Online Appendix to "Assimilating Immigrants: The Impact of an Integration Program"

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A Robustness Checks

A.1 Changes in the Size of the Arrival Cohorts

We start by examining whether the number of observations changes abruptly at the May 1997 threshold. This exercise is motivated by the standard concern in 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 A1, 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.

A.2 Placebo Thresholds

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 May 1993 and May 1998, and examine whether outcomes measured five years later differ between those arriving before and after the threshold. Figure A2 reports the results. There is a clear pattern where estimates around the true threshold of May 1997 resemble our real estimates. Furthermore, a few estimates for the placebo thresholds in early 1998 for social benefits also resemble our real estimates.

We take these results to support our identification strategy. This conclusion is based on two observations. First, one 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. Second, Figure A2 presents a total of 288 estimates. Thus it would be surprising if, purely by chance, we had *not* observed a few estimates such as those for social benefits in the early 1998.

A.3 Alternative Parameterizations

We next turn to the parameterization. We acknowledge that the consistency of our baseline estimates requires the polynomials of the running variable in equations (4) and (5) to be reasonable proxies 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 A1 reports the main estimates using higher order polynomials. The point estimates are remarkably stable across these specifications. However, more flexible functional forms lead to substantially less precise estimates.

A.4 Local Linear Estimates

We have chosen to use a parametric specification for our baseline estimates due to the relatively small sample size. Alternatively, we could have reported local linear estimates using only observations close to the threshold. In practice, we need to widen the observation window in order to obtain sufficiently large sample sizes for meaningful analyzes. Figure A3 reports point estimates and 95% confidence intervals for 2SLS estimates when we gradually increase the bandwidth from 12 to 24 months. The estimates are for 2003 outcomes and control for the same variables as the parametric estimates reported in Table 2. Results for other years and for specifications without covariates are qualitatively similar.

Figure A3 illustrates that in this application one would need to use wide bandwidths in order to obtain sufficient statistical power to reveal even very large effects. However, using such bandwidths would raise the question on whether the approach can truly be consider local. Thus we conclude that given the sample size, the most reasonable approach is to use explicitly parametric approach. In any case, the results reported in Figure A3 lead to similar conclusions as the baseline estimates.

A.5 Outmigration

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. ITT estimates for a dummy for leaving Finland by the end of 2003 are 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, respectively.

A.6 Alternative Estimation Samples

Our final robustness check concerns the estimation sample. The baseline results are obtained from a sample where we have excluded immigrants with extraordinary large income as well as those who did *not* experience unemployment or received 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 A1 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 LATE estimates should not be affected by the inclusion of the 'never-takers'. In line with this prediction, the LATE 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 anything, the results suggest a larger impact on annual earnings and disposable income, while the point estimates for employment and social benefits are close to the baseline estimates.

B More on Training

B.1 Graphical Analysis of the ITT Effects on Training

Figure A4 plots the days of training during the first six years in Finland by month of arrival. While Figure A4a shows an increase in the investments in immigrant training over the 1990s, we find no evidence on the reform having

an impact on the total amount of training days. However, Figures A4b to A4d suggest that it may have changed the content of training.

As we discuss in the main paper, these estimates are sensitive to the parametrization of the running variable. Particularly, we acknowledge that local linear estimates tend to be smaller than the parametric estimates. They are also very imprecise. For example, the LATE estimate for days in language training is 25 with a standard error of 40. That is, the 95% confidence interval for a local linear estimator using a 12 month bandwidth is $[-53, 104]$ days. In comparison, the estimates presented in panel B of Table 3 suggest that the compliers would have spent 6–55 days in training in the absence of the integration plans. Again, these results illustrate that our sample is too small for allowing meaningful local analysis. However, as the pre-reform trends of training are not as well approximated with a linear function (in comparison to employment, earnings and benefits), the estimates for training warant more caution than our main results.

B.2 Distribution of Training Days

In order to provide further descriptives of training, Figure A5 plots the distribution of training days within the Labor Administration during the first six years since immigration among two cohorts in our baseline data. It is important to acknowledge that our sample size is not sufficient for estimating the full distributions for compliers at the threshold. Thus it may not be approriate to draw strong conclusions form Figure A5 with regard to the impact of the reform on the distribution of days in training.

