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Intergenerational Spillovers of Integration Policies: Evidence from Finland's Integration Plans

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Intergenerational Spillovers of Integration Policies: Evidence from Finland's Integration Plans*

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Abstract

This paper shows that an integration policy aimed at unemployed adult immigrants generated positive spillovers for their children. Our research design builds on a discontinuity in the phase-in-rule of Finland's 1999 reform that introduced integration plans—a new approach for allocating unemployed immigrants to active labor market policies. We find that parents' integration plans substantially improved their children's grades and educational attainment and reduced their time out of employment, education, or training. Our examination of potential mechanisms suggests that integration plans increased parents' earnings, employment and exposure to native colleagues and pushed their children to better schools.

Keywords: Immigrants, integration policy, intergenerational effects

JEL codes: J61, J68, J13, H53

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1 Introduction

Immigrants' children often struggle at school. In comparison to the offspring of natives, they tend to have lower grades, perform worse in aptitude tests and complete less education.[1] These differences may arise through many mechanisms. For example, the offspring of immigrants might face discrimination at school or value education less than the children of natives. However, the educational gap could also reflect the fact that immigrants' children often grow up in disadvantaged neighborhoods and low-income families that may be relatively disconnected from the native population. Hence, policies designed to improve the integration of adult immigrants could have important spillover effects on their children.

This paper shows that helping adult immigrants in the labor market can substantially improve their children's outcomes. We reach this conclusion by studying a reform that refined how immigrants were allocated to active labor market policies (ALMP) in Finland. Our empirical strategy builds on the phase-in-rule of integration plans—individualized sequences of ALMP that a caseworker believes to be the most appropriate for each immigrant. The plans increased the amount of language training provided to immigrants and substantially improved their earnings (Sarvimäki and Hämäläinen, 2016). Here, we extend the analysis to their children.

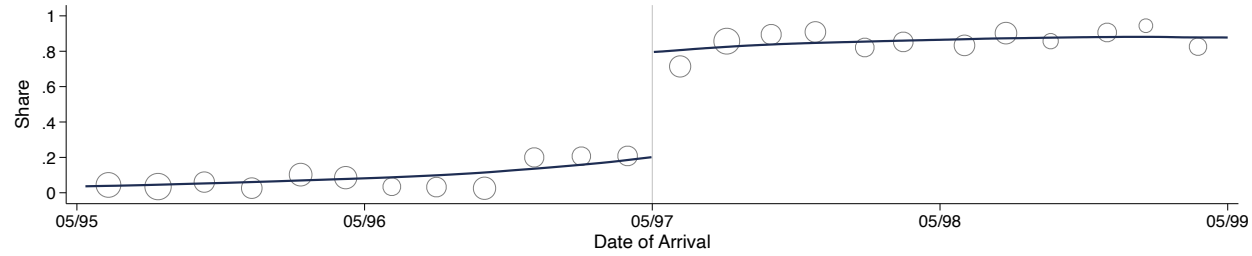
Our research design is based on the phase-in rules of a reform, which made integration plans mandatory only for immigrants who had arrived in the country after May 1st, 1997 and were unemployed at the start of the reform in May 1999. Panel (a) of Figure 1 illustrates the importance of this rule by plotting the share of parents getting an integration plan (vertical axis) on their date of arrival (horizontal axis). It shows that participation increased discontinuously by roughly 60 percentage points at the May 1997 arrival cohort. We use this phase-in rule as a fuzzy regression discontinuity design, where we estimate the impacts of the integration plans by comparing families that arrived in Finland just before and just after the threshold date. The plausibility of our approach is supported by the fact that the threshold date was announced almost two years after the immigrants had made their entry decisions and hence it was impossible to manipulate one's treatment status.

The rest of Figure 1 shows that children whose parents arrived in May 1997 tended to pursue degrees associated with higher earnings (panel b), have better grades at the end of mandatory education (panel c) and spend less time outside of employment, education or training (panel d) than children whose parents arrived slightly earlier. Together with the discontinuity in parents' likelihood of getting an integration plan, these discontinuities yield local average treatment estimates (LATE) for the intergenerational effects of the integration plans. The estimates suggest that

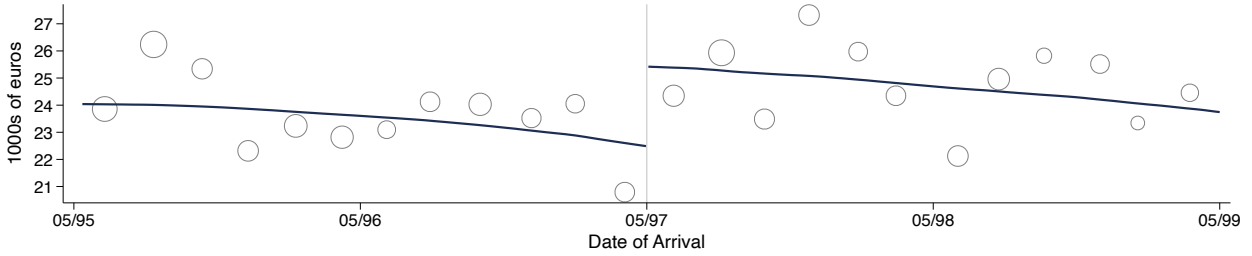
[1] See Heath et al. (2008) for a review and Algan et al. (2010), Belzil and Poinas (2010), Dustmann and Theodoropoulos (2010), Dustmann et al. (2012), Bratsberg et al. (2012) and Ansala et al. (2020) for more recent contributions.

Figure 1: Parents' integration plans and children's outcomes by parents' time of arrival

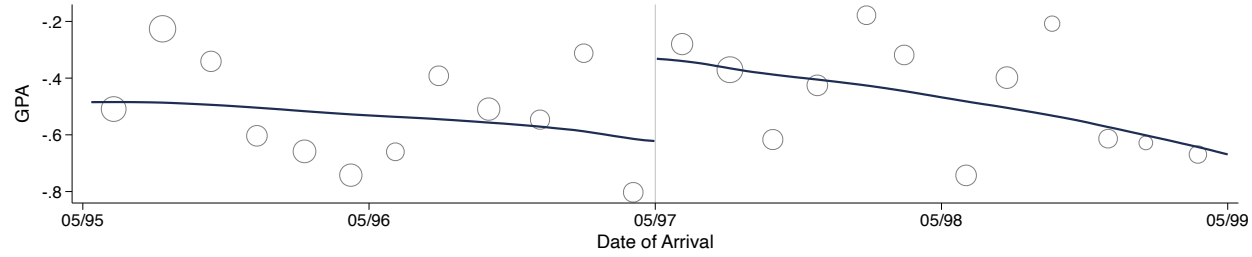
(a) Parents' integration plans



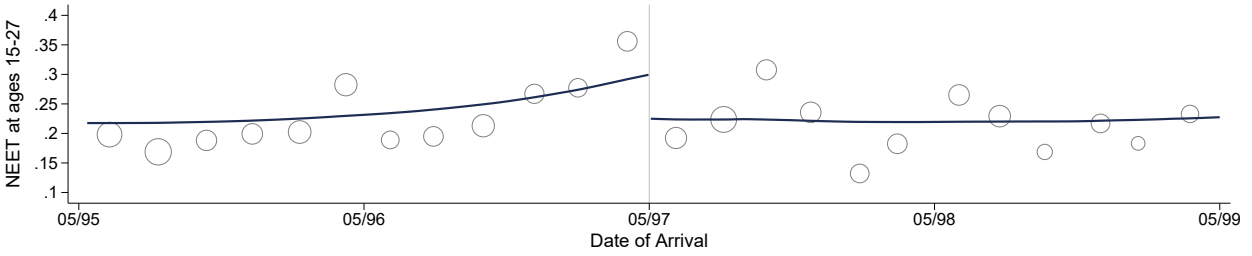
(b) Children's educational attainment (degree's average earnings) at age 27



(c) Children's standardized grade point average at 9th grade



(d) Not in employment, education or training at ages 15–27



Notes. This figure shows date of arrival (horizontal axis) and the share of parents receiving an integration plan (panel a), average earnings associated with the child's highest degree or enrollment at age 27 measured using earnings of former graduates from the degree (panel b), child's GPA at the end of mandatory education (panel c) and time spent outside of employment, education or training at ages 15–27 (panel d). The lines represent local linear estimates using the edge kernel and the optimal bandwidth selection algorithm of Imbens and Kalyanaraman (2012). The dots correspond to sample means in two month bins.

parents' integration plans increased children's degrees' predicted annual earnings (approximated using earlier graduates' earnings) by almost 5,000 euros, or 24 percent, corresponding to a shift from the 30th percentile to the median in the degree's predicted earnings distribution. Furthermore, parents' integration plans pushed their children from the 28th percentile to the 38th percentile of the 9th grade GPA distribution and from the 87th percentile to the 82nd percentile of the idleness (not in employment, education or training) distribution. All estimates are statistically significant and survive a battery of robustness checks and falsification exercises.

The finding that training designed to improve parents' labor market outcomes had a substantial effect on their children's education is perhaps surprising in the Finnish context, where virtually all education is free of charge. Thus, improved financial resources of the affected families are unlikely to explain our main results fully. Our analysis of possible underlying mechanisms suggests that in addition to increasing parents' earnings and employment, the integration plans increased parents' exposure to native colleagues. We also find an impact on children's middle school peer quality as measured by later degrees of these peers. However, while the point estimates are intriguing, we do not have sufficient statistical power to fully distinguish between alternative mechanisms. Nevertheless, our results are consistent with the hypothesis that the effects arise, at least partially, through affecting parents' colleagues at work and children's peers at school.

This paper adds to two branches of literature. First, we contribute to the body of work evaluating the impacts of integration policies. Earlier work has found that integration programs implemented as part of ALMP can improve adult immigrants' later income and employment (Åslund and Johansson, 2011; Joona and Nekby, 2012; Sarvimäki and Hämäläinen, 2016; Battisti et al., 2019; Lochmann et al., 2019; Dahlberg et al., 2020; Arendt et al., 2021; Heller and Slungaard Mumma, 2020; Foged and van der Werf, 2022). The effects of these integration programs tend to be substantially larger than what is typically found in evaluations of ALMP for natives (Card et al., 2018; Hangartner et al., 2021). Another branch of research has shown that interventions implemented in schools can have large positive effects on immigrants' children through career counseling (Goux et al., 2017; Carlana et al., 2021), decreased implicit bias of teachers (Alesina et al., 2019), increased parental involvement (Avvisati et al., 2014), and improved perspective-taking ability among students (Alan et al., 2021).

