Centre for Research and Analysis of Migration

CReAM

Discussion Paper Series

Assimilating Immigrants
The Impact of an Integration Program

Matti Sarvimäki and Kari Hämäläinen

Assimilating Immigrants
The Impact of an Integration Program

Matti Sarvimäki* and Kari Hämäläinen†

* Aalto University School of Economics, Government Institute for Economic Research and London School of Economics
† Government Institute for Economic Research

Non-Technical Abstract

Immigration policy design is an important and controversial topic in most developed countries. We inform this debate by evaluating the effects of an integration program for immigrants to Finland. The program consists of an individualized sequence of training and subsidized employment. Non-compliance is sanctioned by reductions in welfare benefits. Our empirical strategy exploits a discontinuity that made participation obligatory in May 1999 only for those who had entered the population register after May 1997. The results suggest that the program strongly increased the employment and earnings of immigrants and reduced their dependency on social benefits.

Assimilating Immigrants

The Impact of an Integration Program[*]

Matti Sarvimäki   Kari Hämäläinen

August 2010

Abstract

Immigration policy design is an important and controversial topic in most developed countries. We inform this debate by evaluating the effects of an integration program for immigrants to Finland. The program consists of an individualized sequence of training and subsidized employment. Non-compliance is sanctioned by reductions in welfare benefits. Our empirical strategy exploits a discontinuity that made participation obligatory in May 1999 only for those who had entered the population register after May 1997. The results suggest that the program strongly increased the employment and earnings of immigrants and reduced their dependency on social benefits.

---

1 Introduction

Immigrants perform worse in the labor market and collect more social benefits than comparable natives. This empirical fact has made immigration a central theme in many recent elections and pushed governments to reform their immigration policies. Salient reforms include improved border controls, changes to visa systems, stricter eligibility rules for public benefits and the setting up of mandatory integration programs. The reforms have often provoked considerable controversy. Yet, we know little about their impacts.

In this paper, we examine the effects of "integration plans" introduced in Finland in the late 1990s. This program shares key features with integration measures also implemented in other European countries and in North America.[1] Most importantly, the integration plans consist of an individualized sequence of training and subsidized employment, and non-compliance is sanctioned by reductions in welfare benefits.

We focus on Finland due to the quality of the data and the research design. Our longitudinal dataset is created by linking several administrative registers at individual and family level. The research design is based on the phase-in rules of a policy reform. The program was launched on May 1st, 1999, but only those who had entered the population register after May 1st, 1997 had an obligation to participate. This discontinuity allows us to identify the causal effect of the program under the assumption that immigrants entering the population register just before and after the threshold date are comparable. This identifying assumption seems plausible as the threshold date was set more than a year after the affected immigrants had made their entry decisions. Furthermore, our approach survives a battery of robustness checks and falsification exercises.

We find that the integration plans improved the labor market performance of immigrants and reduced their welfare dependency. The point es-

---

[1]The Finnish program closely resembles the Immigration Settlement and Adaption Program (ISAP) in Canada (CIC, 2005). In Europe, comparable programs are present in Austria, Belgium, Denmark, France, Germany, the Netherlands, Portugal, Spain and Sweden (Carrera, 2006; Joppke, 2007). Integration programs are less common in the United States, but some training is provided by state and local governments and by non-governmental organizations (Schmidt, 2007).

timates suggest that the effects were large. To interpret these results, we examine the size and characteristics of the "compliers" (those who were induced to particiapte in the program due to the date rule) and the impact of the reform on training and incentives. We find that roughly a third of all immigrants were compliers and that the most disadvantaged groups were disproportionally presented. A review of the pre- and post-reform legislation suggests that the reform did not change the sanctioning of non-compliance. Instead, the main change was in allowing immigrants to retain their unemployment benefits while participating in training provided outside the Labor Administration. These courses typically focus on language training. Furthermore, we also document a rise in the provision of courses specifically designed for immigrants within the Labor Administration. Thus the reform appears to have worked primarily through the building up of host-country-specific human capital.

These findings add to the vast literature on the assimilation of immigrants. Previous work has shown that immigrants experience rapid earnings and employment growth over time in the host country.[2] However, only a handful of studies have examined whether government policies can help in this process. Åslund and Johansson (2006) document a positive association between the introduction of "supported employment methods" in Swedish municipalities and improvements in immigrant employment in these locations. Rosholm and Vejlin (2007) show that lowering public income transfers to newly admitted refugees to Denmark had a small positive effect on their job finding rate. Cohen-Goldner and Eckstein (2008, 2010) conclude that training programs substantially increased job-offer rates and had a small positive effect on wages among immigrants from the former Soviet Union to Israel.[3]

---

[2]Studies documenting immigrants' labor market performance and use of social benefits in the United States include, but are not limited to, Chiswick (1978), Borjas (1985), Borjas and Trejo (1990), Borjas (1995), Borjas and Hilton (1996), Hu (2000), Lubotsky (2007). Sarvimäki (forthcoming) examines the economic performance of immigrants to Finland. See Borjas (1994) and Boeri et al. (2002) for surveys.

[3]Some studies have also examined the impact of other policies aimed at reducing immigrants' dependency on public benefits. For instance, Borjas (1993) and Antecol et al. (2003) discuss the effectiveness of point system policies, and Borjas (2002) examines the

In comparison to the previous studies, our research design allows for a causal interpretation under weak assumptions. In particular, we complement the structural estimates by Cohen-Goldner and Eckstein (2008, 2010), which suggest that returns to local human capital are very high. Furthermore, to the best of our knowledge, we are the first to examine an explicit integration program. Thus our results directly inform a policy debate that remains active in many countries. In addition, these findings may be helpful for countries that do not currently invest in integration programs, but might benefit from doing so.

The rest of this paper is organized as follows. The next section provides background information on immigrants to Finland and details on the reform. We discuss our empirical strategy in Section 3 and present the data in Section 4. Section 5 reports the results and robustness checks and discusses the interpretation of the estimates. Section 6 concludes.

2   Background

2.1   Immigration to Finland

For most of its history, Finland has been characterized by emigration. Consequently, immigrants have primarily been return migrants and their family members. Genuine immigration only began in the early 1990s, after which the immigrant population has grown fivefold. Given the low initial level, however, their share of the population is still relatively low, being roughly three per cent in 2009.

As in other Western countries, increasing immigration was accompanied by a change in the composition of origin countries. In 1990, almost half of the immigrants came from Western Europe. Today, the bulk of immigrants come from the former Soviet Union and Asia. The trend of a declining proportion of Western Europeans coincides with the experience of most other OECD countries. However, the share of immigrants from the former Soviet Union is unusually high in Finland. In addition, refugees—primarily from Iran, Iraq,

_____

impact of the 1996 U.S. welfare reform on immigrant households.

4

Somalia and former Yugoslavia—make up roughly a sixth of the immigrant population.