With this caveat in mind, Figure A5a shows that roughly three quarters of the sample received some training during their first years in Finland and that the amount of training varied widely across immigrants. Figures A5b to A5f plot the distribution of training by content. Only a quarter of the immigrants in the earlier cohort received some language training, while half of the later cohort did. On the other hand, participation in other training designed for immigrants was slightly larger among the later cohorts, while they were much

less likely to participate in other preparatory training. The distributions of days in vocational training and in subsidized work practice are very similar for both arrival cohorts.

C Background Characteristics of the Compliers

Table A2 reports the means of the background charateristics of the entire immigrant population enterning the population register between 1996–1998 and estimates on the characteristics of the compliers. We employ two approaches to estimate the latter. First, we use Frandsen, Frölich, and Blaise (2010) adaptation of Imbens and Rubin (1997) and Abadie (2003) to a RD setting to get

$$\mathbb{E}\left[X_i | D_{i1} \geq D_{i0}, R = r_0\right] = \frac{\lim_{r \to r_0^+} \mathbb{E}\left[X_i D_i | R = r\right] - \lim_{r \to r_0^-} \mathbb{E}\left[X_i D_i | R = r\right]}{\lim_{r \to r_0^+} \mathbb{E}\left[D_i | R = r\right] - \lim_{r \to r_0^-} \mathbb{E}\left[D_i | R = r\right]} \tag{A1}$$

We estimate (A1) by 2SLS as described in Section 3 of the main paper, but using $X_i D_i$ as the dependent variable. Second, we follow Abadie (2003) and estimate

$$\mathbb{E}\left[X_i | D_{i1} \geq D_{i0}\right] = \frac{\mathbb{E}\left[\kappa_i X_i\right]}{\mathbb{E}\left[X_i\right]} \tag{A2}$$

where

$$\kappa_i = 1 - \frac{\left[1 - 1\left(r_i \geq r_0\right)\right] D_i}{1 - \Pr\left(r_i \geq r_0 | X_i\right)} - \frac{1\left(r_i \geq r_0\right)\left(1 - D_i\right)}{\Pr\left(r_i \geq r_0 | X_i\right)} \tag{A3}$$

The difference between the two approaches is that Abadie's kappa-weighting scheme does not condition for compliers *at the threshold*. Since the instrument, $1\left(r_i \geq r_0\right)$, is a deterministic function of the running variable, equation (A3) is not defined conditional on r_i as one of the denominators would always be zero. On the other hand, the distribution of some background variables is quite uneven over arrival cohorts—for example, refugees from some area tend to arrive only during a certain period—which makes the estimation of (A1) sensitive to the parametrization.

In order to assess the robustness of the results, Table A2 reports estimates

from both approaches. We estimate equation (A1) using 2SLS and a linear specification. Equation (A2) is estimated in two steps using a third order polynomial to estimate the probabilities in (A3). All standard errors are block bootstrapped with 500 replications using the month of arrival as the level of clustering. The estimates suggest that in comparison to the overall immigrant population, the compliers were more likely to be refugees, to come from the former Soviet Union, the former Yugoslavia, Africa and Asia and to have an immigrant spouse. These differences also show up in the compliers being less likely to come from Western Europe or to have a native spouse.

D Details of the Cost-Benefit Calculations

Cost of Training We estimate the average cost of training using average cost information from administrative reports. Ministry of Labour (2002, Table 12) reports an average cost price of 22.6 euro per day in preparatory training and 27.5 euro per day in vocational training in 2002. In order to translate this to the overall expenditure, we use the estimate of Ministry of Labour (2003) suggesting that the cost price corresponds to 60 per cent of the overall expenditure (excluding benefits paid during training). This implies cost of (22.5/0.6) = 37.5 euro per day for preparatory training and (27.5/0.6) = 45.8 euro per day for vocational training. Multiplying these cost estimates with the point estimates for the number of days in training (Panel A of Table 3) yields 12,803 euro for linear specification and 12,286 euro for the quadratic specification.

