Integrating Immigrants: The Impact of Restructuring Active Labor Market Programs

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We examine the impact of restructuring active labor market programs for unemployed immigrants in Finland. Exploiting a discontinuity in the phase-in rules of the reform, we find that it increased compliers' cumulative earnings by 47% over a 10-year follow-up period. We attribute these improvements to a more efficient use of existing resources. The reform did not affect total days in training, but it did modify the content toward training specifically designed for immigrants.

I. Introduction

This paper shows that active labor market programs (ALMP) for disadvantaged immigrants can be remarkably efficient. We examine a policy reform that introduced "integration plans" for unemployed immigrants in Finland. The plans consist of individualized sequences of ALMP that a caseworker believes to be the most appropriate for each immigrant. Our

An earlier version of this paper (using a smaller sample and a shorter follow-up period) was circulated as "Assimilating Immigrants: The Impact of an Integration Program." We thank David Card, Ben Feigenberg, Mika Haapanen, Kristiina Huttunen, Guy Michaels, Steve Pischke, Miikka Rokkanen, Uta Schönberg, Roope Uusitalo, Olof Åslund, and various seminar participants for comments that improved the paper and Tomi Kyyrä, Henna Nivala, Hanna Pesola and Aino Tylli for their generous help with the data. Financial support from the NORFACE project "Migration: Integration, Impact, and Interaction" and the Academy of Finland project "Activation Policies and Basic Security" is gratefully acknowledged. Sarvimäki

[*Journal of Labor Economics*, 2016, vol. 34, no. 2, pt. 1]
 0734-306X/2016/3402-0005$10.00
Submitted January 11, 2014; Accepted September 2, 2014; Electronically published January 19, 2016

results suggest that this restructuring of ALMP strongly increased participants' earnings and reduced their social benefits.

Figure 1 illustrates our research design and presents the main result. The reform was launched on May 1, 1999, but it affected only those immigrants who had arrived in Finland after May 1, 1997. Immigrants arriving just before and after the cutoff date are similar in their observable characteristics, and it seems plausible to assume that they are also comparable in their unobservables. However, those arriving just after the May 1997 cutoff earned roughly €7,000 more during the 2000s than those arriving just before (top panel). They were also 35 percentage points more likely to get an integration plan (bottom panel). Together the discontinuities suggest a local average treatment effect of €21,000. In comparison to the baseline among the compliers, this corresponds to a 47% increase in cumulative earnings over the decade. Using the same approach, we also find a 13% decrease in cumulative social benefits.

The estimates are statistically significant, and they survive a battery of robustness checks and falsification exercises. Specifically, we show that there are no discontinuities in the number of immigrants arriving at the cutoff and that our conclusions are not sensitive to the choice of bandwidth or to controlling for background characteristics. Furthermore, discontinuities of similar magnitudes at made-up cutoffs are rare.

We attribute these improvements to a more efficient use of existing resources. The reform did not provide new funding for ALMP, and we find no discontinuity in the overall days spent in training. Instead, there is a discontinuity in the content of training: the integration plans seem to have increased time spent in language courses and other training specifically designed for immigrants while scaling down traditional ALMP such as job-seeking courses.

These findings add to the vast literature examining the impact of ALMP. Our estimates are large in comparison to the typical findings for natives, but they are in line with the few exisiting estimates on the impact of ALMP on immigrants.[1] In comparison to previous studies, our data and research design allow for examining a long follow-up period and a causal interpretation under weak assumptions. Our findings, together with the earlier results, suggest that providing appropriate training for immigrants

also acknowledges financial support from the Yrjö Jahnsson Foundation, the Jenny and Antti Wihuri Foundation, Palkansaajasäätiö, and the OP-Pohjola Group Research Foundation. Contact the corresponding author, Matti Sarvimäki, at matti.sarvimaki@aalto.fi. Information concerning access to the data used in this article is available as supplementary material online.

[1] See, e.g., Heckman, LaLonde, and Smith (1999) and Card, Kluve, and Weber (2010) for surveys of the ALMP literature. Previous work on the impact of ALMP on immigrants include Cohen-Goldner and Eckstein (2008, 2010), Clausen et al. (2009), Åslund and Johansson (2011), Heinesen, Husted, and Rosholm (2011), and Andersson Joona and Nekby (2012).

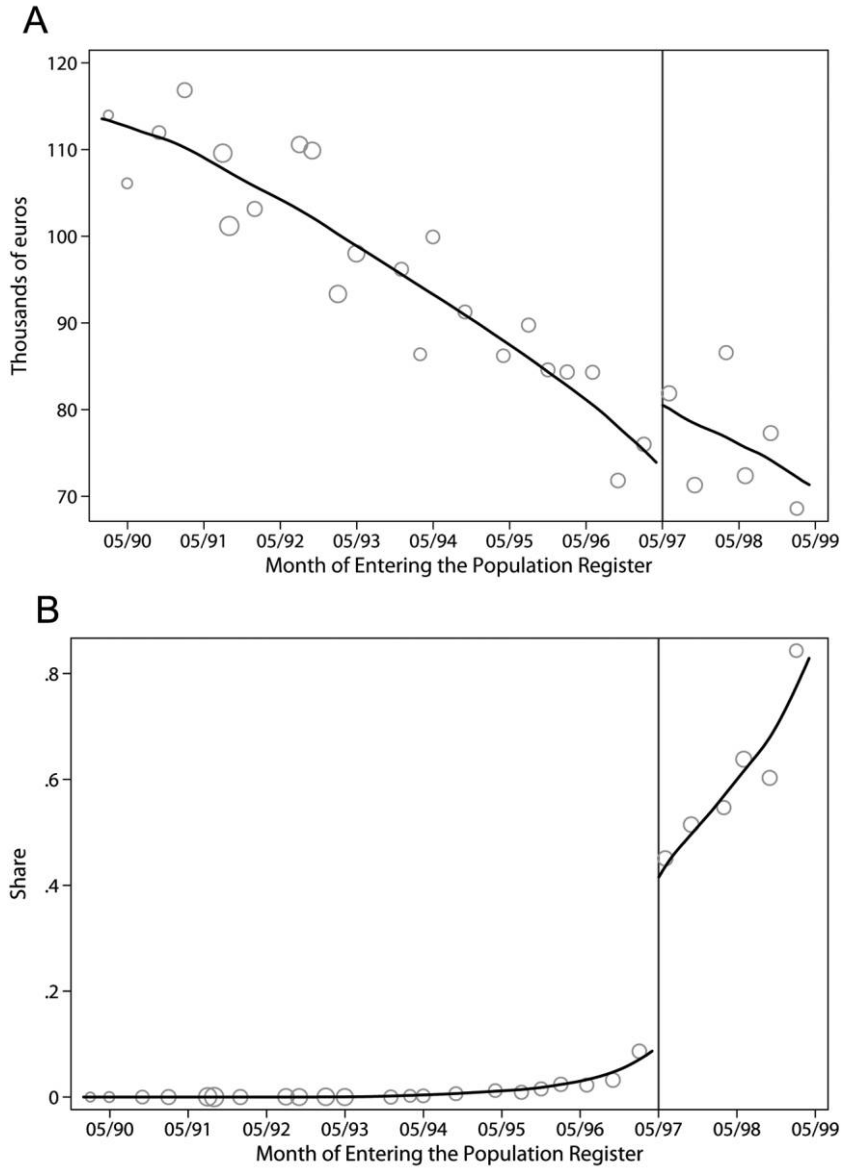

FIG. 1.—Earnings and integration plans by month of arrival: *A*, earnings; *B*, integration plans. The top panel plots total earnings between 2000 and 2009 (including zeros) by month of entering the population register. The bottom panel presents similar analysis for an indicator of a person receiving an integration plan. 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 sample means by 4-month bins.

may have much larger effects than typical ALMP for natives. Furthermore, they indicate that caseworkers are able to allocate immigrants into appropriate training.[2]

We also inform the literature examining the labor market assimilation of immigrants. While the assimilation process has been extensively documented, there is relatively little research assessing the factors that affect it. Improving language skills is often hypothesized to play an important role, but empirical work examining this channel has faced severe measurement error and endogeneity problems.[3] Our results complement the existing studies by focusing on a reform that increased language training.

II. The Treatment

A. Background

We examine an intervention that was introduced as part of the Act on the Integration of Immigrants and Reception of Asylum Seekers (hereafter, the reform), which came into force on May 1, 1999. It was passed during a period of rapidly rising immigration to Finland that was characterized by the poor labor market performance of new arrivals. Earlier immigrants had mostly been family members of Finnish return migrants from Western Europe. During the 1990s, however, the immigrant population grew fourfold, and the former Soviet Union and Asia became the main source areas. Nevertheless, at the time of the reform, the share of immigrants of the population remained relatively low at 2%.