While statistics on the reasons for immigration are incomplete, it is widely agreed that the proportion of economic migrants is low. This is likely to explain the poor economic performance of immigrants in Finland. Upon arrival, their employment rates are very low and hence they earn substantially less than comparable natives. While the gap decreases over time, only the earnings of men from the OECD countries have converged to the earnings of comparable natives within twenty years of arrival (Sarvimäki, forthcoming). As everyone living in Finland on a permanent basis is eligible for social benefits, low earnings lead to high average social benefits among immigrant households.[4]

2.2 The Reform

This paper examines the impacts of a program that was introduced as a part of the Act on the Integration of Immigrants and Reception of Asylum Seekers (henceforth the Integration Act). The Integration Act came in force in May 1st, 1999 with the aim of promoting integration, equality and freedom of choice by providing measures that help immigrants to acquire information and skills needed in Finnish society. In practice, it introduced two reforms. First, it set new rules for the division of responsibilities between the central and local administrations (municipalities) and required the latter to prepare municipality-level integration programs. The aim was to reallocate existing resources more efficiently, to train staff, and to improve co-operation between all local authorities involved in immigrant integration. This part of the Integration Act is likely to affect all immigrants and its impacts are therefore difficult to evaluate.

The second part of the reform only affected some arrival cohorts. The Integration Act introduced an obligation to draw-up individualized integration

---

[4]Eligibility for most Finnish social security is based on permanent residence. The main exceptions are earnings-related unemployment benefits and pensions. Furthermore, eligibility for a student allowance requires that a non-citizen has migrated to Finland for another purpose than to study.

plans for recently arrived non-working immigrants. These plans consist of a sequence of language courses, other preparatory and/or vocational training, career counseling, rehabilitation, work practice, and so forth. The aim is to closely consider the individual characteristics of each immigrant and to design a sequence of measures that is expected to best fit his or her needs.

Eligibility for an integration plan requires that the immigrant (a) is a registered unemployed job-seeker or lives in a household that receives social assistance and (b) has entered the population register within the past three years.[5] When these criteria are fulfilled, an integration plan has to be drawn-up within the first five months of a period of unemployment or social assistance. The integration plan is prepared in a joint meeting between an immigrant, a representative of a local employment office and, if necessary, an interpreter. During this meeting, a sequence of training and other measures is prepared and dates for monitoring visits are agreed. Particular care is taken to ensure that the immigrant fully understands the measures he or she is expected to participate in and knows how to gain access to them. The integration plan is aborted if the immigrant finds permanent, full-time employment or becomes a full-time student. (Ministry of Labour, 2003)

Eligibility is combined with an obligation to participate. Refusal to participate in the preparation process or failure to follow the plan is sanctioned by a reduction in social benefits. These sanctions would typically reduce the benefits by 20–40 percent from a baseline level of roughly 500 euros per month.

Importantly, the obligation to participate only applies to those who entered the population register after May 1st, 1997. Earlier cohorts have a right, but not an obligation, to demand an integration plan. As we discuss in more detail below, we will exploit this date rule to evaluate the impact

---

[5]Social assistance is the last resort of economic assistance in Finland. It is means-tested based on a household's assets, expenses and income. Immigrants have to register to the population register in order to be issued a personal identity code. This creates a strong incentive to register soon upon arrival as the code is required, among other things, for applying for benefits, for the payment of wages and for opening a bank account. Furthermore, immigrants who intend to stay in Finland for over a year are required to register.

of receiving an integration plan. Before turning to the empirical strategy, however, we review the available information on how the reform changed the assistance provided to immigrants.

2.3 Changes to Training and Incentives

Before the Integration Act, immigrants and natives were treated similarly in terms of services provided by local employment agencies and eligibility for social security (Government Proposal for the Integration Act, 1998). Thus training offered to immigrants competed with that offered to natives. To receive unemployment benefits, an immigrant had to register in an employment office as a job seeker regardless of his or her language skills. Employment agencies offered language courses as a part of labor market training, but the supply of the courses did not meet the demand. Only half of the immigrants received language courses soon after arrival. Furthermore, waiting periods between courses could expand to several years due to the lack of resources.

The Integration Act changed the allocation and the supply of labor market measures. Better information and planning were likely to allocate the existing supply of training more efficiently among immigrants. Furthermore, the Integration Act introduced the concept of "comparable labor market measures". These are courses, training, work coaching and the like offered outside the Labor Administration (e.g. in adult education centers and universities). Provided that the Labor Administration accepts the course, an immigrant maintains the eligibility for social benefits during the participation period. Previously, participation in such training had to be financed by student loans or student grants, which were not available for individual courses.[6] As a result, immigrant training outside the Labor Administration was virtually nonexistent prior to the reform. The number of immigrants participating in comparable measures took off rapidly after the introduction of the Integration Act.[7]

---

[6] We note that all unemployed persons who have worked for more than ten years in Finland have been allowed to educate themselves with the aid of the sum equivalent to unemployment benefits from 1998 onwards. However, the precondition of ten years of Finnish work experience ruled out virtually all unemployed immigrants in the late 1990s.

[7] The data on comparable training are scarce and cannot be linked to our individual-

In contrast, there was no change in the sanctioning of non-compliance. While the Integration Act made an explicit reference to sanctions, they were based on the existing legislation governing unemployment benefits and social assistance (Government Proposal for the Integration Act, 1998). Of course, we cannot rule out that the reform could have increased awareness of sanctions or monitoring. However, monitoring of the unemployed—regardless of their immigrant status—was already present before the Integration Act. Thus immigrants arriving shortly before and after May 1997 seem to have faced the same threat of sanctions.

3   Empirical Strategy

Our empirical strategy is based on the phase-in rule of the reform that only made participation obligatory for immigrants entering the population register after May 1st, 1997. This rule creates a research design that resembles the situation where immigrants had been randomized between treatment and control groups. More precisely, we are able to uncover the causal effect of the treatment, at least for a subpopulation of the immigrants, under two identifying assumptions. First, those entering the register just before and after May 1997 need to be comparable. Formally, potential outcomes given the date of entry are assumed to be continuous at the threshold. We argue that this is a plausible assumption given that immigrants arriving around May 1997 made their entry decisions two years before the Integration Act was introduced.[8] Hence, they were not able to self-select into the treatment or control group by choosing when to register. Furthermore, there were no other policy reforms that would have affected potential outcomes at the threshold. The second identifying assumption is local monotonicity. That

level data. Thus we cannot assess its role directly. However, the available sources suggest that it is very popular. According to Ministry of Labour (2005), for example, the number of participants in comparable training exceeds the number of participants in courses provided by the Labor Administration in the capital region of Helsinki (where the majority of immigrants are settled).

[8]The threshold date was published on May 8th, 1998 when the government introduced the bill to the parliament. Next day, the leading Finnish newspaper, *Helsingin Sanomat,* had a short article about the bill, but did not discuss this threshold date.

is, we need to assume that the probability of being treated did not decrease for anyone who entered the population register after May 1st, 1997 rather than before. It seems very unlikely that this assumption would be violated.

