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COMPARING THE EFFECTS OF POLICIES FOR THE LABOR MARKET INTEGRATION OF REFUGEES

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ABSTRACT

This paper estimates, within a common framework, the effects of four types of integration polices on the employment probability and earnings of refugees in Denmark during the last three decades. We first review the studies that use a credible identification strategy to evaluate the causal effects of these types of policies on the assimilation of refugees in developed countries. We then describe the dynamics of labor market outcomes of several cohorts of refugees in Denmark. To our knowledge, Denmark is the only country where the number and design of policy changes and the longitudinal individual data availability make such an analysis possible. Our analysis suggests that improved language training, combined with initial placement of refugees in strong labor markets, significantly improved their long-run labor market outcomes. On the contrary, cutting initial welfare payments and housing them near other refugees does not seem to improve their long-run outcomes. Active labor market policies focused on matching refugees with simple jobs in high demand occupations may have positive short-run effects, but we cannot yet assess their long-run effects.

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1 Introduction

There is a substantial body of literature, beginning with the seminal contributions by Chiswick (1978) and Borjas (1985), that studies the labor market integration of immigrants. Those articles follow the evolution of employment and earnings of immigrants, usually in comparison with natives, for several decades from the date of their arrival. For the US, this literature has identified differential assimilation of immigrants, in terms of earnings, depending on their initial level of schooling, age and country of origin. Recent studies have shown a slowdown of earnings assimilation for the post-2000 cohorts (Borjas, 2015). Concurrent evidence, however, shows that, in terms of employment rates (rather than earnings), immigrants in the US, even the less educated ones and refugees, tend to quickly converge to the level of natives and outperform them (Peri and Rutledge, 2022).

In Europe, while highly educated and economic immigrants usually achieve labor market success, the research has shown slow and incomplete assimilation of less educated immigrants, especially those coming from outside of Europe (mainly Africa and the Middle East). In particular, the literature has identified refugees as a group with significant challenges to their labor market integration (see Brell, Dustmann, and Preston, 2020; Fasani, Frattini, and Minale, 2021b). Refugees represent a special group. They have been displaced, they have experienced trauma, and they may have lost human capital during the journey to asylum. They are not as positively selected as other migrants with respect to schooling and skills, and they are often from countries with culture, norms and traditions that are very different from the destination country. These factors place them at higher skill disadvantage and higher risk of discrimination. Better labor market integration of this group would improve conditions of one of the most vulnerable populations, reducing poverty and inequality in the receiving country.

In spite of the evidence emphasizing the challenges in the labor market integration of refugees, and while several developed countries have attempted different types of integration policies, credible evaluations of the causal impact of these policies have remained very limited. Most of the older studies show correlations and only very few recent studies exist that present well identified causal estimates. Additionally, the literature has not been subject to careful scrutiny and assessment.

This is surprising because the economic gains to identifying effective policies could be large. Better integration would entail a higher economic contribution by refugees, lower public costs in supporting them and a possible range of better economic outcomes for them, for their children and for the community in which they live.[1] Additionally, drawing lessons on effective integration policies will be crucial for the future integration of recent refugees, such as the Ukrainians fleeing the recent war.[2]

We start filling this gap in the literature with the present paper, proceeding in three steps. First, we identify four types of policies implemented in several countries and directed at refugees during the early years after they are granted asylum, often with the explicit goal of improving their labor market integration. The four types of policies we consider are as follows: Active Labor Market Policies (ALMP), Welfare Transfers, Language Training and Initial Placement Policies. We summarize in a systematic way the existing papers that evaluate the impact of these policies on employment and/or earnings of refugees using a quasi-experimental research design and describe the related literature.

Then, focusing on Denmark, one of the countries that provides the most detailed longitudinal data on refugees over the last three decades and that has implemented a large variety

[1] A study by the IMF (Aiyar et al., 2016) undertaken during the Syrian refugee crisis of 2015, forecasted that slow and incomplete assimilation into labor markets by refugees, costing up to 0.4% of GDP by 2020. Ruist (2020) estimates that it would cost the European Union 0.6% of its GDP to accommodate all refugees currently in Africa and Asia.

[2] As shown in Figure A.2 in the Appendix, the number of refugees who have requested asylum in Denmark from Ukraine is already larger than the number at the peak of the previous waves of refugees from Bosnia or Syria.

of policies, we estimate the time profile of employment and earnings for up to 15 years from year of arrival for the average refugees who arrived in Denmark between 1987 and 2008. Additionally, we separate assimilation profiles by arrival cohorts, genders and broad geographic area of origin. This allows us to learn about the profile of employment and earnings during the integration period and in the long run and shows that Denmark (like most European countries) experiences limited integration and many challenges for refugees in labor markets.

Third, using the Danish data and its policy changes, we re-evaluate, update, homogenize and expand existing studies (by us and other economists) of the causal impact of one policy for each of the four types identified above. The choice of policies we evaluate is driven by the possibility of using credible causal evaluation and by the intention of spanning the different approaches.

Specifically, using a staggered difference-in-differences design, we evaluate the introduction of a new ALMP called "Industry Packages" that rolled out across municipalities between 2013 and 2016. We then evaluate a policy introducing a sudden drop in welfare payments to refugees admitted after July 1, 2002, in a Regression Discontinuity (RD) design. Similarly, we use an RD design to study the introduction of new and expanded language training for refugees admitted after January 1, 1999. Finally, we use the conditional randomness of initial placement of refugees across municipalities between 1986 and 1998 to evaluate the impact of labor market conditions and co-ethnic networks on arrival.

Our analysis extends, when possible, the time horizon of previous studies to cover short (1 to 5), medium (6 to 10) and long-run effects (11 to 15 years). We make sure that the choice of outcomes, the selection of individuals, the variable definitions, the considered horizons, and the heterogeneity analyses are consistent and comparable across policies. Within this common framework, we describe and compare the estimates of the causal impact of integration policies on the employment probability and earnings of refugees from years 1 to 15 after they were granted refugee status. We also conduct consistent heterogeneity analysis for the effects along three important dimensions: gender, country of origin (linguistically far/close),

and initial placement (above/below median municipal employment rate). By comparing the estimated effects with the average growth of the employment probability and earnings for each arrival cohort and sub-groups during the 15 years since asylum, we can assess the magnitude of the effects of policies, on average, over time and across subgroups.

Four findings relative to the average causal effects of the policies stand out. First, the most effective policy on employment probability and, even more substantially, on yearly earnings in the long run, is the increased language training policy of 1999. This policy increased the employment probability by 5 to 6 percentage points and yearly earnings by $ 3,000 per year (at 2015-prices). These effects confirm what we found in Foged et al. (Forthcoming) and suggest that language is a crucial skill that unlocks better educational and occupational opportunities for refugees. Second, the other policy with a significant long-run effect consists of placing refugees (as their first location) in an area with a strong labor market as measured by a high employment rate for similar immigrants. The results suggest that placing refugees in the top quartile of municipalities, by Non-Western immigrant employment rate, compared with the bottom quartile, will permanently increase their long-run employment by 2 percentage points and their yearly income by almost USD900. Third, while we can only evaluate a short-run effect, there is some evidence that ALMP focused on matching refugees with jobs experiencing labor shortages may help their employment in the short run. Fourth, we do not find any positive long-run effects on employment and earnings from cutting initial cash-transfers to refugees or placing them near large co-ethnic networks.

