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INTEGRATING REFUGEES BY ADDRESSING LABOR SHORTAGES?
A POLICY EVALUATION

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Integrating Refugees by Addressing Labor Shortages? A Policy Evaluation
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ABSTRACT

We evaluate the effect on newly arrived refugees' employment of a policy, introduced in Denmark in 2013, that matched refugees to occupations with local labor shortages after basic training for those jobs. Leveraging the staggered roll-out across municipalities, we find that the policy increased employment by 5-6 percentage points one year after arrival and 10 percentage points two years after. The policy was especially effective for male refugees and refugees with some secondary education. The findings suggest that this type of policy could alleviate long-term labor shortages and integrate low-skilled immigrants, while having minimal competition effects on natives.

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A data appendix is available at <http://www.nber.org/data-appendix/w29781>

1 Introduction

Technological and demographic changes continue to modify the demand for and supply of labor across occupations in developed countries (Autor and Dorn, 2009; Autor, 2015, 2019). Notably, a large set of manual, low-paid jobs that are hard to mechanize and offshore in sectors such as retail, personal, safety, food and housekeeping services have experienced continued employment growth in developed countries (Goos, Manning, and Salomons, 2014).¹ Traditionally, relatively young and less educated individuals supplied labor for these jobs, however their number and their willingness to do these jobs at current wages in rich countries have been rapidly declining. Recently arrived, young immigrants could be a natural group to address this mismatch, if policies could be designed to facilitate their employment in those jobs.²

The quick and efficient matching of new immigrants to those jobs would have two, possibly large, positive effects. First, faster employment would reduce the fiscal burden of immigrants. Second, employment in jobs experiencing a high level of unfilled vacancies would reduce any competition effects on natives and increase immigrants' complementarity with them, as their entry in the labor market would fill needs not met by natives. Several countries have suggested that their immigration for job purposes should be based on a list of occupations in high demand and short supply according to various labor market indicators.³ However, these lists have mostly targeted jobs that require relatively sophisticated skills and are not thought of as ways to improve the supply-demand matching for low-paying jobs. At the same time many countries have faced difficulties in integrating and employing newly arrived refugees and less skilled immigrants.

Denmark has recently introduced a policy providing a joint solution to labor shortages and immigrant integration. One third of municipalities in Denmark introduced the so called "Industry Packages" between 2013 and 2018 aimed at increasing the employment of immigrants by matching local labor demand with labor supply from recently arrived refugees. The policy is not an admission policy that regulates the skills immigrants need to have to enter the country, but rather an integration policy. It identifies area-specific labor needs, it helps connect employers and newly arrived refugees, and it provides refugees with a minimum of the specific skills needed for those jobs. Such a model could be the blueprint for new policies for immigrants in developed countries. Those policies could potentially in-

¹Mismatches across sectors could also be worsened by pervasive events changing domestic labor supply such as the Covid-19 crisis (Peri and Zaiour, 2022).

²Immigration could be one of a set of solutions for projected labor shortages due to population aging (OECD, 2012). Other potential solutions include technologies and automation Acemoglu and Restrepo (2017, 2022). Clemens, Lewis, and Postel (2018) provide evidence that immigrants and technology adoption are in fact close substitutes when domestic labor is scarce.

³See the discussion in Martin and Ruhs (2011) for the US and in Sumption (2011) for the UK.

crease immigrant employment and be desirable to the host country as they address labor market needs. Importantly, they can be focused on a range of low-paying jobs, in which they minimize the competition effect with natives. This paper evaluates the effect of such a policy on the employment probability of refugees in Denmark. This is, to our knowledge, the first evaluation of a labor shortage-based integration policy.

To identify the effects of the policy, we use its staggered roll-out across municipalities in Denmark and compare refugees who arrived in a municipality when the program was available with those who arrived in the municipality when it was not. The crucial variable for the empirical strategy is the month of placement in the municipality relative to the month of implementation of the policy. We show that the characteristics of newly arrived refugees did not change around the time of implementation in the municipality, suggesting that timing was orthogonal to the composition of the inflow. The outcomes of refugees evolve smoothly prior to implementation, which is consistent with the hypothesis that changes at the time of implementation are produced by the policy, rather than by pre-existing economic or political factors. Additionally, we run a placebo exercise using immigrants from Eastern Europe and other immigrants who did not qualify for the program. The null effect on their labor market outcomes represents an important check that unobservable variables affecting the local labor market conditions for immigrants in low-skilled jobs are not generating a spurious result.