Given these assumptions, the causal effect of the treatment can be evaluated with the local Wald estimator

$$\beta = \frac{y^+ - y^-}{p^+ - p^-} \tag{1}$$

where $y^+ = \lim_{z \downarrow z_0} \mathbb{E}\left[y_i | z_i = z\right]$ is the limit of the outcome y in expectation when approaching the threshold z_0 from above and $y^- = \lim_{z \uparrow z_0} \mathbb{E}\left[y_i | z_i = z\right]$ is the limit from below (Hahn et al., 2001). In our application, the forcing variable z is the date of entering the population register and the threshold z_0 is May 1st, 1997. Similarly $p^+ = \lim_{z \downarrow z_0} \mathbb{E}\left[D_i | z_i = z\right]$ and $p^- = \lim_{z \uparrow z_0} \mathbb{E}\left[D_i | z_i = z\right]$ are the limits for the probability of being treated when approaching the threshold from above and below.

There are two widely used approaches to estimate equation (1): the local linear estimator discussed by Hahn et al. (2001) and the parametric approach adopted by van der Klaauw (2002). We employ the latter due to the relatively small sample size.[9]

Our baseline estimation equation is

$$y_i = \alpha + \beta \mathbb{E}\left[D_i | z_i, X_i\right] + X_i \theta + k\left(z_i\right) + u_i \tag{2}$$

where D_i is an indicator variable for receiving an integration plan, X_i is a vector of observed background characteristics, $k\left(z_i\right)$ is a function of the date of entering the population register and u_i summarizes unobserved factors affecting the outcome. The probability of being treated is modeled as

$$\mathbb{E}\left[D_i | z_i, X_i\right] = \gamma 1\left\{z_i > z_0\right\} + X_i \psi + g\left(z_i\right) \tag{3}$$

---

[9]We have also experimented with local linear estimates. However, to obtain sufficient statistical power to reveal even very large effects, the sample size requires us to use wide bandwidths (up to several years on both sides of the threshold date). Clearly, using such bandwidths cannot be considered as a truly nonparametric approach. In any case, the point estimates are stable across alternative bandwidths and similar to those reported in this paper.

The key idea is that the underlying dependence between the date of arrival and the outcome is controlled by the smooth term $k(z_i)$. In our context, this dependence follows from the assimilation process: the labor market performance of immigrants tends to improve as they spend more time in the host country. Failing to take this into account would lead to biased estimates. Similarly, as we discuss in more detail below, the likelihood of being treated was greater among later cohorts than among those entering the population register just after the threshold. This process is controlled for by $g(z_i)$. If $k(z_i)$ and $g(z_i)$ are smooth over the range of arrival dates, a discontinuous jump in $\mathbb{E}[D_i|z_i, X_i]$ allows for consistent estimation of the causal effect of the treatment for the subpopulation of "compliers".[10] On the other hand, conditioning for the background characteristics, X_i, is not required for consistency, but may improve precision.

In practice, the relatively small sample size forces us to use a parsimonious parameterization of $k(z_i)$ and $g(z_i)$, and this adds a third assumption to our identification strategy. Namely, we need to assume that the choice of the functional form is reasonable. Since the true form of these functions is unknown, we experiment with several alternative parameterizations. In all specifications, we use the same functional form for $k(z_i)$ and $g(z_i)$, which allows us to implement the estimation using a standard 2SLS procedure. Since we observe the forcing variable only at the monthly level, we cluster the standard errors at this level in order to adjust for the consequent group structure in the error term (Lee and Card, 2008).

4 Data

We use individual-level panel data, created by linking information from the population register, the tax register, the pension and benefit registers, the student register, the register of unemployed job-seekers and the register on social assistance. The data were created by drawing a 15 percent random

---

[10] We discuss the definition and characteristics of the complier subpopulation in detail in Section 5.3.

sample of the new immigrants arriving in each year between 1990 and 2003.[11] The data include annual observations for each individual until the end of the year 2003, death or emigration. The data sources were combined by Statistics Finland using personal identity codes.

For our baseline estimates, we restrict the estimation sample using the following criteria. First, we include only male immigrants who first arrived in Finland between January 1990 and April 1999. Second, we restrict the sample to 25- to 60-year-old immigrants who were at least 16 years old at the time of immigration. Third, we exclude those who were not potentially eligible and those in the top 0.1 percent of the earnings or social benefits distributions.[12]

Table 1 presents descriptive statistics for the resulting data. Columns (1) to (5) report average characteristics in the year of arrival for different arrival cohorts. First, consider columns (4) and (5), which refer to cohorts that arrived in Finland within two years from May 1997. The means suggest that these cohorts were similar.[13] When we extend the observation period, some trends become evident. Family unification became more common and the local unemployment rate varied as Finland went through a severe recession in the early 1990s. It seems safe to assume that none of these changes were caused by the anticipation of the immigration policy reform. Yet, immigrants arriving at different phases of business cycle could differ in their characteristics. These changes are likely to be relatively smooth and thus captured by the $k(z_i)$ and $g(z_i)$ functions discussed above. This assumption

---

[11]Statistics Finland restricted the sample size to 15 percent of the immigrant population in order to ensure that individuals cannot be identified from the data.

[12]We define potentially eligible as those who either became a registered job seeker, received unemployment compensation *or* received social assistance during their first three years in Finland. This excludes 1,428 immigrants (a third of the full sample). Dropping the top 0.1 percent of the earnings and benefit distributions excludes 24 immigrants. In Section 5.2, we show that this sample selection rule improves the precision of the estimates, but does not affect the conclusions of the analysis.

[13]We have also regressed background characteristics on a dummy for arriving after May 1997 and several alternative specifications for the month of entering the population register. The estimates tend to be statistically insignificant and the point estimates are sensitive to the chosen specification. Often, the sign of the estimates changes across specifications.

is supported by the fact that the key results are virtually identical with and without controlling for the observable characteristics. We return to columns (6) and (7) in Section 5.3.

5 Results

5.1 Main Results

Figure 1 plots the proportion of immigrants receiving an integration plan against the date of entering the population register. The circles correspond to the raw averages for two-month bins. On average, each circle represents 41 immigrants. The lines represent the fitted values from linear and quadratic OLS specifications corresponding to equation (3) without additional covariates.

Figure 1 reveals that those arriving in May 1997 were substantially more likely to receive an integration plan than those arriving in April 1997. The point estimate for the linear specification suggests a 41 percentage points jump (standard error of five percentage points) at the threshold. The figure also shows that the likelihood of receiving the treatment increased after the threshold date. This is likely to be due to immigrants becoming employed before May 1999 being ineligible.

Figure 2 presents the corresponding information for labor market outcomes measured in 2003. As before, the circles correspond to the raw averages and the lines represent fitted values from OLS regressions without additional covariates. The top panel presents the results for employment, measured as the annual number of months employed in the open labor market (i.e. excluding subsidized work). The downward sloping lines indicate that the labor market prospects of immigrants improve as they spend more time in Finland. That is, those who arrived in the early 1990s worked more in 2003 than those who arrived in the late 1990s. Similarly, earlier cohorts had larger annual earnings (middle panel) and received less social benefits (bottom panel).[14] These observations are in line with previous studies on

---

[14]Since many benefits depend on total household income, social benefits are measured

the assimilation of immigrants in Finland and in other countries.