Additional insights emerge from the heterogeneity analysis.[3] The language training seems particularly beneficial, as it produces more positive effects on female refugees and on refugees from linguistically "far" countries. These tend to be groups with larger employment and earnings' disadvantages. The initial location in high employment labor markets not only seems to have a positive direct effect, but it also increases the effectiveness of the language training

[3]The findings from the heterogeneity analysis are only suggestive, because the differences are not statistically significant, but the pattern is quite clear.

as well as that of the short-run impact of welfare cuts on labor outcomes. Such interactions suggest that potentially combining policies of language training and initial location in strong labor markets can help refugees to achieve higher earnings in the long run.

Finally, while the welfare cuts have a positive effect on employment and earnings in the short run, the extra earnings they generate are much smaller than the welfare cut (only about 30% of it). Refugees, therefore, experienced large drops in disposable income, which, as shown in Andersen, Dustmann, and Landersø (2019), was associated with higher crime and lower education of children.

While cutting welfare transfers generates immediate fiscal savings, it has the cost of no long-run improvements for the refugees and possibly higher crime and lower education of their children. On the contrary, more language training and placement of refugees in strong labor markets generate, with only modest short-run costs, positive and strong effects on income of refugees, producing a large present discounted value in GDP relative to the cost of the policy in the 15 years considered (Foged et al., Forthcoming).

An important lesson for future impact evaluations stands out from our comparison of the performance of the policies over time: Extending the time horizons beyond the immediate impact of the policy allows us to understand whether the impact is temporary or more permanent. This is important because the more promising policies for the labor market integration of refugees are those that seem to persistently improve their outcomes.

The rest of the paper proceeds as follows. In Section 2 we discuss the timing of events from displacement to long-run integration, the labor market dynamics of the refugees, and the policies. In Section 3 we review the most relevant studies assessing causal effects of policies on refugees' integration. Section 4 describes samples, data and variables and Section 5 estimates the employment and earnings dynamics of refugees from year 1 to year 15 after they were granted asylum. In Section 6 we describe the empirical method, validity tests and

the impact estimates for all four policies. Finally, Section 7 discusses costs of each policy in comparison with the Present Discounted Value of the income they generate. Section 8 concludes the paper.

2 Timeline of Refugees' Experience and Description of Policies

2.1 Refugees' Experience

Figure 1 shows the timeline of events for asylum seekers/refugees. The event line begins (origin) at the time they are displaced from their country of origin and proceeds through their arrival in the country of destination, the process of gaining asylum, the recognition as a refugee, and the labor market access and experience in the early, intermediate and long run.

The period between displacement (origin/vertical axis in Figure 1) and arrival at destination (first vertical line in the chart) can be long and tortuous and may imply intermediate destinations, as described in Brell, Dustmann, and Preston (2020). This period can compound the trauma and the human capital loss. Then, from time of arrival until the granting of asylum (the second vertical line in Figure 1), refugees are usually in refugee centers. During this period, they have no or very limmited legal access to employment and have minimal interactions with natives and the local economy.[4] This phase usually lasts from a few months to a year (occasionally more).

Existing studies show lasting negative consequences of delayed entry into the host country labor market due to lengthy asylum processes (Hainmueller, Hangartner, and Lawrence, 2016; Hvidtfeldt et al., 2018) and employment restrictions while waiting for asylum (Marbach, Hainmueller, and Hangartner, 2018; Fasani, Frattini, and Minale, 2021a). The time to

[4]Until 2013, refugees in Denmark could not work before asylum adjudication. Since 2013 they can, but the application process is very cumbersome and uncertain so very few ever do.

being granted asylum recognition is not a policy variable that is controlled by local authorities, and it is usually not thought of as an integration tool. Still, the findings suggest that the effect of the long wait, and possibly the uncertainty and additional loss of human capital during the pre-asylum phase lowers the employment probability of refugees. Recently, a few countries (such as Germany in 2017) have allowed asylum seekers to access training and employment programs during the pre-asylum phase (Battisti, Giesing, and Laurentsyeva, 2019; Fasani, Frattini, and Minale, 2021a). While those programs are potentially effective, the fact that a significant percentage of those individuals do not obtain asylum status and must leave the country discourages these policies. In this study we will not analyze those policies. During this pre-asylum phase, the employment rate in the sample of refugees in Denmark that we use for our analysis was 0, as these individuals were not allowed to work.

The next phase begins when a refugee is granted asylum and, from that point on, he/she is likely to stay in the country.[5] We categorize the first five years after obtaining refugee status as the "integration phase". This phase is special for two reasons. First, this is the period during which the four types of "integration policies" that we will evaluate take place. Second, as qualitatively shown by the line capturing labor market outcomes in Figure 1 (which we confirm with actual data in Figure 2), most of the growth and most of the catching up to natives in those outcomes occur during this initial period.

Some of the integration policies are similar to policies implemented to improve the labor market outcomes of unemployed natives, but we only consider the evaluation of those policies when applied to refugees. While reviewing the impact of those policies on natives can be informative, it is unlikely that the effects on employment are the same for natives and refugees. Natives do not experience a comparable period during which they are new to the receiving economy and culture, and their human capital and mental health status are

[5]In most countries, refugees are granted a temporary, renewable residence permit and there is an additional decision about permanent residence, which usually requires some level of economic integration and language proficiency (see OECD, 2018; Arendt, 2018).

different from newly arrived refugees.

The focus on the policies implemented in the integration phase allow us to evaluate effects early on and for up to fifteen years after implementation. Such a time horizon is much longer than what is covered by evaluation studies of the effects of labor market policies on natives. Those studies typically consider at most up to three years after the end of the intervention (see Card, Kluve, and Weber, 2018).

After these first five years referred to as the short-run "integration phase", we follow the labor market outcomes of the refugees 6-10 years (medium run) and 11-15 years from the grant of asylum (long run).

Evaluating the persistence of a policy in the medium and long run is very important for understanding whether it affects the level of outcomes permanently, or only the short-run transition through a temporary effect. In the medium and long run many refugees become permanent residents of the host country. Some may decide to apply for citizenship in their new home country. While there are studies of the potential impacts of access to citizenship and permanent residency on integration outcomes (Bratsberg, Ragan, and Nasir, 2002; Hainmueller, Hangartner, and Ward, 2019; Arendt, Dustmann, and Ku, 2021), these seem smaller, especially on labor market outcomes, and we will not consider them.

As shown qualitatively in the Figure 1 (and with actual data in Figure 2), the average labor market outcomes of refugees grow much more slowly in the medium and long run after the grant of asylum. Our analysis will focus on the consequences of early policies during each of the three periods - short, medium and long run - rather than on effects of policies enacted in these later phases.

2.2 Types of Policies

To summarize the literature on policies directed at refugees and following, with some modifications, the recent review by Arendt, Dustmann, and Ku (2022), we group the integration

policies into four types. The first two types are policies directed at refugees during their integration period and also widely adopted to improve labor market assimilation of disadvantaged, low-skilled or vulnerable natives. The design and the goals of the other two policy types are specific to refugees as immigrants.

