for unemployment is somewhere between one-and-a-half and six unemployment days less. This means that the triple-difference results in the previous section could be entirely a result of the complementarity of electricians. To understand whether our triple-differences results are driven by this, we next proceed to compare vulnerable occupations in exposed and control regions.

2.5.2 Robustness Check: Difference-in-Differences Analysis

As an alternative to the triple-differences model, we estimate a standard difference-in-differences model similar to the one specified in Equation 2.3. Now, the treatment group consists of individuals in the vulnerable occupations in Helsinki, whereas the control group is formed by individuals in the same occupations but in low exposure control commuting zones.

It is comforting to report that the difference-in-differences results (see Table 2.5) largely resonate with the qualitative conclusions drawn from our triple-difference results in the previous section. Importantly, the effect on annual earnings is still negative, although slightly lower than suggested by the triple-differences results. The effect is

Table 2.4. Complementarity of electricians: difference-in-differences results.

	Earnings	Months of unemployment	Non-construction	Job change	Moved	Retired	Part-time retirement	Disability pension
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Transition period	0.396	0.057	0.024*	-0.028	-0.021	-0.011	0.004***	-0.004
	(0.577)	(0.057)	(0.013)	(0.039)	(0.036)	(0.007)	(0.001)	(0.006)
N	47,803	47,803	43,643	41,624	42,523	47,803	47,803	47,803
Until 2010	0.979	-0.021	0.027**	-0.034	-0.021	-0.012	0.004***	-0.003
	(0.696)	(0.048)	(0.011)	(0.035)	(0.036)	(0.009)	(0.001)	(0.005)
N	68,521	68,521	60,125	57,982	59,007	68,521	68,521	68,521
Until 2016	1.350	-0.087	0.019	-0.033	-0.021	-0.020*	0.003**	-0.002
	(0.851)	(0.054)	(0.014)	(0.030)	(0.035)	(0.012)	(0.001)	(0.004)
N	97,847	97,847	80,640	78,297	79,522	97,847	97,847	97,847

Notes: Earnings refers to annual work-related earnings measured in 2015 euros. Non-construction refers to working in a sector other than construction. Job change refers to change in establishment. Moved refers to a change in commuting zone of work. Standard errors clustered at the municipality level are reported in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

more pronounced during the transition period, about 1,000 euros, and the point estimates become gradually smaller in absolute terms when we expand the time span (column 1). Similarly, we confirm a positive effect on months of unemployment (column 2). The regression results suggest a small increase in the months of unemployment, 0.15 months (i.e., around 5 days), during the transition period. Again, the point estimates become gradually smaller when the time horizon is expanded.

We also find similarities between the DD and DDD estimation results for the adjustment mechanisms. For example, we find positive, though very small, effects on retirement (column 6), and negative effects on part-time retirement (column 8). In contrast, we find that the propensity to not work in the construction sector is positively affected by the EU enlargement (column 3). These estimates are highly significant. The most common industries for vulnerable workers who switched from construction were facility support activities, cleaning, temporary employment services, vocational education, and unknown industries.

Further regression results can be found in Appendix 2.F, where we report estimation results from a event study specification as well as heterogeneous effects by age. These results are also in line with the respective triple-differences results.

Findings from the difference-in-differences analysis should be treated with caution. Unlike in the triple-differences specification, which also exploits within-sector variation, we now cannot partial out endogenous self-selection of foreign workers into a booming sector and region. Moreover, we cannot control for all other factors that are unrelated to the enlargement of the EU but that might affect the regions differently. Our Appendix materials also report an event-study graph to test for parallel trends assumption for this difference-in-differences specification. For most of the outcomes, it seems to perform worse than our main triple-differences specification. This provides yet another justification for relying on the triple-differences approach as our main identification strategy.

Table 2.5. Effects of EU enlargement: difference-in-differences results.

	Earnings	Months of unemployment	Non-construction	Job change	Moved	Retired	Part-time retirement	Disability pension
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Transition period	-0.972***	0.149***	0.022***	-0.002	0.004	0.004	-0.002***	0.001
	(0.210)	(0.024)	(0.005)	(0.016)	(0.007)	(0.003)	(0.001)	(0.002)
N	220,065	220,065	192,913	164,189	184,444	220,065	220,065	220,065
Until 2010	-0.721***	0.111***	0.022***	0.001	0.011	0.008**	-0.003***	0.001
	(0.251)	(0.025)	(0.005)	(0.015)	(0.007)	(0.003)	(0.001)	(0.003)
N	314,842	314,842	260,266	230,365	251,851	314,842	314,842	314,842
Until 2016	-0.385	0.067**	0.020***	-0.004	0.012	0.002	-0.002***	-0.003
	(0.335)	(0.032)	(0.004)	(0.014)	(0.007)	(0.003)	(0.001)	(0.003)
N	447,644	447,644	341,092	309,009	332,690	447,644	447,644	447,644

Notes: Earnings refers to annual work-related earnings measured in 2015 euros. Non-construction refers to working in a sector other than construction. Job change refers to change in establishment. Moved refers to a change in commuting zone of work. Standard errors clustered at the municipality level are reported in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

2.6 Concluding Remarks

There has been heated debate about the consequences of opening national borders to foreign workers, for instance in the context of the expanding European Union. In this paper, we have studied the labor market consequences of freer cross-border mobility of workers using the eastern enlargement of the EU as a natural experiment. In line with standard models of immigration and the labor market, our main results show that vulnerable workers in exposed regions in the construction sector experienced a decrease in their annual earnings and a small increase in unemployment relative to less vulnerable workers in non-exposed regions following the expansion of the EU. We have also shown that the effect on earnings is predominantly driven by workers below 30 years old, who became more likely to switch to other sectors of employment or establishments of work, and workers over 50 years old, who became more likely to retire.

In sum, there is no doubt that opening the Finnish borders to foreign workers created economic losers. But one should bear in mind that we identify the effects of open borders merely on a fraction of society. Several interesting aspects that we do not consider in this paper offer promising avenues for future research. These ought to be explored to reconcile the overall labor market effects of EU enlargement, or more generally, opening borders to cross-border workers.

First, we have not studied occupational mobility and skill updating in the spirit of Foged and Peri (2016) and Peri and Sparber (2009). Second, we have not covered the impacts of EU enlargement at the firm level. Giesing and Laurentsyeva (2017) employ cross-country data to investigate the connection between firm productivity and emigration rates from EU countries, exploiting the opening of EU labor markets between 2004 and 2014. Related work by Beerli et al. (2018) examines effects on firms in the context of freer labor movement across the Swiss border. Third, anecdotal evidence from Finland suggests that the use of foreign workforce was much more common in non-unionized firms. How unionization interacts with open border policies is an unexplored question. Furthermore, exploiting variation in unionization might offer another opportunity for causal identification. Fourth, we should also collect further evidence from the new EU countries to fully grasp the impacts of integration. Dustmann et al. (2015) and Elsner (2013) examine how emigration shaped the Polish and Lithuanian labor markets, respectively, around the eastern enlargement of the EU. Finally, a separate but related issue—that this paper abstracts from—is that the use of temporary posted workers is likely to lead to lost tax revenue. In our case, most of the cross-border workers do not live or pay taxes in Finland. This leads to both directly and indirectly lost tax revenue (income tax and VAT, respectively).[44] Assessing such effects would also be important.

Political and economic integration is still an on-going process in Europe. At present, there are five recognized candidates for future membership of the EU: Turkey, North Macedonia, Montenegro, Albania, and Serbia. The most advanced candidates, Serbia and Montenegro, are expected to join the union before the year 2025. All the potential future members have lower wage levels than the current EU countries, on average, or their border neighbors that belong to the union. Our findings can inform policy-makers about potential consequences of opening borders to workers from new EU countries, or possible effects of similar policies elsewhere in the world. Understanding this is elementary for shaping optimal policies regarding the movement of workers across the borders.

[44]See Dustmann and Görlach (2016) for related theoretical arguments. Dustmann et al. (2010) study the fiscal costs and benefits of A8 migration to the United Kingdom. They find that the A8 have a higher labour force participation rate, pay more indirect taxes, and use less benefits and public services than comparable natives. Battisti et al. (2018) bring together labor market and redistributive issues by assessing the welfare effects of immigration on different types of workers in a general equilibrium model that takes into account search frictions, wage bargaining, and redistribution. Their exercise highlights that the overall effects of immigration may be positive for both high- and low-skilled workers.

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Appendix

This document contains auxiliary materials to the paper ”Labor-Market Effects of Open Borders: Lessons from EU Enlargement”. Appendix A provides further background on the Finnish labor markets, and in particular the construction sector. We discuss the construction of our data set in Appendix B, where we also report the summary statistics. In Appendix C we show additional triple-differences results. Appendix D discusses our back-of-the-envelope calculations. We report auxiliary difference-in-differences results for electricians in Appendix E. Finally, Appendix F presents estimation results comparing vulnerable workers in exposed and non-exposed regions using a difference-in-differences approach.

2.A Further Background Information

In this appendix, we provide additional characterizations of immigration to Finland, the labor market, and the construction sector.

Figure A1 first illustrates that the most common construction occupations in the year 2000 were carpenters, builders, electricians, plumbers, and painters. Individuals in these occupations mainly worked in the building, electricity, painting, and HPAC industries (see Figures A2 and A3).

Next, we demonstrate that commuting zones differed in their use of registered A8 but not in the use of non-A8 foreign workers. In Figure A4 we show that the share of non-A8 workers was low, below six percent (although increasing over the entire sample period), in Helsinki and other major commuting zones in the exposed industries. Figure A5 plots the share of registered A8 in these and all other construction sector industries with at least 2,000 workers in the year 2000. In contrast, there are many more A8 workers in Helsinki than in other major commuting zones.

Last, we present some descriptive statistics on the construction sector in general. Figure A6 shows the development of the number of workers in the exposed industries relative to the size of the industry in 2000 in Helsinki versus other major regions. There are some differences but the drop in the painting industry in Helsinki region may reflect the increased use of unregistered labor. Figure A7 plots the number of construction sector permits granted, projects started, and projects finished in Finland from the early 1990s. The figure shows that Finland experienced a boom in construction during the transition period.[1]

[1] These numbers come from Statistics Finland. Unfortunately, they are not available at the regional level.

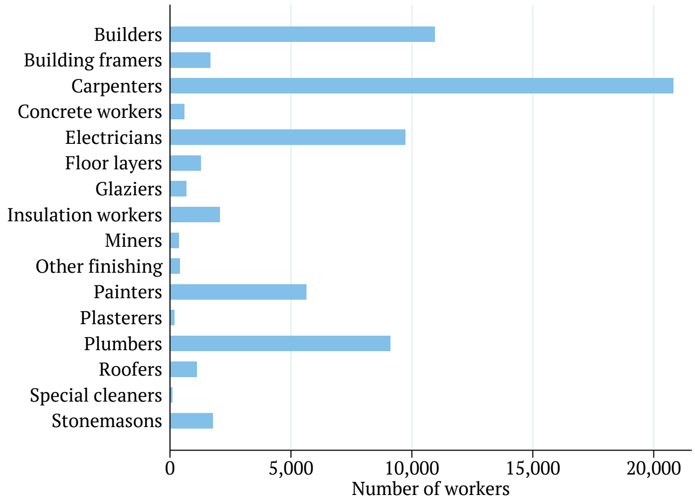


Figure A1. Most common construction occupations within the construction sector.

Source: Authors' own calculations based on administrative data.

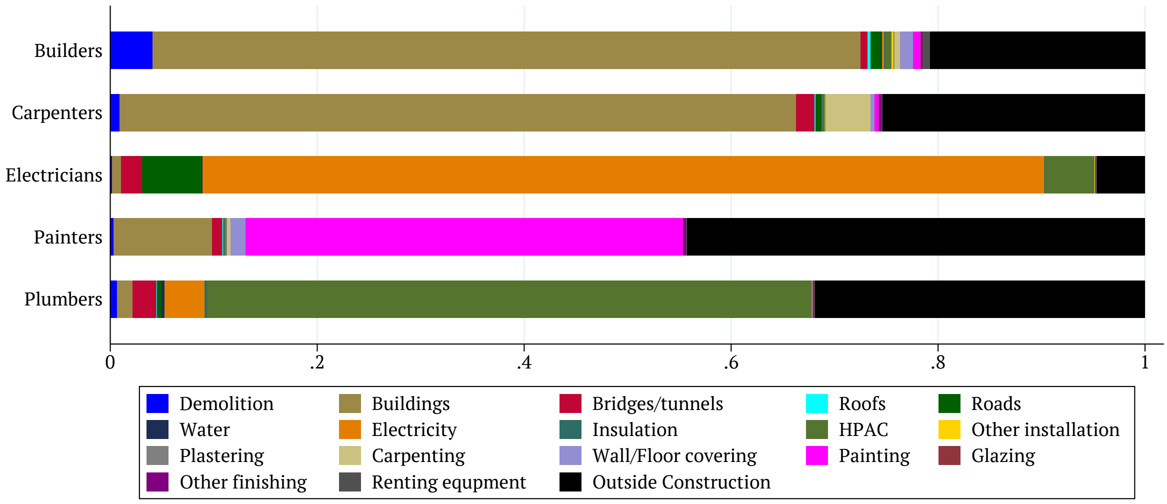


Figure A2. The industries of the five most common occupations in the construction sector.

Source: Authors' own calculations based on administrative data.

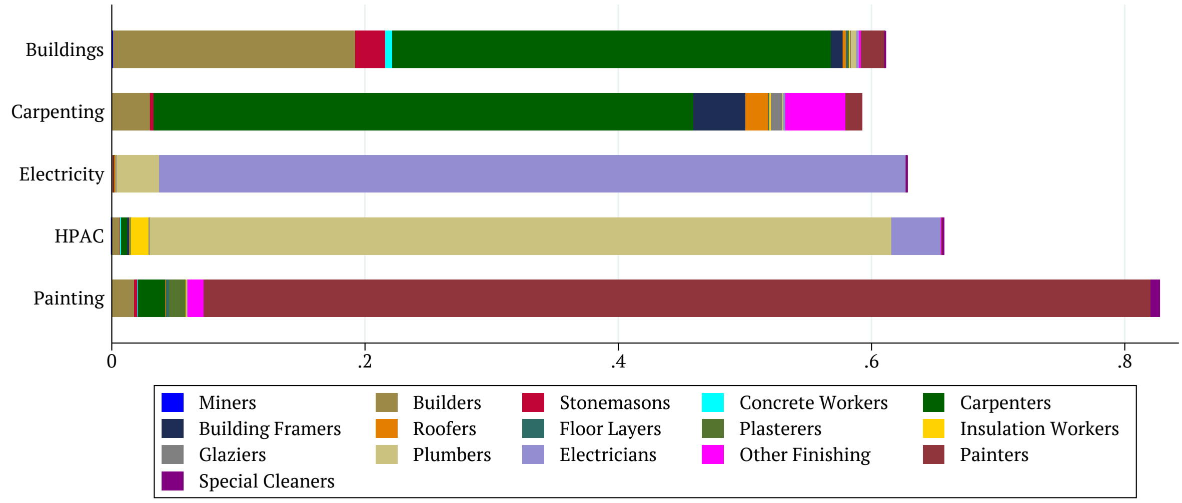


Figure A3. The five most common industries within the construction sector and the most common construction occupation within these.

Source: Authors' own calculations based on FLEED.

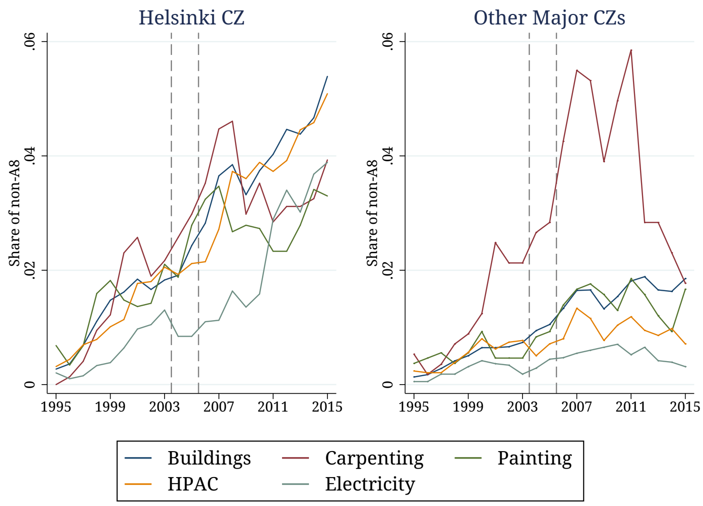


Figure A4. Development of the share of foreign, non-A8, workers in affected construction industries in Helsinki and the five next biggest commuting zones between 1995 and 2015 relative to the number of workers in 2000.

Notes: Vertical lines mark the transition period.
Source: Authors own calculations based on administrative data.

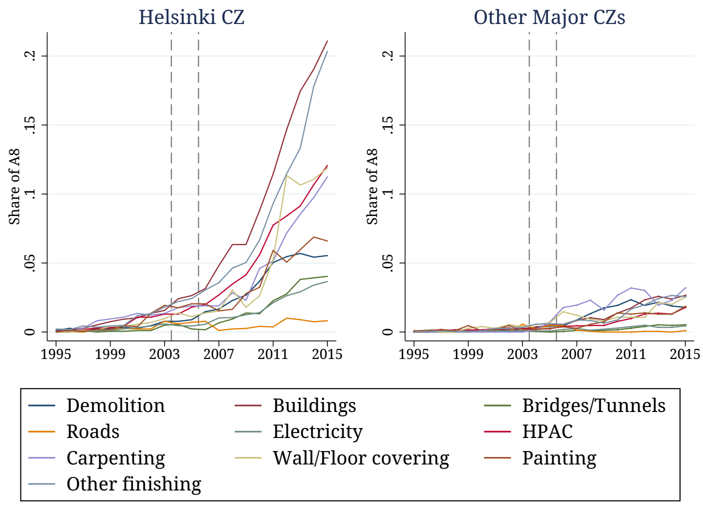


Figure A5. Development of the share of A8 workers in major construction industries in Helsinki and the five next biggest commuting zones between 1995 and 2015 relative to the number of workers in 2000.

Notes: Vertical lines mark the transition period.
Source: Authors own calculations based on administrative data.

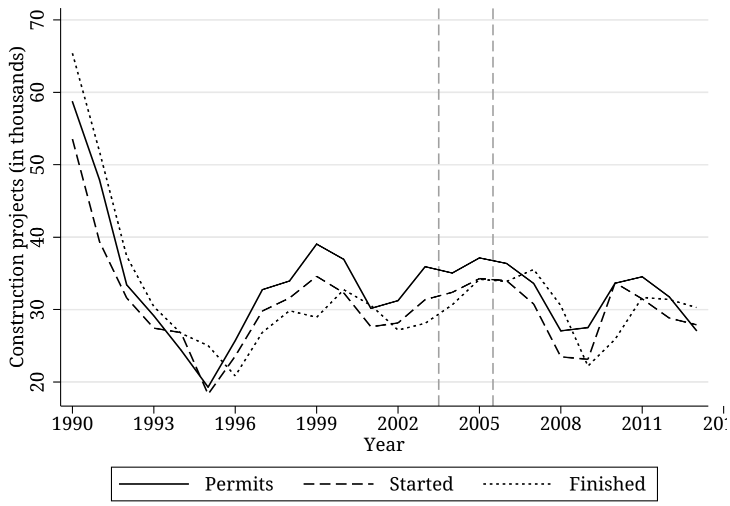


Figure A6. Development of the construction sector.

Notes: Vertical lines mark the transition period.
Source: Statistics Finland.

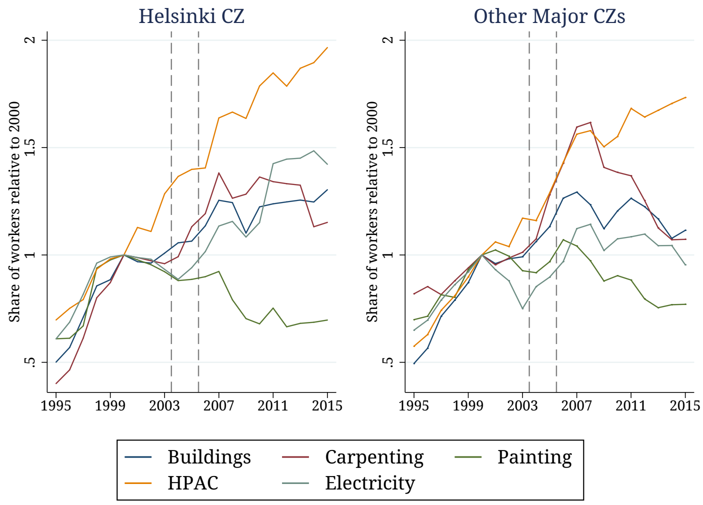


Figure A7. Development of the share of workers in affected construction industries in Helsinki and the five next biggest commuting zones between 1995 and 2015 relative to the share of workers in 2000.

Notes: Vertical lines mark the transition period.
Source: Authors own calculations based on administrative data.

2.B Data and Descriptive Statistics

2.B.1 Data

In this section, we discuss our data sets, data construction, and variables in more detail.

Data Sources We use three different individual-level longitudinal administrative data sets from Statistics Finland. The first is called *Finnish Longitudinal Employer-Employee Data total* (*FLEED total*). These data cover all 15-70-year-old individuals. They contain individual-level information on education and labor-market outcomes. Furthermore, they link the employed individuals to industries and establishments.[2] Variables encompassed by these data include (but are not limited to) highest education level achieved (and the year of completion), income, work earnings, capital income, entrepreneurial earnings, benefits received (such as maternity leave, social, or unemployment benefits), employment status (employed, unemployed, or out of labor force), occupation, and sector/industry of employment. They also include demographic background information such as gender, native language, and date of birth, number of children, nationality and marital status. These data are used to construct a data set of constructions sector workers for years 1998-2016.

The second data source is an employment module of the *FOLK* database, containing all individuals in Finland. In addition to containing most of the same information as *FLEED total*, these data link individuals to the municipality of the establishment they work at.

Our third data source is called *FLEED pension periods*. These data contain individual-level information on all retired individuals in Finland. Specifically, we use these data for the information about the type of retirement, such as disability pension or part-time retirement.

Constructing the Data Set We start by taking all natives (Finnish nationals) in *FLEED total* who work in the construction sector (two-digit level TOL 1995/NACE Rev. 1 -code: 45) in the year 2000. More specifically, we keep only individuals who worked in one of the five most important construction sector jobs in the year 2000: carpenter, builder, painter, plumber, or electrician.[3] For this we use occupation codes (Finland's

[2]By establishment we refer to an economic unit of a firm/enterprise. In our data, larger firms with operations in different parts of Finland would have several different geographically and/or productionally separate units (i.e., establishments).

[3]These occupation classifications are formed by Statistics Finland. Statistics Finland combines employment contract data from public and private employer-side organizations with information on the sector and industry of employment, as well as education of the worker to classify occupations and sign individuals with an occupation. Sector and industry classifications are always based on the (establishment of) employer. Both of these are based on the last employment of the year.

national Classification of Occupations 2001/ISCO-88) 7124, 7121, 71411, 7136, and 7137 respectively. We choose these five occupations as these are the most common occupations (cover about 85 percent of the construction sector occupations in the year 2000, see Figure A1 in Section 2.A).

We then follow these individuals (who worked in one of the five construction occupations in 2000) in *FLEED total* between 1998 and 2016. The reason why we fix everything to year 2000 is two-fold. First, we avoid any anticipatory movements of natives that might affect the composition of individuals in each occupation and region when we fix the year prior to the enlargement of the EU. Second, the year 2000 is chosen because we only have occupation codes for years 1995, 2000, and from 2004 onwards. In addition, the occupation codes from 2004 onwards are too crude to separate construction workers into sub-occupation categories (such as electricians from plumbers).

To construct treatment and control groups that would be as comparable as possible before the treatment, we include only those individuals who, in the year 2000, worked in one of the six biggest commuting zones (CZs): Helsinki, Turku, Tampere, Lahti, Jyväskylä, or Oulu.[4] Altogether, these commuting zones cover 64 municipalities out of 311, and about half of the working age population. In order to do this, we combine the data with the *FOLK employment* module to link individuals to the municipality of the establishment they work at on a yearly basis. Individuals without information on the municipality of work in they year 2000 are excluded, as this is crucial for our identification.

There are a couple of additional remarks worth pointing out. First, during our sample period, some municipalities underwent a merger. Our municipality classification follows that of the last year in our data set, i.e., 2016. In other words, we join these municipalities together beginning from the first year of our data. Second, the municipality composition of the commuting zones is not stable throughout our sample period. We fix them to the year 2000. However, some of the municipalities that merged during the sample period were originally part of another commuting zone. We take the merged municipality to belong to the commuting zone that is larger in terms of population.

Final Sample Our final sample consists of 30,013 individuals. We follow these individuals and their labor market outcomes for the years 1998-2016. Our panel is unbalanced. In the first year of our sample, the year 1998, we have 111 fewer individuals in our data than in 2000. In the year 2016, we lose 4,363 individuals. Some individuals drop out of our data if they have moved abroad or died during the sample

[4] Statistics Finland calls commuting zones travel-to-work areas. A cluster of municipalities is classified as a commuting zone if there is a central municipality into which at least 10 percent of the labor force commutes from the surrounding municipalities, and from which less than 25 percent of the labor force commutes elsewhere.

period. Also some of the younger individuals (in the year 2000) might be too young (and thus, outside of the labor force) to appear in the data before 2000.

Dependent Variables The administrative data provide us with an opportunity to study an extensive set of outcome variables. First, we look at two fundamental labor market outcomes: work-related *earnings* in 2015 euros, and *months of unemployment* (ranging from 0 to 12 months). In our data, we have missing earnings values for some individuals during our sample period. These reflect that the person had no work-related earnings, and we thus treat them as zero earnings.

Second, we study natives' labor market adjustment outcomes. *Switching to a non-construction sector* is a dummy variable that takes the value 1 if the person is working in a sector other than construction, otherwise 0. To construct this variable, we need to harmonize the industry coding, which changes twice (in 2002 and 2008) during our sample period. We harmonize these up to two-digit level using the keys provided by Statistics Finland. *Changing to another work place* is also a dummy variable that takes the value 1 if the person is working in another firm than in year 2000, 0 otherwise. *Moving* is a dummy variable that takes the value 1 if the person is working in another commuting zone than in year 2000. For all of these variables, a person with a missing establishment code (due to, for example, unemployment or retirement) is considered not to have changed establishments, industry, or moved, and is not part of the analysis of these outcomes.

Third and finally, we investigate retirement-related outcomes. These are full retirement, part-time retirement, and disability pension. All of these take the value 1 if the person is retired (fully, partially, or disabled), and 0 otherwise.

Data Limitations An obvious limitation of our study is that when we focus on individuals employed in the construction sector and with construction sector occupations in the year 2000, individuals outside employment are not taken into account. This means that individuals who are unemployed or still studying do not appear in our sample.[5]

Another limitation we face is that the municipality of the establishment might not be the true municipality in which the work is performed. Our data lack information on the actual construction sites that can change on a yearly, even monthly, or weekly basis. However, we believe that these sites are still likely to be within the same commuting zone.

[5]Unemployed individuals rarely have an occupation or industry code in our data.

2.B.2 Descriptive Statistics

Table B1 shows some descriptive statistics on the individuals in different occupations and regions in our sample in the year 2000. We divide our main sample into *treated* (vulnerable) and *control* (non-vulnerable) occupations and *treated* (high-exposure) and *control* (low-exposure) regions. The treated occupations are the vulnerable occupations in construction: builders, carpenters, plumbers, and painters. The control occupation is formed by electricians. The treated region is the Helsinki commuting zone, and the control region includes all other major commuting zones in Finland (Turku, Tampere, Lahti, Jyväskylä, and Oulu).

Table B1. Descriptive statistics of our sample in 2000.

	Helsinki CZ		Control CZs		
	Treated (1)	Control (2)	Treated (3)	Control (4)	Full sample (5)
Earnings	25.689	31.975	23.727	30.093	25.848
	(14.483)	(14.023)	(14.061)	(12.696)	(14.360)
Educated	0.639	0.836	0.711	0.880	0.707
	(0.480)	(0.370)	(0.453)	(0.325)	(0.455)
Married	0.447	0.474	0.498	0.548	0.479
	(0.497)	(0.499)	(0.500)	(0.498)	(0.500)
Average Age	40.080	39.307	40.015	40.375	40.010
	(10.680)	(10.490)	(10.637)	(10.688)	(10.649)
Below 30	0.183	0.193	0.191	0.179	0.187
	(0.387)	(0.395)	(0.393)	(0.383)	(0.390)
Between 30 and 50	0.617	0.640	0.615	0.619	0.619
	(0.486)	(0.480)	(0.487)	(0.486)	(0.486)
Above 50	0.200	0.167	0.194	0.203	0.195
	(0.400)	(0.373)	(0.395)	(0.402)	(0.396)
In sample in 2016	0.845	0.879	0.858	0.863	0.855
	(0.362)	(0.326)	(0.349)	(0.343)	(0.352)
N	12,528	2,700	12,142	2,643	30,013

Notes: The table shows averages within exposed and non-exposed regions and vulnerable ("treated") and non-vulnerable ("control") occupations in 2000. Earnings refers to average annual work-related earnings measured in thousands of 2015 euros. Educated refers to the share with at least a secondary-level education. In sample in 2016 refers to the share of individuals still in our sample in 2016. Standard errors clustered at the municipality level are reported in parentheses.

2.B.3 Additional Differences in Means Results

Table B2 shows the differences in means for the adjustment mechanisms. We show that the EU enlargement had (i) a small positive effect on moving to a non-construction sector occupation ($DDD = 0.004$); (ii) a small positive effect on starting to work at another establishment ($DDD = 0.033$); (iii) a small positive effect on moving to work in another commuting zone ($DDD = 0.035$); (iv) a small positive effect on retiring ($DDD = 0.018$); (v) a negative effect on part-time retirement ($DDD = -0.006$); and (vi) no effect on being on disability pension ($DDD = 0.000$). This triple-difference is statistically significant with $p < 0.01$ for months in part-time retirement. Other differences are not statistically significant at any conventional level.

Table B2. Effects of EU enlargement on adjustment mechanisms: differences in means

	Helsinki CZ			Control CZs		
	Vulnerable	Control	Difference	Vulnerable	Control	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-construction						
Before	0.142	0.093	0.049***	0.144	0.106	0.038***
	(0.349)	(0.291)	(0.008)	(0.351)	(0.307)	(0.009)
	[74,809]	[16,154]	[90,963]	[72,543]	[15,823]	[88,366]
After	0.242	0.187	0.055***	0.223	0.183	0.040***
	(0.428)	(0.390)	(0.008)	(0.417)	(0.387)	(0.010)
	[151,752]	[33,352]	[185,104]	[148,540]	[32,518]	[181,058]
Difference	0.100***	*DD*	0.094***	0.079***	0.077***	*DD*
	(0.003)	(0.005)	0.006	(0.004)	(0.013)	0.002
	[226,561]	[49,506]	(0.007)	[221,083]	[48,341]	(0.012)
			DDD = 0.004 (0.014)			
Panel B: Job change						
Before	0.274	0.186	0.088***	0.237	0.156	0.080***
	(0.446)	(0.389)	(0.010)	(0.425)	(0.363)	(0.027)
	[74,809]	[16,154]	[90,963]	[72,543]	[15,823]	[88,366]
After	0.747	0.588	0.160***	0.708	0.589	0.119**
	(0.435)	(0.492)	(0.020)	(0.455)	(0.492)	(0.045)
	[151,752]	[33,352]	[185,104]	[148,540]	[32,518]	[181,058]
Difference	0.473***	0.402***	*DD*	0.471***	0.433***	*DD*
	(0.004)	(0.013)	0.071***	(0.011)	(0.025)	0.039*
Observations	[226,561]	[49,506]	(0.014)	[221,083]	[48,341]	(0.023)
			DDD = 0.033 (0.027)			

Table continued on the following page.

Panel C: Move						
Before	0.058	0.027	0.031***	0.065	0.073	-0.009
	(0.234)	(0.163)	(0.005)	(0.246)	(0.261)	(0.033)
	[74,809]	[16,154]	[90,963]	[72,543]	[15,823]	[88,366]
After	0.155	0.113	0.042*	0.147	0.180	-0.033
	(0.361)	(0.316)	(0.021)	(0.354)	(0.384)	(0.063)
	[151,752]	[33,352]	[185,104]	[148,540]	[32,518]	[181,058]
Difference	0.096***	0.085***	*DD*	0.082***	0.107***	*DD*
	(0.006)	(0.017)	0.011	(0.006)	(0.030)	-0.024
	[226,561]	[49,506]	(0.017)	[221,083]	[48,341]	(0.031)

DDD = 0.035 (0.035)

Panel D: Retirement						
Before	0.014	0.009	0.005***	0.013	0.012	0.000
	(0.119)	(0.096)	(0.001)	(0.112)	(0.111)	(0.002)
	[74,809]	[16,154]	[90,963]	[72,543]	[15,823]	[88,366]
After	0.153	0.122	0.031***	0.151	0.143	0.008
	(0.360)	(0.327)	(0.008)	(0.358)	(0.350)	(0.009)
	[151,752]	[33,352]	[185,104]	[148,540]	[32,518]	[181,058]
Difference	0.138***	0.112***	*DD*	0.138***	0.131***	*DD*
	(0.003)	(0.008)	0.026***	(0.002)	(0.008)	0.008
	[226,561]	[49,506]	(0.008)	[221,083]	[48,341]	(0.008)

DDD = 0.018 (0.012)

Table continued on the following page.

Panel E: Part-time retirement						
Before	0.004	0.003	0.001	0.004	0.005	-0.001
	(0.064)	(0.053)	(0.001)	(0.062)	(0.069)	(0.001)
	[74,809]	[16,154]	[90,963]	[72,543]	[15,823]	[88,366]
After	0.004	0.007	-0.003***	0.006	0.005	0.000
	(0.060)	(0.083)	(0.001)	(0.074)	(0.071)	(0.001)
	[151,752]	[33,352]	[185,104]	[148,540]	[32,518]	[181,058]
Difference	-0.000	0.004***	*DD*	0.002***	0.000	*DD*
	(0.000)	(0.001)	-0.005***	(0.000)	(0.001)	0.001
	[226,561]	[49,506]	(0.001)	[221,083]	[48,341]	(0.001)
			DDD = -0.006*** (0.002)			
Panel F: Disability pension						
Before	0.011	0.007	0.005***	0.010	0.009	0.001
	(0.106)	(0.081)	(0.001)	(0.100)	(0.095)	(0.001)
	[74,809]	[16,154]	[90,963]	[72,543]	[15,823]	[88,366]
After	0.066	0.050	0.016***	0.067	0.055	0.012***
	(0.248)	(0.218)	(0.004)	(0.250)	(0.228)	(0.003)
	[151,752]	[33,352]	[185,104]	[148,540]	[32,518]	[181,058]
Difference	0.054***	0.043***	*DD*	0.057***	0.046***	*DD*
	(0.002)	(0.003)	0.011***	(0.003)	(0.003)	0.011***
	[226,561]	[49,506]	(0.004)	[221,083]	[48,341]	(0.002)
			DDD = 0.000 (0.004)			

Notes: The table reports region- and occupation-level averages before and after the enlargement of the EU in 2004. Vulnerable occupations are builders, painters, carpenters, and plumbers. Control refers to electricians. Non-construction refers to working in a sector other than construction. Job change refers to a change in establishment. Moved refers to working in another commuting zone. Standard errors clustered at the municipality level and reported in parentheses. Number of observations is shown in brackets.
* p<0.10, ** p<0.05, *** p<0.01

2.C Additional Triple-Differences Results

2.C.1 Occupation-Specific DDD Results

Our main estimation pools together all the vulnerable occupations. In this appendix, we re-estimate our triple-differences specification for each occupation separately. Tables C1-C3 report results from a parametric specification, and Figures C1-C4 plot coefficients (and respective confidence intervals) obtained using an event study specification. We conclude that there are no major differences across the occupations. For example, the effects on earnings are always negative and statistically significant, within the ballpark of 1,000-2,500 euros (results in column 1 of each table). We also see increases in unemployment, especially in the longer run (results in column 2 of each table).

2.C.2 Event-Study Results by Age Group

In the main text, we reported age-group-specific regressions results using a parametric triple-differences specification. Here we verify that the results from a non-parametric event-study specification are similar. Figure C5 first shows the regression results for adjustment mechanisms for three age groups. Second, C6 focuses on old workers and their retirement outcomes. The patterns in these figures are in line with the parametric results we show in the main text.

2.C.3 Results with Region-Occupation Fixed Effects

We have also estimated an alternative model in which we net out the occupation-commuting zone fixed effects rather than individual fixed effects. This specification focuses on what happened to the vulnerable occupations after the enlargement of the EU rather than individuals in vulnerable occupations. However, this is just an approximation since we are unable to take new entrants into account. The conclusions regarding the effects of EU enlargement remain qualitatively unchanged, as we see in the parametric results reported in Table C5 and non-parametric event study graphs in Figure C7.

Table C1. The effects of the eastern enlargement of the EU on plumbers using the pooled sample.

	Earnings (1)	Months of unemployment (2)	Non-construction (3)	Job change (4)	Moved (5)	Retired (6)	Part-time retirement (7)	Disability pension (8)
Transition period	-0.984	0.055	-0.002	0.044	0.028	0.011	-0.005**	0.001
	(0.607)	(0.078)	(0.025)	(0.041)	(0.031)	(0.009)	(0.002)	(0.010)
N	89,219	89,219	81,241	76,980	79,129	89,219	89,219	89,219
Until 2010	-1.164*	0.104	-0.012	0.057	0.035	0.014	-0.006***	0.002
	(0.678)	(0.063)	(0.026)	(0.039)	(0.033)	(0.012)	(0.002)	(0.009)
N	127,629	127,629	111,117	106,598	109,010	127,629	127,629	127,629
Until 2016	-1.418*	0.165**	-0.004	0.052	0.036	0.017	-0.004**	-0.003
	(0.835)	(0.066)	(0.027)	(0.034)	(0.032)	(0.017)	(0.002)	(0.007)
N	181,313	181,313	147,674	142,729	145,566	181,313	181,313	181,313

Notes: Earnings refers to annual work-related earnings measured in 2015 euros. Non-construction refers to working in a sector other than construction. Job change refers to change in establishment. Moved refers to a change in commuting zone of work. Standard errors clustered at the municipality level are reported in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

Table C2. The effects of the Eastern enlargement of the EU on painters using the pooled sample.

	Earnings (1)	Months of unemployment (2)	Non-construction (3)	Job change (4)	Moved (5)	Retired (6)	Part-time retirement (7)	Disability pension (8)
Transition period	-1.275*	0.034	-0.009	0.040	0.037	0.017	-0.012***	0.003
	(0.670)	(0.079)	(0.023)	(0.042)	(0.033)	(0.015)	(0.003)	(0.008)
N	74,825	74,825	66,471	60,583	64,141	74,825	74,825	74,825
Until 2010	-2.139***	0.132**	-0.003	0.059	0.041	0.025	-0.013***	0.001
	(0.716)	(0.065)	(0.023)	(0.037)	(0.035)	(0.016)	(0.002)	(0.008)
N	107,038	107,038	90,479	84,326	88,160	107,038	107,038	107,038
Until 2016	-2.485**	0.146**	0.001	0.065*	0.048	0.026	-0.009***	-0.001
	(0.967)	(0.063)	(0.024)	(0.035)	(0.034)	(0.019)	(0.002)	(0.007)
N	152,210	152,210	119,926	113,299	117,610	152,210	152,210	152,210

Notes: Earnings refers to annual work-related earnings measured in 2015 euros. Non-construction refers to working in a sector other than construction. Job change refers to change in establishment. Moved refers to a change in commuting zone of work. Standard errors clustered at the municipality level are reported in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

Table C3. The effects of the Eastern enlargement of the EU on carpenters using the pooled sample.

	Earnings	Months of unemployment	Non-construction	Job change	Moved	Retired	Part-time retirement	Disability pension
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Transition period	-1.528**	0.096	-0.002	-0.003	0.029	0.018**	-0.005***	0.008
	(0.590)	(0.060)	(0.011)	(0.040)	(0.032)	(0.008)	(0.002)	(0.006)
N	143,891	143,891	129,229	110,043	125,061	143,891	143,891	143,891
Until 2010	-1.780***	0.112**	-0.007	0.005	0.037	0.022**	-0.006***	0.006
	(0.655)	(0.051)	(0.009)	(0.035)	(0.033)	(0.009)	(0.002)	(0.006)
N	206,220	206,220	175,761	155,749	171,607	206,220	206,220	206,220
Until 2016	-1.515*	0.107*	-0.005	-0.001	0.037	0.023**	-0.006**	0.001
	(0.818)	(0.056)	(0.011)	(0.031)	(0.032)	(0.012)	(0.002)	(0.006)
N	294,105	294,105	232,159	210,623	228,007	294,105	294,105	294,105

Notes: Earnings refers to annual work-related earnings measured in 2015 euros. Non-construction refers to working in a sector other than construction. Job change refers to change in establishment. Moved refers to a change in commuting zone of work. Standard errors clustered at the municipality level are reported in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

Table C4. The effects of EU enlargement on builders using the pooled sample.

	Earnings	Months of unemployment	Non-construction	Job change	Moved	Retired	Part-time retirement	Disability pension
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Transition period	-1.398***	0.134**	-0.002	0.019	0.009	0.017*	-0.005***	0.009
	(0.518)	(0.057)	(0.011)	(0.032)	(0.030)	(0.009)	(0.002)	(0.007)
N	103,342	103,342	90,544	83,079	86,205	103,342	103,342	103,342
Until 2010	-1.790***	0.185***	0.001	0.031	0.015	0.022**	-0.005***	0.006
	(0.667)	(0.062)	(0.013)	(0.029)	(0.030)	(0.009)	(0.002)	(0.007)
N	148,039	148,039	123,409	115,620	119,102	148,039	148,039	148,039
Until 2016	-2.003**	0.231***	0.011	0.026	0.018	0.030**	-0.004**	0.003
	(0.877)	(0.060)	(0.014)	(0.024)	(0.030)	(0.013)	(0.002)	(0.006)
N	211,404	211,404	163,893	155,546	159,595	211,404	211,404	211,404

Notes: Earnings refers to annual work-related earnings measured in 2015 euros. Non-construction refers to working in a sector other than construction. Job change refers to change in establishment. Moved refers to a change in commuting zone of work. Standard errors clustered at the municipality level are reported in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

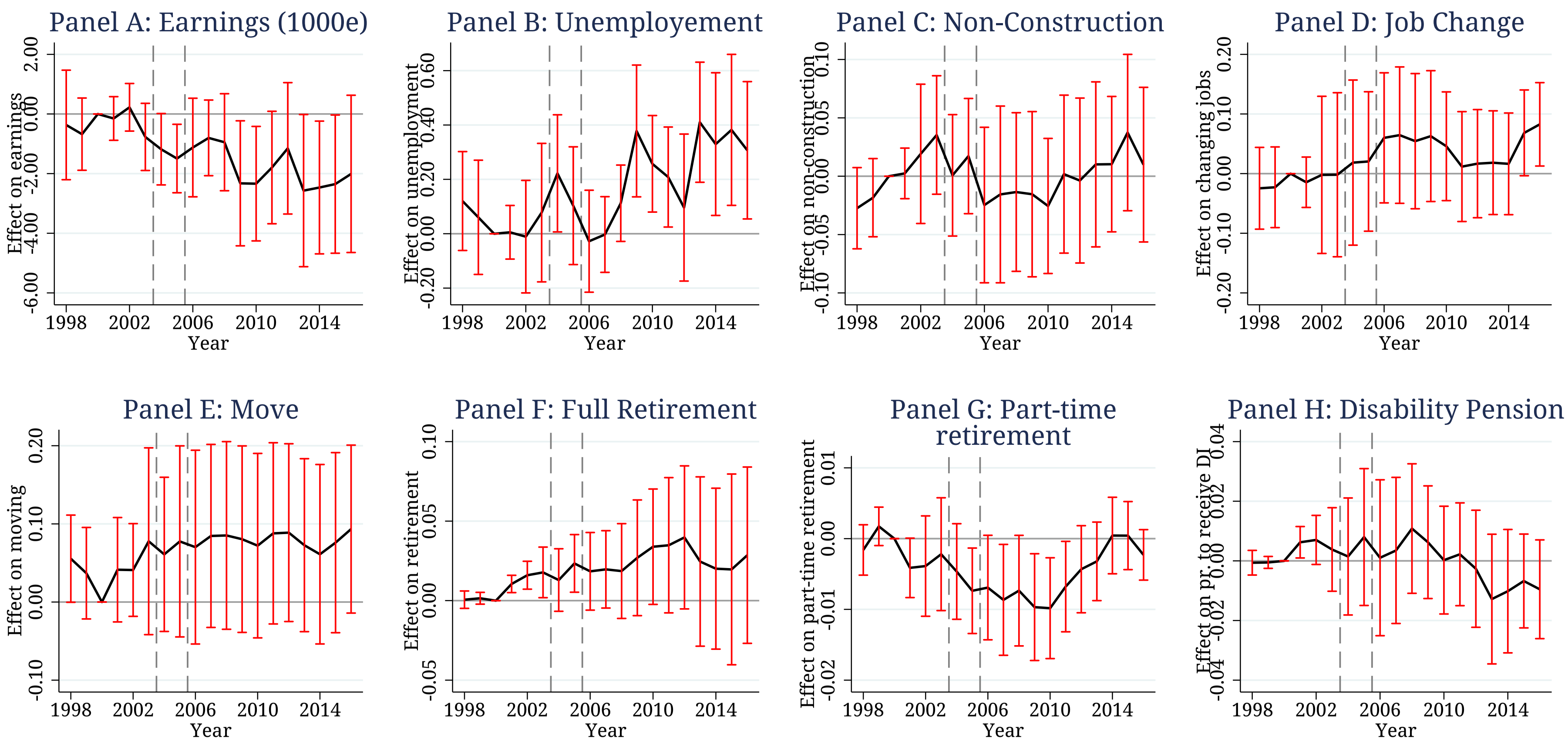


Figure C1. The effects of EU enlargement on plumbers: event study specification.