The statistics on the reasons for immigration are incomplete, but it is clear that the proportion of labor migration was low. This is likely to explain the poor labor market performance of immigrants moving to Finland in the 1990s. Upon arrival, their employment rates were very low, and hence they earned substantially less than comparable natives. While the gap decreased over time, only the earnings of men from Organization for Economic Cooperation and Development (OECD) countries converged with the earnings of comparable natives (Sarvimäki 2011). As everyone living in Finland on a permanent basis is eligible for most social benefits, low average employment rates led to high average social benefits received by immigrant households.

[2] Previous work examining the role of caseworkers includes Lechner and Smith (2007), Behncke, Frölich, and Lechner (2010a, 2010b), and Rinne, Uhlendorff, and Zhao (2013).

[3] Studies documenting the association between language skills and labor market performance include Chiswick (1991), Dustmann (1994), Chiswick and Miller (1995, 2007), Dustmann and van Soest (2001, 2002), Berman, Lang, and Siniver (2003), Dustmann and Fabbri (2003), and Bleakley and Chin (2004). Meng and Gregory (2005) discuss the role of language proficiency in the context of immigrants married to natives. See Borjas (1999) and Pekkala Kerr and Kerr (2011) for surveys of the broader literature of labor market assimilation.

B. Integration Plans

The main change imposed by the reform was that employment offices had to start preparing individualized integration plans for nonworking immigrants who had lived in Finland for less than 3 years. The plans were prepared in a joint meeting with a caseworker, the immigrant, and, if necessary, an interpreter. The aim was to find a sequence of training and other measures that would be the most suitable for each immigrant given her or his skills and circumstances. Immigrants are a heterogeneous group, and thus the integration plans were also very diverse. They could include language courses, other courses specifically designed for immigrants (e.g., training in civic and working life skills), vocational training, subsidized job placements, rehabilitation, and so forth.

In principle, similar ALMP were offered under the old system. Thus it appears that the primary function of an integration plan was to improve the communication between caseworkers and immigrants. In particular, the new guidelines stated that the caseworker had to make sure that the immigrant fully understood the content of her or his integration plan and knew how to follow it. In addition, integration plans included schedules for follow-up meetings with the caseworker.

The reform was also likely to affect the type of courses made available. The law had several references to the importance of learning one of the local languages (Finnish or Swedish) and thus implicitly guided the allocation of resources toward arranging more language courses. Furthermore, integration plans were allowed to include courses provided outside of the Labor Administration conditional on the courses facilitating integration (e.g., language courses provided by adult education centers or universities).[4] The reform also required local governments (municipalities) to prepare municipality-level integration programs that aimed to improve services provided to all immigrants.

In short, the "treatment" we examine is best understood as an increased focus on the circumstances of each immigrant during a time when the supply of suitable training was being increased. As the reform may also have increased the availability of training and other services for immigrants who did not have an integration plan, our estimates will capture its impacts only to the extent that the integration plans affected selection into training. It is also important to note that the reform did not change the rules on the use of sanctions. Furthermore, it did not provide any new funding for the employment offices or incentives for the caseworkers to increase their efficiency. However, resources used for immigrant training may have increased because participation in training outside of the Labor Administartion was allowed.

[4] In the old system, participation in courses outside of the Labor Administration was strongly discouraged, because anyone enrolled in such a course was considered a student and thus ineligible for most social benefits.

III. Empirical Strategy

A. Identification

Our research design was created by the phase-in rule of the reform. Integration plans were prepared for unemployed immigrants who had registered in the population register within the previous 3 years.[5] Importantly, however, participation was mandatory only for those who had entered the population register after May 1, 1997. For them, noncompliance was sanctioned by a temporary withdrawal of unemployment benefits. In contrast, otherwise eligible immigrants who had entered the population register before May 1997 had a right—but not an obligation—to get an integration plan.

The phase-in rule creates a fuzzy regression discontinuity design, where the running variable is the date of entering the population register and the cutoff point is May 1, 1997. We can thus identify the local average treatment effect (LATE) under the standard monotonicity and continuity assumptions (Imbens and Angrist 1994; Hahn, Todd, and van der Klaauw 2001). The same assumptions also allow us to separately identify the potential outcomes and average background characteristics among the compliers (Imbens and Rubin 1997; Abadie 2003; Frandsen, Frölich, and Blaise 2012).

In our context, the monotonicity assumption means that no one became less likely to receive an integration plan if she or he entered the population register on May 1, 1997, rather than the day before. The continuity assumption means that those entering the population register just before and after the cutoff date have similar potential outcomes. Both assumptions appear plausible. Immigrants were not able to manipulate their date of arrival in order to avoid the obligation to get an integration plan, because those arriving around May 1997 had made their entry decisions long before the reform was launched.[6] Furthermore, it is very unlikely that other parts of the reform—or any other policies—had a differential impact on those arriving just before and after the cutoff.

B. Estimation

We use local linear estimators due to their attractive properites in the context of the regression discontinuity (RD) design (Fan and Gijbels 1992; Porter 2003). That is, we use only immigrants who entered the population

[5] Immigrants have a strong incentive to register soon upon arrival as they need to be listed in the population register in order to apply for benefits, to receive wages and to open a bank account.

[6] The threshold date was published on May 8, 1998, when the government introduced the bill to parliament. The leading Finnish newspaper, *Helsingin Sanomat*, ran a short article about the bill, but it did not mention this threshold date. In Sec. IV, we also show that there was no discontinuity in the number of immigrants entering the population register in May 1997.

register within h days of the cutoff and a triangle-shaped kernel that puts most weight on those arriving close to the cutoff.[7] For our baseline regressions, we use the algorithm proposed by Imbens and Kalyanaraman (2012) for choosing the optimal bandwidth and present estimates using a wide range of alternative bandwidths as a robustness check.

We estimate the reduced form effects with weighted OLS regression

$$y_i = \alpha + \beta \mathbf{1}[r_i \geq r_0] + \delta_0(r_i - r_0) + \delta_1 \mathbf{1}[r_i \geq r_0](r_i - r_0) + X_i\theta + \epsilon_i, \quad (1)$$

where y_i is the outcome of interest for immigrant i, $\mathbf{1}[\cdot]$ is an indicator function, r_i is the date of entering the population register, r_0 is May 1, 1997, X_i is a vector of observed characteristics measured at the year of arrival, and ϵ_i summarizes the unobserved factors.

Similarly, we estimate the first-stage estimates as

$$D_i = \mu + \gamma \mathbf{1}[r_i \geq r_0] + \lambda_0(r_i - r_0) + \lambda_1 \mathbf{1}[r_i \geq r_0](r_i - r_0) + X_i\pi + \nu_i, \quad (2)$$

where D_i is an indicator for immigrant i getting an integration plan and the other variables are as above. These regressions estimate the discontinuities at the May 1, 1997, cutoff while controlling for the overall effect of the date of arrival.[8] Conditioning on the background characteristics is not required for consistency, but it may improve precision.

The parameters of interest are β and γ, which measure the jump in the expected outcome and the propensity to receive an integration plan at the May 1997 cutoff. Thus they correspond to the numerator and the denominator of a Wald estimator. The estimate for LATE is $\hat{\tau} = \hat{\beta}/\hat{\gamma}$, which can be estimated using standard weighted two-stage least squares (2SLS). Similarly, we estimate compliers' potential outcomes in the absence of an integration plan by using $y_i(1 - D_i)$ as the dependent variable and $(1 - D_i)$ as the treatment variable in 2SLS regressions (see Frandsen, Frölich, and Blaise [2012] for a discussion). Since we observe only the month of entry to the population register, we cluster all standard errors at this level.[9]

[7] Our sample includes only immigrants who entered the population register before May 1999. When h is greater than 24 months, our kernel is thus a truncated triangle, i.e., those arriving after April 1999 have zero weight, while those arriving before April 1995 receive a positive but small weight.

[8] In eq. (1), the date of arrival captures the assimilation effects and changes in the composition of the arrival cohorts. In eq. (2), the time of arrival predicts the likelihood of getting an integration plan because those arriving earlier had had more time to find employment before the launch of the reform.

[9] See Lee and Card (2008) for a discussion on the importance of taking into account the within-clusters correlation in RD designs when the forcing variable is discrete. Our baseline regression using the Imbens and Kalyanaraman (2012) optimal bandwidth algorithm yields from 36 to 95 clusters, depending on the outcome. The standard errors for our main results (table 3) are based on 66 clusters for earnings and 64 clusters for benefits.