The first policy types we consider are Active Labor Market Policies (ALMP). They consist of help and support in the search for jobs, and specific training and coaching for jobs. While there is a vast literature evaluating the impact of these policies on native workers in many countries and also influential papers summarizing these findings, e.g., Card, Kluve, and Weber (2018), there are very few well-identified studies that evaluate their impact on refugees or newly arrived unemployed immigrants. Furthermore, the studies of these policies are limited to assessing the impact within 1 to 3 years of implementation.

A second policy type concerns the generosity of welfare benefits available to refugees during the integration period. While a vast literature evaluates the impact of welfare transfers on individuals' incentive to work and their labor income (see the review article by Baird, McKenzie, and Özler, 2018, for an overview), only very few studies identify the effects of those policies on newly-arrived immigrants or refugees.

The third policy type is specific to immigrants and we call them language training policies. These policies could be seen as a special case of broader training in general skills (included in ALMP). They are, however, particularly important and deserve specific attention. A long line of research (dating back to Kossoudji, 1988, and, e.g., Chiswick and Miller, 1995) has associated better language skills with better employment outcomes and higher income for immigrants. Moreover, language classes are a conduit for civic and cultural education and for unlocking the ability of refugees to transfer a number of communication and interactive skills.

Finally, we study the effects of some important local characteristics in the labor markets where refugees are first placed after obtaining asylum. In many countries, at the time asylum is granted, the refugee is placed in a new location where the integration services are offered. As a consequence, the government controls the initial location and, lacking perfect mobility, this can affect the long-run outcomes of refugees. The strength of the local labor market and the presence of co-national networks at the time of initial placement can affect the initial job opportunities and the ease of finding a job in the short run. Through persistence or path-dependence, this may affect the long-run labor market success of the refugees.

3 Review of the Estimated Effects of Four Types of Policies

To put the magnitudes of our estimated effects into perspective, we summarize the findings of the existing literature (excluding the papers we replicate). In particular, we focus on the estimated effects from studies with the following three features. First, we consider only studies with a credible causal identification strategy (omitting or only mentioning those based on partial correlations). Second, we focus on the policies implemented for refugees in the early integration phase. Third, we only include studies if they estimate the impact on at least one of two outcomes: the probability of being employed and/or some measure of labor income. These criteria leave only one to four studies for each type of policy. Table 1 summarizes the original studies that evaluate each of the policies, and Appendix Tables A.1 to A.4 provide further details. We discuss each of these strands of literature below.

3.1 Active Labor Market Policies

There is a vast literature evaluating ALMP. Some recent papers have systematically surveyed and summarized the findings from this literature. An influential and recent meta study is

Card, Kluve, and Weber (2018) (which extends and updates the analysis in Card, Kluve, and Weber, 2010). This meta-analysis evaluates more than 200 studies that have used a credible identification strategy and report a clear comparison group.

These studies consider interventions on all workers, many of whom are short-term unemployed natives. Therefore, they are not directly comparable with refugees who come from trauma, are in a new country and lack language, cultural and other country-specific skills. Nevertheless, a few findings of the Card, Kluve, and Weber (2018) paper are useful for our context and worth mentioning. First, the majority of policies documented in the study consist of training and job search assistance (representing 65% of all ALMP studies included in Card, Kluve, and Weber, 2018) with other policies being subsidized private sector employment or public sector employment. Second, the average effect on the employment probability is 1.6 percentage points after one year from the completion of the program, 5.4 after two years and 8.7 after three years (see Table 2 in Card, Kluve, and Weber, 2018). Third, training programs have a larger average effect than job-search assistance programs, especially after two-three years. Job search assistance programs have an average effect of 1.1 percentage points after three years, while training programs average 6.7.[6] Therefore, a regularity to keep in mind is that ALMP policies, especially those including on-the-job training, typically increase employment probability by 5.4 to 8.7 percentage points within the first 2-3 years. We do not know of any estimates beyond three years as the studies do not look at that.

We are aware of four studies (excluding Foged, Kreuder, and Peri, 2022, whose approach and estimates we will replicate in the next section), that evaluate the effects of ALMP implemented in the early years after asylum on the employment rate and/or labor income of refugees. They all consider outcomes after one to two years from the intervention. Three of the studies (Joona and Nekby, 2012; Dahlberg et al., 2020; Battisti, Giesing, and Lau-

[6]See Table 3 in Card, Kluve, and Weber (2018) The table additionally shows that the largest employment effect is achieved in the long run by subsidized private employment, with a an increase in employment probability by 21.1 percentage points by year 3 since the intervention.

rentsyeva, 2019) report effects of randomized control trials (RCT); two in Sweden (in 2006 and 2017) and one in Germany (in 2017). The Sweden 2017 intervention is a bit different in that it includes language training (which we consider as a different type of policy) in addition to support for search and increased training. The other two RCTs include training/coaching and search support. The German study includes some refugees before they were granted asylum and follows refugees for one year at most. The fourth study, Arendt (2022), evaluates a "work-first" policy introduced in Denmark in 2016. The policy increased requirements for refugees to search for jobs and to participate in on-the-job training. The identification strategy in this study uses the exact date that asylum was granted as the threshold for an individual to qualify for the program.

The first row of Table 1 summarizes the average estimates of the effect of ALMP on the employment probability and on labor income of refugees. As already mentioned, the original studies only look at effects within one to two years, and hence, they only consider the short run. Moreover, only one of them estimates earnings effects. The average estimate of the effectiveness of ALMP for refugees is obtained considering the four original studies described in Table A.1 and averaging their estimated effects on the employment rate (or earnings) in year 1 and where available in year 2 after the intervention. We weight each estimate by the inverse of its standard error to weight more precise estimates more heavily. With this procedure we obtain an average effect of 7.5 percentage points on the probability of employment (first row of Table 1). Looking at the individual studies, the largest effects (equal to 17 to 19 percentage points) were estimated for the 2017 reform in Sweden (Table A.1, row 3), which also had a significant language training component. That result, therefore, is likely a combination of regular ALMP and language training. The reforms introducing only job-search support and on-the-job training generated effects ranging from 3.2 to 11.9% in years one and two. The only available estimate for earnings implies an increase of USD222 in yearly earnings in the first year after intervention.

Overall, we can say that the well-identified studies on the impact of ALMP on refugees' integration find coefficients of the same magnitudes as those found for natives. Similar to the much larger literature considering all workers or natives, these estimates pertain to the short run, and we do not know whether the effects are persistent. Given that the average employment probability growth of refugees in the first 5 years (assimilation period) is about 30 percentage points (see Figure 2), effects around 7.5 percentage points are significant, as they would boost this growth by one fourth to one fifth of its total. A key question is whether these effects are only temporary or whether they are permanent, adding an employment probability of around 7.5 percentage points to the long-run level of about a 40 percent employment probability. On this question the literature is silent.

3.2 Welfare (Cash) Payments

Like the ALMP policies, the impact of cash transfers (welfare payments) on employment probability and earnings of individuals has been widely studied. Most studies, however, have focused on all individuals, hence, mainly natives and citizens. On arrival, refugees receive some form of welfare payments in most countries, in order to be able to get started. While estimates from the general population may not be too informative about the potential impact that welfare cuts have on refugees, it is useful to discuss some general findings. A review article by Baird, McKenzie, and Özler (2018), mainly focused on middle-income economies, summarizes the studies of the impact on labor supply and employment of conditional and unconditional cash transfer programs in many countries including Mexico, Honduras, Nicaragua, Colombia, Ecuador and South Africa. Many studies find small effects in the short run (1-3 years) and no effect at all in the medium to long run (over 5 years). Most studies that analyze advanced economies and evaluate the impact of tax and welfare payments on employment (e.g., Immervoll et al., 2007) use a small negative (and also include 0) elasticity response to welfare payments as a consensus estimate. They usually rely on short-run estimates.

