Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01216

Language Training and Refugees' Integration*

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June 2, 2022

Abstract

We evaluate a Danish reform focused on improving language training for those granted refugee status on or after January 1, 1999. Using a Regression Discontinuity Design we find a significant, permanent, positive effect on earnings. This effect emerged after completion of language classes and was accompanied by additional schooling and higher probability of working in complex jobs, consistent with language training, rather than other minor aspects of the reform, producing the results.

JEL Classification: J60, J24, E64, I30.
Keywords: Refugee Integration, Language Skills, Regression Discontinuity.

*Corresponding author: Mette Foged. The paper is a substantially revised version of NBER WP 26834. This project is conducted as part of the Economic Assimilation Research Network (EARN), generously financed by the Innovation Fund Denmark (grant no. 6149-00024B). Mette Foged also acknowledges funding from EPRN (grant no. 107757). We thank the Ministry of Immigration and Integration for assistance with data and reports on early language training in Denmark and Janis Kreuder for excellent research assistance. The project benefited from feedback and discussions with Oddbjørn Raaum, Tommaso Frattini, Jakob Roland Munch, Torben Tranæs, Matti Sarvimäki and participants at the following seminars and workshops: "Forced Displacement, Asylum Seekers and Refugees", Queen Mary University London; the first "EARN Workshop on Integration", University of Copenhagen; Uppsala; Global Migration Research Center, UC Davis; Danish Center for Social Science Research (VIVE); CReAM, UCL; University of Bristol; Senior Migration Seminar (sites.google.com/view/the-economics-of-migration/senior-seminar); FAIR, NHH; Monash University; ZEW, Mannheim; UCLA; Tilburg; ESEM; European Commission CCME; and AEA Annual Meeting. The project has been approved by the relevant authorities in Denmark, including Statistics Denmark and the GDPR office at University of Copenhagen (journal nr. 514-0101/19-2000).

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https://doi.org/10.1162/rest_a_01216

I Introduction

Improving the labor market integration of refugees could bring large economic returns to them and to their host economy as this group experiences significant employment and wage gaps relative to other immigrants and natives, even decades after arrival (Brell, Dustmann, and Preston, 2020; Fasani, Frattini, and Minale, 2021). What policies may improve such integration is not well known. Different countries have taken different approaches (see Bloemraad and De Graauw (2012); Williamson (2018) for the US and Arendt (2018) for Europe) and many of them have combined several measures, including active labor market support, provision of incentives and teaching of relevant skills. Recently countries in Northern Europe have moved away from policies focused on teaching language skills towards more job-search oriented policies. The evidence in favor of either type of program is scarce. Limited high quality data following refugees over time and lack of clean identification strategies have prevented a causal assessment of the effect of policy interventions on refugees' labor market outcomes.

In this paper we evaluate an important reform that was implemented in Denmark in 1999. The reform entitled the treated refugees to 430 additional hours of language training and improved the funding and the quality of the language training. The new program was mandatory for immigrants who obtained refugee status in Denmark on or after January 1, 1999 (our treatment group) and not for the others. This feature allows us to use a Regression Discontinuity (RD) design focusing on refugees whose status was granted between few months before and after that date. Small additional changes were introduced with the same cutoff, including a one-year temporary reduction in welfare benefits, only for those over age 25 or with children. Additionally the reform progressively introduced a mechanical scheme of placement of refugees across municipalities, which became responsible for providing the integration program.

The quasi-random assignment around the threshold date allows a credible causal estimation of the impact of the reform as a whole. We follow the refugees 18 years and look at their labor market, education and criminal outcomes. First, we find that refugees treated by the reform experienced greater employment probabilities by four percentage points and higher earnings by USD 2,500 (at 2000 price level). These effects emerged after completion of language classes and persisted in the long-run (i.e., 18 years after the reform). As, on average, refugees in Denmark had an initial employment probability gap with average natives of about 65 percentage points and reduced it to about 35 percentage points in the long run, such effect would help reduce the long-run employment gap by about 12% of the total. In terms of earning gaps (equal to about USD 40,000 in the long-run) the policy would reduce it by 5

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percent of total.[1]

Second, we find that treated refugees were more likely to obtain education (in Danish) and to upgrade to jobs with stronger communication requirements. Third, we do not find evidence of an additional short-run impact on employment or earnings for the group subject to the one-year welfare cut. Instead, we find a one-year decrease in disposable income and a one-year increase in the probability of criminal charges and convictions for this group (Andersen, Dustmann, and Landersø, 2019, find similar effects of a welfare cut in 2002). Finally, we analyze and discuss the additional small changes introduced by the reform and show that it is unlikely that they affect our main findings.

This paper contributes to two related strands of research. First, we contribute to the understanding of the role of language in the economic success of immigrants. Early studies established a strong and positive association between language proficiency and the earnings of immigrants.[2] Second, a more recent strand of the literature has focused on identifying the effect of language training on immigrants' labor market outcomes. Three recent papers stand out in this literature. Sarvimäki and Hämäläinen (2016) analyze a reform in Finland that affected immigrants who arrived after May 1, 1997. The Finnish reform increased language and immigrant-specific training and reduced job-search support and produced a significant 47 percent increase in earnings and 13 percent decrease in cumulative social benefits over a ten year period but no change in employment. Lochmann, Rapoport, and Speciale (2019) analyze a program in France that assigns non-EU immigrants to language training based on a threshold in their score in an initial test. They find that 100 hours of language training increased labor force participation by 15 to 27 percentage points two years after completion of classes but no effect on employment (in spite of much larger participation) and no effect on the ability to speak the French language. Finally, Heller and Mumma (2020) evaluate the ESOL (English for Speakers of Other Languages) program exploiting lotteries to enroll in the program that were conducted for first time applicants between 2008 and 2016. They find significant average treatment effects on earnings, comparable to what we find.[3]

[1]Our calculations, using data on Danish natives and refugees 1987-2008.