Figure 2 also suggests that those arriving in May 1997 performed better in the labor market than those arriving in April 1997. These jumps correspond to the numerator in equation (1) and can be interpreted as "intention-to-treat" (ITT) or "reduced form" estimates of introducing the integration plans. According to the point estimates using a linear specification, the policy change increased average employment by 1.5 months (standard error 0.6 months) and annual earnings by 3,197 euros (standard error 1,289 euros) among the entire population present in the estimation sample. The improvement in labor market performance is reflected in a decrease in annual social benefits, accounting for 1,323 euros (standard error 375 euros).

Table 2 reports similar estimates after controlling for demographic characteristics, region of origin, legal status for a residence permit, local unemployment rate, type of residence municipality, an indicator for living in the Uusimaa region (where the capital, Helsinki, is located) and indicators for the quarter of entering the population register. All background characteristics are measured in the year of arrival. The results reported in columns (1), (3) and (5) are similar to those obtained without control variables.

The regression-discontinuity estimates reported in the second column of Table 2 suggest that integration plans increased employment by more than four months in the years 2002 and 2003. In other words, according to the point estimates, the entire employment growth from the year 2000 onwards can be attributed to the integration program. The relative magnitude of the estimates for annual earnings (column 4) and benefits (column 6) are similar. We note that the estimates are quite imprecise, and one should not therefore draw strong conclusions from the point estimates. Nevertheless, the estimates are statistically highly significant. Furthermore, as we discuss in detail in Section 5.3, even the magnitudes of the point estimates may not be implausible given the characteristics of the complier population and the nature of the treatment. Before turning to the interpretation, however, we report a set of robustness checks and discuss the internal validity of the

at the household level using the OECD equivalence scale. The scale assigns a value of 1 to the first household member, 0.7 to other adults and 0.5 to each child.

results.

5.2 Robustness Checks

We start by examining whether the number of observations changes abruptly at the May 1997 threshold. This exercise is motivated by the standard concern about RD designs that individuals could manipulate the forcing variable and thus affect their assignment into the treatment (McCrary, 2008). In our context, such manipulation would mean that some immigrants had entered the population register before May 1st 1997 in order to avoid the obligation to receive an integration plan. Given that the cutoff date was published in May 1998—and was unlikely to have become widely known even then—this concern is unlikely to be valid. In fact, it is unlikely that *anyone* knew about the forthcoming date rule in May 1997. This reasoning is supported by Figure 3, which plots the number of immigrants entering the population register over the study period. We find no evidence of a jump at the May 1997 threshold.

Another way to scrutinize the baseline results is to introduce arbitrary discontinuities in the data and to test for their significance. To do this, we create "placebo" thresholds for each possible arrival month between January 1993 and May 1997, and examine whether outcomes measured six years later differ between those arriving before and after the threshold. Figure 4 reports the results. The only estimates that are similar to our real estimates are found around the true threshold of May 1997. Note that we should expect to see similar estimates for placebo thresholds close to May 1997, as they can be considered as measuring the true threshold with a measurement error.

We next turn to the parameterization. We acknowledge that the consistency of our baseline estimates requires the functional form of $g(z_i)$ and $k(z_i)$ in equations (2) and (3) to be a reasonable proxy for the true underlying process. Since we do not know the functional forms of these processes, we experiment with alternative specifications. Panel A of Table 3 reports the main estimates when adding the number of polynomials to $g(z_i)$ and $k(z_i)$. The point estimates are remarkably stable across these specifications.

14

However, more flexible functional forms lead to substantially less precise estimates.

Another potential source of bias is selective outmigration. In principle, our results could follow from the integration plans reducing the emigration of immigrants at the upper end of the skill distribution (or increasing their emigration at the lower end). However, given the large magnitude of the estimates, these outmigration flows would have to be large in order to explain the results. Furthermore, the data suggest that the integration plans had no effect on outmigration.[15]

Our final robustness check concerns the estimation sample. The baseline results are obtained from a sample where we have excluded immigrants who did not experience unemployment and did not receive social assistance during their first three years in Finland. While this sample selection rule should allow us to focus on the relevant population and thus improve the precision of the estimates, it also raises possible concerns. For instance, the treatment might have moved some immigrants to the 0.1 percent of the earnings or social benefits distribution or the reform could have altered the inflow to unemployment or social assistance. Furthermore, our data record social assistance paid to the immigrant and to his possible spouse, but we do not observe social assistance paid to the parents. Thus, our sample selection rule excludes all grown-up children who are eligible for an integration plan, but who do not register as job seekers.

Panel B of Table 3 presents the estimates using the full sample. Since we now also include immigrants who were not targeted by the integration plans, the reduced form estimates are smaller. However, the RD estimates should not be affected by the inclusion of the "never-takers". In line with this prediction, the RD estimates from the full sample are similar to those from the restricted sample. None of the estimates presented in panel B are statistically significantly different from those presented in panel A. If

---

[15]Regressing a dummy for leaving Finland by the end of 2003 on a dummy for entering the population register after May 1997 yields estimates of 0.002 (standard error 0.035), -0.048 (standard error 0.053) and 0.033 (standard error 0.072) when using linear, quadratic and cubic specifications for z_i, respectively.

anything, the point estimates suggest a larger impact on annual earnings, while the point estimates for employment and social benefits are close to the baseline estimates.

5.3 Interpretation

The robustness checks and the *a priori* plausibility of the research design support the internal validity of the estimates. Therefore, we conclude that the reform helped to integrate immigrants into the Finnish labor markets. In order to draw more general lessons from this specific policy reform, we next discuss the interpretation of the estimates in detail.

We start by noting that the RD estimates measure a local average treatment effect (Imbens and Angrist, 1994; Hahn et al., 2001). That is, we identify the mean effect among those entering the population register on May 1st, 1997, who received an integration plan and would not have received it had they arrived earlier. According to the first-stage estimates, this subpopulation of compliers makes up roughly a third of the entire immigrant population.[16] Note that the immigrants who became employed within two years of arriving in Finland are never-takers, as their entry date does not affect their treatment status. In other words, the compliers remained unemployed for at least two years after arrival, which implies that they are a negatively selected subpopulation of immigrants.

To gain further insights, we relate the background characteristics of the compliers to those of the entire immigrant population. Angrist and Pischke (2009) show that

$$\frac{\mathbb{E}\left[D_i|z_i \geq z_0, x_i = 1\right] - \mathbb{E}\left[D_i|z_i < z_0, x_i = 1\right]}{\mathbb{E}\left[D_i|z_i \geq z_0\right] - \mathbb{E}\left[D_i|z_i < z_0\right]} \tag{4}$$

$$= \frac{P\left(x_i = 1|D_{i,z_i \geq z_0} > D_{i,z_i < z_0}\right)}{P\left(x_i = 1\right)}$$

---

[16]Regressing the treatment status on a dummy for entering the population register after May 1997 yields estimates of 0.30 (standard error 0.05), 0.35 (standard error 0.05) and 0.33 (standard error 0.06) when using linear, quadratic and cubic specifications for z_i, respectively.

where x_i is a binary variable measuring a characteristic of immigrant i, z_i is the month when he entered the population register, z_0 is May 1997, $D_{i,z_i<z_0}$ is the potential treatment status if the immigrant enters the population register before the threshold date, and $D_{i,z_i\geq z_0}$ is the potential treatment status if he enters after the threshold. In this notation, compliers are defined as those who have $D_{i,z_i<z_0} = 0$ and $D_{i,z_i\geq z_0} = 1$.