When it comes to evaluating the effect of cash transfer cuts or changes on refugees during the integration phase, we are only aware of three studies (excluding Andersen, Dustmann, and Landersø, 2019). Two of them (Rosholm and Vejlin, 2010; Huynh, Schultz-Nielsen, and Tranæs, 2007) analyze the short-run effects of a welfare cut specific to refugees. With the goal of pushing newly arrived refugees into the labor market, the Danish government cut the amount of the initial cash transfer to them in 2002 by 40%, corresponding to an average decrease in their income of USD6,000 per year during the early years. This is also the policy evaluated in Andersen, Dustmann, and Landersø (2019) whose approach we replicate and extend in our analysis. The third paper, LoPalo (2019) uses variation in the access of newly arrived refugees in the US to "Transfer Assistance for Needy Families" (TANF), a policy that varies in generosity across US states and over time. Using the quasi-random initial resettlement of refugees across US states, LoPalo (2019) identifies effects on employment and labor earnings.

These three studies are described and their estimates shown in Table A.2 in the Appendix. The average employment effect reported in the second row of Table 1 is based on the reported estimates from Huynh, Schultz-Nielsen, and Tranæs (2007) and LoPalo (2019) as Rosholm and Vejlin (2010) do not use employment rate (or earnings) as outcomes but, rather, hazard rates of employment transitions.

The average effect of the welfare cuts is a 3.8% increase in the probability of employment in the short run (1-2 years). No estimates for the medium and long run are available. This is obtained by averaging the positive 5-6 percentage points impact of the Danish 2002 reform on the employment rate and no employment effect of a reduction in the generosity of TANF transfers by USD1,200 per year, as estimated by LoPalo (2019). In terms of labor income, while the Danish studies do not analyze the impact of earnings, the US study finds a negative wage effect of reducing TANF. The magnitude is about 7.7% of the yearly earnings for a

decline of TANF by 1,200 per year (USD100 per month). LoPalo (2019) argues that higher transfers allowed refugees to negotiate better wages and get better jobs. We report the short-run earning loss of USD1,940 per year as the effect of this welfare cut.

Two additional considerations emerge from the literature on the generosity of welfare for newly arrived refugees. Andersen, Dustmann, and Landersø (2019) extend the estimates of Rosholm and Vejlin (2010) and show that after five years the positive employment effects of the cuts fade. Furthermore, they show that the earnings gain for the refugees is much smaller than the welfare cut, and hence, the disposable income of refugees dropped very significantly because of this reform. The drop in available income was associated with an increase in property crime and in a worsening of school outcomes for children of affected refugees. The conclusion of Andersen, Dustmann, and Landersø (2019) is, therefore, that while stimulating a small employment response in the first couple of years, the welfare cut had unintended adverse effects on the affected families.

3.3 Language Training

Economists have long recognized a very strong positive correlation between language proficiency and the labor market outcomes of immigrants (e.g., Chiswick, 1991; Chiswick and Miller, 1995, 2010). More recent studies have used age on arrival to separate the role of English language in the labor market success of immigrants, finding that arrival before age 11 is associated with much better language proficiency and better labor market outcomes (as well as more schooling) (Bleakley and Chin, 2004). In a very recent study, Hangartner and Schmid (2021) use the quasi-random placement of refugees in Switzerland between 1998 and 2017 (a strategy we will describe in the next section) to differentiate between French-speaking refugees being placed in the French- or in the German-speaking part of the country. They find that being placed in a region with the same language as one's own native language improves employment probabilities of the refugees by about 10% and the effect persists up to 5 years after arrival, which is the longest horizon that they can analyze. While this seems

a credible estimate of the labor market impact of being a native speaker for a refugee it is not an evaluation of a policy focused on teaching the local language.

Aside from Foged et al. (Forthcoming), whose analysis we replicate, we are only aware of three studies that apply a credible identification strategy to evaluate policies that increased the language training of immigrants in the early years after arrival and report estimates on employment probability or earnings. They are Foged and van der Werf (2022), which analyzes access to language classes in Denmark; Lochmann, Rapoport, and Speciale (2019), studying a reform in France; and Sarvimäki and Hämäläinen (2016), looking at a policy change in Finland. All these studies assess the impact on employment probability and the last one estimates the impact on earnings in the short and medium run after the implementation. In the third row of Table 1, we show the average estimated short-run effect on employment rate from the three studies (weighted by the inverse of the standard errors of the estimates). The estimate is equal to 1.7 percentage points, which is obtained by averaging a significant positive effect of 9 percentage points in Lochmann, Rapoport, and Speciale (2019) and small insignificant employment effects in Sarvimäki and Hämäläinen (2016) and Foged and van der Werf (2022). The medium-run effect is only available in Sarvimäki and Hämäläinen (2016) and it is close to 0 and not significant. The reported effect on yearly earnings is obtained from the only study that analyzes this outcome, Sarvimäki and Hämäläinen (2016), and is equal to Euros 728 per year in the short and medium run (they estimate an average of the first 10 years).

Lochmann, Rapoport, and Speciale (2019) use an RD design with a cutoff at a specified language-test score threshold applied to decide whether recently arrived immigrants qualify or not for language classes. They find a significant and large effect on labor force participation (9.7 percentage points) but no effects on employment conditional on being in the labor force. We therefore infer a significant impact on the employment-to-population ratio which mirrors the employment probability. The number of observations used is small (a few dozen refugees), especially when estimating using a local bandwidth around the discontinuity.

Sarvimäki and Hämäläinen (2016) use an RD design based on the date of arrival of immigrants in Finland relative to a threshold date that separates those who qualify and those who do not. The intervention is a change in labor market policies, reducing job search support and increasing language and immigrant-specific training. They find significant and persistent effects on labor income (in the order of +728 euros per year), in the short and medium run (in the first 10 years), but no effect on employment probability. The estimated effect should be interpreted as an "Intent to Treat" estimate of the effect of the reform.

Foged and van der Werf (2022) use distance from a language training center combined with the initial random placement of refugees to construct an instrument for the amount of language training. The study finds a strong impact on language proficiency. This in turn is associated with a higher propensity to stay in the local community of resettlement and with a significantly positive probability of enrolling in education. The effect on employment probability, while positive (+0.062) is not significant. The short time period considered and the imprecision of the estimates makes it hard to rule out reasonably sized positive effects.

Two recent additional papers are well identified and give support to the idea that language training can be quite effective in increasing labor market opportunities, but are not quite comparable to our analysis in some crucial features. Heller and Mumma (2022) use the variation generated by lotteries used to give access to publicly funded classes in English for Other Language Speakers in Massachusetts between 2008 and 2017. This policy does not target immigrants during their initial years only, as several individuals who attended the classes had been in the country for 5 years or more. Still, the study finds significant positive earnings effects in the order of USD2,400 per year up to 10 years after treatment.

