

DISCUSSION PAPER SERIES

IZA DP No. 15310

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Integration Policies:  
Evidence from Finland's Integration Plans**

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MAY 2022 (THIS VERSION: AUGUST 2024)

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## ABSTRACT

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

We examine the intergenerational effects of an integration program that increased language training and improved labor market outcomes of adult immigrants in Finland. Exploiting a discontinuity in the phase-in rule of a reform, we find that parents' participation in the program improved their children's grades by 0.5 standard deviations and extended their educational attainment by over a year. Two decades post-arrival, children of the affected immigrants earned 42% more than their counterparts whose parents narrowly missed the policy's implementation.

**JEL Classification:** J61, J68, J13, H53

**Keywords:** immigrants, integration policy, intergenerational effects

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# 1 Introduction

Children of immigrants tend to have lower grades, perform worse in aptitude tests, complete less education and have lower earnings than children of native-born parents.<sup>1</sup> These differences may arise through several mechanisms. For example, immigrants' children might face discrimination or place less emphasis on education compared to natives' children. However, the gaps could also reflect the fact that children of immigrants often grow up in disadvantaged neighborhoods and low-income families with parents who do not speak the local language and may be disconnected from the native population. Therefore, policies aimed at improving the integration of adult immigrants could potentially have spillover effects on their children. This paper shows that such intergenerational effects can be substantial.

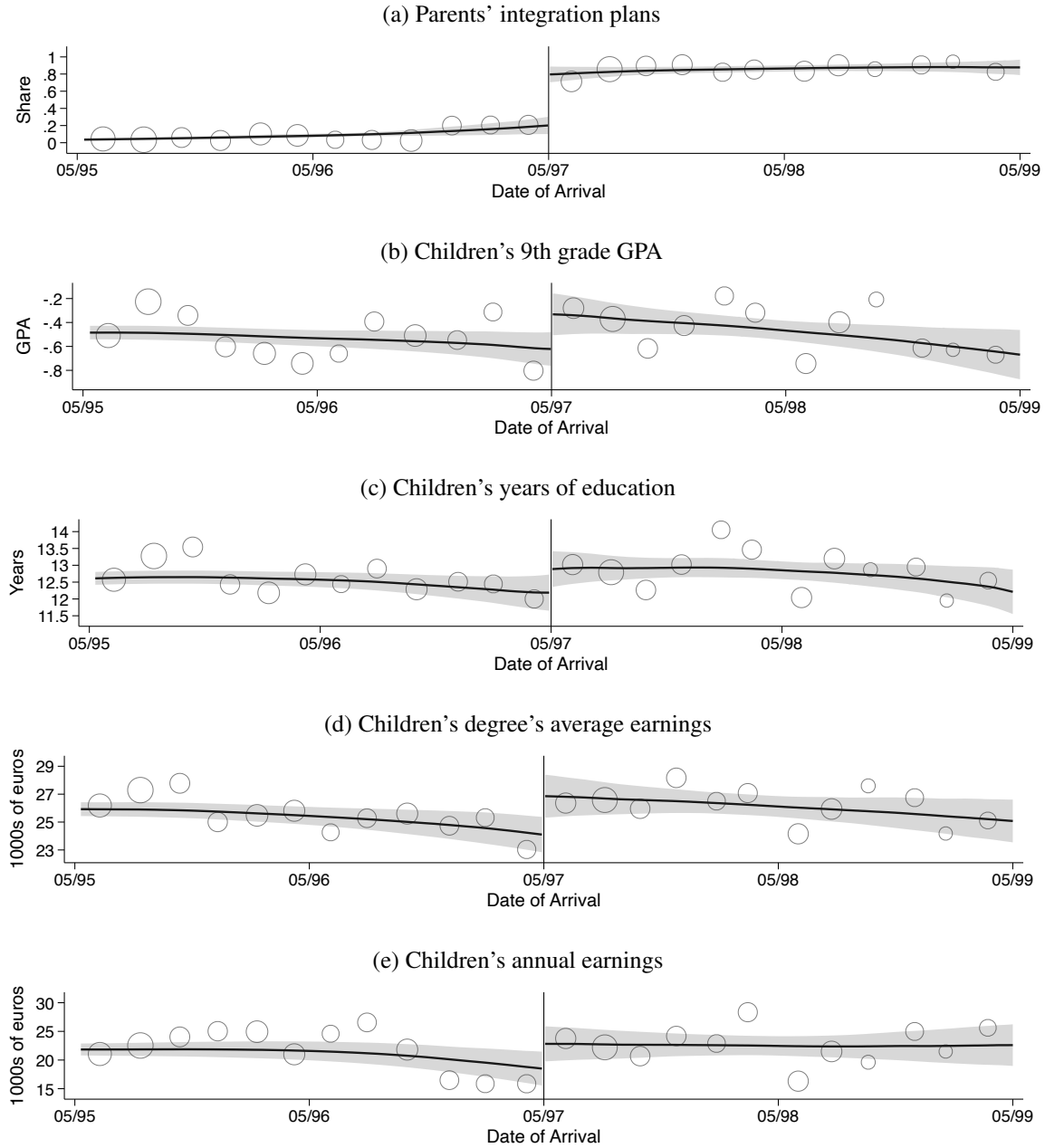
We study a reform that refined how immigrants were allocated to active labor market policies (ALMP) in Finland. We focus on the introduction of integration plans—individualized sequences of ALMP that a caseworker believes to be the most appropriate for each immigrant. [Sarvimäki and Hämäläinen \(2016\)](#) show that these integration plans increased the amount of language training provided to immigrants and improved their earnings. Here, we extend the analysis to the children of the affected immigrants. The children were, on average, 11 years old upon arrival, and we follow them until age 31.

Our research design builds 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.

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<sup>1</sup>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



*Notes.* This figure shows date of arrival (horizontal axis) and the share of at least one parent receiving an integration plan (panel a), children's 9th grade GPA (panel b), years of education (panel c), average earnings of earlier graduates from the highest degree or enrollment (panel d), and earnings (panel e). Earnings, years of education and degree are measured at age 31; GPA is measured at age 16. 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.

The subsequent panels of Figure 1 present our main findings. Panel (b) shows that children whose parents arrived in May 1997 graduated with better grades from middle school compared to children whose parents arrived slightly earlier. Together, the discontinuities in panels (a) and (b) yield a local average treatment estimate (LATE) for the intergenerational effect of the integration plans. The LATE estimate suggests that parents' integration plans increased compliers' grades at the end of mandatory education by 0.5 standard deviations, corresponding to a shift from the 25th percentile to the 38th percentile of the GPA distribution.<sup>2</sup> Panel (c) shows a discontinuity in years of education at age 31, with LATE estimates indicating that parents' integration plans led to an increase of over a year in completed education. Panel (d) reveals an even clearer discontinuity in predicted annual earnings associated with specific degrees (approximated using data from earlier graduates). All estimates are statistically significant and withstand a battery of robustness checks and falsification exercises.

Importantly, panel (e) of Figure 1 shows a discontinuity also in annual earnings at age 31. The LATE estimate suggests that parents' integration plans increased their children's earnings by roughly 7,000 euros or 42 percent, pushing them from the 30th percentile to the 48th percentile in the earnings distribution of their respective birth cohorts. Again, the estimates are statistically significant and robust.

We examine several possible mechanisms behind these results. First, we show that the integration plans substantially increased parents' earnings and improved mothers' employment. Second, we ask whether the integration plans affected the characteristics of parent's colleagues at work and children's peers at school. While the point estimates suggest that such effects may have been present, we lack statistical power and many of the estimates are statistically insignificant. Finally, we note that the integration plans likely improved parents' language skills, which may have directly affected their children's outcomes.

These findings add to two branches of literature. First, we contribute to the body of work evaluating the impacts of integration policies showing that integration programs implemented as part

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<sup>2</sup>By "compliers", we refer to families who got an integration plan because they arrived after the threshold date and would not have gotten it otherwise.

of ALMP can improve adult immigrants' later income and employment ([Åslund and Johansson, 2011](#); [Joonas and Nekby, 2012](#); [Sarvimäki and Hämäläinen, 2016](#); [Battisti et al., 2019](#); [Lochmann et al., 2019](#); [Foged et al., 2022](#); [Foged and van der Werf, 2023](#); [Heller and Mumma, 2023](#); [Dahlberg et al., 2024](#)). 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., 2022](#)), decreased implicit bias of teachers ([Alesina et al., 2023](#)), 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 help immigrants' children. Earlier work examining intergenerational effects of integration programs is limited to [Foged et al. \(2023\)](#), who evaluate a Danish reform changing the approach for integrating refugees. They find intergenerational effects on grades, secondary school completion rates and juvenile crime rates. Here, we dig deeper by investigating subsequent educational choices, labor market outcomes, and potential underlying mechanisms. While our general take-aways are similar to [Foged et al. \(2023\)](#)—thus strengthening the credibility of both papers—our findings differ in some important details such as us finding larger effects for daughters than for sons.<sup>3</sup>

We also add to the broader literature examining the spillover effects of parents' income and employment on children's outcomes. Earlier work has shown that income transfers to parents increase their children's educational attainment, test scores, health, employment and income and reduce their crime rates (e.g. [Akee et al., 2010](#); [Milligan and Stabile, 2011](#); [Dahl and Lochner, 2012](#); [Black et al., 2014](#); [Aizer et al., 2016](#); [Bastian and Michelmore, 2018](#); [Manoli and Turner,](#)

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<sup>3</sup>A possible reason for this difference is that [Foged et al. \(2023\)](#) primarily concentrate on outcomes relevant to the most disadvantaged (predominantly male) children whereas our outcomes capture variation throughout the population. Consistent with this hypothesis, [Foged et al. \(2023\)](#) also find a positive effect on lower secondary school GPA for girls.

2018; Bulman et al., 2021; Barr et al., 2022). Furthermore, parental job loss affects children’s school performance, college attendance and field of study (Coelli, 2011; Rege et al., 2011; Hilger, 2016; Huttunen and Riukula, 2019). 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 next two sections discuss the reform, our research design, and data. Section 4 reports our main results and robustness checks and discusses the possible 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 ALMP measures such as training, subsidized work, and rehabilitation. The immigrants were expected to follow the plan and non-compliance was sanctioned by benefit withdrawal. In addition, the reform increased language course availability for all immigrants (see Sarvimäki and Hämäläinen, 2016, for details). However, this part of the reform likely affected the control and treatment groups similarly and is thus not captured by our estimates.