Our results show that investments in unemployed adult immigrants can have substantial spillover effects on their children. This finding is important both for assessing the costs and benefits of integration programs for adults as well as for designing policies to improve the educational attainment of immigrants' children. Earlier work examining intergenerational effects of integration programs appears to be limited to Arendt et al. (2021), who evaluate a Danish reform changing the approach for integrating refugees. Their focus is on the first generation, but they also find a large intergenerational effect on the likelihood to graduate from lower secondary school for sons but not for

daughters. Here, we focus solely on intergenerational effects and dig deeper in terms of examining both grades and educational choices, idleness and a wide range of potential mechanisms. While our general take-aways are similar to Arendt et al. (2021)—and thus increase the credibility of both papers—our findings differ in some important details such as in finding larger effects for daughters than for sons.[2] Furthermore, the analysis of parents' and children's peer quality is unique to this paper.

We also add to the broader literature examining the spillover effects of parents' income and employment on children's education. Earlier work has shown that income transfers to parents increase their children's educational attainment (Akee et al., 2010; Aizer et al., 2016; Bastian and Michelmore, 2018; Manoli and Turner, 2018; Bulman et al., 2021) and test scores (Milligan and Stabile, 2011; Dahl and Lochner, 2012; Black et al., 2014), and that parental job loss has a negative effect on children's college attendance (Coelli, 2011; Hilger, 2016) and school performance (Rege et al., 2011). Our results complement this work by providing evidence for a very different population, policy and context. Specifically, we examine newly arrived immigrants in a country that has generous social security and no tuition fees. Thus, challenges related to navigating the educational system and forming expectations about returns to different kinds of education are likely to play a larger role in our context than in this earlier literature.

The rest of this paper is organized as follows. The following two sections discuss the reform, our research design, and data. Section 4 reports our main results and robustness checks and section 4.4 discusses the likely mechanisms behind them. We end with some concluding remarks.

2 Treatment and research design

2.1 The treatment

We examine the effects of an intervention that was introduced in 1999 as part of the Act on the Integration of Immigrants and Reception of Asylum Seekers. At the time, only 2.1 percent of Finland's population were of immigrant background, but the share had increased fast from the 0.8 percent a decade earlier and there was growing concern about the poor labor market performance of recent arrivals. The objective of the new legislation was to facilitate the integration of immigrants and increase equality. The most notable part of the reform was the introduction of "integration plans" for non-employed immigrants during their first three years in Finland. The plans were prepared jointly by a caseworker at the employment office, the immigrant and an interpreter. They consisted of an individualized sequence of existing ALPM measures such as training, subsidized

[2] A possible reason for this difference is that the main intergenerational results of Arendt et al. (2021) focus on outcomes relevant only for the most disadvantaged (predominantly male) children whereas our outcomes capture variation throughout the population.

work, and rehabilitation. The immigrants were expected the follow the plan and non-compliance was sanctioned by benefit withdrawal.

In an earlier study, Sarvimäki and Hämäläinen (2016) examine the impact of the integration plans on the labor market outcomes of the participants. Using a similar RDD identification strategy as we use in this paper, they find that the plans increased compliers' cumulative earnings over a 10-year period by 47% and subsequently decreased cumulative social benefits by 13%. Furthermore, they show that the reform did not affect the amount of training immigrants received but changed the content of training to better suit immigrants' needs. In particular, the share of language training increased substantially at the expense of "general preparatory training".

2.2 Identification and estimation

Our identification strategy is based on the discontinuity in the phase-in of the integration plans, which made participation mandatory only for unemployed immigrants who entered the population register on or after May 1, 1997. Those who had registered before May 1, 1997, had the right, but no obligation, to get an integration plan. We interpret these rules as a fuzzy regression discontinuity design where the running variable is the date of entering the population register.

The validity of our design requires that the date of arrival affects the likelihood of getting an integration plan (first-stage) and that the monotonicity and continuity assumptions hold (Imbens and Angrist, 1994; Hahn et al., 2001). The first-stage is very clear, i.e., as we discussed in the introduction, the likelihood of a parent receiving an integration plan jumps by 60 percentage points at the May 1, 1997 threshold (Figure 1, panel a). The continuity assumption means that immigrants entering the population register just before and after the cutoff have similar potential outcomes, whereas monotonicity requires that no one became less likely to receive an integration plan if she entered the population register after the cutoff rather than before. These assumptions appear reasonable because the legislation regarding the reform was passed two years after the cutoff. Thus, no one could manipulate their day of entry to avoid being affected by the reform. In addition, there were no simultaneous other policy changes that might affect the individuals on either side of the cutoff differently.

We use local linear estimators to estimate the effects of the integration plans for immigrants arriving shortly before and after the May 1, 1997 cutoff. We start with the reduced form regression:

$$y_i = \alpha + \beta \mathbf{1}\left[d_i \geq d_0\right] + \delta_0(d_i - d_0) + \delta_1 \mathbf{1}\left[d_i \geq d_0\right](d_i - d_0) + X_i\theta + \varepsilon_i, \tag{1}$$

where y_i is an outcome for individual i, d_i is the day her parents arrived to Finland ($d_0 = 0$ is May 1st, 1997), $\mathbf{1}\left[d_i \geq d_0\right]$ is an indicator taking value one if the parents arrivided to Finland after May 1st, 1997, X_i is a vector of observed characteristics measured at arrival, and ε_i summarizes

unobserved characteristics. The parameter of interest is β, which measures the discontinuity in the outcome at the May 1st, 1997 threshold. We then move to local average treatment effect (LATE) estimates by scaling β with the discontinuity in the likelihood of receiving an integration plan.[3] Our baseline estimates use triangular kernels and Imbens and Kalyanaraman (2012) optimal bandwidths. As robustness checks, we report results for a range of bandwidths and alternative confidence intervals using robust bias correction (Calonico et al., 2014) and adjusted critical values (Armstrong and Kolesár, 2020). In addition, we report estimates with and without conditioning on background characteristics. To faciliate interpretation, we also report estimates of compliers' potential outcomes (Imbens and Rubin, 1997; Frandsen et al., 2012).

3 Data

3.1 Data sources and restrictions

Statistics Finland constructed our data by combing information from various administrative registers. The underlying data cover the total population living in Finland in 1987–2015. We focus on children whose both (or the only known) parents are immigrants who arrived in Finland between January 1990 and April 1999.[4] In addition, we restrict the sample to children born between 1980–1988 who were under 15 years old at arrival. The cohort restriction allows us to follow everyone from the time of leaving comprehensive education at age 16 until they are 27 years old. The extended follow-up period is important in the Finnish context, where it is not uncommon to start tertiary education in one's mid-twenties. The age-at-arrival restriction ensures that everyone in our sample has at least some exposure to the Finnish comprehensive school. To increase the comparability of the families across arrival time, we further restrict the analysis to families where at least one child reaches age 27 within 19 years of arrival. Our final estimation sample consists of 2,953 children, of whom 1,141 arrived within two years of the May 1997 threshold.

[3] In practice, we estimate LATEs with standard weighted 2SLS, where the first-stage is $D_i = \alpha + \gamma \mathbf{1}[d_i \geq d_0] + \delta_0^{FS}(d_i - d_0) + \delta_1^{FS}\mathbf{1}[d_i \geq d_0](d_i - d_0) + X_i\theta^{FS} + \varepsilon_i$, the second-stage is $y_i = \alpha + \tau D_i + \delta_0^{SS}(d_i - d_0) + \delta_1^{SS}\mathbf{1}[d_i \geq d_0](d_i - d_0) + X_i\theta^{SS} + \varepsilon_i$, and D_i is an indicator taking value one if person i's parent received an integration plan and zero otherwise.

[4] We define parents as adults who live in the same dwelling as the child and are part of the same family. This definition does not distinguish between biological and adopted children and parents may change over the years. Our main analysis is based on defining parents as those adults who live with the child the longest while the child is under 15 years old and using father's arrival date as the primary running variable (mother's arrival date when the father is not observed). As we discuss Section 4.2, the results are robust to alternative definitions of parents and the running variable.

3.2 Outcomes

We are primarily interested in immigrants' children's amount and quality of education when they enter the labor market. In our main analysis, we measure education with an index, where we assign each six-digit degree with average earnings at age 35 of earlier graduates with the same degree. These average earnings are based on data covering the years 1996–2005 that include the entire population apart from the individuals in our estimation sample. We assign these average earnings to each individual in our data using the information on their highest completed degree or the program they are enrolled in at age 27.

Our secondary outcome is the grade point average (GPA) in 9th grade. A limitation of this measure is that grades are not based on standardized tests, and thus 9th grade GPA may not be a fully comparable measure of academic aptitude across schools. Nevertheless, it is the primary criterion in the competition for access to secondary education.[5] Thus, we interpret 9th grade GPA as an incomplete but informative measure of both academic aptitude and educational opportunities at the end of mandatory education.

Our third primary outcome is a measure of idleness defined as the person not being in employment, education or training. Specifically, the outcome is defined as the share of years between ages 15 to 27 when the labor market status of the person is defined as unemployed, on disability pension or otherwise outside the labor force (apart from those in military or non-military service) at the end of the year.

3.3 Background characteristics

Table 1 reports the average background characteristics of the children and their parents for those arriving within two years of the May 1997 threshold. On average, children were 11 and their parents 38 years old at arrival. Roughly a tenth arrived with a single parent, almost always a mother. Most of the immigrant families settled in urban areas. Roughly a fifth are refugees, and around a quarter are Ingrian Finns.[6] Almost half were born in the former Soviet Union.

The last two columns of Table 1 report local linear estimates for jumps of the background characteristics at the May 1997 threshold. Most of the estimates are small and statistically insignificant.

[5]The Finnish education system consists of nine years of mandatory comprehesive school followed by secondary education in academic track high school or vocational institution. Selection into the academic track is almost exclusively based on GPA (the only exceptions being high schools specialized in arts and sports). GPA is important also in selection into vocational training but vocational programs may also use aptitude tests and take into account experience, minority gender and applicants' preference ranking; see Silliman and Virtanen (2021) for details.

[6]Ingrian Finns are a minority group of Finnish descent in Russia who were granted return migrant status in 1990. However, due to "Russification" measures in the USSR from the 1930s onwards, their standing in Finland is likely to be more comparable to that of immigrants than to that of natives.