Notes: Figure shows point estimates and their 95% confidence intervals constructed using standard errors clustered at the municipality level. Gray dashed lines mark the transition period.

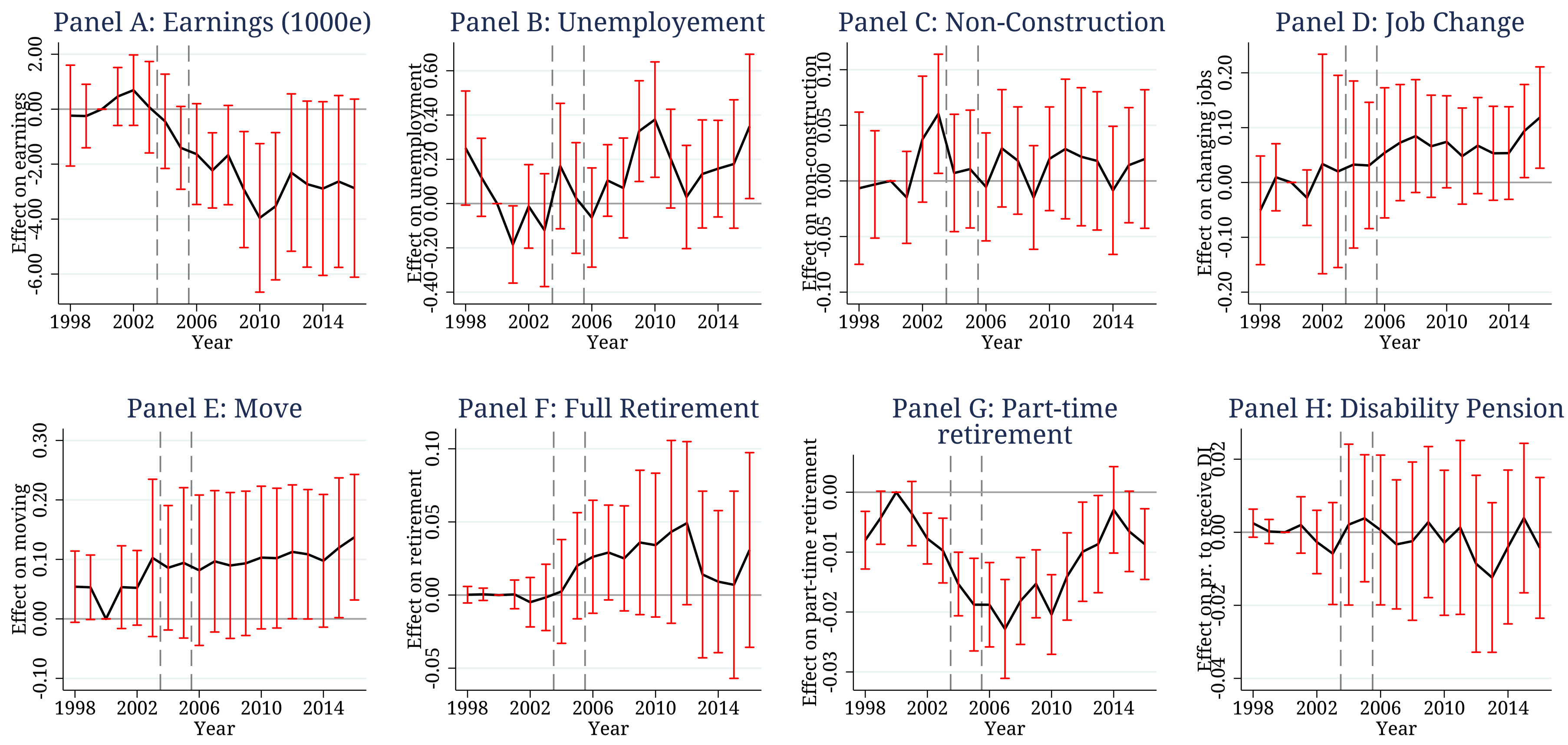


Figure C2. The effects of EU enlargement on painters: event study specification.

Notes: Figure shows point estimates and their 95% confidence intervals constructed using standard errors clustered at the municipality level. Gray dashed lines mark the transition period.

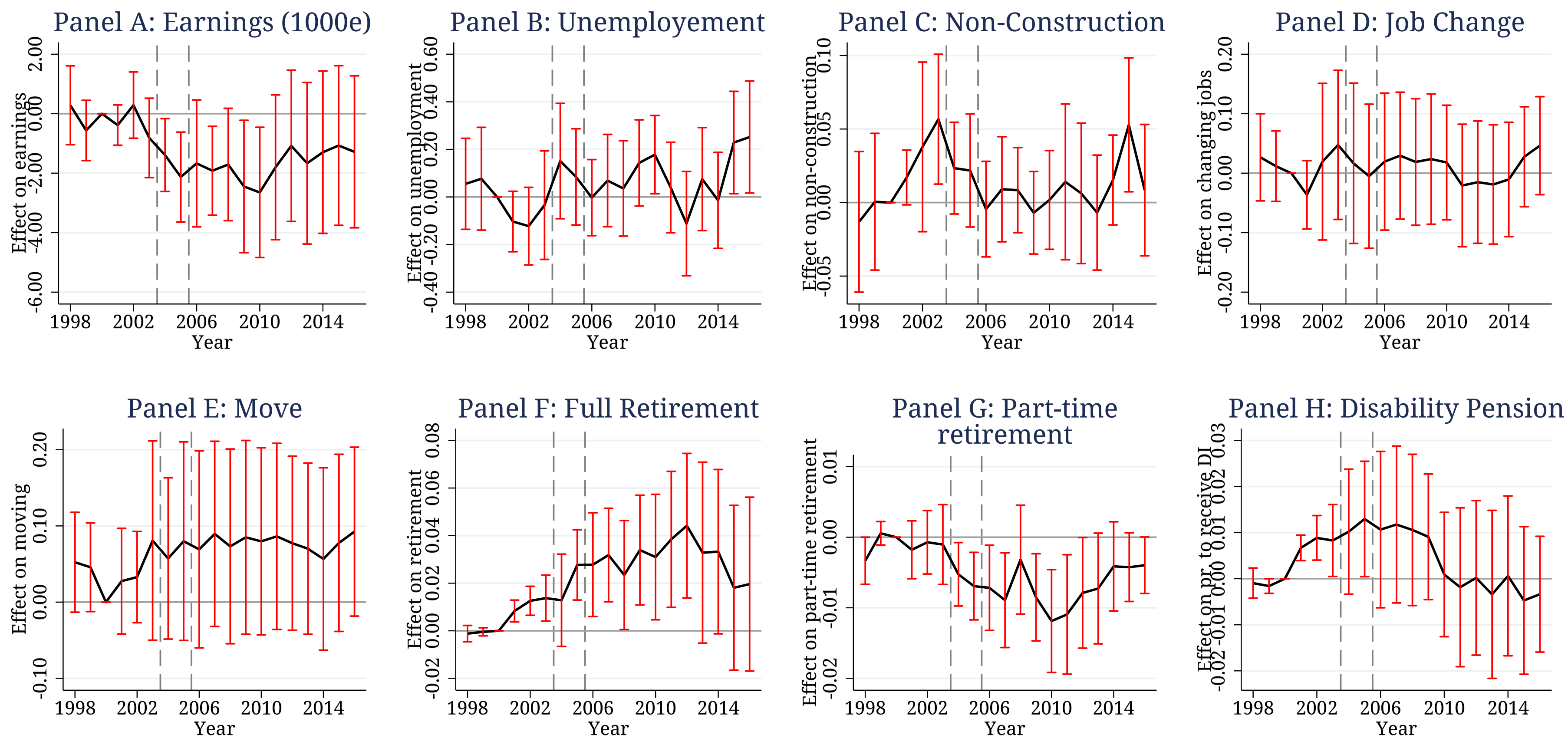


Figure C3. The effects of EU enlargement on carpenters: event study specification.

Notes: Figure shows point estimates and their 95% confidence intervals constructed using standard errors clustered at the municipality level. Gray dashed lines mark the transition period.

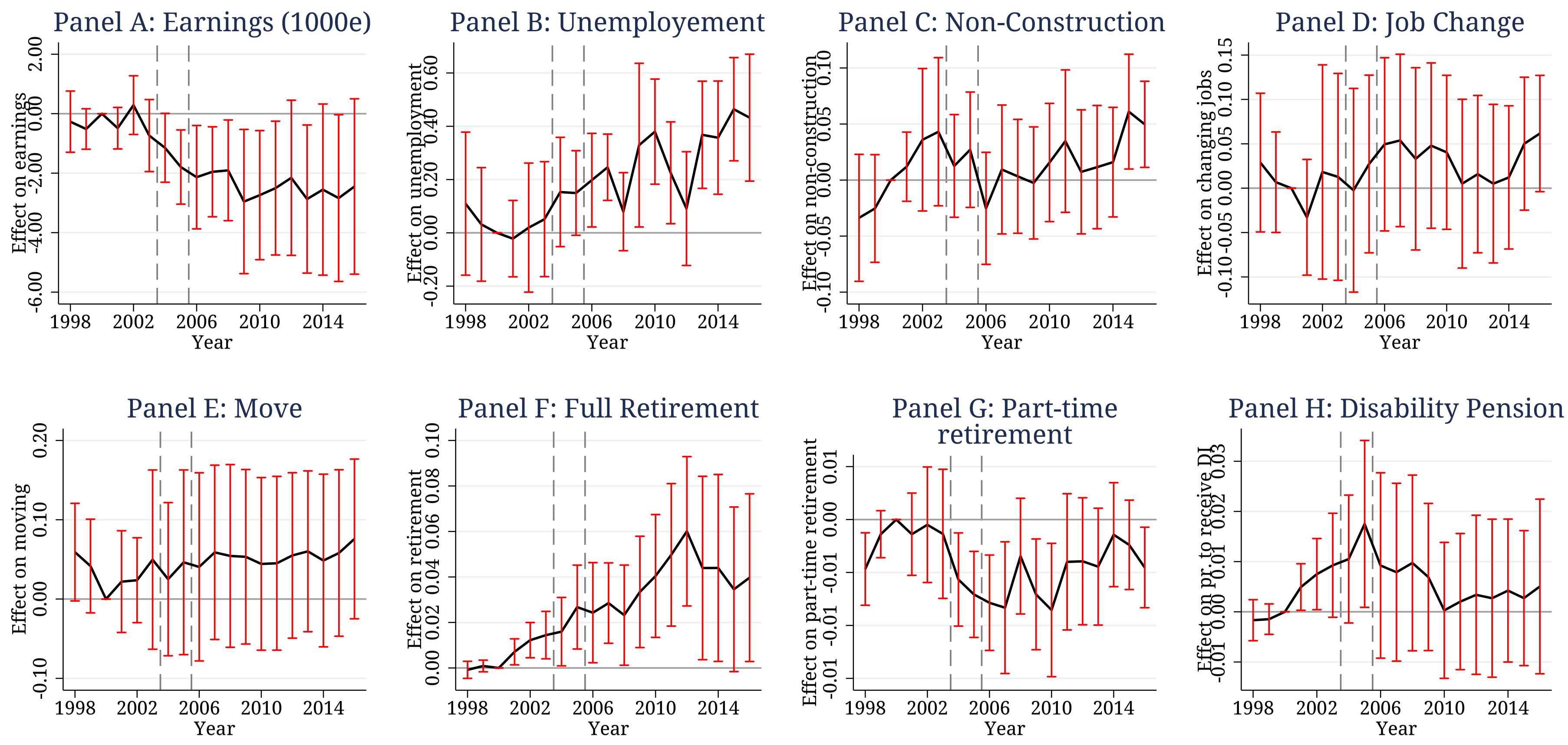


Figure C4. The effects of EU enlargement on builders: event study specification.

Notes: Figure shows point estimates and their 95% confidence intervals constructed using standard errors clustered at the municipality level. Gray dashed lines mark the transition period.

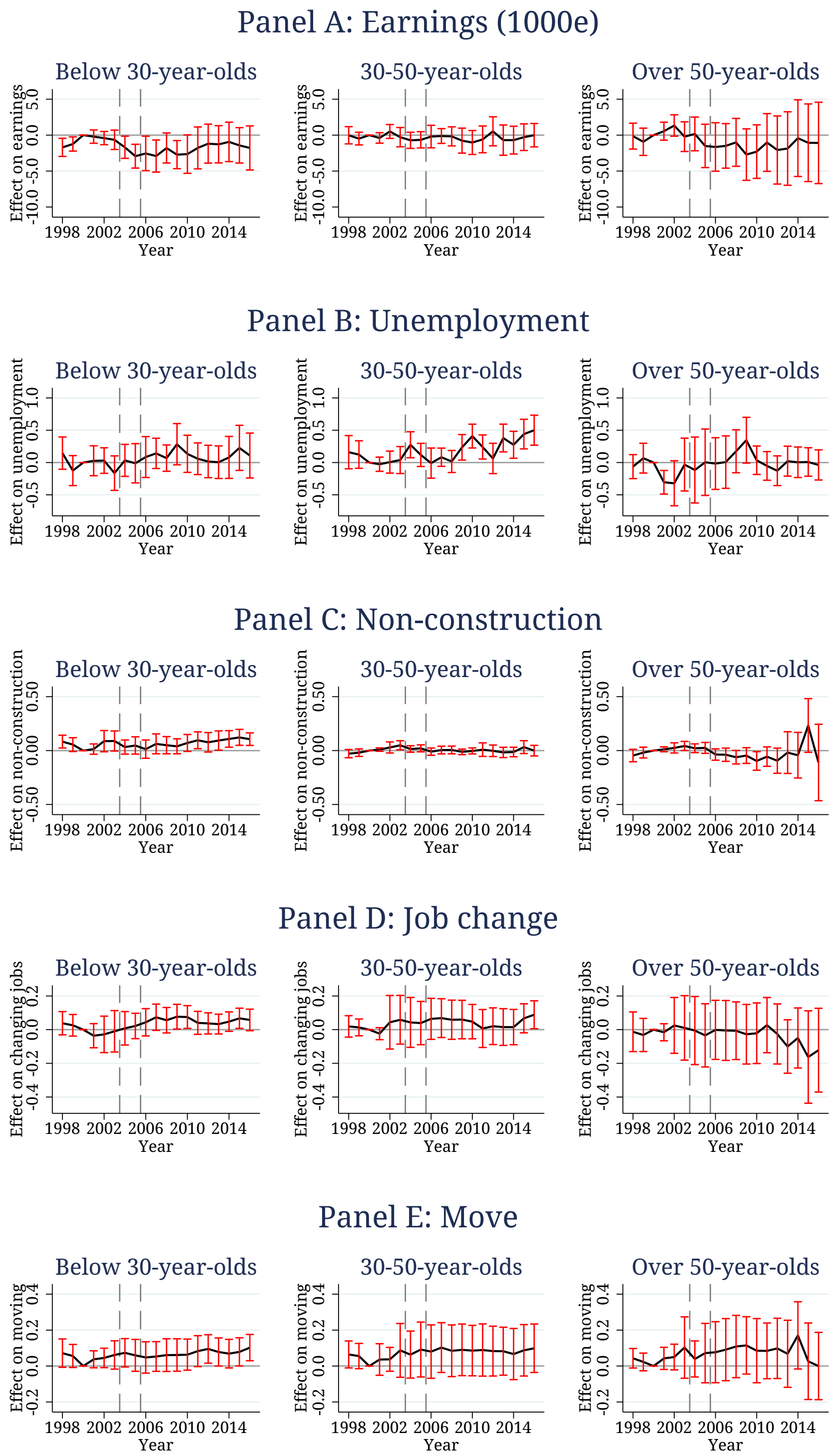


Figure C5. Heterogeneous effects by age group: event study specification.

Notes: Figure shows point estimates and their 95% confidence intervals constructed using standard errors clustered at the municipality level. Gray dashed lines mark the transition period.

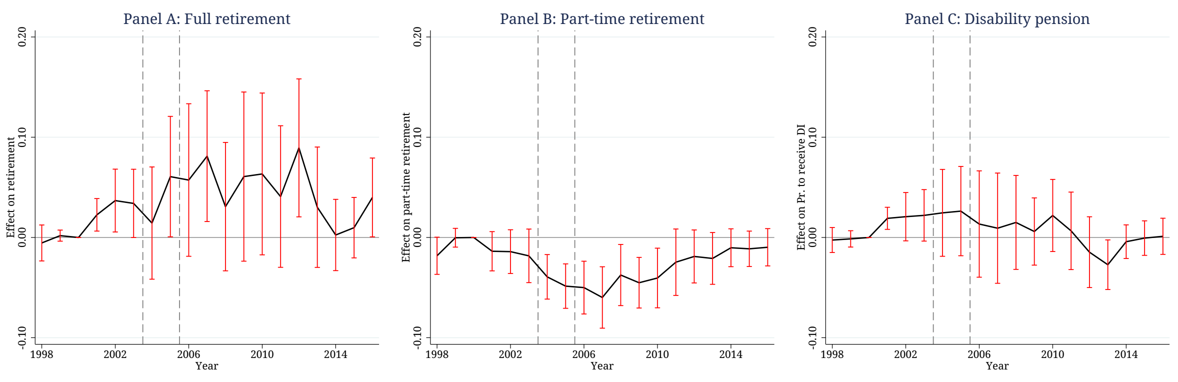


Figure C6. Effects of EU enlargement on old workers' retirement decisions: event study specification.

Notes: Figure shows point estimates and their 95% confidence intervals constructed using standard errors clustered at the municipality level. Gray dashed lines mark the transition period.

Table C5. The effects EU enlargement controlling for region-occupation fixed effects.

	Earnings (1)	Months of unemployment (2)	Non-cons-truction (3)	Job change (4)	Moved (5)	Retired (6)	Part-time retirement (7)	Disability pension (8)
Transition period	-1.276** (0.551)	0.087* (0.052)	-0.003 (0.012)	0.024 (0.036)	0.027 (0.034)	0.015* (0.009)	-0.006*** (0.001)	0.006 (0.007)
N	267,868	267,868	236,564	206,687	227,239	267,868	267,868	267,868
Until 2010	-1.599** (0.641)	0.117*** (0.039)	-0.005 (0.011)	0.036 (0.030)	0.037 (0.034)	0.020** (0.010)	-0.007*** (0.002)	0.005 (0.006)
N	383,363	383,363	320,397	288,762	311,072	383,363	383,363	383,363
Until 2016	-1.502* (0.776)	0.128*** (0.046)	0.003 (0.013)	0.031 (0.025)	0.038 (0.032)	0.020* (0.012)	-0.006*** (0.002)	0.001 (0.004)
N	545,491	545,491	421,738	387,664	412,411	545,491	545,491	545,491

Notes: Earnings refers to annual work-related earnings measured in 2015 euros. Non-construction refers to working in a sector other than construction. Job change refers to change in establishment. Moved refers to a change in commuting zone of work. Standard errors clustered at the municipality level are reported in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

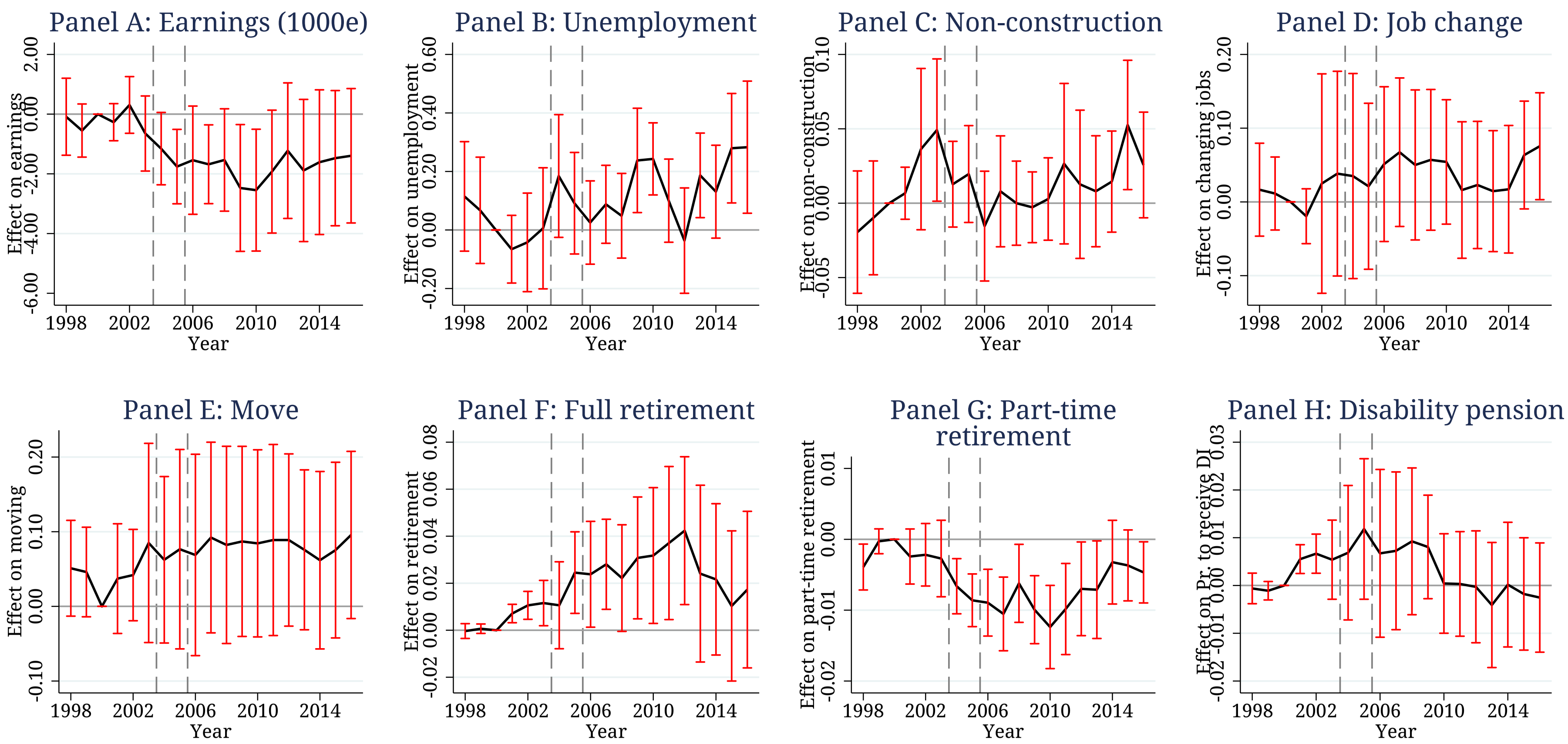


Figure C7. Effects of EU enlargement: event study specification controlling for occupation-region fixed effects.

Notes: Figure shows point estimates and their 95% confidence intervals constructed using standard errors clustered at the municipality level. Gray dashed lines mark the transition period.

2.C.4 Alternative Outcomes

In this section, we asses the robustness of our results to using alternative outcomes. First, we rerun our triple differences analyses using the logarithm of income. Given that there are many individuals with no earnings at all, we also use the logarithm of earnings plus one. The effects on these dependent variables are illustrated in Panels A and B of Figure C8. The pattern that we can see in the event-study graph supports our conclusion regarding a negative impact of EU enlargement on exposed individuals relative to non-exposed individuals. Columns (1) and (2) in Table C6 report the parametric DDD estimates for three different time periods, again supporting the arguments we lay in the main text.

We then investigate three outcomes that complement our analyses on unemployment. Panel A of Figure C9 shows that there appears to be a negative impact on being primarily employed during the year. The effect grows larger during the transition period, after which it remains relatively stable. This is further supported by the parametric estimates that we present in column (3) of Table C6. However, Panel B of Figure C9 and column (4) of Table C6 suggest that there is no statistically or economically significant impact on being primarily unemployed during the year. Lastly, Panel C of Figure C9 shows that there is a small positive effect on having at least a one month of unemployment during the year. This should, however, be interpreted with caution, as the pre-trends are somewhat unstable.

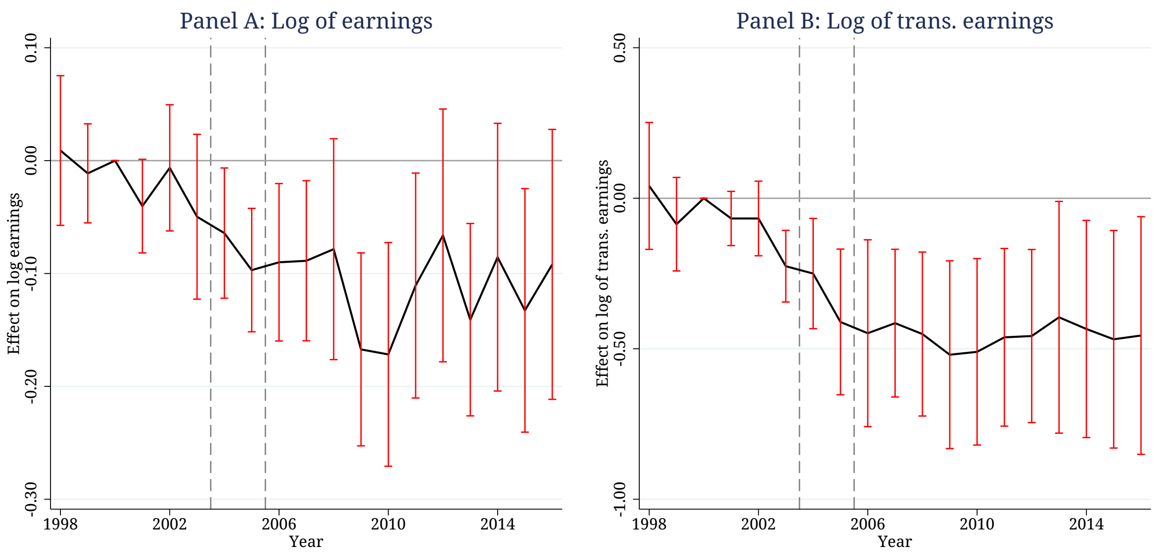


Figure C8. Effects of EU enlargement on alternative earnings outcomes.

Notes: Trans. earnings refers to earnings plus one. Figure shows point estimates and their 95% confidence intervals constructed using standard errors clustered at the municipality level. Gray dashed lines mark the transition period.

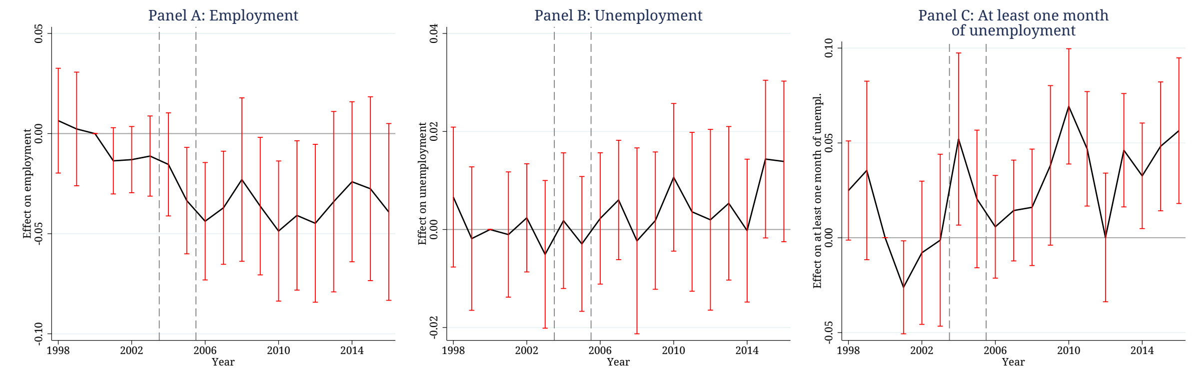


Figure C9. Effects of EU enlargement on alternative employment outcomes.

Notes: Figure shows point estimates and their 95% confidence intervals constructed using standard errors clustered at the municipality level. Gray dashed lines mark the transition period.

Table C6. The effects EU enlargement on alternative earnings and employment outcomes.

	Log earnings	Log transformed earnings	Employed	Unemployed	At least one month of unemployment
	(1)	(2)	(3)	(4)	(5)
Transition period	-0.063***	-0.303**	-0.026**	0.000	0.022**
	(0.020)	(0.117)	(0.012)	(0.003)	(0.010)
N	231,053	267,868	267,868	267,868	267,868
Until 2010	-0.086***	-0.361***	-0.029*	0.002	0.026***
	(0.026)	(0.121)	(0.015)	(0.004)	(0.007)
N	318,414	383,363	383,363	383,363	383,363
Until 2016	-0.088***	-0.368***	-0.030*	0.004	0.030***
	(0.032)	(0.138)	(0.017)	(0.004)	(0.009)
N	428,448	545,491	545,491	545,491	545,491

Notes: Log earnings refers to the logarithm of annual work-related earnings measured in 2015 euros. Log transformed earnings refers to the logarithm of annual earnings in which a 1 is added to the earnings to avoid zeros. Employment and unemployment refer to the probabilities to be mainly employed and unemployed during the year, respectively. At least one month of unemployment refers to being unemployed at least a month during the year. Standard errors clustered at the municipality level are reported in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

2.C.5 Leave-One-Out Estimates

We have also tested whether our main results are sensitive to the set of municipalities included in the sample. To illustrate the robustness of our estimates, Figure C10 plots all leave-one-out triple-difference estimates for the effect of EU enlargement on annual earnings, our main outcome.

The distribution of the estimates is fairly uniform. However, we have highlighted the left-out municipalities that either make our estimate statistically insignificant or more negative for the sake of transparency. Estimates in which the confidence interval includes zero are mostly large municipalities from the control commuting zones. Excluding one of these will decrease the number of individuals in the control sample disproportionately. Furthermore, excluding Turku, the largest commuting zone in the control group, makes the estimate more negative. This could at least in theory highlight a potential limitation of our identification strategy and the fact that we only identify the lower bound of the effect. It is possible that some of the control regions were also affected by the enlargement of the EU. Since Turku is fairly close to Helsinki and also a large city, it is plausible that some of the cross-border workers could have worked there.

Finally, excluding the capital city Helsinki from the analysis decreases the point estimate as well. This may reflect the fact that leaving out Helsinki changes our estimation sample considerably: we lose about half of the treated individuals.

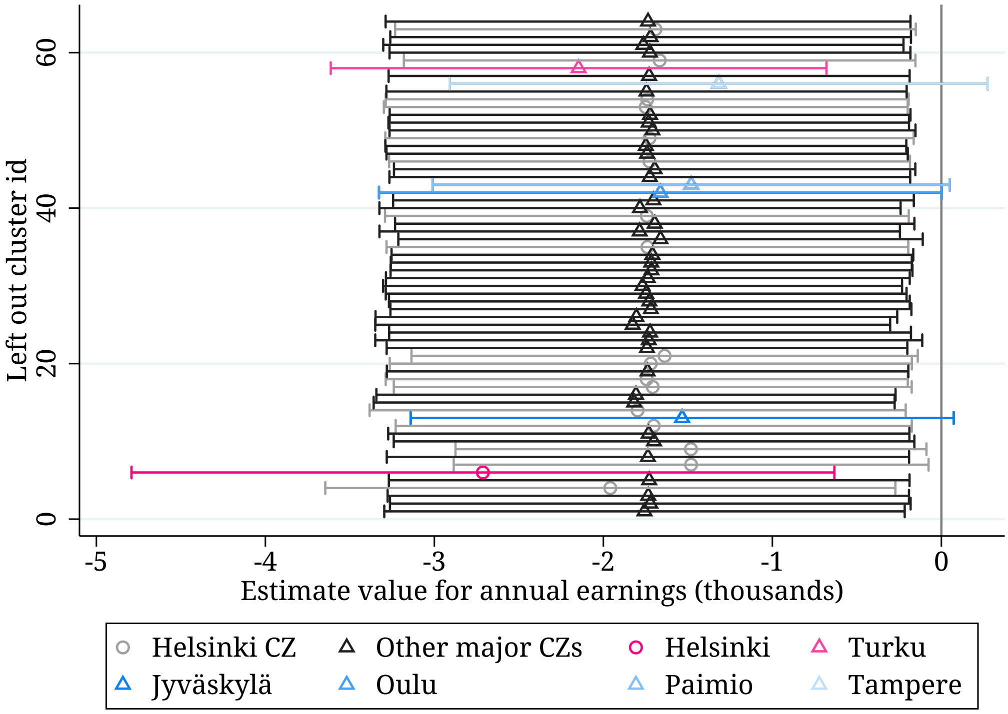


Figure C10. Effects of EU enlargement on alternative employment outcomes.

Notes: Figure shows point estimates and their 95% confidence intervals constructed using standard errors clustered at the municipality level. Gray dashed lines mark the transition period.

2.D Back-of-the-Envelope Calculations

In this appendix, we present naïve back-of-the-envelope calculations to scale our main estimates for earnings. Table D1 lists all our sources for estimated numbers of unaccounted foreign workers (i.e. posted workers), the implied effect sizes, and corresponding elasticities. Given the enormous variation in the estimates of the number of unaccounted workers, these implied effect sizes and elasticities for annual earnings also exhibit great variation. For the transition period effect, the implied effect is between 123 and 743 euros per 1,000 foreign workers; 131 euros until the year 2010; and between 159 and 400 euros until the year 2016. There is also quite a lot of variation in the implied elasticities for annual earnings ($\partial W/\partial(M/N)$), from -0.530 to -0.038. However, most of these estimations suggest that the elasticity for annual earnings was close to zero.

Focusing on building workers, we know that the number of registered A8 workers in the Helsinki commuting zone grew from about 2.8 percent (571) in 2004 to about 15.2 percent (3,792) in 2016—whereas in 2000 less than 1 percent of the workers in the construction occupations were from the A8.[6] Furthermore, we can say something about the number of posted and other unregistered workers based on estimates from other sources.

First, authorities and experts in Finland and Estonia have estimated that about 10,000-20,000 Estonians travel annually to Finland for work. We do not know exactly how many work in the construction sector but it is likely to be a very common destination. The administrative registries suggest that around one third of the A8 residents in Finland worked in the construction sector. We base the lower bound of our calculations on the assumption that the share is the same among those who came to work in Finland without being registered. Second, the Tallinn ferry passengers numbers from the Border Interview Surveys can be used to estimate the number of frequent commuters. Assuming that most of the passengers who reported their trip's purpose as work-related traveled to Finland biweekly, the number of commuters is about 5,600. Again, not all of these commuters came to work in the construction sector—our lower bound calculations assume that a third did. Our upper bound estimates assume that most construction work is done during the summer months (within six months) and all workers came to work in construction. The upper bound number of construction sector commuters is thus about 8,100. Third, the unaccounted person-years in the construction sector suggest that during the transition period, the number of foreign unaccounted workers increased from 16,000 to 30,000. We assume that most of them worked in the Helsinki commuting zone. Fourth, based on the two biggest mobile operator providers in Estonia, 12,698 Estonians traveled to Finland frequently in 2015 (Ahas et al. 2017).[7] On average, these Estonians, presumably cross-border workers, stayed in Finland over 160 days. We thus assume that about 5,600 person-years come to Finland and most likely to the Helsinki commuting zone on an annual basis. The lower bound of our estimates is again based on the guess that one third of cross-border workers work in the construction sector.

[6]We focus on construction sector occupations with code 71 in 2-digit ISCO-08 classification and 7411 in 4-digit ISCO-08. The occupations in our main estimation sample cover 85 percent of these occupations. In total, these building workers cover about 50 percent of the workforce in the entire construction sector. The Helsinki commuting zone is defined in the year 2000.

[7]This data is based on mobile phone locations of two big mobile phone operators in Estonia (Ahas et al. 2017). Obviously, some cross-border workers from Estonia might use another mobile phone provider from Estonia, turn off their phone while in Finland, or use a Finnish mobile phone provider.

Table D1. Back-of-the-envelope calculations.

Period	Source	Lower bound	Upper bound	Implied share of A8	Implied effect	Implied elasticity
Transition period	Border Interviews	1,353	8,115	9% to 30%	215 to 743 euros	-0.530 to -0.117
Transition period	Unaccounted person-years	16,091	21,877	45% to 53%	123 to 143 euros	-0.061 to -0.045
Until 2010	Unaccounted person-years		30,147	63%	131 euros	-0.038
Until 2016	Mobile phone operators	1,855	5,566	21% to 31%	275 to 400 euros	-0.238 to -0.144
Until 2016	Official estimates	3,333	20,000	25% to 53%	159 to 334 euros	-0.188 to -0.056

Notes: The table reports back-of-the envelope calculations for the effect of 1,000 additional foreigners on annual earnings and implied elasticity of annual earnings with respect to the increase in labor supply induced by workers from the A8 countries. Border Interviews refers to the number of Tallinn ferry passengers traveling to Finland for work. Mobile phone operators refers to the number of frequent cross-border workers from Estonia based on mobile phone location data reported by Ahas et al. (2017). Official estimates refers to estimates calculated by Finnish and Estonian authorities. Lower and upper bound refer to the estimated number of unaccounted foreign labor. The calculations for the implied shares, effects, and elasticities additionally use the information in our administrative data about the number of all workers, registered A8 and natives working in the construction sector occupations in Helsinki commuting zone in 2006, 2010, and 2016. At the end of the transition period (in 2006), there are in total 20,588 workers in these occupations of which 571 are registered A8 workers and 19,505 natives. The corresponding numbers for the time periods until 2010 and until 2016 are 20,251 in total, 1,498 A8 and 18,032 natives, and 25,021 in total, 3,792 A8 and 19,943 natives, respectively.

2.E Additional Results for Electricians

In the main text, we report the parametric regression results studying the complementarity of (native) electricians. This appendix contains further results on the effects of EU enlargement on them. First, Figure E1 plots the year-specific point estimates from an event study specification. They are largely in line with our parametric estimates.

We also study heterogeneous effects by age group. Table E1 reports point estimates from a parametric difference-in-differences specification. The DD estimates for our main outcomes seem to be driven mainly by younger workers, for whom we find a positive and significant effect on earnings. Figure E2 plots the heterogeneous effects by age group obtained using the event study specification, and Figure E3 shows the event-study specification results for old workers' retirement outcomes.

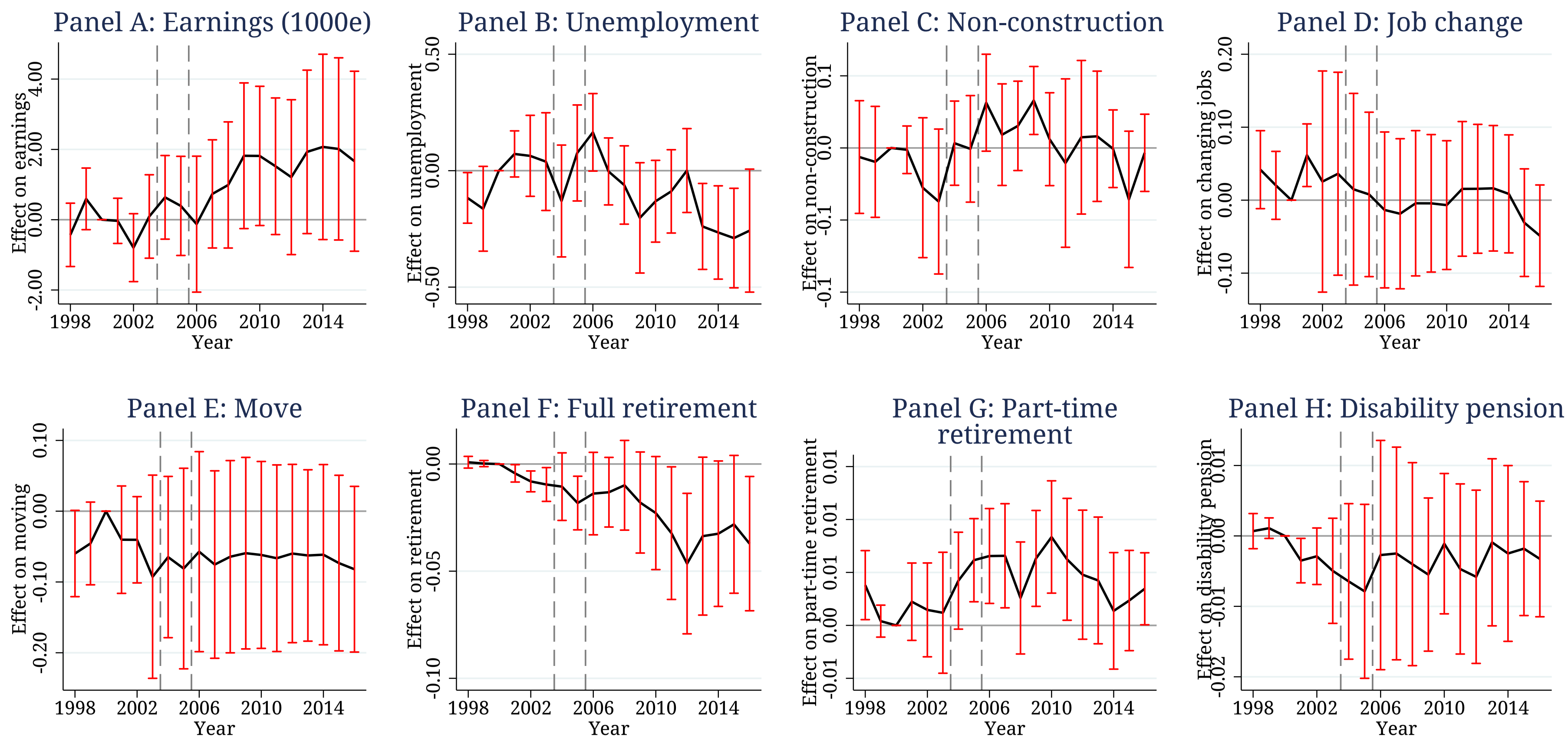


Figure E1. Effects of EU enlargement on electricians: event study specification

Notes: Figure shows point estimates from and their 95% confidence intervals constructed using standard errors clustered at the municipality level. Gray dashed lines mark the transition period.

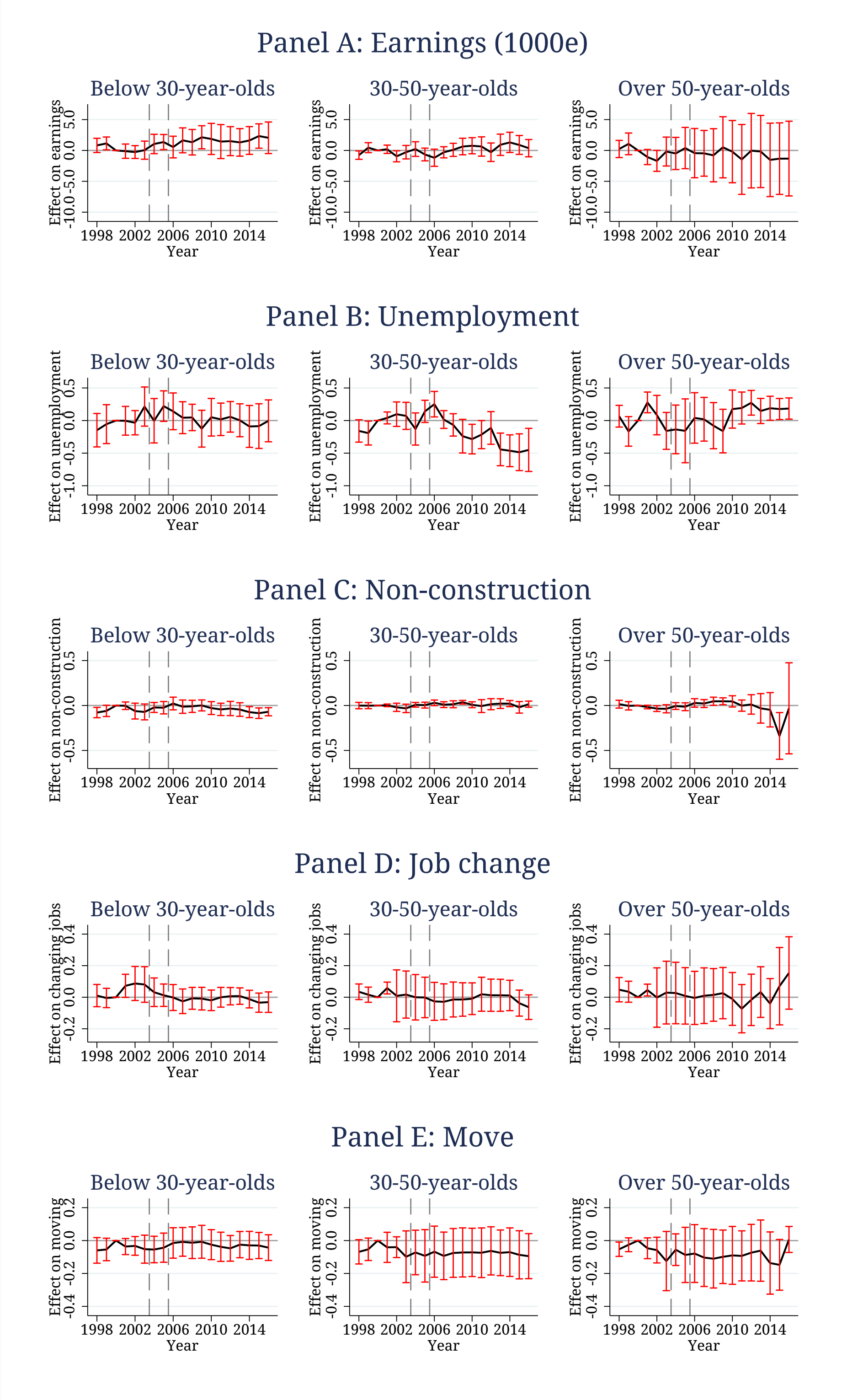


Figure E2. Heterogeneous effects of EU enlargement on electricians: event study specification.

Notes: Figure shows point estimates from and their 95% confidence intervals constructed using standard errors clustered at the municipality level. Gray dashed lines mark the transition period.

Table E1. The effects of the Eastern enlargement of the EU on electricians by age.