Our study has three main findings. First, we find a significant average intention to treat (ITT) effect of the “Industry Packages” on the employment probability of refugees equal to 5-6 percentage points one year after placement and 10 percentage points two years after.⁴ Second, the effects are similar but slightly smaller in magnitude using employment criteria that exclude subsidized jobs or require at least 80 hours of employment per month, which suggest an impact on the extensive and intensive margin of employment. Third, we find that the effect is concentrated on males with secondary education, i.e., those individuals who were already likely to be “on the boundary” of employment. We do not observe any effect on women or on those with no primary schooling. Their skills, more limited working experience and cultural attitudes towards working could make them less likely to participate and benefit from such a policy.

Our results are economically important in the light of the literature pointing out the significant employment gap and the slow and incomplete economic assimilation of refugees. Dustmann et al. (2017) and Brell, Dustmann, and Preston (2020) recently summarized the size of the initial gap and the lack of

⁴The baseline employment rate of the refugees increases sharply in their first years in the host country and the estimated effects after one and two years represent both a roughly a 50 percent increase.

convergence in employment rates of refugees across developed countries. In most European countries refugees start with an employment rate of 0.1 to 0.3, and an employment rate gap to other immigrants ranging from 0.2 to 0.6. Hence, an improvement of 5-6 percentage points in the first year, as we find in this paper, represents an increase of more than 50 percent (the employment rate in our sample is 9 percent after one year in the country).

Focusing on policy evaluations, our findings are relevant to the literature analyzing the effects of general labor market integration policies and to that analysing immigrant-specific ones. First, our main results are in line with the large literature evaluating effects of Active Labor Market Policies (ALMP) recently summarized in Card, Kluve, and Weber (2018). They show that, on average, ALMP have a 1-3 percentage point effect on the employment probability less than one year post program and a 3-5 percentage point effect 1-2 years post program. Our point estimates are larger than that. Card, Kluve, and Weber (2018), however, never focus on immigrants, who may be a group benefiting more intensely from training in targeted industries and connection to employers especially in the first year after arrival.⁵

Second, our estimates are economically significant when compared to studies of the impact of language training for immigrants and refugees. Sarvimäki and Hämäläinen (2016); Lochmann, Rapoport, and Speciale (2019); Arendt et al. (2020); and Heller and Mumma (2020) evaluate the impact of language training. Arendt et al. (2020) find a permanent, positive increase in the employment rate of 4 percentage points in Denmark. The other studies analyze a shorter time horizon and were unable to detect significant effects on employment. Sarvimäki and Hämäläinen (2016) and Heller and Mumma (2020), however, find significant positive earnings effects. Relative to these estimates evaluating immigrant-targeted policies, the impact of an up to 6 (10) percentage point increase within one (two) year(s) of arrival is economically significant for a policy providing immigrants with the minimum skills required for employment in a specific industry of labor shortage.

Finally, a series of papers have analyzed the impact of local economic conditions on the labor market integration of immigrants (Azlor, Damm, and Schultz-Nielsen, 2020; Damm and Rosholm, 2010; Godøy, 2017; Åslund and Rooth, 2007; Aksoy, Poutvaara, and Schikora, 2020). They identify the impact of being placed in municipalities with more or less favorable employment conditions. These papers exploit Spatial Dispersal Policies in Denmark, Norway, Sweden and Germany that allocate refugees quasi-randomly. They find that strong initial labor market conditions are beneficial to the employment

⁵A recent paper, Katz et al. (2022), analyzes the impact of programs targeting sector-specific skills in the U.S. finding significant effects on earnings. These programs consider all unemployed with low qualifications and provides them with general and certifiable skills. The earnings effect is driven by a higher share working in better paid jobs. The Danish program, to the contrary, is for newly arrived refugees and helps the refugees find the fastest possible route to low-skilled jobs experiencing excess demand for labor.

probability of recently arrived refugees. Our findings also speak to the importance of early employment opportunities, but rather than the general labor market conditions, we evaluate the role of improved matching of refugees with vacant jobs and basic training for those.

2 Background and Policy Description

The policy we evaluate targeted refugees and their family members (for brevity we refer to all as refugees). Refugees are placed in an initial municipality of assignment which is responsible for the integration and employment support provided under the Danish Integration Program.⁶ All refugees are offered language training, job-search support and general training upon arrival in their assigned municipality. The “Industry Packages”, which falls under the Danish Integration Program, has gained popularity in the past decade (Simic et al., 2018; Thomassen, 2019).

The policy was developed in 2013 in the municipality of Vejle in collaboration with a consultancy firm and local employers. The idea behind the policy arose from the observation that while some low-wage, mainly manual, jobs in Denmark went unfilled, recently arrived refugees were underemployed. The goal of the new policy was to provide the fastest possible route to a job in one of the industries with local labor shortages. A program of training that was specific to each of the identified industries was carefully crafted.