[2]See for instance Chiswick (1991), Dustmann (1994) and Chiswick and Miller (1995). Similarly, Clausen et al. (2009) analyze the population arriving after the Danish 1999 reform and show that in this group higher language proficiency is correlated with faster transition into employment.

[3]A few additional studies analyze how reforms that change welfare assistance or active labor market policies affected refugees' labor market outcomes. LoPalo (2019) finds that variation in welfare generosity across US states had no significant effect on refugees' employment, while it increased their wages in the long run. A few studies in Denmark look at the effects of a reduction in the welfare benefits for refugees that was introduced by a reform in 2002. They show that this reform produced positive short-term employment effects (Huynh, Schultz-Nielsen, and Tranæs, 2007; Rosholm and Vejlin, 2010; Andersen, Dustmann, and Landersø, 2019). The employment effects disappeared after 7-8 years and were accompanied by adverse effects on crime rates and educational achievement among teenagers in treated households (Andersen, Dustmann, and Landersø (2019)). Joona and Nekby (2012) evaluate a program in Sweden where new immigrants were randomly selected to receive more intensive counselling and find significant employment effects one year after assignment but no evidence of longer-run effects.

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II Refugees in Denmark and the 1999 Reform

The process of refugee settlement in Denmark, around the time of the reform, worked as follows. At first arrival they were registered and placed in accommodation centers and were not allowed to work. When the asylum status was granted, the refugee was placed in a municipality and received housing, social support and employment services.[4] Up to 1999 language training was offered up to 1.5 years after placement.

The first Act on Integration of Immigrants[5] and the new Act on Language Education for Adult Immigrants[6] went into effect on January 1, 1999. Individuals granted refugee status before this date were subject to the old rules, whereas individuals granted status on or after this date were subject to the new rules.

The reform made language training mandatory and more extensive. Non-participants could be financially sanctioned by losing part of their benefits and proof of completion of the program was needed to obtain permanent residence. Potential instruction time was increased from 1,370 to 1,800 hours over an extended period of 3 years. We find no evidence that the greater incentives to attend classes affect the share participating in language training, likely because the language training was considered mandatory even before the reform. The additional hours logged by the treated group, on average, was less than the 430 extra hours for which they would be eligible. At the boundary point (RD estimate), treated refugees accumulated approximately 200 additional hours of training within three years.[7] We also observe that treated and untreated refugees were rarely in the same classes, which suggests that language training differed between the two groups. The reform added a 20-hour additional course on civic education, introduced national tests, and provided resources to increase the qualifications of the teachers. To guarantee the implementation and funding of the training, the municipalities were provided with reimbursement of expenses in the form of a fixed monthly payment per refugee and per month of language services. Those payments were higher in amount and duration for refugees qualifying for the reform to guarantee the expanded services only to treated refugees.

The reform additionally introduced a temporary 29-percent cut in welfare benefits for those older

[4]UNHCR Quota refugees are resettled in Danish municipalities directly from refugee camps abroad. They constitute 10 percent of our sample.

[5]Act 474. In Danish: Lov nr. 474 af 01/07/1998. Lov om integration af udlændinge i Danmark, `https://www.retsinformation.dk/eli/lta/1998/474`.

[6]Act 487. In Danish: Lov nr. 487 af 01/07/1998: Lov om undervisning i dansk som andetsprog for voksne udlændinge m.fl. og sprogcentre, `https://www.retsinformation.dk/Forms/R0710.aspx?id=87625`.

[7]Table 1 of the Online Appendix shows the effect of the reform on participation in language training and on the cumulative hours of language training during the first three years, and the same for employment support. As expected, neither participation nor hours of employment support change around January 1, 1999.

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than 25 or with children (25-percent cut if children were present), which only lasted 13 months.[8] The reform also introduced a principle of centralized and mechanical dispersion of refugees,[9] to be distributed according to county quotas with the goal of achieving a more uniform dispersion (as argued by Azlor, Damm, and Schultz-Nielsen, 2020, who analyze the dispersal of refugees between 1999 and 2010). The municipalities within a county, then, agreed upon the within-county settlement, but if they failed to agree, the central government would determine the municipal quotas. Due to inertia and capacity constraints in suitable housing, this change was slow to take place and did not affect allocation of refugees across municipalities discontinously at the threshold date (Table 1). Finally, as welfare benefits became conditional on participation in the program, which was administered by the initial placement municipality, the incentive to stay there for an extended period of time became stronger. We will analyze the effect of the reform on mobility.

III Empirical Specification

Our empirical approach compares outcomes of those granted refugee status right before (control) and right after (treatment) January 1, 1999 in a regression discontinuity design. The treatment variable, D_i, for individual i, is a deterministic function of the date that refugee status was granted x_i:

$$D_i = 1\{x_i \geq c\},$$

where c is the cutoff date of January 1, 1999. Hence, $x_i - c$ is the time in days between the cutoff date and the date individual i was granted refugee status. Our main specification is as follows:

$$Y_{it} = \alpha + \tau D_i + \beta_1(x_i - c) + \beta_2 D_i(x_i - c) + \varepsilon_{it}. \quad (1)$$

The coefficient τ is the regression discontinuity (RD) estimate and captures the causal intention to treat (ITT) effect of the reform on outcome Y_{it}. The two linear terms $\beta_1(x_i - c)$ and $\beta_2 D_i(x_i - c)$ capture the linear dependence of outcomes on admission dates measured relative to the cutoff date. The intercept α captures the pre-reform mean at the boundary point which we use to re-scale the estimated coefficients into percentage effects. We estimate a separate regression for the outcomes in each year after the reform

[8]In the summer of 1998, the UNHCR stated that the welfare benefit reduction "violates Article 23 of the UN Refugee Convention" (equal public relief and assistance for refugees and nationals). In November 1999, the Danish Prime Minister announced that the reduced benefits did not generate increased employment as intended (Ministry of Interior Affairs, 1999) and dropped it.