Column 6 of Table 1 reports estimates for equation (4).[17] The results suggest that the compliers were more likely to be refugees and family members, to come from outside of the European Union, to have an immigrant spouse, and to live outside the capital region of Uusimaa than other immigrants. In short, the compliers tended to belong to groups that are the most likely to lack the basic skills required in the Finnish labor market and who have the least access to social networks that would help in finding employment.

It seems reasonable to think that this subpopulation may have been particularly responsive to the type of treatment we evaluate. The Finnish reform changed the training provision from a system that did not recognize the special needs of immigrants to a system where the importance of language skills and other host-country-specific human capital was taken more seriously (see Section 2.3 for details). After the reform, immigrants were helped to find training that would fit their needs and were allowed to participate in suitable courses even outside the Labor Administration. As a consequence, language training and courses teaching basic facts about the Finnish society and the labor markets are likely to have become much more available. This type of training may be complementary to the human capital that immigrants have acquired before migration. Furthermore, basic language skills and guidance on how to seek work may be sufficient for finding employment in many low-skilled occupations.

Unfortunately, our ability to document this likely increase in training is limited as we only have access to data for training provided by the Labor Administration. Nevertheless, these data suggest that even within the Labor Administration, the reform shifted resources from general "preparatory

---

[17]For continuous variables we have used an indicator variable taking a value of one if x_i is larger than the sample median and zero otherwise.

17

training"—such as general job search training—to courses specifically designed for immigrants (see panel D of Table 1). Furthermore, other sources suggest that training outside the Labor Administration has been an important source of immigrant training after the reform (see footnote 7). Taken together, the available evidence suggests that the reform increased the provision of courses that aim to build up host-country-specific human capital.

Our final remark concerns the potential impact on incentives. While sanctioning of non-compliance was not affected by the reform, we cannot rule out that some immigrants might have disliked the integration plans and thus avoided them by becoming employed before the program was implemented. In this case, the group of immigrants affected by the treatment would be larger than those who ended up being formally treated. Hence, we would underestimate the denominator of equation (1) and the RD estimates would be biased upwards. While we do not expect this to be of major importance, a conservative interpretation is that the RD estimates are an upper bound of the treatment effect. On the other hand, a very conservative lower bound is obtained by assuming that everyone arriving after May 1997 was affected by the policy change and thus the denominator of equation (1) would be one. Consequently, the ITT estimate would provide a lower bound for the treatment effect.

6   Conclusions

In this paper, we have evaluated the impact of an integration program on labor market performance among male immigrants to Finland. Our empirical strategy is based on a discontinuity that obliged non-working immigrants to participate in the program only if they had entered the population register after May 1st, 1997. This rule was made public in May 1998 and thus could not have affected the entry decisions of immigrants at the threshold. Standard checks for robustness support the internal validity of the results.

We find that receiving an integration plan substantially increased employment and annual earnings and reduced welfare dependency. The integration plans seem to have increased participation in courses aimed at

18

building up host-country-specific human capital. In contrast, sanctioning of non-compliance was not affected by the reform.

References

ANGRIST, J. D. AND J.-S. PISCHKE (2009): *Mostly Harmless Econometrics: An Empiricist's Companion*, Princeton University Press.

ANTECOL, H., P. KUHN, AND S. TREJO (2003): "Immigration Policy and the Skills of Immigrants to Australia, Canada, and the United States," *Journal of Human Resources*, 38, 192–218.

ÅSLUND, O. AND P. JOHANSSON (2006): "Virtues of SIN. Effects of an Immigrant Workplace Introduction Program," IFAU Working Paper 2006:7.

BOERI, T., G. HANSON, AND B. McCORMICK (2002): *Immigration Policy and the Welfare System*, Oxford University Press.

BORJAS, G. (1985): "Assimilation, Changes in Cohort Quality, and the Earnings of Immigrants," *Journal of Labor Economics*, 3, 463–489.

——— (1993): "Immigration Policy, National Origin, and Immigrant Skills: A Comparison of Canada and the United States," in *Small Differences That Matter: Labor Markets and Income Maintenance in Canada and the United States*, ed. by D. Card and R. Freeman, University Of Chicago Press.

——— (1994): "The Economics of Immigration," *Journal of Economic Literature*, 32, 1667–1717.

——— (1995): "Assimilation and Changes in Cohort Quality Revisited: What Happened to Immigrant Earnings in the 1980s?" *Journal of Labor Economics*, 13, 201–245.

——— (2002): "Welfare Reform and Immigrant Participation in Welfare Programs," *International Migration Review*, 36, 1093–1123.

BORJAS, G. AND L. HILTON (1996): "Immigration and the Welfare State: Immigrant Participation in Means-Tested Entitlement Programs," *Quarterly Journal of Economics*, 111, 575–604.

BORJAS, G. AND S. TREJO (1990): "Immigrant Participation in the Welfare System," *Industrial & Labor Relations Review*, 44, 195.

CARRERA, S. (2006): "A Comparison of Integration Programs in the EU: Trends and Weaknesses," Challenge Papers No. 1, Centre for European Policy Studies, Brussels.

CHISWICK, B. (1978): "The Effect of Americanization on the Earnings of Foreign-Born Men," *Journal of Political Economy*, 86, 897–921.

CIC (2005): "Evaluation of the Immigration Settlement and Adaptation Program (ISAP)," Citizenship and Immigration Canada.

COHEN-GOLDNER, S. AND Z. ECKSTEIN (2008): "Labor Mobility of Immigrants: Training, Experience, Language and Opportunities," *International Economic Review*, 49, 837–872.

——— (2010): "Estimating the Return to Training and Occupational Experience: The Case of Female Immigrants," *Journal of Econometrics*, 156, 86–105.

GOVERNMENT PROPOSAL FOR THE INTEGRATION ACT (1998): "HE 66/1998," Parliament of Finland.

HAHN, J., P. TODD, AND W. KLAAUW (2001): "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design," *Econometrica*, 69, 201–209.

HU, W. (2000): "Immigrant Earnings Assimilation: Estimates from Longitudinal Data," *American Economic Review*, 90, 368–372.

IMBENS, G. AND J. ANGRIST (1994): "Identification and Estimation of Local Average Treatment Effects," *Econometrica*, 62, 467–467.

JOPPKE, C. (2007): "Beyond National Models: Civic Integration Policies for Immigrants in Western Europe," *West European Politics*, 30, 1–22.

LEE, D. AND D. CARD (2008): "Regression Discontinuity Inference with Specification Error," *Journal of Econometrics*, 142, 655–674.

Lubotsky, D. (2007): "Chutes or Ladders? A Longitudinal Analysis of Immigrant Earnings," *Journal of Political Economy*, 115, 820–867.