Finally, Ek, Hammarstedt, and Skedinger (2021) use experimental randomization in sending "artificial" applications and CVs to employers. They evaluate whether the probability of being called back by an employer increases when a language class is added to the CV. They do not find a significant effect, but the outcome variable and method are very different from

that of the other studies considered.

The estimates of the impact of language training on the employment rate and earnings are positive, at least in the short run. It is the only type of policy for the existing studies that shows positive effects on earnings in the short and in the medium run. Moreover, it appears that better language proficiency could unlock more education opportunities and promote integration in other aspects of social life.

3.4 Initial Placement

The most studied causal estimates in the literature analyzing the labor market success of refugees are related to characteristics of the location of their initial placement. Several economically advanced countries, e.g., Sweden, Denmark, Norway, Germany, and Switzerland, had or still have policies that house refugees through a spatial dispersal process, which is as good as random conditional on observables used in the process. The refugee receives housing, income support and training in the assigned location. Therefore, the randomness of initial location and the incentives to stay there, at least for a few years, imply that this set-up provides a natural experiment to study the impact of initial labor market conditions, presence of co-ethnic people and, possibly, other local characteristics on short and long-run employment outcomes of refugees.

While very recent studies (Aksoy, Poutvaara, and Schikora, 2021; Müller, Pannatier, and Viarengo, 2022) analyze the impact of local political attitudes on economic outcomes of refugees, most of the published studies analyze how the labor market conditions or co-ethnic networks affect the labor market integration of refugees.

The first category of papers measures labor market conditions by the employment (or unemployment) rate of everyone or other immigrants in the location of arrival. The idea is that economic conditions in the initial location can matter for two reasons. The first is that delays in finding a job after arrival may prolong unemployment and loss of human

capital with a "scarring effect".[7] The second, is that given limited mobility (in our data at least half of the refugees stay in the initial location for 10 years or more), the initial labor market conditions are correlated to future ones and being placed in a weak labor market has, therefore, an impact on the average outcome.

Some classic analyses of this type of policy use Danish data. Damm (2014) takes advantage of the early dispersal policy in Denmark (1986-1998), which placed refugees quasi-randomly, conditional on family characteristics and country of origin. Azlor, Damm, and Schultz-Nielsen (2020) use a more recent version of dispersal in Denmark that placed quasi-randomly only those refugees arriving after quotas for large cities were reached. Godøy (2017) analyzes this question looking at the dispersal policy in Norway and Åslund and Rooth (2007) analyze refugees in Sweden.[8]

It is very hard to extract comparable estimates across these studies, as they use different definitions of local variables capturing the strength of local labor markets. The explanatory variables range from the local employment rate, unemployment rate and their logs to the employment rates for specific groups (immigrants, Non-Western minorities). In most of these studies (described in Table A.4 of the Appendix) the overall estimates of the impact of some measures of labor market strength in the initial location tend to be null to somewhat positive and significant on the probability of finding a job 1-5 years after placement. Few studies extend to 6-7 years after initial placement and no study goes beyond that horizon. As a consequence, the fourth row of Table 1 is more suggestive of the general results rather than an exact average of comparable studies, as shown in the first three rows. We report the short-run effects of the employment rate for Non-Western immigrants from Azlor, Damm, and Schultz-Nielsen (2020), which is the most consistent with our measure. A one percent

[7]This literature is connected to the literature that analyzes the medium to long-run effect of labor market conditions at the beginning of a person's working life. Papers such as Beaudry and DiNardo (1991) and more recently Oreopoulos, von Wachter, and Heisz (2012) find lasting effects of starting one's working career in a recession.

[8]Aksoy, Poutvaara, and Schikora (2021) and Müller, Pannatier, and Viarengo (2022) analyze the effect of initial unemployment (besides that of local political attitudes) on subsequent labor market outcomes of refugees. They find effects in line with the rest of the literature.

higher employment rate of Non-Western immigrants in the location on arrival generates a 0 to 0.5 percentage point higher probability of being employed one to five years after. The long-term effects and the effects on income are rarely available and not easy to translate in our frame. Overall, studies find positive or null effects on earnings from a higher employment rate in the location of first placement and do not analyze horizons beyond 6-7 years.

The second category of papers uses the same dispersal policies to assess the impact of larger or smaller networks of co-ethnics on arrival on employment and earnings of refugees. Edin, Fredriksson, and Åslund (2003); Damm (2009); Battisti, Peri, and Romiti (2022) look at Sweden, Denmark and Germany, respectively, and find no consistent evidence of an effect of density of co-ethnic networks in the arrival location on the employment probability of refugees or on their earnings. The studies that look at the effects over time since initial placement find no significant effect up to 6-7 years after arrival. They sometimes find effects for some sub-groups (least educated) and when digging into the impacts of the "quality" of the network (average education, average wage of that group). In the last row of Table 1 we indicate the qualitative findings of those studies. When positive, the estimated effects are rather small and short-lived. One useful thing that several of those studies establish is that the causal effect of initial networks is not as negative as the correlation between the density of co-ethnic immigrants and the initial employment outcome of immigrants would suggest (see Damm, 2009). In the presence of individual choice and hence selection, it appears that newly arrived immigrants moving to locations with a high density of co-nationals may be negatively selected in their probability of finding a job.

The overall impression from Table 1 is that our knowledge of the causal impact of policies for the labor market integration of refugees is limited and sparse, especially for the medium and long run. Except for the studies on initial conditions, all the other policies have been analyzed by less than 10 well identified studies in total. By filling most of the cells in Table 1, and using Danish policies and comparable estimates, this paper makes progress in expanding

and organizing this literature.

4 Data

4.1 Data Sources and Main Variables

Our data allow us to identify all refugees and family members reunified to refugees in Denmark and to follow them from the first time they settle in a Danish municipality, which is within two months from they have been granted refugee status. Information on their admission category (asylum or family reunited with a refugee in Denmark) is obtained from the Admission Register (*OPHG*) from 1997 onwards. We impute refugee status before 1997 based on year of arrival and country of origin. The migration register (*VNDS*) is the source of information about the date of first settlement in Denmark and it also includes information on later out-migration from Denmark.

The employment variables are from the integrated database for labor market research (*IDA*) and the data on earnings are from the income register (*IND*). A few additional registers are used to obtain information about education and demographic data on the individuals (*BEF, UDDA, FAM*).

In this paper we focus on two main outcomes: the employment probability and earnings. Our main employment variable is equal to one if the individual was employed at some point in the year. We measure earnings as the annual gross labor market income converted to US dollars and deflated to the 2015-price level. In the Appendix, we consider an alternative employment variable that captures employment as a fraction of a full working year and, therefore, combines the intensive and extensive margin of employment.[9] When analyzing the

[9]Our impact estimates of regular ALMP are based on monthly data and our outcomes are monthly variables comparable to the annual ones used in the rest of the analysis. They measure any employment in the month, (employment as a fraction of a full working month in the Appendix,) and gross labor market income in the month multiplied by 12 and denoted in US dollars and 2015 prices.

impacts of policies in the short (1 to 5), medium (6 to 10), and long run (11 to 15), we take a simple average of the individual outcomes within this interval and use this as our outcome variable. For the ALMP intervention ("Industry Packages") that we analyze, we can only estimate the impact in the first year after placement, so this is the actual employment or earnings in that year.