	Earnings	Months of unemployment	Non-construction	Job change	Moved	Retired	Part-time retirement	Disability pension
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Under 30-year-olds								
Transition period	0.701	0.124	0.038	-0.027	0.003			
	(0.491)	(0.093)	(0.023)	(0.032)	(0.022)			
N	8,891	8,891	8,222	7,593	7,672			
Until 2010	1.139*	0.058	0.035	-0.044	0.016			
	(0.628)	(0.079)	(0.022)	(0.031)	(0.024)			
N	12,825	12,825	11,811	11,147	11,262			
Until 2016	1.281	0.026	0.013	-0.049	0.008			
	(0.765)	(0.073)	(0.023)	(0.029)	(0.020)			
N	18,677	18,677	17,102	16,384	16,553			
Panel B: 30-50-year-olds								
Transition period	-0.321	0.114*	0.022*	-0.032	-0.028			
	(0.367)	(0.061)	(0.012)	(0.044)	(0.040)			
N	30,125	30,125	28,384	27,318	27,952			
Until 2010	0.150	-0.018	0.024**	-0.037	-0.029			
	(0.357)	(0.055)	(0.012)	(0.037)	(0.040)			
N	43,274	43,274	39,857	38,718	39,426			
Until 2016	0.479	-0.163**	0.021	-0.032	-0.028			
	(0.452)	(0.069)	(0.015)	(0.032)	(0.040)			
N	62,605	62,605	54,628	53,346	54,197			
Panel C: Over 50-year-olds								
Transition period	0.120	-0.102	0.013	-0.014	-0.022	-0.015	0.025***	-0.010
	(1.370)	(0.122)	(0.020)	(0.062)	(0.042)	(0.024)	(0.008)	(0.020)
N	8,787	8,787	7,037	6,713	6,899	8,787	8,787	8,787
Until 2010	0.086	-0.063	0.032*	-0.014	-0.035	-0.009	0.022**	-0.003
	(1.690)	(0.089)	(0.018)	(0.060)	(0.048)	(0.026)	(0.008)	(0.019)
N	12,422	12,422	8,457	8,117	8,319	12,422	12,422	12,422
Until 2016	-0.206	0.025	0.024	-0.018	-0.035	-0.015	0.016**	0.001
	(2.040)	(0.078)	(0.018)	(0.057)	(0.046)	(0.023)	(0.008)	(0.013)
N	16,565	16,565	8,910	8,567	8,772	16,565	16,565	16,565

Notes: Age is determined in the year 2000. Earnings refers to annual work-related earnings measured in 2015 euros. Non-construction refers to working in a sector other than construction. Job change refers to change in establishment. Moved refers to a change in commuting zone of work. Standard errors clustered at the municipality level are reported in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

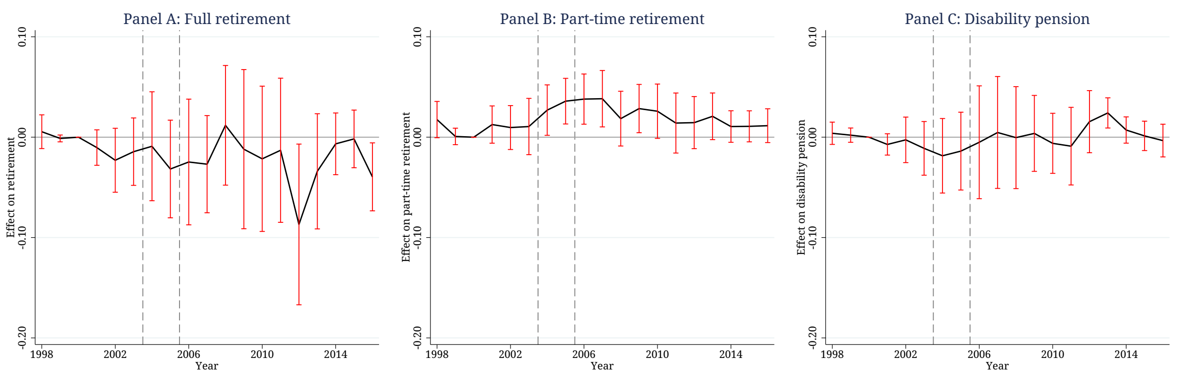


Figure E3. Effects of EU enlargement on old electricians' retirement behavior.

Notes: Figure shows point estimates from and their 95% confidence intervals constructed using standard errors clustered at the municipality level. Gray dashed lines mark the transition period.

2.F Additional Difference-in-Differences Results

This appendix reports additional difference-in-difference results where we use workers in vulnerable occupations in Helsinki commuting zone as the treatment group, and workers in the same occupations in other major commuting zones as the control group. First, Figure F1 plots the year-specific point estimates from an event study specification. They are largely in line with our parametric estimates. One difference, however, is that they tend to have narrower confidence bands.

We also study heterogeneous effects by age group. Table F1 reports point estimates from a parametric difference-in-differences specification. The DD estimates for our main outcomes seem to be driven mainly by younger workers, for whom we find negative (positive) and significant effects for earnings (unemployment). In contrast to the triple-differences results, we now see some significant effects for the 30-50-year old workers, especially in the short run. Nevertheless, the point estimates are slightly larger in absolute terms for the older workers. Figure F2 plots the heterogeneous effects by age group obtained using the event study specification, and Figure F3 shows the event-study specification results for old workers' retirement outcomes.

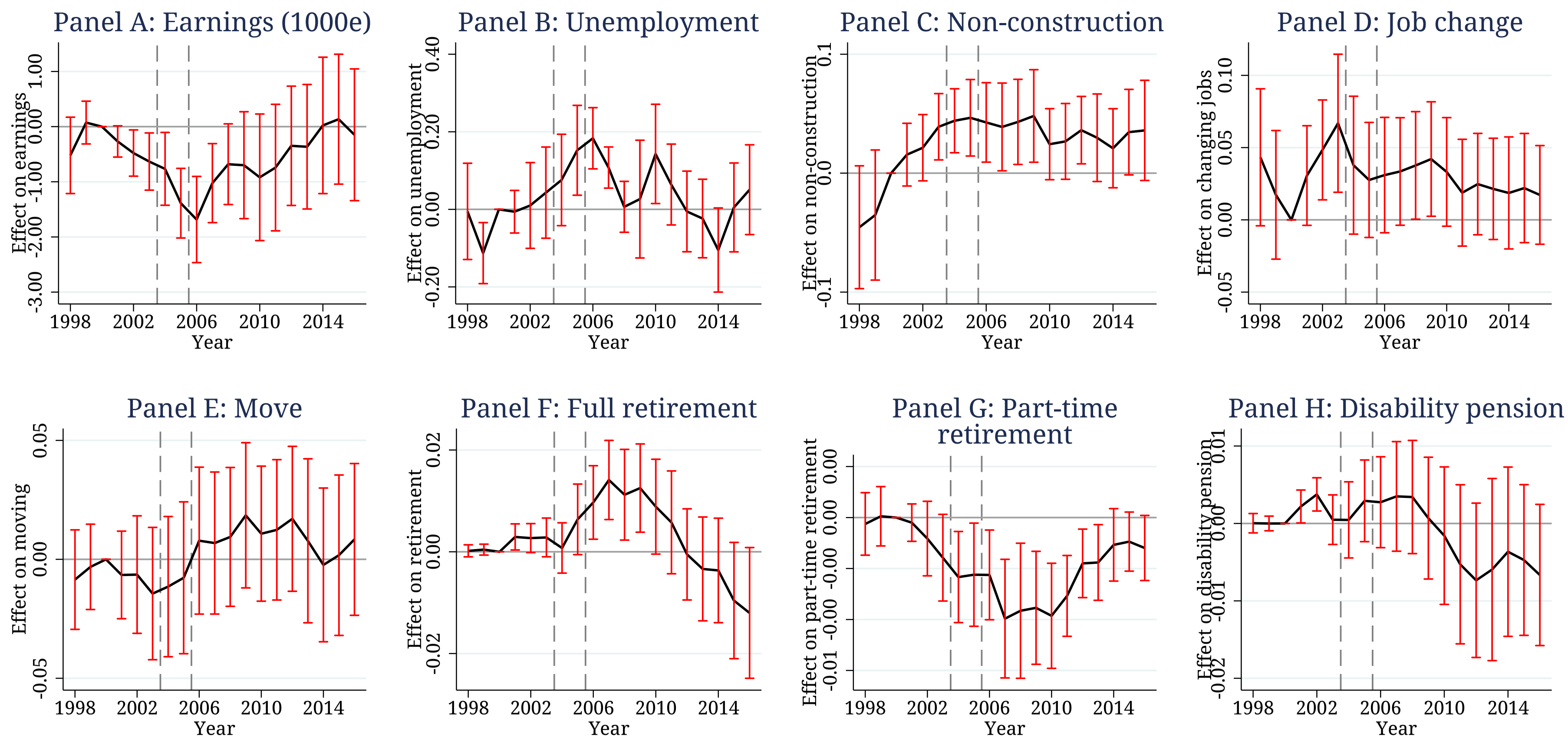


Figure F1. Effects of EU enlargement: event study specification.

Notes: Figure shows point estimates and their 95% confidence intervals constructed using standard errors clustered at the municipality level. Gray dashed lines mark the transition period.

Table F1. Effects of EU enlargement by age: difference-in-differences results.

	Earnings (1)	Months of unemployment (2)	Non-construction (3)	Job change (4)	Moved (5)	Retired (6)	Part-time retirement (7)	Disability pension (8)
Panel A: Under 30-year-olds								
Transition period	-1.126***	0.175**	0.017*	-0.000	0.014			
	(0.420)	(0.086)	(0.009)	(0.015)	(0.008)			
N	41,207	41,207	36,751	32,188	34,092			
Until 2010	-0.785**	0.174**	0.027***	0.007	0.028***			
	(0.367)	(0.072)	(0.009)	(0.012)	(0.008)			
N	59,413	59,413	52,159	47,353	49,518			
Until 2016	-0.200	0.134**	0.031***	0.002	0.033***			
	(0.375)	(0.064)	(0.009)	(0.011)	(0.009)			
N	86,493	86,493	74,420	69,137	71,781			
Panel B: 30-50-year-olds								
Transition period	-0.838***	0.202***	0.024***	0.001	0.003			
	(0.195)	(0.034)	(0.006)	(0.016)	(0.008)			
N	135,829	135,829	122,237	103,900	117,682			
Until 2010	-0.365*	0.094**	0.022***	0.002	0.009			
	(0.212)	(0.037)	(0.006)	(0.015)	(0.008)			
N	194,943	194,943	167,937	148,760	163,411			
Until 2016	0.038	0.019	0.019***	-0.002	0.008			
	(0.276)	(0.038)	(0.005)	(0.016)	(0.008)			
N	281,266	281,266	224,595	203,752	220,078			
Panel C: Over 50-year-olds								
Transition period	-0.869	-0.062	0.018	-0.019	-0.006	0.016*	-0.010***	0.002
	(0.556)	(0.110)	(0.011)	(0.030)	(0.009)	(0.009)	(0.003)	(0.006)
N	43,029	43,029	33,925	28,101	32,670	43,029	43,029	43,029
Until 2010	-1.309	0.093	0.010	-0.019	-0.003	0.029***	-0.013***	0.004
	(0.912)	(0.056)	(0.011)	(0.030)	(0.009)	(0.010)	(0.004)	(0.006)
N	60,486	60,486	40,170	34,252	38,922	60,486	60,486	60,486
Until 2016	-1.482	0.150**	0.002	-0.025	-0.003	0.018**	-0.009***	-0.000
	(1.095)	(0.065)	(0.011)	(0.028)	(0.008)	(0.009)	(0.003)	(0.005)
N	79,885	79,885	42,077	36,120	40,831	79,885	79,885	79,885

Notes: Age is determined in the year 2000. Earnings refers to annual work-related earnings measured in 2015 euros. Non-construction refers to working in a sector other than construction. Job change refers to change in establishment. Moved refers to a change in commuting zone of work. Standard errors clustered at the municipality level are reported in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

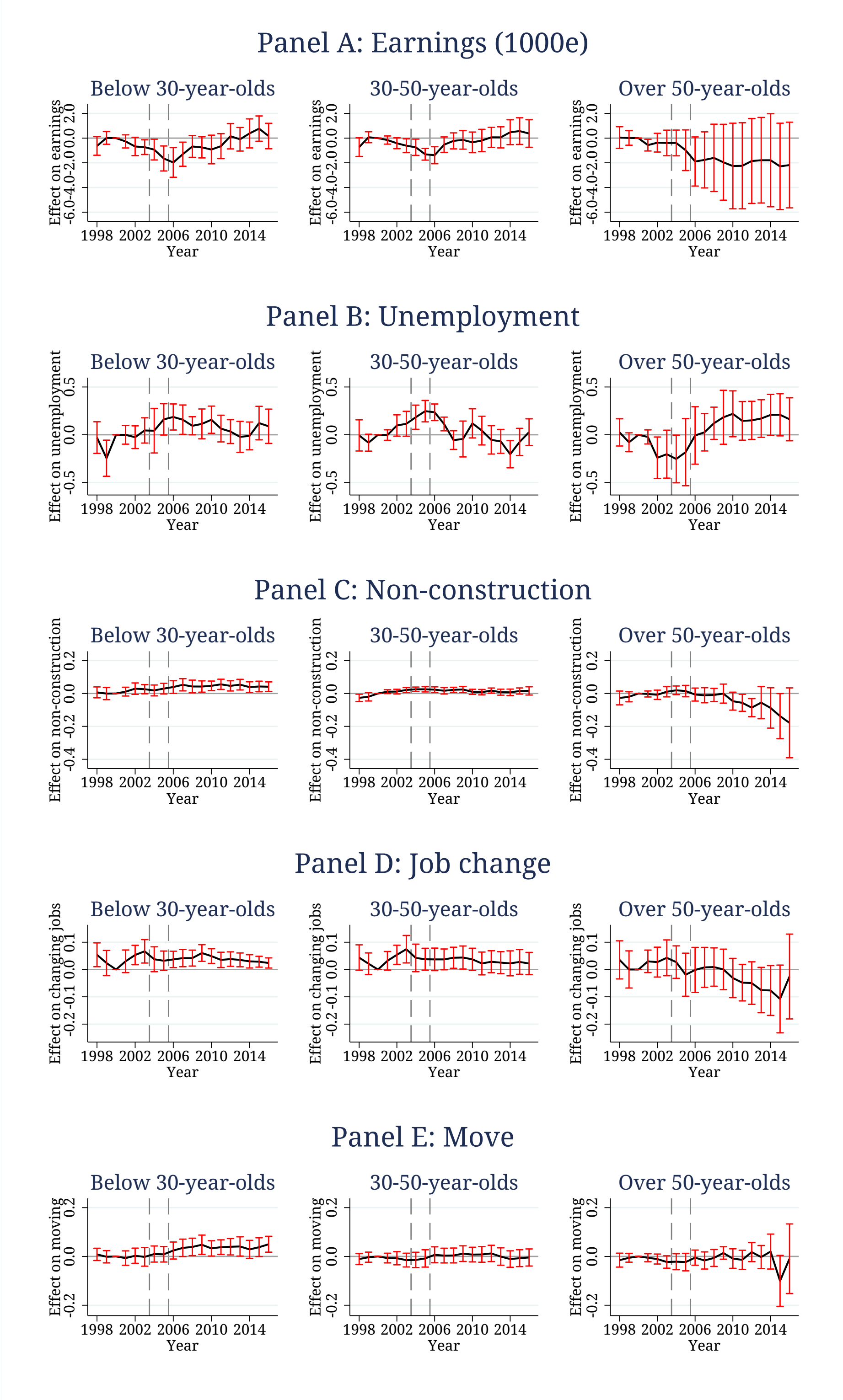


Figure F2. Heterogeneous effects of EU enlargement by age: difference-in-differences specification.

Notes: Figure shows point estimates and their 95% confidence intervals constructed using standard errors clustered at the municipality level. Gray dashed lines mark the transition period.

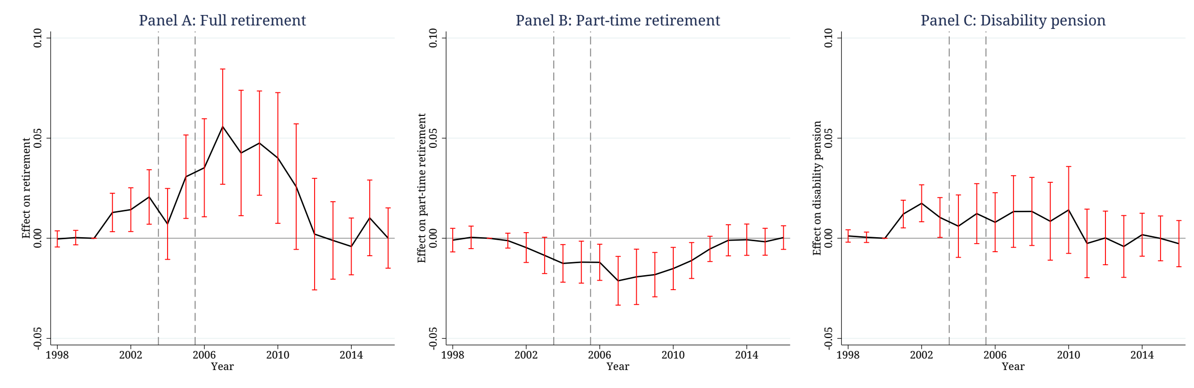


Figure F3. Effect on retirement decisions of old workers: difference-in-differences specification.

Notes: Figure shows point estimates and their 95% confidence intervals constructed using standard errors clustered at the municipality level. Gray dashed lines mark the transition period.

References

Ahas, R., S. Silm, and M. Tiru (2017). Tracking Transnationalism Originating in Estonia Through Mobile Roaming Data. Report. Available online at `https://inimareng.ee/en/open-to-the-world/tracking-trans-nationalism-with-mobile-telephone-data/` (accessed July 1, 2019).

3. Market-level and Distributional Effects of Public School Choice[1]

3.1 Introduction

School choice policies are common but the overall effects of these policies are unknown. School choice policies generally aim to improve social welfare and provide equal educational opportunities for children from different socioeconomic backgrounds by allowing students to choose their school more freely (Friedman 1955; Hoxby 2006). However, recent findings suggest that the effects of school choice can vary: even though the students who exercise choice might benefit from making that choice, the school choice policies may have negligible or negative spillovers to those students not exercising choice (Avery and Pathak 2015; Barseghyan et al. 2019; Epple and Romano 1998; MacLeod and Urquiola 2013, 2015, 2019; Rothstein 2006).

We estimate the overall effects of school choice on students' education and labor market outcomes by exploiting municipal-level variation in school choice possibilities across municipalities and over time in Finland. Our analysis uses a nationwide comprehensive school choice reform implemented in 1993, which allowed students to apply to other than the school they were originally assigned to.[2] We use individual-level longitudinal administrative data consisting of full cohorts of comprehensive school students, before and after the school choice reform. These data contain information on

[1]This chapter is joint work with Liisa T. Laine. We are grateful to Mika Kortelainen and Miikka Rokkanen for their contributions to this project. For their suggestions and comments we thank Manuel Bagues, Anirban Basu, Austin Bean, Jevay Grooms, Mika Haapanen, Dominik Hangartner, Helena Holmlund, Martin Huber, Kristiina Huttunen, Frederik Jørgensen, Jaakko Meriläinen, Tuomas Pekkarinen, Matti Sarvimäki, Benjamin Solow, Roope Uusitalo, and participants at ASSA 2019 poster session, EEA-ESEM 2017, HECER, IIPF 2017, Nordic Summer Institute 2017, LEER Workshop on Education Economics in 2017, SOLE 2018, ESPE 2018, Immigration Policy Lab in ETH Zürich, PhD seminar of University of Fribourg, PHEnOM Seminar at University of Washington, and VATT weekly seminar. We are grateful to the VATT Institute for Economic Research for providing the data access. Kuosmanen thanks the Yrjö Jahnsson Foundation and the Finnish Cultural Foundation for financial support, and IPL Public Policy Group at ETH Zurich for their hospitality. Laine undertook some of this research during her visit to Columbia University; she is grateful for their generosity.

[2]Before the reform, students were assigned to the nearest school based on their residential address and distance to schools. After the reform schools could accept students outside their catchment area boundaries.

the class and school attended by the students, linked to students' short- and long-term education and labor market outcomes.

Our empirical strategy focuses on the market-level effects of school choice. Our estimates thus capture not only the impact of school choice on students exercising choice (direct effect) but also the spillover effects on other students (indirect effect). Our estimates thus include changes in the peer group in addition to other school market-level changes, such as effects on school networks and the organization of education within schools. Taking both the direct and indirect effects of school choice into account helps us evaluate the overall effects of school choice.

We begin by showing that students from all backgrounds made choices after the reform, and the reform had a positive and economically meaningful effect on student outcomes on average. School choice increased GPA by 0.1 standard deviations and high school graduation probability by 2 percentage points on average. These short-term benefits persist in the long term, as students are 1 percentage point more likely to receive a higher education degree.

In addition to the average treatment effects of the reform, we study whether the school choice reform had distributional consequences. In this analysis, we use the information on students' household income in our data. Taking the distributional effects into account is important because public school choice policies often aim to decrease economic disparities between different socioeconomic groups. For example, school choice can help students from socioeconomically disadvantaged backgrounds to apply to a school with a better match or higher quality more easily than before school choice became available.

We find that our positive average treatment effects mask a significant treatment effect heterogeneity in terms of household characteristics (household income), and the average effect is driven by students from high-income households benefiting from school choice. Students from high-income households receive 0.1 standard deviations higher GPAs and are 3 percentage points more likely to graduate from high school than before the reform. These short term gains persist, as high-income students are 2 percentage points more likely to attain higher education. The short term education outcomes for students from low-income households are unaffected by the reform in the short term. In the long term, the labor market outcomes of low-income students deteriorate, and they are 1 percentage point less likely to attain higher education.

Because students from all income groups made choices after the reform, it is unlikely that our results are explained by differential propensities to make choices. Instead, our findings suggest that the heterogeneous effects could be explained by changes in peer quality (average GPA) of the school and class and selection into education and occupation. We find that high-income students attend higher peer quality schools and classes after the reform. High-income students are also more likely to select into education and occupation with higher earning potential than before the reform. In contrast, low-income students end up in occupations with lower earning potential.

The Finnish education system has simplifying features that are useful for studying the overall and distributional effects, and the mechanisms of public school choice. Comprehensive school education is entirely publicly funded (based on the number of students), and schools are not allowed to collect tuition fees. Thus, in comparison to other education systems, such as those in the U.S., Chile, and many European countries, the system in Finland can be interpreted as being fully public. School entry is needs-based and rare, and the reform did not change the entry regulation of new schools.[3] Lastly, in contrast to many local school choice or voucher programs that are particularly common in the U.S. (see, for example, Epple et al. 2017, for a survey), the reform was introduced nationwide, affecting all students throughout the country.

We contribute to the prior school choice literature by providing evidence on the overall effects of a nationwide public school choice reform. Prior research has focused mostly on estimating the impact of school choice only on those who made choices (see, for example, Abdulkadiroglu et al. 2018; Cullen et al. 2006; Deming et al. 2014) or considered spillover or overall effects when studying the effects of private school vouchers (Muralidharan and Sundararaman 2015) or the combined effects of choice and vouchers (Böhlmark and Lindahl 2015; Hsieh and Urquiola 2006; Sandström and Bergström 2005). Closest to our paper are Lavy (2010, 2015), studying the introduction of school choice in a school district in Tel Aviv. We complement these studies by analyzing the school market-level effects of a national level reform in a pure public school setting. Thus, our analysis takes into account all potential general equilibrium responses that might not be present in a smaller-scale setting.

Our paper is also related to Wondratschek et al. (2013) and Edmark et al. (2014), who study the effects of public school choice after a school choice reform in Sweden. The reform in Sweden differs from the Finnish reform because unlike in Finland, the Swedish reform introduced non-profit private schools and a voucher system in addition to the choice between public schools. Thus our setting is different to the reform in Sweden, where the potential entrants, i.e. new (private) schools, can affect the existing school network, making the mechanism analysis more complicated. Importantly, the focus of the studies by Wondratschek et al. and Edmark et al. is to use individual-level variation in school choice possibilities. We focus instead on the market-level (overall) effects of school choice in a purely public school setting.

The rest of the paper is structured as follows. We begin by describing the Finnish education system and the public school choice reform. Section 3.3 describes our data and

[3] This is in contrast to the reform in Sweden studied by Wondratschek et al. (2013); Edmark et al. (2014) which also included vouchers to private schools and allowed the entry of new private schools.

our empirical strategy. Our results are presented and discussed in Section 3.4, and the last section concludes.

3.2 Finnish Education System and the School Choice Reform

Finnish comprehensive education consists of primary school and lower secondary school. There is no formal division between primary school and lower secondary school today, but some schools still offer primary or lower secondary school education only. Primary school starts the year the student turns seven and lasts for six years. Lower secondary school takes three additional years. Almost all students complete comprehensive school: for example, the completion rate in 2013 was around 99.7 percent. (EDUFI 2017)

Municipalities are required by law to provide comprehensive school education for all their comprehensive school aged (7-16 year old) residents. Comprehensive education is financed by central government subsidies paid to municipalities (or other local education providers), but municipalities are also partially responsible for financing education. Municipalities are free to allocate the funding to the schools as they see fit.[4]

The vast majority of schools are operated by municipalities (over 95 percent) but some private schools exist in larger cities (2 percent) (Kumpulainen 2010). The rest of the schools are operated by the government and joint municipal authorities. Opening new schools is needs-based, for example, new schools might be founded in expanding or newly-built neighborhoods. Schools are not allowed to collect tuition fees or to make a profit. Importantly, education is publicly funded using the same funding principles regardless of the school's ownership status. For example, the private schools also receive their funding from the government and the municipality of operation (L 1704/2009). Therefore, compared to the education system in the U.S., for instance, the Finnish education system can be interpreted as being fully public despite the differing ownership status of schools.

We focus on the comprehensive school choice reform implemented in 1993, which allowed students to apply to schools other than the school they were originally assigned to. In general, school choice meant a possibility to choose a school within the municipality of residence. Choosing a school from another municipality was not specifically forbidden, but schools prioritize applicants from their municipality of operation (L 628/1998, section 28). However, this is not a very common practise: in our data, less than 3 percent of students attend a school in another municipality. We do not view this as a major issue and if anything, it would attenuate our estimates.

[4]For more on the financing, see information provided by the Ministry of Education and Culture available at `https://minedu.fi/en/financing-of-general-education`; accessed February 26, 2020.

Before the reform, students were assigned to the nearest school based on their residential address and distance to schools. Each school had its own school catchment area from which students were eligible to attend the school. We call such a school hereafter a neighborhood school. Crossing of the school catchment area boundaries was uncommon and required specific reasons, such as specific medical conditions. Schools were not allowed to select their students or to define the catchment areas they served. School catchment areas were set by municipalities, but this process was also regulated by several laws set by the government and other guidelines set by the provincial-level authorities. For example, all changes to school catchment areas required the approval of provincial-level authorities.

Before the school choice reform, municipalities were financially disincentivized to allow students to choose schools other than the assigned neighborhood school. Government subsidies for comprehensive school funding were tied to the number and size of school catchment areas within the municipality.[5] Municipalities designed their school catchment areas to maximize the funding for comprehensive education (Hirvenoja 1998). Thus, crossing the catchment area boundary—that is, allowing school choice—could have affected the level of government subsidies (HE 215/1991).

School choice was introduced through several law and policy changes, and the main law came into force in 1993. The main part of the reform made the allocation of government subsidies for comprehensive education simpler and more transparent in 1993 (L 705/1992). The financial disincentives for school choice were removed. Government subsidies were no longer tied to school catchment areas, and instead, were now solely based on the number of students. This meant that schools could accept students from outside the catchment area boundaries without it affecting government subsidies of the municipality.

Other changes to the legislation were smaller. The general goal of these law and policy changes was to enable municipalities to organize the provision of municipal-level services more freely. Two clauses from laws were removed and these took effect in 1993 and 1994, respectively. First, a clause that stated that students could get into other than their assigned school only for a specific reason, was removed (L 707/1992, section 38). Second, clause that defined the maximum distance (5km) to schools was removed (L 682/1993). Furthermore, municipalities no longer needed the approval of provincial-level authorities in order to, for example, change school catchment area boundaries. These changes, combined with the change in government subsidies, meant that municipalities were allowed to design their school networks quite freely.[6]

[5]Although, we have been unable to find the old formulas for government subsidies, the link between government subsidies and school catchment areas is mentioned in several sources (see government proposal HE 215/1991 and Koskinen (1994); Hirvenoja (1998); Varjo (2007)).

[6]In addition, there were two law changes that took effect in 1991, prior to the main law change in 1993. These did not introduce school choice but made implementing it easier in the future. These law changes allowed schools with special educational tasks to take on students from outside their own

After the reform, the law still required municipalities to assign each student to a neighborhood school. These students would have a priority in this school if the school was oversubscribed. Distance to school or an older sibling in the school could be used to prioritize the applicants from outside the school catchment area. Importantly, previous grades or household characteristics could not be used in student admission.

Municipalities implemented school choice differently. For example, the application processes and the acceptance rates to schools other than the neighborhood schools vary between municipalities. Some municipalities require students 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 individual schools 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)

The reform also introduced flexible curriculum guidelines that allowed schools to create specialized programs that offer extra teaching hours in, for example, sports or the natural sciences, on top of the normal curriculum.[7] Additionally, the flexibility in curriculum design allowed schools to specialize by offering elective courses that the students could freely choose within the school. More flexible curriculum design, and specialized programs in particular, were one of the main political motivations for school choice in Finland and it was thought that this would inspire and motivate students (Seppänen 2006).

Importantly, specialized programs offered schools a way to select their students. Schools could admit students from all over the municipality to the programs and students residing in the school catchment area did not have priority. Specialized programs became very popular after the reform and, today, around a fifth of all lower secondary school students attend a specialized program (Kupiainen and Hotulainen 2019). A law change in 1999 even allowed schools to use aptitude tests to select students for the programs (L 628/1998, section 28).

The integral part of the reform was the law change that simplified government subsidies to comprehensive schooling and, at the same time, removed the financial disincentives for school choice. This took place in 1993, which is why we consider it to be the first reform year. In fact, the laws did not formally speak of school choice until 1999, (L 628/1998, section 28), but it is mentioned in passing in one of the laws that took effect in 1993 (L 707/1992, section 47) and, thus, *de facto* allowed school choice

catchment area (L 171/1991, section 7) and gave schools permission to offer education in a foreign language (L 261/1991, section 25), as previously schools could only offer language immersion.

[7]Today, specialized programs offered, for example in Helsinki, include math, natural sciences, music, sports, performing and visual arts, media education and communication, and Latin (see city of Helsinki's web page on specialized programs offered by each school, available at `https://www.hel.fi/helsinki/en/childhood-and-education/comprehensive/what-how/painotettu/schools-offering-weghted-curriculum/`; accessed February 5th, 2020.)

(Varjo 2007).[8] Also, some pilot schools without a school catchment area had already existed in Helsinki in 1993 (Koskinen 1994), whereas school choice spread to other municipalities from 1993 onward.

3.3 Data and Empirical Strategy

We use an identification strategy that exploits variation in school choice possibilities across municipalities and over time generated by the school choice reform. However, we face two data limitations: we do not know if a student made a choice or not, and we do not know if municipalities had timing or implementation differences in the introduction of school choice. Thus, we cannot use variation across municipalities in their potential level of school choice activity in the post-reform period, such as implementation differences or applications rates to schools.

We use our data to overcome these problems as follows. We address the first problem by exploiting the residential information of the student and the most common school attended from each region to create proxies for choice. We address the second problem by approximating the intensity of the reform with the number of school choice opportunities measured by the number of schools in each municipality prior to the reform.

Section 3.3.1 collects the main features the administrative data sets and describes our sample construction. Further details on data construction can be found in the Appendix 3.A. Section 3.3.2 presents our empirical framework, lays out how we use our data to create the proxies for school choice and treatment intensity measures and how these are used to examine the market-level and distributional effects of the school choice reform, and then presents descriptive evidence on students' household characteristics in addition to describing our dependent variables. Details of variable construction can be found from Appendix Sections 3.B and 3.C. Section 3.3.3 gives details and the assumptions for our regression framework.

3.3.1 Data

Our first data record all students at the end of comprehensive school (grade 9) who apply to upper secondary schools between 1991 and 2007. We use these data to identify the comprehensive school and the class that students attended at the end of comprehensive school (grade 9). These students are 16 years old at the end of the comprehensive school education or turn 16 during the year.

We use these data to obtain a proxy for student's lower secondary class and school attended (and thus lower secondary school choice) on the 7th grade in 1988–2004. The

[8]School choice is also mentioned in the arguments of the government proposals for these laws (see HE 215/1991).

data only contain a unique school identifier but no other information about the schools. In particular, we do not know the addresses of the schools, if a student attends a specialized program, or if the school offers specialized programs. Second, the data also record the students' GPA at the end of 9th grade in addition to their municipality of residence, age, gender, and native language (Finnish, Swedish, or other).

Our second set of data contains yearly residential locations of the students at 250m×250m to postal code -level and covers the period from 1988 to 2007. These data are used to obtain the residential location of the student in the year that the student applies to lower secondary school at age 13 (grade 7).[9]

Our third set of data records information on the earnings, income, education, and employment status of the parent(s) of the student in addition to whether the student lives with both, one, or neither of the parent(s). We use these data for our distributional analysis when we divide the students into two groups based on their household income. We have information on the household characteristics of the student when the student is 14 years old (in grade 8).[10] These data run from 1989 to 2007.

Our last administrative data set is individual-level longitudinal data on the education and labor market outcomes of everyone in Finland aged 15 to 65 between 1988 and 2015. These data are used for our outcome variables on student's high school completion, earnings, education level, and occupation later in life.

The final sample is deidentified and constructed by using coded individual-level identifiers which link the students to their residential location information and their household characteristics, in addition to their education and labor market outcomes later in life.

Our final sample includes 984,478 students who started lower secondary school between 1988 and 2004 (between 54,000 and 60,000 students per cohort), meaning that we have data on five years before the reform. These students lived in 399 different municipalities, and all municipalities had at least one student per cohort.

3.3.2 Empirical Framework

Our identification strategy is difference-in-differences with continuous treatment intensity. We exploit variation in the intensity of the reform between municipalities. Similar identification strategies have been used, for example by Acemoglu et al. (2004), Card (1992), Cooper et al. (2011), Duflo (2001), Foged and Peri (2016), Gaynor et al. (2013), and Lucas and Mbiti (2012a).

[9] In an ideal case, we would use this information from a year or two earlier but as the first year in these data is from 1988, we do not have this information for the first (two) cohort(s) of students that applied to and started lower secondary school in 1988–1989.

[10] Ideally, we would use this information from the year or even prior to when the lower secondary schools applications are done but we only have the household characteristics for three consecutive years for each student when the student is in lower secondary school, aged 14–16.

We use the aspect that the reform was more intense in municipalities with multiple schools as there are more possibilities to exercise school choice. As we do not observe the actual intensity of the reform, we proxy it using the average number of schools in a municipality prior to the reform. Our empirical strategy thus compares changes in outcomes before and after a treatment between units across different levels of treatment intensity.

Proxy for Intensity of the Reform—We start by presenting descriptive evidence on our treatment intensity measure, number of schools, which is the average number of comprehensive schools with grades 7 to 9 in a municipality between 1988 and 1992 to capture the pre-reform variation in school choice possibilities and thus the potential intensity of the reform. This measure and its potential caveats are described in greater detail in Appendix 3.B.

Table 3.1 shows big differences between municipalities in the number of schools, and hence in school choice possibilities: on average there are only about 1.3 schools in smaller municipalities (with fewer than 10 schools before the reform), whereas in larger municipalities (with at least 10 schools) there are on average more than 20 schools to choose from. This discrepancy in the number of schools is also illustrated by Figure 3.1, which shows the distribution of the average number of schools prior to the reform. There are more than 300 municipalities with fewer than two schools and only 8 municipalities that have more than 10 schools. Some municipalities do not have schools and these students attend schools in neighboring municipalities.

Additionally, Table 3.1 highlights the difference in cohort size, as there are on average over 10 times more students in larger municipalities than in smaller ones. Smaller municipalities shrink slightly in terms of cohort and school size after the reform, but the number of schools remains constant. Even though Table 3.1 suggests that there are no clear changes in the number of schools after the reform, we use the average number of schools in a municipality before the reform as our treatment intensity measure to avoid any endogeneity bias.

Table 3.1. Municipal-level descriptive statistics.

	At least 10 schools		Fewer than 10 schools	
	1988–1992	1993–2004	1988–1992	1993–2004
	(1)	(2)	(3)	(4)
Number of schools	21.5	22.3	1.3	1.3
	(14.29)	(13.86)	(1.32)	(1.37)
School size	67.2	57.1	52.2	42.4
	(47.02)	(47.91)	(55.42)	(50.18)
Cohort size	1,782	1,819	114	110
	(960.1)	(1,065.6)	(129.8)	(123.5)

Notes: These are the means of municipal-level variables calculated separately for municipalities that have fewer than 10 or 10 or more schools before the reform and calculated separately for all cohorts before and after the reform. This is a balanced panel of municipalities. Number of municipalities in total is 399 of which 8 have more than 10 schools. Standard errors are clustered at the municipal level and shown in parentheses.

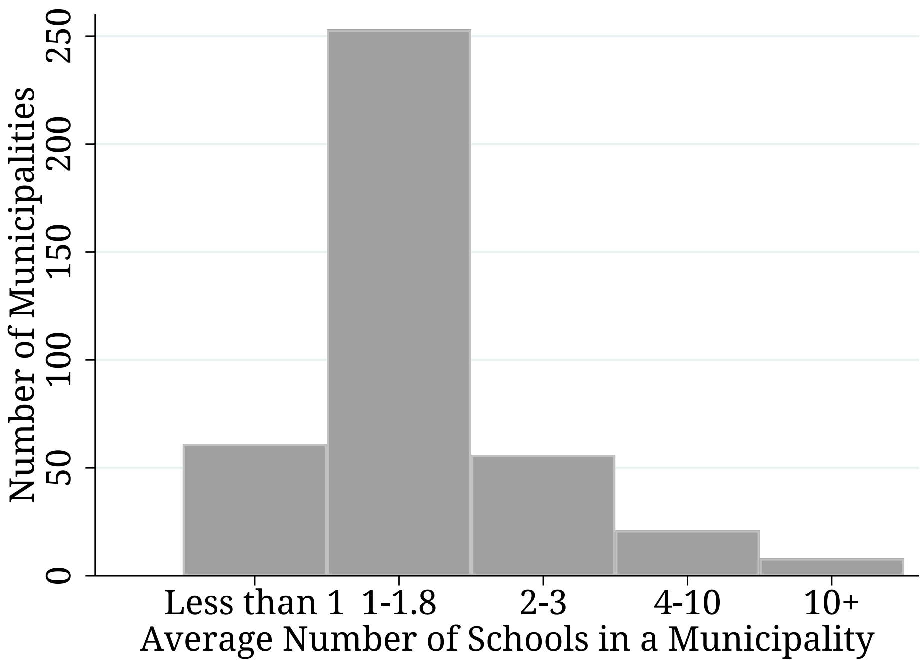


Figure 3.1. Distribution of the average number of schools within a municipality before the reform.

School Choice Proxies—We use a proxy for realized school choice activity, because information on applications or original neighborhood schools of the students is not systematically collected in Finland. We use three different proxies, of which one is our main proxy variable and two alternative measures are used as robustness checks. All our proxies for school choice are based on students' residential location at age 13 and the comprehensive school attendance information in our data. Thus, when we later study how the number of schools is related to the changes in realized school choice, we use proxy measures.

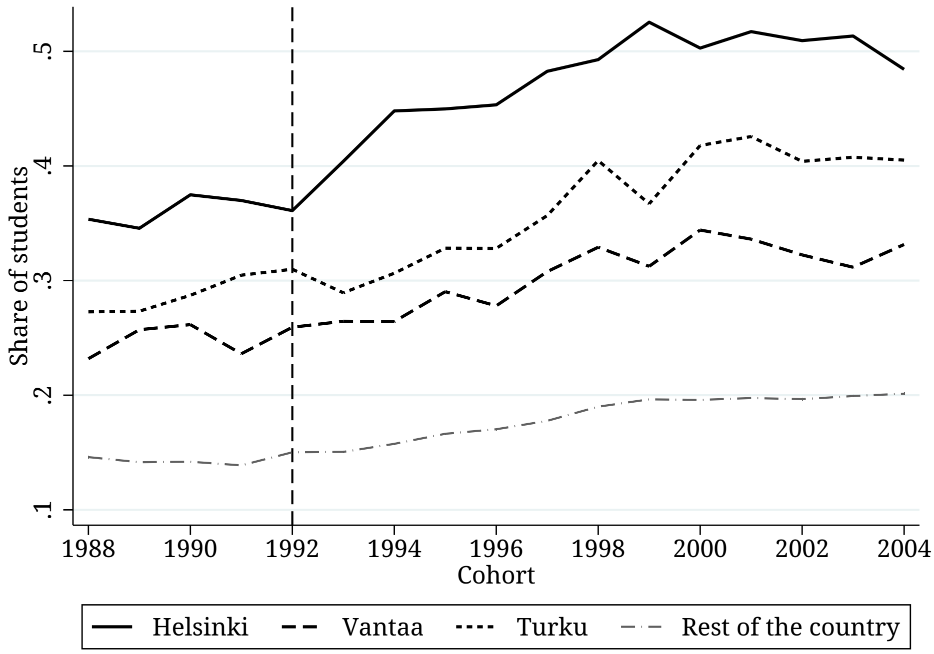
We call our main proxy variable *non-neighborhood school.* It measures if a student attends a school other than the most common school in the region that year. The precise definition of the variable is that the dummy takes value 100, if a student attends other than the most common school of a particular 1km by 1km grid that year, otherwise the variable gets value zero.[11]

Figure 3.2 shows the development of this non-neighborhood school-proxy for school choice in three major cities of Finland: Helsinki, Turku, and Vantaa. It shows that school choice increased 10-15 percentage points (pps) in Helsinki and Turku. In Vantaa, a city with a more restricted school choice policy (Kalalahti et al. 2015), the increase in school choice after the reform was more modest.

The development of school choice activity is in line with information from an early municipal report from Helsinki: as early as 1994, around a third of the students entering lower secondary school applied, and around 80 percent were accepted, into other than their neighborhood school in Helsinki (Koskinen 1994). Moreover, at the beginning of the 2000's, about half of the students starting lower secondary school in Helsinki applied to another school (Seppänen 2006). The corresponding numbers are a bit lower in other major municipalities (Seppänen 2006). Unfortunately these numbers are not available on a yearly basis.

We use non-neighborhood school as our main proxy for choice activity because it is a simple, realistic, and transparent measure. This measure, however, can be upward biased. Some students living in the same region (1km by 1km grid) may be assigned to different neighborhood schools. This would incorrectly be measured as choice activity. This bias may be larger if there are several schools close by that serve a densely populated, but relatively small geographic area, such as the city centers of large municipalities. As an example, in comparison to the figures by (Koskinen 1994) and Seppänen (2006) this measure slightly overestimates choice activity in Helsinki and Turku. This bias can also explain why students seem to have made choices before the reform (Figure 3.2). However, a few schools in Helsinki, and other major cities, had special language or music programs (without school catchment areas) even before the school choice reform (see, for example, Seppänen 2003), which is also picked up by our proxy. This is not a concern for our identification strategy as long as the share of students attending these programs stays relatively constant in these municipalities prior to the reform.

[11] We use the value 100 for scaling and interpretation purposes.



Notes: Share of students attending a non-neighborhood school is a proxy for school choice activity. It measures whether the students attended a school other than the most common school of their region. Cohort refers to the year when the students applied to lower secondary school (7th grade). Vertical line marks the last cohort before the school choice reform.

Figure 3.2. The development of the share of students attending a non-neighborhood school in Helsinki, Turku, and Vantaa between 1988 and 2004.

Our main proxy measure may 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 this, we use two complementary measures to our main school choice proxy. The first measure takes a different approach to approximating the neighborhood school. In this measure, a school is a neighborhood school if at least 30 percent of the region's students attend it: the measure takes a value of 100 if the student attends a school other than a school that at least 30 percent of the students of the 1km by 1km grid attend that year, otherwise 0. We call this alternative proxy for school choice *non-neighborhood school 30*.

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 attends a school different from that attended by the other students in the grid and 0 that the student attends the same school as everyone else in the grid. We calculate mobility index measure 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. Comparing all the three approximation measures above shows similar patterns in the development of school choice activity (see Appendix 3.D).

Descriptive Statistics with Respect to Household Characteristics—We study whether the school choice reform had distributional consequences. To do this, we describe how students from different socioeconomic backgrounds differ in terms of various household and student-level characteristics.

We divide our sample into quartiles each year using household income. Household income includes all work- and entrepreneurship-related earnings of the household in addition to the benefits, such as maternity leave, social or unemployment benefits, received by the household.The upper and bottom quartiles represent the students from high and low-income households, respectively. In Appendix 3.D.2, we combine the two middle quartiles into one, representing the students from middle income households for transparency of our results.

Table 3.2 summarizes our main household- and student-level characteristics, with respect to household income, before and after reform, and between small (fewer than 10 schools before the reform) and large (at least 10 schools before the reform) municipalities. All of these household characteristics are measured the year the student turns 14. We provide a full set of descriptive statistics of our outcome and control variables in Appendix 3.C.

Table 3.2 shows striking differences in household characteristics between the students from high- and low-income households. Compared to the students from high-income households, students from low-income households are more likely to have one or both parents who lack a (upper) secondary or higher education and employment. They are also more likely to come from a single parent household. The share of single parent households also varies between small and large municipalities and over the years.

The Finnish welfare system aims to even out some of these differences between socioeconomic groups. One way to illustrate these differences is to compare the household income including both work-related earnings and benefits to pure work-related earnings. Table 3.2 shows striking differences in the household earnings relative to household income between the socioeconomic groups and small and large municipalities. Low-income students' household income is over 50 percent higher than household earnings on average, whereas high-income students' household income is only slightly (10 percent) higher than household earnings on average.