McCrary, J. (2008): "Manipulation of the Running Variable in the Regression Discontinuity Design: a Density Test," *Journal of Econometrics*, 142, 698–714.

Ministry of Labour (2003): "Government Report on Implementation of the Integration Act," Publication of the Labour Administration 317. Helsinki: Ministry of Labor.

——— (2005): "Yhteistyössä kotouttamiskoulutusta toteuttamaan – Työ- ja opetushallinnon työryhmän raportti [Implementing Integration Training Together – A Report of the Ministry of Labour and Ministry of Education]," Publication of Labor Administration 351. Helsinki: Ministry of Labor.

Rosholm, M. and R. Vejlin (2007): "Reducing Income Transfers to Refugee Immigrants: Does Starthelp Help You Start?" IZA Discussion Paper No. 2720.

Sarvimäki, M. (forthcoming): "Assimilation to a Welfare State: Labor Market Performance and Use of Social Benefits by Immigrants to Finland," *Scandinavian Journal of Economics*.

Schmidt, R. (2007): "Comparing Federal Government Immigrant Settlement Policies in Canada and the United States," *American Review of Canadian Studies*, 37, 103–122.

van der Klaauw, W. (2002): "Estimating the Effect of Financial Aid Offers on College Enrollment: a Regression-Discontinuity Approach," *International Economic Review*, 43, 1249–1287.

Figure 1: The Proportion of Immigrants Entering the Integration Program According to the Month of Arrival
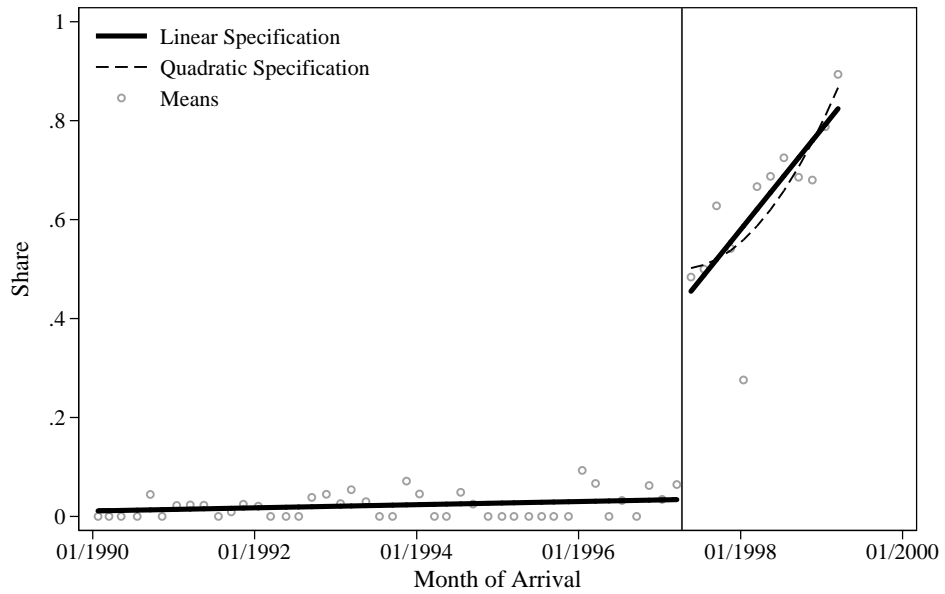
Note: Monthly means and OLS fitted values. Linear Specification: $k(z_i) = \pi_1 z_i + \pi_2 z_i 1\{z_i > z_0\}$, Quadratic Specification: $k(z_i) = \pi_1 z_i + \pi_2 z_i^2 + \pi_3 z_i 1\{z_i > z_0\} + \pi_4 z_i^2 1\{z_i > z_0\}$, where z_i is the distance from z_0 (May 1997). Outcome: Receives an integration plan before the end of 2003.

Figure 2: Labor Market Outcomes in 2003 According to Month of Arrival
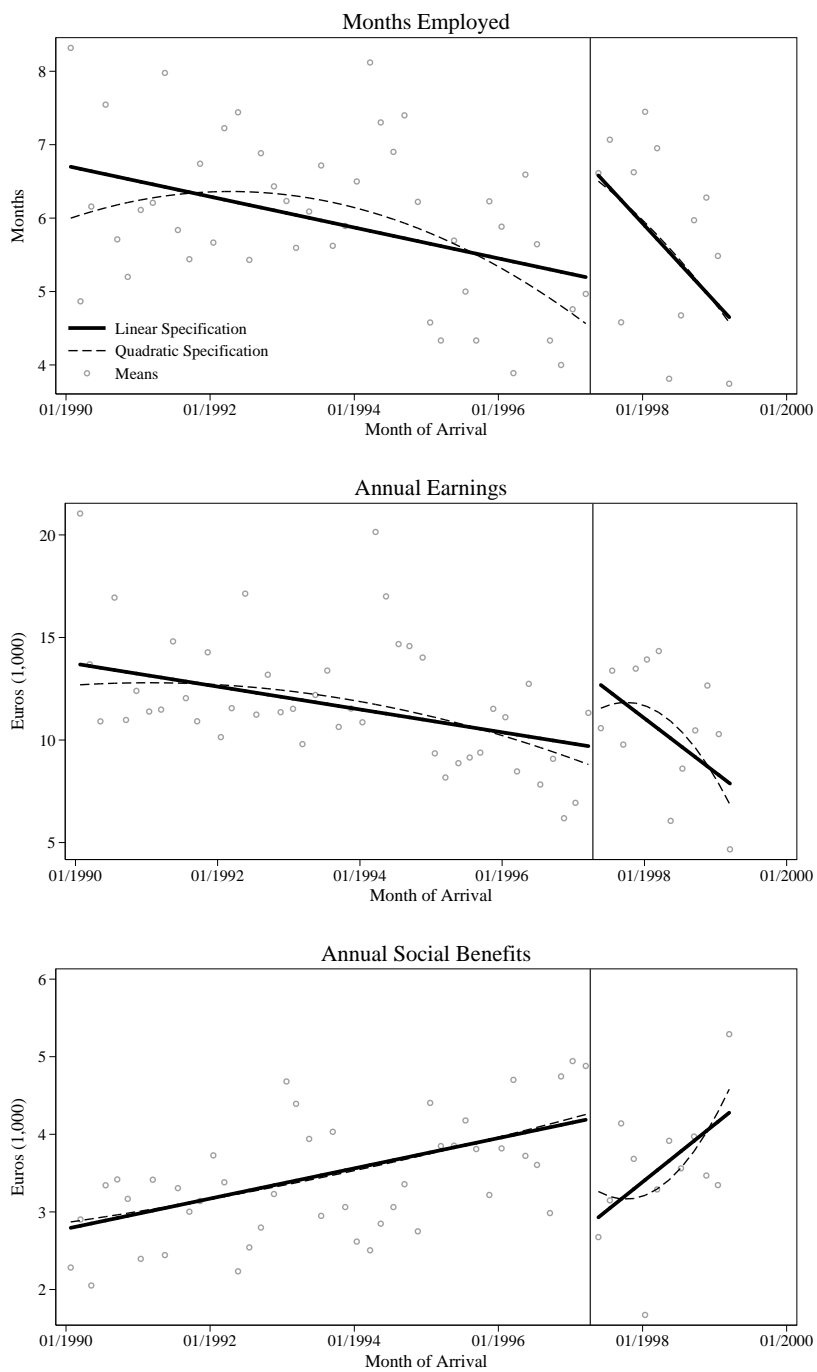
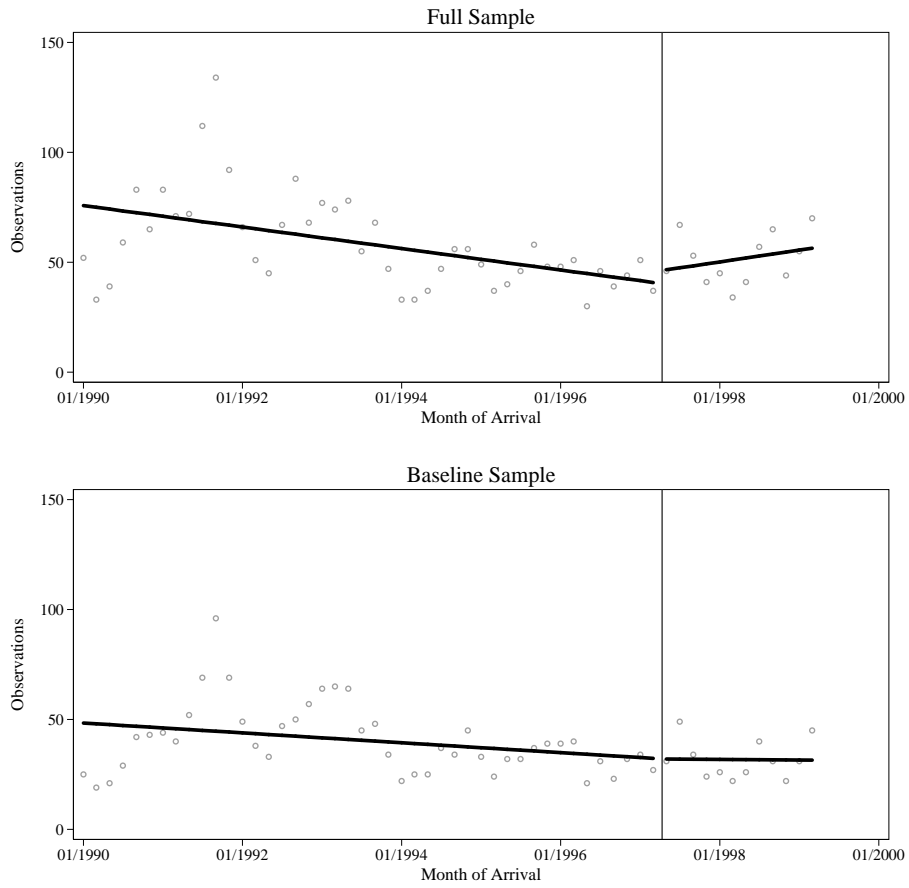
Months Employed