IV. Data

A. Sources and Sample

Statistics Finland created our data by linking information from several administrative registers. We have access to a 90% random sample of immigrants who arrived between January 1990 and April 1999, and we focus on those who were targeted by the policy (immigrants arriving at ages 16–60 and either registered as unemployed job seekers or receiving social assistance during their first 3 years in Finland).

B. Background Characteristics

Tables 1 and A1 present immigrants' average background characteristics by the date of arrival. All characteristics are measured during the first full calendar year that the person lived in Finland. A casual comparison of columns 7 and 8 suggests that immigrants arriving a year before and after the May 1997 cutoff were very similar along the dimensions measured in our data.

In order to formally assess this impression, we run local linear regressions to examine whether background characteristics jump at the May 1997 cutoff. Of the 44 estimates reported in column 10, six are statistically significant (see the table notes). This is slightly more than what one should expect from 44 independent regressions purely by chance. However, these six variables are clearly not independent of each other. Three of them are related to the regional distribution of the immigrants, and the other three to the immigrants' legal statuses and countries of origin.

It seems unlikely that our results would be driven by a change in the composition of immigrants. In Section V.A, we show that our main estimates are not affected by controlling for background characteristics. Furthermore, the differences in background characteristics are small, and they are mostly negatively associated with earnings (being an Ingrian Finn is the only exception). If anything, those who arrived in May 1997 had less favorable background characteristics than those who arrived just before.[10]

Columns 1–9 reveal some long-term trends. Immigrants arriving in the early 1990s tended to be younger and more often male, single, and Estonian speakers (a language relatively close to Finnish) than those arriving later. They also entered a country that was just about to go through a severe

[10] In order to summarize the overall difference in background characteristics, we multiplied the estimates presented in cols. 10 of tables 1 and A1 by the unreported estimates for background characteristics in our main specification (table 3, col. 2). This calculation suggests that the expected cumulative earnings in 2000–2009 for those arriving in May 1997 were €8,500 less than for those arriving in April 1997.

recession, whereas the later cohorts arrived after the economy had started to grow. In addition, family unification became more common over time. While it is clearly important to control for these trends by including the date of arrival as a covariate in the regressions, they do not affect the validity of our identification strategy. We return to the interpretation of columns 12 and 13 in Section V.D.

C. Outcomes

Table 2 introduces our outcome variables. Our primary measure for economic performance is cumulative earnings over the period 2000–2009. We also examine a similar measure of social benefits. Because many benefits are targeted at households rather than individuals, we sum all benefits received by the immigrant or her or his spouse and use an equivalence scale to take into account differences in the size of households (see the table notes). We include observations with zero earnings or benefits throughout the analysis. For employment, we use three measures: an indicator for working at all during the 2000s, the time between arrival and starting in the first job, and total days in employment during the 2000s. We have removed subsidized employment from all employment measures, but we cannot distinguish between part-time and full-time work. We also report the average native earnings in the occupations that the immigrant held between 2004 and 2009.[11]

The top panel of table 2 presents a familiar assimilation profile, where those who have lived longer in the host country have higher employment and earnings, collect fewer social benefits, and work in better-paying occupations than those who have arrived more recently. Nevertheless, the labor market performance of all arrival cohorts was modest. For example, the average annual earnings among those who had arrived in the early 1990s was only €11,000 during the 2000s. In comparison, average annual earnings for natives in the same age bracket was €29,000.

The bottom panel presents our measures of ALMP. Among all arrival cohorts, they refer to the first 5 years the immigrant lived in Finland. Total days in training or subsidized job placements have increased over time. Among the earlier cohorts, this growth is largely driven by an increase in "traditional training," such as job-seeking courses and vocational training programs (see table A2). For the later cohorts, traditional training is

[11] Our data include at most one occupation per calendar year referring to the last quarter of the year for the period 2004–9. We assign average native earnings to each three-digit occupation and take an average of these averages. Occupation data are not available for 2000–2003.

Table 1
Average Characteristics at Arrival

	Arrival Cohort									Discontinuity at May 1997		Compliers	
	1/90–4/91 (1)	5/91–4/92 (2)	5/92–4/93 (3)	5/93–4/94 (4)	5/94–4/95 (5)	5/95–4/96 (6)	5/96–4/97 (7)	5/97–4/98 (8)	5/98–4/99 (9)	(10)	(11)	(12)	(13)
A. Demographic characteristics:													
Age	29.5	29.6	30.3	31.2	31.5	31.7	31.5	31.7	31.4	.29	(.44)	35.3	(.64)
Woman	.44	.46	.47	.53	.55	.53	.56	.56	.55	.00	(.02)	.55	(.04)
Married/cohabits	.62	.58	.66	.73	.73	.71	.67	.67	.69	−.01	(.03)	.73	(.04)
Has children	.39	.39	.49	.52	.50	.48	.44	.44	.46	−.02	(.03)	.53	(.04)
Estonian speaker	.16	.19	.15	.16	.14	.10	.07	.06	.05	.00	(.01)	.02	(.01)
Regional unemployment rate	3.9	7.1	12.6	15.4	15.2	14.1	12.8	11.2	10.4	.80	(.23)	12.2	(.25)
B. Type of residence municipality:													
Urban	.84	.80	.84	.87	.91	.87	.89	.90	.89	.00	(.01)	.94	(.04)
Semi-urban	.08	.11	.07	.06	.04	.07	.05	.05	.06	.00	(.01)	.03	(.02)
Rural	.08	.09	.09	.07	.05	.07	.05	.05	.05	.01	(.01)	.04	(.02)
C: Legal status:													
Refugee	.15	.19	.23	.23	.18	.16	.14	.11	.13	−.01	(.03)	.13	(.07)
Ingrian Finn	.08	.19	.26	.27	.25	.20	.15	.15	.11	.05	(.02)	.24	(.03)

Family member	.25	.26	.23	.26	.30	.29	.33	.33	.36	−.08	(.03)	.22	(.04)
Other/unknown	.52	.37	.28	.24	.27	.36	.38	.41	.40	.02	(.03)	.34	(.03)
D. Region of birth:													
Russia/Soviet Union	.36	.39	.36	.34	.35	.38	.38	.40	.41	.00	(.05)	.56	(.08)
Asia	.27	.16	.15	.15	.17	.16	.20	.22	.21	−.02	(.05)	.20	(.08)
Africa	.13	.21	.10	.13	.12	.09	.11	.11	.10	.00	(.02)	.10	(.03)
New EU members	.10	.13	.15	.18	.18	.12	.10	.09	.08	.01	(.01)	.06	(.02)
EU15 and EFTA	.06	.03	.03	.03	.05	.07	.08	.07	.08	−.01	(.01)	.04	(.01)
Turkey	.04	.04	.03	.03	.03	.03	.03	.03	.02	.00	(.01)	.04	(.01)
Former Yugoslavia	.00	.01	.16	.11	.06	.11	.06	.02	.05	.02	(.01)	.01	(.01)
Latin America	.02	.01	.01	.01	.01	.01	.02	.01	.02	.00	(.00)	.00	(.01)
USA, Canada, Australia, and New Zealand	.01	.01	.01	.01	.01	.02	.02	.02	.02	.00	(.01)	−.01	(.00)
Other/unknown	.01	.01	.01	.00	.01	.01	.01	.01	.01	.00	(.00)	.00	(.00)
No. of observations	3,058	4,687	4,290	3,224	2,774	2,959	3,046	3,328	3,272	18.4	(32.3)		

NOTE.—The table shows sample means by month of entering the population register (cols. 1–9), local linear estimates for jumps at May 1997 (cols. 10 and 11), and local linear estimates for the average characteristics of the compliers (cols. 12 and 13). Local linear estimates use the edge (triangle) kernel and the optimal bandwidth selection algorithm of Imbens and Kalyanaraman (2012). Standard errors (in parentheses) are clustered by month of arrival. The estimates for jumps at May 1997 are statistically significant for local unemployment rate, legal status Ingrian Finn, legal status family member, and region of birth former Yugoslavia.