Table 1: Background characteristics by time of arrival

	Time of arrival				Jump in 5/97	
	5/95-4/96 (1)	5/96-4/97 (2)	5/97-4/98 (3)	5/98-4/99 (4)	RD estimate (5)	Standard error (6)
A: Children						
Age	10.99	11.20	11.85	12.26	0.20	(0.21)
Female	0.46	0.50	0.49	0.50	-0.03	(0.06)
B: Parents						
Age	37.59	37.62	38.83	39.12	0.83	(0.62)
Female	0.50	0.50	0.50	0.50	0.01	(0.01)
Single parent	0.09	0.13	0.1	0.06	0.02	(0.03)
Children under 18	0.87	0.86	0.87	0.87	-0.05	(0.04)
Regional unemployment rate	14.31	13.02	11.41	11.13	0.07	(0.38)
Urban municipality	0.81	0.84	0.86	0.82	0.06	(0.04)
Legal status:						
Refugee	0.23	0.19	0.15	0.17	-0.09	(0.05)
Ingrian Finn	0.28	0.29	0.32	0.16	0.14	(0.05)
Family member	0.03	0.06	0.09	0.12	-0.05	(0.02)
Other/Unknown	0.46	0.47	0.44	0.55	0.00	(0.05)
Region of birth						
Asia	0.16	0.21	0.21	0.21	-0.14	(0.05)
Africa	0.05	0.04	0.03	0.07	0.04	(0.03)
New EU members	0.09	0.13	0.06	0.08	0.01	(0.02)
form. Soviet Union	0.50	0.52	0.65	0.57	0.20	(0.06)
form. Yugoslavia	0.17	0.09	0.02	0.07	-0.03	(0.02)
Other	0.03	0.01	0.01	0.01	0.01	(0.02)
Observations	362	255	298	226	0.41	(0.27)

Notes. The table reports sample means of background characteristics by time of arrival. Parents' characteristics denote the mean of mother's and father's characteristics. All characteristics are measured at the year of arrival. The share of female parents includes parents arriving in separate years.

However, the estimates suggest that the share of refugees and Ingrian Finns increased while the share born in Asia decreased. Given that the May 1997 threshold was published on May 8, 1998 (when the government introduced the bill to parliament), these estimates are likely spurious. In the next section, we show that conditioning on background characteristics in our main analysis has little impact on the estimates. Thus, we argue that changes in background characteristics at the threshold are unlikely to drive our results.

4 Results

4.1 Main results

Table 2 presents our main result. The first row, columns (1), (3) and (5), report reduced form estimates corresponding to the discontinuities we already discussed in the introduction (panels b, c and d of Figure 1). The baseline point estimates suggest that in comparison to those arriving at the end of April 1997, children of immigrants arriving on May 1st, 1997, continued to degrees associated with €2,935 higher earnings, had 0.29 standard deviations higher 9th grade GPA and spent 7 percentage points less time outside of employment, education or training at ages 15–27. All estimates are statistically significant with p-values ranging between 0.005 and 0.012. Controlling for background characteristics slightly reduces the points estimates, but conditional and unconditional estimates are statistically indistinguishable.

The second row reports estimates for the discontinuity in the likelihood of receiving an integration plan, i.e., the first-stage. These estimates correspond to the top panel of Figure 1 and show roughly 60 percentage points higher participation rates for those arriving just after the May 1997 threshold than for those arriving just before. Together, the reduced form and first-stage estimates give rise to local average treatment effect (LATE) estimates reported in the third row. The point estimates suggest that the integration plans pushed the compliers towards degrees associated with €4,078–€4,964 higher earnings, increased their GPA at the end of mandatory education by 0.39–0.51 standard deviations, and reduced idleness by 0.08–0.13 years. Again, all estimates are statistically significant (p-values ranging between 0.007 and 0.017).

To put these estimates into context, the bottom panel of Table 2 reports three types of benchmarks: average outcomes of natives, average outcomes of never-takers and average outcomes of the compliers' in the absence of the treatment.[7] This analysis reveals that the compliers largely

[7] "Compliers" refers to families that got an integration plan because they arrived after the threshold date (and would not have gotten it otherwise) and "never-takers" to families that did not get an integration plan despite arriving after the threshold data. We estimate the average outcomes of the compliers using the modification by Frandsen et al. (2012) of the Imbens and Rubin (1997) approach.

Table 2: Impact of parent's integration plan on GPA and educational attainment

	Degree's average earnings		Standardized GPA		Not in employment, education or training	
	(1)	(2)	(3)	(4)	(5)	(6)
A: Estimates						
Reduced form	2,935	2,514	0.29	0.23	-0.07	-0.05
	(1,041)	(1,037)	(0.12)	(0.11)	(0.03)	(0.03)
First-stage	0.59	0.62	0.57	0.60	0.57	0.60
	(0.05)	(0.04)	(0.05)	(0.04)	(0.05)	(0.04)
Local average treatment effect (LATE)	4,964	4,078	0.51	0.39	-0.13	-0.08
	(1,828)	(1,695)	(0.22)	(0.19)	(0.05)	(0.04)
B: Benchmarks						
Compliers' expectation in the absence of the treatment	20,559	21,301	-0.83	-0.74	0.36	0.33
	(1,210)	(1,166)	(0.14)	(0.13)	(0.04)	(0.03)
Never-takers' average	26,231		-0.28		0.20	
Native's average	27,433		0.01		0.12	
Additional covariates	No	Yes	No	Yes	No	Yes
Bandwidth (months)	31.8		26.8		27.6	
Observations	1,345		1,201		1,237	

Notes. This table reports local linear estimates for the jump at the May 1, 1997 cutoff of father's arrival time for educational attainment as measured by average earnings of earlier graduates with the same degree (columns 1–2), standardized 9th grade GPA (columns 3–4) and the share of years the person is not in employment, education or training at ages 15–27. Reduced form refers to the jump in the outcome at the May 1997 threshold and first-stage to the jump in the likelihood for either parent getting an integration plan. Additional covariates are child's sex and age at arrival and parents' age, marital status, number of children under 18, regional unemployment rate, type of residence municipality (urban, semi-ruban, rural), legal status (refugee, Ingrian Finn, family member, other/unknown) and region of birth. All background characteristics are measured at the year of arrival. The bandwidths are chosen using the optimal bandwidth selection algorithm of Imbens and Kalyanaraman (2012).

consisted of the most disadvantaged children. Compared to natives, complier families' offspring had final degrees associated with 22 percent lower average earnings, 0.74–0.83 standard deviations lower 9th grade GPA, and were three times more likely to be idle. These gaps mean that an average complier was at the 31st percentile of the distribution of degrees' average earnings, the 26th percentile of the GPA distribution, and the 87th percentile of the idleness distribution. In comparison, the averages of never-takers correspond to the 55th percentile of the degree's average earnings distribution, 40th percentile of the GPA distribution and 77th percentile of the idleness distribution. The LATE estimates suggest that parents' integration plans entirely closed the gap between the compliers and never-takers and considerably narrowed the gap between the compliers and natives' children. More precisely, the point estimates suggest that parents' integration plans pushed the av-

erage complier to the median in the degree's predicted earnings distribution, the 38th percentile of the 9th grade GPA distribution, and the 82nd percentile in the distribution of our idleness measure.

4.2 Robustness

We next examine the robustness of our main estimates. In the previous subsection, we found that the results are not sensitive to controlling for observable characteristics at arrival. Figure 2 examines their sensitivity to bandwidth and estimation method. It shows reduced form estimates for bandwidths varying between 9 and 48 months. For each estimate, we report conventional 95% confidence intervals and Armstrong and Kolesár (2020) "honest" 95% confidence intervals, which take into account the possible bias of the local linear estimates. In addition, we report Calonico et al. (2014) bias-corrected estimates and their 95% confidence intervals for the same bandwidths.

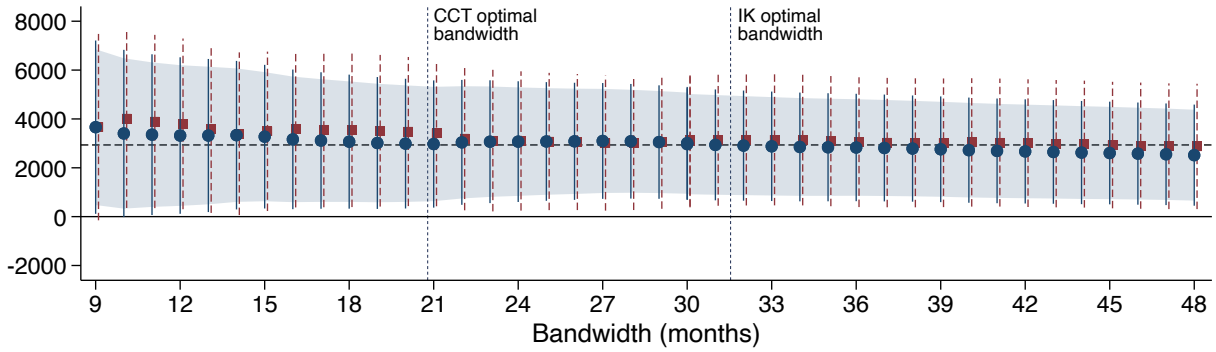
The top panel of Figure 2 shows that reduced form estimates for earnings associated with a person's highest degree vary between €2,500 and €4,000. Estimates using smaller bandwidths tend to be larger than those using data further away from the threshold. All estimates are statistically significant at a 5% level except the Calonico et al. (2014) estimate using 9 months bandwidth, which has a p-value of 0.060. The confidence intervals of Calonico et al. (2014) are substantially wider than Armstrong and Kolesár (2020) confidence intervals. This is in line with Armstrong and Kolesár (2020), who show that their method based on adjusting for critical values produces more efficient confidence intervals than approaches that aim to estimate and correct the potential bias of the conventional local linear estimators (such as Calonico et al., 2014).

The middle and bottom panels of Figure 2 present a similar analysis for 9th grade GPA and idleness. The point estimates vary between 0.26 and 0.83 standard deviations for GPA and between -0.03 and -0.21 percentage points for idleness across these specifications. Again, the estimates tend to be larger when using smaller bandwidths. Most of the estimates are statistically significant at conventional levels, but the confidence intervals include zeros for intermediate bandwidths for GPA and large bandwidths for idleness.