[9]The new act on the dispersal of refugees. In Danish: Bekendtgørelse nr. 630 af 25/08/1998; Bekendtgørelse om boligplacering af flygtninge, `https://www.retsinformation.dk/eli/lta/1998/630`.

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using Weighted Least Squares with a triangular kernel. The mean squared error (MSE) optimal bandwidth from Calonico et al. (2019) used in all estimations ranges between one and nine months, and we use heteroskedasticity-robust standard errors (e.g., in Lee and Lemieux, 2010).

IV Data and Balancing Tests

The population we draw from Danish administrative registers are refugees and reunified family members (for brevity we refer to all as refugees) admitted in a four year window around January 1, 1999. We select 18 to 49 year old at admission so that they qualify for the program (18 or older) and are in working age (65 or younger) by 2016, the last year for which we observe our outcomes. These restrictions leave us with 8,558 refugees.[10] Table 1 shows descriptive statistics for them (column 1 and 2) and balancing tests for all the available characteristics at arrival (column 3 and 4).[11]

Panel a describes the density of refugee admissions. The first row shows that the Danish Immigration Service admitted 180 new refugees per month from January 1, 1997 to February 28, 2001. To account for seasonality in refugee admissions, we show in the second row the monthly admissions as deviations from the mean share for that month. The balancing test for both variables reported in column 3 and 4 shows no significant discontinuity around January 1, 1999. Additionally, the estimated discontinuity test of waiting times for asylum status in the last row shows an insignificant and small difference of 17 days. Hence, there is no evidence of manipulation of the processing time for asylum applications around the cutoff.[12]

Panel b of Table 1 provides summary statistics of the characteristics of the refugees as well as estimates of any discontinuity in their characteristics at the date threshold. The average age at admission was 31.5 years; 67 percent were married, 58 percent were male, and the average number of children was slightly above one at the time of admission. The most important country of origin was Iraq (people fleeing Saddam Hussain's regime), representing 44 percent of our sample. 16 percent of refugees were from Afghanistan (escaping the Taliban regime), and the rest came from a range of other countries. Consistent with national origins, 35 percent of the admitted refugees spoke Arabic as their mother tongue, while 9 percent reported Dari (a dialect of Farsi and the most common language in Afghanistan) as their first language. As we do not have information on education from the country of origin, we report

[10] We exclude Somali refugees due to irregularities in the processing of asylum applications for this particular group in the last months of 1998.

[11] Figures 1 to 3 of the Online Appendix shows RD plots for all the variables in Table 1.

[12] The reform was proposed on April 16, 1998, and approved on June 26, 1998. The median waiting time was slightly more than ten months. Hence, the majority of the refugees who were granted asylum near January 1, 1999 had applied before the law was even proposed.

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the level assigned to refugees for their language training based on their skills at arrival. Those with primary schooling or less were typically assigned to Danish 1. Tertiary educated refugees were most often assigned to Danish 3, and Danish 2 was an intermediate group. Finally, we show the percentage of refugees admitted under different channels, such as the Geneva convention, UNHCR quota refugees and family-reunification with existing refugees. Columns 3 and 4 of Table 1 show no evidence of a discontinuity in any characteristics. Each characteristic of the refugees, after controlling linearly for the date of asylum recognition relative to January 1, 1999, is statistically the same for our treated and control group.[13]

Panel c of Table 1 shows the mean and standard deviation for the characteristics of the municipalities where the refugees were initially placed (columns 1 and 2) and the regression discontinuity estimates of the difference in characteristics before and after the cutoff (columns 3 and 4). 33 percent of refugees were placed in urban municipalities and the RD estimate for this characteristic is 0.11. Although not significant, this is a relatively large difference. It is driven by municipalities on the margin between urban and non-urban. When looking at the 17 percent of our sample placed in one of the five largest cities (likely to provide an economic advantage), we observe no difference across January 1, 1999. Additionally, the treated and the control groups are distributed in municipalities with no significant differences in employment and unemployment rates nor in average labor income in 1996. In the last two rows of Table 1 we look at a discontinuity in the number and share of co-nationals already residing in the municipality. This variable has been shown to affect employment rates of newly arrived refugees (Edin, Fredriksson, and Åslund, 2004). Even this variable shows no significant difference across the cutoff. Overall, the balancing tests show a remarkable similarity of observable characteristics of the locations where refugees settled before and after the cutoff date. We return to this when we discuss the channels and mechanisms in Section VI.

The outcomes we analyze are annual earnings, employment, indicators for complex and non-complex jobs, disposable income, criminal charges and convictions, education and mobility. All nominal variables are converted to the price level in 2000 using the Danish consumer price index and converted to USD using an exchange rate of 6.6 DKK/USD. The earnings are gross labor market income, while disposable income is net of tax and includes welfare benefits. The employment is equal to a fraction of a full-time year and is equal to one for a full-time worker. The occupations we define as complex range from managers (*ISCO 1*) to services and sales workers (*ISCO 5*). They require some level of schooling

[13] While there is no evidence of discontinuity at the cutoff in any observable characteristics, Figure 2 in the Online Appendix shows that there are long-term trends in some characteristics by month of arrival. This does not pose a threat to identification as we include a function of month of arrival as a control.