Table 2
Average Outcomes by Arrival Cohort

	1/90–4/91 (1)	5/91–4/92 (2)	5/92–4/93 (3)	5/93–4/94 (4)	5/94–4/95 (5)	5/95–4/96 (6)	5/96–4/97 (7)	5/97–4/98 (8)	5/98–4/99 (9)
A: Labor market outcomes (2000–2009):									
Cumulative earnings (€)	112,961	104,701	104,230	94,591	92,508	86,206	77,170	79,478	72,998
Cumulative benefits (€)	34,784	35,902	39,437	41,211	41,606	42,440	43,442	42,458	44,999
Any employment (0/1)	.79	.79	.79	.78	.77	.74	.73	.73	.76
Time to first job (days)	701	1,086	1,396	1,453	1,399	1,290	1,180	1,109	1,110
Cumulative employment (days)	1,794	1,704	1,709	1,620	1,565	1,478	1,335	1,292	1,231
Occupational quality (average native earnings in the occupation, €)	27,809	26,921	27,105	25,976	25,315	25,807	25,600	26,216	25,585
B. ALMP (first 5 years in Finland):									
Total days in ALMP of which:	55	88	117	158	197	212	215	233	246
Traditional training	55	88	108	131	142	128	92	66	61
Immigrant training	0	0	0	3	10	22	50	81	91
On-the-job training	0	0	8	25	44	61	73	86	94
Sanctions (0/1)	.14	.25	.35	.33	.34	.37	.41	.42	.47
Integration plan (0/1)	.00	.00	.00	.00	.01	.02	.05	.50	.68
No. of individuals	3,058	4,687	4,290	3,224	2,774	2,959	3,046	3,328	3,272

NOTE.—The table presents the sample means by month of entering the population register. In panel A, earnings and benefits are summed over the years 2000–2009 and include zeros. Benefits are summed over the immigrant and her or his (possible) spouse and are scaled using an equivalence scale, which assigns a value of 1 to the first household member, 0.7 to other adults, and 0.5 to each child. Any employment is an indicator of the immigrant working at least 2 weeks in a nonsubsidized job during the 2000s. Cumulative employment is the number of days in nonsubsidized employment during the 2000s. Occupational quality is measured as the average native earnings in the occupations that the immigrant held between 2004 and 2009. Panel B shows the ALMP during the immigrants' first 5 years in Finland. Traditional training refers to job-seeking courses and vocational training programs, and immigrant training refers to language training and broader integration courses.

increasingly replaced by language training and broader integration courses.[12] Furthermore, the use of sanctions has increased over time.[13]

V. Results

A. Earnings and Benefits

Table 3 presents our main results. Each entry comes from a separate regression that differs in the object of estimation, the outcome examined, and whether the specification controls for observed characteristics measured at arrival. The first column corresponds to figure 1. As we discussed in the introduction, we find a €7,286 jump in cumulative earnings in the 2000s (first row) and a 35 percentage point increase in the likelihood of receiving an integration plan (second row). Together, these estimates yield a LATE of a €20,916 increase in earnings over the decade (third row). The *p*-values for both the reduced form and the LATE are .080.

One way to put these results into perspective is to note that the LATE corresponds to the average income difference between immigrants who arrived to Finland roughly 3 years apart (fig. 1). Another natural benchmark is the expected outcomes among compliers if they had not received an integration plan. The point estimate suggests that in the absence of an integration plan they would have earned only €44,445 in the 2000s (fourth row). Thus the LATE estimate corresponds to a 47% increase in their earnings.

The second column presents corresponding estimates after controlling for the observed characteristics at arrival. The point estimates are almost identical to the baseline regression. However, the inclusion of the background characteristics improves precision and reduces the *p*-values to .022 for the reduced form and to 0.023 for the LATE.

The third and the fourth columns present similar estimates for social benefits. The patterns documented in figure 2 mirror those of figure 1: immigrants who have lived longer in Finland tend to have higher earnings and thus lower benefits. However, the point estimate suggests a €2,785 decline at the May 1997 cutoff. This implies a LATE of €8,017 corresponding to a 13% decrease in benefits (*p*-value .116). The point estimates from a specification controlling for background characteristics are again virtually identical to the baseline specification, but they are more precise (*p*-value .037).

[12] According to the 2012 guidelines, the curriculum for integration training must include 1,050–1,400 hours of Finnish/Swedish language training, 525–875 hours of training in civic and working life skills, and 175 hours of guidance counseling (Finnish National Board of Education 2012). We have not been able to find explicit guidelines referring to the early 2000s.

[13] We measure sanctions using Labor Administration's registers that report incidences such as "refused to accept a job without an acceptable reason" or "dropped out from ALMP without an acceptable reason" that automatically lead to withdrawal of unemployment benefits.

Table 3
Impact of the Integration Plans on Earnings and Benefits

	Earnings		Benefits	
	(1)	(2)	(3)	(4)
Reduced form	7,286	7,238	−2,785	−2,684
	(4,094)	(3,091)	(1,758)	(1,281)
First-stage	.35	.35	.35	.35
	(.02)	(.02)	(.02)	(.02)
Local average treatment effect (LATE)	20,916	20,702	−8,017	−7,698
	(11,891)	(9,107)	(5,103)	(3,681)
Compliers' expected outcomes in the absence of the treatment	44,445	44,420	61,249	60,810
	(9,962)	(8,900)	(4,314)	(3,049)
LATE relative to the baseline	.47	.47	−.13	−.13
Additional covariates	No	Yes	No	Yes
Bandwidth (months)	42	42	40	40
First-stage *F*-statistic for the excluded instrument	322.0	390.1	318.1	384.5
Observations	16,615	16,615	16,173	16,173

NOTE.—The table shows the local linear estimates for the jump at the May 1, 1997, cutoff using the edge (triangle) kernel and the optimal bandwidth selection algorithm of Imbens and Kalyanaraman (2012). Standard errors (in parentheses) are clustered by month of arrival. Additional covariates are age, age squared, sex, marital status, has children, mother tongue Estonian, type of residence municipality (urban, semi-urban, rural), log regional unemployment rate at the year of arrival, region of residence (20 categories), region of birth (10 categories), legal status (refugee, Ingrian Finn, family member, other/unknown). All time-variant characteristics are measured at the first full calendar year the person is resident in Finland. Observations with zero earnings or social benefits are included in the regressions. Social benefits are measured at the household level using an equivalence scale, which assigns a value of 1 to the first household member, .7 to other adults, and .5 to each child.

There are several reasons to think that these estimates measure the causal impact of the integration plans. First, immigrants' observable characteristics evolve smoothly over the the cutoff point (Section IV.B), and the estimates are not sensitive to controlling for these background characteristics. Second, the research design passes the McCrary (2008) Density Test (see cols. 10 and 11 in the last row of table 1, and fig. A1). Third, we reach similar conclusions regardless of the chosen bandwidth (fig. 3). In fact, our baseline estimates can be regarded as conservative because shorter bandwidths result in much larger effects.

The fourth factor supporting the validity of our research design is that we rarely find discontinuities of similar magnitude at placebo cutoffs. Figure 4 reports the results for such made-up cutoff points using the same approach as our baseline reduced form estimates. That is, we restrict the sample to immigrants who entered the population register between January 1990 and April 1997, set the cutoff point at each month between May 1993 and May 1996, and run local linear regressions controlling for background characteristics using an edge kernel and the Imbens and Kalyanaraman

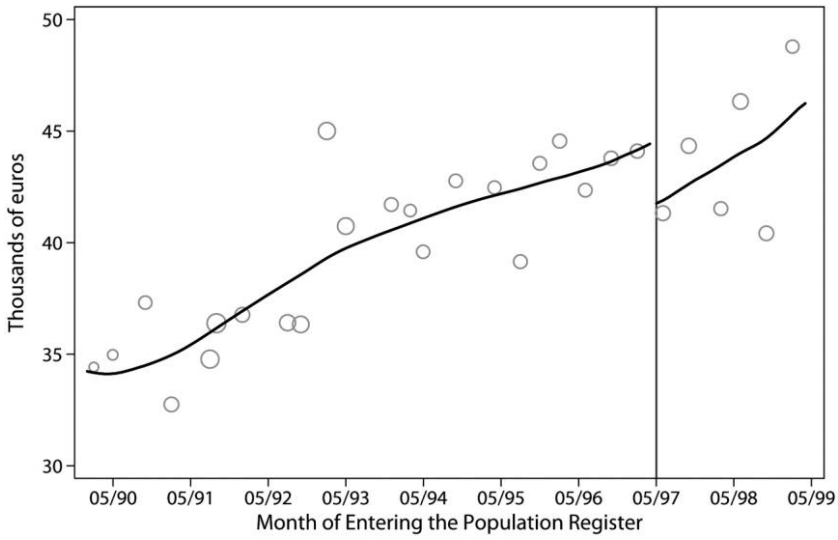

FIG. 2.—Total benefits between 2000 and 2009 by month of entering the population register. Benefits are measured at the household level using an equivalence scale, which assigns a value of 1 to the first household member, 0.7 to other adults, and 0.5 to each child. The lines represent local linear estimates using the edge kernel and the optimal bandwidth selection algorithm of Imbens and Kalyanaraman (2012). The dots correspond to the sample means by 4-month bins.