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 1st, 1997. Those who had registered before May 1st, 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 increases by roughly 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 1st, 1997 cutoff. We start with the reduced form regression:

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

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),  $\mathbf{1}[d_i \geq d_0]$  is an indicator taking value one if the parents arrived in Finland after the threshold and zero otherwise,  $X_i$  is a vector of observed characteristics measured at arrival, and  $\varepsilon_i$  summarizes unobserved characteristics. The parameter of interest is  $\beta$ , which measures the discontinuity in the outcome at the May 1st, 1997 threshold. We move to local average treatment effect (LATE) estimates by scaling  $\beta$  with the discontinuity in the likelihood of receiving an integration plan.<sup>4</sup> 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 facilitate 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 combining information from various administrative registers. The underlying data cover the total population living in Finland in 1987–2019. We focus on children whose both (or the only known) parents are immigrants who arrived in Finland between January 1990 and April 1999. Statistics Finland defines parents as adults who live in the same

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<sup>4</sup>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 at least one of person  $i$ 's parents received an integration plan and zero otherwise.

dwelling as the child and are part of the same family.<sup>5</sup> This definition does not distinguish between biological and adopted children, and parents may change over the years. In our main analysis, we define parents as adults who live with the child the longest while the child is under 15 years old and use father's arrival date as the running variable. In the Online Appendix, we show that the results are robust to alternative definitions of parents and the running variable.

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 31 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 31 within 23 years of arrival. Our final estimation sample consists of 2,874 children, of whom 1,102 arrived within two years of the May 1997 threshold.

## 3.2 Outcomes

Our first educational measure is the grade point average (GPA) at the end of mandatory education (9th grade) at age 16 as reported in the Finnish National Board of Education's Application Registry. A limitation of this measure is that these grades are not based on standardized tests, and thus 9th grade GPA may not be a fully comparable measure of academic skills across schools. Nevertheless, it is the primary criterion in the competition for access to secondary education.<sup>6</sup> Therefore, we interpret 9th-grade GPA as an informative yet incomplete measure of academic aptitude and

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<sup>5</sup>A child and an adult are defined to belong to the same family if they live together and the adult is the child's biological or adoptive parent. In addition, the family may include a spouse of the biological or adoptive parent.

<sup>6</sup>For the birth cohorts we examine, the Finnish education system consisted of nine years of mandatory comprehensive school followed by non-mandatory secondary education in academic track high school or vocational institution and tertiary education in polytechnics and universities. Selection into the academic track high school was almost exclusively based on GPA (the only exceptions being high schools specialized in arts and sports). GPA was important also in selection into vocational training but vocational programs could also use aptitude tests and take into account experience, minority gender and applicants' preference ranking; see [Silliman and Virtanen \(2022\)](#) for details. Education was free-of-charge at all levels and students were entitled to generous allowances and government-backed loans.

educational opportunities upon completing mandatory education.

For educational attainment, we use two complementary measures using data from the Register of Attained Degrees and the Student Register. These sources contain information on completed degrees and enrollment in upper secondary and tertiary education at the six-digit level. First, we transform the completed degrees into years of education using the recommended time required to complete each degree. Second, we characterize each six-digit degree with average earnings at age 31 of earlier graduates with the same degree. These average earnings are based on data covering the years 1995–2019 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 31. In the Online Appendix, we also report estimates for indicators for completing different types of secondary and tertiary degrees.

Finally, we measure annual earnings, including zeros, at age 31 using information from the Finnish Tax Authorities. In our main analysis, we focus on earnings winsorized at the 99th percentile. As robustness checks, we report results for raw earnings and earnings percentile in the Online Appendix.

### **3.3 Background characteristics**

Table 1 reports the average background characteristics of the children and their parents 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.<sup>7</sup> Almost half were born in the former Soviet Union.

The last two columns of Table 1 and Online Appendix Figure A2 report local linear estimates for jumps of the background characteristics at the May 1997 threshold. Most of the estimates

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<sup>7</sup>Ingrian Finns are a minority group of Finnish descent in the former Soviet Union 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 at May 1, 1997	
	5/95- 4/96	5/96- 4/97	5/97- 4/98	5/98- 4/99	RD estimate	Standard error
	(1)	(2)	(3)	(4)	(5)	(6)
<i>A: Children</i>						
Age	10.99	11.23	11.85	12.27	0.16	(0.22)
Female	0.46	0.49	0.49	0.51	-0.01	(0.06)
<i>B: Parents</i>						
Age	37.63	37.74	38.85	39.00	0.85	(0.63)
Female	0.50	0.50	0.50	0.50	0.00	(0.01)
Single parent	0.09	0.13	0.10	0.06	0.02	(0.04)
Children under 18	0.88	0.86	0.87	0.87	-0.08	(0.04)
Regional unemployment rate	14.30	13.00	11.38	11.17	0.11	(0.39)
Urban municipality	0.82	0.85	0.86	0.81	0.04	(0.04)
Legal status:						
Refugee	0.22	0.19	0.15	0.17	-0.11	(0.05)
Ingrian Finn	0.28	0.29	0.32	0.17	0.14	(0.05)
Family member	0.03	0.06	0.09	0.12	-0.05	(0.02)
Other/Unknown	0.46	0.46	0.44	0.54	0.02	(0.05)
Region of birth						
Asia	0.16	0.22	0.21	0.21	-0.16	(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	(0.03)
form. Soviet Union	0.51	0.52	0.66	0.57	0.20	(0.06)
form. Yugoslavia	0.16	0.08	0.02	0.07	-0.03	(0.02)
Other	0.03	0.01	0.01	0.00	0.03	(0.02)
Observations	346	245	291	220	0.33	(0.27)

*Notes.* This table reports sample means of background characteristics by time of arrival (columns 1 to 4) and local linear estimates for jumps at May 1, 1997 (point estimate in column 5; standard error in column 6). Parents' characteristics denote the mean of mother's and father's characteristics. All characteristics are measured at the year of arrival. While the characteristics are aggregated to annual averages in the table, the discontinuities at the May 1, 1997 cut-off are estimated from the underlying data using exact dates of arrival. Hence, the number of observations used in estimating the discontinuity is also at the daily level. The share of female parents includes parents arriving in separate years.

are small and statistically insignificant. 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 results. The first row, columns (1), (3), (5) and (7), report reduced form estimates corresponding to the discontinuities we discussed in the introduction (panels b–e 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, had 0.28 standard deviations higher 9th grade GPA, 0.7 years more education, degrees associated with 2,770 euros higher earnings and 4,358 euros higher actual earnings at age 31. All estimates are statistically significant at the 5% level with the exception of the point estimate for years of education which has a p-value of 0.067. Controlling for background characteristics slightly increases the point estimates for earnings and reduces the point estimates for education, but conditional and unconditional estimates are statistically indistinguishable from each other.

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, depending on the bandwidth, show 51–62 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 yield to local average treatment effect (LATE) estimates reported in the third row. The point estimates suggest that parents' integration plans increased compliers' GPA by 0.33–0.48 standard deviations and educational attainment by 1.1–1.4 years. Furthermore,

Table 2: Impact of parent’s integration plan on GPA, educational attainment and earnings

	9th grade GPA		Years of education		Degree’s average earnings		Annual earnings at age 31	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>A: Estimates</i>								
Reduced form	0.28 (0.11)	0.20 (0.11)	0.70 (0.38)	0.63 (0.37)	2,770 (1,018)	2,539 (1,010)	4,358 (2,218)	4,598 (2,194)
First-stage	0.59 (0.05)	0.62 (0.04)	0.51 (0.06)	0.55 (0.05)	0.58 (0.05)	0.61 (0.04)	0.59 (0.05)	0.62 (0.04)
Local average treatment effect (LATE)	0.48 (0.20)	0.33 (0.18)	1.38 (0.79)	1.14 (0.68)	4,787 (1,828)	4,162 (1,673)	7,373 (3,773)	7,408 (3,518)
<i>B: Benchmarks</i>								
Compliers’ expectation in the absence of the treatment	-0.97 (0.13)	-0.86 (0.12)	11.27 (0.57)	11.38 (0.52)	22,731 (1,155)	23,223 (1,086)	17,390 (2,563)	16,871 (2,442)
Never-takers’ average	-0.39		13.2		27,677		22,370	
Native’s average	0.00		13.7		28,248		27,382	
Additional covariates	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth (months)	32.6		20.8		30.1		33.3	
Observations	1,318		945		1,268		1,339	

*Notes.* This table reports local linear estimates for the jump at the May 1, 1997 cutoff of father’s arrival time on standardized 9th grade GPA (columns 1–2), years of education measured as the recommended completion time of the degree (columns 3–4), average earnings at age 31 of earlier graduates with the same degree (columns 5–6) and actual average earnings at age 31 (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-urban, 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 \(2012\)](#).

parents' integration plans pushed their children towards degrees associated with 4,162–4,787 euros higher earnings and led to 7,373–7,408 euros increase in their actual earnings at age 31.<sup>8</sup> All estimates are statistically significant at the 5% level with the exception of the unconditional point estimate for years of education and the conditional point estimates for GPA and years of education which have p-values of 0.08, 0.06 and 0.10, respectively.

To put these estimates into context, the bottom panel of Table 2 reports three types of benchmarks: expectations of the outcomes for the compliers' in the absence of the treatment, average outcomes of never-takers and average outcomes of natives.<sup>9</sup> This analysis reveals that the compliers largely consisted of the most disadvantaged children. Compared to natives, complier families' offspring had, on average, almost a standard deviation lower 9th grade GPA, 2.4 years less education, final degrees associated with 20 percent lower average earnings and 36 percent lower actual annual earnings. These gaps mean that an average complier was at the 25th percentile of the GPA distribution, and at the 30th percentile of both the distribution of degrees' average earnings and the income distribution (Online Appendix Table A2). By comparison, the averages of never-takers correspond to the 39th percentile of the GPA, the 48th percentile of degree's average earnings and the 43rd percentile of the annual earnings distributions, respectively. Thus, the LATE estimates suggest that parents' integration plans largely closed the gaps between the compliers and never-takers and considerably narrowed the gaps between the compliers and natives' children.

## 4.2 Robustness

In the previous subsection, we found that the results are not sensitive to controlling for observable characteristics at arrival. Figure 2 examines their robustness to bandwidth and estimation method

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<sup>8</sup>The effects on degrees' average earnings arise from a shift from vocational training to more academic degrees (Online Appendix Figure A3). The point estimates suggest 18 percentage points decrease in the likelihood of completing a vocational degree and 20, 22 and 10 percentage points increases in the likelihood of graduating from academic track high school, polytechnic and university, respectively (Online Appendix Table A1). However, these estimates are statistically significant only for academic track high school and polytechnic degrees.

<sup>9</sup>"Compliers" refers to families that got an integration plan because they arrived after the threshold date and would not have gotten it otherwise. "Never-takers" are 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.