Benefits during Training Our estimate for the benefits received during training is derived from the administrative register of the Social Insurance Institution of Finland. According to these records, the mean daily benefit paid to immigrants who had an integration plans was 25.5 euro during their participation in active labor market policies. Multiplying this with the estimated number of days in training yields (25.5*336) = 8,568 euro.

Impact on Gross Earnings We evaluate the impact of receiving an integration plan on the mid-term gross earnings using the same RD design as for the other outcomes. We sum up over all earnings between 2000 and 2003 and use a 5% discount rate to calculate the present value in 1999. The baseline point estimate is 20,698 with a standard error of 11,137 euro. Once we control for background characteristics the a point estimate drops to 15,199 euro with a standard error of 10,170 euro.

The very large standard errors in the estimates reported above are due to the high variance and skewness of the earnings distribution. Figure A6a present the results using trimmed samples. For a reference, the rightmost estimate and 95% confidence interval correspond to the full sample. The next estimate and confidence interval immediately to the left is from a sample exluding observations in the top percentile of the earnings distribution, the next one from a sample exluding those in the top two percentiles and so forth. All specifications control for background characterstics. The point estimates from a trimmed sample are slightly larger and more precise. For example, dropping those in the the top five percentiles yields a point estimate of 21,032 with standard error 7,777.

Impact on Taxed and Benefits Figures A6b to A6f present similar estimates for taxes and benefits. In Figure A6b, we use the 1999 present value of the sum of income taxes during 2000–2003. Again, trimming the sample increases the point estimates and reduces the confidence intervals. Once we drop the top three percentiles, the estimates stabilize at around 5,000 euro.

Figures A6c and A6d present similar analysis for social benefits. Many benefits are granted to a household and thus we present results both for the benefits received by the immigrant himself and the sum over all benefits received by the immigrant or his spouse. Since our aim is to evaluate the absolute costs and benefits, we do *not* apply the OECD equivalence scale to the household level measures (see Section 5.2 of the main paper for discussion). The estimates suggest that the integration plans dereased benefits by 10,000–20,000 euro between 2000–2003.

Figures A6e and A6f present the results for the net impact on direct taxes and benefits during 2000–2003. We find a roughly 15,000 net effect at individual and roughly 20,000 euro net effect at household level. Thus the medium-term impact on govermnet budget appears to be sufficiently large to fully cover the cost of the training. Unfortunately, our data do not include the training days of the spouse and thus we cannot provide an estimate for the costs at household level.

References

Abadie, A. 2003. "Semiparametric Instrumental Variable Estimation of Treatment Response Models." *Journal of Econometrics* 113 (2):231–263.

Frandsen, B., M Frölich, and M. Blaise. 2010. "Quantile Treatment Effects in the Regression Discontinuity Design." Mimeo, MIT.

Imbens, G.W. and D.B. Rubin. 1997. "Estimating outcome distributions for compliers in instrumental variables models." *Review of Economic Studies* 64 (4):555.

McCrary, J. 2008. "Manipulation of the Running Variable in the Regression Discontinuity Design: a Density Test." *Journal of Econometrics* 142 (2):698–714.