(2012) bandwidth selection algorithm.[14] Of the 72 resulting estimates, only three are larger in absolute value than our main estimates. Thus our baseline results appear robust.

B. Employment and Occupations

Figure 5 and table 4 examine whether integration plans affect earnings through hours, wages, or both. We find no evidence of impacts on the likelihood of any employment, the time to the first job, or the total days employed. However, it is important to bear in mind that we cannot distinguish between part-time and full-time work. Thus it is possible that our estimates do not capture the full impact on hours. This possibility is further corroborated by the results for occupational quality (measured as the average annual earnings among natives in the same occupation). While the integration plans seem to have helped immigrants to enter occupations

[14] The reason for excluding immigrants entering the population register after May 1997 is that including them would allow the actual reform to affect the estimates in this falsification exercise. The reason for starting from May 1993 and ending at May 1996 is that this leaves preperiods and postperiods comparable to our main estimates.

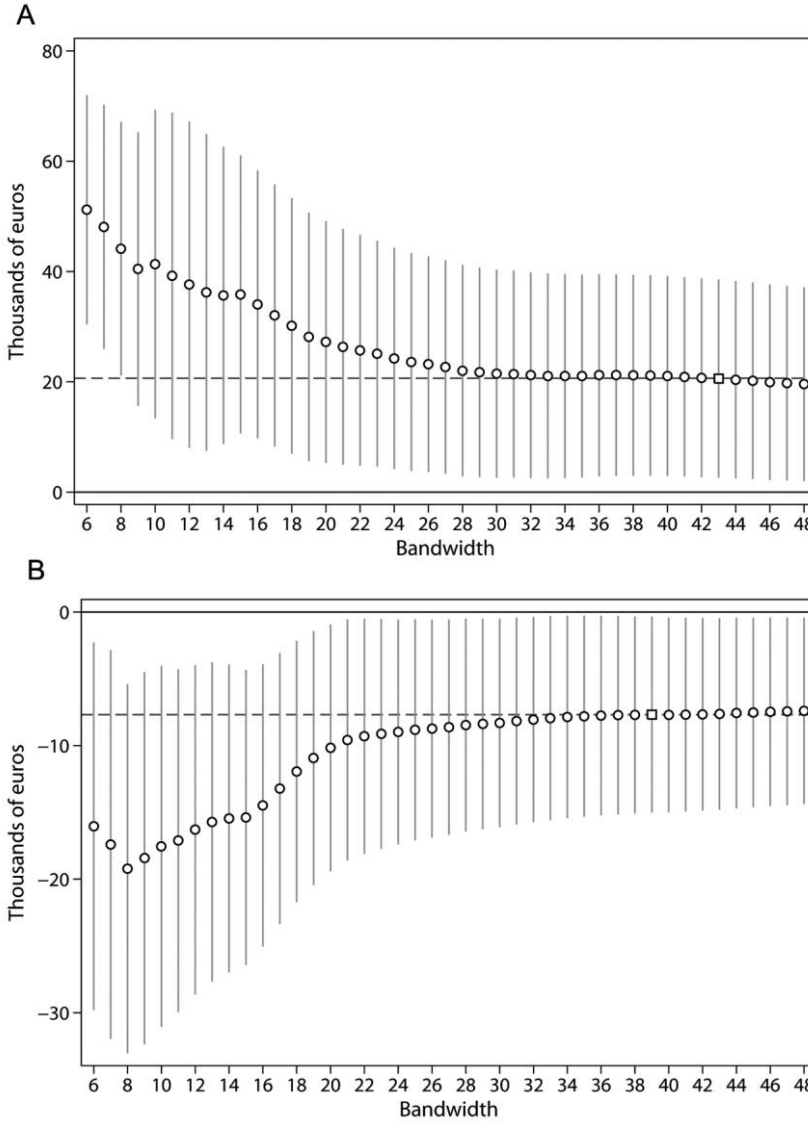

FIG. 3.—Sensitivity to bandwidth: *A*, earnings; *B*, benefits. Two-stage least squares estimates (*y*-axis) using alternative bandwidths (*x*-axis) and controlling for background characteristics (see the note in table 3). The dashed lines correspond to the baseline estimates reported in table 3, row 3, columns 2 and 4.

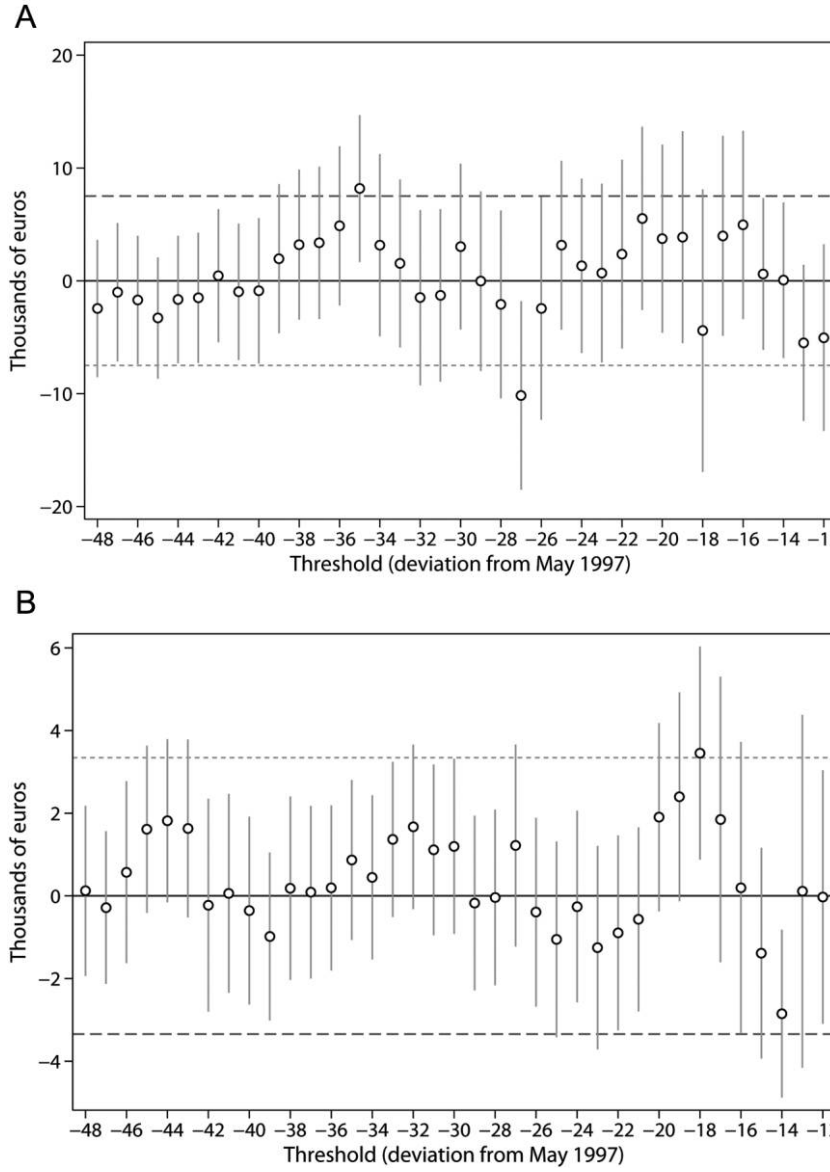

FIG. 4.—Discontinuities at other cutoffs: *A*, earnings; *B*, benefits. Reduced form local linear estimates using the edge kernel and the optimal bandwidth selection algorithm of Imbens and Kalyanaraman (2012) for cutoff dates away from May 1, 1997. The horizontal lines show the jump at the May 1997 threshold when using an identical approach.