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and use of sophisticated skills either in the form of technical knowledge or literacy and good interpersonal communication skills. The most common complex occupation among the refugees is "Personal and protective service workers" (*ISCO 51*), which employs 21 percent of them. Non-complex jobs are the remaining occupations (ISCO 6 to 9), which require no or basic education. A short period of on-the-job training will often be enough to undertake these types of jobs, e.g. "Sales and service elementary occupations" (*ISCO 91*) and "Machine operators and assemblers" (*ISCO 82*), which represent 16 percent and 12 percent of employment in the sample. The last three outcomes we analyze are dummies for being charged and convicted of a crime, a dummy for having obtained any formal education in Denmark, and a dummy for having left the municipality of initial placement.

V Main Results

Our empirical analysis proceeds in two steps. We first present the aggregate long-run impact of the reform on the labor market integration of refugees. Then we analyse the heterogeneity, the dynamics and additional outcomes, which help us gain insight into which component of the reform affected the outcomes of refugees and how.

V.A The Long-Run Impact of the Reform

We show the average long-run effects of the reform in Table 2. The dependent variable is the average of yearly outcomes between one and eighteen years since obtaining refugee status.

The estimated coefficients in the main specification, reported in Panel a of Table 2, are obtained from local linear estimation of equation (1) with a triangular kernel and the MSE-optimal bandwidth selector from Calonico et al. (2019). The estimates imply statistically and economically significant discontinuities in earnings and employment in complex jobs. The long-run annual earnings level in column 1 increased by USD 2,500 as a result of the reform, which corresponds to a remarkable 34 percent increase in yearly earnings relative to the baseline. Employment in complex jobs in column 3 increased by 9.2 percentage points, more than doubling the baseline employment in those types of jobs. Employment in non-complex jobs decreased by a statistically non-significant 3.6 percentage points. Hence, the extensive margin of employment grew by $(9.2 - 3.6 =)$ 5.6 percentage points. This is slightly more than the increase in full-time equivalents in column 2, which increased by 4.2 percentage points, corresponding to a 23 percent increase relative to the baseline. This estimate is only significant at the 10-percent level.

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Panels b and c of Table 2 test the sensitivity of our main results to alternative functional forms of the running variable. Panel b replaces the linear functions with constants (zero polynomials) and drops the kernel weighting, and therefore, simply gives the treatment-control difference in means within the MSE-optimal bandwidth (ranging between 37 and 81 days). Panel c uses a second order local polynomial on either side of the cutoff date. The coefficients in Panels b are slightly smaller and usually more precise, while the estimates in Panel c are slightly larger and less precise. Simple treatment-control differences weigh all post-reform arrival cohorts equally within the MSE-optimal bandwidth. Higher order polynomials risk to over-fit the data close to the cutoff. We follow recent consensus, exemplified in Gelman and Imbens (2019), and take the local linear specification as our main results. Finally, in Panel d we include control variables. The point estimates decline slightly but remain very similar to the main results in magnitude and statistical significance.[14]

V.B Who Benefited the Most?

Table 3 shows the average labor market integration outcomes in the long-run splitting the sample by characteristics of refugees: their native language, their initial geographical placement and their gender. Panel a shows that separating refugees whose native language uses the Latin alphabet (is closer to Danish) from those using other alphabets, the effects on earnings and employment were entirely on the second group (columns 1 and 2). This suggests that more time spent learning to read and write was particularly beneficial for those learning a language very different from their own. The larger earnings and employment effects for this group seem to come from a significant transition from non-employment to complex occupations (column 3), while users of the Latin alphabet seem to transition from non-complex to complex with no effect on their employment probability. Middle Eastern refugees are less economically integrated than European ones (see Figure 9 in Dustmann et al., 2017). The effects on speakers of non-Latin based languages (often from Middle East) are economically important considering their baseline (a 50 percent increase in their employment probability and an even larger improvement in earnings).

Panel b of Table 3 shows the estimated effects split by municipality of placement. The reform improved all four outcomes more in urban municipalities than in rural ones. This is consistent with the explanation that better language skills allowed the refugee access to more communication-intensive jobs

[14]The Online Appendix presents the following additional evidence on the robustness of our main results: Figure 4 contains RD plots of the outcomes. Figure 5 shows that the local linear specification is robust to bandwidth choices between 150 to 360 days and also stable but less precise for bandwidths between 60 and 120 days. The specification also survive a falsification exercise, where we calculate estimates obtained at arbitrary cutoff dates between 6 to 18 months before and 6 to 18 months after the actual cutoff date (Figure 6 of the Online Appendix). Table 2 shows that attrition is the same for treated and untreated and the characteristics of those who leave the sample do not show significant discontinuity between treatment and control in most cases.

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and those are more prevalent and better paid in urban municipalities. Finally, Panel c shows statistically similar effects of the reform for men and women, possibly marginally stronger for women.

VI Channels and Mechanisms

In this section we test and discuss mechanisms and channels of the reform by analyzing the timing of the effects, the impact among different groups, and additional outcomes.

VI.A Timing and Formal Education

Panels a to c of Figure 1 show the impact of the reform on the main outcomes (earnings, employment and complex job) measured each year after the reform. While we see small to no effects on earnings and employment in the period corresponding to language class attendance (years one to three), a gradual improvement occurred starting in year three. The share employed in a complex job grew significantly from year 4 to year 11 revealing a strong and persistent occupational change towards jobs that are more intense in language and interpersonal skills. This is consistent with the effects being the consequences of better language skills learned.