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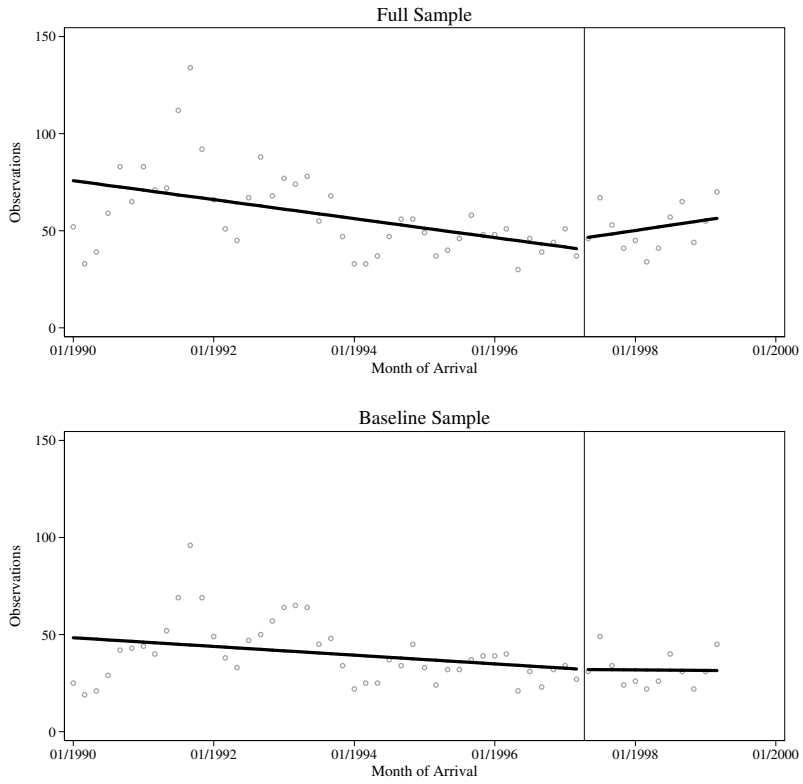
Figure A1: Size of Arrival Cohorts



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 A2: Jumps at non-discontinuity points