The characteristics of the students themselves also vary by household income: there are more foreign native language speakers in students who come from low-income households, especially after the reform in larger municipalities (almost 10 percent) (Table 3.2).[12] On average, a student from a low-income household receives lower grades

[12]Finnish and Swedish are the native languages of Finland.

Table 3.2. Descriptive statistics on household characteristics

	At least 10 schools		Fewer than 10 schools	
	1988-1992	1993-2004	1988-1992	1993-2004
	(1)	(2)	(3)	(4)
Average household income (in 2015 euros)				
Average	63,170	72,629	49,556	57,494
	(40,763)	(71,414)	(29,919)	(47,757)
Low-income	24,391	26,430	24,822	26,955
	(9,311)	(12,662)	(8,895)	(12,051)
High-income	94,201	114,079	84,457	98,712
	(48,095)	(101,028)	(45,778)	(84,613)
Average household earnings (in 2015 euros)				
Average	54,942	64,123	38,650	46,843
	(34,018)	(68,932)	(26,771)	(48,625)
Low-income	16,815	16,914	13,585	14,605
	(11,269)	(14,374)	(11,483)	(13,761)
High-income	83,776	104,265	70,201	86,314
	(32,815)	(95,895)	(28,885)	(83,838)
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)
High-income	0.94	0.96	0.94	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)	(0.37)
Low-income	0.22	0.41	0.19	0.33
	(0.42)	(0.49)	(0.39)	(0.47)
High-income	0.02	0.03	0.02	0.03
	(0.14)	(0.17)	(0.15)	(0.16)

Table continued on the following page.

Table 3.2. *(continued)* Descriptive statistics on household characteristics

	At least 10 schools		Fewer than 10 schools	
	1988-1992	1993-2004	1988-1992	1993-2004
	(1)	(2)	(3)	(4)
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)
High-income	0.16	0.20	0.12	0.16
	(0.36)	(0.40)	(0.32)	(0.37)
Student has a foreign native language				
Average	0.01	0.03	0.00	0.01
	(0.08)	(0.16)	(0.05)	(0.08)
Low-income	0.03	0.09	0.01	0.02
	(0.16)	(0.29)	(0.07)	(0.13)
High-income	0.00	0.01	0.00	0.00
	(0.04)	(0.07)	(0.03)	(0.04)

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 household income* is an average of the sum of all work- and entrepreneurship-related earnings of the household in addition to the benefits, such as maternity leave, social or unemployment benefits, received by the household. *Student has a foreign language* is defined as a student whose native language is not either Finnish or Swedish. Standard errors are clustered at municipal-level and shown in parentheses.

at the end of comprehensive school, is less likely to have a high school and higher education, and also earns less in 2015 than a student from a high-income household (Table C2 in Appendix).

Dependent Variables—We study the following student-level outcome variables: GPA at the end of 9th grade, high school graduation, earnings in 2015, and higher education in 2015. We use the following mediating outcomes to explain these main outcomes: average GPA of the school and class, and earnings potential of the education group and occupation. We explain the construction of each variable as we present the result for each outcome. We also present a more detailed description of the variable construction in Appendix 3.C.

3.3.3 Regression Framework

Our identification strategy is difference-in-differences with continuous treatment intensity. We begin by estimating the effects of the treatment intensity measure on realized school choice and student outcomes.

First Stage—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

$$S_{imc} = \alpha_c^{FS} + \gamma_m^{FS} + \beta^{FS} N_m \times Post_c + \mu_c^{FS} X_i + \epsilon_{imc}^{FS}, \tag{3.1}$$

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 in which cohort is the year in which the 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 (3.1) also includes student-level controls X_i, the coefficients of which are allowed to vary across cohorts. We add cohort-specific trends to address the issue that students from different backgrounds might have differential pre-treatment trends, because our identification strategy exploits municipal-level pre-treatment variation in the exposure to the school choice reform. These differences could confound our estimates as student composition varies between municipalities (Jaeger et al. 2018).

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, and gender and native language of the student, in addition to controls for the household characteristics: earnings, education and employment of parents (either single parent or both parents), household 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 household characteristic controls.

Because our observation period is long, it is possible that different sized municipalities were exposed to differential economic shocks, as well as other reforms, that could have impacted municipalities differently, which could confound our estimates. We use alternative specifications, such as including time-varying municipal-level controls, cohort-specific county-level fixed effects, and flexible non-parametric rural/urban municipality fixed effects to Specification 3, as well as dropping the largest municipality, Helsinki, the capital city of Finland, from our analysis, to test the sensitivity of our results. Our results are robust to these alternative specifications (Appendix 3.E).

To our knowledge, there are two reforms–other than the school choice reform–that might have affected student outcomes during our observation period and that could be picked up by our identification strategy. First, in 1999, the caps to class size (32 students) in comprehensive education were removed (Ministry of Education and Culture 2014). We do not consider this as a threat to our identification since less than 1 percent of the students in our data attend a class with more than 30 students each year.

Second, our observation period also coincides with the closures of small rural schools. Between 1990 and 2012, over 2,000 small schools in rural municipalities were closed (Autti and Hyry-Beihammer 2014). Panel A of Appendix Table E1 adds the number of schools each municipality has in a given year as a control, and this does not alter our results.

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

$$y_{imc} = \alpha_t^{RF} + \gamma_m^{RF} + \beta^{RF} N_m \times Post_c + \mu_c^{RF} X_i + \epsilon_{imc}^{RF}, \tag{3.2}$$

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 (3.1)).

Event-study—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 time (the parallel trends assumption). We test the parallel trends assumption underlying the differences-in-differences design using the following non-parametric event-study equation that includes interactions between each cohort and the treatment intensity variable:

$$\begin{aligned} y_{imc} = \alpha_c^{RF} + \gamma_m^{RF} + &\sum_{c=1988}^{1991} \beta_c^{RF} N_m \times \text{D(cohort=c)} \\ &+ \sum_{c=1993}^{2004} \beta_c^{RF} N_m \times \text{D(cohort=c)} + \mu_c^{RF} X_i + \epsilon_{imc}^{RF}, \end{aligned} \tag{3.3}$$

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). The rest of the model is defined analogously to equations (3.1) and (3.2).

The parallel trends assumption requires that, before the reform, the relationship between the treatment intensity variable and the outcome should be stable. This means that all the β^{RF}'s in equation (3.3) before the reform should be zero. In addition to testing the parallel trends -assumption, equation (3.3) allows us to study the dynamics of the effects of the reform. This allows us to detect whether the effects of the reform are stronger for younger cohorts, as school choice became more prevalent over time.

IV Approach—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 do this by estimating a 2SLS model in which the interaction between the reform and the number of schools in the municipality as an instrument for realized school choice, $N_m \times Post_c$, 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 (3.1). The IV estimates are obtained from the following equation

$$Y_{imc} = \alpha_c^{2SLS} + \gamma_m^{2SLS} + \beta^{2SLS}\hat{\mathrm{S}}_{imc} + \mu_c^{2SLS} X_i + \epsilon_{imc}^{2SLS}, \tag{3.4}$$

where $\hat{\mathrm{S}}_{ims}$ is the realized choice predicted by the intensity of the reform from our first stage given by equation (3.1), and β^{2SLS} is the coefficient of interest that measures the effect of a percentage point increase in the share of students who made a choice in a municipality. We define the rest of the variables in the equation (3.4) as in equation (3.1).

We make some additional assumptions in our IV strategy. First, we do not observe choices directly in our data and we use a proxy for realized school choice as our endogenous variable. This does not introduce bias into 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. Second, realized school choice may not fully capture all the channels through which the effects of the school choice reform operate. This may be the case, for instance, if schools respond 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.

3.4 Results

We begin with our first-stage results and show that our instruments (main and complementary measures) for choice activity are related to changes in realized school choice (Section 3.4.1). In Section 3.4.2, we show how school choice has affected students' short- and long-term education and labor-market outcomes. Section 3.4.3 provides our analysis of mechanisms of school choice. All our results are based on Specification 3 with cohort-specific controls, and the other two specifications are presented in Appendix 3.D.2.

We show our estimates for all students and also by dividing students in two groups based on their household income. We call these groups low and high-income students. This heterogeneity analysis allows us to study how school choice has affected students' outcomes based on their household characteristics. For the sake of transparency, we present results with the middle income group to show the effects of the reform for the entire spectrum of the income distribution in Appendix 3.D.2.

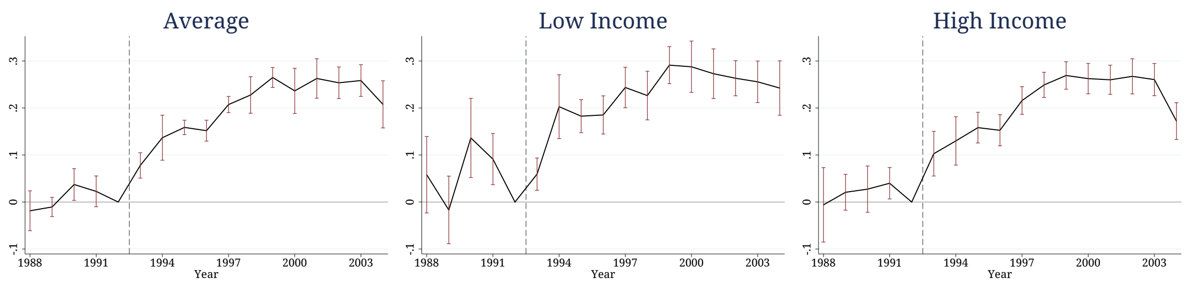
3.4.1 Did the Reform Affect School Choice Activity?

Figure 3.3 plots our event-study estimates (Equation 3.3) for our main school choice proxy, non-neighborhood school, measuring if the student attends a school other than the neighborhood school. Our first stage results using Equation 3.1 are summarized in Table 3.3.

Our results show that school choice increased gradually after the reform in municipalities with more schools (Figure 3.3). We find no evidence of differential pre-trends. We also find that students from all socioeconomic backgrounds make choices: there are no significant differences in the shares of students making choices between students from the low and high-income households (Figure 3.3).

Our identification strategy compares a municipality with low treatment intensity to a municipality with high treatment intensity. In our empirical setting, a rough comparison would be a comparison between an average municipality with fewer than two schools to the capital city, Helsinki with more than 50 schools. Using this comparison, our estimate in column 1 of Table 3.3 shows an almost 10 (=0.198*50) percentage point (pps) increase on average in the probability of attending a non-neighborhood school. Figure 3.3 shows that this estimate is even greater after the 2000s, about 15pps. This shows that our instrument captures the gradually increasing school choice activity after the reform.

We also confirm these results using our two alternative proxies for school choice, non-neighborhood school 30 and mobility index. The results are collected in our Appendix (Panel A and B of Figure D2, respectively). The results are in line with the event-study estimates of our proxy: the other two approximation measures also show no evidence of



Notes: The outcome of interest is non-neighborhood school that measures if the student attends a school other than the most commonly attended school of the region. Figures show the coefficient estimates from the event-study regression and their 95% confidence intervals constructed using standard errors clustered at the municipal-level. The gray dashed line marks the school choice reform.

Figure 3.3. Effects of school choice on the probability of attending the non-neighborhood school.

differential pre-trends and an upward trend in school choice after the reform. These are confirmed by the first stage results (Equation 3.1) summarized in Table 3.3.

Our results show a sizable increase in school choice activity by students from all socioeconomic backgrounds. One goal of school choice has been to help students with socioeconomically disadvantaged backgrounds to apply to a school with a better match or higher quality more easily than before school choice. The Finnish school system has consistently ranked as one of the world's least segregated in terms of student outcomes in international PISA comparisons (OECD 2013). This may suggest that there are only small (quality) differences between schools. Thus, the increased choice activity in the Finnish context is surprising.

Increased choice activity can also be surprising because no average student attainment measures, or school rankings, are published in Finland. One potential explanation for increased school choice is specialization (differentiation) of the schools (Seppänen 2006). Specialized programs and elective courses can also serve as quality signals. Although municipalities are free to choose how they allocate the central government subsidies and municipal-level funding to the municipal-run schools, schools also have significant autonomy. Most schools are, for example, free to choose the specialization of the elective courses they offer (Varjo and Kalalahti 2011).[13] Unfortunately, our data does not contain information on these classes (for further discussion on specialized programs see also Section 3.4.3.).

[13]Municipal-level authorities usually decide the specialized programs that schools offer (Varjo and Kalalahti 2011).

Table 3.3. First stage results.

	NNS (1)	NNS-30 (2)	Mobility (3)
Average	0.198***	0.272***	0.002***
	(0.019)	(0.016)	(0.000)
N	984,478	984,478	959,442
Low-income	0.174***	0.249***	0.002***
	(0.028)	(0.024)	(0.000)
N	246,099	246,099	242,255
High-income	0.192***	0.251***	0.002***
	(0.017)	(0.017)	(0.000)
N	246,102	246,102	234,851

Notes: NNS refers to non-neighborhood school that measures whether the student attends a school other than the most common school of the region. NNS-30 refers to non-neighborhood school 30 that measures whether the student attends a school other than the school that at least 30% of the region's students attend. Mobility refers to mobility index and it 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/postal code as student i. The regressions control for cohort and municipality fixed effects, student and household-level controls, and interactions between the cohort and controls. Standard errors are clustered at the municipal-level and shown in parentheses. N refers to number of observations.
* p<0.05, ** p<0.01, *** p<0.001.

3.4.2 Effects on Education and Labor Market Outcomes

Next we investigate how increased school choice activity translates into education and labor-market outcomes. Our heterogeneity analysis allows us to study how school choice has affected student outcomes based on students' socioeconomic background and whether school choice has decreased economic disparities between different socioeconomic groups.

We collect all the results of this section in Figure 3.4 and Table 3.4. We present the results from our event-study specification (Equation 3.3) in Panel A and B of Figure 3.4 for short-term education outcomes, whereas we plot the corresponding long-term labor market outcome estimates in Panel C and D of the same figure. Similarly, we show the reduced form (Equation 3.2 and IV (Equation 3.4) results for the short-term in column 1 and 2 of Table 3.4 and the long-term outcome variables in columns 3 and 4 of the same table.

Short-term Education Outcomes—We first show how school choice has affected short-term education outcomes: *GPA* and the probability of graduating from *high school*. GPA is an important outcome because students apply to upper secondary schools (academic or vocational track) based on it.[14] Probability of graduating from high school signals the students' opportunity to continue on to higher education: academic track high school degree is used for college or university applications.[15]

The event-study estimates show that school choice has, on average, a positive effect on GPA and that the effect increases gradually after the reform. These findings are confirmed by our results based on reduced form specification. We quantify the reduced form estimates using the same logic as with our school choice proxy and we compare a low treatment intensity municipality to a high treatment intensity municipality, i.e. Helsinki, with more than 50 schools vs. an average municipality in Finland with fewer than two schools. Using this comparison, our estimates show an approximate increase of 0.09 (=0.0017*50) standard deviations in GPA.

This effect is sizable even in comparison to some empirical studies that have used school choice lotteries to estimate the impact of school choice only on those who choose another school. For example, Muralidharan and Sundararaman (2015) find that winning a choice lottery in a large-scale private school voucher experiment in India increased English and math grades by around 0.1 standard deviations. The evidence from smaller scale voucher experiments is more mixed. For example, Cullen et al. (2006) find no evidence that winning a lottery in the Chicago Public School system affects traditional student outcomes such as grades and graduation. On the other hand, Abdulkadiroglu et al. (2018) find sizable negative effects of winning a private school voucher lottery in the Louisiana Scholarship Program on math and other subjects: math test grades decreased by 0.4 standard deviations.

The breakdown of these average market-level effects by household income reveals that the positive average effect on GPA seems to be driven by a positive effect on students from high-income households. Students from high-income households have approximately 0.1 standard deviations higher GPA. There are no significant effects on GPA for students from low-income households. Because there appears to be some trend in the pre-period

[14]GPA is the average of theoretical subjects at the end of the comprehensive school and ranges from 4 (failed) to 10 (outstanding). The grades are given by teachers. There is no standardized country-wide testing in Finland. We standardize this to have a mean of 0 and standard deviation of 1. We drop around 10 municipalities in year 1994 from our regressions with GPA as the dependent variable because of a high share of missing GPA values in these municipalities.

[15]This variable takes value 1 if the student has completed high school and matriculation examinations by the end of 2015, otherwise 0.

for students from high-income households, our evidence for the effects on students from high-income households should be taken with some caution.

Regarding the results on GPA results, we also acknowledge the possibility of grade inflation: the grades are given by teachers and there is no country-wide standardized testing for ninth graders in Finland. Regardless, we emphasize that GPA is used by all students to apply to upper secondary schools. A higher GPA, inflated or not, would still mean better chances to get into better high schools. Moreover, if grade inflation explained our GPA results instead of school choice, high-income students should have higher levels of grade inflation than low-income students. We do not think that this is plausible: Recent studies have shown that teachers in Finland grade their students relative to the ability of student's peers in the school or classroom, i.e. a student will get a better grade in a class in which the overall ability of the peers is lower (Hildén et al. 2016). Thus, if grade inflation explains our results, students from high-income households should have attended schools with lower peer ability after the reform. Our identification strategy would also require the level of grade inflation to be higher in larger municipalities. We think that this is unlikely too.

Our second short-term education outcome is the probability of graduating from high school. We find that school choice increased the probability of graduating from high school by 2pps on average according to our reduced form results. Similarly to the GPA, the event-study estimates show that this effect is gradually increasing.

Again, we find that the positive average effect is driven by a positive effect on students from high-income households: students from high-income households are about 3pps more likely to graduate from high school. Also, similarly to our GPA results, we find no significant effects on either of these outcomes for students from low-income households.

We consider an increase of 10pps in realized choice activity measured by the share of students attending a non-neighborhood school in the interpretation of our IV results. Recall that Section 3.4.1 shows that school choice, proxied by non-neighborhood school attendance, increased 10pps after the reform. Figure 3.2 also shows around a 10pps increase in the share of students attending a non-neighborhood school after the reform in Turku and around 15pps in Helsinki. With these quantifications, our IV estimates show that, on average, GPA increases 0.08 standard deviations and probability to graduate from high school around 2pps. These are very similar in magnitude to our reduced form estimates.[16]

Long-term Labor Market Outcomes—Next, we study whether school choice affected the probability of obtaining a bachelor's (or higher) degree and earnings in 2015. We

[16]For the other school choice approximation measure, *non-neighborhood school 30*, see Appendix Section 3.D.3. These alternative IV results confirm the findings here: GPA increases on average by 0.08pps and probability to graduate from high school by 2pps when we consider an increase of around 13pps in the share of students attending a non-neighborhood school 30 after, shown by our first stage results in Table 3.3.

study these only for cohorts who started seventh grade between 1988 and 2001. This is because the later cohorts are quite young and still studying when we observe them in 2015, and may thus have zero earnings and no higher education. This is illustrated in Figure D4 in our Appendix 3.D.5. Furthermore, the share of zero earners is highest among the students from low-income households. Most of the high-income household students who have zero earnings are still working on a higher education degree. This could complicate the interpretation of our results.

We start by studying whether school choice affected the probability of having a bachelor's (or higher) degree in 2015, as in the previous section we showed that school choice had an impact on high school graduation and high school diplomas are used in university applications. We call this variable the probability of having *higher education* (the term in italics refers to the outcome used in table).[17]

We find that the probability of having a higher education increased on average by 1pps after the reform. We consider this market-level effect to be sizable. The empirical literature on the impacts of school choice on later life labor market outcomes is scarce, but our result is comparable to estimates by Lavy (2015) and Wondratschek et al. (2013). Both of these studies also find positive effects on average for later life labor market outcomes.[18]

Similarly to the results of our short-term education outcomes above, the positive average effect for the probability of having a higher education is driven by a positive effect for students from high-income households. Both the reduced form and IV estimates for this outcome show that students from high-income households are about 2pps more likely to have a higher education. The effect for higher education is negative and insignificant for students from low-income households when measured with our reduced form specification. However, the IV estimates show that students from low-income households are 2pps less likely to attain higher education.

Our reduced form and IV estimates for the average effect on earnings in 2015 are positive but insignificant.The breakdown of these average results by household income suggests that the effects are positive for students from high-income households and negative but insignificant for students from low-income households. However, these results should be interpreted with caution as the event-study estimates after the reform are not that different from the pre-treatment estimates and there is evidence of differential pre-trends.

Our results thus far suggest that although students from all backgrounds made choices after the reform, there is considerable heterogeneity in the effects of the reform

[17]Higher education takes value 1 if the student has a degree that is equivalent to a Bachelor, or higher, in 2015, otherwise 0.

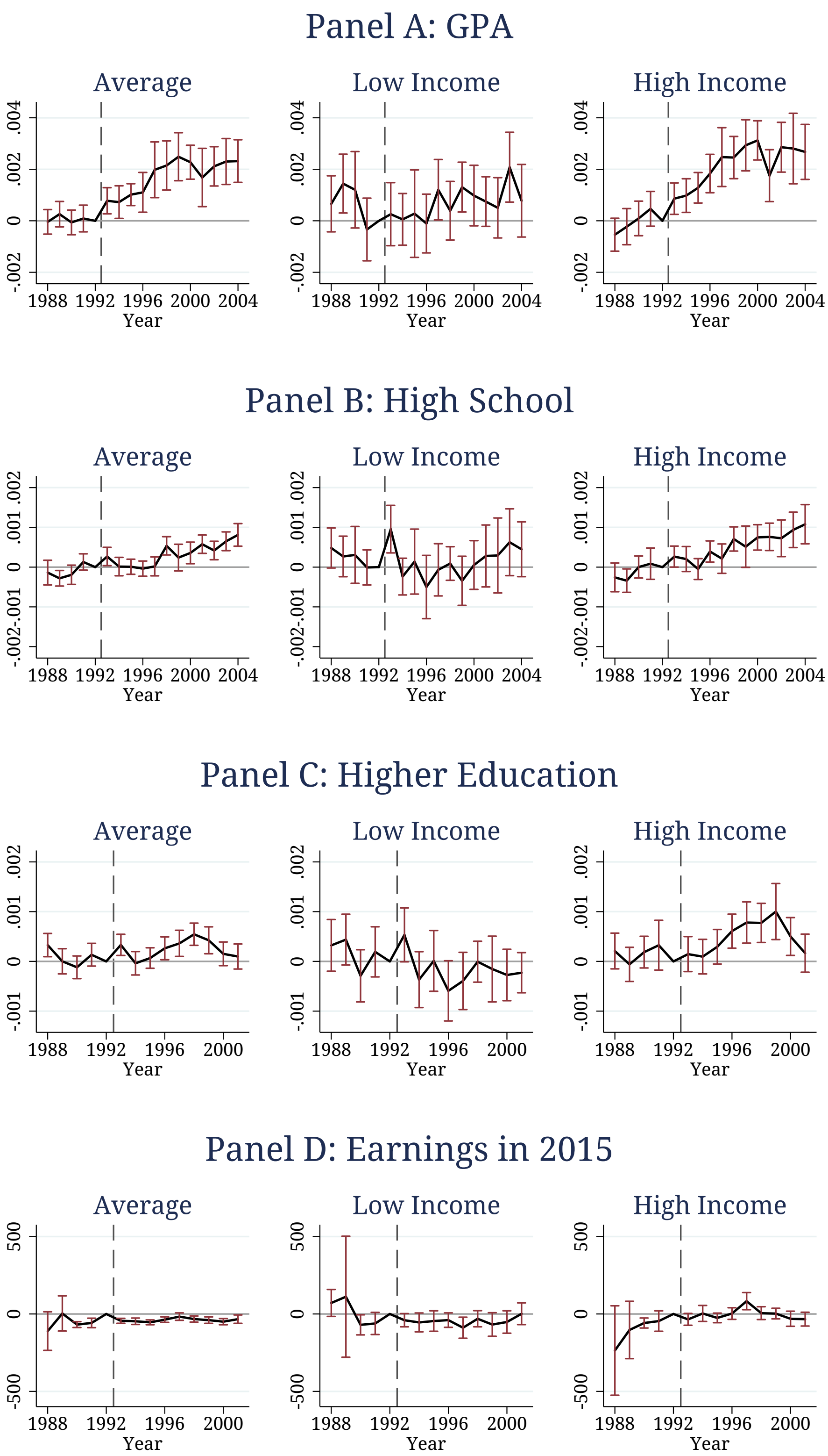
[18]The estimates of Wondratschek et al. (2013) are not directly comparable to ours as they exploit individual-level differences in school choice opportunities rather than municipal-level (market-level) differences. Thus, their identification strategy does not account for potential spillovers.

Table 3.4. Effect of school choice on education and labor market outcomes.

	GPA (1)		High School (2)		Higher Education (3)		Earnings in 2015 (4)	
	RF	IV	RF	IV	RF	IV	RF	IV
Average	0.0017***	0.0085***	0.0004***	0.0021***	0.0002*	0.0009*	7.29	38.82
	(0.0004)	(0.0012)	(0.0001)	(0.0003)	(0.0001)	(0.0004)	(15.07)	(80.51)
		[114.0]		[114.6]		[149.4]		[149.4]
N	967,525	967,525	984,219	984,219	795,184	795,184	795,184	795,184
Low-income	0.0002	0.0009	-0.0000	-0.0002	-0.0003	-0.0018*	-53.11	-324.67
	(0.0003)	(0.0016)	(0.0002)	(0.0013)	(0.0002)	(0.0008)	(45.08)	(234.48)
		[44.1]		[37.4]		[37.4]		[37.4]
N	239,745	239,745	245,976	245,976	199,272	199,272	199,272	199,272
High-income	0.0022***	0.0113***	0.0006***	0.0034***	0.0004***	0.0019**	86.96**	467.89**
	(0.0004)	(0.0014)	(0.0001)	(0.0004)	(0.0001)	(0.0006)	(30.46)	(165.89)
		[134.2]		[126.5]		[174.0]		[174.0]
N	243,152	243,152	246,063	246,063	197,034	197,034	197,034	197,034

Notes: GPA refers to the standardized value of GPA at the end of the 9th grade. High school refers to high school graduate. Higher education refers to a student with at least a bachelor level degree in 2015. Earnings in 2015 refer to sum of work- and entrepreneurship-related earnings and capital-income in 2015. RF refers to reduced form estimations and IV to instrumental variable estimations. The regressions control for cohort and municipality fixed effects, student and household-level controls, and interactions between the cohort and controls. Standard errors are clustered at the municipal-level and shown in parentheses. First stage F-tests in square brackets. *N* refers to number of observations.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$.

depending on the household income of the students. The next section will study potential mechanisms of school choice that aim to explain these findings.



Notes: Figures show the point estimates from an event-study regression and their 95% confidence intervals constructed using standard errors clustered at the municipal-level.

Figure 3.4. Effects of school choice on education and labor market outcomes: event-study specification.

3.4.3 Mechanisms of School Choice

In this section, we study potential mechanisms of school choice in order to explain the distributional effects of the previous section. We begin by studying whether school choice changed the peer quality of schools and classes in comprehensive schools. We also study whether students with different level of household income attended different peer quality schools and classes after the reform. Our analysis on these specific mechanisms is inspired by recent findings in literature that suggest that there is considerable heterogeneity in students' school choices, according to their socioeconomic status (Hastings and Weinstein 2008; Lucas and Mbiti 2012b). We describe our results for peer quality of the school and class in panels A and B of Figure 3.5 and columns 1 and 2 of Table 3.5. Appendix 3.D.4 covers additional results based on alternative definitions of peer quality: average predicted GPA and average household income decile of the school and class. No conclusions are drawn from these results because the event-study estimates show notable pre-trends.

We then continue by studying whether school choice affected selection into education and occupations, to study the long-term consequences of school choice to better understand the long-term consequences of school choices. We describe our results related to selection into education and occupation in panels C and D of Figure 3.5 and columns 3 and 4 of Table 3.5.

Peer quality of school and class—We measure changes in school's peer quality with the measure *GPA of the school*. The change in peer quality for class is measured by *GPA of the class* in school. These measures reflect the average GPAs of the school and class (within the school), respectively. These measures are calculated taking the average GPA and by leaving the student's own GPA out of the calculations

We find that the reform had a positive effect on the average GPA of the school and the average GPA of the class on average. According to our reduced form and IV estimates, average GPA of the school increased by 4pps (or 0.15 standard deviations) and average GPA of the class by 3pps (or 0.08 standard deviations) after the reform. Recall, that 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.

The results also show that students from high-income households attend both schools and classes with higher average GPA than students from low-income households. Average GPA of the school increases more for students from high-income households, 6pps, whereas the effect is also positive but barely significant and smaller, 2pps, for students from low-income households after the reform. Similarly, average GPA of the class increases 6pps for students from high-income households whereas the effects are still positive (and smaller) but insignificant for students from low-income households.

Our results are in line with Kupiainen and Hotulainen (2019), who suggest that students from higher socioeconomic status households are more likely to attend specialized programs in comprehensive school. The school choice reform in Finland popularized specialized programs that offer extra teaching hours in certain subjects, such as math or music, and into which aptitude tests could be used to determine entry Seppänen (2006). It has been suggested that specialized programs can act as a driver for increased school choice (Kalalahti et al. 2015). For example, today, most schools in Helsinki offer specialized programs in which entry is determined with an aptitude test.[19] Although it has not been established whether attending such a class improves student outcomes, student attainment in these classes is significantly higher than in normal classes at comprehensive schools (Kupiainen and Hotulainen 2019).

Based on this information, our results could thus reflect that students from high-income households are better able to access these classes than their less well-off peers, for example, because of asymmetric information or prior investments that successful passing of an aptitude test might require. Unfortunately, we are unable to investigate this hypothesis further as our data does not contain any information on whether a school offers these classes and if the student attended such a class. However, heterogeneity in the access to specialized programs does offer a compelling explanation as to why we find the benefits of school choice to be unequally distributed despite that students from all backgrounds make choices.

Selection into education and occupation—We next study whether school choice affected selection into education and occupation later in life to complement the results of the previous section on long-term labor market outcomes. For selection into education, we use the logarithm of *average earnings of the education group* in 2015.[20] For selection into occupation, we use the logarithm of *average earnings of the occupation* in 2015.[21] These outcomes reflect the earning potential of the education and occupation choice made by the student.

We only study these two outcomes for cohorts who started 7th grade between 1988 and 2001, as the later cohorts might still be studying and hence not have a degree or an occupation yet. Additionally, the latter outcome is only available for students who are employed in 2015. Thus, this outcome suffers from potential selection bias and should be interpreted with caution.

We make a puzzling finding: on average, the students attain education with higher earning potential and they also end up in an occupation with higher earning potential.

[19] See city of Helsinki's web page on specialized programs offered by each school available at `https://www.hel.fi/helsinki/en/childhood-and-education/comprehensive/what-how/painotettu/schools-offering-weghted-curriculum/`; accessed February 5th, 2020.

[20] The variable is defined as the logarithm of the average earnings of all individuals above 40 years old with the same level and field of education in Finland in 2015.

[21] This outcome is calculated as the logarithm of average earnings of all individuals above 40 years old with the same 5-digit ISCO-08 occupation code in Finland in 2015.

Our reduced form and IV estimates show that using the same quantification on the estimates as above, earnings of the education group increase on average by 3 percent whereas the earnings of the occupation decrease on average by 1 percent.

We explain these puzzling findings by the breakdown of the results with respect to household income: students from high-income households end up with an education with 4 percent higher earning potential than before the reform, whereas the effects are negative but statistically insignificant for student from low-income households. Students from high-income households also get an occupation with 1 percent higher earning potential, whereas the students from low-income households end up with an occupation with 3 percent lower earning potential.

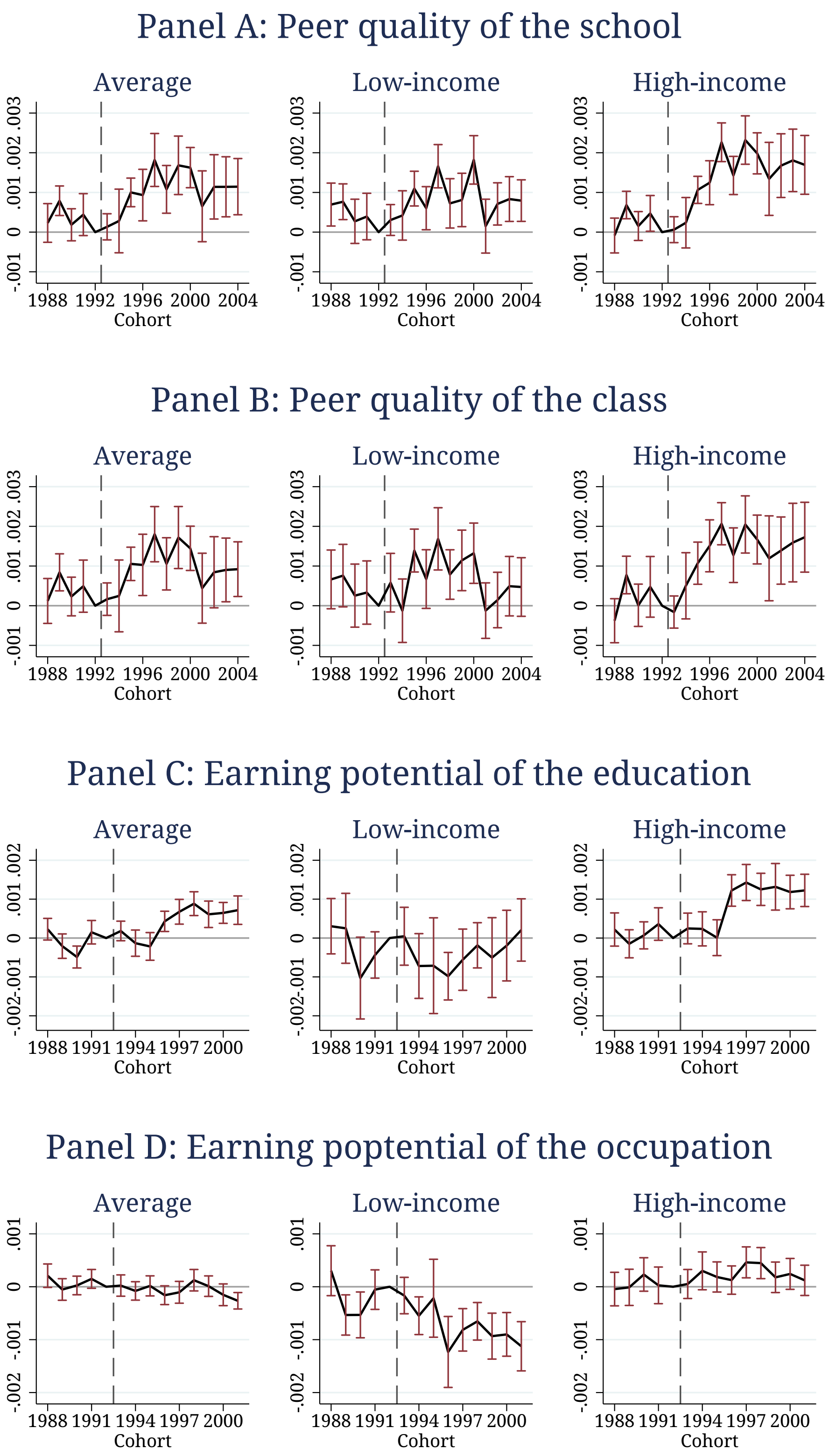
Thus, high-income students are not only more likely to attain higher education, they also acquire an education and an occupation with higher earning potential. On the other hand, students from low-income households are not only less likely to attain higher education, they end up with an occupation with lower earning potential after the reform.

To summarize, our results suggest that the positive effects on students from high-income households come at the potential expense of the students from low-income households: although students from low-income households are unaffected by the reform in the short term, they are less likely to attain higher education and end up with an occupation that has a lower earning potential. The exact mechanisms behind this remains unknown, but we speculate that this could be a result of displacement in the upper secondary school application process. As there is likely to be only a certain number of seats available in the best upper secondary schools, students from high-income households might take up a higher share of these after the reform (due to the higher grades they receive from the higher (peer) quality schools). This could restrict the education- and occupation-related choices of the less-well students later in life.

Table 3.5. Effect of school choice on peer composition and earnings potential.

	GPA of the School (1)		GPA of the Class (2)		Average Earnings of the Education Group (3)		Average Earnings of the Occupation (4)	
	RF	IV	RF	IV	RF	IV	RF	IV
Average	0.0007***	0.0036***	0.0006**	0.0031***	0.0005***	0.0026***	-0.0001**	-0.0007***
	(0.0002)	(0.0008)	(0.0002)	(0.0009)	(0.0001)	(0.0004)	(0.0000)	(0.0002)
		[112.3]		[111.8]		[149.6]		[211.0]
N	980,671	980,671	978,393	978,393	795,166	795,166	635,660	635,660
Low-income	0.0004**	0.0023*	0.0003	0.0016	-0.0002	-0.0012	-0.0006***	-0.0030***
	(0.0002)	(0.0009)	(0.0002)	(0.0013)	(0.0001)	(0.0007)	(0.0001)	(0.0005)
		[38.9]		[42.1]		[37.5]		[87.9]
N	244,681	244,681	243,609	243,609	199,266	199,266	149,096	149,096
High-income	0.0012***	0.0062***	0.0012***	0.0060***	0.0008***	0.0043***	0.0002**	0.0010*
	(0.0002)	(0.0008)	(0.0002)	(0.0009)	(0.0001)	(0.0007)	(0.0001)	(0.0004)
		[122.9]		[120.3]		[174.2]		[185.2]
N	245,548	245,548	245,286	245,286	197,029	197,029	164,647	164,647

Notes: GPA of the class refers to average end of 9th grade GPA of the school. GPA of the class refers to the average 9th grade GPA of the class. Average earnings of the education group refers to the log of average earnings in 2015 of everyone above 40 years old with the same level and field of education. Average earnings of the occupation refers to the log of average earnings in 2015 of everyone above 40 years old with the same occupation. RF refers to reduced form estimations and IV to instrumental variable estimations. The regressions control for municipality fixed effects, student- and household-level controls, and interactions between the cohort and controls. Standard errors are clustered at the municipal-level and shown in parentheses. First stage F-tests in square brackets. N refers to number of observations.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$.



Notes: Figures shows point estimates from an event-study regression and their 95% confidence intervals constructed using standard errors clustered at the municipal-level.

Figure 3.5. Effects of school choice on peer composition and earnings potential: event-study specification.

3.5 Discussion

We study the overall effects of choice between public schools on students' education and labor market outcomes. We find the average effect of school choice to be positive but unequally distributed, despite students from different socioeconomic groups making choices. These findings may help to explain some of the mixed findings of previous empirical literature, which range from small positive effects on various student outcomes (Böhlmark and Lindahl 2015; Cullen et al. 2006; Deming et al. 2014; Hsieh and Urquiola 2006; Muralidharan and Sundararaman 2015; Sandström and Bergström 2005; Wondratschek et al. 2013, for example) to sizeable negative impacts on test scores (Abdulkadiroglu et al. 2018).

Students from high-income households seem to receive better grades as a result of attending higher peer quality schools.[22] Because these grades are used in upper secondary school applications, students from high-income households are more likely to graduate from high school than before the reform. These short-term gains of school choice can translate not only into better opportunities to attain higher education, but may also improve the chances of obtaining an education with higher earning potential later in life. At the same time, students from low-income households are unaffected by the reform in the short term, but they are less likely to attain higher education and end up in occupations with lower earning potential later in life. Together our results suggest that the reform had spillover effects on students from low-income households.

The finding that students from high-income households attend schools and classes with higher peer quality after the reform relative to students from low-income households could be explained by differential preferences for schools. These findings become problematic if instead of differential preferences, they reflect asymmetric information about choice opportunities or other constraints faced by socioeconomically disadvantaged students. Our findings can emphasize the importance of school counseling and decision support for students applying to upper secondary schools. Further research on the effects of school choice on the upper secondary education school application process would be interesting.

Our results show surprisingly large effects in Finland, despite small quality differences between Finnish schools in international comparisons (OECD 2013). Consequently, these effects can potentially be even greater in countries with greater quality differences between schools and more diverse populations. The effects of school choice on segregation is left for Essay 3 of this dissertation.

[22] This reasoning is in line with previous findings by Deming et al. (2014). They find that the benefits of winning a choice lottery in Charlotte-Mecklenburg's public schools are predicted by gains in various school quality measures, such as peer quality, of the lottery winners.

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Appendix

This document contains auxiliary materials to the paper "Market-Level and Distributional Effects of Public School Choice". Appendix A provides our data appendix. We discuss the construction of the treatment intensity variable in Appendix B. In Appendix C, we discuss the construction of our outcome and control variables and report summary statistics. In Appendix D, we show additional figures and tables related to our main results in Section 3.4. Finally, we test the sensitivity of our results in Appendix E.

3.A Data Appendix

We combine administrative data from four sources. The first data set is Centralized Secondary School Application and Admissions Data, which contains information on everyone who applies to upper secondary schools (either academic track, i.e. high school, or vocational track), including all 9th graders, regardless of whether they have finished their comprehensive school education or not, and regardless of whether they apply to upper secondary schools.

From these data, we take all the 9th graders who turned 16 in 1991–2007 because we want to focus on students who started their 7th grade between 1988-2004. Thus, we drop everyone who is repeating 9th grade or doing 10th grade, as these same students appear in our data the year before.[23] We also drop a small number of students who started their comprehensive school education a year earlier (at age 6), started a year later (at age 8), skipped a grade, or redid a grade other than the 9th grade. This is because we do not know for sure when these students started 7th grade.

These data include information on the school and class attended, completion year of comprehensive school education, upper secondary schools applied to, upper secondary school place received (either in academic or vocational track, or no place received), and grades at the end of 9th grade. These data also include information on student characteristics: place of residence, gender, native language (Finnish, Swedish, or other). We keep only students with a school code because the school code is needed for our empirical analysis.[24]

Our second set of data is called the Finnish Longitudinal Employer-Employee Data (FLEED). These data contain everyone aged 15 to 70 and run from 1988 to 2015. The data contain individual-level information on education and labor market outcomes, and links the employed individuals to the industries and establishments they work at. Outcomes in these data include (but are not limited to) high school completion, highest education level achieved (6-digit ISCED-1997 code) and the year of completion, income, work-, capital- and entrepreneurship-related earnings, benefits received (such as maternity leave, social or unemployment benefits), employment status (employed, unemployed, out of labor force), occupation (5-digit ISCO-08 code), and other individual-level characteristics such as gender, native language, and date of birth, number of children, and marital status.

Our third set of data contains information on household characteristics for each student when the students are 14–16 year old: highest education level, income, earnings,

[23]If the requirements for comprehensive school education have not been met by the end of 9th grade, the student may have to repeat that grade or do an extra grade, called the 10th grade. After this, the student may be exempted from finishing comprehensive school even if she/he has not successfully completed it.

[24]To the best of our knowledge, these students without a school code may have completed their education abroad, be home-schooled and/or have spent several years in hospital schools.

and employment status of the parent(s), and whether the student lives in single-parent household. We use the information from the year the students turn 14. This data set has been constructed for us by Statistics Finland using household identifiers in FLEED.

Our fourth set of data is called the Grid Database, and it contains the yearly residential locations of the students from 1988 onward on 250m by 250m, 1km by 1km, postal code, and municipal-level.

Final Sample—Our final sample consists of all Finnish students who are 16 years old at the end of their comprehensive school education (or who turned 16 this year). These students entered lower secondary school (7th grade, aged 13) between 1988 and 2004, and this means that we have data on five cohorts before the reform. We keep only the students living in mainland Finland because there is missing information on the students who live in the autonomous and demilitarised region of Finland, called the Åland Islands. Only 0.5 percent of the population lives in the Åland Island.

Our sample period coincides with 36 municipal mergers. In this case, we merge these municipalities from the first cohort onward. This can cause measurement errors in the number of schools in each municipality. However, we consider this choice in data construction to be of a minor concern.

Altogether we have 984,478 observations, between 54 to over 60 thousand students per cohort, living in 399 different municipalities, and all municipalities have at least one student per cohort.

3.B Construction of the Treatment Intensity Measure

In our difference-in-differences identification strategy, we use the average number of comprehensive schools that have grades 7 to 9 in a municipality between 1988 and 1992 to capture the pre-reform variation in school choice possibilities and thus the potential intensity of the reform.

The construction of this measure has two minor caveats. First, we only include schools with 5 or more students. In large municipalities, schools with fewer than 5 students are likely to be hospital schools or schools for the disabled, and hence not a real choice possibility. For example, Helsinki has around 10 small schools. In small municipalities, these can be actual schools. We make this choice because including these schools would overestimate the number of schools in large municipalities more than it would underestimate it in smaller municipalities.

Second, we merge all 36 municipalities that underwent a merger during our sample period from the first cohort onwards.[25] Because of these caveats our choices for data construction can lead to a slight overestimation of the intensity of the reform in these municipalities.

3.C Variables and Summary Statistics

First Stage Outcome Variables—Our main first stage outcome variable is *non-neighborhood school*. It takes the value 100 if the student attends a school other than the neighborhood school (i.e. made a choice), and 0 otherwise. We define the neighborhood school as the most commonly attended school in a 1km by 1km grid where the student lives. We use the 1km by 1km grid because we do not have the residential location at 250m by 250m grid level for all municipalities and some of these 250m by 250m grids have too few students to make a reasonable inference about the most commonly attended school in the area. If the 1km by 1km grid is not available to us (due to very few students living in that grid that year) we use a postal code.

We define an alternative first stage outcome variable *non-neighborhood school 30* similarly: it takes the value 100 if the student attends a school other than the neighborhood school, and 0 otherwise. Our definition of a neighborhood school in this case is a school that at least 30 percent of the students in the grid (or postal code) attend. This way, several schools can be counted as neighborhood schools.

Our third first stage outcome variable *mobility index*. It measures the mobility of students in 1km by 1km grid each year, or in the case of too few observations per grid, we use a postal code. The index 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/postal code as student i. The index varies between 0 and 1, where 1 means that the student attends a school other than the rest of the students who reside in the same grid that year.