Improved language skills could also encourage formal educational upgrading in the form of more schooling. Panel d of Figure 1 shows the probability of completing additional education in Denmark for those under 30 years old. A positive and significant progressive increase in the average effect on treated refugees appears, starting in year 7 and continuing in the long-run. Some refugees may have started their education early on while others may have decided to take additional education after loosing their first job, or along their career, and having better language skills may have encouraged them to do so. Towards the end of the analysis window young treated refugees are roughly 15 percentage points more likely to have obtained some education in Denmark relative to the young untreated refugees. Both the occupational transitions and the completion of educations suggest language training as the underlying cause of improvement in long-run labor market integration.

VI.B Geographical Distribution and Mobility

As mentioned in Section II, the reform was supposed to introduce a "mechanical" formula to distribute refugees across municipalities, with the goal of spreading them more uniformly across Denmark. We find no evidence that this altered the sorting of refugees right at the cutoff (Panel c of Table 1). However, there is evidence of a trend in placement away from large urban centers. When comparing one year

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before to one year after the reform (1997 to 2000), there is evidence that the redistribution formula did produce, gradually, a reduction in urban location and an even stronger and significant decrease in placement into the five largest urban municipalities with large pre-existing immigrant populations (Table 3 of the Online Appendix). The change appears to be slow and gradual and it does not affect the RD identification we use in this analysis. As a final check that our results are indeed unaffected by progressive changes in the placement policy, we reproduced all the main results in Table 2 with municipality fixed effects (Table 4 of the Online Appendix). The employment and earnings effects are, if anything, slightly stronger holding the initial placement fixed.

Panel e and f of Figure 1 show the effect on the probability of having moved from the initial municipality, separately estimated in each year after placement. Panel e shows the effects for all refugees, while Panel f splits the impact by initial placement in rural or in urban municipalities. The pattern is interesting and quite clear. The time profile of the estimates implies a delay in mobility. This is consistent with the idea that refugees in order to attend the additional language training in their municipality of placement, were more likely to stay during the first three to four years to take advantage of this service.[15] Panel f of Figure 1 shows that the initial mobility gap between treated and control refugees was larger for those assigned to rural municipalities. This is consistent with the idea that a larger share of those initially settled in rural locations delayed their mobility to attend the language class. Some of them would likely have relocated earlier to seek economic opportunities if language courses were not expanded and made mandatory. The treated refugees placed in urban areas had a smaller initial mobility gap and quickly appear to catch up to mobility of the control group. Those in rural areas, instead, where more likely to stay after the reform for several years, and even in the long run their mobility never fully catches up with the control group, possibly because they have become better integrated in the local communities.

In summary, the characteristics of municipalities where refugees were placed did not change discontinuously with the reform. The treated refugees, eventually, were almost as likely to leave the initial municipality as the untreated ones. The main difference in their geographic mobility seems to be a delay in leaving the initial locations, especially when placed in non-urban municipalities, for a period corresponding to the duration of language training (consistent with Azlor, Damm, and Schultz-Nielsen, 2020). Existing evidence shows that initial placement in rural areas, often characterized by lower average income and employment rates, is associated with worse labor market outcomes for refugees (Edin, Fredriksson, and Åslund, 2004; Åslund and Rooth, 2007; Damm and Rosholm, 2010; Gody, 2017;

[15] A recent study by Azlor, Damm, and Schultz-Nielsen (2020) and an older report by Nielsen and Jensen (2006) following post-reform refugees for seven years have already noted that the mobility of the post-reform cohorts was low in the first years but increased upon completion of the three-year program.

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Azlor, Damm, and Schultz-Nielsen, 2020). Section V.B showed that rural location is associated with smaller reform effects on employment and earnings. This, plus the additional finding of delayed mobility, especially out of rural locations, likely driven by the mandate to attend the language class may have attenuated the overall positive effects of language training, especially for those placed in non-urban areas. This suggests that our estimates could be even larger if one could perfectly isolate the impact of language training only.

VI.C One-Year Welfare Cut

As already mentioned, during the first year after January 1, 1999, the group of refugees aged 25 and older (or those with children) was subject to a significant welfare cut. Therefore, it is useful to show the earnings discontinuity between treated and untreated separately for those exposed and those not exposed to the one-year welfare cut in year 1 and year 2 after the reform. We show this in Table 4 together with the effect (discontinuity) on disposable income and crime. We show the average impact of the reform on all treated refugees (Panel a) and then we separate the sub-groups of refugees subject (Panel b) and not subject (Panel c) to the welfare cut. We show outcomes in year 1, when the cut was in place, and in year 2, when full transfers were reinstated.

Column 1 in Panel c shows a clear zero effect on earnings when welfare was cut, in year 1, and as a consequence disposable income in column 2 of Panel c dropped by 22 percent for the treated compared to the untreated (an estimated decrease of USD 2,723 relative to the mean of the untreated at the boundary point equal to USD 12,507). The effect is strongly significant overall (Panel a) and it is driven by the group who experienced the cut in welfare benefits (Panel c). The disposable income decline was similar to the size of the welfare cut, equal to 25 to 29 percent depending on the family structure.[16] On the contrary, the refugees who did not experience the welfare cut did not show a significantly different level of disposable income between treatment and control groups (Panel b column 2 of Table 4). Notice that this group, being much smaller in size, shows imprecisely estimated effects both on earnings and disposable income. While not significant at standard levels, their estimated earnings change is larger than for the group receiving a welfare cut, suggesting no positive incentive effect from such a cut. Moreover, disposable income was equalized again between treatment and control groups in year 2 after the reform with earning effects still not significantly different from 0. The welfare cut weighted on most adult refugees, before they experienced any effect of the reform on employment and earnings, and hence

[16]Disposable income is net of tax and transfers are taxed in Denmark but at very low rate in this income bracket. The actual welfare cut is well within the bandwidth of the estimated reduction in disposable income.