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.

Figure A3: Local Linear Estimates, 2003

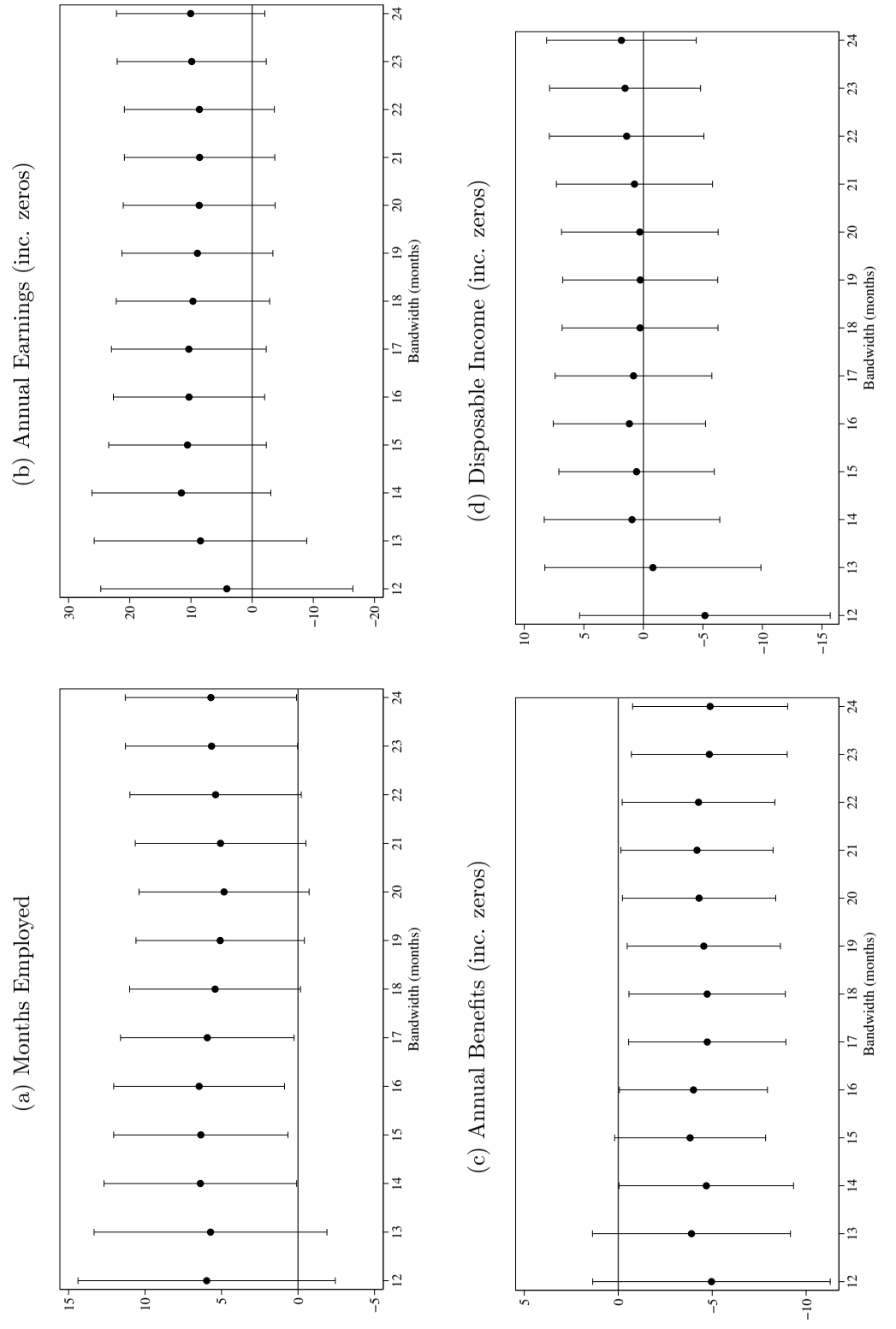
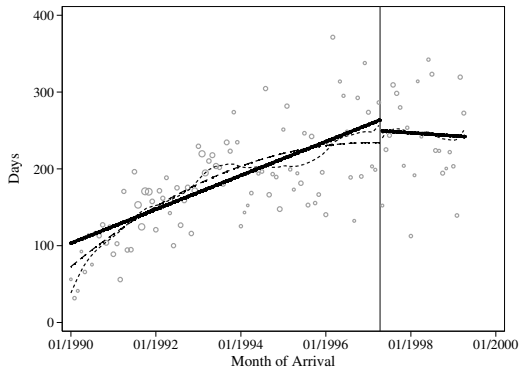
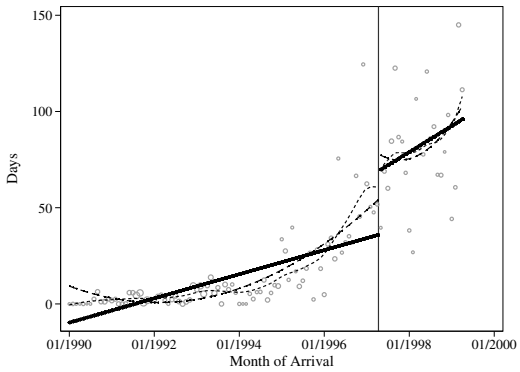