Education and Labor Market Outcomes—*GPA* at the end of comprehensive education (9th grade), the grade point average of theoretical subjects, ranges from 4 (failed) to 10 (outstanding). The GPA is a subjective measure of attainment as there is no country-wide standardized testing at comprehensive school level. All grades are given by teachers but teachers follow a nationally set guideline for student assessment. For the empirical analysis, we standardize this measure to have a mean of 0 and standard deviation of 1. For each cohort, there are between 200 to fewer than 2000 missing GPA observations. Also, for the cohort of 1994, we drop altogether 10 municipalities from our empirical analysis (on the effects of the reform on GPA) due to several missing GPA observations.

[25]These mergers are mostly between small municipalities with fewer than 2 schools or small municipalities merging with a larger municipality.

High school takes value 1 if the student has completed high school education (academic track of upper secondary school) before or during 2015. Because not all of the students can be linked to their education and labor market outcomes FLEED in 2015 (due to emigration or death), we check whether these individuals appear in the previous years of our data to obtain the information on high school graduation but respecting that it takes (usually) at least three years to complete high school. This way we recover this information for almost our entire sample.

Higher education takes a value 1 if the student has a Bachelor (i.e. college) level or higher education in 2015. Because of the young age of the later cohorts, this information is retrieved only for the cohorts who started 7th grade between 1988 and 2001. This gives the students at least 11 years to achieve this after finishing comprehensive school. If the student is not in FLEED in 2015, this variable is coded as missing.

Earnings in 2015 are the sum of work, entrepreneurship-related, and capital earnings of the student in 2015. As with the previous outcome, this measure is only retrieved for cohorts between 1988-2001 because of the young age of the later cohorts.[26] Again, if the student is not in our data in 2015, this variable is coded as missing.

Measures for School Choice Mechanism Analysis—Average GPA of the school is the average GPA of the comprehensive school, calculated using school-level average of students' GPAs without student's own GPA. For students with a missing GPA, we calculate the average GPA of the school (without missing observations). This is measured using the unstandardized measure of GPA that ranges from 4 (failed) to 10 (outstanding). Again, for the cohort of 1994, we drop 10 municipalities from our empirical analysis (on the effects of the reform on GPA) altogether due to several missing GPA observations.

Average GPA of the class is the average GPA of the class in a comprehensive school that the student attends. We calculate it in the same way as average GPA of the school: taking the class-level average of students' GPAs but leaving out student's own GPA from the calculations. The variable that defines the class in our data has 387 potentially faulty entries (with non-number, non-letter, or missing codes)[27]. We keep the potentially faulty class entries, as some of these might be actually refer to true classes[28]. As for the previous outcome variable, we drop 10 municipalities for the cohort of 1994 from our empirical analysis (on the effects of the reform on GPA) due to several missing GPA observations.

The logarithm of *average earnings of the occupation in 2015* is the average earnings (work, entrepreneurship-related, and capital earnings) in 2015 euros of everyone above

[26] In Section 3.D.5, we show how the share of zero-earners is higher for the later cohorts.

[27] Usually the class codes in our data include a number 9 followed by a letter, such as 9A, or vice versa.

[28] For example, for many of the potentially faulty class entries in our data, there are more than one observation of the same entry per school and cohort, indicating that this might actually be a class that the students attended.

40 years old with the same 5-digit ISCO-08 occupation code in FLEED in 2015. In other words, we use the entire labor force sample in Finland in 2015 to measure the average earnings for each occupation code. This measure is only available for employed individuals in 2015. Thus, students that are unemployed in 2015, including those not present in FLEED that year, this outcome variable is coded as missing. For our empirical analysis, we take the logarithm of this value, but for the sake of transparency table C3 shows these without it. This outcome is only retrieved for cohorts who started 7th grade between 1988 and 2001 because of the young age of the students in later cohorts.

We define the logarithm of *average earnings of the education group in 2015* analogously to the previous outcome variable. Instead of the occupation code, we measure this by the field and level of education (the first 2 digits of the education code) for everyone above 40 years old with the same 2-digit education code in the labor force in 2015. Students without formal education after comprehensive education are coded as one "education" group. This outcome is only available for those present in our administrative data in 2015 and for cohorts who started 7th grade between 1988 and 2001. For everyone else, it is coded as missing. For the empirical analysis, we take the logarithm of this value, but table C3 shows these without it.

Tables C1, C2, and C3 summarize our first stage variables and outcome variables before and after the reform, and in small (fewer than 10 schools) and large (more than 10 schools) municipalities.

Table C1. Descriptive statistics of first stage outcome variables

	At least 10 Schools		Fewer than 10 schools	
	1988–1992	1993–2004	1988–1992	1993–2004
	(1)	(2)	(3)	(4)
Share attending non-neighborhood school				
Average	29.89	38.21	9.40	11.56
	(45.78)	(48.59)	(29.18)	(31.98)
Low-income	34.28	42.95	10.17	13.09
	(47.47)	(49.50)	(30.22)	(33.72)
High-income	30.06	38.48	10.75	12.81
	(45.85)	(48.66)	(30.98)	(33.42)
Share attending non-neighborhood school 30				
Average	26.71	36.55	7.78	9.53
	(44.25)	(48.16)	(26.78)	(29.36)
Low-income	31.62	41.91	8.66	11.15
	(46.50)	(49.34)	(28.13)	(31.48)
High-income	27.14	36.79	8.80	10.50
	(44.47)	(48.23)	(28.33)	(30.65)
Average Mobility Index				
Average	0.431	0.525	0.113	0.135
	(0.315)	(0.308)	(0.225)	(0.244)
Low-income	0.464	0.561	0.101	0.130
	(0.330)	(0.315)	(0.226)	(0.253)
High-income	0.434	0.529	0.142	0.165
	(0.315)	(0.306)	(0.244)	(0.257)

Notes: All values are means calculated over all individuals who live in municipalities with fewer than 10 or 10 or more schools before the reform and calculated separately for all cohorts before and after the reform. Standard errors are in parentheses, and clustered at municipal level.

Table C2. Descriptive statistics of education and labor market outcome variables

	At least 10 Schools		Fewer than 10 schools	
	1988–1992	1993–2004	1988–1992	1993–2004
	(1)	(2)	(3)	(4)
GPA				
Average	0.100	0.121	-0.032	-0.040
	(0.990)	(1.001)	(1.001)	(0.997)
Low-income	-0.257	-0.278	-0.271	-0.273
	(1.000)	(1.013)	(1.000)	(0.995)
High-income	0.430	0.475	0.381	0.334
	(0.894)	(0.898)	(0.923)	(0.935)
High School				
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)
High-income	0.80	0.81	0.74	0.73
	(0.40)	(0.39)	(0.44)	(0.45)
Higher Education				
Average	0.46	0.39	0.43	0.38
	(0.50)	(0.49)	(0.50)	(0.49)
Low-income	0.31	0.23	0.32	0.27
	(0.46)	(0.42)	(0.47)	(0.45)
High-income	0.61	0.53	0.62	0.54
	(0.49)	(0.50)	(0.49)	(0.50)
Earnings in 2015				
Average	38,905	24,812	35,448	24,823
	(160562)	(33,268)	(75,310)	(23,612)
Low-income	32,697	20,469	30,755	22,136
	(225,146)	(24,176)	(33,105)	(23,196)
High-income	46,539	27,972	44,130	28,065
	(192,677)	(41,839)	(155,142)	(28,541)

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 clustered at municipal-level and shown in parentheses.

Table C3. Descriptive statistics of channels of choice -outcomes

	At least 10 Schools		Fewer than 10 schools	
	1988–1992	1993–2004	1988–1992	1993–2004
	(1)	(2)	(3)	(4)
Average GPA of the school				
Average	7.83	7.79	7.67	7.60
	(0.27)	(0.32)	(0.20)	(0.22)
Low-income	7.78	7.73	7.66	7.59
	(0.27)	(0.33)	(0.20)	(0.22)
High-income	7.89	7.88	7.69	7.63
	(0.28)	(0.32)	(0.20)	(0.22)
Average GPA of the class in school				
Average	7.82	7.79	7.67	7.61
	(0.42)	(0.50)	(0.35)	(0.40)
Low-income	7.74	7.68	7.64	7.59
	(0.44)	(0.52)	(0.36)	(0.40)
High-income	7.92	7.93	7.72	7.67
	(0.42)	(0.48)	(0.36)	(0.40)
Average earnings of the education group in 2015				
Average	31,942	28,561	30,245	27,324
	(18,665)	(16,911)	(16,738)	(15,130)
Low-income	25,851	22,606	26,352	23,797
	(15,810)	(13,676)	(14,705)	(13,176)
High-income	38,085	34,272	37,293	33,096
	(20,304)	(18,692)	(19,043)	(17,594)
Average earnings of the occupation in 2015				
Average	39,847	32,199	37,882	31,495
	(26,241)	(22,176)	(23,361)	(20,435)
Low-income	33,377	26,668	33,647.055	28,053
	(23,618)	(19,887)	(21,434	(19,043)
High-income	45,968	36,697	44,740	36,227
	(28,850)	(24,626)	(27,002)	(23,326)

Notes: All values are means calculated over all individuals who live in municipalities with fewer than 10 or 10 or more schools before the reform and calculated separately for all cohorts before and after the reform. The average earnings is the sum of all work- and entrepreneurship-related earnings of everyone above 40 years old with the same field and level of education or occupation in 2015 in euros. Standard errors are in parentheses and clustered at municipal level.

Control variables—Earnings of a parent measures all the work-related earnings (in 2015 euros), whereas household income is the sum of parents' (single or both) earnings and all non-work related income (such as maternity leave, social or unemployment benefits). Household income excludes capital income, as this information is not available to us. For parental earnings and household income, we choose to code missing values as zero.

Education of a parent is an indicator variable that takes three possible values: no formal education after comprehensive school, upper secondary schooling (high school or vocational), or higher education (Bachelor level or higher). Unemployment 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 with only 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 household characteristics are measured the year the student turns 14 because this is the first year this information is available to us.

On a student-level, we control for the native language of the student, and this can take three values: Finnish, Swedish, or other. In the table C4, the foreign native language dummy takes the value 0 if student speaks Finnish or Swedish, and 1 otherwise. In addition to these variables, we control for the gender of the student. Gender does not differ significantly between socioeconomic groups, small and large municipalities, or before and after the reform. These student-level control variables are measured the year the student turns 16, as this information is retrieved from the Centralized Secondary School Application and Admissions Data described in 3.A. We also control for the cohort the student started 7th grade and municipality of residence when student turned 13 (obtained from teh Grid Database).

Table C4 summarizes our control variables used in our regressions, before and after the reform, and in small (fewer than 10 schools) and large (more than 10 schools) municipalities.

Table C4. Descriptive statistics of control variables

	At least 10 Schools		Fewer than 10 schools	
	1988–1992	1993–2004	1988–1992	1993–2004
	(1)	(2)	(3)	(4)
Average household income (in 2015 euros)				
Average	63,170	72,629	49,556	57,494
	(40,763)	(71,414)	(29,919)	(47,757)
Low-income	24,391	26,430	24,822	26,955
	(9,311)	(12,662)	(8,895)	(12,051)
High-income	94,201	114,079	84,457	98,712
	(48,095)	(101,028)	(45,778)	(84,613)
Mother's earnings (in 2015 euros)				
Average	21,831	26,001	15,979	19,618
	(14,464)	(22,542)	(12,460)	(16,707)
Low-income	10,809	11,145	7,520	8,444
	(10,397)	(12,765)	(8,971)	(11,081)
High-income	29,526	37,411	25,761	32,177
	(15,864)	(28,077)	(14,360)	(20,131)
Father's earnings (in 2015 euros)				
Average	33,111	38,122	22,671	27,225
	(28,118)	(61,507)	(20,525)	(42,697)
Low-income	6,006	5,770	6,065	6,162
	(9,615)	(10,390)	(9,461)	(10,623)
High-income	54,250	66,854	44,440	54,136
	(30,434)	(90,893)	(25,035)	(80,754)
Mother secondary or higher educated				
Average	0.679	0.786	0.659	0.804
	(0.467)	(0.410)	(0.474)	(0.397)
Low-income	0.517	0.615	0.541	0.693
	(0.500)	(0.487)	(0.498)	(0.461)
High-income	0.824	0.906	0.832	0.910
	(0.380)	(0.291)	(0.374)	(0.285)
Father secondary or higher educated				
Average	0.669	0.733	0.603	0.710
	(0.471)	(0.442)	(0.489)	(0.454)
Low-income	0.400	0.461	0.428	0.538
	(0.490)	(0.498)	(0.495)	(0.499)
High-income	0.856	0.903	0.843	0.883
	(0.351)	(0.297)	(0.364)	(0.322)

Table continued on the following page.

Table C4. *(continued)* Descriptive statistics of control variables

	At least 10 Schools		Fewer than 10 schools	
	1988–1992	1993–2004	1988–1992	1993–2004
	(1)	(2)	(3)	(4)
Unemployed mother				
Average	0.041	0.084	0.056	0.100
	(0.197)	(0.277)	(0.231)	(0.300)
Low-income	0.107	0.229	0.101	0.200
	(0.309)	(0.420)	(0.302)	(0.400)
High-income	0.014	0.021	0.017	0.023
	(0.116)	(0.144)	(0.130)	(0.150)
Unemployed father				
Average	0.053	0.081	0.057	0.075
	(0.223)	(0.273)	(0.232)	(0.263)
Low-income	0.146	0.255	0.116	0.188
	(0.354)	(0.436)	(0.320)	(0.391)
High-income	0.008	0.008	0.007	0.004
	(0.089)	(0.088)	(0.083)	(0.066)
Single parent				
Average	0.275	0.345	0.198	0.256
	(0.447)	(0.475)	(0.398)	(0.436)
Low-income	0.589	0.651	0.338	0.412
	(0.492)	(0.477)	(0.473)	(0.492)
High-income	0.158	0.201	0.115	0.158
	(0.364)	(0.401)	(0.319)	(0.365)
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)	(0.074)	(0.134)
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 fewer than 10 or more than 10 schools before the reform. The means are calculated separately for all cohorts before and after the reform. *Average household income* is an average of the sum of all work- and entrepreneurship-related earnings of the household in addition to the benefits, such as maternity leave, social or unemployment benefits, received by the household. *Student has a foreign language* is defined as a student whose native language is not Finnish or Swedish. Standard errors are in parentheses and clustered at municipal-level.

3.D Additional Figures and Tables

Did Students Make Choices After the Reform? Here we present some descriptive evidence on whether students make choices after the reform. Figure D1 shows the development of our second proxy for school choice, *non-neighborhood school 30*, in three major cities in Finland.

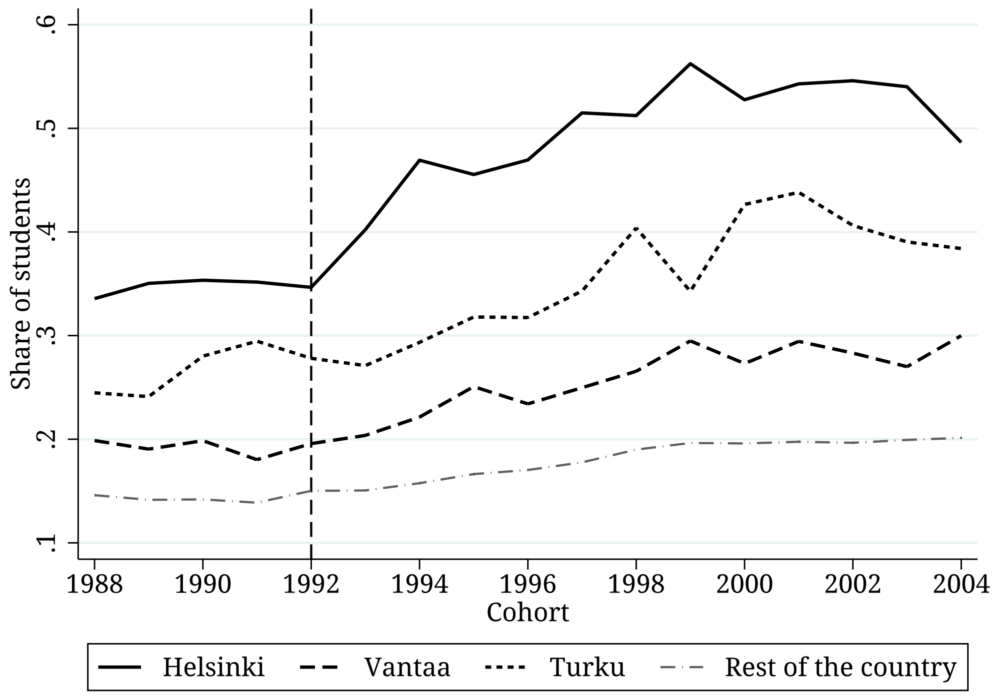
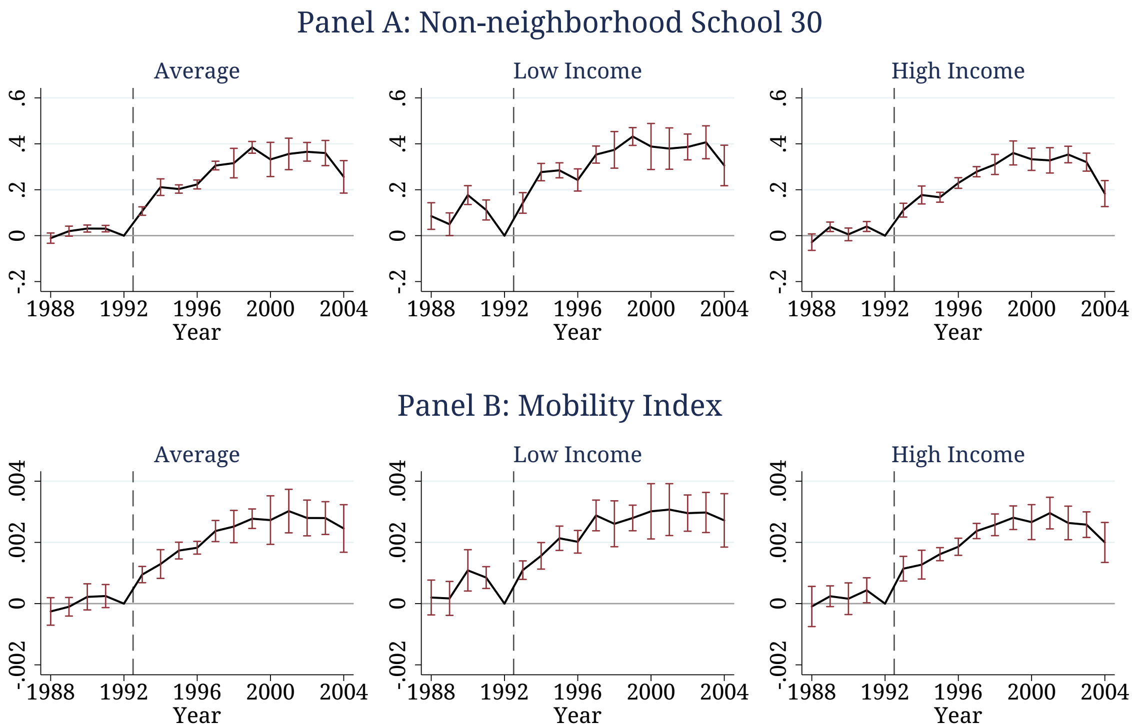


Figure D1. The development of the share of students attending a school other than the school that at least 30percent of the students of the region attend in Helsinki, Turku and Vantaa.

3.D.1 First Stage Results with Alternative Proxies for School Choice

This subsection shows our first stage results using the two alternative measures for school choice (Figure D2).



Notes: The outcome of interests are non-neighborhood school 30 that measures if the student attends a school other than the school that 30 percent of the students of the region attend and mobility index that measures the mobility of the student compared to other students living in the same region. Figures show the coefficient estimates from the event-study regression and their 95% confidence intervals constructed using standard errors clustered at the municipal-level. The gray dashed line marks the school choice reform.

Figure D2. Event-study specification for the first stage with two alternative proxies for school choice.

3.D.2 Reduced Form Results for All Specifications and Income Groups

In here we present our results based on the reduced form specification in Equation 3.2 with all three income groups(low, middle, and high income) and by gradually adding student-level background characteristics.

Table D1. First stage results.

	(1)	(2)	(3)
Panel A: Non-neighborhood school			
Average	0.2106***	0.2032***	0.1985***
	(0.0225)	(0.0200)	(0.0185)
N	984,478	984,478	984,478
Low-income	0.1969***	0.1780***	0.1736***
	(0.0354)	(0.0291)	(0.0282)
N	246,099	246,099	246,099
Middle-income	0.2226***	0.2152***	0.2157***
	(0.0185)	(0.0174)	(0.0168)
N	492,277	492,277	492,277
High-income	0.1910***	0.1915***	0.1919***
	(0.0194)	(0.0185)	(0.0170)
N	246,102	246,102	246,102
Panel B: Non-neighborhood school 30			
Average	0.2821***	0.2739***	0.2717***
	(0.0208)	(0.0181)	(0.0164)
N	984,478	984,478	984,478
Low-income	0.2700***	0.2498***	0.2486***
	(0.0316)	(0.0244)	(0.0237)
N	246,099	246,099	246,099
Middle-income	0.3077***	0.3002***	0.3028***
	(0.0138)	(0.0127)	(0.0135)
N	492,277	492,277	492,277
High-income	0.2491***	0.2495***	0.2512***
	(0.0196)	(0.0189)	(0.0167)
N	246,102	246,102	246,102

Table continued on the following page.

Table D1. *(continued)* First stage results.

Panel C: Mobility Index			
Average	0.0024***	0.0023***	0.0023***
	(0.0003)	(0.0003)	(0.0003)
N	959,442	959,442	959,442
Low-income	0.0023***	0.0021***	0.0020***
	(0.0005)	(0.0004)	(0.0004)
N	242,255	242,255	242,255
Middle-income	0.0026***	0.0025***	0.0025***
	(0.0003)	(0.0003)	(0.0003)
N	482,336	482,336	482,336
High-income	0.0021***	0.0021***	0.0021***
	(0.0003)	(0.0003)	(0.0003)
N	234,851	234,851	234,851
FE	Yes	Yes	Yes
Controls	No	Yes	Yes
Interactions	No	No	Yes

Notes: Non-neighborhood school proxies whether the student attends a school other than the most common school of the region. Non-neighborhood school 30 proxies whether the student attends a school other than the school that at least 30% of the region's students attend. Mobility index 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/postal code as student i. FE refers to cohort and municipality fixed effects. Controls refers to student and household-level controls. Interactions refers to interactions between the cohort and the controls. Standard errors are clustered at municipal-level and are shown in parentheses. N refers to number of observations.

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

Table D2. Effect of school choice on education and labor market outcomes.

	(1)	(2)	(3)
Panel A: GPA			
Average	0.0006**	0.0011***	0.0017***
	(0.0002)	(0.0003)	(0.0004)
N	967,525	967,525	967,525
Low-income	-0.0005	-0.0002	0.0002
	(0.0005)	(0.0003)	(0.0003)
N	239,745	239,745	239,745
Middle-income	0.0018***	0.0017***	0.0019***
	(0.0005)	(0.0005)	(0.0005)
N	484,628	484,628	484,628
High-income	0.0022***	0.0022***	0.0022***
	(0.0004)	(0.0004)	(0.0004)
N	243,152	243,152	243,152
Panel B: High School			
Average	-0.0001	0.0001	0.0004***
	(0.0001)	(0.0001)	(0.0001)
N	984,219	984,219	984,219
Low-income	-0.0004	-0.0004	-0.0000
	(0.0004)	(0.0003)	(0.0002)
N	245,976	245,976	245,976
Middle-income	0.0004***	0.0002***	0.0004***
	(0.0001)	(0.0001)	(0.0001)
N	492,180	492,180	492,180
High-income	0.0006***	0.0006***	0.0006***
	(0.0001)	(0.0001)	(0.0001)
N	246,063	246,063	246,063

Table continued on the following page.

Table D2. *(continued)* Effect of school choice on education and labor market outcomes.

Panel C: Higher Education			
Average	-0.0003*	-0.0002	0.0002*
	(0.0001)	(0.0001)	(0.0001)
N	795,184	795,184	795,184
Low-income	-0.0006	-0.0006**	-0.0003
	(0.0004)	(0.0002)	(0.0002)
N	199,272	199,272	199,272
Middle-income	0.0001	0.0001	0.0002*
	(0.0001)	(0.0001)	(0.0001)
N	398,878	398,878	398,878
High-income	0.0004***	0.0003**	0.0004***
	(0.0001)	(0.0001)	(0.0001)
N	197,034	197,034	197,034
Panel D: Earnings in 2015			
Average	-68.59**	-64.71*	7.29
	(24.95)	(25.03)	(15.07)
N	795,184	795,184	795,184
Low-income	-66.98	-58.58	-53.11
	(45.26)	(41.93)	(45.08)
N	199,272	199,272	199,272
Middle-income	-39.60***	-38.64***	-35.07***
	(10.26)	(10.04)	(9.00)
N	398,878	398,878	398,878
High-income	12.26	7.07	86.96**
	(32.24)	(32.78)	(30.46)
N	197,034	197,034	197,034
FE	Yes	Yes	Yes
Controls	No	Yes	Yes
Interactions	No	No	Yes

Notes: FE refers to cohort and municipality fixed effects. Background refers to student and household-level controls. Interactions refers to interactions between the cohort and the controls. Standard errors are clustered at the municipality level and are shown in parentheses. N refers to number of observations.
* p<0.05, ** p<0.01, *** p<0.001.

Table D3. Effect of school choice on peer composition and earnings potential.

	(1)	(2)	(3)
Panel A: GPA of the School			
Average	0.0007***	0.0007***	0.0007***
	(0.0002)	(0.0002)	(0.0002)
N	980,671	980,671	980,671
Low-income	0.0003	0.0003	0.0004**
	(0.0002)	(0.0002)	(0.0002)
N	244,681	244,681	244,681
Middle-income	0.0006*	0.0006*	0.0006*
	(0.0003)	(0.0003)	(0.0003)
N	490,442	490,442	490,442
High-income	0.0012***	0.0012***	0.0012***
	(0.0002)	(0.0002)	(0.0002)
N	245,548	245,548	245,548
Panel B: GPA of the Class			
Average	0.0007**	0.0007**	0.0006**
	(0.0002)	(0.0002)	(0.0002)
N	978,393	978,393	978,393
Low-income	0.0000	0.0000	0.0003
	(0.0002)	(0.0002)	(0.0002)
N	243,609	243,609	243,609
Middle-income	0.0004	0.0004	0.0005
	(0.0003)	(0.0003)	(0.0003)
N	489,498	489,498	489,498
High-income	0.0013***	0.0013***	0.0012***
	(0.0002)	(0.0002)	(0.0002)
N	245,286	245,286	245,286

Table continued on the following page.

Table D3. *(continued)* Effect of school choice on peer composition and earnings potential.

Panel C: Average Earnings of the Education Group			
Average	-0.0000	0.0001	0.0005***
	(0.0001)	(0.0001)	(0.0001)
N	795,166	795,166	795,166
Low-income	-0.0006	-0.0005**	-0.0002
	(0.0004)	(0.0002)	(0.0001)
N	199,266	199,266	199,266
Middle-income	0.0004***	0.0004***	0.0005***
	(0.0001)	(0.0001)	(0.0001)
N	398,871	398,871	398,871
High-income	0.0009***	0.0008***	0.0008***
	(0.0001)	(0.0001)	(0.0001)
N	197,029	197,029	197,029
Panel D: Average Earnings of the Occupation			
Average	-0.0005***	-0.0005***	-0.0001**
	(0.0001)	(0.0001)	(0.0000)
N	635,660	635,660	635,660
Low-income	-0.0008***	-0.0007***	-0.0006***
	(0.0002)	(0.0001)	(0.0001)
N	149,096	149,096	149,096
Middle-income	-0.0003***	-0.0003***	-0.0002**
	(0.0001)	(0.0001)	(0.0001)
N	321,917	321,917	321,917
High-income	0.0001	0.0000	0.0002**
	(0.0001)	(0.0001)	(0.0001)
N	164,647	164,647	164,647
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 household-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. *N* refers to number of observations.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$.

3.D.3 IV Results Based on Alternative Choice Proxy

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, *non-neighborhood school 30*. Results are shown in Table D4.

Table D4. Effect of school choice education and labor market outcomes and channels of choice using IV with non-neighborhood school 30 as a proxy for choice.

	GPA	High School	Higher Education	Earnings in 2015	GPA of the School	GPA of the Class	Earnings of Education Group	Earnings of Occupation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Average	0.0062***	0.0016***	0.0007*	28.14	0.0025***	0.0023***	0.0019***	-0.0005***
	(0.0010)	(0.0002)	(0.0003)	(58.33)	(0.0007)	(0.0007)	(0.0003)	(0.0001)
	[267.9]	[274.1]	[388.2]	[388.2]	[261.1]	[263.4]	[389.0]	[484.8]
N	967,525	984,219	795,184	795,184	980,375	978,393	795,166	635,660
Low-income	0.0006	-0.0002	-0.0012*	-224.35	0.0014*	0.0011	-0.0008	-0.0022***
	(0.0011)	(0.0009)	(0.0006)	(174.19)	(0.0007)	(0.0009)	(0.0005)	(0.0004)
	[131.2]	[109.0]	[127.6]	[127.6]	[113.3]	[120.6]	[127.7]	[343.7]
N	239,745	245,976	199,272	199,272	244,551	243,609	199,266	149,096
High-income	0.0086***	0.0026***	0.0014**	355.74**	0.0046***	0.0046***	0.0033***	0.0008*
	(0.0011)	(0.0003)	(0.0004)	(125.45)	(0.0007)	(0.0007)	(0.0005)	(0.0003)
	[236.6]	[225.7]	[268.3]	[268.3]	[216.6]	[215.3]	[268.7]	[240.5]
N	243,152	246,063	197,034	197,034	245,503	245,286	197,029	164,647

Notes: The regressions control for cohort and municipality fixed effects, student and household-level controls, and interactions between the cohort and the controls. Standard errors are clustered at the municipal-level and shown in parentheses. First stage F-tests in square brackets. *N* refers to number of observations.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

3.D.4 Reduced Form Results Based on Alternative Peer Quality Measures

Here we present the results for the effects of school choice on peer quality of schools and classes using two alternative measures of peer quality: average predicted GPA and average household income decile of the school and class. These outcome variables are defined analogously to the main peer quality measures: calculating the average without student's own value.

We predict GPA using household income decile, parental earnings decile and education (three levels), single parent and employment status of each parent, and the municipality of residence. These background characteristics, excluding municipality, are allowed to have a cohort-specific effect. The income decile, used to calculate the latter outcomes, is determined separately for each cohort.

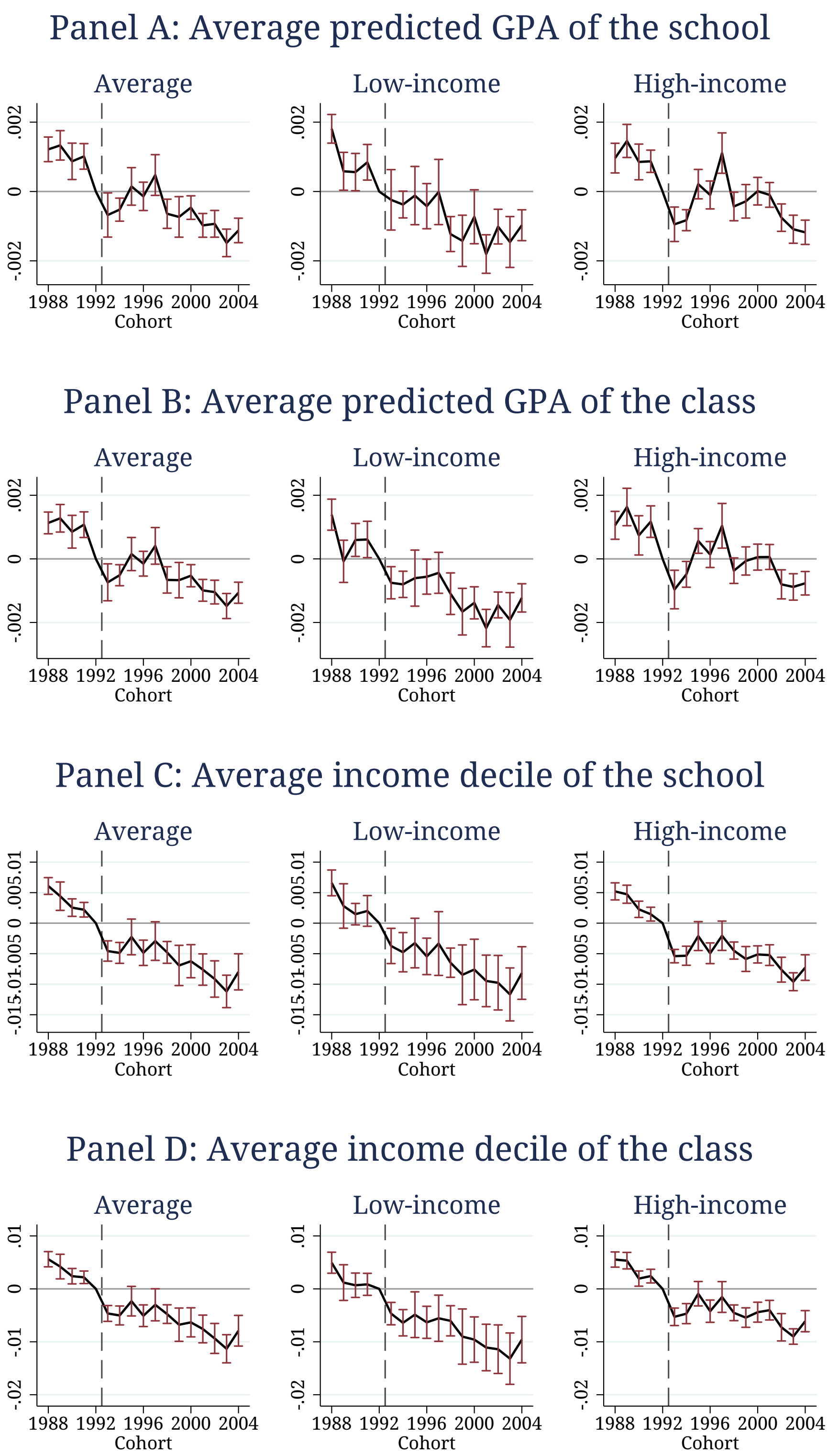
The motivation behind this analysis is to investigate peer quality using pre-determined student characteristics rather than GPA that can be affected by school choice, as shown by our results in Section 3.4.

Event-study estimates and reduced form results are collected to Figure D3 and Table D5, respectively. Unfortunately, the event-study estimates show a notable pre-trend for each of these outcomes. Thus, we do not draw any conclusions from these results.

Table D5. Effect of school choice education and labor market outcomes and channels of choice using IV with non-neighborhood school 30 as a proxy for choice.

	Predicted GPA of school (1)	Predicted GPA of the class (2)	Income decile of the school (3)	Income decile of the class (4)
Average	-0.001***	-0.001***	-0.009***	-0.009***
	(0.000)	(0.000)	(0.001)	(0.001)
N	982,833	980,562	982,989	981,584
Low-income	-0.002***	-0.002***	-0.010***	-0.010***
	(0.000)	(0.000)	(0.001)	(0.001)
N	245,296	244,381	245,354	244,668
High-income	-0.001***	-0.001***	-0.008***	-0.008***
	(0.000)	(0.000)	(0.001)	(0.001)
N	245,963	245,553	245,991	245,854

Notes: All outcome variables are school or class-level averages calculated without student's predicted GPA or household's income decile. The regressions control for cohort and municipality fixed effects, student and household-level controls, and interactions between the cohort and the controls. Standard errors are clustered at the municipal-level and shown in parentheses. First stage F-tests in square brackets. N refers to number of observations.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$



Notes: Figures shows point estimates from an event-study regression and their 95% confidence intervals constructed using standard errors clustered at the municipal-level. The gray dashed line marks the school choice reform.

Figure D3. Effects of school choice on alternative measures of peer composition: event-study specification.

3.D.5 The Share of Zero-Earners

In here, we illustrate why we estimate the effects of the school choice reform on the long-term labor market outcomes only for students who started 7th grade between 1988 and 2001. Figure D4 shows how the share of zero earners in year 2015 differs across cohorts and household income in our sample.

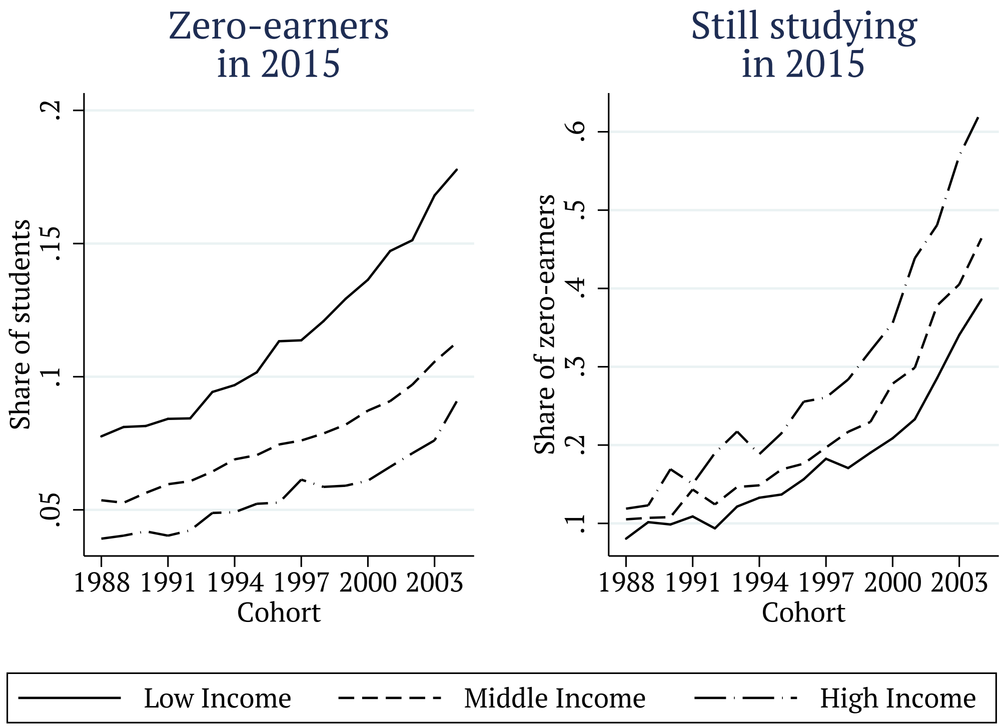


Figure D4. Share of students who have zero earnings by cohort and household income. Share of zero-earners still studying in 2015.

3.E Sensitivity Analysis

3.E.1 Municipal-level Controls

As our first robustness check, we check whether our results 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 in FLEED between 1988 and 2004.

The motivation behind these controls is two-fold. First, potential opening and closing of schools in cities versus smaller municipalities may affect our results. 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 3.1). Second, funds available to schools may have changed systematically between the municipalities if municipalities had differential unemployment and employment level trends during the observation period. This could potentially have an effect on student outcomes.

Results are visualized in Table E1 Panel A. These results show that our main effects are not driven by changes in cohort size or other municipal-level trends. However, the effects for the probability to have a higher education are no longer significant.

3.E.2 Cohort-specific County-level Fixed Effects

We also check if our results are driven by differential county-level trends. The motivation behind this is differential exposure to Soviet Union trade and its collapse in the early-90s. Another motivation for this check is that if some of the municipalities are co-operating in the supply of comprehensive school education, it is likely to happen within the county borders.

We study this by including cohort-specific fixed effects for the county of the municipality in the regressions that also include municipal-level controls of Section 3.E.1. Results are visualized in Table E1 Panel B. These results show are qualitatively in line with the results presented in Section 3.4. However, the estimates for the probability to attain high school education are no longer significant for students from high-income households. The probability of attaining higher education is also not significant on average, but instead there is a significant negative effect for students from low-income households.

3.E.3 Cohort-specific Controls for the Rural/ Urban -status of the municipality

In this robustness check, we address a potential threat to our identification strategy. The school choice reform was part of government decentralization process that started in the late-80s and gave municipalities more freedom to provide social, education, and health-related services. The provision of these public services may directly impact our student outcomes. This is potentially a threat to our identification if municipalities of different sizes and types were differentially affected by the decentralization process.

In order to test if this is the case, we use a categorical rural/urban -variable that tells whether the municipality is a city, city-like, or completely rural.[29] We use a flexible

[29]The type of the municipality is from 2007.

non-parametric specification with interactions between each cohort-year dummy and the categorical rural/urban variable, in addition to the rural/urban fixed effects.[30]

We confirm that our results survive this specification qualitatively (Panel C of Table E1). However, there is a small positive and significant effect on GPA also for students from low-income households.

3.E.4 Results without Helsinki

In our fourth robustness check, we study if our 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 our observation period. The next biggest cities have fewer than half the number of schools as Helsinki.

Results are visualized in Table E1 Panel D. They show that the effects on school choice and GPA are not driven by Helsinki, but the average positive effect for high school is no longer significant. Leaving out Helsinki, the point estimates for GPA and the probability to attain high school education are negative and significant for students from low-income households. The positive average effects for probability to attain high school and higher education also disappear without Helsinki.

3.E.5 Standard Difference-in-differences

In this robustness check, we study whether our results change when we use a traditional difference-in-differences setup. A municipality with more than 1 school before the reform on average, will have treatment status 1 whereas a municipality with 1 or fewer schools are untreated.

This robustness check addresses a concern raised by Fricke (2017) about the somewhat unreasonable assumption on treatment effect homogeneity one has to make in a setting in which there are multiple levels of treatment and no unit is untreated. In our empirical setup, treatment effect homogeneity would mean that the intensity of the reform, induced by one additional school, should be the same across municipalities.

We thus check whether our results, and the conclusions we draw from them, still hold up when we compare treated municipalities (regardless of the intensity of the treatment) to untreated municipalities (where choice is not possible).

Results in Panel E of Table E1 confirm that using this specification does not significantly alter the conclusions we draw from our results in Section 3.4. However, students from low-income households seem to make fewer choices than students from high-income households. For the probability to attain high school education, the point

[30]For example, we would add an interaction between the rural/urban variable and a cohort dummy for 1988 $Cohort(c = 1988)$ that takes value 1 if the cohort started 7th grade in 1988 and zero otherwise, and similarly for all other cohorts.

estimate is no longer significant on average. The breakdown of this average result by household income shows a negative impact on students from low-income households, whereas the estimate is still positive for student from high income households, but insignificant. Similarly, students from low-income households are also less likely to attain higher education later in life whereas the estimate is positive but insignificant for students from high income households.

3.E.6 Heterogeneity with Respect to Other Household Characteristics

In our last robustness check, we show the heterogeneity of our results using predicted GPA as an indicator for socioeconomic status of the student. We predict GPA using household income decile, parental earnings decile and education (three levels), single parent and employment status of each parent, and the municipality of residence. These background characteristics, excluding municipality, are allowed to have a cohort-specific effect. 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.

The motivation for this heterogeneity analysis is that household income alone does not capture the true socioeconomic status of the student, and parental education is likely to play a role as well. The reason why we do not use parental education in our heterogeneity analysis stems from the long observation period (17 years). As shown in Table C4, the probability of the parent not having a formal education after comprehensive school 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 makes comparison over the years difficult.

The results for this robustness check are shown in Panel F of Table E1. These are largely in line with our main heterogeneity analysis that uses household income—with a few exceptions. First, it seems that low SES students make slightly fewer choices than high SES students. Second, the estimate for GPA for low SES students is positive and significant (although barely). Third, the estimates for higher education are no longer significant.

Table E1. Effect of school choice on education and labor market outcomes: robustness checks.

	NNS (1)	GPA (2)	High School (3)	Higher Education (4)
Panel A: Municipal-level controls				
Average	0.1921***	0.0013**	0.0003**	0.0002
	(0.0239)	(0.0004)	(0.0001)	(0.0001)
N	984,478	967,525	984,219	795,184
Low-income	0.1775***	-0.0002	-0.0001	-0.0003
	(0.0363)	(0.0003)	(0.0001)	(0.0002)
N	246,099	239,745	245,976	199,272
High-income	0.1819***	0.0017***	0.0003***	0.0003
	(0.0201)	(0.0004)	(0.0001)	(0.0002)
N	246,102	243,152	246,063	197,034
Panel B: County and county-cohort fixed effects				
Average	0.1871***	0.0010*	0.0003*	-0.0000
	(0.0229)	(0.0004)	(0.0001)	(0.0001)
N	984,478	967,525	984,219	795,184
Low-income	0.1780***	-0.0007	-0.0002	-0.0004*
	(0.0334)	(0.0005)	(0.0002)	(0.0002)
N	246,099	239,745	245,976	199,272
High-income	0.1804***	0.0009*	0.0001	-0.0000
	(0.0200)	(0.0004)	(0.0001)	(0.0002)
N	246,102	243,152	246,063	197,034
Panel C: Non-parametric rural/urban fixed effects				
Average	0.1718***	0.0016***	0.0004***	0.0002**
	(0.0113)	(0.0004)	(0.0001)	(0.0001)
N	984,478	967,525	984,219	795,184
Low-income	0.1307***	0.0005*	0.0002	-0.0001
	(0.0152)	(0.0003)	(0.0002)	(0.0001)
N	246,099	239,745	245,976	199,272
High-income	0.1764***	0.0019***	0.0006***	0.0004***
	(0.0151)	(0.0004)	(0.0001)	(0.0001)
N	246,102	243,152	246,063	197,034

Table continued on the following page.

Table E1. *(continued)* Effect of school choice on education and labor market outcomes: robustness checks.