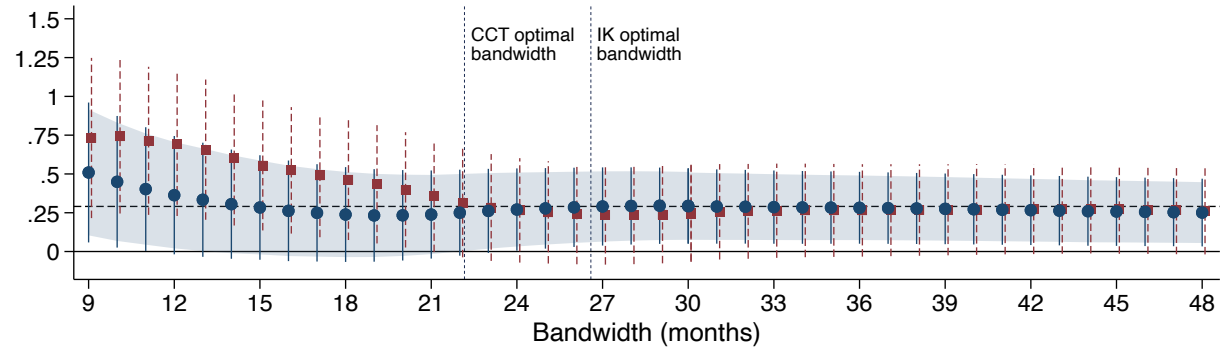
In the Appendix, we further show that jumps of similar size as our main estimates rarely occur in made-up "placebo" thresholds (Appendix Figure A4), and the results are robust to "donut hole" specifications, where we leave out observations close to the threshold (Appendix Figure A5). The estimates are also robust to alternative definitions of our running variable, specifically (i) using the date of arrival of the parent who first arrived in Finland (Appendix Figure A2 and Appendix Table A1) and (ii) defining parents as adults who lived with the child when she was 15 years old (Appendix Figure A3 and Appendix Table A2). In short, our key conclusion that parents' integration plans improved their children's educational outcomes, and reduced their idleness, appears robust to alternative estimation approaches.

Figure 2: Robustness to alternative bandwidths and estimation approaches

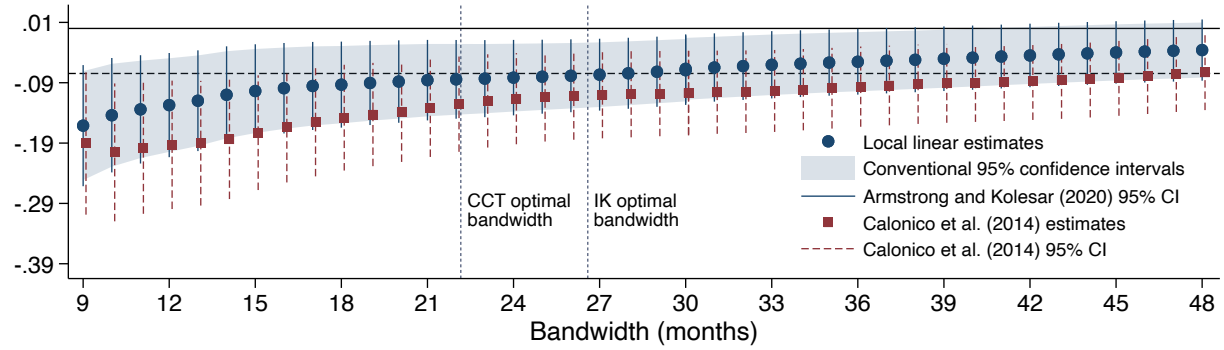
(a) Average earnings of the highest degree



(b) Grade point average at 9th grade



(c) Not in employment, education or training



Notes. The blue circles show local linear reduced form estimates using triangle kernels and alternative bandwidths (x-axis). The shaded area corresponds to the corresponding conventional 95% confidence intervals and the blue spikes to the "honest confidence intervals" of Armstrong and Kolesár (2020). The red squares and spikes are the Calonico et al. (2014) bias-corrected estimates for alternative bandwidths and the corresponding 95% confidence intervals. For reference, we also show the baseline estimates reported in Table 2 (horizontal dashed line) and the optimal bandwidths based on Calonico et al. (2019) and Imbens and Kalyanaraman (2012) (vertical dashed lines).

4.3 Effect heterogeneity

The top panel of Figure 3 summarizes the results of our treatment effect heterogeneity analysis.[8] To ease the comparison of the estimates, we have normalized all outcome variables to have a mean of zero and a standard deviation of one. The results show that parents' integration plans have a larger effect on daughters than on sons. Specifically, the point estimates suggest that parents' integration plans increased degree's average earnings by 0.64 standard deviations among daughters and by 0.25 standard deviations among sons. The estimates are statistically significant only for daughters, and the difference between sons and daughters is significant (p-value 0.006). The other estimates are similar with a point estimate of 0.65 (daughters) and 0.39 (sons) for GPA and 0.40 (daughters) and 0.77 (sons) for idleness. However, the difference is statistically significant only for idleness (p-values 0.122 for GPA and 0.032 for idleness). By contrast, all estimates are similar across parents' origin country Human Development Index.

4.4 Mechanisms

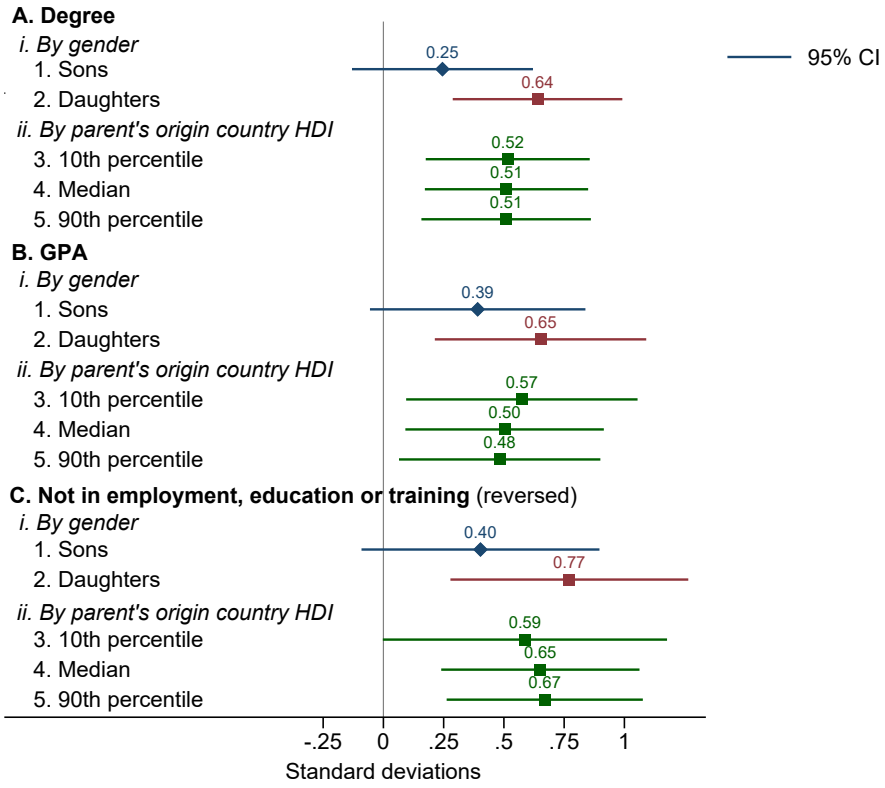
The bottom panel of Figure 3 examines some possible mechanisms that could give rise to intergenerational effects in our context. Each entry corresponds to a LATE estimate from the same specification for our main results. Again, all outcome variables are normalized. For parents, we report estimates separately for mother, father and both, i.e., summing up the outcomes of the mother and father. To increase statistical power and reduce issues related to multiple hypothesis testing, we follow Kling et al. (2007) and construct an overall index for each "family" of outcomes.

The first results suggest that integration plans improved parents' combined earnings by 0.42 standard deviations corresponding to 11,408 euros or a 58% increase in parents' average annual earnings (Appendix Table A4). The estimate is statistically significant with a p-value of 0.038. The results suggest that both parents contribute to the overall effect: the point estimates are 6,083 euros or 69% for mothers and 6,173 euros or 53% for fathers. However, the estimates are imprecise and statistically insignificant for fathers. Furthermore, unlike our main results, controlling for background characteristics substantially reduces the point estimates (Appendix Table A5). Nevertheless, the point estimates from specifications controlling for background charactersitics suggest that integration plans increased mothers' earnings by 35% and fathers' earnings by 9%, and the estimate for mothers remains borderline significant with a p-value of 0.073.

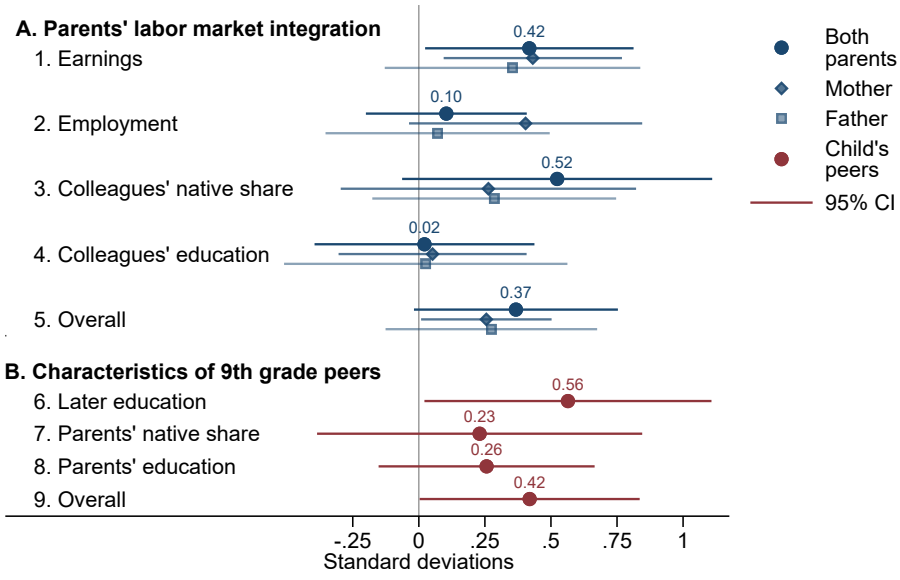
[8] We estimate effect heterogeneity by adding an interaction term with parent's integration plan but otherwise use the same specification for the main analysis. Specifically, the first-stage is now $D_i = \alpha + \gamma_0 \mathbf{1}[d_i \geq d_0] + \gamma_1 \mathbf{1}[d_i \geq d_0] \times W_i + \delta_0(d_i - d_0) + \delta_1 \mathbf{1}[d_i \geq d_0](d_i - d_0) + X_i\theta + \varepsilon_i$, and the second-stage is $y_i = \alpha + \tau_0 D_i + \tau_1 D_i \times W_i + \delta_0(d_i - d_0) + \delta_1 \mathbf{1}[d_i \geq d_0](d_i - d_0) + X_i\theta + \varepsilon_i$, where W_i is either an indicator for child's gender or parent's country of birth Human Development Index. We report the estimates in Appendix Table A3. Figure 3 summarizes the results by showing $\hat{\tau}_0$ and $\hat{\tau}_0 + \hat{\tau}_1$ when comparing the effects between sons and daughters. For HDI, we report $\hat{\tau}_0 + \hat{\tau}_1 \times Q_p$, where Q_p is either the 10th, 50th, or 90th percentile of the HDI distribution.