The different training programs were termed “Industry Packages” for their industry specificity. The policy works as follows. Refugees with no university degree and no job (representing the large majority of them) were presented with the job options in the target industries soon after their placement in the municipality. After a short introduction course of typically eight weeks they had to choose an “Industry Package” to attend. The following training varied depending on the progress of the participant. Some completed all steps within three months while other took up to a year to complete.

The industry-specific training took place directly in the businesses operating within the selected industry. They were kept short, and they focused on the necessary skills and knowledge for employment in the industry. For instance, within the “Cleaning Package” participants are taught which equipment and products to use for cleaning specific materials, and are provided training on safety issues and on the correct postures to minimize injuries. They are also taught soft skills, such as interacting with customers and colleagues, directly on-the-job. As they progress, participants are introduced to more advanced cleaning equipment and materials. While differing in specific training, all “Industry Packages” have a

⁶Azlor, Damm, and Schultz-Nielsen (2020) provide information about the dispersal policy and Arendt et al. (2020) describe the Danish Integration Program in place since 1999.

similar structure and a clear progression towards the competence requirements of the targeted industry. The competencies of the refugees are measured against these requirements to ensure progression and identify needs for additional training. The consultants we interviewed stressed that the clear structure, goal and progression are important and create a sense of meaning, purpose and motivation for the individual refugee.

Between 2013 and 2018 the policy was adopted in other municipalities, after initial anecdotal evidence of its success in the municipality of Vejle and possibly also in response to the national increase in asylum seekers.⁷ Most municipalities had the program implemented by one of the two consultancy firms, LG Insight and Foreningen Nydansker. We were able to get the starting date of the first introductory course and details about implementation of the program from the digital archives of the two firms. As information about the program became available, some municipalities adopted the policy without involving the two firms. We ran a nationwide survey to identify municipalities who had ever adopted the policy and a follow up survey about their implementation of the policy.⁸

In the wake of the increase in refugee arrivals especially from Syria, between 2014 and 2016, the Danish government took two types of measures. On the one hand, a series of tightening of asylum rules tried to slow the flow. On the other hand, in order to accelerate integration in the labor market it introduced requirements that all refugees actively search for a job and participate in on-the-job training immediately upon arrival (Arendt, 2020). In addition, a paid internship program became available for newcomers aged 18 to 40. These policies were enacted at the same time by all municipalities in 2016 and, therefore, they do not interfere with the identification of the impact of the “Industry Packages”.

3 Empirical Strategy

3.1 Event Study Design

To identify the effect of the policy we exploit the staggered roll-out across 30 Danish municipalities, which represent all the municipalities that we know eventually adopted the policy. They include half of the Danish working age population and are representative of the whole country in terms of their average immigrant shares, employment rates, immigrant employment rates and incomes. Event time for each refugee placed in municipality m is the month of placement in the municipality, t , relative to the month of first implementation of the policy in that municipality, E_m .

⁷Table 1 of the Online Appendix shows the month of implementation of the policy in the municipalities.

⁸See Sections 1.1 to 1.4 of the Online Appendix about the data we collected on the roll-out and implementation of the policy as well as detailed information about the policy and how it was implemented.

Figure 1 shows the mean employment of refugees one year after arrival plotted against event time, $K_{t,m} = t - E_m$, binned in half-year intervals. The plotted coefficients are obtained from a regression including the same control variables as our baseline specification (see Section 3.2). The first month of the interval 0 is the initial month of policy implementation. Refugees arriving at interval 0 or later are fully exposed to the policy. Those arriving at -1 or -2 could not be treated from their first day in the municipality. They can be treated for at most 11 and 5 months, respectively. The group arriving at -3 is the latest cohort arriving at least one year prior to implementation and not treated at all by the policy one year after arrival, when we measure the employment outcome. The employment rate of this latest untreated arrival cohort is standardized to 0. The figure provides the first piece of evidence of an impact of the policy on the employment rate of refugees. The graph shows that there is a higher employment rate for partially treated refugees, those between the two vertical lines. The increase in employment is even larger for the cohorts that were fully treated, to the right of the solid vertical line. The graph shows no evidence of trends in employment outcomes predating the implementation of the policy. This is a check that underlying trends in refugee integration are not biasing our results. The F-test statistic for zero pre-trend in Figure 1 is 1.99 (P-value 0.084).⁹

3.2 Estimating Equation

The estimating equation imposes a zero pre-trend, and therefore, estimates the average intention to treat (ITT) effect of the policy on the policy-exposed relative to the non-exposed cohorts:

$$Y_{i,t+T,m} = \alpha f_i(K_{t,m}) + X_i' \beta + \text{empl_rate}_{t,m} + \gamma_t + \sigma_m + \varepsilon_{i,t,m}. \quad (1)$$

f is a function of event time, $K_{t,m}$, and α is the parameter(s) of interest. We estimate three versions of equation (1) to investigate the robustness of the results to different ways of capturing policy exposure and to learn about the persistence of the impact over time.