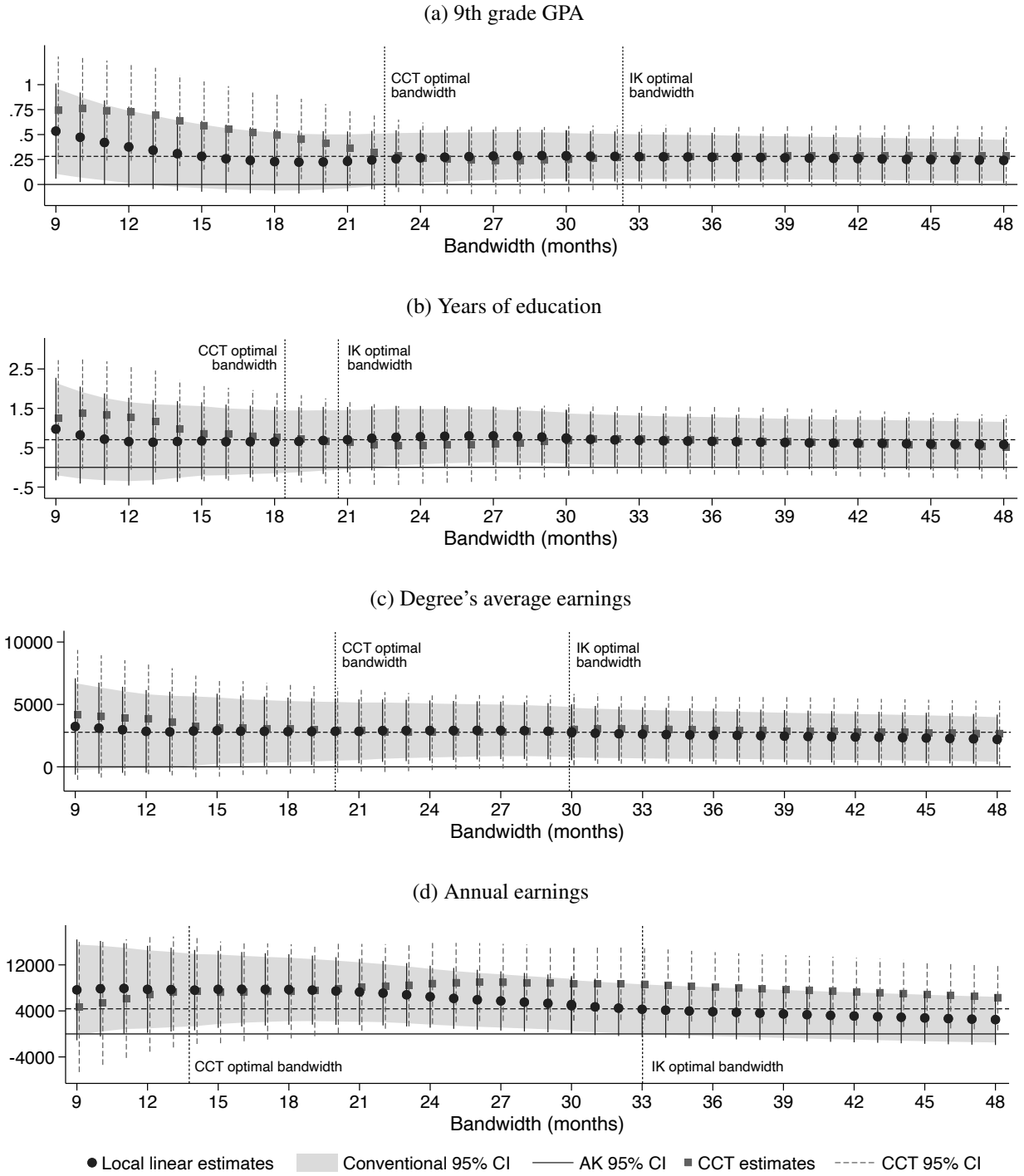
by showing reduced form estimates for bandwidths varying between 9 and 48 months. For each estimate, we report conventional 95% confidence intervals and [Armstrong and Kolesár \(2020\)](#) “honest” 95% confidence intervals, which take into account the possible bias of the local linear estimates. In addition, 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 GPA vary between 0.22 and 0.76 standard deviations. Estimates using smaller bandwidths and the [Calonico et al. \(2014\)](#) bias correction approaches tend to be larger than those using data further away from the threshold or those based on conventional local linear regressions. The confidence intervals of [Calonico et al. \(2014\)](#) are substantially wider than [Armstrong and Kolesár \(2020\)](#) confidence intervals. Most of the estimates are statistically significant at conventional levels, but the confidence intervals include zeros for intermediate bandwidths.

The remaining panels of [Figure 2](#) present similar analyses for our measures of educational attainment and earnings. The point estimates vary across alternative bandwidths and estimation approaches, but they all indicate the same direction. For example, all point estimates for annual earnings are positive and range between 2,567 and 9,006 euros, with largest estimates coming from the [Calonico et al. \(2014\)](#) approach. Again, most but not all estimates are statistically significant at conventional levels.

In the Online Appendix, we further show that jumps of similar size as our main estimates rarely occur in made-up “placebo” thresholds ([Online Appendix Figure A6](#)), and the results are robust to “donut hole” specifications, where we leave out observations close to the threshold ([Online Appendix Figure A7](#)). The estimates are also robust to alternative definitions of our outcome variables ([Online Appendix Figures A4–A5](#) and [Online Appendix Tables A2–A3](#)) as well as alternative definitions of the running variable, specifically using the date of arrival of the parent who first (or last) arrived in Finland, mother’s arrival date or defining parents as adults who lived with the child when she was 15 years old ([Online Appendix Tables A5–A7](#)). In short, our key conclusion that parents’ integration plans improved their children’s education and earnings appears robust to alternative

Figure 2: Robustness to alternative bandwidths and estimation approaches



*Notes.* The black circles show local linear reduced form estimates using triangle kernels and alternative bandwidths (x-axis). The shaded area shows the corresponding conventional 95% confidence intervals and the black spikes the “honest confidence intervals” of [Armstrong and Kolesár \(2020\)](#). The grey 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 lines) and the optimal bandwidths based on [Calonico et al. \(2014\)](#) and [Imbens and Kalyanaraman \(2012\)](#) (vertical dashed lines).

estimation approaches.

### 4.3 Effect heterogeneity

The left panel of Figure 3 summarizes the results of our treatment effect heterogeneity analysis.<sup>10</sup> 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 suggest that parents’ integration plans have a larger effect on daughters than on sons. Most importantly, the point estimates reported in panel D indicate that parents’ integration plans increased average earnings at age 31 by 0.56 standard deviations among daughters and by 0.15 standard deviations among sons, and the estimate is statistically significant only for daughters. The pattern is similar for other outcomes with larger point estimates for daughters than for sons. However, the difference between sons and daughters is statistically significant (p-value 0.011) only for annual earnings. By contrast, we find no treatment effect heterogeneity across parents’ origin country Human Development Index.

### 4.4 Mechanisms

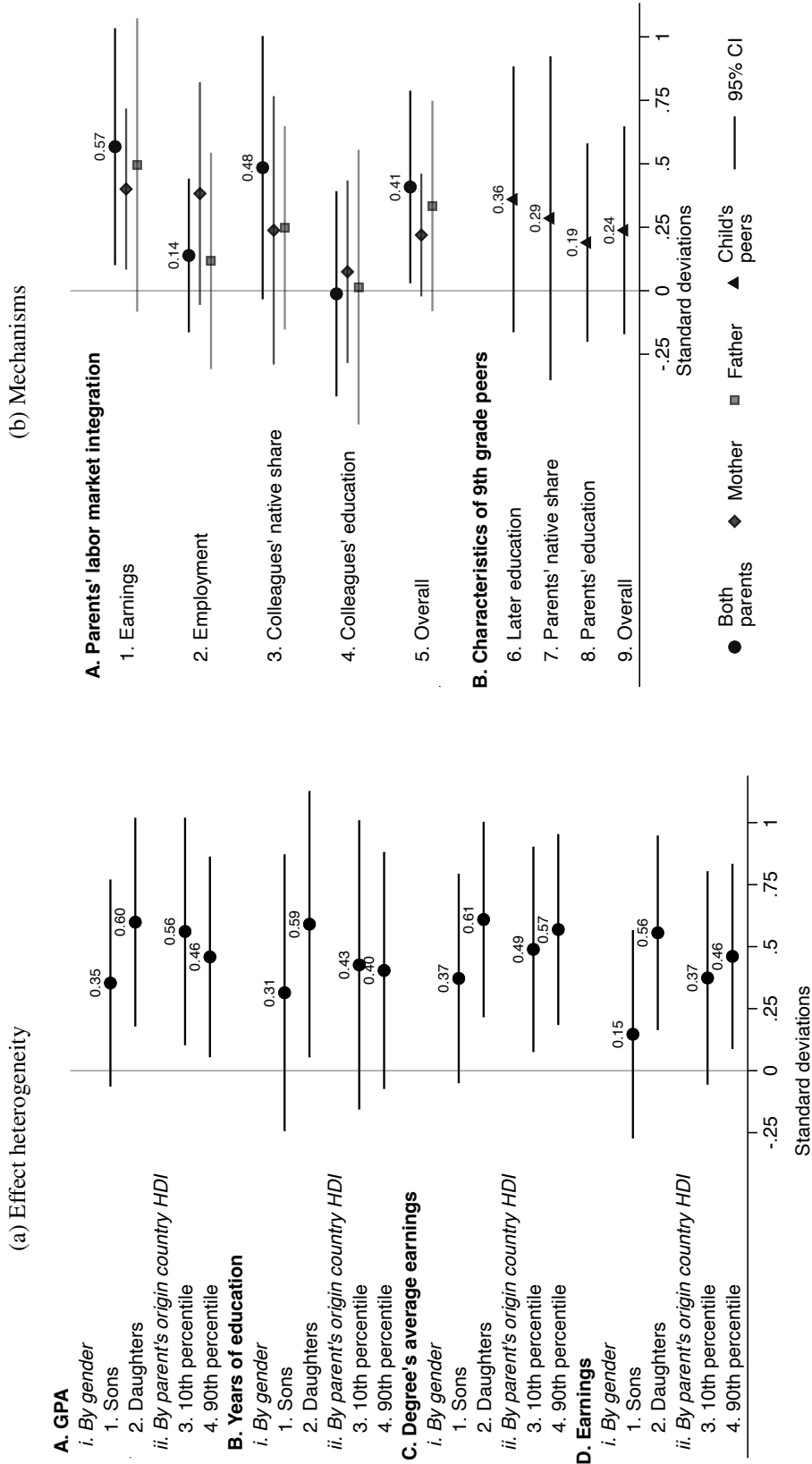
The right panel of Figure 3 examines some possible mechanisms that could give rise to intergenerational effects. Each entry corresponds to a LATE estimate from the specification used in our main analysis. Again, all outcome variables are normalized. For parents, we report estimates separately for mother, 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 also 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 0.57

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<sup>10</sup>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 Online Appendix Table A8. 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 or 90th percentile of the HDI distribution. Missing values of the HDI for Somalia are replaced with average value of countries in the group of low human development.