Figure 3: Effect heterogeneity and potential mechanisms

(a) Effect heterogeneity



(b) Mechanisms



Notes. Panel (a) reports LATE estimates for parents' integration plan on child's outcomes by child's gender and parent's origin country Human Development Index; see footnote 8. Panel (b) reports LATE estimates for parent's integration plan on parental outcomes and child's 9th grade peers. All outcomes are normalized to have zero mean and standard deviation of one. The overall indices in the bottom panel are constructed as in Kling et al. (2007) using the components listed in each subpanel.

We next examine effects on employment. For mothers, the baseline point estimate show an increase of 0.40 standard deviations or 16 percentage points (from a baseline of 41 percent).[9] However, the estimate is again imprecise and only borderline significant with a p-value of 0.073. For fathers, the point estimate for employment is 0.07 standard deviations or 3 percentage points (from a baseline of 46 percent). Similar to the case of earnings, controlling for background characteristics reduces the employment estimates (Appendix Table A5).

These findings align with Sarvimäki and Hämäläinen (2016), who find that integration plans increased earnings by 49% among all complier immigrants. Our analysis is restricted to those who arrived with children of a certain age, and the resulting smaller sample likely explains why our estimates are less precise and vary more across specifications than those reported in Sarvimäki and Hämäläinen (2016). While we acknowledge that the estimates are quite imprecise, the results suggest that integration plans improved the financial resources of immigrant families. However, the direct importance of this channel on children's education may be limited because education is free-of-charge at all levels and stundents are entitled to generous allowances and government backed loans, i.e., credit constraints are unlikely to play an important role in the Finnish context. Hence, we also explore the hypothesis that parents' integration plans may have affected their children also through other channels. For example, integration training may have directly delivered information about the Finnish education system. Alternatively, improved labor market outcomes might have affected social networks and thus led to better information or helped overcome cultural barriers affecting educational choices.

We next turn to parents' social networks. Our register-based data does not include information about friends or acquaintances, but we can identify one important social group: colleagues at work.[10] This group might be particularly important for newly arrived immigrants as workplaces provide perhaps the most potent place to establish connections with the native population. The estimates suggest that integration plans may have increased parents' exposure to natives. While the estimates are again regrettably imprecise (the p-value for the estimate for both parents is 0.080), the point estimates vary between 0.26 and 0.52 standard deviations. By contrast, the estimates for colleagues' education are close to zero and statistically insignificant.

In short, the estimates for the impact of integration plans on parents' labor market integration are imprecise, but the point estimates are consistent with large positive effects. The results for the overall index support this interpretation. The point estimate using data on both parents is 0.37

[9] Parents' employment is measured as the share of years employed between 2000 and 2015, with employment status defined at the end of each year.

[10] Specifically, we use our population-level data and establishment identifiers to characterize other people working in the same establishment at the same time as the parents. An establishment is defined as an entity that is owned by one firm and operates in one location and industry. The median establishment size is 25 persons for mothers and 19 persons for fathers.

standard deviations with a p-value of 0.062. The wide confidence intervals are primarily driven by high variation in fathers' outcomes. For mothers, the point estimate for the overall index is 0.26 standard deviations with a p-value of 0.043.

Finally, parent's integration plan could have affected their children's peers. Such effects could occur, for example, if parents' improved labor market integration allowed the families to move to better neighborhoods. Again, we do not directly observe children's social networks, but we can approximate them using register-based information on their middle schools. Specifically, we characterize 9th-grade peers using information on their later degrees (measured in the same way as our main outcome) and their parents' immigrant status and education.

The results suggest that integration plans substantially affected the kind of school immigrants' children attended. The point estimate for peers' later education is 0.56 standard deviations with a p-value of 0.042. These effects correspond to a 1,575 euros or 6% increase in the average earnings of peers' later degrees (Appendix Table A6). The point estimates for peers' parental education and native share are 0.23 and 0.26 standard deviations, respectively, but neither is statistically significant. Furthermore, expressing effect size in standard deviations may be misleading because there is relatively little variation across schools in these outcomes. The point estimates correspond to a 2 percentage point increase in parents' native share (from a baseline of 86 percent) and a 1,356 euro or 2.8 percent increase in the average earnings associated with parents' degrees (Appendix Table A6). Controlling for background characteristics at arrival has little effect on the estimates (Appendix Table A6). The estimates for the overall index of 9th-grade peers is 0.42 standard deviation with a p-value of 0.048.

To summarize, these results are consistent with a narrative where integration plans improved parents' financial resources and increased their exposure to native colleagues. Consequently, their children went to schools where their peers more often came from highly educated native families and went on to earn better degrees themselves. However, while the point estimates are intriguing, we acknowledge that we do not have sufficient statistical power to rigorously test this narrative.

5 Conclusions

This paper examines whether policies improving immigrants' labor market integration have spillover effects on their children. Our findings suggest they do. Specifically, we use a discontinuity created by the phase-in rules of a reform that introduced integration plans in Finland. We find that children whose parents arrived just after the threshold date (and thus received an integration plan) graduated with better grades, continued to post-mandatory programs associated with higher earnings and spent less time idle than the offspring of comparable immigrants who just missed the threshold. The effects were sufficiently large to push the children of particularly disadvantaged

immigrants to the level of other immigrants' children and close a substantial part of the immigrant-native gap. Daughters benefitted more than sons, while we find no treatment effect heterogeneity across parents' origin countries. Our analysis of the potential underlying mechanisms suggests that these effects may be, at least partially, due to improved parental labor market outcomes and school quality.

These findings imply two broader lessons. First, they show that interventions targeted at adult immigrants may have positive intergenerational effects that should be taken into account when assessing the cost-efficiency of integration policies. Second, the results are consistent with the hypothesis that parents' exposure to more informed colleagues at work and children's exposure to better peers at school allow children to make better educational choices. We stress that this hypothesis remains a conjecture, and we cannot provide strong direct evidence supporting it. Thus, future work more directly examining these channels would be highly valuable.

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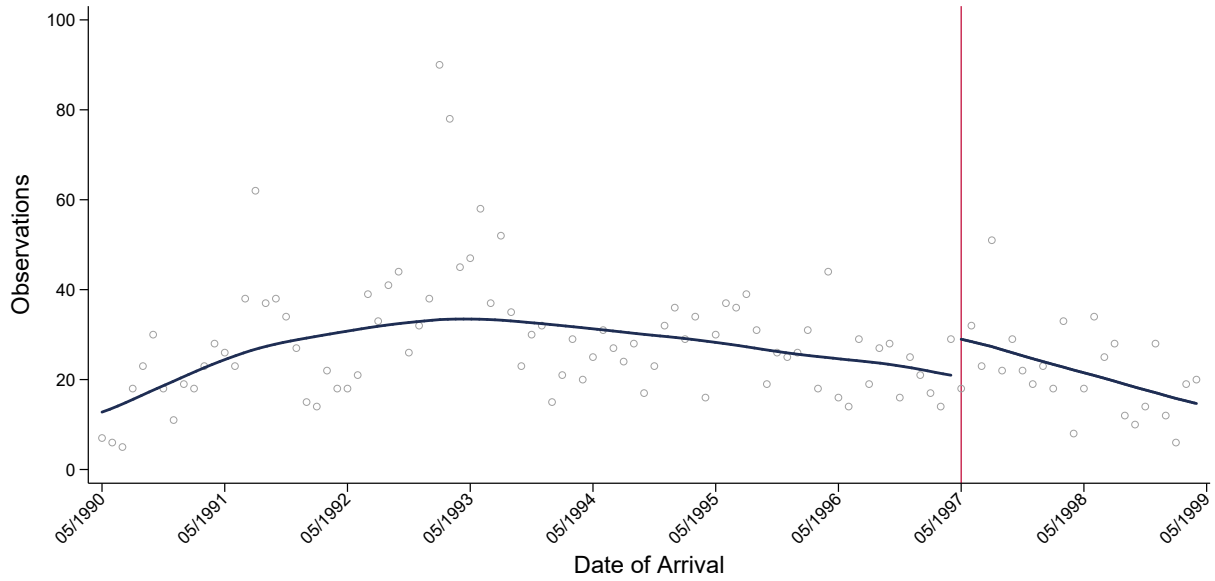
A Appendix

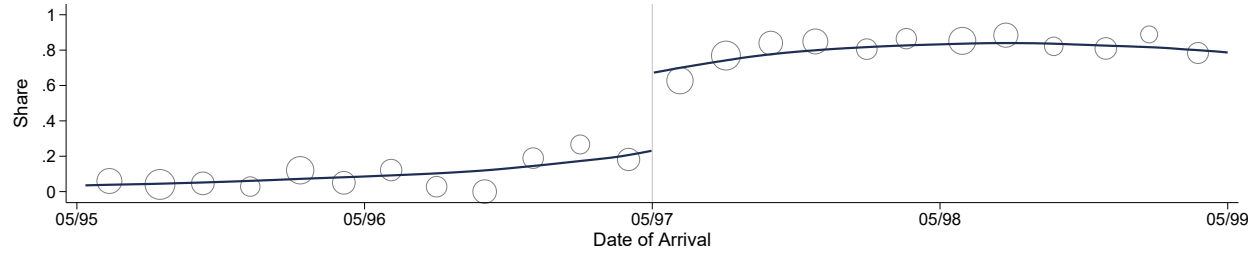
Figure A1: Observations by month of arrival



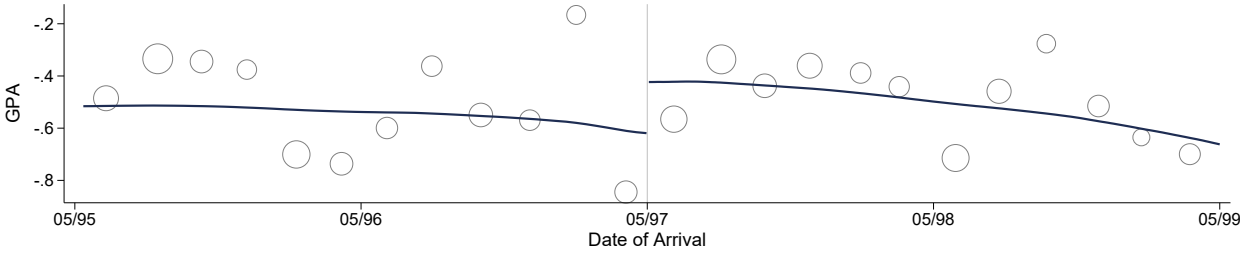
Notes. The figure shows observations by month of arrival of the father. The lines represent local linear estimates using the edge kernel and the optimal bandwidth selection algorithm of Imbens and Kalyanaraman (2012). The dots correspond to the number of observations entering the population register by month.