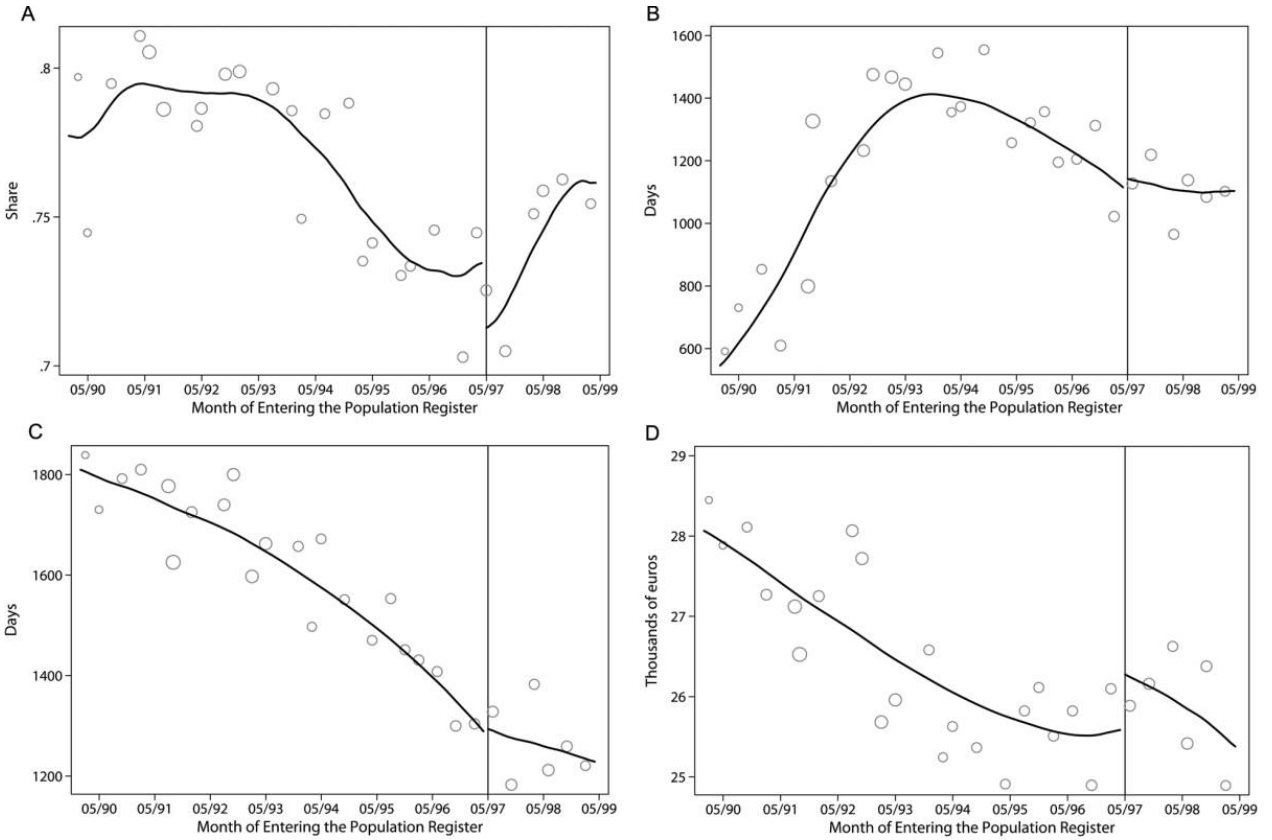

FIG. 5.—Employment and occupations by month of entering the population register: *A*, any nonsubsidized employment between 2000 and 2009; employment; *B*, days between arrival and starting in the first nonsubsidized job; *C*, days employed in nonsubsidized jobs between 2000 and 2009; *D*, average annual earnings of natives in the occupations that the immigrant held between 2004 and 2009. 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 sample means by 4-month bins.

Table 4
Impact on Employment and Occupations

	Any Employment		Days to First Job		Days Employed		Occupational Quality	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reduced form	−.02	−.01	39	45	15	8	696	582
	(.02)	(.01)	(76)	(60)	(48)	(37)	(332)	(346)
First-stage	.33	.32	.34	.34	.35	.35	.36	.36
	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)
Local average treatment effect (LATE)	−.07	−.04	115	133	42	24	1,860	1,537
	(.05)	(.04)	(224)	(178)	(137)	(105)	(892)	(902)
Compliers' expected outcomes in the absence of the treatment	.78	.76	1,430	1,400	1,061	1,078	23,033	23,381
	(.05)	(.04)	(202)	(169)	(108)	(91)	(824)	(861)
LATE relative to the baseline	−.09	−.05	.08	.10	.04	.02	.08	.07
Additional covariates	No	Yes	No	Yes	No	Yes	No	Yes
First-stage *F*-statistic for the excluded instrument	260.2	251.8	356.0	445.3	323.6	392.4	476.4	589.4
Bandwidth (months)	17	17	28	28	42	42	54	54
No. of observations	9,486	9,486	10,851	10,851	16,615	16,615	14,021	14,021

NOTE.—The table shows local linear estimates for the jump at the May 1, 1997, cutoff using the edge (triangle) kernel and the optimal bandwidth selection algorithm of Imbens and Kalyanaraman (2012). Standard errors (in parentheses) are clustered by month of arrival. Any employment is an indicator of the immigrant working at least 2 weeks in a nonsubsidized job during the 2000s. Cumulative employment is the number of days in nonsubsidized employment during the 2000s. Occupational quality is measured as the average native earnings in the occupations that the immigrant held between 2004 and 2009. For additional covariates, see the note to table 3.

associated with 6%–8% higher earnings, this effect is not sufficient to account for the full impact on annual earnings.

C. Training and Sanctions

Figures 6 and 7 and table 5 examine how integration plans affected the amount and type of ALMP. Interestingly, these plans did not lead to more training. While there is a secular increasing trend in ALMP, there may be a slight drop at the May 1997 threshold. The baseline specification suggests that integration plans decreased training by 55 days or 5% (*p*-value .059). However, conditioning on the background characteristics halves the estimate to 28 days, or 2%, and renders it statistically insignificant (*p*-value .174).

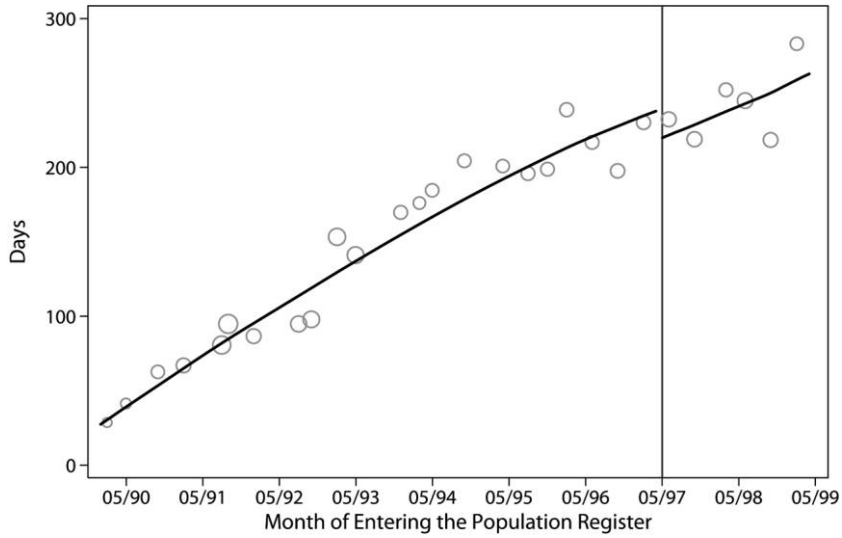

FIG. 6.—Total days in training and subsidized job placements during the first 5 years in Finland by month of entering the population register. The lines represent local linear estimates using the edge kernel and the optimal bandwidth selection algorithm of Imbens and Kalyanaraman (2012). The dots correspond to the sample means by 4-month bins.

Instead, integration plans appear to have changed the content of training. Figure 7 shows that "immigrant training"—language courses and other training specifically designed for immigrants—was increasing already among the cohorts arriving in Finland before May 1997. This trend may be partly due to the reform changing the mix of courses offered. Nevertheless, there is a jump at the May 1997 cutoff that implies a LATE of about 30 days, or a 10% increase, in "immigrant training" due to receiving an integration plan (p-value .004). Correspondingly, "traditional training" started to decline already among cohorts who arrived in Finland before the cutoff, but we find a negative LATE of 30 days, or 7%, at the May 1997 cutoff (p-value .027). The estimates for days of subsidized job placements are small and statistically insignificant.

Splitting the training into just three broad categories may miss much of the possible improvement in the match quality between training and immigrants due to the integration plans.[15] Thus these results are probably best understood as qualitative in the sense that we provide some evidence of the integration plans affecting the content of training. However, we are unlikely to measure the full extent of these effects.

[15] We also experimented with finer categories, but the estimates turn out to be too noisy to be informative.