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they costed a significant drop in their disposable income during the first year.

The rest of Table 4 explores whether this drop in disposable income might have been associated with unintended consequences. Columns 3 and 4 show the RD estimates of the average annual probability of being charged and convicted of a crime, columns 5 to 8 split these probabilities into shoplifting in supermarkets (the most common crime committed by refugees) and other crimes. Interestingly, we find in year 1 a positive and significant increase in shoplifting of 8 percentage points with no significant change in other crime (Panel a). The effect disappears in year 2. When splitting between the group subject to the welfare cut (Panel c) and the other refugees (Panel b), we see that the entire increase in criminal charges is for the group experiencing the welfare cut and only for the duration of the lower welfare benefits (year 1). As the legal process takes some months, the charges are still somewhat higher for the treated in year 2. The estimated impact on average cumulative crime rates is close to zero and not significant in year 18, implying that we do not detect any significant long-run impact of the reform on crime rate (Table 5 in Online Appendix). The timing, the group affected, the type of crime and the drop in disposable income, suggest that welfare cuts may have produced the temporary increase in property crime by pushing people close to or below subsistence level. One has to acknowledge that the two groups may differ in other dimensions and the period in which this policy was in place was short. Still, our findings are consistent with the impact on crime found in response to a cut in welfare benefits for immigrants in the later reform of 2002, documented in (Andersen, Dustmann, and Landersø, 2019).

VII Conclusion

Our results suggest that investments in language training for refugees lead to more education, more complex jobs and higher earnings for them. The cost of the expansion of language training (detailed in the law) was only a fraction of the economic gains generated as measured by the net present value of the earnings effects we estimate. The program therefore pays for itself after 5-6 years and every dollar invested generated up to USD 15 in return for the Danish economy within 18 years.[17] Furthermore, we find that cutting welfare benefits to refugees in their first year after arrival did not increase their income from employment. Instead it lowered their disposable income and increased shoplifting.

[17]Tables 5 and 6 of the Online Appendix.

Review of Economics and Statistics Just Accepted MS.
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Tables and Figures

Table 1: Summary Statistics and Balancing Tests

	Mean	S.D.	RD Estimate	Confidence Interval
	(1)	(2)	(3)	(4)
Panel a. Admission Process				
Monthly Admissions	180.490	65.58	-69.08	[-160.00 ; 21.83]
Demeaned Monthly Share of Annual Admissions	-0.00	0.02	0.01	[-0.05 ; 0.06]
Asylum Processing Time (Days)	404.33	348.11	16.58	[-72.41 ; 105.56]
Panel b. Refugee Characteristics				
Age	31.50	7.68	-0.39	[-1.71 ; 0.92]
Married	0.67	0.47	0.05	[-0.04 ; 0.13]
Female	0.42	0.49	0.04	[-0.04 ; 0.13]
No. Children < 3y	0.19	0.43	0.03	[-0.05 ; 0.10]
No. Children 3-17y	1.00	1.51	-0.04	[-0.42 ; 0.34]
Iraq	0.44	0.50	-0.03	[-0.14 ; 0.08]
Afghanistan	0.16	0.36	-0.02	[-0.11 ; 0.07]
Other Country	0.40	0.49	0.03	[-0.10 ; 0.15]
Speaks Arabic	0.35	0.48	-0.04	[-0.13 ; 0.05]
Speaks Dari	0.09	0.29	-0.01	[-0.05 ; 0.03]
Danish 1	0.22	0.42	-0.07	[-0.19 ; 0.05]
Danish 2	0.35	0.48	-0.00	[-0.09 ; 0.08]
Danish 3	0.32	0.46	0.05	[-0.04 ; 0.15]
Quota Refugee	0.10	0.30	-0.03	[-0.08 ; 0.02]
Convention Refugee	0.18	0.38	-0.08	[-0.19 ; 0.02]
Family-Reunified	0.19	0.40	0.03	[-0.07 ; 0.13]
Other Refugee	0.53	0.50	0.01	[-0.13 ; 0.14]
Panel c. Municipality Characteristics				
Urban Municipality	0.33	0.47	0.11	[-0.01 ; 0.24]
Five Largest Cities	0.17	0.37	0.01	[-0.08 ; 0.10]
Employment Rate 1996	0.74	0.04	-0.00	[-0.01 ; 0.01]
Unemployment Rate 1996	0.09	0.02	0.00	[-0.00 ; 0.01]
Earnings 1996	28.52	3.69	0.43	[-0.37 ; 1.23]
Number of Co-Nationals	222.09	679.63	-23.55	[-180.01 ; 132.91]
Share of Co-Nationals	0.00	0.00	0.00	[-0.00 ; 0.00]

Notes: Summary statistics (columns 1-2) and balancing tests (columns 3-4) of the impact of the reform on the admission process (Panel a), predetermined characteristics of refugees obtaining refugee status in Denmark between January 1997 and December 2000 (Panel b) and characteristics of the municipality of initial placement (Panel c). The 95-percent confidence intervals are constructed based on the robust standard errors (column 4). The RD estimates are from local linear regressions of model (1) using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019). Asylum processing time is the number of days between application and admission, and it is calculated for refugees (excluding quota refugees). Age, marital status and the number of children are measured at date of immigration. Danish 1 to 3 refer to the language track the individual was initially placed in. Quota refugee refers to those granted refugee status under the UNCHR quota, and Convention refugee refers to the Geneva Convention. Urban municipalities are municipalities in the capital area or municipalities with a town of more than 45,000 inhabitants. The five largest cities are Copenhagen (including Frederiksberg Municipality), Aarhus, Odense, Aalborg and Esbjerg. Average income in the municipality is measured in 1,000 USD (2000 level). The number of observations is 8,558.