Figure A2: Parents' integration plans and children's outcomes by parents' time of arrival: First parent

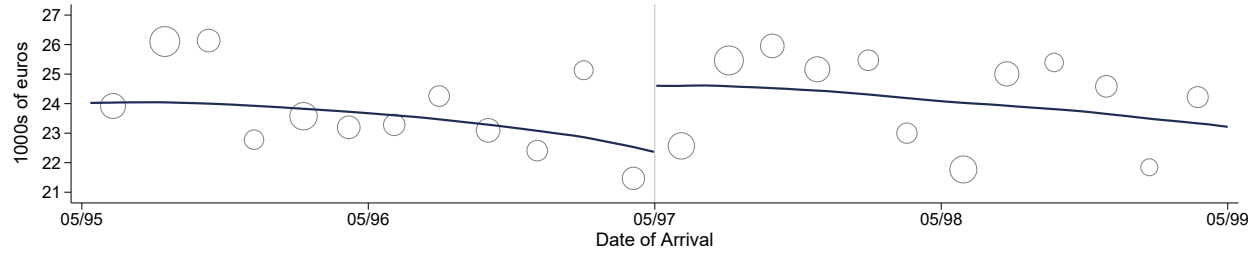
(a) Parents' integration plans



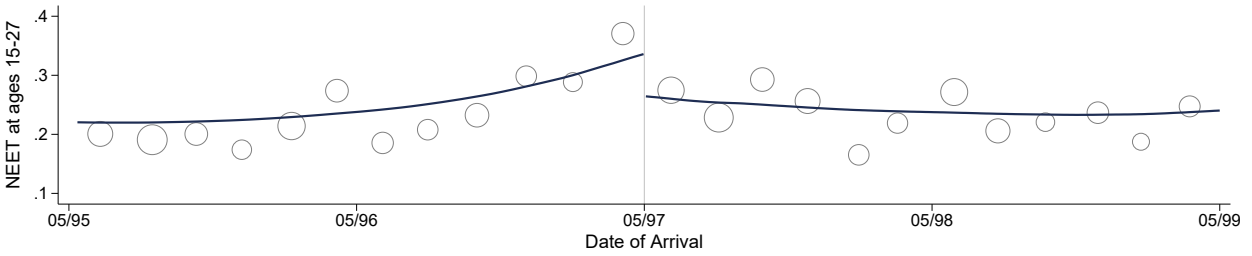
(b) Children's educational attainment (degree's average earnings) at age 27



(c) Children's standardized grade point average at 9th grade



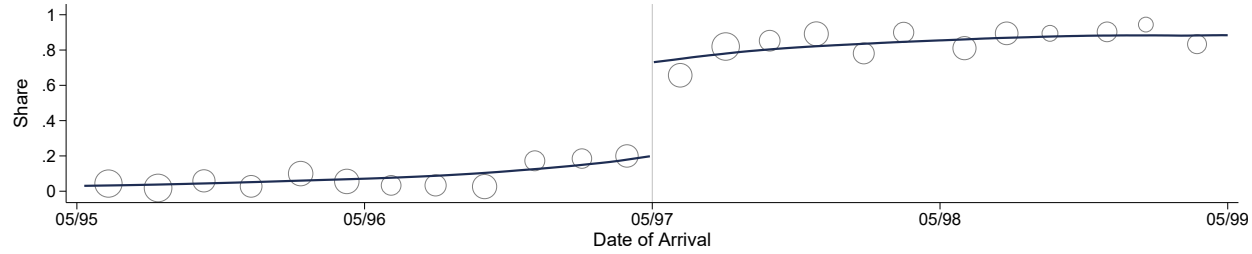
(d) Not in employment, education or training at ages 15–27



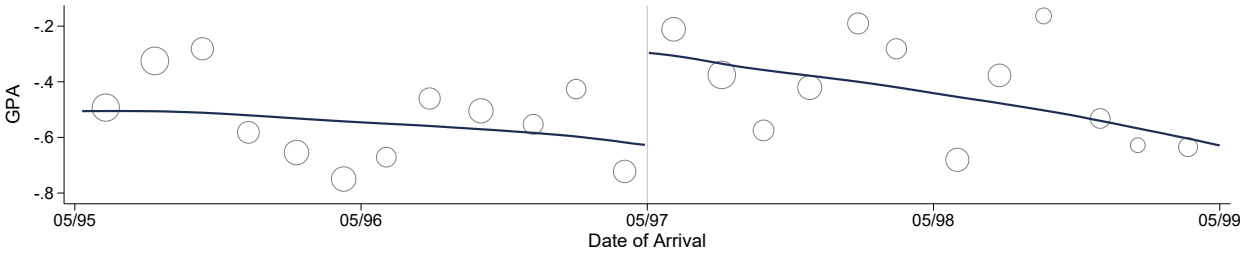
Notes. This figure is identical to Figure 1 except that we now use the date of arrival of the parent who first arrives in Finland as the running variable, while our main analysis is based on the date of arrival of the father.

Figure A3: Parents' integration plans and children's outcomes by parents' time of arrival: Parent at age 15

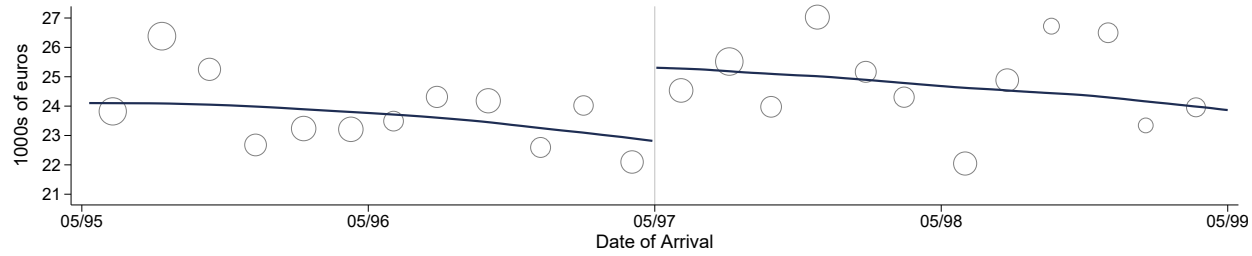
(a) Parents' integration plans



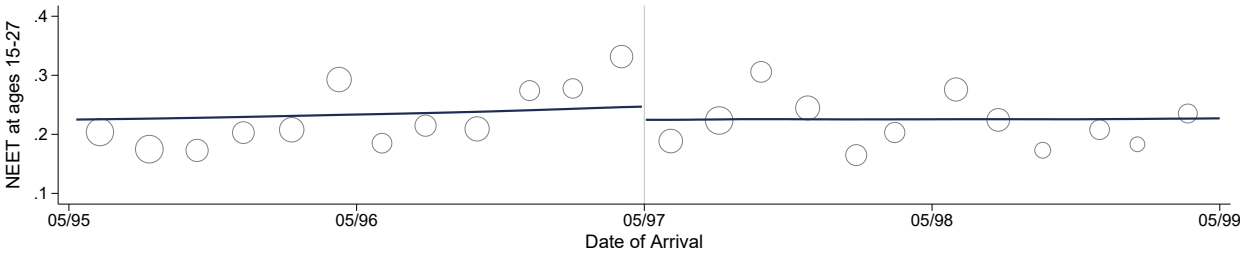
(b) Children's educational attainment (degree's average earnings) at age 27



(c) Children's standardized grade point average at 9th grade



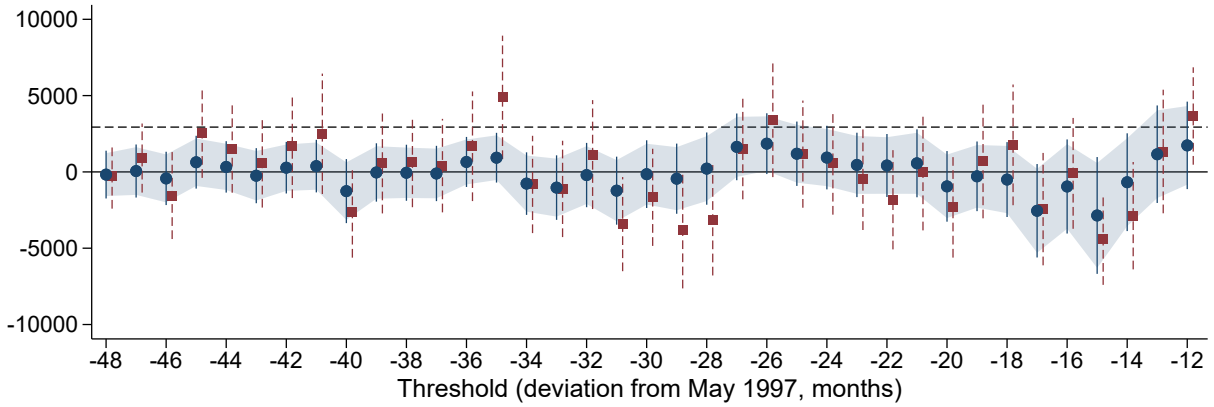
(d) Not in employment, education or training at ages 15–27



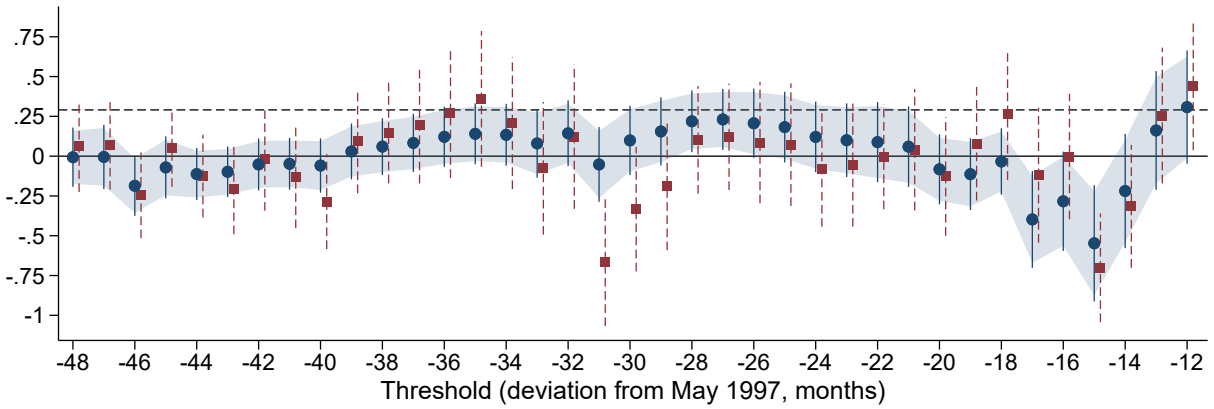
Notes. This figure is identical to Figure 1 except that we now define parents as the adult living in the same dwelling and belonging to the same family as the child when the child is 15 years old.