In our first specification, the function f associates refugees who arrived in event-month $K_{t,m}$ to the share of the first year after arrival for which the policy was active. Hence, this variable is zero for cohorts that arrived more than one year before implementation and are not treated, it is one for cohorts that arrived at or after implementation and are fully treated, and it is a fraction between 0 and 1 for those who arrived from one year before up to the initial month of implementation (between the vertical lines

⁹This is a joint test that all the coefficients -12 to -3 are equal to zero. This cannot be rejected with a P-value of 0.084. Notice, that the dynamic specification is under-identified and we follow Borusyak, Jaravel, and Spiess (2021) and exclude one additional event-time parameter (the earliest event-time parameter where we observe all municipalities).

in Figure 1). In a second specification, the function f consists of three dummies. One for arrival cohorts where the policy was implemented in the second semester after arrival ($K_{m,t} = -2$), one where the policy was implemented in the first semester after arrival ($K_{m,t} = -1$), and one for everyone arriving in or after the month of implementation ($K_{m,t} \geq 0$). In the third specification, we drop the partially treated cohorts ($K_{m,t} = -2$ and $K_{m,t} = -1$) and compare fully exposed to the non-exposed using only one dummy, $\mathbb{1}(K_{m,t} \geq 0)$. We use the simpler third version of equation (1) to estimate the impact on outcomes after 12, 18 and 24 months to obtain evidence on the persistence of the effect. In this implementation we exclude one additional event time interval for each half year that we expand the time horizon, so that we always compare cohorts that are fully treated with those untreated at the time we measure the outcome.¹⁰

Our main outcome variable is an indicator equal to one if refugee, i , who arrived in municipality, m , in month, t , is employed at least four hours in month $T = 12, 18, 24$ after placement, $Y_{i,t+T,m}$. Our alternative employment indicators exclude subsidized employment or consider probability of employment for at least 80 hours in the month, corresponding to roughly half of fulltime employment. We also measure employment in a more continuous way, as a fraction of fulltime employment. Finally we estimate the impact on the probability of being unemployed.

The control variables - also used in Figure 1 - are the following; X_i is individual characteristics, $empl_rate_{t,m}$ is municipal employment rate in month t , γ_t is year fixed effects, σ_m is municipality fixed effects, and $\varepsilon_{i,t,m}$ is an idiosyncratic zero-mean error term. The individual characteristics are; a quadratic function of age, a dummy variable to control for gender, country of origin dummies, one for asylum vs family reunified, one capturing the initial reported level of education of the refugee, and dummies for having children under ages 3 or 6.

4 Data

The month and year of implementation of the "Industry Packages" in each municipality were made available to us by the consultancy firms involved in implementing the policy or in a few cases by the municipalities of implementation via a survey.¹¹ The other important piece of information is the date of first placement of the refugee in a municipality, which we sometimes refer to as the "arrival date". This

¹⁰Section 4.4 (Figure 3) of the Online Appendix contains impact estimates up to 36 months after arrival. Estimates are imprecise and unstable after 24 months due to a sharp decline in observations.

¹¹We show that the results are robust to excluding the municipalities where information about the program is recall-data based on the survey (Figure2). Sections 1.1 and 1.2 in the Online Appendix provide detailed information about the data collection and the roll-out dates.

is available from Danish administrative registers.

The population we draw from the registers are individuals who were granted refugee status (79 percent) or family reunification with a refugee (21 percent) and who were legally settled in a Danish municipality from January 2008 to April 2019. We restrict the sample to individuals targeted by the policy, namely those 25 to 64 years old at arrival and without a university degree.¹² The majority in the sample are men (63 percent) who fled the conflict in Syria (58 percent) and had only basic education at arrival (70 percent).

For outcome variables we rely on monthly register data. The primary data source stems from monthly employer-reported tax returns, which contain information on the hours, earnings and industry of employment of each individual. We supplement this with weekly information on unemployment. The detailed tax return data allow us to produce several employment measures. Our main employment variable follows ILOs definition of employment, corresponding to at least one hour in the reference week, which we translate into at least four hours in the considered month.¹³

5 Results

5.1 Main Results

Table 1 shows the average "intention to treat" effects of the policy one year after arrival. In the first row we show the parameter estimate on the variable measuring the share of the first 12 months since arrival during which the policy was active in the municipality. This treatment variable shows a significant impact of the policy on the employment rate of refugees. The estimate implies 5.3 percentage points higher employment rate, one year after arrival, when the policy was active for the full year than when it was not. The employment effect is slightly smaller in magnitude when we exclude subsidized employment (3.6 percentage points), or measure it as a probability of at least half of fulltime employment (80 hours) or as a share of fulltime equivalent (about 3 percentage points in both cases).