Annual Earnings

Annual Social Benefits

Note: Monthly means and OLS fitted values. Linear Specification: $k(z_i) = \pi_1 z_i + \pi_2 z_i 1\{z_i > z_0\}$, Quadratic Specification: $k(z_i) = \pi_1 z_i + \pi_2 z_i^2 + \pi_3 z_i 1\{z_i > z_0\} + \pi_4 z_i^2 1\{z_i > z_0\}$, where z_i is the distance from z_0 (May 1997). Outcomes measured in 2003.

Figure 3: Size of Arrival Cohorts

Note: Size of arrival cohorts and OLS fitted values. Specification: $g(z_i) = \pi_1 z_i + \pi_2 z_i 1\{z_i > z_0\}$. Outcome: Monthly number of arrivals.

Figure 4: Jumps at non-discontinuity points

Months Employed

Annual Earnings

Social Benefits

Placebo Threshold Date
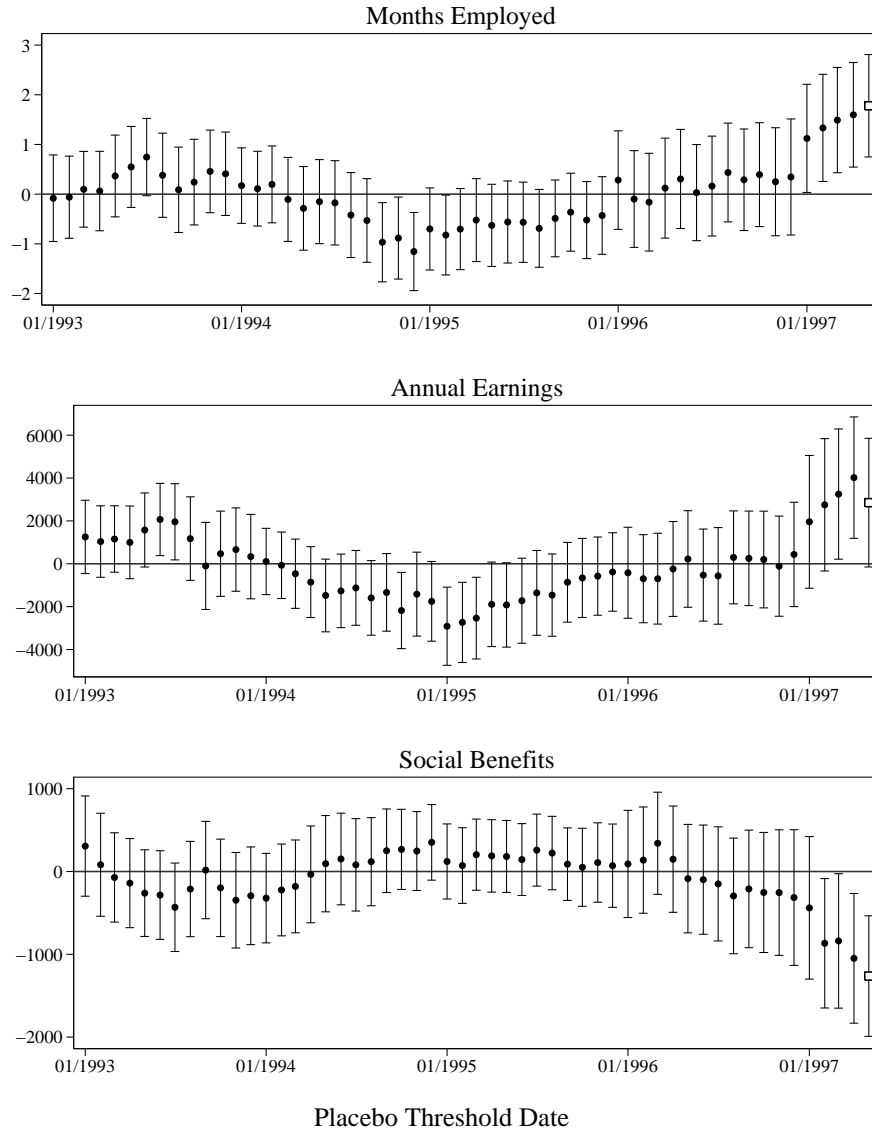
Note: Reduced form (OLS) estimates and 95% confidence intervals of jumps at non-discontinuity points. X-axis: Placebo threshold date. Outcome measured five years after the year of the placebo threshold. The square marker corresponds to the estimate for the real threshold of May, 1997.