Figure 3: Effect heterogeneity and potential 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 10. 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. The bandwidths are chosen using the optimal bandwidth selection algorithm of [Imbens and Kalyanaram \(2012\)](#). Appendix Tables [A8-A11](#) report the parameter estimates from the underlying regressions.

standard deviations corresponding to 15,718 euros or 84% increase (Online Appendix Table A9). The estimate is statistically significant with a p-value of 0.017. Both parents appear to contribute to the overall effect: the point estimates are 0.40 standard deviations for mothers and 0.50 standard deviations 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 (Online Appendix Table A10). Nevertheless, the point estimates from specifications controlling for background characteristics suggest that integration plans increased mothers' earnings by 0.23 standard deviations and fathers' earnings by 0.15 standard deviations, and the p-value for the estimate for both parents is 0.072.

We next examine effects on employment, measured as the share of years that the parent was employed at the end of each year between 2000 and 2019. For mothers, the baseline point estimate shows an increase of 0.38 standard deviations or 15 percentage points (from a baseline of 41 percent). However, the estimate is again imprecise and only borderline significant with a p-value of 0.087. For fathers, the point estimate for employment is 0.12 standard deviations or 4 percentage points (from a baseline of 43 percent) and statistically insignificant. Again, the estimates are sensitive to controlling for background characteristics (Online Appendix Table A10).

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, they suggest that integration plans improved the labor market outcomes of immigrant parents.

Parents' integration plans may have affected their children also through other channels. For example, integration training is likely to have directly improved parents' language skills and provided them with information about the Finnish education system. In addition, improved labor market outcomes may have influenced social networks, leading to better access to information or helping overcome cultural barriers that may affect educational choices.

Regrettably, we do not observe parent’s language skills. However, while our register-based data does not include information about friends or acquaintances, we can identify one important social group: colleagues at work.<sup>11</sup> 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 imprecise—the p-value for the estimate for both parents is 0.067—the point estimates vary between 0.24 and 0.48 standard deviations. By contrast, the estimates for colleagues’ education are close to zero and statistically insignificant. Finally, the estimate for the overall index of parents’ labor market integration is 0.41 standard deviations with a p-value of 0.035.

Parents’ integration plans could also have affected their children’s peers. Most importantly, parents’ improved labor market integration may have allowed the families to move to more affluent neighborhoods. Again, we do not directly observe children’s social networks, but we can approximate them using register-based information on children’s 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 point estimates suggest that integration plans may have affected the kind of school immigrants’ children attended. The point estimate for peers’ later education, measured using average earnings of earlier graduates, is 0.36 standard deviations corresponding to a 1,138 euros or 4% increase in the average earnings of peers’ later degrees (Online Appendix Table A11). The point estimates for peers’ parental education and native share are 0.19 and 0.29 standard deviations, respectively, although 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 1,010 euro or 2 percent increase in the average earnings associated with parents’ degrees. Controlling for

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<sup>11</sup>We use our population-level data and establishment identifiers to characterize other people working in the same establishment at the same time as the parent. 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.

background characteristics at arrival has little effect on the estimates. Importantly, however, none of the estimates is statistically significant. The estimates for the overall index of 9th-grade peers is 0.24 standard deviations with a p-value of 0.25.

To summarize, these results suggest that integration plans improved parents' labor market outcomes. They may have also exposed parents to more native colleagues and pushed their children 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 the effects on peer quality.

## 5 Conclusions

This paper investigated whether policies aimed at improving immigrants' labor market integration generate spillover effects on their children. Our findings indicate that they do. Utilizing a discontinuity created by the phase-in rules of a reform that introduced integration plans in Finland, we find that children of parents who arrived just after the threshold date—and thus received an integration plan—graduated with better grades, attained more education, and earned more compared to the offspring of similar immigrants who narrowly missed the threshold. These children were, on average, 11 years old upon arrival, and we follow them until age 31.

Our estimates suggest that the effects were substantial enough to elevate the earnings of the compliers' children to the level of other immigrants' offspring and to close a substantial part of the immigrant-native gap. Daughters benefited more than sons, while we observe no variation in treatment effects across different parental countries of origin. Our analysis of potential underlying mechanisms suggests that these effects may be partially attributed to improved parental labor market outcomes. Moreover, the integration plans likely improved parents' language skills, which could have directly contributed to their children's outcomes.

These findings yield two broader lessons. First, they demonstrate that interventions targeting adult immigrants can have positive intergenerational effects, which should be taken into account

when evaluating the cost-efficiency of integration policies. Second, and more speculatively, our results align with the hypothesis that parents’—particularly mothers’—exposure to the host country’s labor markets may assist their children in making more informed educational choices and pursuing more successful careers. However, we emphasize that this hypothesis remains tentative, and we cannot provide strong direct evidence to support or to refute it. Therefore, future research more directly examining these channels would be highly valuable.

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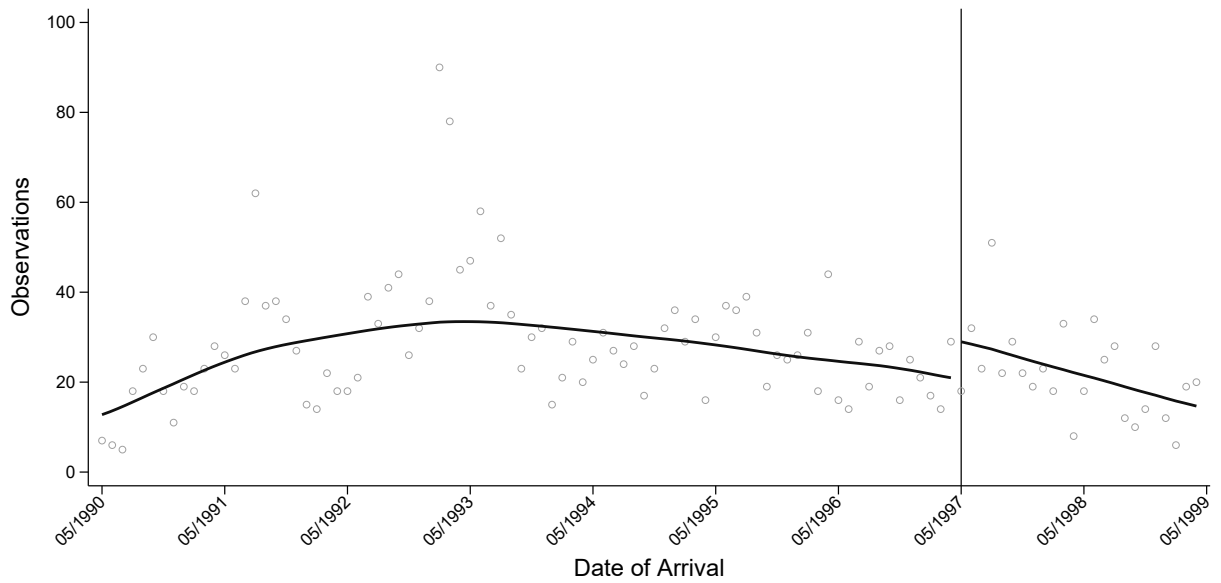
Online Appendix to  
**Intergenerational Spillovers of Integration Policies:  
Evidence from Finland's Integration Plans**