In the next three rows we split the treatment into full first year exposure ($K_{m,t} \geq 0$), between 6 and 11 months exposure ($K_{m,t} = -1$), between 1 and 5 months exposure ($K_{m,t} = -2$). For those with at least 6 months exposure, we find significant evidence of a positive effect on the employment rate of 3.0 to 5.6 percentage points, depending on the employment definition. The higher probability of

¹²Highly educated refugees typically receive support to use their qualifications. Those who are 18 to 24 year old are directed to undertake education in Denmark.

¹³Section 2 and 3 of the Online Appendix describes the data sources, sample and variable definitions in detail and provides descriptive statistics.

employment corresponds to a similar and opposite decrease in probability of unemployment (−5.4 to −5.8 percentage points), suggesting that the effect stems from a higher rate of job finding rather than from a change in labor force participation. Overall these results indicate that the program was effective in connecting refugees and jobs and increasing their probability of being employed in the first year.

5.2 Robustness and Placebo Tests

Our results are unbiased estimates of the impact of the policy under three conditions. The robustness and placebo tests in Figure 2 address each of them and sensitivity to changes in the sample used for estimation.

First, implementation of the policy must be uncorrelated with the characteristics of refugees placed in the municipality. Figure 2) – baseline versus second estimate – shows that the coefficient of interest is unaffected by the inclusion/exclusion of the individual controls. This indicates that the implementation is uncorrelated with refugee characteristics which may be correlated with the outcome.

Second, macro trends in refugee outcomes must be sufficiently controlled for because uncontrolled macro trends correlated with the roll out of the policy lead to bias. The municipality and year fixed effects are crucial and included in all our specifications. They control for structural differences across municipalities and national trends in refugee integration. To assuage concerns that national trends could still contaminate the parameter of interest, we exclude municipalities adopting after the notable shift in Danish integration policy towards a job-first approach in July 2016 (sixth estimate). Half of the municipalities adopted after this date, but the parameter of interest is remarkably robust to this exercise. One reason is, that the number of new refugees drops sharply in year 2017 and 2018. Hence, the latest adopting municipalities carry little weight in the individual-level regressions because few refugees got placed there close to the implementation time.¹⁴

Third, the estimates are biased if implementation coincide with other labor market changes in the municipalities such as a labor demand shock. We include the local employment rate in our baseline estimate and exclude it in the third estimate in Figure 2. The two point estimates are practically identical, suggesting that differences in employment opportunities over time within municipalities are unlikely to drive our results.¹⁵ Denmark has five regions as intermediate administrative level between the state and

¹⁴We did a few additional checks that specific municipalities are not driving our results: The seventh estimate shows our results are robust to excluding the self-adopting municipalities (“Survey Municipalities”). The fifth estimate shows robustness to excluding the pilot municipality.

¹⁵The literature on the impact of initial local conditions rely on spatial variation to identify the impact of local employment or unemployment rates. They generally find that local employment rates are more strongly related to refugee outcomes than local unemployment rates, and that the local economic conditions vary little over time within municipalities. Åslund and Rooth (2007); Damm and Rosholm (2010); Azlor, Damm, and Schultz-Nielsen (2020) experiment with fixed effects while Godøy

the municipalities. The fourth estimate include a separate time trend for each region. As additional checks that our results are not driven by local labor demand shocks, we perform placebo tests. The small and not statistically significant effects estimated for immigrants from Eastern Europe and other source countries arriving at the same time as refugees but not eligible to the program, suggest that higher demand for immigrant labor is unlikely to drive the positive employment effects (Panel b and c of Figure 2). Online Appendix Table 4 show that Eastern European immigrants and refugees work in similar industries and receive a similar hourly pay, and they are our preferred comparison group.¹⁶ Additionally the small and not significant estimates of the employment effect on Eastern Europeans suggest that the policy did not generate negative spillover effects on that potentially competing group. The fact that the policy selected industries experiencing shortages of workers likely minimized any crowding-out on other workers.

5.3 Heterogeneity, Persistence and Additional Findings

Figure 3 shows heterogeneity of the estimates along two important dimensions. We split the employment effect by the level of human capital at arrival (Panel a) and gender (Panel b). Human capital at arrival is measured either by assignment to a Danish language training track (estimates indicated with squares) or by the self-assessed level of schooling at arrival (estimates indicated with triangles). The group denoted as “Low” are individuals with elementary schooling or less measured by the reported education or by assignment to the lowest language training track. The group denoted as “High” are either those assigned to the fast-learner language track (intended for people with some post-secondary education from abroad) or those reporting post-secondary education. The group denoted as “Medium” includes people assigned to the intermediate language training track or reporting secondary education. The graph shows that the employment effect of the policy is strongest for group with the highest education among those eligible for the policy (university graduates were not included in the policy), while we find no effect for the low human capital group. This indicates that the policy was more likely to help individuals on the margin of finding a job, likely those with higher schooling. Panel b of Figure 3 shows that the whole impact of the policy is on men. Due to cultural/historical reasons refugee women likely have less labor market experience and are farther from being employable than refugee men.¹⁷

(2017) sees the effect of the initial employment rate disappears when employment rate in the year of observation is included.