Figure A4: Alternative thresholds

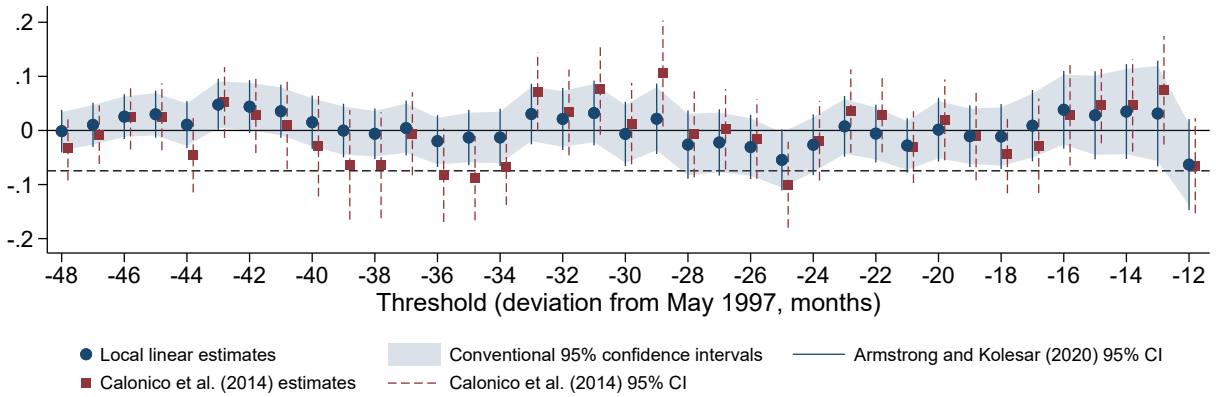
(a) Average earnings of the highest degree



(b) Grade point average at 9th grade



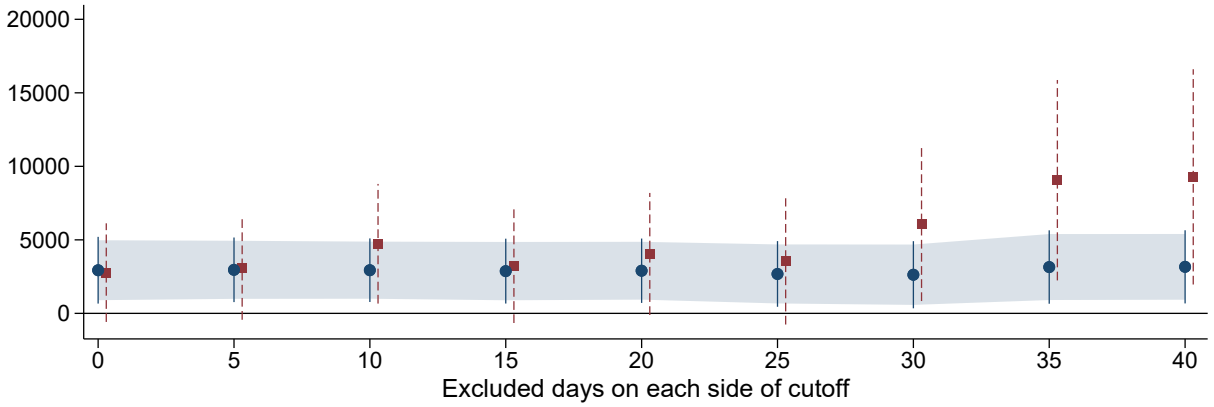
(c) Not in employment, education or training



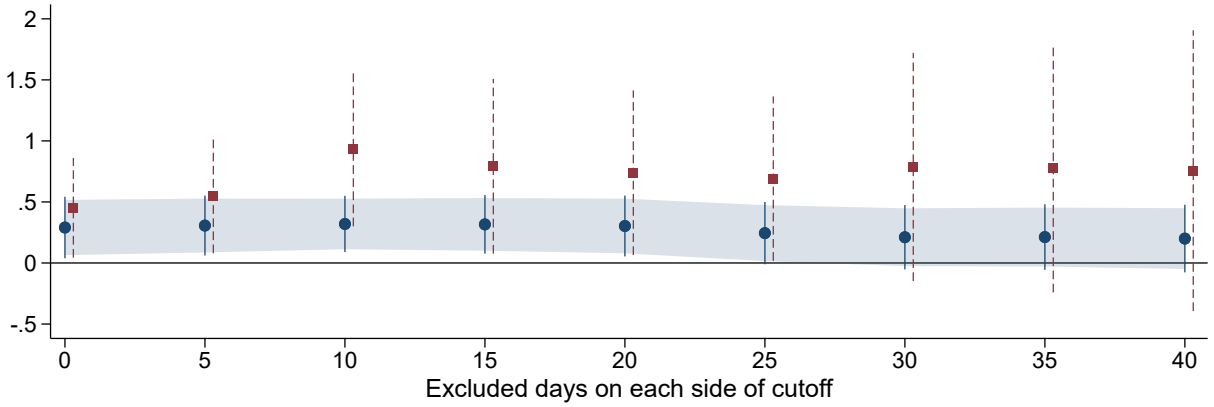
Notes. This figure presents estimates for made-up thresholds using the same approach as our baseline reduced form estimates and data only for families arriving to Finland before May 1st, 1997. The horizontal axis shows alternative cutoff dates away from May 1st, 1997. The blue circles represent local linear reduced form estimates, the shaded area depicts the corresponding conventional 95% confidence intervals and the blue spikes the "honest confidence intervals" of Armstrong and Kolesár (2020). The red squares and spikes are the Calonico et al. (2014) bias-corrected estimates for alternative bandwidths and the corresponding 95% confidence intervals. For reference, we also show the baseline estimates reported in Table 2 (horizontal dashed line).

Figure A5: Excluding observations around the cutoff

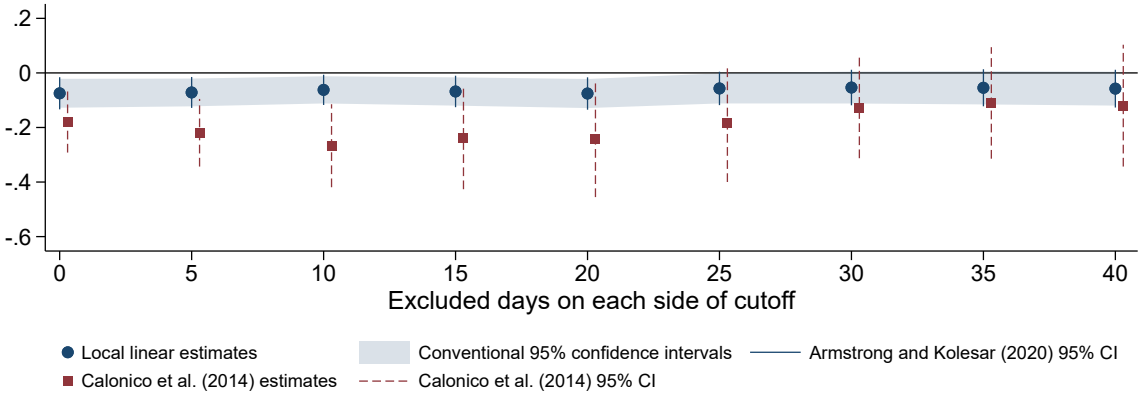
(a) Average earnings of the highest degree



(b) Grade point average at 9th grade



(c) Not in employment, education or training



Notes. This figure reports estimates from "donut hole" specifications, where we leave out observations close to the threshold. The horizonal axis shows the number of days excluded around the cutoff of May 1st, 1997. The blue circles show local linear reduced form estimates, the shaded area depicts the corresponding conventional 95% confidence intervals and the blue spikes the "honest confidence intervals" of Armstrong and Kolesár (2020). The red squares and spikes are the Calonico et al. (2014) bias-corrected estimates for alternative bandwidths and the corresponding 95% confidence intervals.

Table A1: Impact of parent's integration plan on GPA and educational attainment using first parent's arrival time

	Degree's average earnings		Standardized GPA		Not in employment, education or training	
	(1)	(2)	(3)	(4)	(5)	(6)
A: Estimates						
Reduced form	2,246	3,186	0.20	0.29	-0.07	-0.06
	(987)	(1,088)	(0.12)	(0.12)	(0.03)	(0.03)
First-stage	0.47	0.61	0.44	0.58	0.42	0.57
	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
Local average treatment effect (LATE)	4,796	5,261	0.47	0.49	-0.17	-0.10
	(2,168)	(1,824)	(0.28)	(0.22)	(0.07)	(0.05)
B: Benchmarks						
Compliers' expectation in the absence of the treatment	21,238	20,526	-0.30	-0.31	0.38	0.37
	(1,607)	(1,280)	(0.19)	(0.15)	(0.05)	(0.04)
Never-takers' average	26,231		-0.28		0.20	
Native's average	27,433		0.01		0.12	
Additional covariates	No	Yes	No	Yes	No	Yes
Bandwidth (months)	28		24		22	
Observations	1,603		1,429		1,332	

Notes. This table is identical to Table 2 except that we now use the date of arrival of the parent who first arrives in Finland as the running variable, while our main analysis is based on the date of arrival of the father.

Table A2: Impact of parent's integration plan on GPA and educational attainment with parents defined at age 15

	Degree's average earnings		Standardized GPA		Not in employment, education or training	
	(1)	(2)	(3)	(4)	(5)	(6)
A: Estimates						
Reduced form	2,502	1,784	0.34	0.25	-0.02	0.00
	(1,032)	(1,030)	(0.11)	(0.11)	(0.02)	(0.02)
First-stage	0.56	0.59	0.55	0.58	0.63	0.64
	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)
Local average treatment effect (LATE)	4,497	3,044	0.63	0.43	-0.04	-0.00
	(1,924)	(1,764)	(0.23)	(0.19)	(0.03)	(0.03)
B: Benchmarks						
Compliers' expectation in the absence of the treatment	21,100	22,213	-0.48	-0.34	0.28	0.25
	(1,329)	(1,250)	(0.15)	(0.13)	(0.02)	(0.02)
Never-takers' average	26,231		-0.28		0.20	
Native's average	27,433		0.01		0.12	
Additional covariates	No	Yes	No	Yes	No	Yes
Bandwidth (months)	34		31		65	
Observations	1,376		1,306		2,387	

Notes. This table is identical to Table 2 except that we now define parents as the adult living in the same dwelling and belonging to the same family as the child when the child is 15 years old.