Hanna Pesola

Matti Sarvimäki

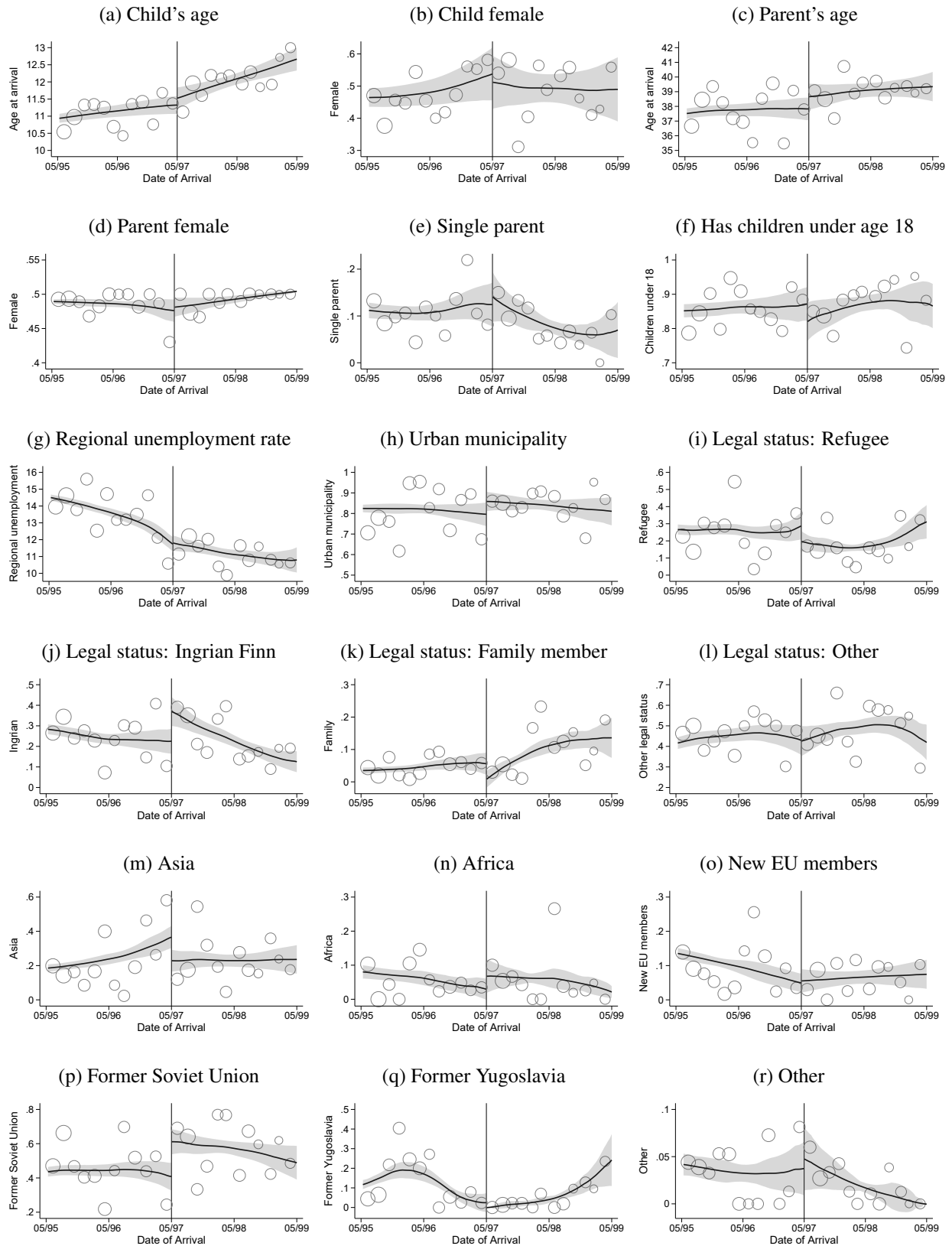
August 6, 2024

Figure A1: Observations by month of arrival



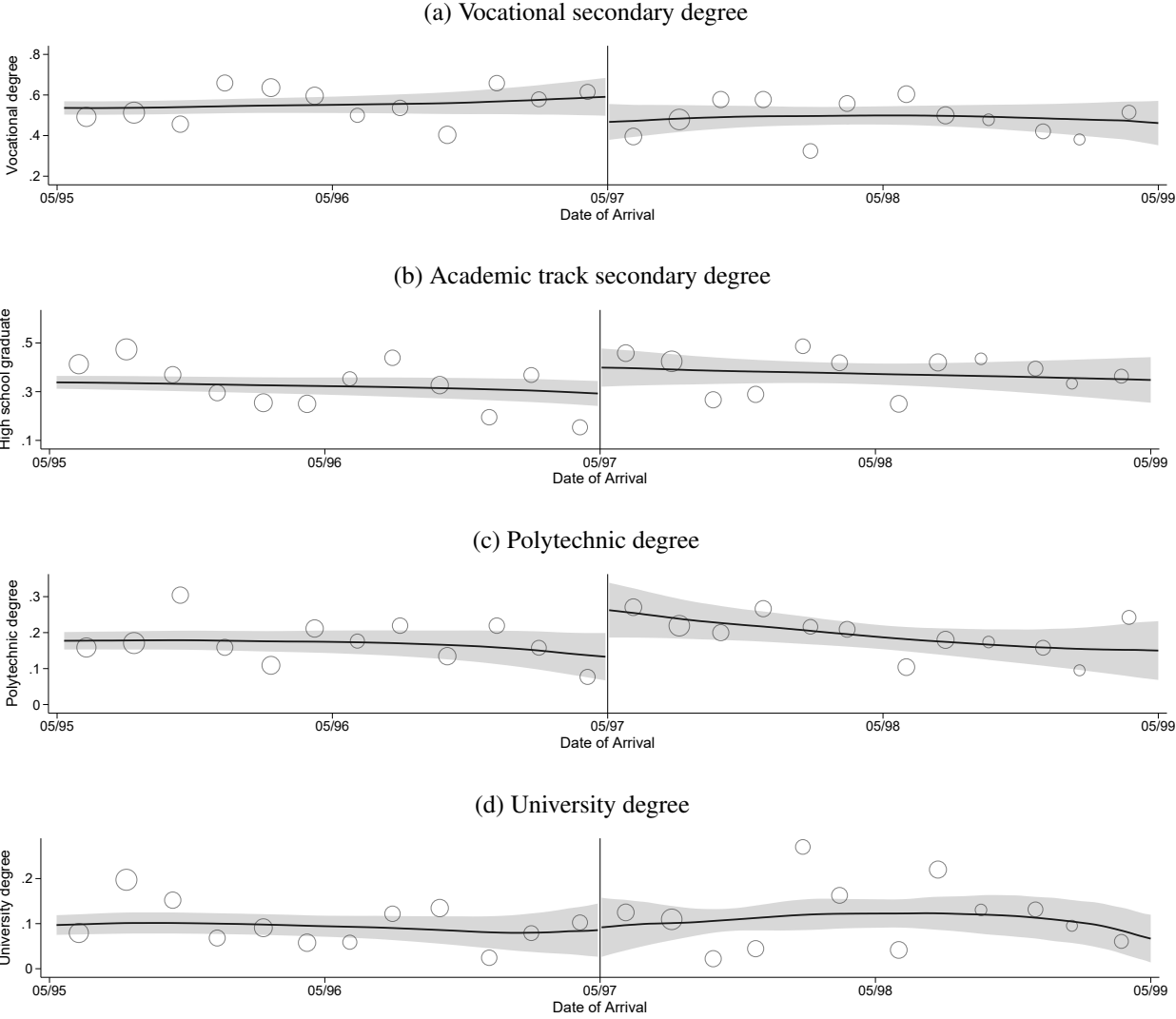
*Notes.* The figure shows observations by month of arrival of the father. The lines represent local linear estimates using the edge kernel and the optimal bandwidth selection algorithm of [Imbens and Kalyanaraman \(2012\)](#). The dots correspond to the number of observations entering the population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

Figure A2: Characteristics of children and parents at time of arrival



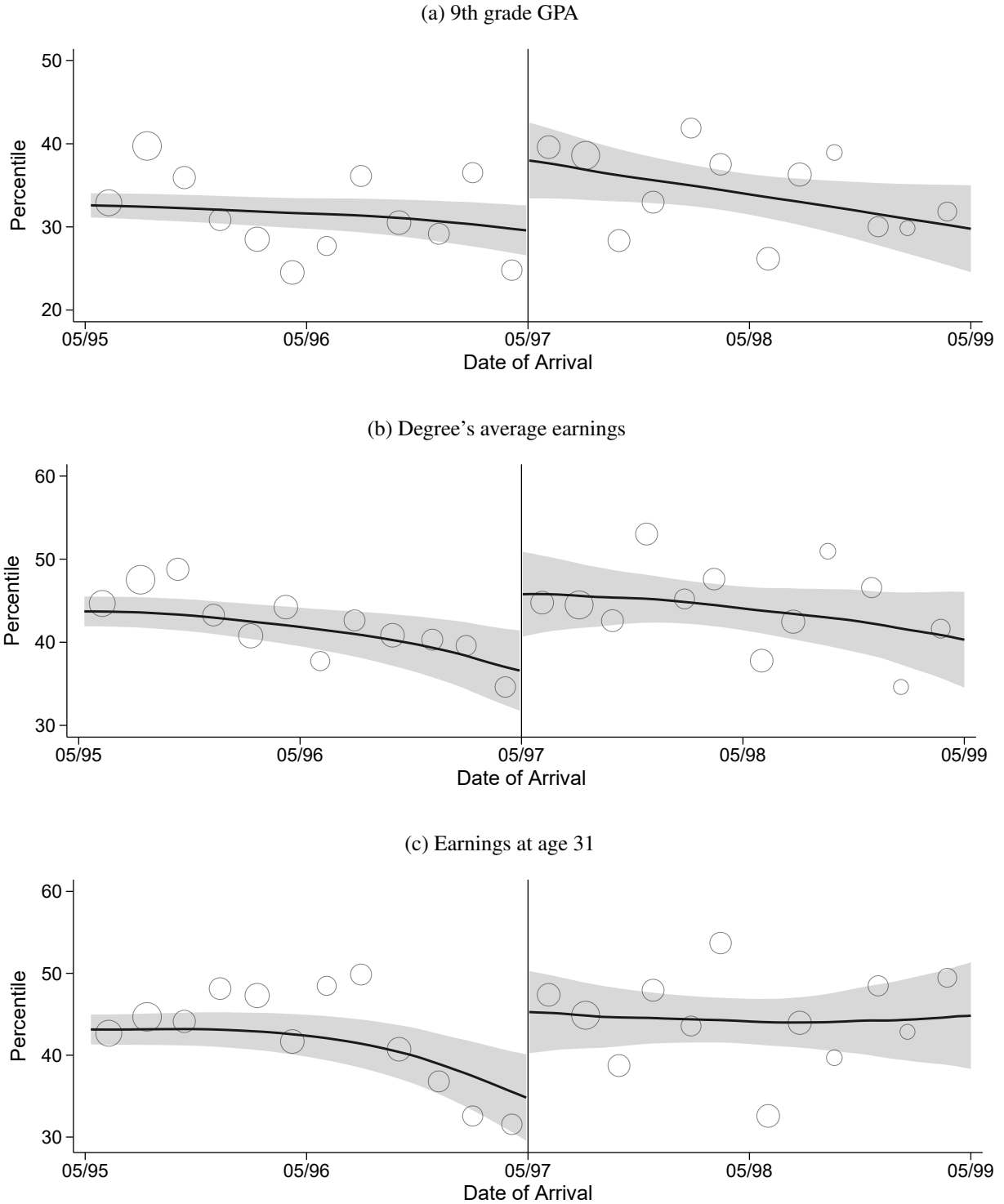
*Notes.* This figure shows date of arrival of the father (horizontal axis) and the background characteristics of children and parents (vertical axis). The lines represent local linear estimates using the edge kernel and the optimal bandwidth selection algorithm of [Imbens and Kalyanaraman \(2012\)](#). The dots correspond to sample means in two month bins.

Figure A3: Probability of attaining a secondary or tertiary degree



*Notes.* This figure shows date of arrival of the father (horizontal axis) and the shares of children obtaining a vocational secondary degree (panel a), an academic track secondary degree (panel b), a post-secondary degree from a polytechnic institution (panel c) and a university degree (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.