Our data include refugees who settled in a Danish municipality from 1986, when Denmark introduced its first spatial dispersal policy, up to 2008.[10] This implies that we are able to follow all refugee arrival cohorts for at least 10 years and in most cases for 15 (one contribution of our paper is to show effects of immigration policies beyond the first 1-2 years after treatment).[11] As shown by the summary statistics in Table A.6, refugees in the sample are, on average, 31 years old, 56% of them are male (versus 51% among natives), the largest origin groups are Bosnia and Iraq, and 54% of refugees only have basic education (versus 41% among natives). Additionally, their average employment rate and income on arrival are far below the averages for natives.

4.2 Refugee Cohort Samples

We will describe here the four arrival cohorts that we use to evaluate the impact of each of four types of policies.[12]

The first arrival group that we consider obtained refugee status between 1987 and 1998 and was subject to the initial (conditionally random) dispersal policy across municipalities. In the early years of this period, the largest groups of refugees were from Lebanon (usually Palestinians), Iran and Iraq. In the later part of the period, the largest groups came from Somalia and Iraq.

In this period refugees received basic job skills assistance, job search support and lan-

[10] January 2008 to April 2019 when we analyze a recent ALMP intervention, which was rolled out in Danish municipalities in this period.

[11] This is not possible with the ALMP intervention, where we can only look at outcomes after one year.

[12] The reader can see the specific summary statistics for each cohort in Table A.7 of the Appendix.

guage training during the first 18 months of their stay. There was no constraint to secondary settlement, but a significant share of refugees remained in the municipality of first settlement even after many years (on average about one third of them were still in the initial municipality, after 15 years).

The next cohorts we consider obtained refugee status in a four- year window between 1997 and 2000.[13] During this period they experienced the largest expansion of language training in Denmark since the beginning of our observation window. The 1999 reform, substantially increased language training, added civic education and made those mandatory in order to receive welfare support during the first 3 years in Denmark. It also delegated municipalities to provide these services and gave them the economic resources to do that. Only refugees recognized after January 1, 1999 qualified for such reform. The largest countries of origin in this group were people from Afghanistan and Iraq, where the brutal regimes of the Taliban and of Saddam Hussein were strengthening their grip on those countries.

The next group we consider are those who arrived in the period 2001-2003, which is in a three-year window around the introduction of the "Start Aid" program in Denmark. This program shifted the focus to job search and strong incentives for refugees to join the labor market immediately. The main change is that it implied a reduction in welfare benefits of close to 40% for many young individuals. The reduction in welfare benefits was, however, cancelled in 2012, and the previous level of transfers was reinstalled.[14]

Finally, we will look at the post-2004 group of refugees for whom policies remained relatively static until 2012, when welfare support was reinstated, and various labor market integration policies were introduced between 2013 and 2016 as the number of refugees was increasing again due to the Syrian crisis. For the more recent arrivals in this group, we are unable to analyze the long-run effects of these more recent policies.

[13]Somali refugees are excluded from the analysis using this group as the changing nature of conflict in Somalia changed the processing of asylum applications.

[14]In the analysis of this group we omit refugees from Bosnia and Afghanistan, as they experienced large changes to processing of asylum applications at the time of the reform.

5 Dynamics of Employment and Earnings of Refugees

5.1 Measuring Labor Market Dynamics by Years Since Asylum

We produce an average profile of the labor market outcomes of refugees since the time of asylum in Denmark, controlling for individual observable characteristics and for characteristics of the assigned municipality based on the following regression:

$$y_{it} = \sum_{k=1}^{15} \beta_k (YSA_{ki}) + \gamma X_{it} + \delta Z_{mt} + \gamma_m + \varepsilon_{it}, \tag{1}$$

where the variable y_{it} is an indicator of employment or the annual earnings for refugee i in year t. The variable YSA_{ki} (acronym for "Years Since Asylum") is a dummy equal to one if refugee i has been granted asylum in Denmark in year $t-k$. Notice, that this is not exactly the time of arrival in Denmark for most refugees, but it is the first time that they can access labor markets and are not mandated to reside in a refugee reception center. The coefficients β_k for $k = 1...15$ capture the conditional average difference in the considered outcome relative to the first year after asylum (β_1 is standardized to zero) and we plot those in Figures 2 and 4, while Figure 3 plots similar estimates including native born in the regression and interacting cohort of arrival dummies and the "year since asylum" variables with a dummy equal to one in the case of refugees and zero for natives.

The vector X_{it} in equation (1) contains a cubic in age, country of origin (reference is Afghanistan), gender, single (reference is cohabiting or married), dummies for having children aged 0-2 and 3-17 (reference is no children). We also include a set of cohort-of-arrival dummies allowing the following cohorts of arrival 1986-1996, 1997-2000, 2001-2003 and 2004-2008 to have different initial intercepts. The choice of arrival cohorts captures the different samples used separately in our policy estimations.[15] The vector Z_{mt} represents a set of controls specific to the initial location. These are the municipal employment rate,

[15] Our ALMP evaluation extends the sample to arrivals up to April 2019.

the total population in the municipality (share of country total), and share of non-Western immigrants in the municipality. Finally, γ_m is a set of municipality-of-residence fixed effects, and ε_{it} is a zero mean idiosyncratic error.[16]

5.2 Average Dynamics

The estimated dynamics of the average outcomes of refugees since asylum are reported in Figure 2. Panel (a) shows the evolution of the employment probability for the average refugee in the period considered, and Panel (b) does the same for earnings. Employment is a dummy for any employment in the year and earnings are the gross labor market income in thousands of US dollars and converted to 2015 prices.

These graphs show three important pieces of information. First, over 15 years the employment probability and earnings of refugees grow significantly. Second, they are still only about 0.4 and USD20,000 in the long run, relative to 0.8 and around USD38,000 for the average native. Such limited convergence confirms that Denmark is quite typical among European countries in terms of refugees' economic success (see Brell, Dustmann, and Preston, 2020). Third, by far the largest improvements take place in the first 5 to 6 years (the "integration phase"). Three quarters of the 15-years of employment probability growth and two thirds of the earnings growth take place by year 6. This suggests that a better integration trajectory during the early period could have important consequences in the long run.

5.3 Catching up to Natives

To see how the labor market dynamics of refugees compare to those of similar natives, we include all native-born individuals in the sample used to estimate equation (1). Each cohort of arrival dummy interacted with the refugee dummy provides the estimate of the initial gap of this cohort of refugees relative to natives with the same observable characteristics and

[16]The regressions are based on full-population register data, and we do not use weights (except for triangular kernel weighting in the RD designs).

living in the same municipalities. In this case, the β_k coefficients will capture the outcome relative to natives, i.e. netting out the average experience premium for similar natives. These estimates for employment probability and earnings are shown in Panels (a) and (b) of Figure 3.

The progression of the estimates, which now represents the catching up to natives with similar observable characteristics, is much more compressed than in Figure 2, especially for earnings. This is because a significant fraction of the employment and earnings growth is simply a labor market experience premium, largely captured by the age progression of similar natives. Refugees in our sample start with an employment probability gap relative to natives of 71 to 79 percentage points (see arrival cohort estimates in Table A.5. This is almost the full employment rate of natives as refugees have employment probabilities close to 0 in the early years. Similarly, they start with a USD40,000 to USD42,000 earning gap. Both variables exhibit "catching up growth" in the first 5-6 years only. After that a flat (or even slightly declining) profile relative to natives prevails. [17] For the employment probability there is a convergence by 22 to 29 percentage points, mostly achieved in the first 5 years. For earnings, the convergence peaks at USD1,600. Relative to this very limited convergence, policies that may produce five percentage points additional employment that lasts in the long run could be considered as being very successful. Similarly, on improvement in earnings of USD2-3,000 yearly would be very significant. We will keep these convergence rates over 15 years in mind when discussing the success of the four types of policies.