¹⁶Non-refugee immigrants from other source countries are a more heterogeneous group, and we find that their placebo impact estimates are more sensitive to the inclusion of control variables suggesting that their composition could change over time in adopting municipalities.

¹⁷Since we estimate an intention to treat effect, we cannot rule out that the lack of an effect for women and individuals with little human capital at arrival is partly due to lower participation in the treatment for these groups.

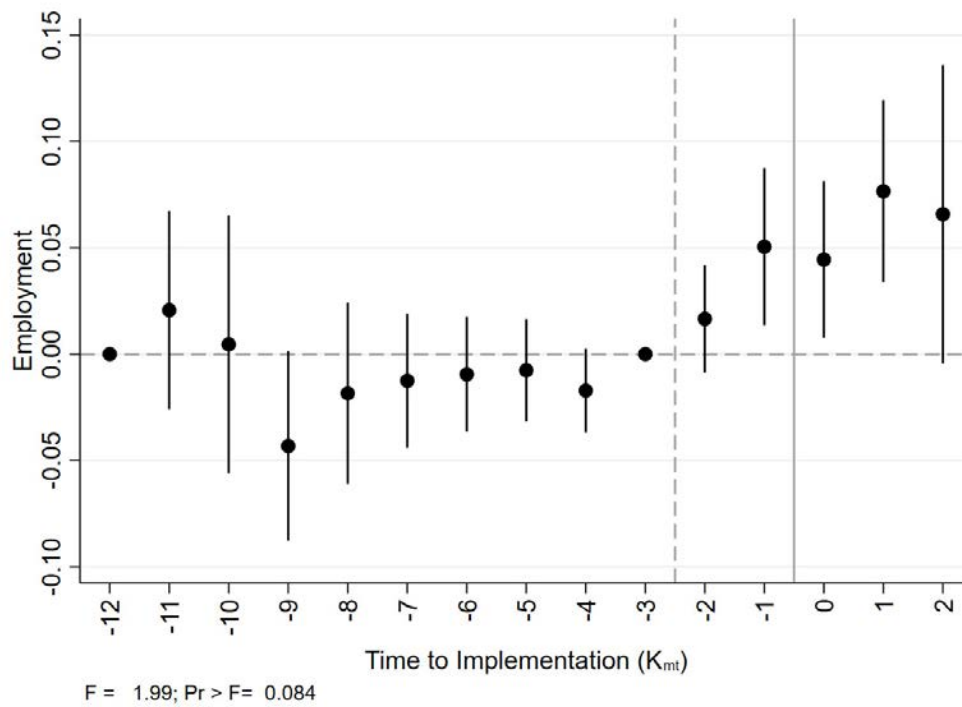
Figure 4 is based on the third version of equation (1), where we compare cohorts who could participate from arrival to cohorts that arrived too early to participate at all in the program when the employment outcome was measured. It shows the effects on employment rate 12 months (Panel a), 18 months (Panel b) and 24 months (Panel c) after arrival. We see a gain by 5-6 percentage points in the first 1-1.5 years after arrival (baseline estimate in Panel a and b) and a larger and statistically significant effect equal to 10 percentage points after 2 years (baseline estimate in Panel c). As the employment rate is also growing with time since arrival, the impact is roughly constant as a percentage of the sample mean. Some caution is needed when interpreting the estimated impact beyond the first year, since the number of observations is smaller, as we need to drop a larger number of "partially treated" cohorts and those who are not observed 2 years after arrival. Part of the difference in estimates between Panel c and Panel a can be due to the different sample.

Panel b of Figure 4 shows that the employment effect is smaller and more noisy 12-18 months after arrival when we exclude subsidized employment (fourth estimate). However Panel c shows a significant effect on this variable after 24 months. The fifth to seventh estimate of panel b and c provides additional results. We see a significant effect on the share who worked at least 80 hours in the past month and a significant increase in employment measured in fulltime equivalents. The effect on unemployment is generally more noisy, but the point estimates are similar to negative of the employment effect and to what reported in Table 1. These findings suggest that the policy helped refugee find employment and the effect lasted for at least 2 years.

6 Concluding Remarks

This article provides evidence that an integration policy helping refugees to match with jobs experiencing shortages of labor can raise their employment early on. The policy was particularly effective for male refugees and refugees with intermediate levels of education. We find suggestive evidence that it did not have an impact on crowding out other immigrants' employment probability. Such an approach could be the basis for economically successful and politically acceptable integration policies for immigrants in developed countries.