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Table 2: The Impact of the Reform on Long-Run Integration

	Earnings (1)	Employment (2)	Complex Job (3)	Non-Complex Job (4)
	Panel a. Main (Linear)			
RD Estimate	2.504**	0.042*	0.092***	-0.036
	(1.087)	(0.025)	(0.023)	(0.033)
Mean of Untreated at Cutoff	7.457	0.183	0.075	0.238
Bandwidth	256	217	197	148
	Panel b. Treatment-Control Difference in Means			
Estimate	1.931**	0.036**	0.078***	-0.005
	(0.868)	(0.017)	(0.027)	(0.026)
Mean of Untreated	7.492	0.184	0.093	0.215
Bandwidth	71	81	37	44
	Panel c. 2nd Order			
RD Estimate	3.220*	0.050	0.120***	-0.052
	(1.883)	(0.039)	(0.036)	(0.038)
Mean of Untreated at Cutoff	7.185	0.187	0.067	0.239
Bandwidth	230	235	219	253
	Panel d. Controls			
RD Estimate	2.216**	0.032	0.080***	-0.037
	(1.039)	(0.022)	(0.024)	(0.030)
Mean of Untreated at Cutoff	7.449	0.186	0.071	0.239
Bandwidth	246	239	168	144
N	8,558	8,558	8,558	8,558

Notes: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$. Table entries are the RD estimates ($\widehat{\tau}$), robust standard errors in parentheses, the mean of the outcome for the untreated group measured at the cutoff ($\widehat{\alpha}$), and the bandwidth from estimation of model (1) using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019). Panel a, b and c show the main specification (linear), the simple difference in means between treated and control (zero polynomials and no kernel weighting), and second order polynomials. Panel d includes control variables. Control variables are age, age squared, unmarried, female, number of children between 0-2 years old and 3-17 years old, Iraq, Afghanistan, speaks Arabic, speaks Dari, Danish 1, 2 or 3 (unknown level is the reference), quota refugee, family-reunified or other refugee (convention refugee is the reference). The outcomes (shown in the columns) are the average of yearly outcomes over 18 years.

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https://doi.org/10.1162/rest_a_01216

Table 3: Heterogeneous Effects

	Earnings (1)	Employment (2)	Complex Job (3)	Non-Complex Job (4)
		Panel a. Alphabet of Mother Tongue		
Latin Alphabet	-0.542	-0.012	0.105***	-0.094
	(1.574)	(0.042)	(0.033)	(0.058)
Mean of Untreated at Cutoff	9.330	0.234	0.051	0.305
Other Alphabet Than Latin	4.112***	0.074**	0.093***	-0.002
	(1.500)	(0.031)	(0.031)	(0.041)
Mean of Untreated at Cutoff	6.381	0.149	0.082	0.199
		Panel b. Initial Placement		
Urban Area	4.758***	0.112***	0.127***	0.055
	(1.656)	(0.040)	(0.039)	(0.058)
Mean of Untreated at Cutoff	5.891	0.150	0.066	0.194
Rural Area	1.205	0.005	0.067**	-0.093**
	(1.431)	(0.031)	(0.028)	(0.037)
Mean of Untreated at Cutoff	8.386	0.202	0.081	0.263
		Panel c. Gender		
Female	3.026**	0.058*	0.106***	-0.025
	(1.298)	(0.034)	(0.036)	(0.036)
Mean of Untreated at Cutoff	4.931	0.139	0.092	0.132
Male	2.460	0.035	0.073**	-0.025
	(1.745)	(0.036)	(0.029)	(0.050)
Mean of Untreated at Cutoff	9.798	0.224	0.060	0.332

Notes: $*p < 0.10, **p < 0.05, ***p < 0.01$. Table entries are the RD estimates, robust standard errors in parentheses, and the mean of the outcome for the untreated group measured at the cutoff ($\widehat{\alpha}$) from a local linear estimation of model (1) using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019). Each estimation is based on a sub-sample (described in the row) and the optimal bandwidth from the full sample (Panel a in Table 2). The outcomes (shown in the columns) are the average of yearly outcomes over 18 years. The samples sizes are 2,215 (Latin alphabet), 6,343 (other alphabet), 2,826 (urban municipality), 5,732 (rural municipality), 3,597 (female), 4,961 (male).