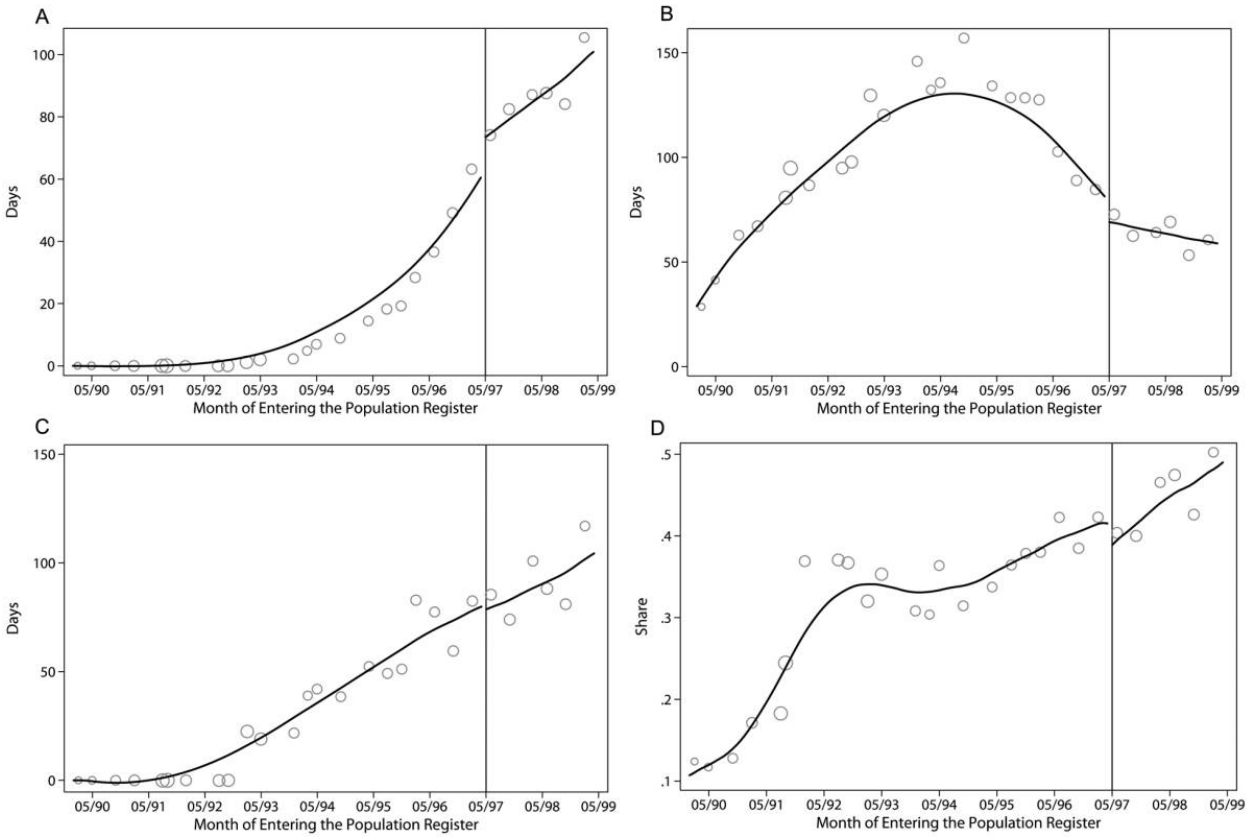

FIG. 7.—Days in training and subsidized job placements during the first 5 years in Finland by month of entering the population register: *A*, days in immigrant training; *B*, days in traditional training; *C*, days in subsidized job placements; *D*, sanctions. The lines represent local linear estimates using the edge kernel and the optimal bandwidth selection algorithm of Imbens and Kalyanaraman (2012). The dots correspond to the sample means by 4-month bins.

Table 5
Impact on Training and Sanctions

	Total Training		Immigrant Training		Traditional Training		On-the-Job Training		Sanctions	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Reduced form	−19.8	−10.3	10.8	10.2	−9.7	−9.5	−2.34	−2.05	−.03	−.02
	(10.0)	(7.4)	(4.6)	(3.7)	(4.9)	(4.1)	(7.45)	(6.42)	(.03)	(.03)
First-stage	.36	.36	.35	.35	.34	.34	.34	.35	.33	.32
	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)
Local average treatment effects (LATE)	−54.6	−28.5	31.2	29.4	−28.3	−27.7	−6.78	−5.91	−.08	−.08
	(28.9)	(20.9)	(12.5)	(10.1)	(15.2)	(12.5)	(21.52)	(18.42)	(.09)	(.08)
Compliers' expected outcomes in the absence of the treatment	430.5	411.9	104.6	105.6	136.6	135.6	135.17	134.83	.58	.58
	(27.8)	(20.5)	(9.9)	(7.5)	(12.1)	(10.1)	(17.11)	(14.73)	(.06)	(.06)
LATE relative to the baseline	−.05	−.02	.10	.10	−.07	−.07	−.02	−.02	−.05	−.04
Additional covariates	No	Yes	No	Yes	No	Yes	No	Yes	Yes	Yes
First-stage *F*-statistic for the excluded instrument	381.2	433.9	315.0	379.3	297.2	343.8	306.9	364.3	259.7	254.0
Bandwidth (months)	71	71	39	39	30	30	35	35	18	18
No. of observations	26,862	26,862	15,782	15,782	13,982	13,982	14,929	14,929	9,920	9,920

NOTE.—The table shows local linear estimates for the jump at the May 1, 1997, cutoff using the edge (triangle) kernel and the optimal bandwidth selection algorithm of Imbens and Kalyanaraman (2012). Standard errors (in parentheses) are clustered by month of arrival. For additional covariates, see the note to table 3.

The reform also allowed immigrants to retain their benefits while participating in appropriate courses outside of the Labor Administration—typically language training at a university or an adult education center. Our data do not contain information about these courses, but according to the Ministry of Labour (2002b), 10%–20% of immigrants who had an integration plan took such courses in the period 1999–2001. Thus this part of the reform was likely to augment the increase in the provision of "immigrant training."

Finally, it seems unlikely that changes in the use of sanctions would be an important mechanism explaining our results. While the share of immigrants facing sanctions increased over time, there is no statistically significant jump at the May 1997 cutoff. This is not surprising, of course, because the reform did not affect the rules governing the use of sanctions.

D. Compliers

Our research design allows us to identify the effects only for a subpopulation of immigrants. Strictly speaking, these compliers are immigrants who entered the population register on May 1, 1997, received an integration plan, and would not have received one if they had entered a day earlier. However, it is unlikely that immigrants arriving at the cutoff would have been particularly responsive or unresponsive to the integration plans. Thus, the compliers are likely to represent one-third of the immigrants who moved to Finland around this period.[16]

It seems reasonable to think that compliers largely belonged to the least succesful third of immigrants, because they had to be out of work at some point between May 1999 (when the reform was launched) and April 2000 (after which they were not eligible due to having lived in Finland for more than 3 years). Accordingly, the estimates for potential outcomes indicate that without integration plans, they would have earned only about €4,500 per year and collected more than €6,000 (equivalence-scaled) benefits per year during the 2000s (table 3). In comparison, the corresponding earnings and benefits among immigrants who arrived between May and December 1997, but did not get an integration plan, were €9,400 and €3,300, respectively. Furthermore, estimates for compliers' background characteristics suggest that they were a few years older than other immigrants and less likely to speak Estonian as their mother tongue, to come from high-income countries, or to have arrived as a family member (table 1).

[16] In the case of a binary instrument and binary treatment (as in our application), the first-stage estimates correspond to the population share of compliers (see, e.g., Angrist and Pischke 2009). See also Lee and Lemieux (2010, Sec. 3.3) for a discussion of how regression discontinuity estimates can be interpreted as a weighted average treatment effect across all individuals, where the weights are proportional to the ex ante likelihood that an individual's realization of the forcing variable will be close to the threshold.

These observations suggest that the compliers are immigrants who are the most likely to make a negative net contribution to public finances. Thus our estimates clearly correspond to a policy-relevant group. However, it is important to bear in mind that the results are not necessarily informative about the impacts of the integration plans for immigrants whose prospects in the Finnish labor markets were more favorable.

E. Costs and Benefits

We end by discussing the costs and benefits of the integration plans. While our data do not allow us to conduct a full cost-benefit analysis, we argue that the reform was likely to yield high returns to public investment. According to the point estimates, the integration plans increased cumulative gross income by more than €20,000 and decreased (equivalence-scaled) cumulative benefits by roughly €8,000 per immigrant during the 10 year follow-up period. We find little impact on the days of training provided by the employment offices. Thus the costs of the reform appear to be due entirely to the increase in training outside of the Labor Administration and the time that caseworkers and interpreters spent in the preparation and monitoring of integration plans.

A typical integration plan takes about 1.5 hours to prepare and corresponds to a cost of roughly €100 (Ministry of Labour 1999). Only 10%–20% of the compliers participated in training outside of the Labor Administration (Ministry of Labour 2002b). Even if all of them had spent, say, an additional 250 days in training outside of the Labor Administration, the impact of the integration plans on earnings would be an order of magnitude larger than the cost of the reform.[17] Furthermore, the returns to the integration plans may continue to accumulate beyond our follow-up period—conceivably even over future generations—while the costs are concentrated on the first years since migration.