Figure A4: Training Provided by the Labor Administration According to Month of Arrival

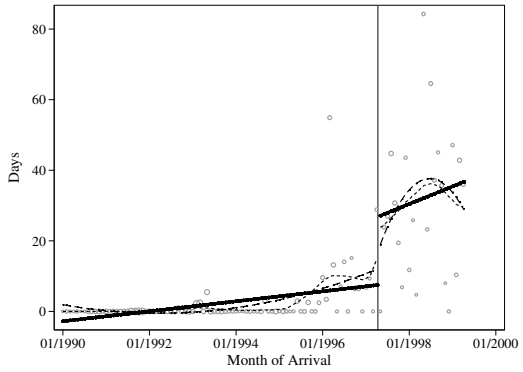
(a) Total



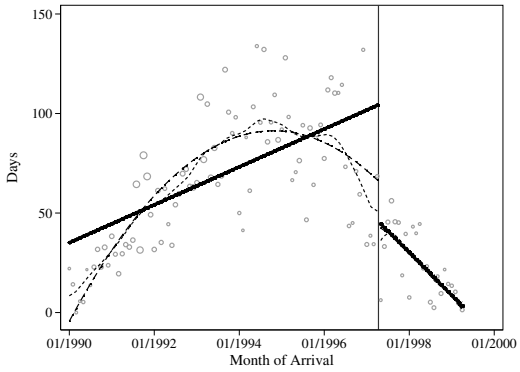
(b) Language Training



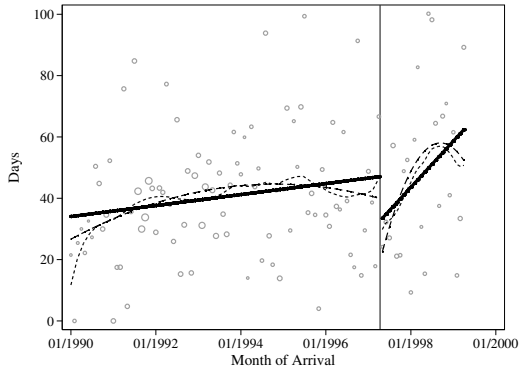
(c) Other Immigrant Training



(d) Other Preparatory Training



(e) Vocational



(f) Job Placement

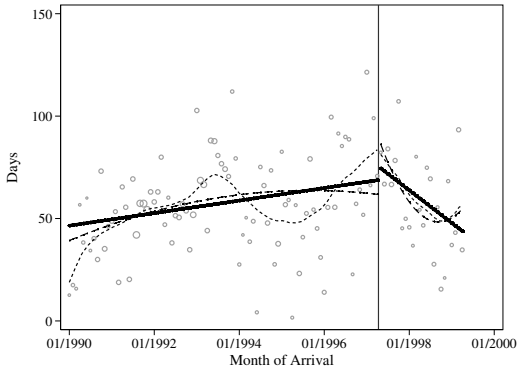
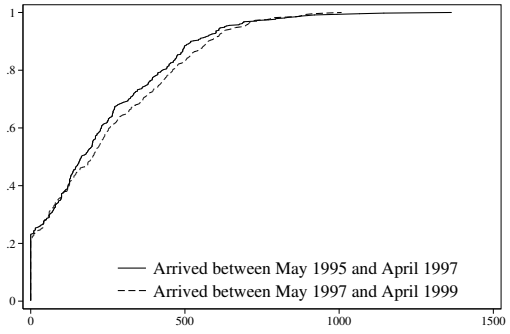
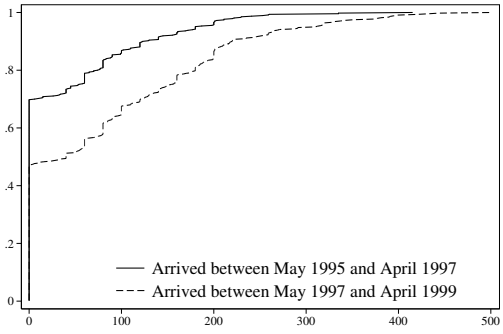