Table A3: Impact of parent's integration plan by gender and parents' origin country

	Degree		GPA		NEET	
A: By gender						
Local average treatment	2,608	1,985	0.39	0.28	-0.09	-0.05
effect (LATE)	(2,039)	(1,912)	(0.23)	(0.21)	(0.05)	(0.05)
× female	4,197	4,037	0.26	0.20	-0.08	-0.05
	(1,517)	(1,487)	(0.17)	(0.16)	(0.04)	(0.04)
Compliers' expected outcomes	22,949	23,397	-0.98	-0.90	0.33	0.32
in the absence of the treatment	(1,334)	(1,289)	(0.15)	(0.14)	(0.04)	(0.04)
× female	-4,293	-4,043	0.29	0.01	0.03	0.00
	(995)	(997)	(0.11)	(0.05)	(0.03)	(0.01)
B: By parent's origin country HDI						
Local average treatment	5,449	4,420	0.52	0.40	-0.14	-0.09
effect (LATE)	(1,803)	(1,725)	(0.21)	(0.19)	(0.05)	(0.04)
× HDI	-36	179	-0.05	-0.02	-0.01	-0.02
	(573)	(589)	(0.08)	(0.08)	(0.03)	(0.02)
Compliers' expected outcomes	20,330	21,252	-0.84	-0.75	0.36	0.34
in the absence of the treatment	(1,202)	(1,181)	(0.14)	(0.13)	(0.04)	(0.03)
× HDI	1,308	1,328	0.16	-0.11	-0.06	0.02
	(379)	(392)	(0.05)	(0.07)	(0.02)	(0.01)
Additional covariates	No	Yes	No	Yes	No	Yes

Notes. The table shows local linear estimates for the jump the outcomes at the May 1, 1997 cutoff based on father's arrival time. The outcome in the first two columns is earnings predicted based on educational attainment at age 27, in the second and third column the ninth grade GPA and in the last two columns the share of years not in education, employment or training at ages 15 to 27. The dependent variable in the first stage is an indicator for either parent getting an integration plan. Additional covariates are parents' age, marital status, number of children under 18, regional unemployment rate at time of arrival, type of residence municipality (urban, semi-ruban, rural), legal status (refugee, Ingrian Finn, family member, other/unknown) and region of birth, child's sex and age at arrival. The bandwidths are chosen based on the optimal bandwidth selection algorithm of Imbens and Kalyanaraman (2012)

Table A4: Impact of parent's integration plan on parents' outcomes

	Earnings			Employment			Colleagues' native share			Colleagues' education			Overall		
	B	M	F	B	M	F	B	M	F	B	M	F	B	M	F
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
A: Estimates															
Reduced form	6,513	3,619	3,292	0.02	0.09	0.02	0.08	0.04	0.05	59	178	80	0.20	0.16	0.15
	(2,988)	(1,394)	(2,204)	(0.03)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)	(586)	(620)	(842)	(0.10)	(0.08)	(0.11)
First-stage	0.57	0.61	0.53	0.64	0.57	0.56	0.56	0.55	0.62	0.59	0.64	0.57	0.54	0.64	0.54
	(0.05)	(0.04)	(0.06)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)	(0.05)
LATE	11,408	6,083	6,173	0.04	0.16	0.03	0.13	0.06	0.08	96	261	134	0.37	0.26	0.27
	(5,490)	(2,428)	(4,295)	(0.05)	(0.09)	(0.08)	(0.07)	(0.07)	(0.06)	(947)	(908)	(1,397)	(0.20)	(0.13)	(0.20)
B: Benchmarks															
Compliers' $Y(0)$	19,809	8,868	11,729	0.44	0.41	0.46	0.80	0.84	0.82	24,561	23,706	25,707	-0.25	-0.24	-0.26
	(3,247)	(1,404)	(2,570)	(0.04)	(0.06)	(0.06)	(0.05)	(0.05)	(0.04)	(721)	(650)	(984)	(0.13)	(0.08)	(0.13)
Never-takers' $Y(0)$	26,095	12,460	14,006	0,50	0.51	0,51	0.84	0.88	0.81	24,427	23,670	23,735	-0.04	-0.12	-0.07
Native's $Y(0)$	48,756	22,664	28,238	0.74	0.76	0.73	0.98	0.98	0.99	25,183	24,133	26,365	0.60	0.55	0.50
Additional covariates	No	No	No	No	No	No	No	No	No	No	No	No	No	No	No
Bandwidth (months)	28	38	22	49	27	26	25	25	41	33	49	27	26	34	26
Observations	1,233	1,482	1,008	1,895	712	1,173	876	688	944	1,018	950	738	1,108	1,890	1,087

Notes. This table reports local linear estimates for the jump at the May 1, 1997 cutoff of father's arrival time for parents' earnings (columns 1-3), employment (columns 4-6), share of native colleagues (columns 7-9), colleagues education as measured by predicted earnings of the degree (columns 10-12) and an overall index of parent' labor market integration as per Kling et al. (2007). Columns B refer to both parents, M to mothers and F to fathers. Reduced form refers to the jump in the outcome at the May 1997 threshold and first-stage to the jump in the likelihood for either parent getting an integration plan. The bandwidths are chosen using the optimal bandwidth selection algorithm of Imbens and Kalyanaraman (2012).

Table A5: Impact of parent's integration plan on parents' outcomes (conditional on background characteristics)

	Earnings			Employment			Colleagues' native share			Colleagues' education			Overall		
	B	M	F	B	M	F	B	M	F	B	M	F	B	M	F
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
A: Estimates															
Reduced form	3,311	2,213	694	-0.02	0.03	-0.05	0.06	0.04	0.06	-294	-129	-440	0.06	0.07	0.00
	(2,358)	(1,238)	(1,790)	(0.03)	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)	(567)	(638)	(786)	(0.07)	(0.06)	(0.08)
First-stage	0.60	0.64	0.56	0.66	0.60	0.59	0.59	0.59	0.64	0.62	0.66	0.60	0.58	0.66	0.57
	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)
LATE	5,527	3,557	1,220	-0.03	0.05	-0.08	0.10	0.06	0.09	-459	-181	-705	0.10	0.11	0.00
	(3,925)	(1,982)	(3,117)	(0.04)	(0.07)	(0.07)	(0.06)	(0.07)	(0.05)	(877)	(891)	(1,242)	(0.12)	(0.10)	(0.14)
B: Benchmarks															
Compliers' $Y(0)$	22,475	10,049	14,039	0.48	0.46	0.51	0.82	0.82	0.81	25,084	24,140	26,320	-0.12	-0.15	-0.12
	(2,802)	(1,328)	(2,217)	(0.03)	(0.06)	(0.05)	(0.05)	(0.05)	(0.04)	(704)	(670)	(940)	(0.11)	(0.08)	(0.11)
Never-takers' $Y(0)$	26,095	12,460	14,006	0,50	0.51	0,51	0.84	0.88	0.81	24,427	23,670	23,735	-0.04	-0.12	-0.07
Native's $Y(0)$	48,756	22,664	28,238	0.74	0.76	0.73	0.98	0.98	0.99	25,183	24,133	26,365	0.60	0.55	0.50
Additional covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth (months)	28	38	22	49	27	26	25	25	41	33	49	27	26	34	26
Observations	1,233	1,482	1,008	1,895	712	1,173	876	688	944	1,018	950	738	1,108	1,890	1,087

Notes. This table reports local linear estimates for the jump at the May 1, 1997 cutoff of father's arrival time for parents' earnings (columns 1-3), employment (columns 4-6), share of native colleagues (columns 7-9), colleagues education as measured by predicted earnings of the degree (columns 10-12) and an overall index of parent' labor market integration as per Kling et al. (2007). The estimates are conditional on background characteristics: child's sex and age at arrival and parents' age, marital status, number of children under 18, regional unemployment rate, type of residence municipality (urban, semi-ruban, rural), legal status (refugee, Ingrian Finn, family member, other/unknown) and region of birth. Columns B refer to both parents, M to mothers and F to fathers. Reduced form refers to the jump in the outcome at the May 1997 threshold and first-stage to the jump in the likelihood for either parent getting an integration plan. The bandwidths are chosen using the optimal bandwidth selection algorithm of Imbens and Kalyanaraman (2012).

Table A6: Impact of parent's integration plan on 9th grade peers

	Later education		Parents' native share		Parents' education		Overall	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Estimates								
Reduced form	893	879	0.01	0.02	804	1,011	0.22	0.21
	(429)	(422)	(0.01)	(0.01)	(657)	(672)	(0.11)	(0.11)
First-stage	0.56	0.59	0.56	0.59	0.61	0.63	0.52	0.56
	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)	(0.04)	(0.06)	(0.05)
LATE	1,575	1,479	0.02	0.03	1,356	1,642	0.42	0.38
	(773)	(717)	(0.03)	(0.02)	(1,102)	(1,082)	(0.21)	(0.19)
B: Benchmarks								
Compliers' $Y(0)$	24,562	24,688	0.86	0.86	47,664	47,588	-0.45	-0.41
	(499)	(493)	(0.02)	(0.02)	(682)	(701)	(0.15)	(0.14)
Never-takers' $Y(0)$	26,192		0.93		49,179		0.10	
Native's $Y(0)$	27,731		0.98		48,325		0.29	
Additional covariates	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth (months)	26	26	25	25	36	36	21	21
Observations	1,173	1,173	1,089	1,089	1,362	1,362	1,002	1,002

Notes. This table reports local linear estimates for the discontinuity at the May 1, 1997 cutoff of father's arrival time in the characteristics of the child's peers: average later education (columns 1-2, measured as expected earnings in the attained degree), the native share of peer's parents (columns 3-4), the average education of peers' parents (columns 5-6, measured as the expected earnings in the attained degree of both parents combined) and an overall index as per Kling et al. (2007) (columns 7-8). Reduced form refers to the jump in the outcome at the May 1997 threshold and first-stage to the jump in the likelihood for either parent getting an integration plan. Additional covariates are child's sex and age at arrival and parents' age, marital status, number of children under 18, regional unemployment rate, type of residence municipality (urban, semi-ruban, rural), legal status (refugee, Ingrian Finn, family member, other/unknown) and region of birth. All background characteristics are measured at the year of arrival. The bandwidths are chosen using the optimal bandwidth selection algorithm of Imbens and Kalyanaraman (2012).