Of course, even if replacing the old system with a new one appears extremely cost efficient, it does not mean that Finnish integration policy is free. An average complier spent roughly 400 days in training (table 5), corresponding to a cost of roughly €15,000. Unfortunately, our research design does not allow us to properly evaluate returns to this investment. However, we note that an incremental change in the way immigrants were trained yielded gains that are of a similar magnitude as the total costs of the training.

[17] The 250 days example is chosen purely for illustration, and we expect that the true number is much smaller. The rough cost calculation is based on a Ministry of Labour (2002a, table 12) estimate that the average cost price of preparatory training was €22.6 per day in 2002 and a Ministry of Labour (2003) estimate that the cost price of training courses corresponds to 60% of the overall expenditure. This implies a cost of (€22.5/0.6) = €37.5 per day. Assuming a 20% participation rate in training outside of the Labor Administration and 250 days of training per participant yields a cost estimate of €37.5 × 0.2 × 250 = €1,875.

VI. Conclusions

This paper presents evidence that individualized integration plans had a large positive impact on the earnings of disadvantaged immigrants in Finland. These gains appear to accrue from a more efficient use of existing resources. We find no evidence of the reform having an impact on the total amount of training. Instead, it seems to have increased the provision of training specifically designed for immigrants while scaling down traditional training.

These findings point toward two broader lessons. First, they show that it is possible to create interventions that affect the integration of disadvantaged immigrants. Our LATE estimates measure the impact of the integration plans among the one-third of immigrants whose earnings would have been very low in the absence of the intervention. Thus our findings are particularly relevant for many European countries, where a large fraction of immigrants remain dependent on social benefits even after a prolonged stay in the host country. Implementing or maintaining similar integration programs elsewhere is clearly feasible. Indeed, many features of the Finnish policy are similar to the policies in other European countries and in Canada. An important question for future research concerns the extent to which these findings generalize to other settings such as the US labor market.

Second, the results shed light on the question of what kind of training governments should offer to immigrants. Our results suggest that an important component of the reform was a reallocation of resources away from traditional active labor market programs (ALMP) toward training specifically designed for immigrants, particularly language courses. Nevertheless, we do not advocate a simple interpretation that "language training works" (although this may be true in many contexts). Rather, we interpret our findings as suggesting that a focus on improving the match quality between immigrants' preexisting skills and the training offered may substantially improve the efficiency of ALMP.

Finally, it is important to acknowledge that while the integration plans created impressive effects, they were no panacea. Even with the help of an integration plan, the average annual earnings of the compliers was just €6,500 between their third and thirteenth year in Finland. Thus, there remains much scope for future research and policy experimentation.

Appendix

Table A1
Distribution of Immigrants across Regions at Arrival

	Arrival Cohort									Discontinuity at May 1997		Compliers	
	1/90–4/91 (1)	5/91–4/92 (2)	5/92–4/93 (3)	5/93–4/94 (4)	5/94–4/95 (5)	5/95–4/96 (6)	5/96–4/97 (7)	5/97–4/98 (8)	5/98–4/99 (9)	(10)	(11)	(12)	(13)
Uusimaa	.47	.42	.43	.50	.50	.50	.54	.52	.47	−.02	(.02)	.39	(.04)
Varsinais-Suomi	.09	.10	.11	.10	.12	.13	.12	.13	.14	.00	(.01)	.12	(.01)
Satakunta	.02	.03	.03	.02	.01	.02	.01	.02	.03	.00	(.01)	.01	(.01)
Kanta-Häme	.03	.03	.03	.03	.03	.03	.02	.02	.03	.00	(.01)	.02	(.01)
Pirkanmaa	.06	.05	.06	.08	.10	.10	.10	.10	.10	.00	(.01)	.08	(.02)
Päijät-Häme	.04	.05	.04	.04	.03	.05	.05	.06	.06	.01	(.01)	.08	(.01)
Kymenlaakso	.02	.04	.03	.03	.03	.03	.03	.04	.04	−.01	(.01)	.02	(.01)
South Karelia	.03	.03	.02	.02	.03	.03	.05	.05	.05	.00	(.01)	.06	(.02)
South Savo	.04	.02	.04	.03	.02	.02	.02	.02	.03	.01	(.01)	.02	(.01)
North Savo	.03	.04	.03	.03	.03	.03	.03	.03	.04	.00	(.01)	.03	(.02)
North Karelia	.02	.04	.04	.03	.02	.04	.03	.03	.04	.02	(.01)	.04	(.01)
Central Finland	.04	.04	.04	.05	.05	.05	.05	.04	.06	−.01	(.01)	.04	(.01)
South Ostrobothnia	.02	.01	.01	.01	.01	.01	.01	.01	.01	.00	(.00)	.00	(.00)
Ostrobothnia	.03	.04	.04	.04	.03	.04	.04	.04	.04	.00	(.01)	.04	(.02)
Central Ostrobothnia	.00	.01	.01	.00	.01	.01	.00	.01	.01	.01	(.00)	.00	(.00)
North Ostrobothnia	.03	.04	.04	.04	.04	.03	.03	.04	.05	.00	(.01)	.04	(.01)
Kainuu	.01	.01	.01	.01	.00	.01	.01	.01	.02	.00	(.00)	.01	(.01)
Lapland	.02	.02	.03	.03	.02	.02	.03	.02	.03	.00	(.00)	.02	(.01)
Eastern Uusimaa	.01	.05	.03	.02	.02	.02	.01	.02	.02	.01	(.00)	.00	(.01)
Åland	.00	.00	.00	.00	.00	.00	.00	.01	.00	.01	(.00)	.00	(.00)
Individuals	3,058	4,687	4,290	3,224	2,774	2,959	3,046	3,328	3,272	18.4	(32.3)		

NOTE.—The table presents sample means by month of entering the population register (cols. 1–9), local linear estimates for jumps at May 1997 (cols. 10 and 11), and local linear estimates for the average characteristics of the compliers (cols. 12 and 13). Local linear estimates use the edge (triangle) kernel and the optimal bandwidth selection algorithm of Imbens and Kalyanaraman (2012). The estimates are statistically significant for Central Ostrobothnia and North Karelia.

Table A2
Most Common Course Types in Traditional and Immigrant Training

Course Type	Total Days	%	Most Common Course Title
A. Traditional training:			
Preparatory training	209,988	.54	Job-seeking training
Welders	11,436	.03	Course to obtain a welder certificate
Machinists	9,538	.02	Machinist course
Salespersons	10,822	.03	Course preparing for vocational studies in retail
B: Immigrant training:			
Language training	224,918	.51	Finnish language course for immigrants
Integration training	216,953	.49	Integration course for immigrants

NOTE.—The table shows days in training during the first 5 years in Finland for the most common course types among immigrants arriving in 1997.

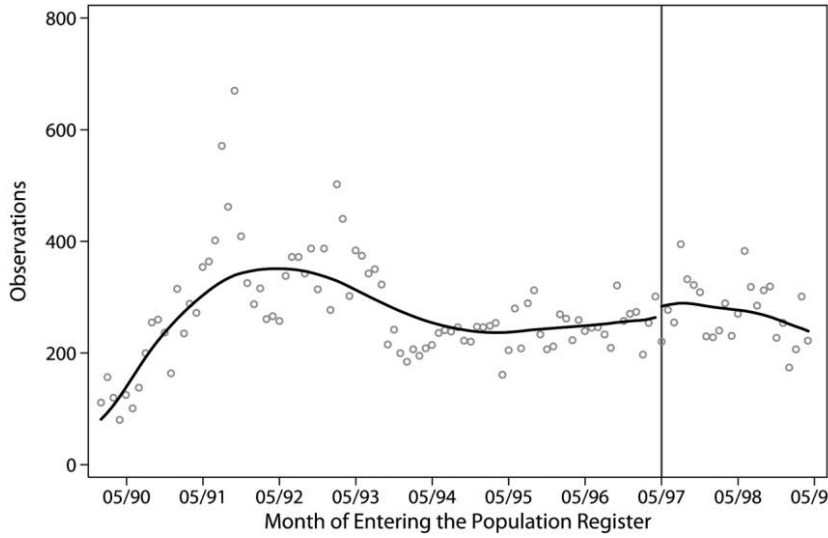

Fig. A1.— Observations by month of arrival. The lines represent local linear estimates using the edge kernel and the optimal bandwidth selection algorithm of Imbens and Kalyanaraman (2012). The dots correspond to the number of immigrants entering the population register per month.

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