Figure 1: The Employment Rate By Event Time



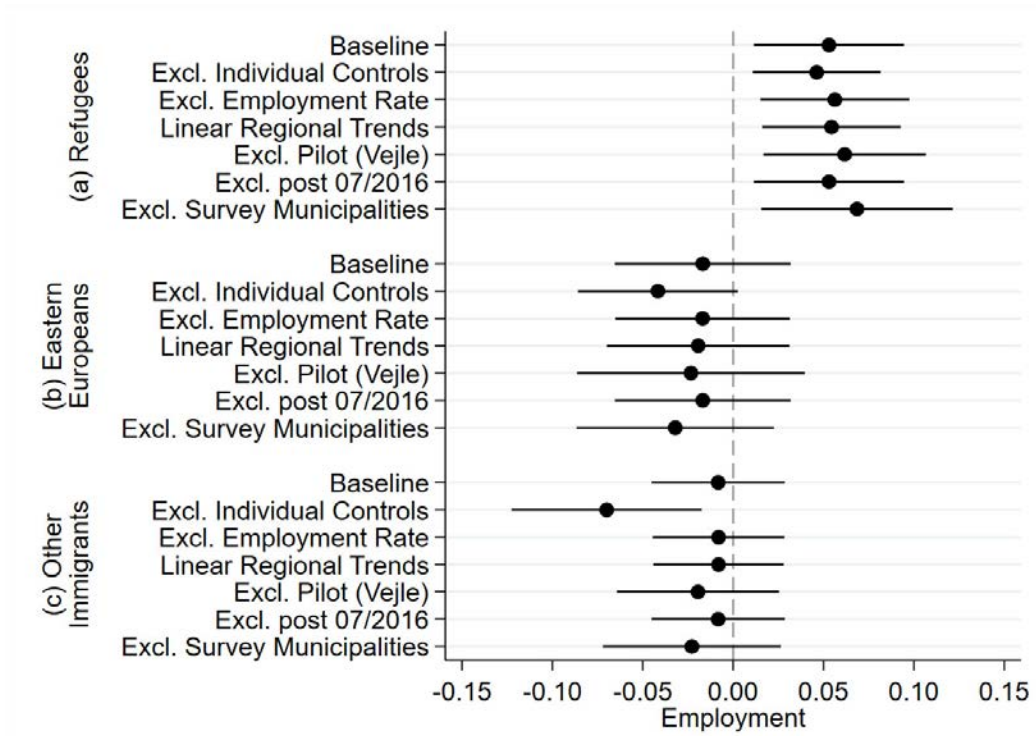
Notes: The plotted estimates are the conditional mean employment one year after placement by half-year bins of event time and relative to event time -3, using the same controls as in our baseline specification (equation 1), and 95-percent confidence intervals.

Table 1: The Effect On Employment One Year After Placement

| | Main, 4 Hours (1) | Excl. Wage subsidies (2) | 80 Hours (3) | Share of Fulltime (4) | Share of Unemployment (5) |
|------------------------------|-------------------------|--------------------------------|--------------------|-----------------------------|---------------------------------|
| Share of first year | 0.053** (0.020) | 0.036** (0.017) | 0.030* (0.015) | 0.031** (0.014) | -0.047* (0.023) |
| $\mathbb{1}(K_{m,t} = -2)$ | 0.023* (0.012) | 0.015 (0.011) | 0.003 (0.009) | 0.009 (0.009) | -0.019 (0.014) |
| $\mathbb{1}(K_{m,t} = -1)$ | 0.055*** (0.017) | 0.034** (0.016) | 0.030** (0.013) | 0.033** (0.012) | -0.054*** (0.014) |
| $\mathbb{1}(K_{m,t} \geq 0)$ | 0.056*** (0.019) | 0.041*** (0.013) | 0.034* (0.017) | 0.037** (0.016) | -0.058** (0.026) |
| Mean (Dep.) | 0.09 | 0.07 | 0.06 | 0.06 | 0.88 |
| Observations | 8205 | 8205 | 8205 | 8205 | 8205 |
| Individual Controls | X | X | X | X | X |
| Municipality FE | X | X | X | X | X |
| Year FE | X | X | X | X | X |
| Employed-to-Population | X | X | X | X | X |