Panel D: Without Helsinki				
Average	0.3116***	0.0036***	0.0005	-0.0000
	(0.0409)	(0.0010)	(0.0003)	(0.0005)
N	914,465	900,140	914,246	740,675
Low-income	0.3273***	-0.0005	-0.0012**	-0.0010
	(0.0451)	(0.0010)	(0.0004)	(0.0005)
N	232,583	227,084	232,477	189,189
High-income	0.3317***	0.0051***	0.0011*	0.0001
	(0.0391)	(0.0011)	(0.0005)	(0.0006)
N	215,785	213,588	215,757	173,123
Panel E: Standard diff-in-diff (0/1 treatment dummy)				
Average	3.4904***	0.0216*	0.0033	-0.0007
	(0.7051)	(0.0103)	(0.0032)	(0.0034)
N	984,478	967,525	984,219	795,184
Low-income	3.0974***	-0.0132	-0.0111*	-0.0128**
	(0.6050)	(0.0112)	(0.0045)	(0.0044)
N	246,099	239,745	245,976	199,272
High-income	4.1551***	0.0525**	0.0097	0.0077
	(1.0768)	(0.0171)	(0.0063)	(0.0069)
N	246,102	243,152	246,063	197,034
Panel F: Heterogeneity by predicted GPA				
Low SES	0.1505***	0.0008*	0.0002	0.0001
	(0.0212)	(0.0003)	(0.0002)	(0.0001)
N	246,112	240,692	245,979	199,512
High SES	0.2029***	0.0019***	0.0003***	0.0001
	(0.0167)	(0.0004)	(0.0001)	(0.0001)
N	246,110	243,172	246,084	196,895

Notes: NNS refers to our first stage variable, non-neighborhood school. The regressions control for cohort and municipality fixed effects, student and household-level controls, and interactions between the cohort and the controls. Standard errors are clustered at municipal-level and are shown in parentheses. *N* refers to number of observations.

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

References

Fricke, H. (2017). Identification Based on Difference-in-Differences Approaches with Multiple Treatments. *Oxford Bulletin of Economics and Statistics 79*(3), 426–433.

4. The Unintended Consequences of a Public School Choice Reform on Segregation[1]

4.1 Introduction

Providing students from all socioeconomic groups a chance to attend better quality schools has been a recent motivation for school choice policies, particularly in the U.S (The U.S. Department of Education 2019). Theoretical studies have suggested that choice between schools should benefit these students more, as more affluent households have had the ability to choose schools prior to any school choice policies either by moving (Tiebout choice) or by attending private schools (Hoxby 2000; Nechyba 2003a). In contrast, recent empirical and theoretical evidence suggests that such policies, especially if implemented on a large-scale and not targeting disadvantaged students, have increased segregation of schools (Epple, Romano, and Urquiola 2017). Peer preferences (Abdulkadiroglu et al. 2020; Rothstein 2006) and information constraints (Hastings and Weinstein 2008), as well as the perverse incentives these create for schools (Abdulkadiroglu et al. 2020; Bergman and McFarlin 2018; Ladd and Fiske 2001), may help to explain the well documented phenomenon of increasing segregation after the introduction of school choice.

I study how a nationwide universal public school choice reform affected sorting of students across schools by ability and socioeconomic background. I also examine whether school choice decreased residential segregation as parents no longer had to move to get their children to desired schools (see, for example Brunner et al. 2012; Machin and Salvanes 2016). Furthermore, in order to better understand how the reform changed the extent to which students from different backgrounds interact with each other after the reform, I study how school choice impacted class-level segregation.

[1]For their suggestions and comments, I thank Mika Haapanen, Helena Holmlund, Liisa T. Laine, Moritz Marbach, Jaakko Meriläinen, Roope Uusitalo, and participants at Political Economy Workshop in Immigration Policy Lab, ETH Zürich. I am grateful to the VATT Institute for Economic Research for providing the data access. I thank the Yrjö Jahnsson Foundation and the Finnish Cultural Foundation for financial support, and IPL Public Policy Group at ETH Zurich for their hospitality.

My identification strategy uses municipal-level variation in school choice opportunities before and after the introduction of school choice to comprehensive school in Finland in 1993. Finnish education system moved from strict residence-based student selection to a system in which one can choose a school other than the assigned school. My individual-level administrative data allows me to link full cohorts of students to the schools and classes attended, student attainment measures, residential locations, and household characteristics.

This paper provides evidence on the effects of school choice on segregation in an entirely public education system without simultaneous introduction of private schools, or voucher to private or charter schools. This is important since private and charter schools can usually enter school-markets more freely, and thus create general equilibrium responses that can complicate the interpretation of effects of choice between public schools on segregation. In my setting, school entry is uncommon.

To my knowledge, this is also the first paper to empirically study the effects of school choice on class-level segregation. These results provide a better understanding of what happened to the classroom composition, i.e. the actual peer group faced by the students, after the reform. This is important, as changes in opportunities for social interactions across different ability and socioeconomic groups can have consequences on later life labor market outcomes via peer or network effects (see, for example Hoxby and Avery 2013; Zimmerman 2019).

I find that segregation of schools increased significantly after the reform in municipalities with more choice opportunities. Segregation of schools by GPA—a measure that is highly correlated with various household characteristics—increased by 4–5σ after the reform. I find that not all the increase in segregation of schools by GPA is due to sorting of students by pure ability, and segregation of schools by household characteristics increases by 1–2σ. For residential segregation, I find no consistent evidence that choice would have impacted sorting of students across residential locations.

Lastly, I find that students from different ability and socioeconomic groups are less likely to meet in a classroom, as class-level segregation, measured at municipal level, increased after the reform by 1–6σ. Even after controlling for school-level differences within the municipality, the increase in class-level segregation amounts to 1–5σ.[2] This suggest that student sorting within schools changed after the reform. My results highlight that school choice reforms are likely to have unintended consequences. These could dampen or even erase the potential benefits associated with school choice.

I contribute to the vast literature on school and residential segregation. My results on increasing segregation of schools after the introduction of school choice are in line with

[2]Class-level segregation, measured at municipal level, reflects both segregation between and within schools.

previous theoretical and empirical findings from several countries.[3] However, only few empirical papers have been able to empirically study how school choice affects segregation of schools by exploiting a quasi-experimental approach provided by a choice reform.

The empirical literature tends to find modest to substantial increases in segregation of schools after an introduction of a large-scale school choice reform. These reforms have been accompanied by modest to non-existent gains in average student attainment. Hsieh and Urquiola (2006) find substantial increase in sorting of students across schools but no improvements in student outcomes after an introduction of a large-scale private school voucher reform in Chile. In Sweden, where the comprehensive school choice reform also introduced publicly funded private schools, the reform had a small positive effect on student attainment (Böhlmark and Lindahl 2015; Wondratschek et al. 2013) but increased segregation of schools by ability, socioeconomic background, and immigrant status (Böhlmark et al. 2016).[4]

On the contrary, my findings on residential segregation are not in line with theoretical predictions or previous empirical findings. Theoretical models suggest that giving students the opportunity to choose a school without having to move to the catchment area of the school should decrease residential segregation.[5] Previous empirical studies have used boundary discontinuity analysis to show that school quality is capitalized into housing prices (for a review, see Black and Machin (2011)) and that after introducing school choice this link weakens significantly (see Machin and Salvanes (2016) for evidence from Norway and Brunner et al. (2012); Reback (2005) for evidence from the U.S.).

My results on residential segregation square more easily with previous evidence, if the implementation of the school choice reform in Finland is taken into account: Even after the reform, the distance to schools was one of the key determinants of student selection in Finnish schools. Priority is given to students living in the school catchment area and, in case of over-subscription, schools can use distance to prioritize the applicants from outside the school catchment area. Thus, it is perhaps not that surprising that the introduction of school choice did not reduce residential segregation.

As the school choice reform in Finland did not abolish all ties to distance-based student selection, and taking this together with the fact that, in international terms, differences between schools and students are small (OECD 2013), I argue that my findings can be taken as a lower bound. Thus, school choice reforms that are implemented in countries

[3]For theoretical considerations see, for example, Epple and Romano (1998); MacLeod and Urquiola (2013, 2019). For empirical studies see Allen and Vignoles (2007); Burgess and Wilson (2007); Böhlmark et al. (2016); Hsieh and Urquiola (2006); Ladd and Fiske (2001); Söderström and Uusitalo (2010).

[4]Another Swedish reform, that changed the student admission criteria even further to a pure ability-based selection, but only in Stockholm and at a high school level, increased sorting of students across schools by ability, socioeconomic background, and immigrant status (Söderström and Uusitalo 2010).

[5]For theoretical considerations on school choice and residential segregation, see for example, Brunner et al. (2012) Epple and Romano (2003), Nechyba (2000, 2003a,b), and Ferreyra (2007).

with greater differences between students or that are more drastic (for example change to ability-based student selection) are likely to experience greater increases in segregation of schools and classes.

The rest of the paper is organized as follows. In the next section I describe the school choice reform. In Section 4.3, I describe the data and its construction. In Section 4.4, I lay out how segregation is measured and provide descriptive evidence on segregation. In the next two sections, I introduce my identification strategy (Section 4.5) and present the results (Section 4.6). Lastly, Section 4.7 concludes.

4.2 School Choice Reform

This section summarizes the relevant parts of the Finnish education system and the comprehensive school reform that allowed students to choose other than their assigned school in Finland in the 1990s. A more thorough description of the reform can be found in Essay 2.

In Finland, basic education is compulsory and takes 9 years. This is called comprehensive school. Students start a six-year primary school the year they turn seven. After primary school, students continue their comprehensive school education to a three-year lower secondary school.

The education system is entirely publicly funded despite the ownership status of the school. Municipalities provide education to their comprehensive school aged students (7–16-year-old) and most of the schools (95 percent) are operated by municipalities. Some privately run schools exist (2 percent) but these schools are not allowed to collect tuition fees, or make a profit, and they are funded using the same principals as municipal-run schools. Schools cannot open or close strategically. Opening a new school, private or public, is always based on a need.[6]

Before the comprehensive school choice reform, students were assigned to schools based on their residential location. Each school had its own school catchment area. These schools are hereafter called neighborhood schools. Only in special situations, such as specific medical conditions, were students allowed to cross catchment area boundaries and attend a school other than the assigned neighborhood school.[7] Importantly, schools did not choose their students or the school catchment area.

After the reform in 1993, students could opt out of their assigned school and try to apply to another school within the municipality. In case of an oversubscribed school, students living in the catchment area are given a priority. Siblings and distance to school

[6]Private schools need a permit from the central government to enter the school market. See *Yksityiskoulujen liitto ry* at `http://www.yksityiskoulut.fi/yksityiskoulut-suomessa/koulun-perustaminen-ja-rahoitus/` (accessed June 3, 2019).

[7]Also, students with musical abilities could attend special programs offered by some schools in bigger cities.

can be used to prioritize the applicants from outside the catchment area, but previous grades or household characteristics are not be used in student selection. Schools also do not publish any information on average student attainment.

The reform was part of government decentralization process that started in the late-80s and gave municipalities more freedom to provide social, education, and health-related services. There were two important regulatory changes that introduced choice between schools (Koskinen 1994; Varjo 2007): First, the formation and allocation of central government subsidies to municipalities for education provision became simpler and more transparent in 1993 (L 705/1992). Previously, central government subsidies for education provision were linked to the size of the school catchment areas in each municipality.[8] Also, any changes in the boundaries of school catchment areas required provincial-level approval before 1993. After 1993, the financial disincentives for school choice were removed: changing or crossing the school catchment area boundary had no impact on the level of these subsidies.

Second, the curriculum guidelines changed in 1994. The new guidelines provided more flexibility for schools to design their own curriculum, as central government only specified the minimum teaching hours for each (core) topic. This led to the popularization of specialized programs: some schools provide a class with extra teaching hours in, for example, sports, natural sciences, arts, or foreign languages. Students living in the catchment area of the school do not have a priority in a specialized program, and from 1999 onwards, schools could even use aptitude tests to select students to these classes.

In Helsinki and some other bigger cities, few schools already provided education in another language, or a class with special emphasis on arts or music before the reform. After the reform, these classes became very popular and the topics more diverse. Today, around 5th of all the students in Finnish lower secondary schools study in a specialized program (Kupiainen and Hotulainen 2019) and almost all the lower secondary schools in Helsinki offer these programs.

I take the first reform year to be 1993.This is the year when the financial disincentives for choice were removed. Also, Helsinki, the capital city of Finland, introduced pilot schools without catchment areas already in 1993 Koskinen (1994). However, the reform is likely to have been a gradual process and municipalities have had different practices in the implementation of school choice (see, for example, Kalalahti et al. 2015).

[8]Although I was not able to retrieve any information regarding the previous subsidy allocation formulas, government proposals (see HE 215/1991) suggest that in the old subsidy system (prior to 1993) crossing the school catchment area boundary (making a choice) could affect the subsidies municipalities receive.

4.3 Data

I focus on students who enter lower secondary school (aged 13, grade seven) as this is the most common time to choose a school other than the assigned school in Finland (Seppänen 2006). My final sample consists of full cohorts of students who started lower secondary school between 1988 and 2004. These students form a balanced panel of 399 municipalities over the sample period. I will next describe my data and the final sample. The data construction is discussed in more detail in the Appendix of Essay 2.

I use three administrative data sources to form my final sample. The first data contain all 9th graders (end of comprehensive school) and everyone who applied to upper secondary education in Finland in 1991–2007.[9] I use these data to obtain a unique identifier of students' comprehensive school and class attended at 9th grade. This is a good approximation for the school and class attended by the students in lower secondary school (grades 7–9), as students usually stay in the same school and class they were assigned to in the 7th grade until the end of comprehensive school. These data contain information on students' GPA at the end of 9th grade that I will later use to study segregation by ability.

Unfortunately, this data set has its limitations. The data do not contain any additional information on the schools or classes, such as information on specialized programs or aptitude tests to these programs. This is not an issue in terms of identifying if school choice impacted segregation of schools, but I am not able to further investigate potential mechanisms. Additionally, the data do not contain information on school addresses, applications to schools, or if the student attends a neighborhood school or has made a choice. This is discussed further in Section 4.5.[10]

The second data contain household characteristics when the students are aged 14–16 (1989–2007). These data are used to study segregation by household income and other household characteristics. My last data contain students' residential locations between 1988 and 2015. This information is used to study residential segregation.

To construct my data, I take all students who finished comprehensive school (9th grade), and turned 16 that year, between 1991 and 2007. I make this choice as I want to focus on students who started 7th grade between 1988 and 2004.[11] Second, I use the set of data on education and labor market outcomes of the parent(s) of the student the

[9]Upper secondary school in Finland is divided into academic (high school) and vocational track.

[10]Also, Appendix 4.C and Essay 2 discuss this data limitation and the strategy used to overcome them.

[11]Specifically, I drop everyone who are redoing 9th grade, doing 10th grade (an extra grade for those who have not completed comprehensive school education successfully), started comprehensive school education a year earlier/later, skipped or redid a grade before the 9th grade, or are reapplying/applying at older age to upper secondary schools.

year the student turns 14.[12] The information in these data includes earnings, income, education, and employment of the parent(s). The data also has dummy variables if the student lives with the parent(s). Lastly, I combine these data with data that contain students' yearly residential locations on a 250m×250m grid, 1km×1km, postal code, and municipal level. I link students to their residential locations the year they started lower secondary school (age 13).[13]

There are couple of data construction choices I make. First, as the information on the comprehensive school attended is of interest, I drop everyone with faulty or missing comprehensive school code. To the best of my knowledge, these are students who either finished their comprehensive school education abroad and moved back to Finland aged 16, have been home schooled, or have been to hospital schools because of a severe illness.

Second, I drop everyone who lives in the autonomous region Åland Islands, because of missing information. This is unlikely to be of concern , as only 0.5 percent of the Finnish population lives in Åland Islands.

Third, during my sample period, there are 36 municipal mergers. I merge these municipalities together already from the first year of my data. This does come with a trade-off, as some of the merged municipalities will appear to have more schools than they truly have for some years before the actual merger and this will affect the measurement of segregation of schools. The bias this causes for segregation measurement is likely to be very small. In most cases, merging municipalities are demographically similar. My results are not sensitive to dropping the merged municipalities from the analysis (see Appendix 4.E).My final sample contains 984,478 students who started lower secondary school between 1988 and 2004. These students form a balanced panel of 399 municipalities in which segregation of schools, residential locations, and classes of schools can be measured. My final sample contains 5 years of observations before the reform (5 full cohorts of students who started lower secondary school before the reform).

4.4 Measuring Segregation and Descriptive Evidence

In this section, I will first describe the indices that I use to measure segregation of schools, residential locations, and classes of schools within a municipality in Section 4.4.1. The advantage of using several indices to measure segregation is that it provides a more comprehensive understanding of segregation, as these indices do not individually

[12]Ideally, I would use information on students' household characteristics from earlier years but this information is not available to me.

[13]Ideally, I would take this from a year or two earlier, but I do not have this information for the students who started 7th grade in 1988–1989, as the first observation is from the year 1988.

fulfill all properties that a good segregation index should possess (for these properties, see Allen and Vignoles 2007). After introducing the indices, I will address a potential concern related to detecting segregation even if students were randomly allocated to schools (residential locations/classes) in Section 4.4.2. Last, I present descriptive statistics in Section 4.4.3.

4.4.1 Segregation Indices

R^2 The first measure that I use for segregation is the coefficient of determination, R^2. This has previously been used by Söderström and Uusitalo (2010). The idea of R^2 is to measure how well schools, residential locations, or classes of schools explain student-level variation in ability or students' household characteristics within a municipality. This measure is calculated separately for each municipality and year, it is obtained by explaining the student-level characteristics (i.e. ability measure) by the schools or classes attended by the students, or in case of residential segregation, by the residential location of the students. The advantage of using R^2, compared of other widely used segregation indices is that it can be used to measure segregation by continuous variables, such as GPA or household income.

The index is defined as follows,

$$\hat{R}^2_{mt} = \frac{\sum_{s=1}^{S_{mt}} \frac{N_{smt}}{N_{mt}} (\bar{y}_{smt} - \bar{y}_{mt})^2}{\sum_{i=1}^{N_{mt}} \frac{(y_{ismt} - \bar{y}_{mt})^2}{N_{mt}}}, \tag{4.1}$$

where N_{smt} is the total number of students in a school s, a municipality m, and a year t. N_{mt} is the total number of students within each municipality and year, y_{ismt} is a student-level measure of a student i, in a school s, municipality m, and a year t, $\bar{y}_{smt}$ and $\bar{y}_{mt}$ are school and municipal-level averages of the student measures, respectively.

R^2 varies between 0 and 1, where 0 reflects that schools (or residential locations/classes) explain none of the variation in the student-level characteristic, indicating that there is no segregation. On the other hand, a value of 1 suggests that schools fully explain the variation in the student-level characteristic, and that students are sorted into schools based on this characteristic.

Partial R^2 For class-level segregation, I additionally take advantage of R^2's decomposability to separate class-level segregation from segregation of schools. Unless school-level differences are accounted for, potential changes in peer composition of schools are reflected as changes in class-level segregation. For this reason, I will use coefficient of partial determination, *Partial* R^2, to study how much more of the variation in various student characteristics does the class within the school explain, *in* *addition* to the school attended by the student. This is defined as follows,

$$Partial\ R^2_{mt} = \frac{RSS_{reduced,mt} - RSS_{full,mt}}{RSS_{reduced,mt}}, \tag{4.2}$$

where $RSS_{reduced,mt}$ is residual sum of squares of the reduced model that explains student characteristics only by the schools attended, and $RSS_{full,mt}$ is the residual sum of squares of the full model with classes of the schools as an additional explanatory variable in municipality m and year t.

In other words, this measure of class-level segregation controls for segregation of schools. Partial R^2 is measured for each municipality and year separately.

Overexposure Index The next segregation measure that I use to measure segregation of schools, residential locations and classes is called the own-group overexposure index, hereafter overexposure index. The overexposure index was first introduced by Åslund and Nordström Skans (2009). It measures the *excess* exposure to one's peers. The idea is to measure how segregated the units, i.e. schools, classes, or residential locations, are compared to the overall distribution within the municipality. The index is given by,

$$Overexposure_{mt} = \frac{\bar{e}_{mt}}{\hat{e}_{mt}}, \tag{4.3}$$

where $\bar{e}_m t$ is the average own-group peer exposure and is $\hat{e}_m t$ the expected exposure in municipality m and time t. Full derivation of the index is given in Appendix 4.A.

If $Overexposure_m > 1$, then there is excess segregation of schools/residential locations on top of what the expected distribution within the municipality would imply. The overexposure index can only be used with categorical variables by which segregation is measured.

Dissimilarity Index The third index I use to measure segregation of schools, residential locations and classes is the most widely used segregation index: the dissimilarity index. It was introduced first by Duncan and Duncan (1955). The idea of dissimilarity index is to measure how evenly two groups are distributed across units, such as schools, within a municipality. Alternatively, it can be interpreted as the percentage of the students that would need to be reshuffled across the units in order to achieve an equal distribution between the two groups. The dissimilarity index can only be used with a dummy variable.

The dissimilarity index, D_{mt} for municipality m and year t, is computed as follows,

$$D_{mt} = \frac{1}{2}\sum_{s=1}^{S}\left|\frac{n_{1smt}}{M_{1mt}} - \frac{n_{2smt}}{M_{2mt}}\right|, \tag{4.4}$$

where n_{1smt} and n_{2smt} are the number of students belonging to group 1 and 2, respectively, in school (or residential location/class) s. Similarly, M_{1mt} and M_{2mt} are the total number of students within the municipality in a given year who belong to group 1 and 2, respectively. S is the total number of schools.

According to Equation 4.4 there is segregation if there is a school (or residential location/class) s such that $\frac{n_{1smt}}{M_{1mt}} \neq \frac{n_{2smt}}{M_{2mt}}$. The index values range from 0 to 1, where a value 1 implies that students are sorted across units by the dummy variable.

4.4.2 Deviation from Randomness

Next, I will discuss a randomness-correction for three of my segregation measures introduced in the previous section. The motivation for this correction is that even if students were randomly allocated to schools, classes, or residential locations, the distribution is unlikely to be even (Carrington and Troske 1997). Therefore, segregation can be detected even if there is no systematic segregation.

Measuring segregation just by chance is an issue, especially in my empirical setting, as there are several small units (schools, classes, and residential locations) and the number of these units varies over municipalities and years (see Carrington and Troske 1997, for a discussion). This can lead to systematic bias in three of my segregation measures: dissimilarity index, R^2, and partial R^2. Importantly, this bias could lead to wrong conclusions about the effects of school choice on segregation.[14]

To overcome this issue, I use a method introduced by Carrington and Troske (1997) to adjust my segregation measures to measure whether there exists systematic sorting of students across the units of a municipality. This is done by subtracting the expected segregation, that can be detected even if students were randomly assigned to schools (or residential locations/classes), from the actual segregation measured in the data. This is then scaled back to range between 0 and 1. Equation 4.5 illustrates this procedure.

$$\hat{Z}_m = \begin{cases} \frac{Z_m - E(Z_m)}{1 - E(Z_m)} & \text{, if } Z_m \geq E(Z_m) \\ 0 & \text{, if } Z_m < E(Z_m) \end{cases} \tag{4.5}$$

where Z_m is the municipal-level segregation index, and $E(Z_m)$ is the expected segregation from random allocation. The expected segregation, $E(Z_m)$, is obtained by randomly allocating students to schools (or residential locations/classes) each year within a municipality, keeping the unit and municipal size fixed. This is repeated 100 times and, for each repetition, segregation is calculated.[15] The sample mean of these segregation measures is the expected value of segregation from random allocation.

[14]The overexposure index already measures segregation *over* the expected distribution and, thus, no correction is needed (see Åslund and Nordström Skans (2009) for further discussion).

[15]Preferably, I would use more repetitions, but the computations take too long.

Allen et al. (2015) criticize this correction, and show in their simulations that this randomness-corrected measure of segregation underestimates the true underlying segregation by a larger amount than the uncorrected measure overestimates it. I will compare the randomness-corrected and uncorrected segregation measures in Appendix Sections 4.B. Additionally, I show in Appendix Section 4.E that my results do not significantly change if I use the uncorrected measures.

4.4.3 Descriptive Statistics and Trends in Segregation

This section will first describe all the variables I use to calculate the segregation indices before presenting descriptive statistics on segregation of schools, residential locations, and classes of schools. I start with the units across which segregation is measured: schools, residential locations (postal codes), and classes of schools within a municipality.[16]

Units Table 4.1 shows average number and size of schools, postal codes, and classes before and after the reform in large municipalities with at least 10 schools and small municipalities with fewer than 10 schools on average before the reform. There are couple of things worth pointing out. First, there are only 8 large municipalities compared to 391 small ones. Second, a large municipality has on average 10 times more students, over 20 schools, and 36 postal codes, whereas a small municipality has on average fewer than 2 schools and around 4 postal codes (with 7th grade student residing in them) (Panel A). Third, school-level measures are surprisingly similar in small and large municipalities (Panel B). Lastly, all of these measures stay remarkably constant before and after the reform.

Ability and Household Characteristics Variables I measure segregation of schools, residential locations, and classes by two ability and two household characteristic variables. For ability, I use *GPA* at the end of 9th grade and *residual GPA* that has been cleaned from student's household characteristics, because GPA is heavily correlated with the background of the student. To do this, I explain 9th grade GPA with household characteristics (household income decile, earnings decile of mother and father, education of mother and father, single parent household, and municipality), allowing these (excluding municipality) to have a differential impact over the years.[17] I use the residuals as a measure of ability. GPA ranges from 4 (failed) to 10 (outstanding) and residual GPA from -4.6 to 3.4.[18]

[16]I use the postal code -level information because in smaller municipalities there are too few observations on 250m×250m or even within a 1km×1km grid level.

[17]The earnings and income deciles are defined yearly. Education is measured on a three-level scale: only comprehensive school education, secondary school education, or a degree akin to bachelor or higher.

[18]For segregation of schools and classes by these two ability measures, I drop in total 32 (municipal-level) observations (17 of these are from the year 1994) due to too many missing GPA values and too

Table 4.1. Descriptive evidence on GPA and family income before and after the reform and in small and large municipalities.

	Large municipalities		Small municipalities	
	1988-1992 (1)	1993-2004 (2)	1988-1992 (3)	1993-2004 (4)
Panel A: Municipal-level measures				
Average size of a municipality	1781.9	1819.0	113.8	110.1
	(911.1)	(1009.1)	(130.1)	(124.3)
Average number of schools	21.5	22.3	1.3	1.3
	(14.29)	(13.86)	(1.32)	(1.37)
Average number of postal codes	35.6	35.3	4.0	3.9
	(15.5)	(16.0)	(3.5)	(3.5)
N	40	96	1,955	4,687
Panel B: School-level measures				
Average school size	74.4	72.3	63.7	61.2
	(46.3)	(47.1)	(55.9)	(52.4)
Average class size	15.5	15.2	13.2	13.0
	(8.7)	(9.3)	(8.1)	(7.2)
Average number of classes	4.3	4.4	3.9	3.9
	(2.2)	(2.3)	(2.8)	(2.8)
N	957	2,392	3,495	8,458
Panel C: Residential-level measures				
Average postal code size	45.7	46.9	14.8	15.1
	(41.0)	(41.2)	(23.8)	(23.6)
N	1,559	3,723	15,030	34,070
Number of municipalities in the sample	8	8	391	391

Notes: Large municipalities refers to municipalities with at least 10 schools and small municipalities to municipalities with fewer than 10 schools before the reform. Standard errors are clustered at municipal-level and shown in parentheses. N refers to number of observations.

For student's household characteristics, I use *household income* and *predicted GPA*. Household income reflects all work-related earnings and benefits received by the parent(s) of the student. Household income ranges from 0 to several million euros each year.[19] Predicted GPAs are obtained from the same model as residual GPAs by predicting the 9th grade GPA with various household characteristics of the student, including parental education and earnings. This value is thus a much broader measure of the socioeconomic background of the student than household income. This value ranges from 6.2 to 9.6.

Education level of parents is often used to measure socioeconomic background of the student, but during my long observation period (17 years) there are significant changes to parental education depicted in Table 2 of Essay 2. Thus, I do not measure segregation by the education level of the parents, and, in constructing the measure for predicted GPA, I allow the impact of parental education to change over the years to address these trends in parental education.

Table 4.2 summarizes GPA and household income in small (fewer than 10 schools) and large (at least 10 schools) municipalities for low and high income households. Low income refers to students from households with total household income below or equal to the first quartile of the year-specific household income distribution. High income refers to students from households with total income equal to or higher than the last quartile of this distribution. Appendix Table B1 summarizes residual and predicted GPA.

Two remarks from Table 4.2 are worth mentioning. GPA is almost 1 grade lower for students from low income households compared to students from high income households but there are no significant changes to GPA before and after the reform (Panel A). Average household income is significantly higher in large municipalities both before and after the reform (Panel B).

As overexposure and dissimilarity index can only be used with categorical variables, I divide my sample to below and above (or equal to) median GPA, residual GPA, household income, and predicted GPA. This is done separately for each year. Thus, for example, an overexposure index of segregation by GPA, measures the exposure of students to above median and below median GPA peers (at school, class, or residential area) in a given year. This means that I lose most of the variation in these student-level characteristics.

Segregation Next, I will show levels and trends in segregation of schools, residential locations, and classes. The sample I use to construct these measures is discussed in Appendix 4.A, but in general, I require a municipality to have at least ten 7th grade students and two units (such as schools) per year to be able to measure segregation

little observations left to be able to calculate segregation meaningfully. For residential segregation by these two ability measures, I drop in total 25 (municipal-level) observations (19 of these are from the year 1994) for the same reason. In total, I have over 16,000 students with a missing GPA observation in my data.

[19]A student without parents would have zero household income.

Table 4.2. Average GPA and family income before and after the reform and in small and large municipalities.

	Large municipalities		Small municipalities	
	1988-1992	1993-2004	1988-1992	1993-2004
	(1)	(2)	(3)	(4)
Panel A: Average GPA				
Average	7.82	7.79	7.67	7.61
	(1.14)	(1.12)	(1.15)	(1.11)
	[71,276]	[174,620]	[222,487]	[516,095]
Low Income	7.41	7.34	7.39	7.35
	(1.15)	(1.13)	(1.15)	(1.11)
	[12,315]	[34,794]	[61,117]	[137,873]
High Income	8.20	8.18	8.14	8.03
	(1.03)	(1.00)	(1.06)	(1.05)
	[28,091]	[64,891]	[45,345]	[107,775]
Panel B: Average Household Income				
Average	63,170	72,629	49,556	57,494
	(40,763)	(71,414)	(29,919)	(47,757)
	[71,276]	[174,620]	[222,487]	[516,095]
Low Income	24,391	26,430	24,822	26,955
	(9,311)	(12,662)	(8,895)	(12,051)
	[12,315]	[34,794]	[61,117]	[137,873]
High Income	94,201	114,079	84,457	98,712
	(48,095)	(101,028)	(45,778)	(84,613)
	[28,091]	[64,891]	[45,345]	[107,775]

Notes: Large municipalities refers to municipalities with at least 10 schools and small municipalities to municipalities with fewer than 10 schools before the reform. Standard errors are clustered at municipal-level and shown in parentheses. Number of observations in brackets.

meaningfully. I assign a value of no segregation (0 or 1 depending on the segregation measure used) to municipalities that are too small or have fewer than two schools, residential locations, or classes. This does not significantly bias the analysis.

Table 4.3 summarizes school (Panel A), residential (Panel B), and class-level (Panel C) segregation measured using R^2 before and after the reform in large and small municipalities. In addition, the table also summarizes class-level segregation *in addition to* segregation of schools measured using partial R^2 (Panel D). This is called within school sorting hereafter. Appendix Table B2 summarizes school, residential, and class-level segregation measured using overexposure and dissimilarity index. R^2, partial R^2, and dissimilarity index have been randomness-corrected. The uncorrected values can be found in Appendix 4.B.

All the index values for segregation of schools are substantially higher in large municipalities. In contrast, residential segregation is only moderately higher in large municipalities compared to small ones, and for residual GPA it is in fact lower. This difference between school and residential segregation in small municipalities is, at least partially, mechanical. Smaller municipalities have fewer than two schools on average, meaning that, for most for these municipalities, sorting between schools is not possible. In contrast, sorting across residential locations is possible as there are on average four postal codes (see Table 4.1).

The table also reveals the level of class-level segregation to be higher than segregation of schools both before and after the reform and in large and small municipalities. Because class-level segregation is likely to reflect both school- and class-level differences, this suggests that sorting of students happens also *within* schools. This raises an obvious question whether there is also systematic sorting of students to classes within the schools, on top of the measured segregation of schools. Panel D shows that this is likely to be the case, as the average values for within school sorting, measured by partial R^2, are nonzero, especially in larger municipalities after the reform.

The table also reveals that segregation of schools by GPA, and residual and predicted GPA increase after the reform, but there are no significant changes in residential segregation. Class-level segregation and within school sorting also increase after the reform by all ability and household characteristics variables.

Trends in Segregation Figure 4.1 shows the development of school, residential, and class-level segregation measured using R^2 and within school sorting measured using partial R^2 by two ability variables and two household characteristics of the students in three big cities in Finland (Helsinki, Vantaa, and Turku). For the other two other segregation indices, see Figures B1 and B2. For uncorrected measures see Appendix 4.B.

These cities differ in terms of size and school choice policies. Helsinki is about twice the size in terms of number of schools and students to Vantaa and Turku. Helsinki and

Table 4.3. Descriptive evidence on school, residential, class-level segregation, and within school sorting before and after the reform and in small and large municipalities measured using R^2 (partial R^2 for within school sorting).

	Large municipalities		Small municipalities	
	1988-1992	1993-2004	1988-1992	1993-2004
	(1)	(2)	(3)	(4)
Panel A: Segregation of schools				
GPA	0.030	0.051	0.002	0.003
	(0.020)	(0.028)	(0.008)	(0.013)
Residual GPA	0.017	0.025	0.002	0.003
	(0.012)	(0.013)	(0.007)	(0.011)
Household income	0.044	0.042	0.003	0.003
	(0.022)	(0.022)	(0.013)	(0.013)
Predicted GPA	0.055	0.067	0.003	0.003
	(0.029)	(0.034)	(0.011)	(0.012)
Panel B: Residential segregation				
GPA	0.021	0.026	0.017	0.015
	(0.011)	(0.012)	(0.040)	(0.036)
Residual GPA	0.011	0.010	0.016	0.014
	(0.008)	(0.008)	(0.038)	(0.035)
Household income	0.056	0.047	0.022	0.020
	(0.025)	(0.024)	(0.042)	(0.040)
Predicted GPA	0.046	0.044	0.013	0.013
	(0.022)	(0.022)	(0.030)	(0.031)
Panel C: Class-level segregation				
GPA	0.058	0.105	0.006	0.012
	(0.026)	(0.047)	(0.020)	(0.034)
Residual GPA	0.033	0.062	0.005	0.009
	(0.019)	(0.034)	(0.017)	(0.025)
Household income	0.056	0.059	0.021	0.023
	(0.026)	(0.030)	(0.039)	(0.038)
Predicted GPA	0.076	0.096	0.015	0.021
	(0.034)	(0.041)	(0.032)	(0.039)

Table continued on the following page.

Table 4.3. *(continued)* Descriptive evidence on school, residential, class-level segregation, and within school sorting before and after the reform and in small and large municipalities measured using R^2 (partial R^2 for within school sorting).

	Large municipalities		Small municipalities	
	1988-1992	1993-2004	1988-1992	1993-2004
	(1)	(2)	(3)	(4)
Panel D: Within school sorting				
GPA	0.036	0.079	0.006	0.011
	(0.020)	(0.027)	(0.019)	(0.032)
Residual GPA	0.018	0.046	0.004	0.008
	(0.013)	(0.020)	(0.016)	(0.023)
Household income	0.018	0.023	0.004	0.006
	(0.013)	(0.016)	(0.014)	(0.016)
Predicted GPA	0.041	0.059	0.005	0.008
	(0.013)	(0.019)	(0.015)	(0.022)
N	40	96	1,955	4,687

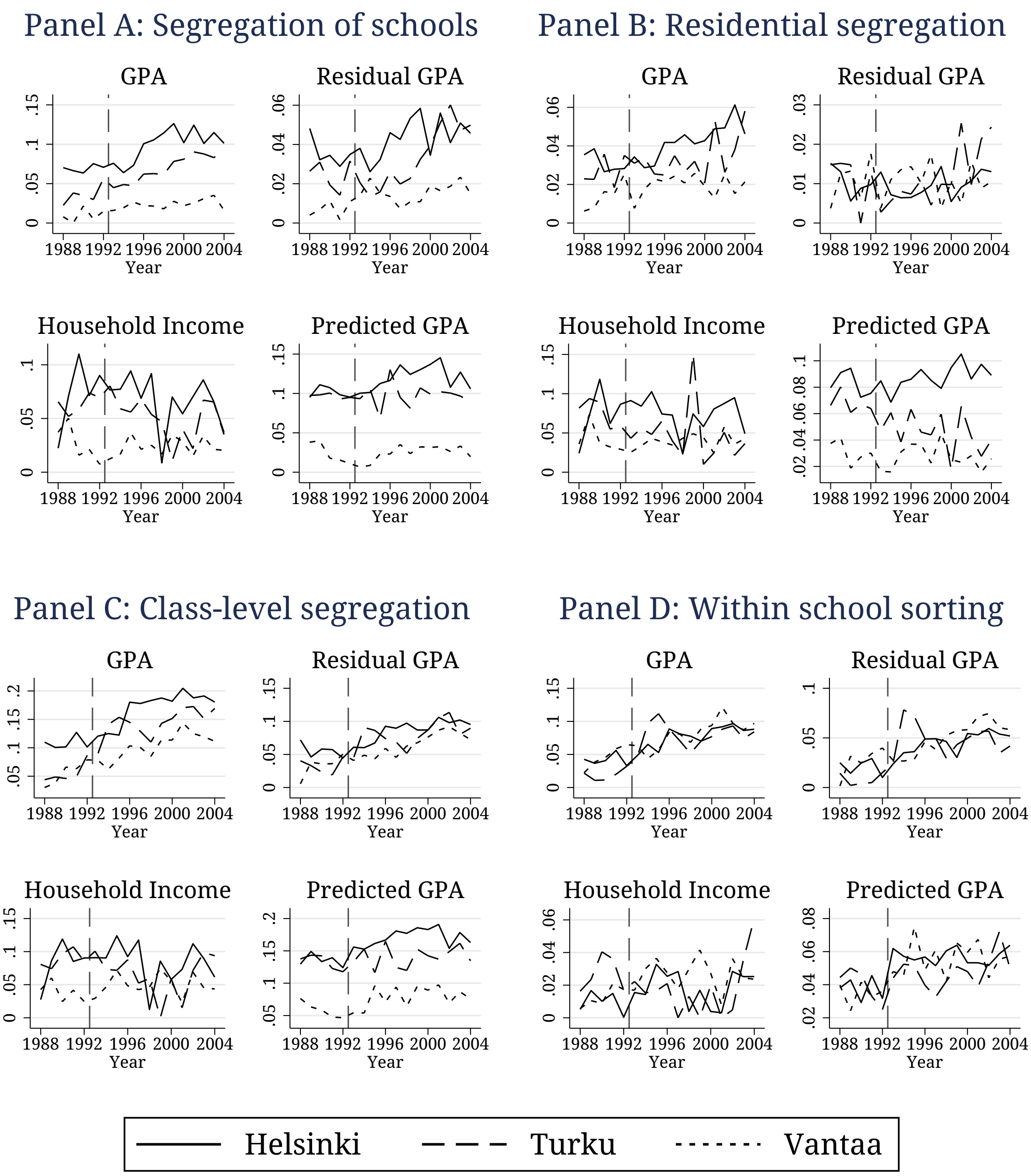
Notes: Large municipalities refers to municipalities with at least 10 schools and small municipalities to municipalities with fewer than 10 schools before the reform. GPA, residual GPA, household income, and predicted GPA refer to the variables by which segregation is measured. School, residential, and class-level segregation are measured using R^2 and within school sorting using partial R^2. All indices are municipal-level measures and all of them are corrected to measure deviation from randomness. Standard errors are clustered at municipal-level and shown in parentheses. N refers to number of observations.

Turku have both provided choice opportunities for students after the reform and most of the lower secondary schools in these two cities offer specialized programs in which aptitude tests can be used for student intake (Kalalahti et al. 2015; Seppänen 2006). Choice is also much more common in these cities than in Vantaa that has taken a stricter stance towards school choice (see Appendix 4.C for descriptive evidence on school choice activity, and Kalalahti et al. (2015) for discussion on types of choice policies in Vantaa, Helsinki, and Turku).

Segregation of schools developed similarly in Helsinki and Turku, whereas in Vantaa segregation of schools is stable throughout the sample period (Panel A). Segregation of schools by GPA and residual GPA, exhibit an upward sloping trend in Helsinki and Turku after the school choice reform. There is no clear trend for household income but segregation of schools by predicted GPA gradually increases in Helsinki.

On the contrary, there are no big differences between the cities in the levels of residential segregation (Panel B). Also, there are no clear (or stable) trends, except that segregation by GPA, that increases in Helsinki, and segregation by household income and predicted GPA, that decreases in Turku, after the reform.

The trends in class-level segregation resemble those of segregation of schools (Panel C). However, the differences (in levels of segregation) between the three municipalities are not as pronounced. Similarly, within school sorting by GPA, residual GPA, and predicted GPA increases in all the municipalities but the levels of sorting are similar across the cities (Panel D).



Notes: Gray dashed lines mark the introduction of the school choice reform. All indices corrected to measure deviation from randomness.

Figure 4.1. The development of school, residential, class-level segregation measured with R^2 and within school sorting measured with partial R^2 for three big cities in Finland in 1988–2004.

4.5 Identification Strategy

To investigate how school choice impacted segregation, I exploit variation in school choice opportunities across municipalities before and after the reform. My identification strategy is difference-in-differences with continuous treatment intensity. The idea is to compare units that received differential levels of treatment before and after the treatment.

In an ideal case, I would exploit variation in the potential level of school choice activity, such as implementation differences and/or variation in application rates, in each municipality, but my data lacks this information. Specifically, my data has two important limitations that motivate my approach. First, I do not know if a student made a choice or not. I only know the schools *actually* attended by the students, not the schools the students were originally *assigned* to attend.[20] Second, I do not know exactly if, when, and how municipalities implemented school choice. The regulatory changes were simultaneously introduced nationwide.

To overcome these issues, I first approximate the intensity of the reform by the average number of schools within a municipality before the reform. The idea is that students are more likely to exercise choice if there are more schools from which to choose. Thus, I measure the differential effect of more intensive reform (more school choice) on segregation.

Second, I approximate realized school choice activity by the share of students who attend a school other than the most commonly attended school of the region. This is done in order to link the treatment intensity value, number of schools, to school choice activity in the municipality. The most common school is defined at 1km by 1km grid-level. A student residing in this 1km by 1km grid during the year she/he enters lower secondary school (7th grade, aged 13), receives a value 1 if she/he attends other than the school that most of the other students residing in the same grid attend, and 0 otherwise. This is done separately for each year, as school catchment areas can change. Then, I calculate the share of students within a municipality that attend a school other than the most common school of their region. This share is then scaled to range from 0 to 100. For more on the treatment intensity variable and approximated school choice activity, see Appendix 4.C.

This section starts by estimating the effect of the (approximated) intensity of the reform on segregation. This is a reduced form specification. Next, I continue with an instrumental variable approach that links segregation to realized school choice activity in each municipality.

[20]For this, I would need the school catchment areas of each school and these are not available for all municipalities and years.

4.5.1 Reduced Form Specification

The reduced form specification on the differential effects of the reform across municipalities with varying number of schools on segregation is given by

$$Z_{mt} = \alpha_t^{RF} + \gamma_m^{RF} + \beta^{RF} N_m \times Post_t + \epsilon_{mt}^{RF}, \tag{4.6}$$

where Z_{mt} is the segregation index value of municipality m and year t, α_t^{RF} is a year fixed effect (in which the year refers to the year the students started lower secondary school), and γ_m^{RF} is a municipality fixed effect. The coefficient of interest is β^{RF}. It measures the effect of the reform, $Post_t$, at a differential level of treatment, N_m (number of schools).

In order to study the key identifying assumption of differences-in-differences strategy, the parallel trends -assumption, I use a non-parametric event-study specification that includes interactions between a year dummy (for each year) and the treatment intensity measure (number of schools). The idea is that without treatment (before the reform) segregation would have evolved the same way in municipalities with differential treatment intensity (with different number of schools).

$$\begin{aligned} Z_{mt} = \alpha_t^{RF} + \gamma_m^{RF} &+ \sum_{t=1988}^{1991} \beta_t^{RF} N_m \times \text{D(year=t)} \\ &+ \sum_{t=1993}^{2004} \beta_t^{RF} N_m \times \text{D(year=t)} + \epsilon_{mt}^{RF}, \end{aligned} \tag{4.7}$$

where everything else is defined as in Equation 4.6 but D(year=1988), is a dummy variable that gets a value 1 if the year is 1988 (i.e. if students started lower secondary school in 1988), and 0 otherwise. The other year dummies are defined in a similar manner. The coefficients of interest are the β_c^{RF}'s (where $c = 1988, 1989, ..., 1991, 1993, ..., 2004$) and these capture the effect between the year and the differential level of treatment. These are measured relative to year 1992 (the last year before the reform). In order to fulfil the parallel trends -assumption, β_t^{RF}'s are expected to be close to zero and exhibit no trend before the reform.

Equation 4.7 can also be used to study the dynamic sorting effects of the reform. The reform in Finland was likely gradual, meaning that school choice became more and more common over time as students and parents learned about choice opportunities and schools started to offer more choice opportunities.

4.5.2 IV strategy

In here, I present an instrumental variable approach, a 2SLS model, in which the reduced form estimates of the previous section (Equation 4.6) are scaled with realized school choice activity. Thus, my instrument is an interaction between the treatment intensity (number of schools, N_m) and the reform (after 1992, $Post_t$): $N_m \times Post_t$.

I start by defining the first stage model. The first stage links the approximated realized school choice activity of a municipality to the intensity of the reform.

$$S_{mt} = \alpha_t^{FS} + \gamma_m^{FS} + \beta^{FS} N_m \times Post_t + \epsilon_{mt}^{FS}, \tag{4.8}$$

where S_{mt} is the share of students attending other than the most common school of their region in a municipality m and year t. Everything else is defined as in the reduced form model in Equation 4.6.