Figure A5: Distribution of Training during the First Six Years to Finland

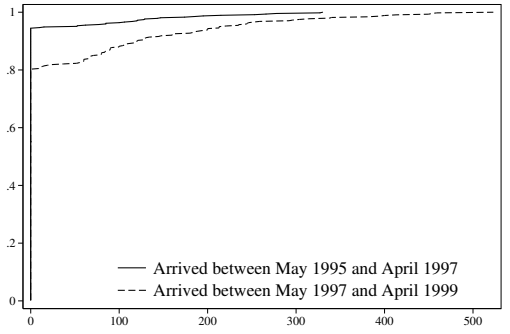
(a) Total



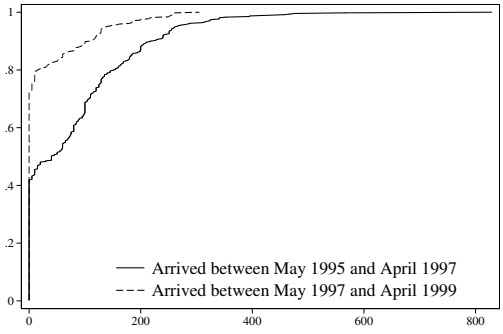
(b) Language Training



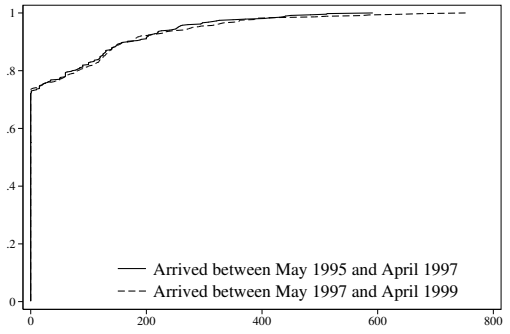
(c) Other Immigrant Training



(d) Other Preparatory Training



(e) Vocational Training



(f) Job Placements

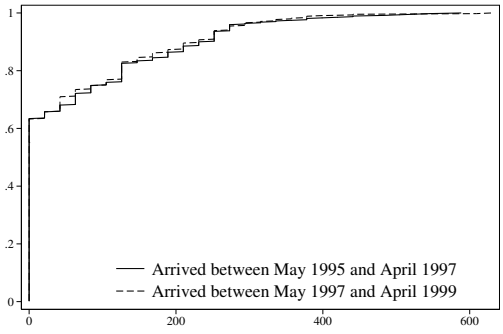
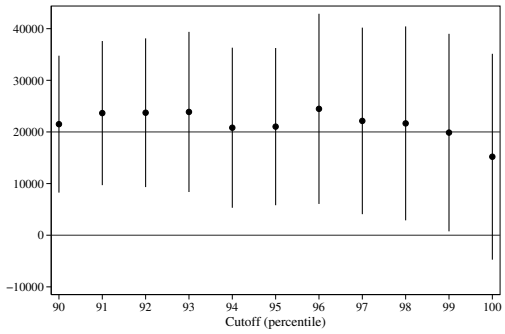
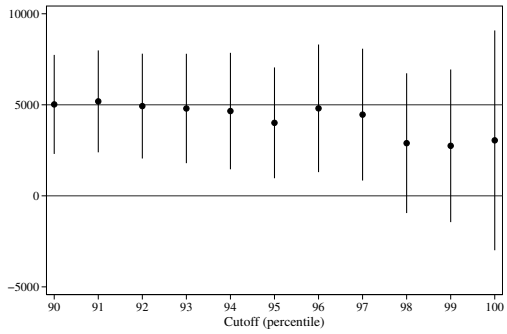


Figure A6: Impact on 2000-2003 Outcomes (Trimmed Samples)

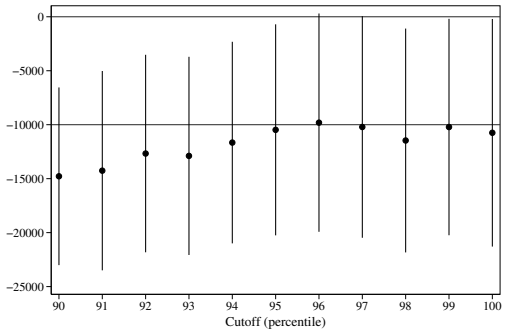
(a) Gross Earnings (individual)



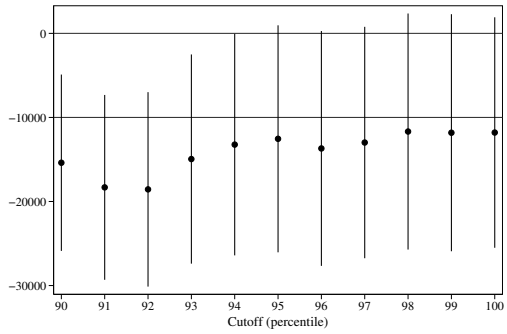
(b) Income Taxes (individual)



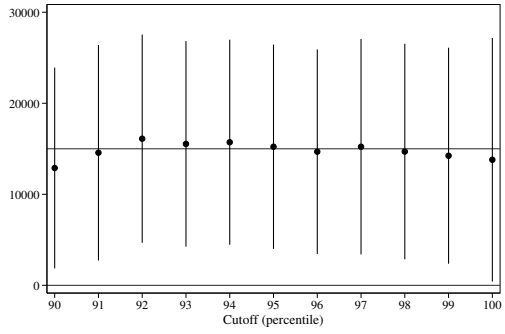
(c) Benefits (individual)