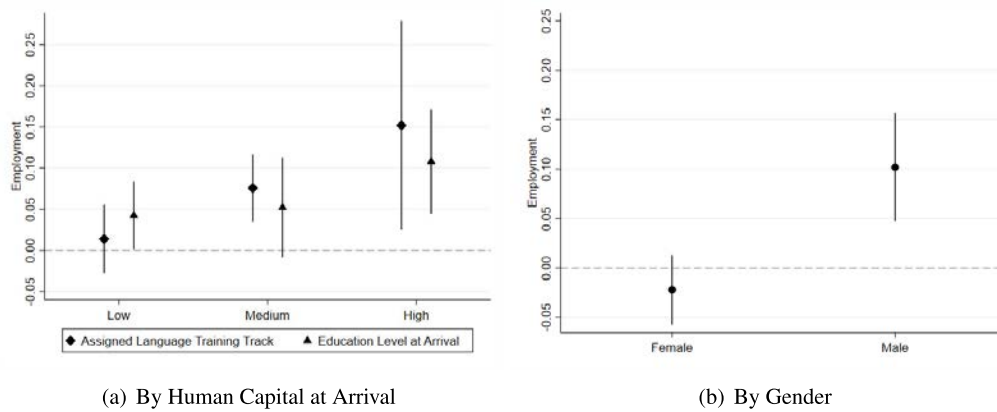
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each column contains two separate regressions based on equation (1) and shows the parameter(s) of interest (α) and the standard error(s) in parentheses. Each column shows a different employment measure or unemployment.

Figure 2: Robustness and Placebo Tests



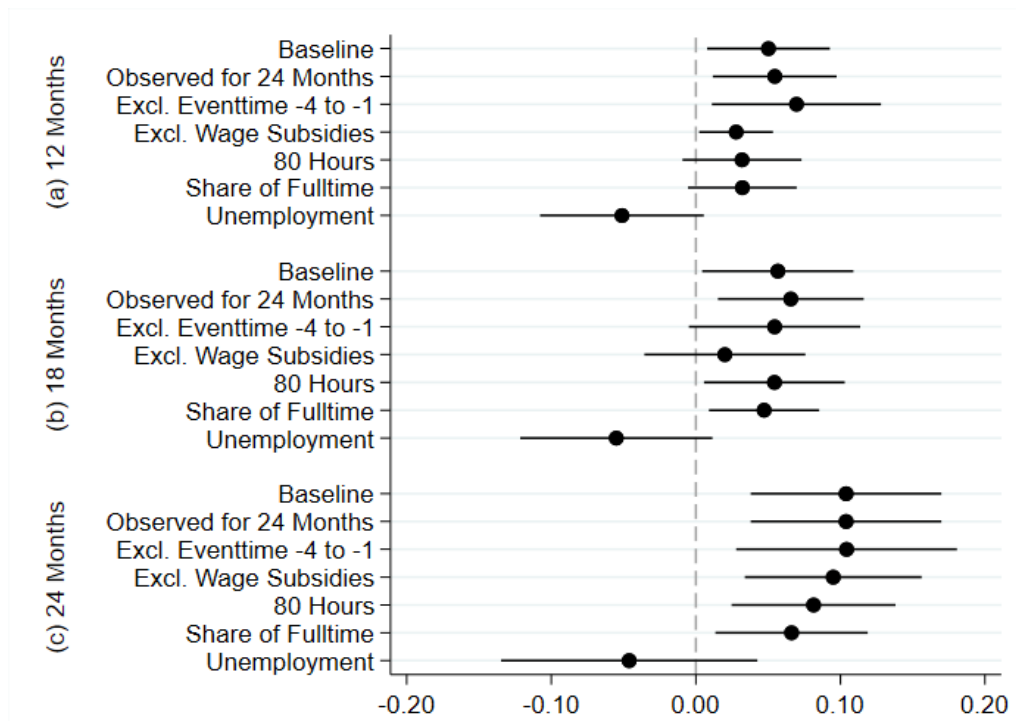
Notes: The figure shows estimates of α and 95-percent confidence intervals based on the share specification of equation (1).

Figure 3: Heterogeneous Effects



Notes: The figure shows estimates of α and 95-percent confidence intervals based on the share-specification of equation (1). Initial human capital is categorized into low, medium or high based on assignment to Danish language training track 1, 2 or 3 and level of education from abroad. Danish 1 is intended for individuals with only basic schooling (and those who do not know the Latin alphabet). Basic schooling is the lowest education category. Danish 3 is the fast learner track and Danish 2 the intermediate track. Danish 3 is intended for individuals with post-secondary education which corresponds to the highest education level.

Figure 4: Impact Over Time



Notes: The figure shows estimates of α and 95-percent confidence intervals based on the dummy-specification of equation (1) excluding cohorts who could not be treated from arrival. Hence, event time $K_{m,t} = -2$ and $K_{m,t} = -1$ are when the outcome is measured after 12 months and one additional event-time parameter is excluded for each additional semester the time horizon is extended. The regressions after 2 years (24 months), therefore, exclude observations towards the end of the sample period that cannot be observed for 24 months because the panel ends and observations with event times $t = \{-4, -3 - 2, -1\}$.

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