The IV specification for the effects of realized school choice activity on segregation is defined as follows

$$Z_{mt} = \alpha_t^{2SLS} + \gamma_m^{2SLS} + \beta^{2SLS} \hat{\mathrm{S}}_{mt} + \epsilon_{mt}^{2SLS}, \tag{4.9}$$

where $\hat{\mathrm{S}}_{mt}$ is the instrumented realized school choice activity and β^{2SLS} is the coefficient of interest that captures the effect of a change in the share of students who attend a non-neighborhood school (as a result of the reform). The other variables are defined as in Equation 4.6.

There are two potential threats to the IV strategy. First, an IV specification has to satisfy the exclusionary restriction. In my case, this means that the intensity of the reform, approximated by the number of schools interacted with a post reform dummy, cannot have a direct impact on segregation. This is violated if the intensity of the reform would have an impact on segregation other than through realized school choice. This can happen if, for example, schools responded to the threat of choice without an actual increase in choice. However, I perceive this to be a minor issue.

Another potential threat is measurement error in the approximation of realized school choice activity. The measurement error should not correlate with the intensity of the reform. In other words, the measurement error can correlate with the intensity (number of schools) but it cannot systematically change after the reform. This is potentially a threat, as I use the most common school of the region to approximate neighborhood school. When school choice becomes more common, the most common school of the region could be other than the actual neighborhood school. To address this issue, I use another approximation for school choice activity that is less prone to measurement error but that will most likely overestimate school choice activity: the share of students who attend other than the school that at least 30 percent of the students of the region attend. In Appendix 4.C, I show that this measure evolves similarly to the main measure, the share of students who attend other than the most common school of the region.

4.6 Results

In this section, I show how the school choice reform impacted segregation of schools, residential segregation, class-level segregation and within school sorting.

The results of the first stage specification (Equation 4.8) can be found in Appendix 4.C. School choice activity increased on average by 15 percentage points (pps) after the reform.

4.6.1 Segregation of Schools

Ability I begin by showing the results for segregation of schools by GPA and residual GPA. Panels A and B of Figure 4.2 plot the results for these variables, respectively, using the non-parametric event-study specification of Equation 4.7 and R^2 as the segregation measure. The corresponding results using dissimilarity and overexposure index can be found Panels A and B of Figures 4.A and D4, respectively. Columns 1–4 in Panel A of Table 4.4 show the results using the reduced form (Equation 4.6) and IV (Equation 4.9) specification for all segregation indices.

The figures show that segregation of schools increased gradually after the reform in municipalities with more choice opportunities both by GPA and residual GPA. The estimates stabilize around 2000. There is no evidence of differential pre-trends.

As my reduced form identification strategy is not a standard difference-in-differences model, I quantify my results by comparing a municipality with low treatment intensity to a municipality with high treatment intensity. For this, I compare an average municipality with fewer than 2 schools to Helsinki, the capital city of Finland, with more than 50 schools. With this quantification, the reduced form results for segregation of schools by GPA increases by 4.2pps (0.00083×50) after the reform when segregation is measured using R^2. The corresponding values for overexposure and dissimilarity index are 3.4pps and 8.0pps, respectively. At first glance, this might not seem like huge increase but given the low initial values and small differences between municipalities shown in Table 4.3, this amounts to 4–5 standard deviations. The reduced form effect size for segregation of schools by residual GPA is roughly half of that, 1.7pps measured with R^2, 1.3pps with overexposure, and 3.9pps with dissimilarity index. This amounts to around 2 standard deviations.

For the IV results, I consider an increase of 10pps in realized school choice activity. This is because the weighted average of approximated school choice activity increased roughly around 10pps after the reform if weighted by the number of schools in a municipality. The effect size of my IV estimates is similar but more modest. Segregation of schools by GPA increases around 2.8pps (0.00277×10, roughly 3 standard deviations) and by residual GPA 1.1pps (roughly 2 standard deviation) when measured using R^2. The corresponding values for overexposure and dissimilarity index are 2.3pps (3 standard deviations) and 5.3pps (2 standard deviations), respectively. The IV estimates for segregation of schools by residual GPA amount to 1–2 standard deviations.

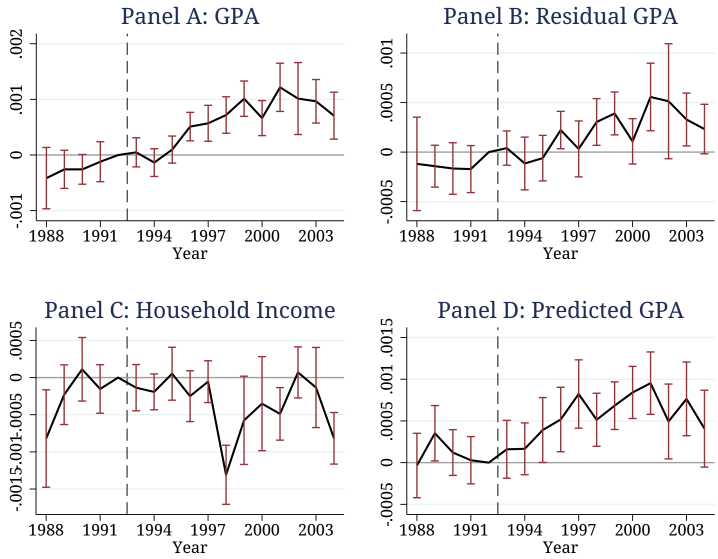
The effect size of segregation of schools by GPA is more than double the effect size of residual GPA (a measure of GPA that has been cleaned from household characteristics). This suggests that part of the increase in segregation of schools by GPA might be explained by household characteristics, such as household income, and I will investigate this next.

Household Characteristics The next two variables, by which segregation of schools is measured, are household income and predicted GPA. The event-study estimates for these, using R^2 to measure segregation, are collected in to Panels C and D of Figure 4.2, respectively. Panels C and D of Figures D1 and D4, show the event-study estimates using overexposure and dissimilarity index, respectively. Columns 5–8 of Panel A of Table 4.4 show the reduced form and IV results for these outcomes for all segregation indices.

For segregation of schools by household income, the event-study estimates show a negative effect for two years in mid-90s but this tails off for the later years. This effect is not present when segregation is measured using overexposure and dissimilarity index. Further caution should be exercised when interpreting the negative effect on segregation of schools by household income, as the parallel trends -assumption may not be fulfilled.

For segregation of schools by predicted GPA, the point estimates show a gradually increasing positive effect after the reform until the early 2000's. The effect slowly tails off for the later years. This pattern is also present when segregation is measured using overexposure or dissimilarity index.

The estimates for segregation of schools by household income are negative but statistically insignificant for reduced form and IV specifications when measured using R^2, positive but statistically insignificant with dissimilarity index, and positive and statistically significant with overexposure index. Quantifying the latter results using the same logic as above, segregation of schools by household income increased by 0.8–1.2pps after the reform. This amounts to around 1 standard deviation. The point-estimates for segregation of schools by predicted GPA are all positive and statistically significant, and systematically measured across all the segregation indices. Quantifying the reduced form and IV estimates suggest an increase of 1–2 standard deviations (1.6–2.4pps with R^2, 1.4–2.1pps with overexposure and 2.4–3.7pps with dissimilarity index).



Notes: Figure shows point estimates from an event-study specification and their 95% confidence intervals. Standard errors are clustered at municipal level. Gray dashed lines mark the introduction of the school choice reform. The indices have been corrected to measure deviation from randomness.

Figure 4.2. Effects of school choice on segregation of schools by ability and household characteristics measured R^2:event-study specification.

Discussion To conclude, I find strong evidence that segregation of schools increased substantially by ability and household characteristics as a result of the school choice reform. This is in line with previous empirical evidence. For example, Hsieh and Urquiola (2006) and Ladd and Fiske (2001) both find evidence of substantial increases in sorting of students by ability after an introduction of nationwide school choice voucher reforms in Chile and New Zealand, respectively. Previous empirical evidence from Sweden has also shown modest increases in segregation of schools by various household characteristics after an introduction of combined public school choice and private school voucher reform (Böhlmark et al. 2016) and also after a high school choice reform that abolished residence-based student allocation system (Söderström and Uusitalo 2010).

It is worth noting that school choice can also directly impact one of the variables by which I measure segregation of schools: 9th grade GPA. In fact, Essay 2 shows that school choice had a positive effect on average GPA, and more importantly, this positive average effect is driven by students from high income households. Thus, the increase in segregation of schools by GPA may partially be explained by the heterogeneous direct effects of school choice on GPA, rather than just students sorting into schools based on their ability. As I do not have any prior measures of student attainment (than the 9th grade GPA), I cannot distinguish between the two mechanisms. However, the positive effect on segregation of schools by household characteristics do suggest that sorting to schools increased, at least in that dimension, after the reform. Thus, I find it likely that students were also sorted into schools by ability.

4.6.2 Residential Segregation

Ability I begin by showing the results for residential segregation by GPA and residual GPA. Panels A and B of Figure 4.3 plot the event-study estimates for these, respectively, using R^2 as the segregation measure. The corresponding estimates using overexposure and dissimilarity index can be found in Panels A and B of Figures D2 and D5, respectively. Columns 1–4 in Panel B in Table 4.4 show the reduced form and IV results for all segregation indices.

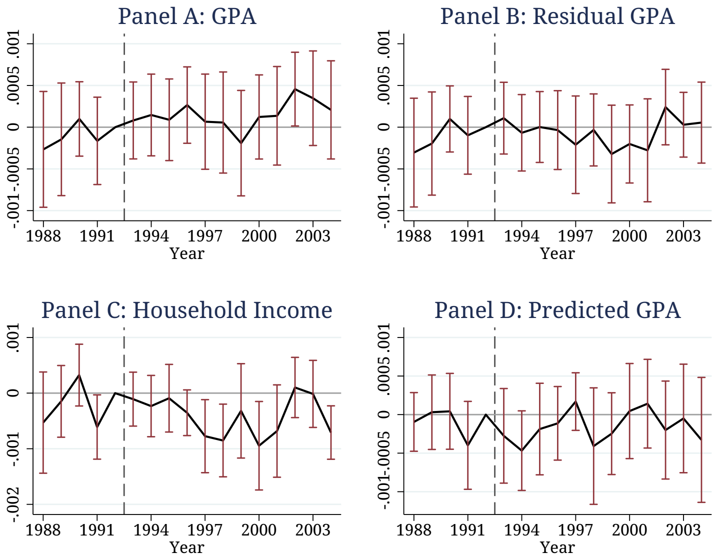
The event-study estimates for residential segregation by GPA are positive after the reform but not significantly different from zero, expect for 2002. The estimates with overexposure and dissimilarity index are also statistically insignificant and exhibit no clear pattern. Similarly, the event-study estimates for residential segregation by residual GPA are statistically insignificant.

The reduced form and IV estimates are all positive but statistically insignificant, except for residential segregation by GPA measured using R^2. Residential segregation by GPA increased by 1.2pps (0.00024×50) according to the reduced form specification and 0.8pps (0.00081×10) with the IV. These effects are small, only around 0.2–0.3 standard deviations, compared to the effects on segregation of schools in the previous section.

Household Characteristics The next two outcomes measure residential segregation by household income and predicted GPA. Panels C and D of Figure 4.3 plot the event-study estimates, respectively, using R^2 to measure segregation. The corresponding values measured using overexposure and dissimilarity index are depicted in Panels C and D of Figures D2 and D5, respectively. Columns 5–8 in Panel B in Table 4.4 show the estimates from reduced from and IV specifications for all segregation indices.

The event-study estimates show a gradually decreasing negative effect for residential segregation by household income using R^2 after the mid-90s, but this tails off for the later years. For residential segregation by predicted GPA, the event-study estimates are all statistically insignificant. The estimates measured using the dissimilarity index are all statistically insignificant, whereas with overexposure index, these estimates show a weak positive trend after the reform.

Both the reduced from and IV estimates are negative and statistically insignificant when R^2 is used to measure residential segregation by household income and predicted GPA, positive and statistically insignificant with dissimilarity index, and positive and statistically significant with overexposure index. Quantifying the latter estimates, using the same logic as before, residential segregation by household income increased by 1.5 to



Notes: Figure shows point estimates from an event-study specification and their 95% confidence intervals. Standard errors are clustered at municipal level. Gray dashed lines mark the introduction of the school choice reform. The indices have been corrected to measure deviation from randomness.

Figure 4.3. Effects of school choice on residential segregation by ability and household characteristics measured with R^2: event-study specification

2.2pps and by predicted GPA by 1 to 1.5pps. Again, these effects are rather small and amount to about 0.3–0.6 standard deviations.

Discussion I conclude that I find no consistently measured evidence that school choice would have had an impact on residential segregation by ability or household characteristics. I only find suggestive evidence of small positive effects but these are not consistently measured.

These results are not in line with previous theoretical and empirical findings. It has been proposed that school choice can alleviate the pressure for residential sorting, as households no longer have to move to the school catchment area with a desired school. Previous empirical studies on the effects of school choice on residential segregation have studied how school quality is capitalized into housing prices. For example, Machin and Salvanes (2016) finds that the link between house prices and schools weakens significantly after changing residence-based student allocation system to choice-based in Oslo, Norway.

There are two explanations for the null findings. First, even after reform the main student selection mechanism was based on distance to schools in Finland. Thus, it is perhaps not that surprising that school choice did not have an impact on residential segregation. Second, other changes, unrelated to the school choice reform, could have impacted relocation decisions of households, as the sample period is long. For example, Vilkama (2011) documents increasing share of foreigners and ethnic segregation in the capital city area in the late-2000s. Changes in the composition of the residents in the cities might dampen the potential alleviatory impacts of school choice on residential segregation.

I do acknowledge that my segregation indices may not give a complete picture of residential segregation in a given municipality and year, as these estimates are not based on the entire population. My data only contain the residential information of students who started lower secondary school in 1988–2004. Nevertheless, I believe that potential changes in relocation decisions of households with children starting lower secondary school should be captured by these estimates.

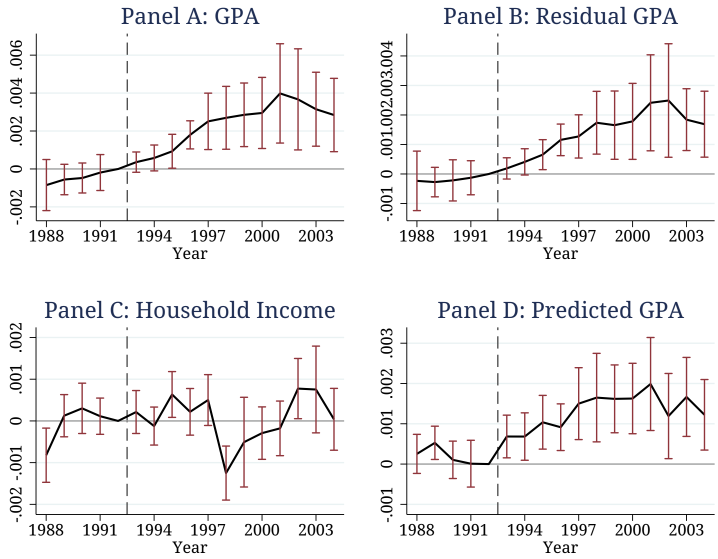
4.6.3 Class-level Segregation

Ability The non-parametric event-study estimates for the two ability measures, by which class-level segregation is measured, using R^2 are plotted Panels A and B of Figure 4.4. These estimates show a gradually increasing positive effect after the reform that stabilizes around 2000's. The corresponding estimates using overexposure and dissimilarity index show very similar patterns (see Panels A and B of Appendix Figures D3 and D6, respectively).

Columns 1–4 of Panel C in Table 4.4 collects the reduced form and IV estimates for these two outcomes for all the segregation indices. Class-level segregation by GPA, measured using R^2, increased by 13.9pps (0.00278×50) according to the reduced form specification and a more modest 9.3pps (0.00927×10) according to the IV specification, if quantified using the same logic as before. The corresponding values for overexposure index are 5.6–8.4pps, and 7.8–11.7pps for dissimilarity index. These estimates amount to around 1–6 standard deviations.

Similarly, the class-level segregation by residual GPA increases by 5.4–8.1pps if measured with R^2, 3.0pps with overexposure, and 4.1–6.1pps with dissimilarity index after the reform. These estimates amount to around 1–4 standard deviations.

Household Characteristics For the two household characteristic measures, household income and predicted GPA, the estimates from the non-parametric event-study specification are shown in Panels C and D of Figure 4.4 for R^2 and in Panels C and D of Appendix Figures D3 and D6 for overexposure and dissimilarity index, respectively. The estimates for class-level segregation by household income are mostly insignificant if segregation is measured with R^2. However, if measured with overexposure and dissimilarity index, the estimates exhibit an increasing trend after 2000's. For predicted GPA, the estimates show a clear positive trend after the reform that stabilizes just before the 2000's, and this is consistent for all three indices.



Notes: Figure shows point estimates from an event-study specification and their 95% confidence intervals. Standard errors are clustered at municipal level. Gray dashed lines mark the introduction of the school choice reform. The indices have been corrected to measure deviation from randomness.

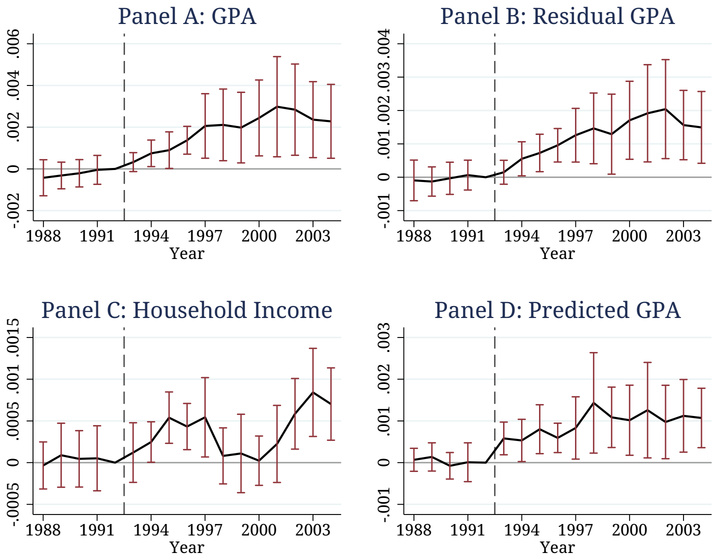
Figure 4.4. Effects of school choice on class-level segregation by ability and household characteristics measured with R^2: event-study specification

Columns 5–8 of Panel C in Table 4.4 collects the reduced form and IV estimates for these outcomes for all segregation indices. Class-level segregation by household income measured with R^2 is positive but statistically insignificant for both reduced form and IV specifications. If measured with overexposure and dissimilarity index, the results show an positive effect of 1.8–2.7pps. This amounts to 0.2-0.8 standard deviations. The estimates for predicted GPA are slightly higher, around 1–2 standard deviations (3.8–5.7pps if measures with R^2, 3.5–5.3pps with overexposure, and 4.8–7.2pps with dissimilarity index).

These estimates for class-level segregation by ability and household characteristics are comparable both in sing and size to the estimates for segregation of schools, studied in Section 4.6.1. This is not surprising, as class-level segregation is likely to reflect both sorting of students across schools and sorting of students across classes within these schools. I will next disentangle these and study within school sorting only.

Within school sorting Within school sorting reflects class-level segregation *in addition* to segregation of schools. It is measured using *partial* R^2. This segregation measure takes into account school-level differences and also all changes in school-level differences as a result of the school choice reform. Thus, this measure is unaffected by increasing segregation of schools.

The non-parametric event-study specification estimates for within school sorting are depicted in Figure 4.5. These estimates show similar patterns as school and class-level



Notes: Figure shows point estimates from an event-study specification and their 95% confidence intervals. Standard errors are clustered at municipal level. Gray dashed lines mark the introduction of the school choice reform. The indices have been corrected to measure deviation from randomness.

Figure 4.5. Effects of school choice on within school sorting by ability and household characteristics measured with partial R^2: event-study specification

segregation—except for household income. The estimates for within school sorting by household income are positive and statistically significant during the mid-90s but this effect tails off in 1998 and shows up again with gradually increasing positive trend after the 2000's.

Panel D in Table 4.4 presents the reduced form and IV estimates for within school sorting. These estimates suggest that within school sorting by ability increased roughly by 3–5 standard deviations (6.9–10.4pps for GPA and 4.3–6.5pps for residual GPA). Within school sorting by household characteristics increased around 1–3 standard deviations (1.1–1.7pps for household income and 3.1–4.5pps for predicted GPA).

Discussion To sum up, class-level segregation by ability and household characteristics increased after the reform in municipalities with more school choice opportunities. To put this differently, I find that the learning environment of students changed after the reform, and students from different ability and household characteristics were less likely to meet in a classroom.

I also find that schools seem to have changed the way students are sorted into classes within schools after the school choice reform. Although I cannot fully explain this finding because of data limitations, this is likely to stem from the original motivation of the Finnish school choice reform that led to specialization of schools. After the reform, schools specialized both in terms of elective courses that students of the school could freely choose and also introduced specialized programs (Kalalahti et al. 2015).[21] Student selection into specialized programs that offer extra teaching hours in certain topics can be based on aptitude tests, rather than distance to schools. Students enrolled in such programs form a separate class within the school. Similarly, students can be sorted into classes based on their elective course choices (Puttonen 2019), for example, to help schedule teaching or to optimize the level and pace of teaching.[22]

If these elective course choices and selection into specialized programs differ by ability or socioeconomic background of the student, as suggested by the findings of Kupiainen and Hotulainen (2019), this could explain why within school sorting increased after the reform.[23] This explanation remains speculative, as my data does not contain information on elective courses or specialized programs.

[21]Technically, municipality would decide on the special programs each municipal-run school could offer, but schools do have autonomy regarding the elective courses they offer (Varjo and Kalalahti 2011).

[22]This type of sorting is even in accordance with the Basic Education Act that states that classes should be formed in a manner that guarantees that the goals of the curriculum guidelines are fulfilled (L 628/1998, section 30). As an example, this law has been used to justify sorting of students into classes based on native language in one comprehensive school in Helsinki. However, the court ruled this to be against the Non-discrimination Act (L 21/2004, section 6) and the Constitution of Finland (L 731/1999, section 6) (see Helsinki Administrative Court 2007, for court ruling).

[23]Note that increased within school sorting as a result of school specialization would be picked up by my identification strategy only if schools specialize more in larger municipalities. This is likely to be the case, especially in the case of specialized programs (see Seppänen 2006), but I cannot verify it with my data.

Table 4.4. The effects of school choice on school, residential, and class-level segregation, and within school sorting.

Segregation by:	GPA		Residual GPA		Household Income		Predicted GPA	
	RF	IV	RF	IV	RF	IV	RF	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Segregation of schools								
R^2	0.00083***	0.00277***	0.00033**	0.00112***	-0.00013	-0.00045	0.00047***	0.00157***
	(0.00018)	(0.00031)	(0.00013)	(0.00025)	(0.00007)	(0.00024)	(0.00012)	(0.00030)
		[24.0]		[24.0]		[23.9]		[23.9]
N	6,759	6,759	6,759	6,759	6,778	6,778	6,778	6,778
Overexposure	0.00068***	0.00228***	0.00025*	0.00083***	0.00024**	0.00081**	0.00041***	0.00136***
	(0.00018)	(0.00025)	(0.00011)	(0.00023)	(0.00008)	(0.00027)	(0.00008)	(0.00021)
		[24.0]		[24.0]		[23.9]		[23.9]
N	6,759	6,759	6,759	6,759	6,778	6,778	6,778	6,778
Dissimilarity	0.00159***	0.00530***	0.00077**	0.00258***	0.00024	0.00080	0.00073***	0.00243***
	(0.00045)	(0.00070)	(0.00030)	(0.00056)	(0.00021)	(0.00064)	(0.00021)	(0.00043)
		[24.0]		[24.0]		[23.9]		[23.9]
N	6,759	6,759	6,759	6,759	6,778	6,778	6,778	6,778
Panel B: Residential segregation								
R^2	0.00024*	0.00081*	0.00004	0.00014	-0.00022	-0.00074	-0.00008	-0.00025
	(0.00010)	(0.00034)	(0.00009)	(0.00028)	(0.00023)	(0.00065)	(0.00015)	(0.00047)
		[24.0]		[24.0]		[23.9]		[23.9]
N	6,759	6,759	6,759	6,759	6,778	6,778	6,778	6,778
Overexposure	0.00017	0.00057	0.00028	0.00093	0.00044**	0.00148***	0.00031**	0.00102***
	(0.00009)	(0.00031)	(0.00021)	(0.00059)	(0.00014)	(0.00040)	(0.00011)	(0.00030)
		[24.0]		[24.0]		[23.9]		[23.9]
N	6,759	6,759	6,759	6,759	6,778	6,778	6,778	6,778
Dissimilarity	0.00011	0.00036	0.00011	0.00036	0.00046	0.00155	0.00017	0.00057
	(0.00024)	(0.00081)	(0.00016)	(0.00049)	(0.00025)	(0.00087)	(0.00019)	(0.00067)
		[24.0]		[24.0]		[23.9]		[23.9]
N	6,759	6,759	6,759	6,759	6,778	6,778	6,778	6,778
Panel C: Class-level segregation								
R^2	0.00278**	0.00927***	0.00161*	0.00538***	0.00012	0.00041	0.00114**	0.00379***
	(0.00104)	(0.00186)	(0.00067)	(0.00126)	(0.00015)	(0.00046)	(0.00037)	(0.00062)
		[24.0]		[24.0]		[23.9]		[23.9]
N	6,759	6,759	6,759	6,759	6,778	6,778	6,778	6,778
Overexposure	0.00167*	0.00557***	0.00088	0.00295**	0.00053***	0.00177***	0.00106***	0.00352***
	(0.00071)	(0.00142)	(0.00047)	(0.00107)	(0.00013)	(0.00038)	(0.00031)	(0.00045)
		[24.0]		[24.0]		[23.9]		[23.9]
N	6,759	6,759	6,759	6,759	6,778	6,778	6,778	6,778
Dissimilarity	0.00234*	0.00782***	0.00122*	0.00406**	0.00054*	0.00179**	0.00143**	0.00476***
	(0.00095)	(0.00187)	(0.00058)	(0.00128)	(0.00023)	(0.00068)	(0.00044)	(0.00068)
		[24.0]		[24.0]		[23.9]		[23.9]
N	6,759	6,759	6,759	6,759	6,778	6,778	6,778	6,778
Panel D: Within school sorting								
Partial R^2	0.00207*	0.00690***	0.00130*	0.00434***	0.00034*	0.00114***	0.00091*	0.00305***
	(0.00090)	(0.00179)	(0.00055)	(0.00108)	(0.00014)	(0.00033)	(0.00036)	(0.00071)
		[24.0]		[24.0]		[23.9]		[23.9]
N	6,759	6,759	6,759	6,759	6,778	6,778	6,778	6,778

Notes: Unit of observation is municipality. All indices have been randomness-corrected. The regressions control for year and municipal-level fixed effects. GPA, residual GPA, household income, and predicted GPA refer to the variables by which segregation is measured. RF refers to reduced form and IV to instrumental variable estimation. Standard errors are clustered at the municipal-level and are shown in parentheses. First stage F-statistics are in brackets. *N* refers to number of observations.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$.

4.7 Conclusions and Discussion

This paper showed that a nationwide universal public school choice reform increased segregation of schools by ability and household characteristics in municipalities with more choice opportunities. There is no consistent evidence that the school choice reform affected residential segregation. This paper also showed that students from different ability and household characteristics are less likely to meet in a classroom after the reform, as classes became more segregated in municipalities with more choice opportunities. Furthermore, this paper provided suggestive evidence that schools changed the way students are sorted into classes after the reform.

Increasing segregation of schools and classes in a public education system can be problematic, especially in the Finnish context. The idea that tax payer funded comprehensive school system should provide equal opportunities to all students could be at odds with a school choice policy that potentially leads to students from more affluent households attending better peer quality schools and classes (see Pekkarinen et al. 2009, for a discussion about the aims of the Finnish comprehensive education system). However, the levels of segregation are still relatively modest in international terms (OECD 2017).

There are several interesting features in the Finnish school choice reform that are out of the scope of this paper but that have potential for future research. First, the effects of the reform on residential segregation could be studied via the change in capitalization of school quality into housing prices (see Machin and Salvanes 2016) or the change in mobility of households with comprehensive school aged children. Second, the role of specialized programs and use of aptitude tests in increasing segregation of schools and classes should be studied. These would be a great extensions to this study.

The results of this paper highlight that school choice reforms can have unintended consequences on segregation. Increased segregation means fewer opportunities for social interactions across income (and ability) groups, which may be undesirable because of its potential repercussions, for example, on intergenerational mobility (see Chetty et al. 2014, for evidence and discussion).

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Appendix

This document contains auxiliary materials to the paper "The Unintended Consequences of a Public School Choice Reform on Segregation". Appendix A provides further derivation of the overexposure index and discusses the construction of data used to measure segregation. Additional summary statics are shown in Appendix B. Appendix C discusses the approximation of treatment intensity and school choice activity, and shows the results from my first stage specification. In Appendix D, I show additional figures and tables of my results. Lastly, I test the sensitivity of my results in Appendix E.

4.A Measuring Segregation

This section provides supplementary material for Section 4.4. I will first fully derive the overexposure index and then describe the sample that I use to calculate segregation.

Derivation of Overexposure Index To define the overexposure index, I start by defining some notation and the own-group peer exposure index.[24] Firstly, there are all together M_m students within a municipality m. These student are divided into two groups g (high/low GPA, residual GPA, household income, or predicted GPA) of size N_{gm}. The own-group peer exposure is defined as follows,

$$e_{igsm} = \frac{1}{n_{sm}-1} \sum_{\substack{j \in s \\ j \neq i}}^{n_{sm}} D_{jgsm}, \tag{A1}$$

where e_{igsm} is the exposure of student i to his/hers own group g in school/residential location/class s and municipality m. D_{jgs} equals 1 if peer j in school/residential location/class s and municipality m also belongs to group g, and 0 otherwise. n_sm is the size of the school/residential location/class. Thus, e_{igsm} is a measure of the share of peers who belong to the same group.

Next, I will sum over all the individuals and the two groups to get an average own-group peer exposure:

$$\bar{e}_m = \frac{1}{M_m} \sum_{g=1}^{2} \sum_{i \in g} e_{igsm}. \tag{A2}$$

If students were randomly allocated to the schools/residential locations/classes of schools of the municipality, the expected share of peers belonging to the same group should equal the share of this group within the municipality, $\frac{N_{gm}}{M_m}$. Hence, the expected own-group peer exposure is defined as follows,

$$\hat{e}_m = \frac{1}{M_m} \sum_{g=1}^{2} \sum_{i \in g} \left(\frac{N_{gm}}{M_m} \right)_{igsm} \tag{A3}$$

Finally, the overexposure index for each municipality follows,

$$Overexposure_m = \frac{\bar{e}_m}{\hat{e}_m}. \tag{A4}$$

[24]I follow notation from Böhlmark et al. (2016).

If $Overexposure_m > 1$, then there is excess sorting of schools/residential locations/classes on top of what the expected distribution of these groups within the municipality would imply.

Sample Used to Calculate the Segregation Indices There are couple of additional data construction measures I make in order to calculate the segregation indices. For segregation of schools, the first difference is that I only take into account schools that have at least five students in the cohort in a given year. This is because some of these small schools, specifically in larger cities, are likely to be hospital schools, and thus not part of a choice set of a student. On the contrary, these are likely to be actual schools in smaller municipalities. Including these does not alter my results, but I decide not to include these. Second, I also require municipalities to have at least two schools, as there cannot be any sorting of students between schools in municipalities with fewer than two schools. This means that I require the municipality to have at least 10 students to meaningfully measure segregation of schools (i.e. at least two schools with minimum five students) each year. A municipality that does not fulfil these requirement or has no schools will be coded as if there is no segregation between the schools.[25] These restrictions on sample construction are also used in the case of within school sorting, as this measure calculates the class-level segregation in addition to segregation of schools.

For residential segregation, I use similar restrictions. First, the minimum number of students residing in postal codes is five. Second, I require a municipality to have at least two postal codes (with students residing on them) with at least five students in each of these per year.

For class-level segregation, I require the municipality to have at least one school with at least two classes and 10 students. In addition, I correct some potentially misspecified class codes in my data in the following way. The class code is a two-digit code, usually with a number (such as nine as it refers to 9th grade, which is the first time I observe the students) followed by a letter, such as A or B. First, in case the letter is in lower case, I change it to upper case because it is likely that, for example, classes 9A and 9a refer to the same class within the same school. Second, I take all the missing class codes to be one class (in a given school and year). The rest of the potentially misspecified class codes I treat as if actual classes. This is because in many cases there seems to be several observations of same potentially misspecified class codes within the school.

[25] Some municipalities have no schools offering grades 7 to 9. Students within these municipalities attend schools of neighboring municipalities.

4.B Additional Summary Statistics

This section will present additional descriptive evidence from Section 4.4.3. Table B1 shows descriptive evidence on residual and predicted GPA before and after the reform in large and small municipalities. Table B2 summarizes the index values for school and residential segregation measured using overexposure and dissimilarity index before and after the reform in large and small municipalities.

Figures B1 and B2 show how school, residential, and class-level segregation measured with the overexposure and dissimilarity index, respectively, develop in three big cities Helsinki, Turku, and Vantaa over the sample period. Reassuringly, these indices show very similar patterns as the ones measured using R^2, in Section 4.4.3.

Table B1. Descriptive evidence on residual and predicted GPA before and after the reform and in small and large municipalities.

	Large municipalities		Small municipalities	
	1988-1992	1993-2004	1988-1992	1993-2004
	(1)	(2)	(3)	(4)
Panel A: Residual GPA				
Average	-0.03	0.01	0.01	-0.00
	(1.01)	(0.98)	(1.03)	(0.99)
	[71,276]	[174,620]	[222,487]	[516,095]
Low-income	0.00	0.02	-0.02	0.00
	(1.08)	(1.06)	(1.07)	(1.03)
	[12,315]	[34,794]	[61,117]	[137,873]
High-income	-0.03	-0.00	0.06	-0.01
	(0.94)	(0.91)	(0.96)	(0.93)
	[28,091]	[64,891]	[45,345]	[107,775]
Panel B: Predicted GPA				
Average	7.84	7.77	7.66	7.61
	(0.56)	(0.57)	(0.49)	(0.49)
	[71,276]	[174,620]	[222,487]	[516,095]
Low-income	7.40	7.32	7.41	7.34
	(0.43)	(0.42)	(0.41)	(0.41)
	[12,315]	[34,794]	[61,117]	[137,873]
High-income	8.23	8.19	8.08	8.04
	(0.51)	(0.50)	(0.50)	(0.49)
	[28,091]	[64,891]	[45,345]	[107,775]

Notes: Large municipalities refers to municipalities with at least 10 schools and small municipalities to municipalities with fewer than 10 schools before the reform. Standard errors are clustered at municipal-level and shown in parentheses. Number of observations in brackets.

Table B2. Descriptive evidence on school, residential, class-level segregation before and after the reform and in small and large municipalities measured using overexposure and dissimilarity index.

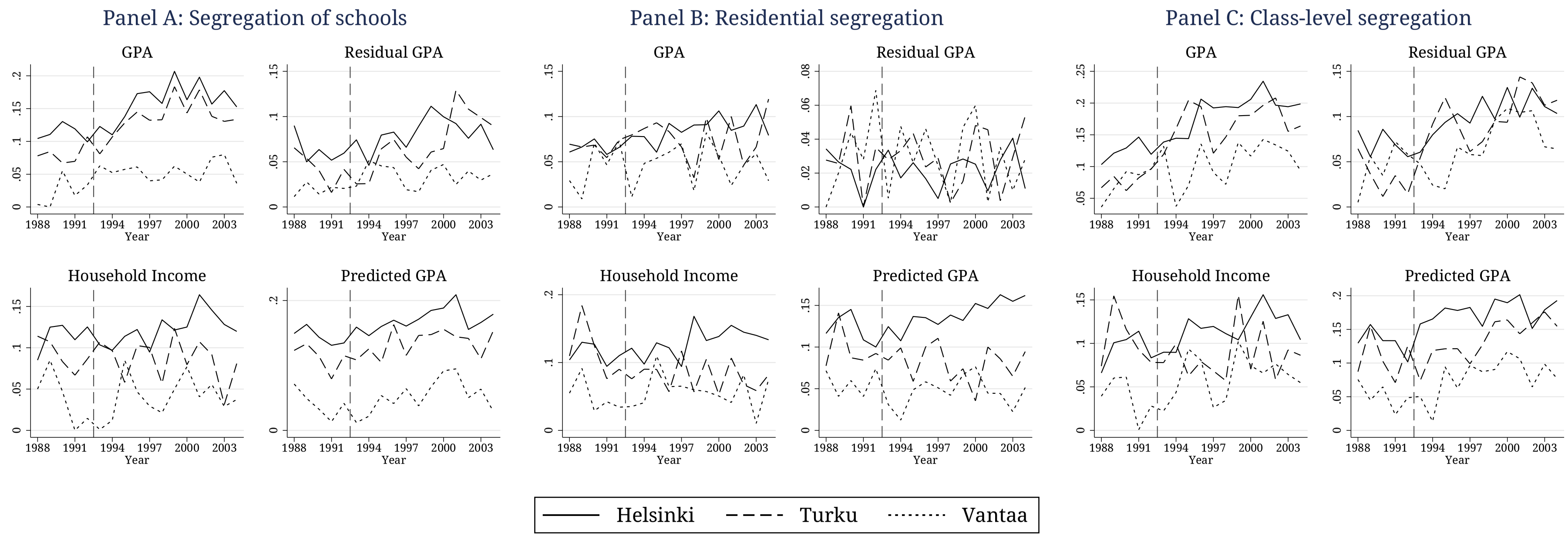
	Large municipalities		Small municipalities	
	1988-1992	1993-2004	1988-1992	1993-2004
	(1)	(2)	(3)	(4)
Panel A: Segregation of schools				
Overexposure				
GPA	1.021	1.039	1.000	1.001
	(0.014)	(0.021)	(0.007)	(0.010)
Residual GPA	1.011	1.017	1.000	1.000
	(0.009)	(0.011)	(0.008)	(0.010)
Household income	1.020	1.027	1.001	1.001
	(0.011)	(0.014)	(0.009)	(0.010)
Predicted GPA	1.028	1.038	1.000	1.001
	(0.015)	(0.020)	(0.008)	(0.009)
Dissimilarity				
GPA	0.063	0.105	0.006	0.008
	(0.034)	(0.044)	(0.021)	(0.029)
Residual GPA	0.035	0.054	0.006	0.008
	(0.020)	(0.028)	(0.022)	(0.027)
Household income	0.072	0.083	0.009	0.009
	(0.031)	(0.038)	(0.030)	(0.030)
Predicted GPA	0.087	0.107	0.007	0.008
	(0.037)	(0.047)	(0.025)	(0.026)
Panel B: Residential segregation				
Overexposure				
GPA	1.015	1.020	0.992	0.990
	(0.010)	(0.010)	(0.038)	(0.038)
Residual GPA	1.007	1.006	0.989	0.985
	(0.008)	(0.007)	(0.057)	(0.057)
Household income	1.026	1.030	1.000	0.997
	(0.015)	(0.013)	(0.038)	(0.037)
Predicted GPA	1.022	1.027	0.991	0.989
	(0.012)	(0.014)	(0.033)	(0.036)

Table continued on the following page.

Table B2. *(continued)* Descriptive evidence on school, residential, class-level segregation before and after the reform and in small and large municipalities measured using overexposure and dissimilarity index.

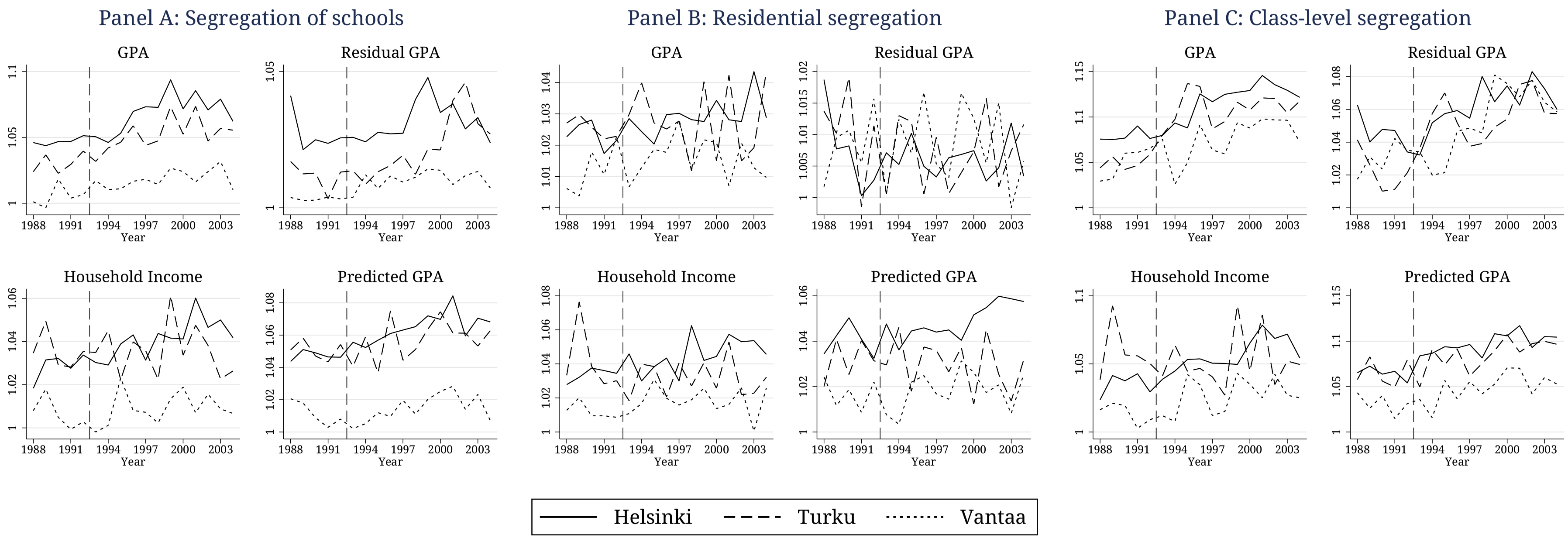
	Large municipalities		Small municipalities	
	1988-1992	1993-2004	1988-1992	1993-2004
	(1)	(2)	(3)	(4)
Dissimilarity				
GPA	0.046	0.055	0.033	0.033
	(0.027)	(0.027)	(0.061)	(0.064)
Residual GPA	0.024	0.024	0.032	0.030
	(0.022)	(0.018)	(0.063)	(0.059)
Household income	0.076	0.079	0.048	0.043
	(0.035)	(0.033)	(0.078)	(0.073)
Predicted GPA	0.068	0.070	0.030	0.031
	(0.037)	(0.040)	(0.055)	(0.061)
Panel C: Class-level segregation				
Overexposure				
GPA	1.039	1.075	0.995	1.007
	(0.022)	(0.038)	(0.042)	(0.057)
Residual GPA	1.024	1.047	0.994	1.004
	(0.017)	(0.027)	(0.046)	(0.061)
Household income	1.034	1.039	1.001	1.001
	(0.022)	(0.021)	(0.034)	(0.038)
Predicted GPA	1.039	1.053	0.989	0.993
	(0.023)	(0.029)	(0.035)	(0.041)
Dissimilarity				
GPA	0.065	0.118	0.034	0.054
	(0.039)	(0.058)	(0.061)	(0.083)
Residual GPA	0.042	0.076	0.032	0.048
	(0.028)	(0.041)	(0.058)	(0.075)
Household income	0.067	0.077	0.044	0.047
	(0.037)	(0.039)	(0.073)	(0.075)
Predicted GPA	0.069	0.093	0.026	0.033
	(0.040)	(0.050)	(0.054)	(0.061)
N	40	96	1,955	4,687

Notes: Large municipalities refers to municipalities with at least 10 schools and small municipalities to municipalities with fewer than 10 schools before the reform. GPA, residual GPA, household income, and predicted GPA refer to the variables by which segregation is measured. Standard errors are clustered at municipal-level and shown in parentheses. All indices have been randomness-corrected. *N* refers to number of observations.



Notes: Gray dashed lines mark the introduction of the school choice reform. All index values have been randomness-corrected.