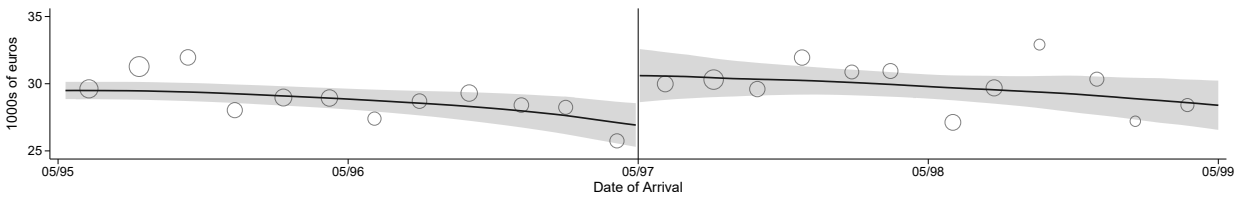
Figure A4: GPA, degree and earnings percentiles



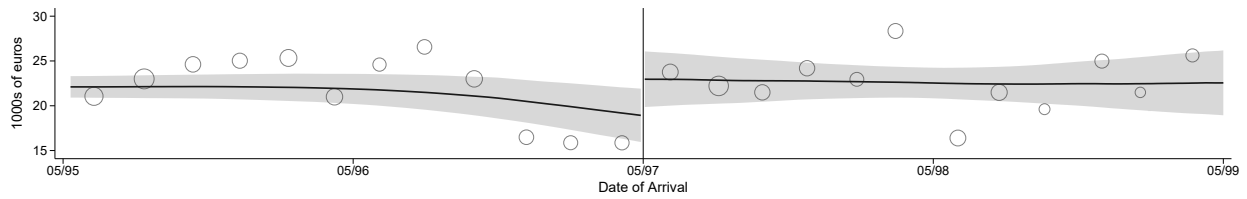
*Notes.* This figure shows date of arrival of the father (horizontal axis) and our main outcomes in percentiles instead of levels: 9th grade GPA (panel a), earlier graduates' average earnings of the child's highest degree or enrollment (panel b), and earnings (panel c). Earnings and degree are measured at age 31; GPA is measured at age 16. We do not report the percentiles for the (imputed) years of education distribution, because this outcome takes only four values. 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.

Figure A5: Alternative definitions of the outcome variables

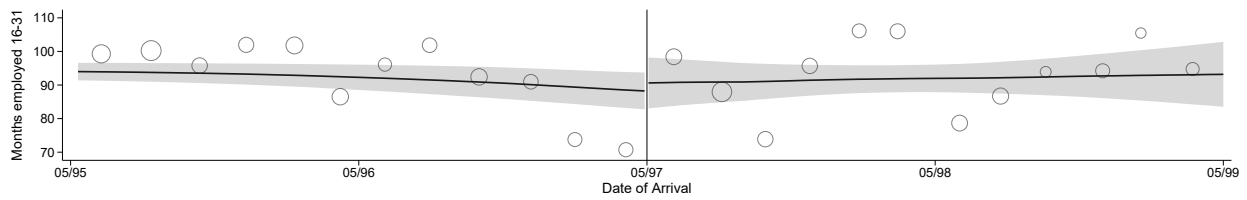
(a) Degree's average earnings at age 35



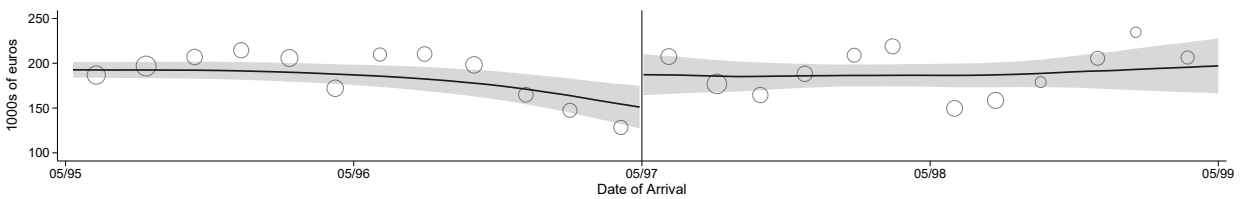
(b) Earnings at 31 (unwinsorized)



(c) Cumulative months employed ages 16 to 31

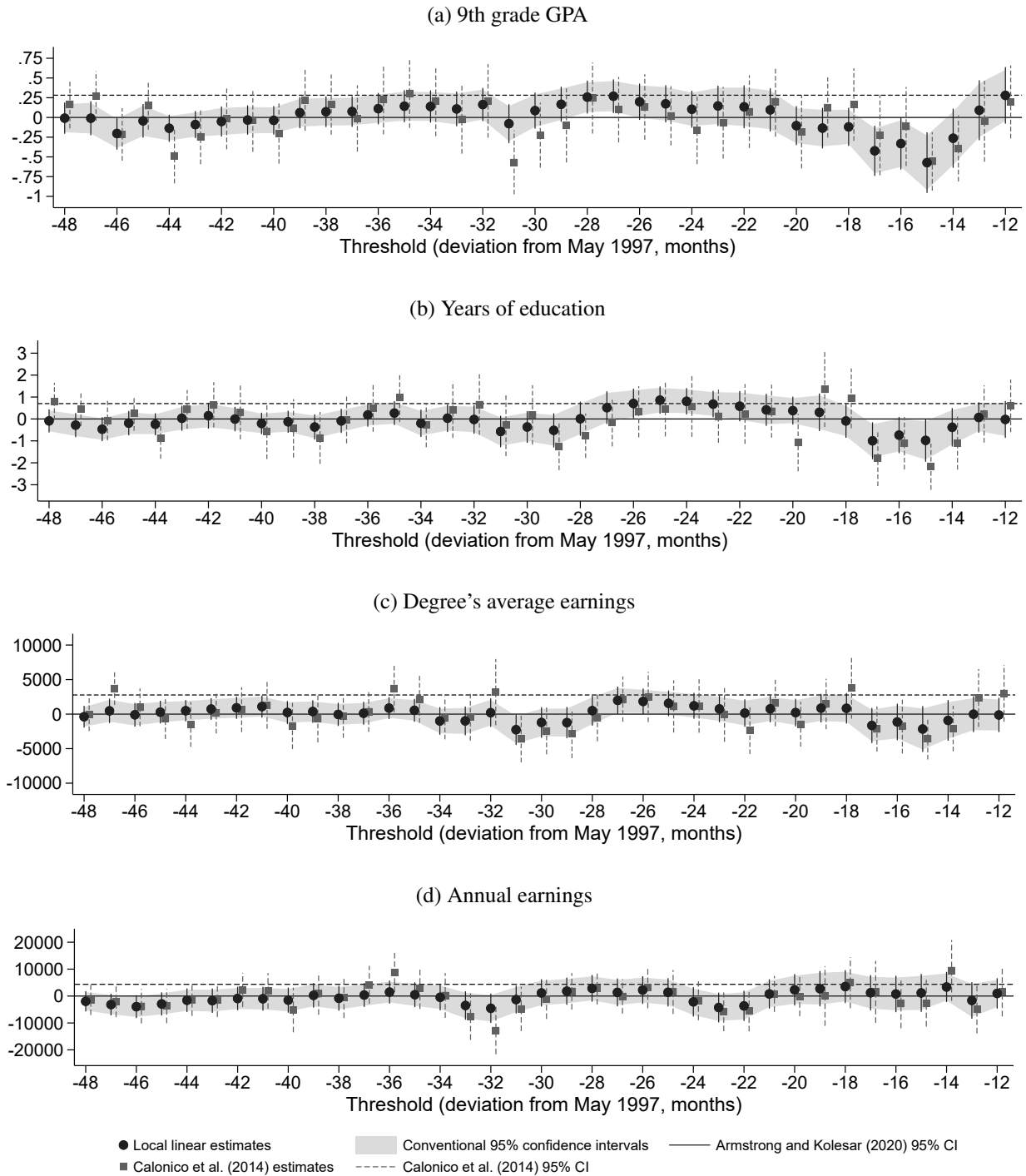


(d) Cumulative earnings ages 16-31



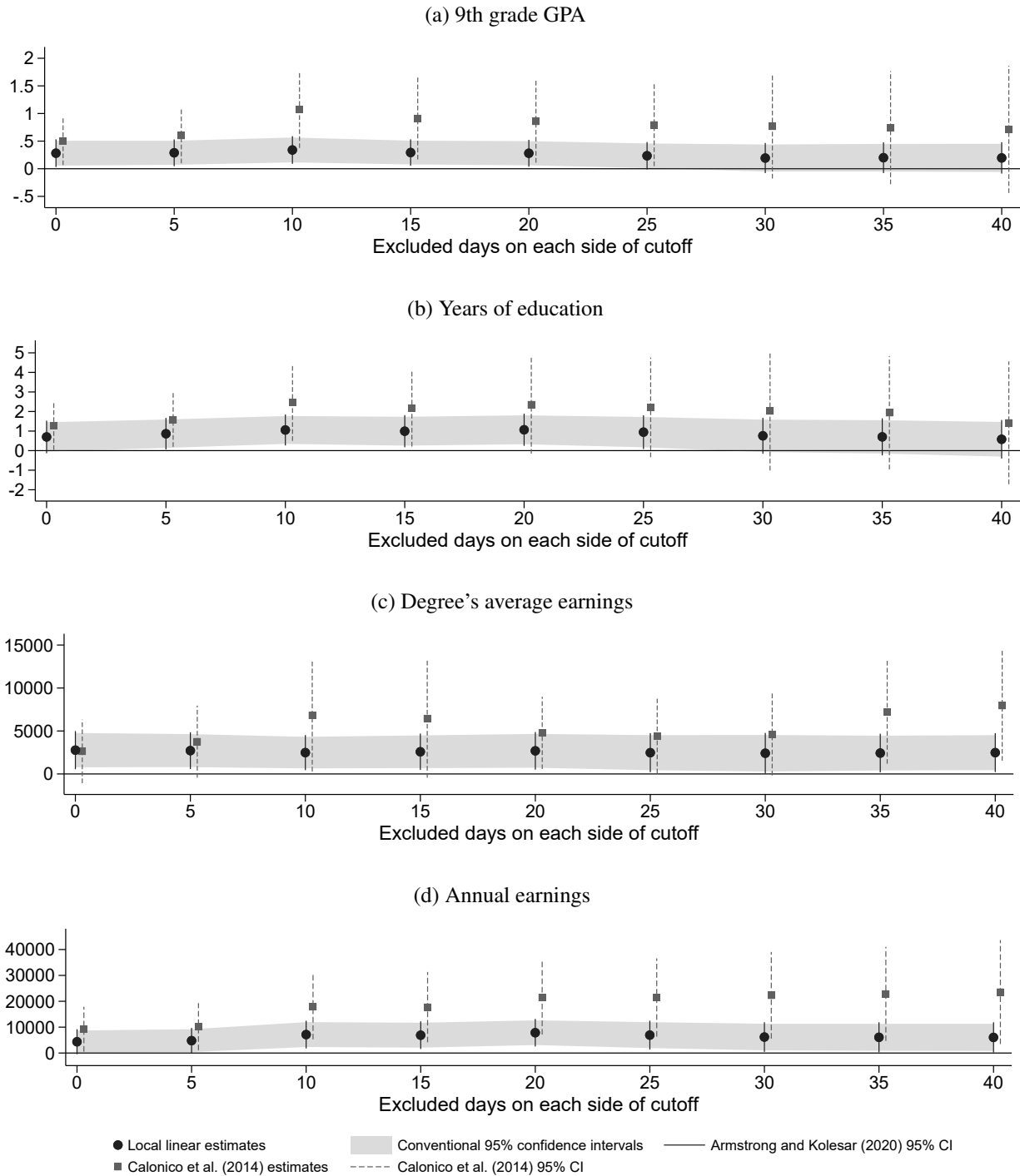
*Notes.* This figure shows date of arrival of the father (horizontal axis) and the degree's average earnings at age 35 (panel a), earnings at age 31 (panel b, unwinsorized), cumulative months employed between ages 16 and 31 (panel c), and cumulative earnings between ages 16 and 31 (panel d). The lines 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.