5.4 Heterogeneity in Dynamics

The profiles in Figures 2 and 3 show the dynamics averaging all refugees. Different cohorts of refugees, however, have different labor market conditions and characteristics that determine their skills. In Figure 4 we show the absolute dynamics (i.e., not relative to natives) of the employment probability (left panels) and earnings (right panels) by arrival cohort (Panels a

[17] Schultz-Nielsen (2017) and Bratsberg, Raaum, and Roed (2017) show similar patterns of initial convergence and then a widening of the gap after five to eight years.

and b), by geographic area of origin (Panels c and d) and by gender (Panels e and f). The growth profiles of both variables show a slowing down from early cohorts (1986-1996) to more recent ones (2004-2008). In the early cohorts, refugees from Bosnia represented the largest group, while in the more recent ones, especially around the 2000 arrival cohort, Afghani, Iraqi and Somali refugees represent the majority. The next panels show the significant difference in dynamics between the refugees from Europe and Asia (better performance) and those from Africa and the Middle East. Possibly because of larger cultural differences, lower quality of schooling and differences in unobservable skills needed in the labor markets, the group of refugees from Iraq and the Middle East had a much lower convergence in labor market outcomes. The cumulated difference in growth over 15 years between these two groups and European refugees was as large as 20 percentage points in employment probability and USD12,000 in earnings. Similarly, women had a much lower cumulative growth than men in those outcomes.

These figures suggest that besides assessing the average effect of policies, it would be very important to evaluate possible differential effects at least by gender and area of origin. This will help us establish which interventions help particular groups that are more likely to fall behind, and which help groups that are already positioned to do better on the labor markets.

6 Identification, Estimation and Causal Impact of Policies

6.1 Industry Packages: Staggered Difference in Differences across Adopting Municipalities

The first policy that we evaluate, following the approach of Foged, Kreuder, and Peri (2022) is the so called "Industry Packages". This an example of an active labor market policy

(ALMP) as it involved some support to search for a job and on-the-job training. 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. Within this program, newly-arrived refugees with no higher education were matched to employers offering training and potential job opportunities in occupations experiencing shortages of labor. The refugees select an occupation/industry in which they receive intensive training. The program has a clear structure and learning goals specific to the selected occupation/industry. The duration of the training depends on the progress of the participant. Some acquire all the required competencies within a few months, while other took up to a year to complete the training.

We use a repeated cross-section of refugees observed one year after placement and analyze the impact of the policy using the staggered roll-out across Danish municipalities between 2008 and 2018, and considering treated municipality-arrival cohorts relative to arrival cohorts in municipalities that had not yet adopted the program. Hence, we estimate the impact of the policy in the short run. The design is not suited to estimating impacts beyond the first year because cohorts arriving prior to implementation of the policy eventually have the possibility to enter the program. The innovative approach of this policy, i.e., tackling the refugee assimilation problem with an eye to addressing labor shortages, makes its evaluation interesting even though we only assess the short-run impact.[18]

6.1.1 Empirical Specification

We define event time for refugees placed in municipality m at time t, relative to the month of introduction of the policy in that municipality, E_m. Hence, event time is $K_{t,m} = t - E_m$

[18]The other ALMP intervention that can be analyzed with a credible identification strategy in the Danish data is the 2016 reform analysed by Arendt (2022). The analysis design adopted by Arendt similarly only allows estimation of the impact in year 1.

representing the month of arrival of the cohort relative to the introduction of the policy, and we use a repeated cross-section of monthly refugee-arrival cohorts in Danish municipalities observed after one year. Refugees who settle in a municipality after implementation of the program gets treated on arrival in the municipality. It is very likely that refugees arriving just a few months before the implementation are considered "newly-arrived" (the target group) and are also included in the program. Treatment, therefore, does not "switch on" (from 0 to 1) in the month of implementation. Instead, we can define exposure to the policy as the share of the first year in the municipality where the policy was available. This is different from dose-response in the cross-section dimension and the modern staggered difference-in-differences estimators do not cover this situation. One could mimic the binary treatment either by throwing out one year prior to implementation (the potentially treated who do not have access to the program from arrival) or assuming that treatment switches from 0 to 1, e.g., for those arriving six months prior to implementation. We find those approaches somewhat arbitrary and proceed with Ordinary Least Squares in the main specification.

This approach is fine if treatment effects are homogeneous across month of implementation of the policy and across time since implementation. (Goodman-Bacon, 2021) also shows that many periods prior to the onset of treatment reduce the potential problem with forbidden controls (that arise under treatment effect heterogeneity), because observations after the onset of treatment weigh very little in this case. We have monthly data on refugees who obtain asylum and are being assigned to a municipality between January 2008 to April 2019. The pilot municipality (Vejle) implemented the program in august 2013, while the remaining municipalities implemented from 2015 to early 2018. Hence, the observations after the onset of treatment weight relatively little in our case. Consistent with this, we obtain similar results if we drop observations after event time 1. The event-study graph in Figure A.1 in the Appendix shows similar trends for consecutive refugee-arrival cohorts up to one year before implementation of the policy, then an impact of the policy emerges and seems relatively stable and statistically significant from half a year prior to the cohort that arrived

in the month of implementation.[19] This is consistent with the fact that most refugees are able to complete the "Industry Package" program within 4 to 5 months and, hence, refugees arriving shortly before implementation of the policy in the municipality are likely to have enough time to pass through the program and find employment before being observed one year after settlement in the municipality.

Furthermore, Foged, Kreuder, and Peri (2022) provide evidence consistent with constant effects of the policy across municipalities / time of implementation. While the limited number of municipalities adopting the policy (30) leaves the power of this method limited, the lack of pre-trends, the stable coefficients after the onset of treatment, and the robustness of the results to several specifications and subsamples with respect to time of implementation (see Foged, Kreuder, and Peri, 2022) provides some reassurance.

We use the following specification to estimate the average intention to treat (ITT) effect of the policy on the outcome of refugees one year after arrival:

$$Y_{i,t+T,m} = \alpha f_i(K_{t,m}) + X_i'\beta + \gamma_t + \sigma_m + \varepsilon_{i,t,m}, \tag{2}$$

where 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, and α is the parameter of interest capturing the causal effect of the policy. Standard errors are clustered at municipality of assignment.

In the regression we control for calendar month and year of asylum and municipality fixed effects as well as individual refugee characteristics summarized in X_i.[20]

[19] Note, that we bin event time in half year bins in Figure A.1 to increase the precision of the reported parameters.

[20] Not all municipalities receive newly recognized refugees each month. OLS extrapolates over such holes in the repeated cross-section, while the entire observation is dropped in some of the more flexible modern DID estimators. Hence, in an alternative specification (not shown) we aggregate to quarterly data to reduce the problem of holes in the repeated cross-section of refugee-arrival cohorts. Throwing out observations that settle in the municipality prior to implementation but close enough to implementation to be treated, we can define a binary treatment and get similar point estimates with modern estimators Sun and Abraham (2021); Borusyak, Jaravel, and Spiess (2022) but we lose precision.