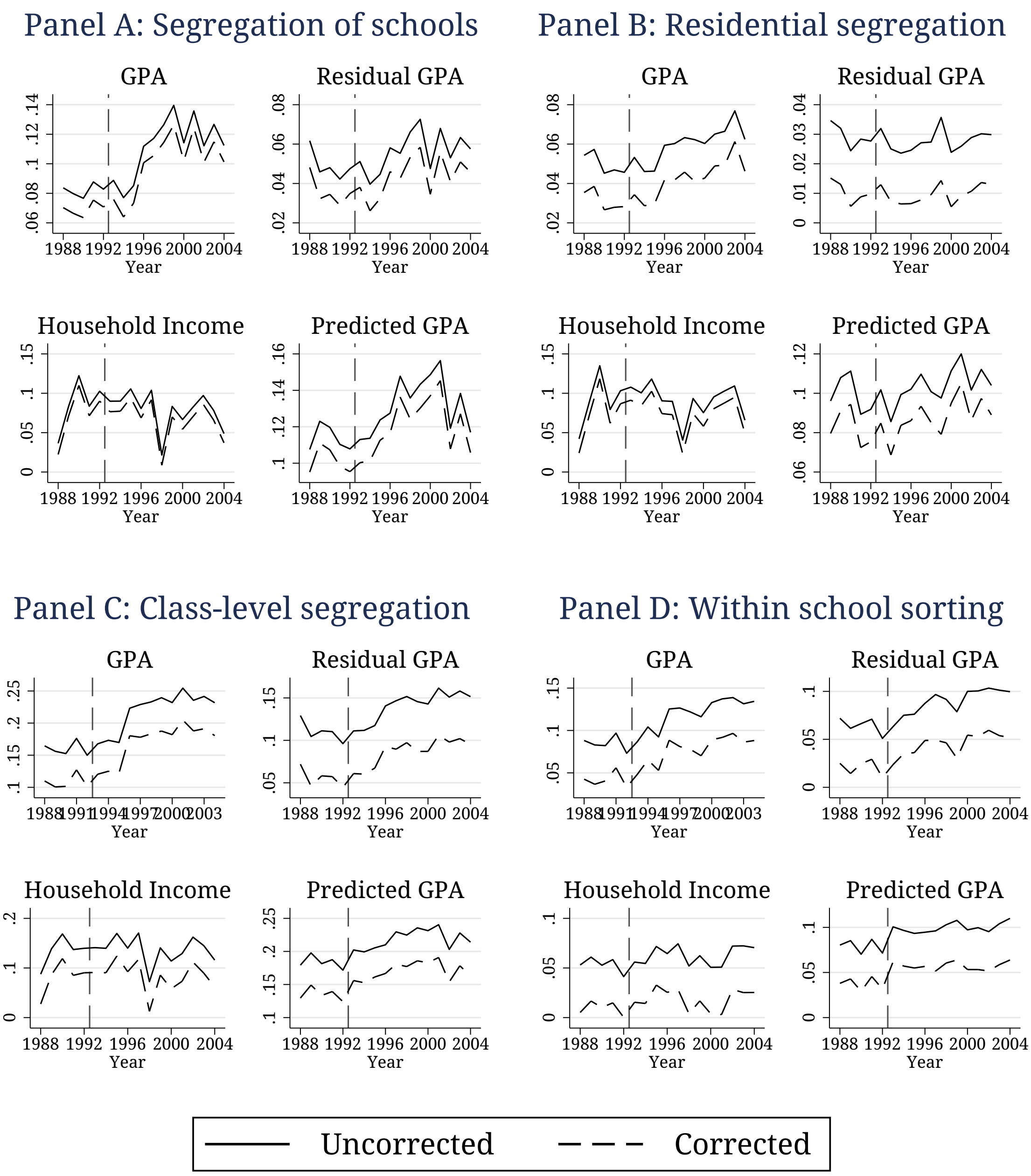
Figure B1. School, residential, and class-level segregation using overexposure index.



Notes: Gray dashed lines mark the introduction of the school choice reform. All index values have been randomness-corrected.

Figure B2. School, residential, and class-level segregation using dissimilarity index.

Comparing Trends in Corrected and Uncorrected Segregation Indices This section will compare trends in segregation measured using randomness-corrected R^2 and uncorrected R^2 (partial R^2 for within school sorting). Figure B3 shows how school, residential, and class-level segregation measured with the corrected and uncorrected R^2, and within school sorting measured with corrected and uncorrected partial R^2, differ in Helsinki during my sample period. The uncorrected values, deviations from evenness, are consistently higher than the corrected ones, but they exhibit similar trends.



Notes: Gray dashed lines mark the introduction of the school choice reform.

Figure B3. Development of school, residential, and class-level segregation measured using R^2 and within school sorting measured using partial R^2 that are not corrected to measure deviation from randomness.

4.C School Choice Activity and Treatment Intensity

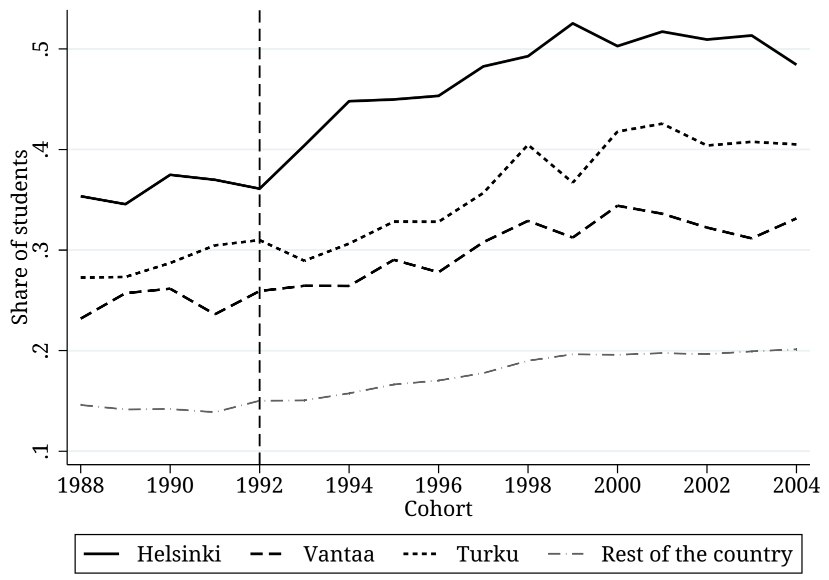
In this section I will discuss how to I approximate school choice and the intensity of the reform used in my identification strategy. I will also provide descriptive evidence on both, and lastly, discuss how the treatment intensity measure of a municipality relates to increased school choice activity. A more in-depth coverage of these issues can be found in Essay 2.

Approximating School Choice Activity Because of the limitations of my data, I do not know if a student made a choice or attends the municipal-assigned neighborhood school. For this, I use the same procedure as in Essay 2, and approximate a neighborhood school via the most common school of a 1km×1km grid for each student. If a student residing in this grid, attends other than the most common school, then she/he has made a choice. This means that the choice approximation is measured with an error. As an example for measurement error, if the grid is at the boarder of two or more school catchment areas, then this approximation is likely to measure the true neighborhood school with error, and thus lead to upward bias.

In addition, this bias might become stronger after the reform in municipalities with more school choice. The most common school of a grid might not be the actual neighborhood school if most of the students residing in the grid have made a choice. This is potentially a threat to the IV strategy introduced in Section 4.5.2.

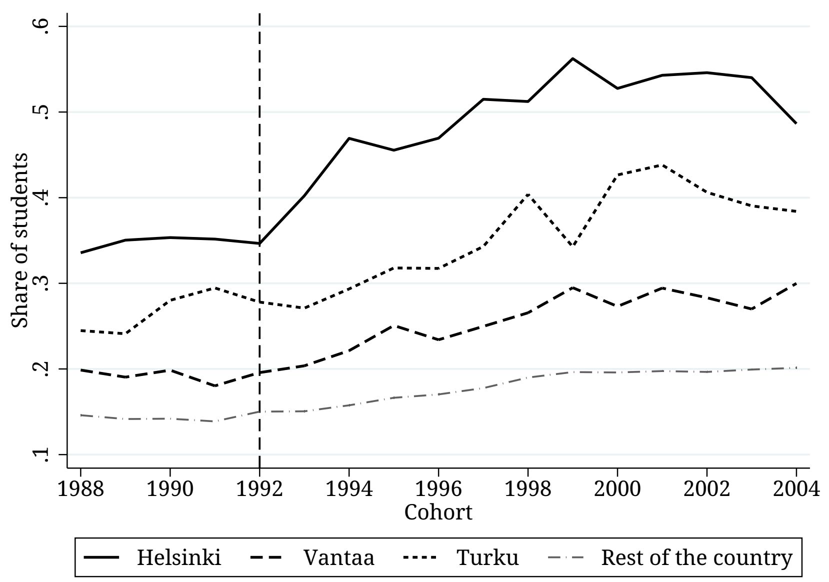
To overcome this, I introduce an alternative measure in which a student in a 1km×1km grid has made a choice if she/he does not attend a school that at least 30 percent of the students within the grid attend. This means that there can be several neighborhood schools within each grid and thus this measure is less vulnerable to increased choice. However, this approximation is likely to severely overestimate school choice activity.

Figures C1 and C2 show descriptive evidence of the share of students who have made a choice in three big cities in Finland that differ in terms of size and approach to school choice policies (Kalalahti et al. 2015). Helsinki, the capital city of Finland, is about double the size of Turku and Vantaa in terms of number of schools and students. School choice seems to increase more in Helsinki and Turku than it does in Vantaa.



Notes: Gray dashed lines mark the introduction of the school choice reform.

Figure C1. Share of 7th grade students who attend other than the most common school of the region in Helsinki, Vantaa, and Turku between 1988 and 2004.



Notes: Gray dashed lines mark the introduction of the school choice reform.

Figure C2. Share of 7th grade students who attend other than the school that 30 percent of the region attend in Helsinki, Vantaa, and Turku between 1988 and 2004.

Intensity of the Reform To construct this measure, I calculate the average number of schools in each municipality between 1988 and 1992. I do not take into account schools with fewer than five students in a given year. This is because in many big cities, such as in Helsinki, these small schools are likely to be hospital schools or schools for disabled students, and are thus not considered a real choice. However, for smaller municipalities, these are likely to be actual schools and the treatment intensity variable will underestimate the intensity of the reform for these municipalities. This choice is made because these small schools overestimate the number of schools in bigger municipalities (almost 10 in Helsinki, for example) more than they underestimate it for smaller municipalities.

Another source of bias for this measure is municipal mergers. In my data construction, I make a choice to merge all municipalities that undergo a merger with another municipality at some point during my sample periods from the first year onward. This means that the treatment intensity measure, number of schools, may overestimate the intensity of the reform in these municipalities slightly.

Figure C3 shows how the treatment intensity variable is distributed across municipalities. The vast majority of municipalities in Finland have only one school, whereas there are only eight with more than ten schools.

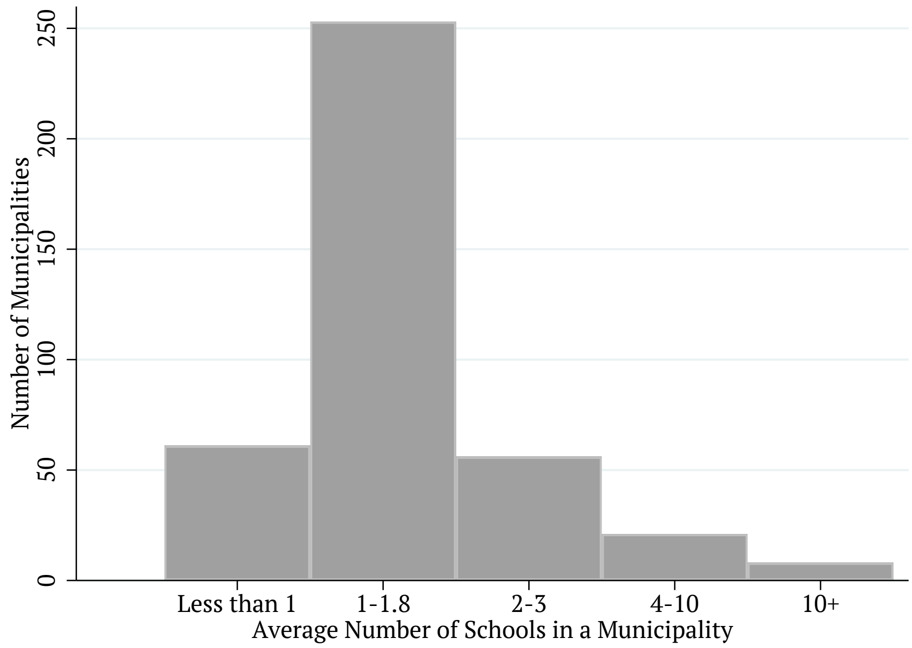
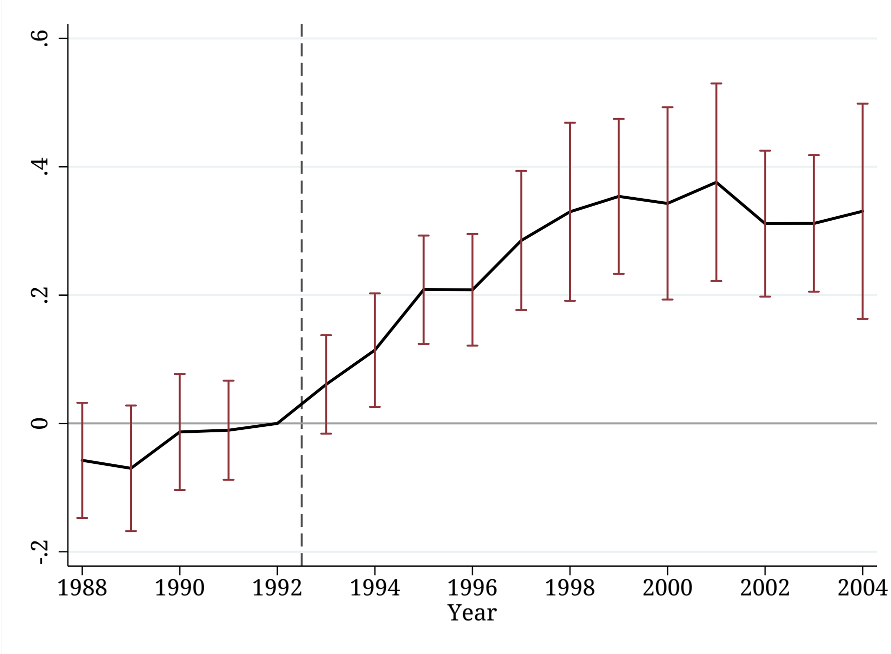


Figure C3. Distribution of average number of schools in municipalities before the reform.

First Stage Next, I will show how my treatment intensity, number of schools before the reform, is related to school choice activity, share of students who attend other than the most common school of the region. Figure C4 shows the results using an event-study specification (Equation 4.7) for the first stage.

School choice increases substantially already from the first years of the reform. The estimates using the first stage specification in equation 4.8, show an increase of 15pps (0.300×50) if a municipality with fewer than two schools is compared to the capital city, Helsinki, with over 50 schools (column 1 of Table C1). Column 2 of Table C1 plots the result using the alternative choice approximation, share of students who attend other than the school that 30 percent of the region attend.



Notes: Figure shows point estimates and their 95% confidence intervals. Gray dashed line marks the introduction of the school choice reform.

Figure C4. The event-study estimates for the effect on the share of students who attend other than the most common school of the region.

Table C1. First stage results for the effect on the share of students who attend other than the most common school of the region.

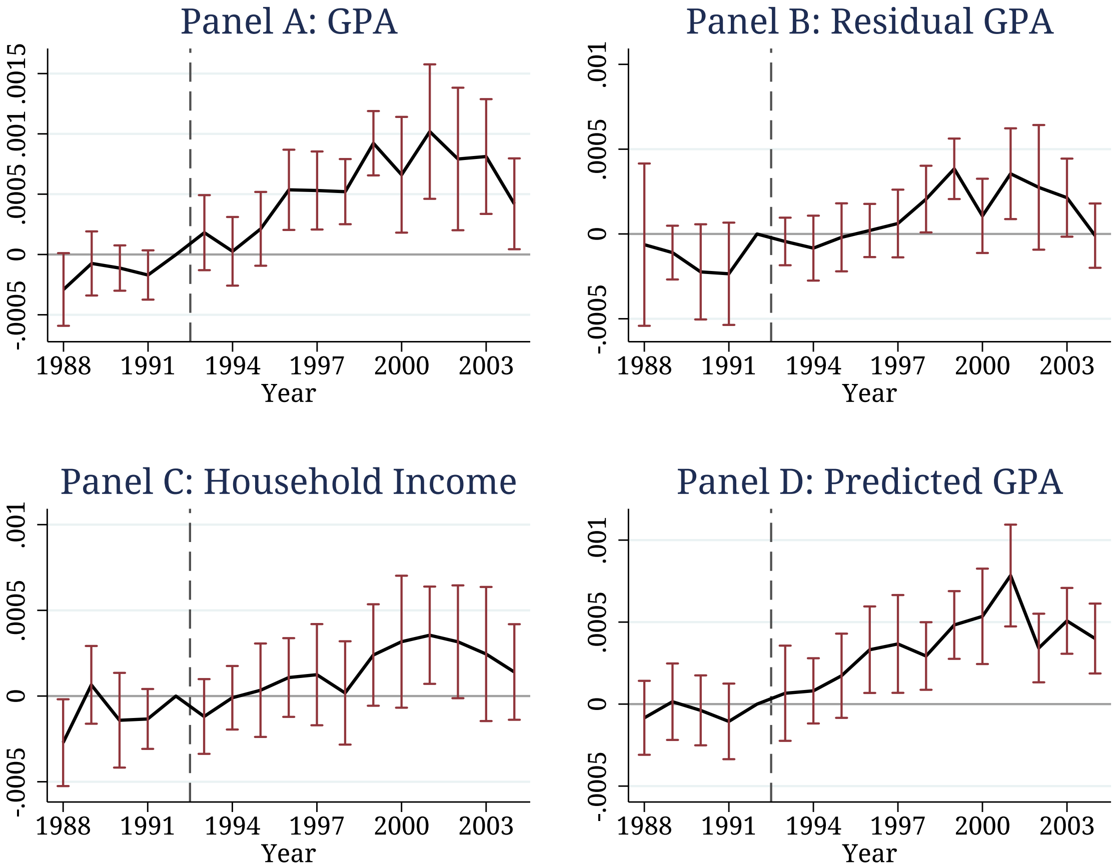
	Share that attends other than the most common school (1)	Share that attends other than the school 30% attend (2)
	0.300***	0.341***
	(0.061)	(0.046)
N	6,778	6,778

Notes: Unit of observation is municipality. Independent variable is the intensity of the reform, approximated by the number of schools in a municipality prior to the school choice reform. The regressions control for year and municipal-level fixed effects. Standard errors are clustered at municipal-level and shown in parentheses. *N* refers to number of observations.
* p<0.05, ** p<0.01, *** p<0.001.

4.D Results Appendix

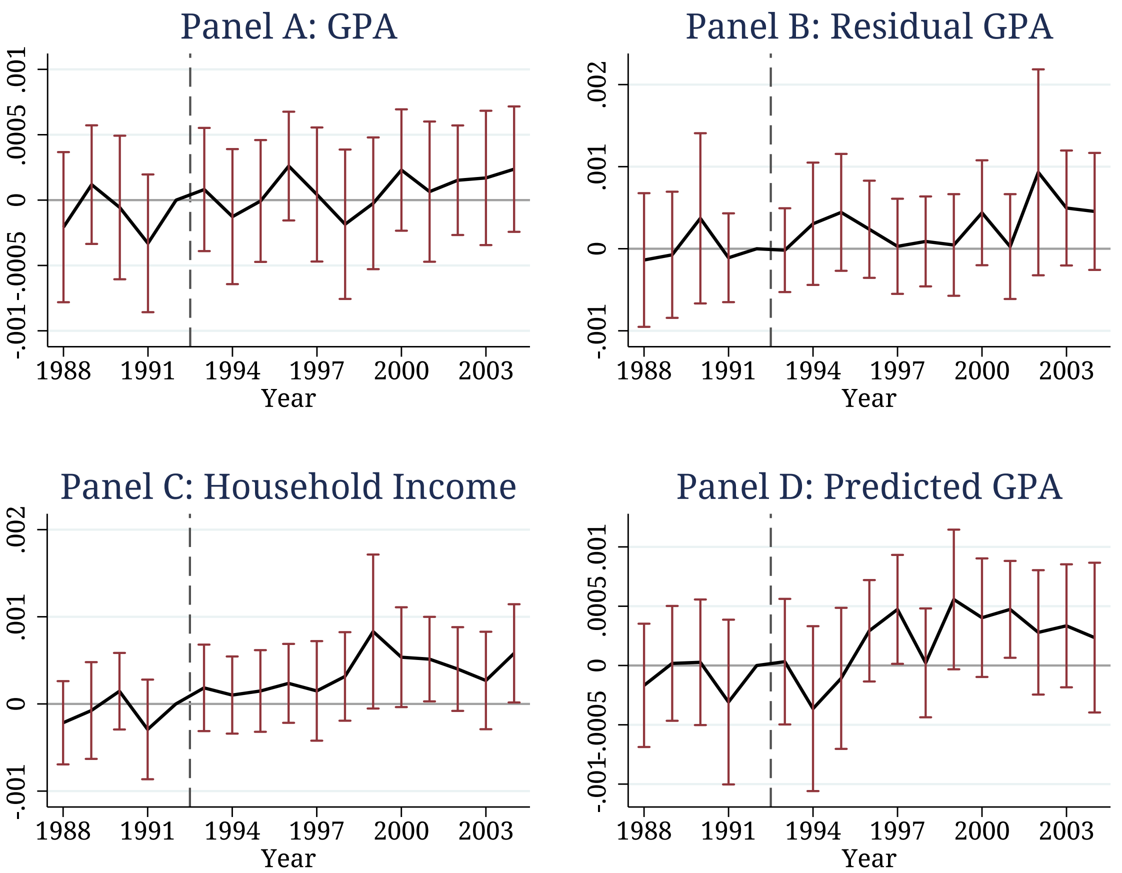
This section will present the event-study estimations using overexposure and dissimilarity index, IV results using the alternative approximation for school choice activity, and sensitivity analysis for segregation of schools.

Non-parametric Event-study Plots using Overexposure and Dissimilarity Index In here, I present the results from the non-parametric event-study specification of Equation 4.7 using overexposure and dissimilarity index. Figures D1–D3 show the results for school, residential, and class-level segregation using the overexposure index. Figures D4–D6 show the results for school, residential, and class-level segregation using the dissimilarity index.



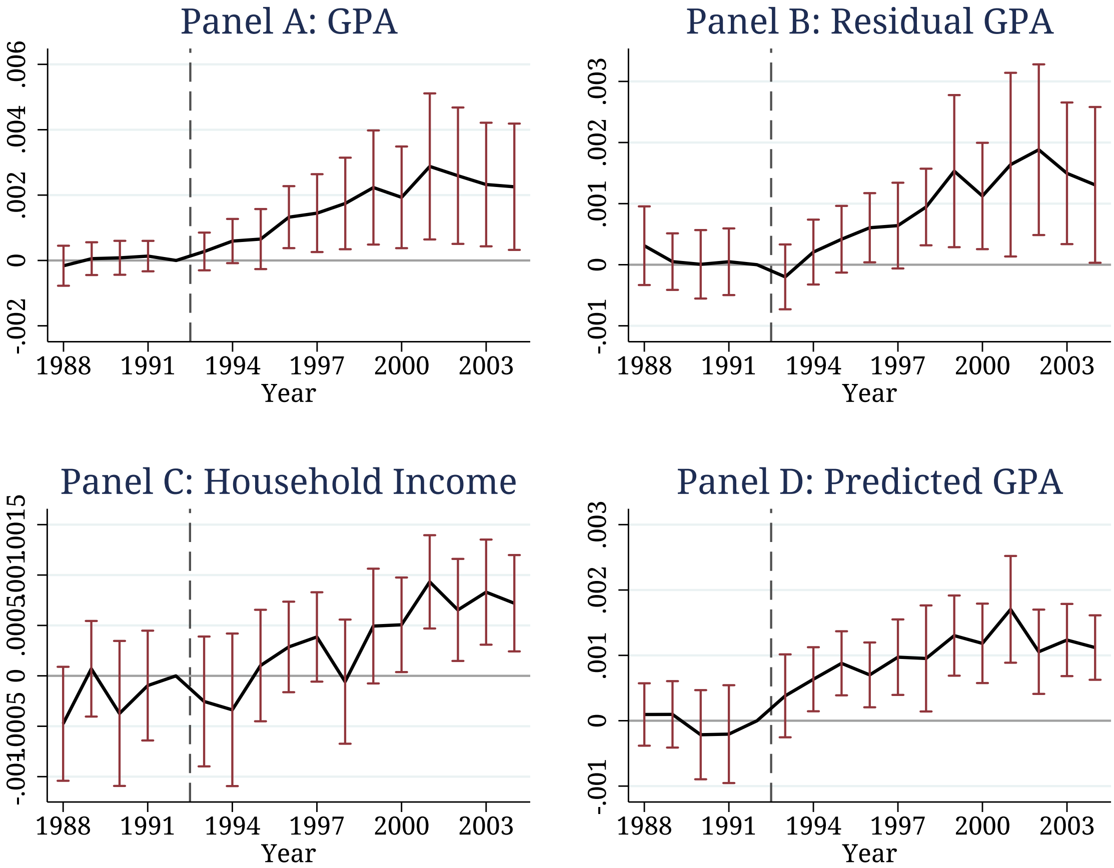
Notes: Figure shows point estimates and their 95% confidence intervals. Gray dashed lines mark the introduction of the school choice reform.

Figure D1. Event-study estimates for the effects of school choice on segregation of schools using overexposure index.



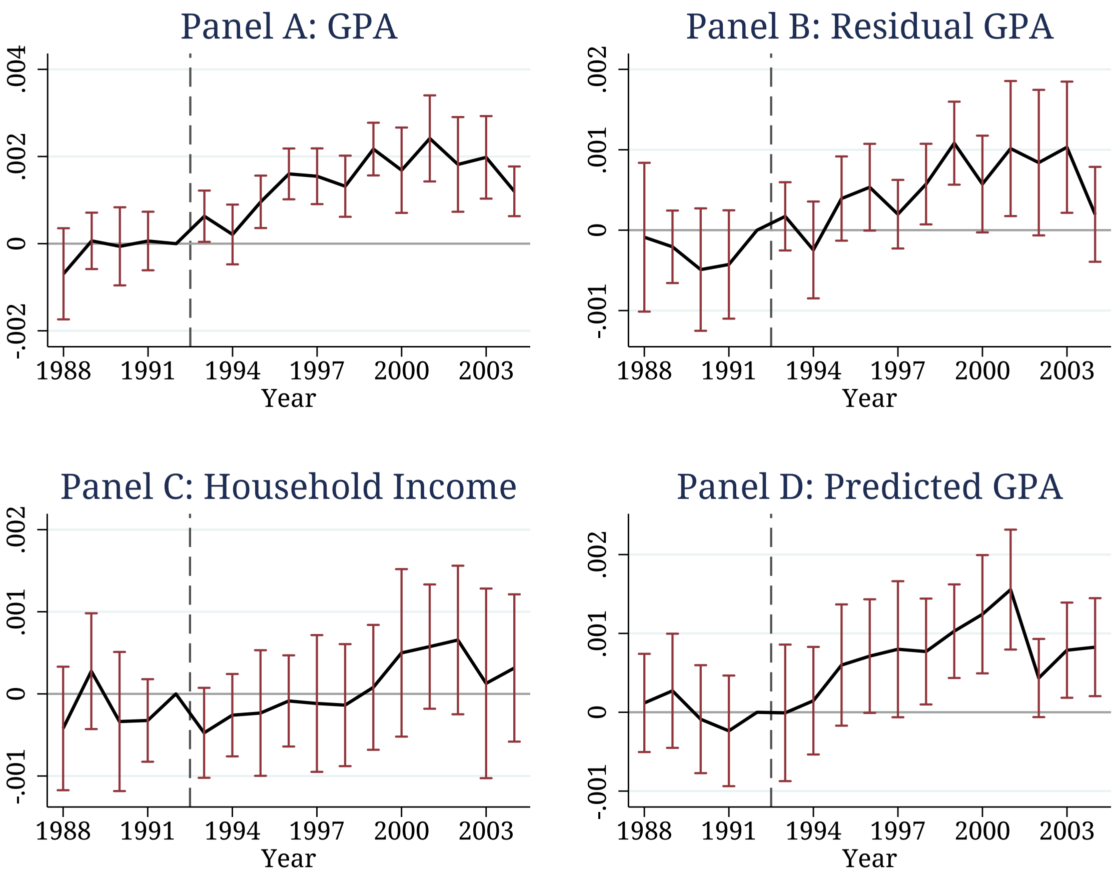
Notes: Figure shows point estimates and their 95% confidence intervals. Gray dashed lines mark the introduction of the school choice reform.

Figure D2. Event-study estimates for the effects of school choice on residential segregation using overexposure index.



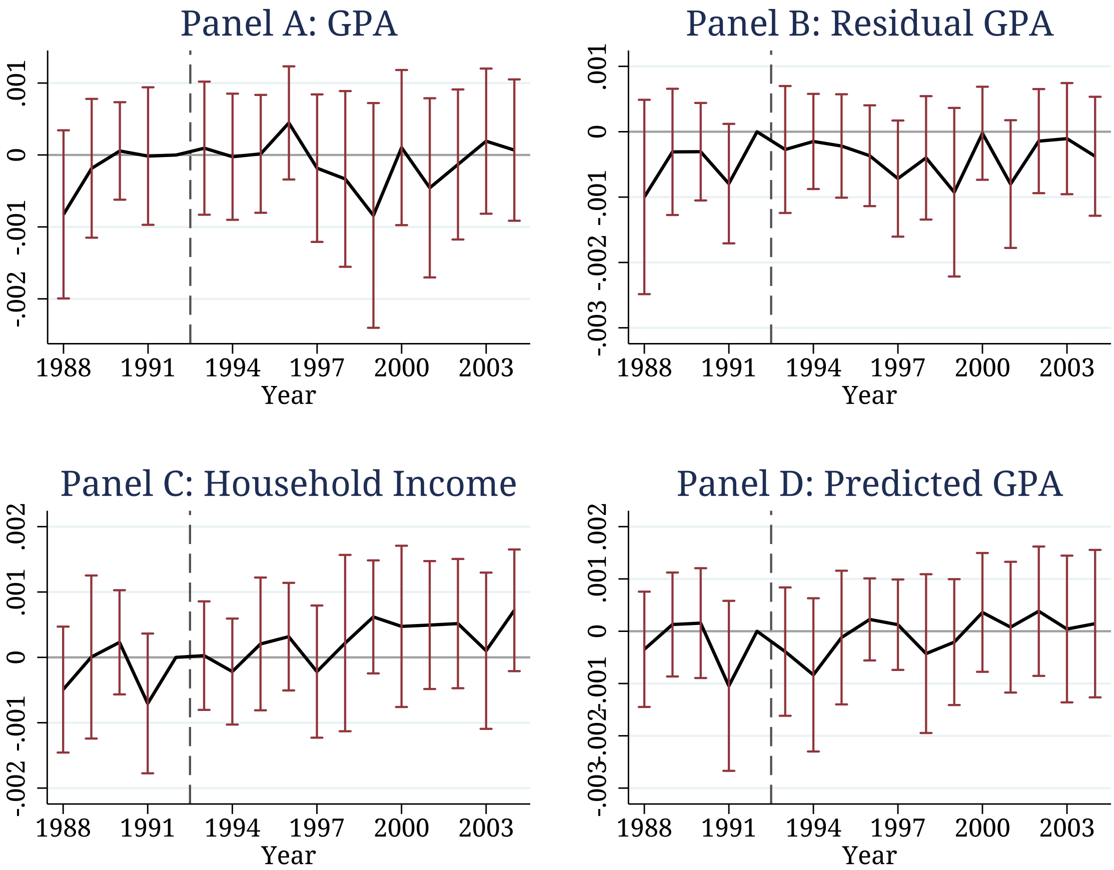
Notes: Figure shows point estimates and their 95% confidence intervals. Gray dashed lines mark the introduction of the school choice reform.

Figure D3. Event-study estimates for the effects of school choice on class-level segregation using overexposure index.



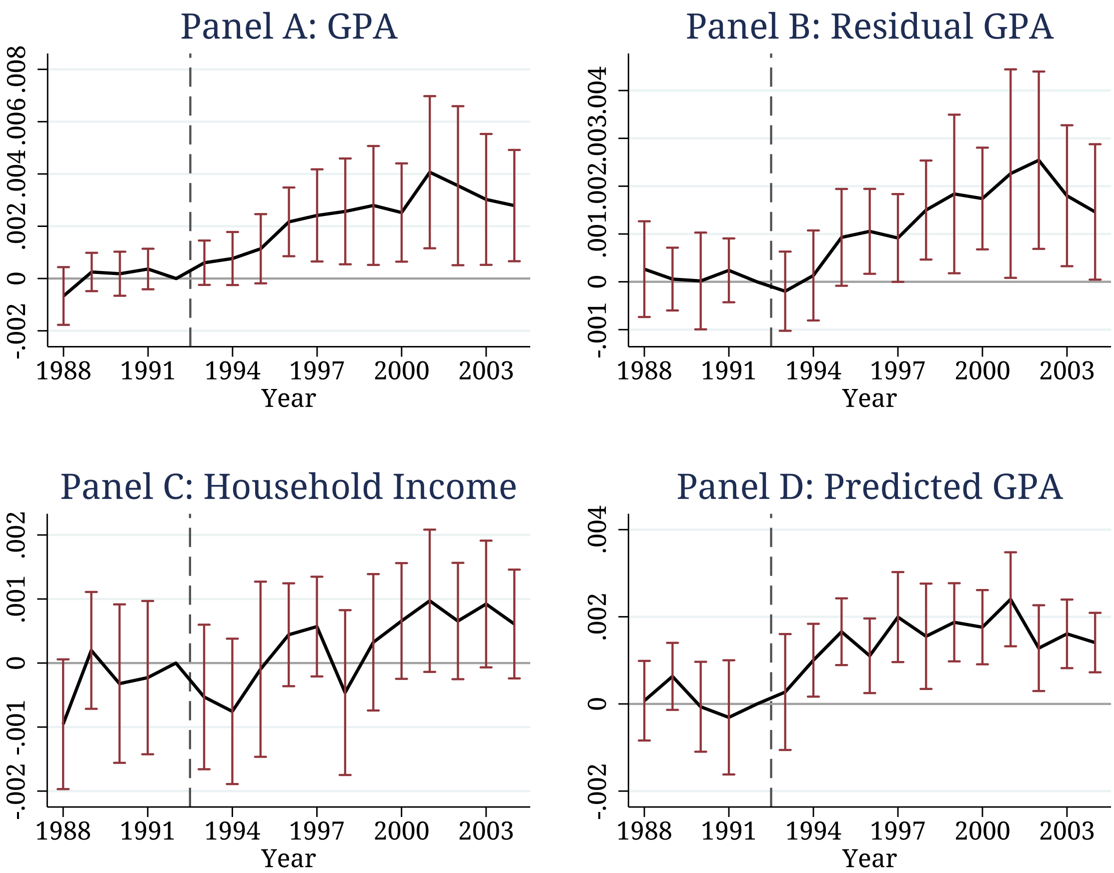
Notes: Figure shows point estimates and their 95% confidence intervals. Gray dashed lines mark the introduction of the school choice reform.

Figure D4. Event-study estimates for the effects of school choice on segregation of schools using dissimilarity index.



Notes: Figure shows point estimates and their 95% confidence intervals. Gray dashed lines mark the introduction of the school choice reform.

Figure D5. Event-study estimates for the effects of school choice on residential segregation using dissimilarity index.



Notes: Figure shows point estimates and their 95% confidence intervals. Gray dashed lines mark the introduction of the school choice reform.

Figure D6. Event-study estimates for the effects of school choice on class-level segregation using dissimilarity index.

IV Results with Alternative Proxy for School Choice Activity In here, I present the IV results using an alternative proxy for school choice activity, share of students who attend other than the school that 30 percent of the region attend, explained in more detail in Section 4.C.

The results closely follow the IV results presented in Section 4.6 (see Table D1).

Table D1. The effects of school choice on school, residential, and class-level segregation and within school sorting using IV specification with alternative school choice approximation: share of students attending other than the school that at least 30 percent of the students of the region attend.

Segregation by:	GPA	Residual GPA	Household Income	Predicted GPA
	(1)	(2)	(3)	(4)
Panel A: Segregation of schools				
R^2	0.00243***	0.00098***	-0.00040*	0.00138***
	(0.00028)	(0.00026)	(0.00020)	(0.00027)
	[55.4]	[55.4]	[55.3]	[55.3]
N	6,759	6,759	6,778	6,778
Overexposure	0.00200***	0.00073**	0.00072**	0.00119***
	(0.00028)	(0.00024)	(0.00022)	(0.00017)
	[55.4]	[55.4]	[55.3]	[55.3]
N	6,759	6,759	6,778	6,778
Dissimilarity	0.00466***	0.00227***	0.00070	0.00214***
	(0.00079)	(0.00060)	(0.00058)	(0.00042)
	[55.4]	[55.4]	[55.3]	[55.3]
N	6,759	6,759	6,778	6,778
Panel B: Residential segregation				
R^2	0.00072*	0.00012	-0.00065	-0.00022
	(0.00028)	(0.00024)	(0.00060)	(0.00042)
	[55.4]	[55.4]	[55.3]	[55.3]
N	6,759	6,759	6,778	6,778
Overexposure	0.00050	0.00082	0.00130***	0.00090**
	(0.00026)	(0.00055)	(0.00036)	(0.00027)
	[55.4]	[55.4]	[55.3]	[55.3]
N	6,759	6,759	6,778	6,778
Dissimilarity	0.00031	0.00031	0.00136	0.00050
	(0.00070)	(0.00044)	(0.00074)	(0.00057)
	[55.4]	[55.4]	[55.3]	[55.3]
N	6,759	6,759	6,778	6,778

Table continued on the following page.

Table D1. *(continued)* The effects of school choice on school, residential, and class-level segregation and within school sorting using IV specification with alternative school choice approximation: share of students attending other than the school that at least 30 percent of the students of the region attend.

Segregation by:	GPA	Residual GPA	Household Income	Predicted GPA
	(1)	(2)	(3)	(4)
Panel C: Class-level segregation				
R^2	0.00815***	0.00473***	0.00036	0.00334***
	(0.00207)	(0.00137)	(0.00041)	(0.00071)
	[55.4]	[55.4]	[55.3]	[55.3]
N	6,759	6,759	6,778	6,778
Overexposure	0.00489**	0.00259*	0.00156***	0.00310***
	(0.00150)	(0.00108)	(0.00033)	(0.00055)
	[55.4]	[55.4]	[55.3]	[55.3]
N	6,759	6,759	6,778	6,778
Dissimilarity	0.00688***	0.00357**	0.00158**	0.00419***
	(0.00198)	(0.00130)	(0.00061)	(0.00079)
	[55.4]	[55.4]	[55.3]	[55.3]
N	6,759	6,759	6,778	6,778
Panel D: Within school sorting				
Partial R^2	0.00607**	0.00381***	0.00100**	0.00268***
	(0.00190)	(0.00116)	(0.00032)	(0.00076)
	[55.4]	[55.4]	[55.3]	[55.3]
N	6,759	6,759	6,778	6,778

Notes: Unit of observation is municipality. All indices have been randomness-corrected. The regressions control for year and municipal-level fixed effects. GPA, residual GPA, household income, and predicted GPA refer to the variables by which segregation is measured. Standard errors are clustered at municipal-level and shown in parentheses. First stage F-statistics is in brackets. *N* refers to number of observations.
* p<0.05, ** p<0.01, *** p<0.001.

4.E Sensitivity Analysis

Uncorrected Segregation Index Values I start with presenting the results without randomness-correcting the segregation measures. I will only show the results using the reduced form approach (Equation 4.6). Descriptive evidence can be found in Appendix Section 4.B.

This sensitivity analysis is meant to address the critique by Allen et al. (2015). They argue that the corrected measure of systematic segregation underestimates the true segregation by a larger amount than the uncorrected measure overestimates it. Although, this is only problematic for my identification strategy if the bias introduced by the correction correlates with my treatment intensity measure (or systematically changes after the reform), the estimates for uncorrected segregation are still of interest. This I deem unlikely.

Table E1 shows the reduced form estimates for segregation of schools (Panel A), residential segregation (Panel B) and class-level segregation (Panel C) measured using uncorrected R^2 and dissimilarity index, as well as the exposure index (not scaled with the expected exposure). Panel D shows the results for within school sorting. These are all very similar in size (and sign) to the results presented in the main result section.

Table E1. The effects of school choice on segregation of schools using the index values that measure deviation from *evenness*.

Segregation by:	GPA (1)	Residual GPA (2)	Household Income (3)	Predicted GPA (4)
Panel A: Segregation of schools				
R^2	0.00085***	0.00036*	-0.00010	0.00049***
	(0.00021)	(0.00015)	(0.00007)	(0.00014)
N	6,759	6,759	6,778	6,778
Exposure	0.00126*	0.00173**	-0.00041	0.00022
	(0.00049)	(0.00055)	(0.00024)	(0.00020)
N	6,759	6,759	6,778	6,778
Dissimilarity	0.00156**	0.00078*	0.00018	0.00070**
	(0.00051)	(0.00035)	(0.00023)	(0.00025)
N	6,759	6,759	6,778	6,778
Panel B: Residential segregation				
R^2	0.00019	-0.00000	-0.00022	-0.00008
	(0.00011)	(0.00011)	(0.00023)	(0.00016)
N	6,759	6,759	6,778	6,778
Exposure	0.00118	0.00193*	0.00030	0.00077
	(0.00072)	(0.00092)	(0.00066)	(0.00062)
N	6,759	6,759	6,778	6,778
Dissimilarity	0.00010	0.00009	0.00038	0.00014
	(0.00024)	(0.00023)	(0.00028)	(0.00020)
N	6,759	6,759	6,778	6,778

Table continued on the following page.

Table E1. *(continued)* The effects of school choice on segregation of schools using the index values that measure deviation from *evenness.*

Segregation by:	GPA	Residual GPA	Household Income	Predicted GPA
	(1)	(2)	(3)	(4)
Panel C: Class-level segregation				
R^2	0.00207**	0.00110*	0.00013	0.00049***
	(0.00079)	(0.00049)	(0.00017)	(0.00014)
N	6,759	6,759	6,778	6,778
Exposure	0.00203*	0.00244**	0.00040	0.00127
	(0.00100)	(0.00092)	(0.00061)	(0.00072)
N	6,759	6,759	6,778	6,778
Dissimilarity	0.00168*	0.00080	0.00020	0.00093**
	(0.00073)	(0.00045)	(0.00025)	(0.00035)
N	6,759	6,759	6,778	6,778
Panel D: Within school sorting				
Partial R^2	0.00201*	0.00130*	0.00035*	0.00096*
	(0.00088)	(0.00056)	(0.00016)	(0.00039)
N	6,759	6,759	6,778	6,778

Notes: Unit of observation is municipality. The regressions control for cohort and municipal-level fixed effects. GPA, residual GPA, household income, and predicted GPA refer to the variables by which segregation is measured. Standard errors are clustered at municipal-level and shown in parentheses. *N* refers to number of observations.
* p<0.05, ** p<0.01, *** p<0.001.

Municipal-level Controls I continue by adding three time-varying municipal-level controls to the reduced from model (Equation 4.6). The controls are employment and unemployment rate, and average earnings of the municipality. These are calculated using the entire working age population in Finland from data set called FLEED. Unemployment rate of a municipality reflects the share of unemployed individuals over all unemployed and employed individuals in that municipality. Employment rate of a municipality is the share of employed over all the residents (mainly people aged 15-65) of the municipality. For average earnings, I first calculate the earnings of everyone by summing over work-related earnings, entrepreneurial and capital income. I then take the average of these earnings within a municipality for each year. For everyone with a missing earnings information, I impute a zero.

The motivation for this sensitivity analysis is the long observation period and that the economic situation of the municipality might impact some of my outcome variables. Panel A of Table E2 shows the reduced form estimates for segregation of schools only. The point estimates barely change after the inclusion of these time-varying municipal-level controls.

Without Municipal-mergers Next, I present the results without municipalities that have undergone a municipal-merger with another municipality during my sample period. Municipal-mergers can bias my results in two ways. The merged municipality will have an overestimated treatment intensity (number of schools). The merger will also affect measurement of segregation. To address these concerns, I show the results from my reduced form model (Equation 4.6) without municipalities that have undergone a merger. This means that I drop altogether 36 municipalities.

Panel B of Table E2 shows the reduced form estimates for segregation of schools only. The point estimates, and conclusions drawn from them, similar to the results presented in Section 4.6.1.

Standard Difference-in-differences Setting Lastly, I present results using a standard difference-in-differences specification. This means that instead of my treatment intensity variable in 4.6, I have a dummy variable that gets a value 1 if the municipality has more than 1 schools (prior to the reform) and 0 otherwise. The results for segregation of schools are in Panel C of Table E2. Although, not directly comparable to the results with continuous treatment intensity, these are quantitatively similar to the results from the main specification with continuous treatment intensity.

Table E2. Robustness checks for the effects of school choice on segregation of schools.

Segregation by:	GPA (1)	Residual GPA (2)	Household Income (3)	Predicted GPA (4)
Panel A: Municipal-level controls				
R^2	0.00082***	0.00033*	-0.00012	0.00047***
	(0.00019)	(0.00013)	(0.00007)	(0.00012)
N	6,778	6,778	6,778	6,778
Overexposure	0.00068***	0.00025*	0.00026***	0.00041***
	(0.00018)	(0.00012)	(0.00007)	(0.00008)
N	6,774	6,774	6,778	6,778
Dissimilarity	0.00156***	0.00076*	0.00027	0.00073***
	(0.00046)	(0.00030)	(0.00021)	(0.00022)
N	6,778	6,778	6,778	6,778
Panel B: Without merged municipalities				
R^2	0.00078***	0.00027**	-0.00012	0.00047***
	(0.00017)	(0.00010)	(0.00007)	(0.00013)
N	6,166	6,166	6,166	6,166
Overexposure	0.00065***	0.00020*	0.00028***	0.00044***
	(0.00017)	(0.00009)	(0.00008)	(0.00010)
N	6,162	6,162	6,166	6,166
Dissimilarity	0.00146***	0.00068*	0.00028	0.00074***
	(0.00041)	(0.00026)	(0.00023)	(0.00022)
N	6,162	6,162	6,166	6,166

Table continued on the following page.

Table E2. *(continued)* Robustness checks for the effects of school choice on segregation of schools.

Segregation by:	GPA	Residual GPA	Household Income	Predicted GPA
	(1)	(2)	(3)	(4)
Panel C: 0/1 treatment dummy				
R^2	0.00170***	0.00142***	0.00002	0.00088*
	(0.00039)	(0.00033)	(0.00033)	(0.00035)
N	6,778	6,778	6,778	6,778
Overexposure	0.00129***	0.00087**	-0.00002	0.00031
	(0.00035)	(0.00039)	(0.00023)	(0.00033)
N	6,774	6,774	6,778	6,778
Dissimilarity	0.00357***	0.00298***	0.00018	0.00137*
	(0.00081)	(0.00076)	(0.00070)	(0.00077)
N	6,774	6,774	6,778	6,778

Notes: Unit of observation is municipality. All indices have been randomness-corrected. The regressions control for year and municipal-level fixed effects. GPA, residual GPA, household income, and predicted GPA refer to the variables by which segregation is measured. Standard errors are clustered at municipal-level and shown in parentheses. *N* refers to number of observations.
* p<0.05, ** p<0.01, *** p<0.001.

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