Figure A6: Alternative thresholds



*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 black circles represent local linear reduced form estimates, the shaded area depicts the corresponding conventional 95% confidence intervals and the black spikes the “honest confidence intervals” of [Armstrong and Kolesár \(2020\)](#). The grey 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 A7: Excluding observations around the cutoff



*Notes.* This figure reports estimates from “donut hole” specifications, where we leave out observations close to the threshold. The horizontal axis shows the number of days excluded around the cutoff of May 1st, 1997. The black circles show local linear reduced form estimates, the shaded area depicts the corresponding conventional 95% confidence intervals and the black spikes the “honest confidence intervals” of [Armstrong and Kolesár \(2020\)](#). The grey 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 probability of secondary or tertiary degree

	Vocational secondary degree		Academic track secondary degree		Polytechnic degree		University degree	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<b>A: Estimates</b>								
Reduced form	-0.12 (0.07)	-0.14 (0.07)	0.11 (0.05)	0.07 (0.05)	0.13 (0.05)	0.13 (0.05)	0.01 (0.04)	0.00 (0.04)
First-stage	0.53 (0.06)	0.57 (0.05)	0.65 (0.04)	0.67 (0.04)	0.54 (0.05)	0.58 (0.05)	0.49 (0.06)	0.54 (0.05)
LATE	-0.23 (0.13)	-0.24 (0.12)	0.17 (0.08)	0.11 (0.07)	0.24 (0.10)	0.23 (0.09)	0.01 (0.09)	0.00 (0.08)
<b>B: Benchmarks</b>								
Compliers' $Y(0)$	0.64 (0.09)	0.63 (0.08)	0.25 (0.05)	0.30 (0.05)	0.03 (0.07)	0.04 (0.06)	0.03 (0.07)	0.04 (0.06)
Never-takers' $Y(0)$		0.48		0.39		0.28		0.14
Native's $Y(0)$		0.53		0.53		0.24		0.19
Additional covariates	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth (months)		23		53		24		20
Observations		1,056		2,066		1,102		871

*Notes.* This table reports local linear estimates for the discontinuity at the May 1, 1997 cutoff of father's arrival time in the child's probability of obtaining a secondary or tertiary degree by age 31: vocational secondary degree (columns 1-2), academic track secondary degree (columns 3-4, enables applying to university), degree from a polytechnic post-secondary institution (columns 5-6) and degree from a university (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-urban, 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 \(2012\)](#).

Table A2: Impact of parent’s integration plan on GPA, educational attainment and earnings percentiles

	9th grade GPA		Degree’s average earnings		Annual earnings at age 31	
	(1)	(2)	(3)	(4)	(5)	(6)
<b>A: Estimates</b>						
Reduced form	8.44 (2.85)	5.88 (2.69)	9.25 (3.61)	8.15 (3.57)	10.54 (3.80)	10.50 (3.73)
First-stage	0.63 (0.04)	0.65 (0.04)	0.56 (0.05)	0.59 (0.05)	0.56 (0.05)	0.59 (0.05)
Local average treatment effect (LATE)	13.46 (4.76)	9.01 (4.18)	16.65 (6.80)	13.80 (6.11)	18.82 (6.91)	17.64 (6.27)
<b>B: Benchmarks</b>						
Compliers’ expectation in the absence of the treatment	24.82 (2.98)	28.23 (2.75)	30.21 (4.58)	31.84 (4.28)	29.62 (4.75)	28.94 (4.51)
Never-takers’ average	38.55		48.17		43.28	
Native’s average	50.35		51.53		51.67	
Additional covariates	No	Yes	No	Yes	No	Yes
Bandwidth (months)	46		26		27	
Observations	1,688		1,148		1,172	

*Notes.* This table presents our main results when outcomes are in percentiles. That is, we report local linear estimates for the jump at the May 1, 1997 cutoff of father’s arrival time on the percentile of 9th grade GPA (columns 1–2), average earnings at age 31 of earlier graduates with the same degree (columns 3-4) and actual average earnings at age 31 (columns 5-6). We do not report the percentiles for the (imputed) years of education distribution, because this outcome takes only four values. See notes to Table 2 in the paper for further details.

Table A3: Impact of parent's integration plan on alternative measures of education and labor market outcomes in young adulthood

	Degree's average earnings at 35		Earnings at 31 (unwinsorized)		Months emp. ages 16-31		Cumulative earnings ages 16-31	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<b>A: Estimates</b>								
Reduced form	3,699 (1,301)	3,402 (1,293)	4,074 (2,263)	4,282 (2,254)	2.41 (4.90)	-0.36 (4.59)	36,630 (17,410)	27,150 (16,328)
First-stage	0.56 (0.05)	0.60 (0.05)	0.60 (0.05)	0.63 (0.04)	0.66 (0.04)	0.68 (0.04)	0.55 (0.05)	0.59 (0.05)
LATE	6,534 (2,425)	5,671 (2,193)	6,826 (3,808)	6,844 (3,585)	3.67 (7.49)	-0.53 (6.76)	66,801 (32,771)	46,379 (27,888)
<b>B: Benchmarks</b>								
Compliers' av. without treatment	24,924 (1,505)	25,545 (1,399)	18,075 (2,620)	17,709 (2,505)	82.81 (4.92)	85.68 (4.69)	129,114 (22,233)	133,158 (20,123)
Never-takers' av. Native's average	31,762 32,453		22,498 27,662		98.84 102.8		211,280 229,256	
Additional covariates	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth (months)	27		35		57		25	
Observations	1,179		1,392		2,200		1,115	

*Notes.* This table reports local linear estimates for the discontinuity at the May 1, 1997 cutoff of father's arrival time in alternative education and labor market outcomes: earlier graduates' average earnings (at age 35) of the child's highest degree or enrollment (at age 31) (columns 1-2), unwinsorized earnings at age 31 (columns 3-4), months employed between ages 16 to 31 (columns 5-6) and cumulative labor earnings between ages 16 to 31 (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-urban, 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 \(2012\)](#).

Table A4: Impact of parent’s integration plan on children’s outcomes using first parent’s arrival time

	9th grade GPA		Years of education		Degree’s average earnings		Annual earnings at age 31	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>A: Estimates</i>								
Reduced form	0.27 (0.12)	0.21 (0.12)	0.70 (0.40)	0.65 (0.41)	2,697 (1,025)	2,591 (1,022)	4,087 (2,239)	4,150 (2,223)
First-stage	0.58 (0.05)	0.63 (0.04)	0.50 (0.06)	0.56 (0.06)	0.60 (0.05)	0.64 (0.04)	0.59 (0.05)	0.63 (0.04)
Local average treatment effect (LATE)	0.46 (0.22)	0.34 (0.19)	1.40 (0.83)	1.16 (0.73)	4,520 (1,775)	4,072 (1,624)	6,871 (3,787)	6,539 (3,483)
<i>B: Benchmarks</i>								
Compliers’ expectation in the absence of the treatment	-0.92 (0.14)	-0.83 (0.13)	11.17 (0.60)	11.32 (0.56)	23,257 (1,116)	23,665 (1,051)	17,349 (2,502)	17,216 (2,389)
Never-takers’ average	-0.37		13.24		28,086		23,479	
Native’s average	0.00		13.7		28,248		27,382	
Additional covariates	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth (months)	30		20		34		33	
Observations	1,221		850		1,314		1,340	

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

Table A5: Impact of parent’s integration plan on children’s outcomes using second parent’s arrival time

	9th grade GPA		Years of education		Degree’s average earnings		Annual earnings at age 31	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>A: Estimates</i>								
Reduced form	0.24 (0.11)	0.13 (0.11)	0.86 (0.35)	0.63 (0.34)	1,869 (969)	1,528 (948)	4,555 (2,213)	5,114 (2,162)
First-stage	0.65 (0.04)	0.68 (0.04)	0.60 (0.05)	0.64 (0.04)	0.65 (0.04)	0.68 (0.04)	0.64 (0.05)	0.67 (0.04)
Local average treatment effect (LATE)	0.37 (0.18)	0.19 (0.16)	1.44 (0.60)	0.98 (0.53)	2,867 (1,504)	2,249 (1,392)	7,170 (3,508)	7,658 (3,223)
<i>B: Benchmarks</i>								
Compliers’ expectation in the absence of the treatment	-0.94 (0.12)	-0.81 (0.11)	11.60 (0.45)	11.83 (0.40)	24,611 (984)	25,013 (918)	18,128 (2,512)	17,556 (2,320)
Never-takers’ average	-0.46		12.90		26,795		24,146	
Native’s average	0.00		13.7		28,248		27,382	
Additional covariates	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth (months)	35		24		37		31	
Observations	1,472		1,148		1,498		1,327	

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

Table A6: Impact of parent’s integration plan on children’s outcomes using mother’s arrival time

	9th grade GPA		Years of education		Degree’s average earnings		Annual earnings at age 31	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>A: Estimates</i>								
Reduced form	0.17 (0.11)	0.06 (0.11)	0.87 (0.37)	0.61 (0.37)	1,843 (969)	1,513 (960)	4,165 (2,286)	4,717 (2,243)
First-stage	0.63 (0.04)	0.66 (0.04)	0.56 (0.06)	0.61 (0.05)	0.64 (0.04)	0.66 (0.04)	0.60 (0.05)	0.64 (0.04)
Local average treatment effect (LATE)	0.28 (0.19)	0.09 (0.17)	1.55 (0.69)	1.00 (0.60)	2,896 (1,534)	2,279 (1,441)	6,917 (3,828)	7,335 (3,477)
<i>B: Benchmarks</i>								
Compliers’ expectation in the absence of the treatment	-0.81 (0.12)	-0.67 (0.11)	11.64 (0.50)	11.92 (0.45)	25,167 (943)	25,608 (903)	18,256 (2,667)	17,676 (2,475)
Never-takers’ average	-0.39		13.1		27,095		25,182	
Native’s average	0.00		13.7		28,248		27,382	
Additional covariates	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth (months)	42		24		47		31	
Observations	1,502		1,020		1,740		1,246	

*Notes.* This table is identical to Table 2 in the paper except that we now use the date of arrival of the mother as the running variable, while our main analysis is based on the date of arrival of the father.