Table 1: Background Characteristics

	Arrival Cohort					Compliers	
	1/90-4/91	5/91-4/93	5/93-4/95	5/95-4/97	5/97-4/99	coef.	se.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A: Characteristics at Arrival							
Age	33.4	33.2	34.5	35.5	35.5	0.89	(0.12)
Single	0.36	0.30	0.25	0.24	0.23	0.57	(0.14)
Has a native spouse	0.37	0.28	0.28	0.29	0.30	0.75	(0.18)
...an imm. spouse	0.27	0.42	0.47	0.47	0.47	1.54	(0.16)
Number of children	0.60	0.86	1.00	0.79	0.81	1.24	(0.17)
Local unemp. rate	7.1	15.4	20.6	17.0	13.7	1.16	(0.19)
Lives in Uusimaa	0.42	0.43	0.45	0.47	0.43	0.60	(0.12)
B: Region of birth							
EU15/EFTA	0.12	0.05	0.09	0.12	0.13	0.20	(0.11)
New EU-members	0.04	0.10	0.13	0.09	0.05	0.41	(0.28)
form. Soviet Union	0.27	0.28	0.24	0.28	0.35	1.50	(0.22)
form. Yugoslavia	0.00	0.10	0.10	0.10	0.04	4.07	(1.16)
Turkey	0.03	0.06	0.04	0.06	0.06	1.07	(0.58)
Africa	0.21	0.20	0.16	0.09	0.13	0.78	(0.33)
Asia	0.26	0.15	0.18	0.21	0.20	1.23	(0.26)
Other/Unknown	0.08	0.05	0.06	0.05	0.04	0.59	(0.40)
C: Legal Status							
Ingrian Finn	0.05	0.11	0.24	0.16	0.17	1.17	(0.38)
Family Member	0.09	0.12	0.23	0.26	0.30	1.73	(0.30)
Refugee	0.07	0.15	0.21	0.20	0.15	2.76	(0.57)
Other/Unknown	0.79	0.61	0.32	0.38	0.39	0.49	(0.09)
D: Days in Training (Labor Administration only)							
Total	179	270	314	304	290	1.35	(0.05)
Immigrant training	8	13	23	48	115	1.23	(0.05)
Other preparatory	72	135	179	169	95	1.32	(0.05)
Vocational	99	122	111	87	80	1.23	(0.06)
Individuals	234	633	411	370	371		

Note: Sample means at arrival, OLS estimates for a jump at May 1997, and complier means divided by sample means. Column (6) reports estimates of the ratio of the expected values of binary background characteristics among the compliers divided by the expected value among the entire sample, $P\left(x_i = 1 | D_{i,z_i \geq z_0} > D_{i,z_i < z_0}\right)/P\left(x_i = 1\right)$, see Section 5.3 for discussion. Bootstrapped standard errors in Column (7) are obtained with 1,000 replications.

Table 2: Impact of the Integration Plan

	Months Employed		Annual Earnings		Social Benefits	
	ITT	RD	ITT	RD	ITT	RD
	(1)	(2)	(3)	(4)	(5)	(6)
A: Estimates						
2000	0.38	0.90	660	1,569	-604	-1,436
	(0.41)	(1.00)	(961)	(2,304)	(305)	(780)
2001	0.75	1.89	756	1,921	-1,050	-2,667
	(0.45)	(1.23)	(1,188)	(3,049)	(281)	(845)
2002	1.70	4.35	2,087	5,329	-1,381	-3,526
	(0.50)	(1.44)	(1,279)	(3,374)	(338)	(1,030)
2003	1.78	4.54	2,853	7,272	-1,263	-3,220
	(0.53)	(1.52)	(1,532)	(4,022)	(371)	(1,138)
B: Means of the treated 1997 cohort						
2000	2.00		3,626		5,973	
2001	3.51		6,502		5,140	
2002	4.83		8,236		4,859	
2003	5.03		9,769		4,382	

Note: Reduced form / intention to treat estimates (ITT) and regressions-discontinuity estimates (RD). Standard errors (in parentheses) are clustered according to the month of arrival. Rows correspond to the year of measuring the outcome. Parameterization: $g(z_i) = k(z_i) = \pi_1 z_i + \pi_2 z_i 1\{z_i > z_0\}$. The partial R^2 and F-statistics for the excluded instruments are 0.06 and 70.1, respectively. Controlling for age, age squared, region of origin, legal status, local unemployment rate at arrival, quarter of arrival, type of municipality (city, semi-rural, rural) at arrival, lives in the Helsinki region (Uusimaa) at arrival, marital status at arrival, indicators for having children younger than 3 years old, 7 years old and 18 years old in the household at arrival. Social benefits are measured at the household level using the OECD equivalence scale, see footnote 14.

Table 3: Robustness Checks (2003 outcomes)

	Months Employed		Annual Earnings		Social Benefits	
	ITT	RD	ITT	RD	ITT	RD
	(1)	(2)	(3)	(4)	(5)	(6)
A: Baseline Sample						
Linear	1.78	4.54	2,853	7,272	-1,263	-3,220
specification	(0.53)	(1.52)	(1,532)	(4,022)	(371)	(1,138)
Quadratic	2.07	4.72	3,381	7,732	-1,078	-2,464
specification	(0.80)	(2.05)	(2,372)	(5,537)	(483)	(1,236)
Cubic	2.12	4.94	3,158	7,362	-1,565	-3,647
specification	(1.09)	(2.97)	(3,478)	(8,442)	(596)	(1,702)
B: Full Sample						
Linear	1.44	4.97	4,189	14,455	-985	-3,397
specification	(0.45)	(1.64)	(1,434)	(5,418)	(309)	(1,246)
Quadratic	1.16	3.81	3,946	12,923	-824	-2,698
specification	(0.58)	(2.03)	(1,880)	(6,769)	(392)	(1,402)
Cubic	0.99	3.48	4,619	16,295	-1,556	-5,489
specification	(0.69)	(2.75)	(2,679)	(11,169)	(426)	(1,916)

Note: Reduced form / intention to treat estimates (ITT) and regressions-discontinuity esti-mates (RD). Standard errors (in parentheses) are clustered according to the month of arrival. Rows correspond to different modeling assumptions of $g(z_i)$ and $k(z_i)$. Controlling for age, age squared, region of origin, legal status, local unemployment rate at arrival, quarter of arrival, type of municipality (city, semi-rural, rural) at arrival, lives in the Helsinki region (Uusimaa) at arrival, marital status at arrival, indicators for having children younger than 3 years old, 7 years old and 18 years old in the household at arrival. Social benefits are measured at the household level using the OECD equivalence scale, see footnote 14.