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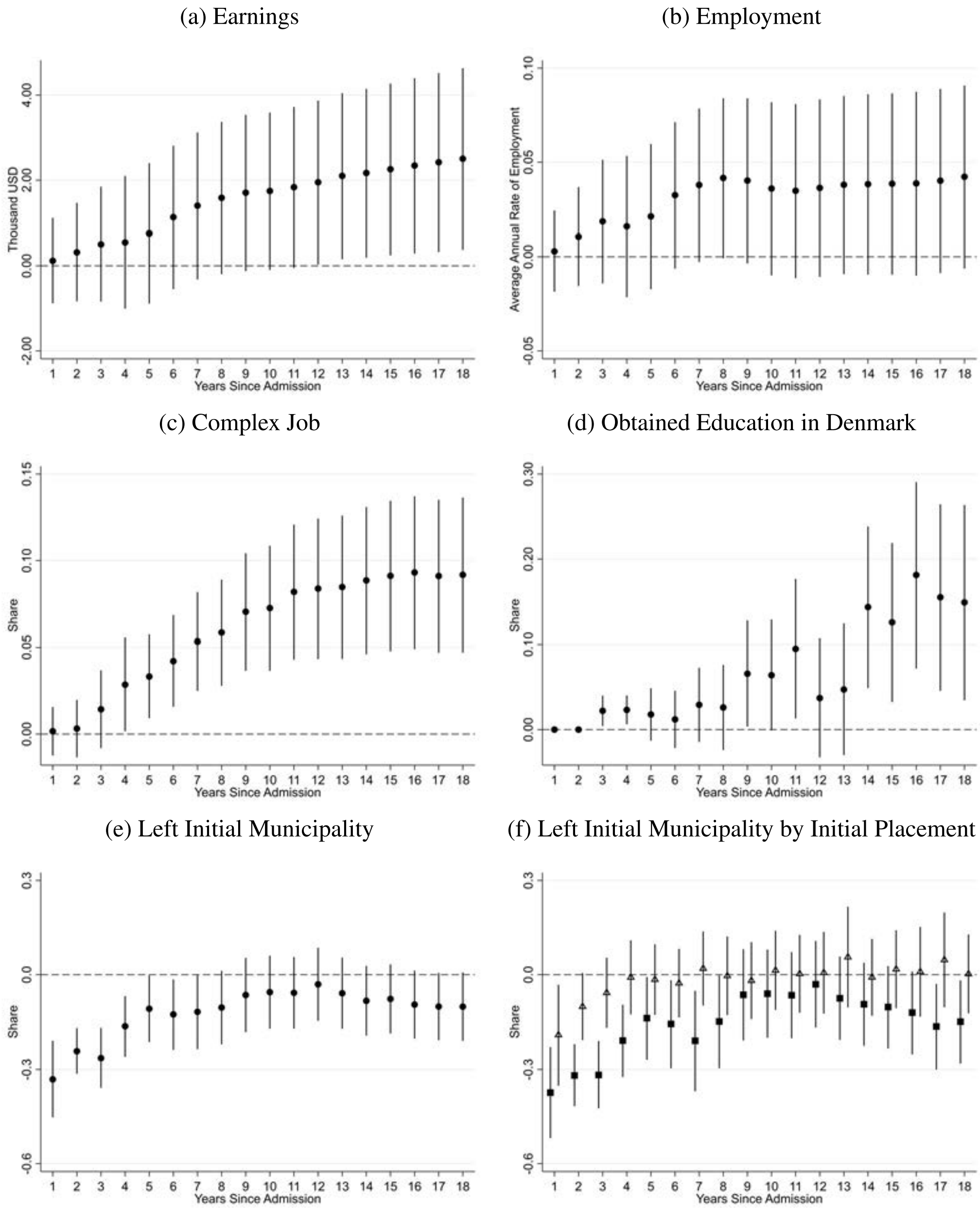
https://doi.org/10.1162/rest_a_01216



● all, ■ initially placed in rural municipalities, and △ initially placed in urban municipalities.

Figure 1: Effects Over Time

Notes: RD estimates and 95-percent confidence intervals based on robust standard errors from a local linear estimation of model (1) using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019). The outcomes are the average yearly outcomes in Panels a-c. In Panels d-f the outcomes are dummies for ever obtaining education (for 18-29 year olds at admission) and having left initial municipality.

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https://doi.org/10.1162/rest_a_01216

Table 4: The Impact of the Reform on Earnings, Disposable Income and Crime

		Disposable	All Crime		Shoplifting Crime		Other Crime	
	Earnings	Income	Charges	Convictions	Charges	Convictions	Charges	Convictions
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel a. All				
Year 1	0.185	-2.698***	0.086**	0.109***	0.076**	0.070**	0.019	0.037
	(0.506)	(0.501)	(0.040)	(0.042)	(0.032)	(0.027)	(0.025)	(0.028)
Year 2	0.499	-0.146	0.015	0.030	0.005	0.008	0.013	0.023
	(0.803)	(0.525)	(0.024)	(0.026)	(0.016)	(0.014)	(0.018)	(0.021)
				Panel b. Age $<$ 25 and No Dependents (Not Subject to Welfare Cut)				
Year 1	5.125*	-1.739	-0.197	-0.025	-0.053	-0.042	-0.153	-0.019
	(2.880)	(1.237)	(0.175)	(0.119)	(0.059)	(0.043)	(0.169)	(0.114)
Year 2	-0.709	-0.952	-0.133	-0.162	-0.045	-0.052	-0.031	-0.022
	(2.587)	(1.212)	(0.122)	(0.105)	(0.051)	(0.051)	(0.086)	(0.072)
				Panel c. Everyone Else (Subject to Welfare Cut)				
Year 1	0.082	-2.723***	0.135***	0.123***	0.091**	0.076**	0.039	0.041
	(0.382)	(0.513)	(0.051)	(0.044)	(0.036)	(0.030)	(0.028)	(0.026)
Year 2	0.746	0.029	0.028	0.065**	0.008	0.014	0.017	0.040*
	(0.795)	(0.511)	(0.023)	(0.030)	(0.016)	(0.013)	(0.014)	(0.023)

Notes: $*p < 0.10, **p < 0.05, ***p < 0.01$. Table entries are the RD estimates and robust standard errors in parentheses from a local linear estimation of model (1) using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019). The outcomes are for the years shown in the rows.