Table A7: Impact of parent’s integration plan on children’s outcomes with parents defined at age 15

	9th grade GPA		Years of education		Degree’s average earnings		Annual earnings at age 31	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>A: Estimates</i>								
Reduced form	0.31 (0.12)	0.20 (0.11)	0.56 (0.40)	0.40 (0.39)	2,392 (1,068)	1,959 (1,046)	3,414 (2,171)	3,495 (2,155)
First-stage	0.59 (0.05)	0.62 (0.04)	0.49 (0.06)	0.55 (0.06)	0.56 (0.05)	0.60 (0.05)	0.60 (0.05)	0.63 (0.04)
Local average treatment effect (LATE)	0.53 (0.21)	0.32 (0.18)	1.13 (0.84)	0.73 (0.72)	4,272 (1,979)	3,277 (1,759)	5,676 (3,624)	5,534 (3,393)
<i>B: Benchmarks</i>								
Compliers’ expectation in the absence of the treatment	-1.01 (0.13)	-0.87 (0.12)	11.27 (0.63)	11.46 (0.56)	22,839 (1,293)	23,458 (1,176)	18,432 (2,494)	18,038 (2,389)
Never-takers’ average	-0.39		13.0		27,383		21,217	
Native’s average	0.00		13.7		28,248		27,382	
Additional covariates	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth (months)	33		19		27		37	
Observations	1,272		833		1,127		1,380	

*Notes.* This table is identical to Table 2 in the paper 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 A8: Impact of parent's integration plan by gender and parents' origin country

	9th grade		Years of		Degree's ave-		Annual earnings	
	GPA		education		rage earnings		at age 31	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>A: By gender</i>								
Local average treatment effect (LATE)	0.35	0.23	0.95	0.76	3,502	3,055	2,888	3,644
	(0.21)	(0.20)	(0.86)	(0.79)	(2,030)	(1,890)	(4,221)	(4,055)
× female	0.25	0.19	0.83	0.69	2,239	2,104	8,050	7,212
	(0.16)	(0.15)	(0.60)	(0.58)	(1,470)	(1,459)	(3,150)	(3,119)
Compliers' expectation in the absence of the treatment	-1.14	-1.04	10.98	11.05	24,888	25,165	24,038	22,947
	(0.14)	(0.13)	(0.61)	(0.59)	(1,295)	(1,231)	(2,824)	(2,733)
× female	0.33	0.01	0.60	0.17	-3,875	-3,691	1,727	1,510
	(0.10)	(0.05)	(0.43)	(0.20)	(963)	(976)	(901)	(944)
<i>B: By parent's origin country HDI</i>								
Local average treatment effect (LATE)	0.50	0.35	1.25	1.14	5,050	4,379	8,364	8,448
	(0.20)	(0.18)	(0.75)	(0.69)	(1,811)	(1,707)	(3,732)	(3,562)
× HDI	-0.05	-0.02	-0.03	0.19	385	618	877	1,530
	(0.08)	(0.08)	(0.30)	(0.28)	(629)	(639)	(1,479)	(1,471)
Compliers' expectation in the absence of the treatment	-0.99	-0.88	11.31	11.37	22,547	23,164	16,785	16,491
	(0.13)	(0.12)	(0.56)	(0.53)	(1,161)	(1,103)	(2,556)	(2,465)
× HDI	0.15	0.16	0.60	-0.51	866	-680	2,777	-1,752
	(0.05)	(0.05)	(0.21)	(0.22)	(493)	(614)	(1,050)	(1,381)
Additional covariates	No	Yes	No	Yes	No	Yes	No	Yes

*Notes.* The table shows local linear estimates for the jump in outcomes at the May 1, 1997 cutoff based on father's arrival time. Panel A shows treatment effect heterogeneity by gender and panel B by the parents' origin country Human Development Index. The outcomes are standardized 9th grade GPA (columns 1–2), years of education measured as the recommended completion time of the degree (columns 3–4), average earnings at age 31 of earlier graduates with the same degree (columns 5–6) and actual average earnings at age 31 (columns 7–8). 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-urban, 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 A9: Impact of parent's integration plan on parents' outcomes

	Earnings			Employment			Colleagues' native share			Colleagues' education			Overall						
	B (1)	M (2)	F (3)	B (4)	M (5)	F (6)	B (7)	M (8)	F (9)	B (10)	M (11)	F (12)	B (13)	M (14)	F (15)				
<i>A: Estimates</i>																			
Reduced form	8,528 (3,367)	3,582 (1,414)	4,427 (2,517)	0.03 (0.03)	0.08 (0.05)	0.03 (0.05)	0.07 (0.04)	0.04 (0.04)	0.04 (0.04)	0.04 (0.04)	0.04 (0.04)	0.04 (0.04)	-34 (574)	254 (626)	44 (859)	0.22 (0.10)	0.14 (0.08)	0.18 (0.11)	
First-stage	0.54 (0.05)	0.63 (0.04)	0.50 (0.06)	0.64 (0.04)	0.57 (0.05)	0.56 (0.05)	0.58 (0.05)	0.57 (0.05)	0.65 (0.04)	0.65 (0.04)	0.65 (0.04)	0.57 (0.05)	0.60 (0.05)	0.63 (0.04)	0.57 (0.05)	0.54 (0.05)	0.65 (0.04)	0.53 (0.06)	
LATE	15,718 (6,595)	5,784 (2,337)	8,721 (5,190)	0.05 (0.05)	0.15 (0.09)	0.04 (0.08)	0.12 (0.06)	0.06 (0.07)	0.07 (0.06)	0.07 (0.06)	0.07 (0.06)	0.07 (0.06)	-54 (921)	374 (920)	73 (1,414)	0.41 (0.19)	0.22 (0.12)	0.33 (0.21)	
<i>B: Benchmarks</i>																			
Compliers' $Y(0)$	18,746 (3,635)	9,572 (1,281)	10,811 (2,879)	0.42 (0.03)	0.41 (0.06)	0.43 (0.06)	0.79 (0.04)	0.83 (0.05)	0.81 (0.04)	0.81 (0.04)	0.81 (0.04)	0.81 (0.04)	24,597 (690)	23,535 (647)	25,704 (1,001)	-0.28 (0.13)	-0.21 (0.08)	-0.28 (0.13)	
Never-takers' $Y(0)$	27,527	12,850	14,878	0.49	0.50	0.49	0.84	0.87	0.81	0.81	0.81	0.81	23,665	23,561	24,381	-0.03	-0.10	-0.08	
Native's $Y(0)$	50,434	23,195	29,445	0.72	0.70	0.75	0.98	0.98	0.99	0.99	0.99	0.99	25,175	24,133	26,351	0.62	0.53	0.56	
Additional covariates	No	No	No	No	No	No	No	No	No	No	No	No	No	No	No	No	No	No	No
Bandwidth (months)	24	47	20	51	28	27	31	28	55	28	55	28	36	47	28	25	53	23	23
Observations	1,103	1,724	908	2,037	1,184	1,160	957	724	1,241	724	1,241	733	1,062	1,016	733	1,113	2,045	1,056	1,056

*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 parents' 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 Kalyanaram \(2012\)](#).

Table A10: 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)
<i>A: Estimates</i>															
Reduced form	4,836 (2,699)	2,116 (1,244)	1,492 (2,095)	-0.01 (0.03)	0.03 (0.04)	-0.03 (0.04)	0.07 (0.03)	0.04 (0.04)	0.05 (0.03)	-345 (556)	-22 (651)	-384 (800)	0.08 (0.07)	0.05 (0.06)	0.03 (0.08)
First-stage	0.58 (0.05)	0.66 (0.04)	0.55 (0.05)	0.67 (0.04)	0.60 (0.04)	0.59 (0.05)	0.61 (0.04)	0.60 (0.04)	0.67 (0.04)	0.63 (0.04)	0.65 (0.04)	0.60 (0.04)	0.58 (0.05)	0.67 (0.04)	0.57 (0.05)
LATE	8,328 (4,632)	3,285 (1,919)	2,683 (3,730)	-0.02 (0.04)	0.04 (0.07)	-0.05 (0.06)	0.11 (0.05)	0.06 (0.06)	0.08 (0.05)	-532 (850)	-31 (910)	-607 (1,248)	0.13 (0.12)	0.07 (0.10)	0.05 (0.14)
<i>B: Benchmarks</i>															
Compliers' $Y(0)$	22,313 (3,066)	11,067 (1,229)	13,987 (2,440)	0.47 (0.03)	0.45 (0.06)	0.48 (0.05)	0.80 (0.04)	0.82 (0.05)	0.81 (0.03)	25,080 (666)	23,953 (665)	26,227 (939)	-0.14 (0.10)	-0.11 (0.07)	-0.13 (0.11)
Never-takers' $Y(0)$	27,527	12,850	14,878	0.49	0.50	0.49	0.84	0.87	0.81	23,665	23,561	24,381	-0.03	-0.10	-0.08
Native's $Y(0)$	50,434	23,195	29,445	0.72	0.70	0.75	0.98	0.98	0.99	25,175	24,133	26,351	0.62	0.53	0.56
Additional covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth (months)	24	47	20	51	28	27	31	28	55	36	47	28	25	53	23
Observations	1,103	1,724	908	2,037	1,184	1,160	957	724	1,241	1,062	1,016	733	1,113	2,045	1,056

*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 parents' 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-urban, 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 A11: 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)
<i>A: Estimates</i>								
Reduced form	642 (471)	619 (483)	0.01 (0.01)	0.02 (0.01)	607 (641)	842 (650)	0.13 (0.12)	0.18 (0.12)
First-stage	0.56 (0.05)	0.59 (0.05)	0.54 (0.06)	0.58 (0.05)	0.61 (0.04)	0.64 (0.04)	0.56 (0.05)	0.59 (0.05)
LATE	1,138 (844)	1,036 (809)	0.02 (0.03)	0.04 (0.02)	1,010 (1,062)	1,340 (1,027)	0.24 (0.21)	0.30 (0.20)
<i>B: Benchmarks</i>								
Compliers' $Y(0)$	29,982 (573)	30,098 (576)	0.86 (0.02)	0.85 (0.02)	48,110 (645)	47,992 (653)	-0.45 (0.15)	-0.45 (0.14)
Never-takers' $Y(0)$	31,411		0.93		49,239		0.04	
Native's $Y(0)$	32,161		0.98		48,325		0.29	
Additional covariates	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth (months)	26	26	24	24	40	40	27	27
Observations	1,134	1,134	1,021	1,021	1,409	1,409	1,164	1,164

*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-urban, 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 \(2012\)](#).

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