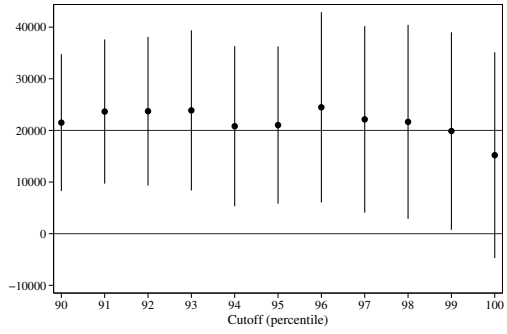
(d) Benefits (household)



(e) Taxes minus Benefits (individual)



(f) Taxes minus Benefits (household)



Note: LATE estimates and 95% confidence intervals for the 1999 present value of the sum of 2000-2003 outcomes. The rightmost estimate in each panel is for the full sample. The remaining estimates are for trimmed samples, see text for discussion.

Table A1: Robustness Checks (2003 outcomes)

	Months Employed		Annual Earnings		Social Benefits		Disp. Income	
	ITT	LATE	ITT	LATE	ITT	LATE	ITT	LATE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Baseline Sample								
Linear	1.78	4.54	2,984	7,603	-1,265	-3,223	549	1,400
specification	(0.53)	(1.52)	(1,529)	(4,028)	(371)	(1,137)	(796)	(2,002)
Quadratic	2.06	4.72	3,325	7,605	-1,077	-2,463	671	1,534
specification	(0.80)	(2.05)	(2,386)	(5,569)	(483)	(1,236)	(1,197)	(2,720)
Cubic	2.11	4.93	3,020	7,043	-1,563	-3,645	-211	-492
specification	(1.09)	(2.97)	(3,475)	(8,422)	(596)	(1,703)	(1,471)	(3,380)
B: Full Sample								
Linear	1.44	4.97	4,189	14,455	-985	-3,397	1,398	4,849
specification	(0.45)	(1.64)	(1,434)	(5,418)	(309)	(1,246)	(687)	(2,528)
Quadratic	1.16	3.81	3,946	12,923	-824	-2,698	823	2,751
specification	(0.58)	(2.03)	(1,880)	(6,769)	(392)	(1,402)	(856)	(2,929)
Cubic	0.99	3.48	4,619	16,295	-1,556	-5,489	-81	-288
specification	(0.69)	(2.75)	(2,679)	(11,169)	(426)	(1,916)	(1,086)	(3,797)

Note: Intention to treat (ITT) and local average treatment effects (LATE) estimates. Standard errors (in parentheses) are clustered according to the month of arrival. 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.

Table A2: Background Characteristics of the Compliers

	All immigrants arriving between 1996–1998 (1)	Compliers: Frandsen et. al. (2010) (2)	Compliers: Abadie (2003) (3)
Age	34.8	35.6	36.4
		(1.7)	(0.6)
Single	0.34	0.33	0.25
		(0.09)	(0.04)
Has a native spouse	0.27	0.20	0.21
		(0.06)	(0.04)
... an immigrant spouse	0.39	0.47	0.55
		(0.09)	(0.04)
Number of children	0.62	0.80	0.88
		(0.20)	(0.08)
Lives in Uusimaa	0.49	0.43	0.42
		(0.12)	(0.04)
Region of birth			
EU15/EFTA	0.22	0.11	0.09
		(0.05)	(0.02)
New EU-members	0.08	0.02	0.06
		(0.03)	(0.02)
form. Soviet Union	0.24	0.36	0.39
		(0.12)	(0.04)
form. Yugoslavia	0.04	0.06	0.07
		(0.03)	(0.03)
Turkey	0.05	0.06	0.04
		(0.03)	(0.02)
Africa	0.09	0.15	0.13
		(0.04)	(0.02)
Asia	0.20	0.25	0.23
		(0.16)	(0.05)
Other/Unknow	0.07	0.0	0.0
		(0.0)	(0.0)
Legal Status			
Ingrian Finn	0.11	0.28	0.17
		(0.09)	(0.03)
Family Member	0.18	0.10	0.28
		(0.09)	(0.04)
Refugee	0.12	0.22	0.25
		(0.13)	(0.04)
Other/Unknow	0.58	0.40	0.35
		(0.08)	(0.04)

Note: Sample means and estimates of (A1) and (A2). See text for discussion.