6.1.2 Results, Validity Check and Discussion

The average ITT estimated effect of the ALMP "Industry Packages" on the probability of employment and on yearly labor income is shown in Panel (a) of Table 3. The coefficients imply that refugees who arrived in a municipality where the program was available had, on average, a 5 percentage point higher probability of being employed and were earning USD1,600 more in their first year, compared with those who arrived where no program was available. The short-run effect of the "industry packages" policy is non-trivial. A 5 percentage points higher employment rate is comparable to the effect of ALMP involving training, as estimated by (Card, Kluve, and Weber, 2018) one to two years after the program. In our case, as refugees start from such a low initial employment rate, this effect is important.

In Table 4, we perform heterogeneity analyses of the impact of "Industry Packages". Panels (a) and (b) show the separate estimates for men and women and reveal that the effect is all concentrated among men. This group experienced a significant 7 percentage points increase in employment probability and a USD2,000 higher yearly earnings as consequence of the policy. These are definitely economically significant results given the very low baseline of refugees in the first year. The Industry Packages policy, which was designed to generate quick and very direct access to jobs with a high demand for labor, seems to help those refugees who were on the margin of potentially being employable given their general skills and motivation on arrival. Due to the skills, past experience and culture these were often men. Since we estimate an ITT effect, we cannot rule out that the differential effect by gender stems from different program exposure of men and women.

In Panels (c) and (d) we distinguish the impact of the policy on refugees from countries whose main language is closer (Linguistically Close) or farther from Danish, in that the first group uses the same Latin alphabet as the Danish language while the second does not. In this case we do not observe much difference in average effects between the groups, but the group that is linguistically closer shows a very imprecisely estimated coefficient due to very few observations in this category.

Finally, Panels (e) and (f) of Table 4 show the impact of the policy separately for refugees, who were placed initially in a municipality with relatively high/low employment rate for Non-Western immigrants (above/below the median). Interestingly, the effects in employment probability and earnings are similar in both locations. This is reasonable as the program is explicitly aimed at matching immigrants with occupations with significant local labor shortages, independently of the general local opportunities and tightness of the labor markets for immigrants.

The estimates of employment and earning outcomes are only short run (year one after asylum) and should be subject to further investigation. Nevertheless, this new policy that combines alleviation of labor market shortages in occupations with shrinking native labor supply with initial help to get refugees into employment is one to be monitored in future studies.

6.2 Benefit Reduction and Language Training: Regression Discontinuity Design with Date of Admission Threshold

The next two policies, whose impact we evaluate focusing on the same outcomes tracked in the previous section and performing the same heterogeneity analyses, entail a cut in welfare benefits and an increase in language training. These two policies are examples of very typical interventions put in place in the early assimilation phase of refugees. Their evaluations inform us about the impact of changing incentives to work due to reducing cash transfers (welfare) after asylum and of changes in employment opportunities and earnings in response to more language training.

The two policies have been the object of the study of two recent papers, namely Andersen, Dustmann, and Landersø (2019) and Foged et al. (Forthcoming). We follow their approach, but harmonize the outcomes and the estimating equations, extend the time horizon (of Andersen, Dustmann, and Landersø, 2019), and provide the same heterogeneity analyses that allow us to compare them with the Industry Packages policy evaluated in the previous

section and with the dispersal policy we evaluate in the next section.

Due to the way these polices were implemented, they can both be evaluated using a Regression Discontinuity (RD) design. In particular, both policies were implemented with a specific cutoff in date of asylum. Hence, the running variable in both cases is the exact date of asylum was granted relative to this cutoff, $z_i - c$, and treatment, D_i, is equal to one for individuals who obtained refugee status on or after this cutoff date.

Selecting individuals whose refugee status was awarded within a few months of the cutoff date and controlling for distance to the cutoff, we show that being on either side of the threshold is as good as random. In particular, we can show that being treated or not is not correlated with any characteristic of the refugee or with the initial location where he/she is placed.

With such quasi-random assignment of treatment near the cutoff date, we can estimate the impact of each reform on our two key outcomes by comparing the outcomes of refugees admitted before and after the cutoff date in a regression discontinuity design.

The reform that cut welfare benefits by about 50% was evaluated in Andersen, Dustmann, and Landersø (2019). This reform was proposed, approved and very quickly implemented in 2002 with a threshold in admission dates at July 1, 2002. Andersen, Dustmann, and Landersø (2019) analyze the impact of the welfare cut and emphasize the positive employment effect on males within the first five years after admission, but also the negative impact on disposable income, as the earnings far from make up for the lost cash transfers. They also pointed out the negative effects on women's participation rate, on children's schooling and higher crime rates. We will replicate their results for employment and earnings and extend them over time to cover 15 years.

The language training reform made refugees eligible to 430 additional hours of language training in the first 3 years following asylum being granted.[21] The policy was evaluated in

[21]On average, qualifying refugees attended 200 extra hours relative to those not qualifying. The reform also added additional resources, for instance, to improve the qualification of teachers, and introduced a common structure for the language training and national tests.

Foged et al. (Forthcoming). This reform was introduced in with a cutoff date January 1, 1999. Foged et al. (Forthcoming) emphasize the long-run effect on earnings, and they also study the impact on schooling and occupational type. In a follow-up paper (Foged et al., 2022) they study the intergenerational spillover effects.

6.2.1 Empirical Specification

To estimate the impact of the two reforms, we first calculate each refugee's average employment rate (probability) and average earnings after the reform in 5-year intervals. Then we estimate an average treatment effect in the short (1 to 5), medium (6 to 10) and long run (11 to 15 years after the reform). The refugees we consider are between 18 and 55 at the time they obtained refugee status so that we can follow them for 15 years.

The impact of the reform on the outcomes of refugees in the interval s after the reform is obtained from the following standard RD model:

$$y_{is} = \tau^s D_i + \alpha_0^s + \alpha_1^s(z_i - c) + \alpha_2^s D_i(z_i - c) + X_{it}\alpha_3^s + \varepsilon_{is}, \tag{3}$$

where τ^s is the average treatment effect in five-year interval s, D_i is the treatment indicator, and X_{it} is a set of individual controls that includes individual refugee characteristics (see Table 3 for details). Equation 3 allows outcomes of individuals to depend linearly on the distance in days between the threshold date and the date individual i was granted refugee status, $z_i - c$ (the running variable), with a potentially different slope before and after the discontinuity.

We estimate the equation using weighted least squares and a triangular kernel to give more weight to observations near the cut-off. We use a bandwidth of 548 days on each side of the cut-off, and we cluster standard errors by date of asylum.[22]

[22]We follow Andersen, Dustmann, and Landersø (2019) in the choice of the bandwidth. They use a bandwidth of 1.5 years and provide robustness checks for bandwidths between 1 year and 1.5 years in Appendix Table A.2. The mean-squared-error optimal bandwidth used in (Foged et al., Forthcoming) is smaller (usually about half a year depending on the outcome and the time horizon), but the estimated impacts of the language reform are robust to bandwidth sizes from 3 or 4 months and longer (and stable but

6.2.2 Validity Checks

Both Foged et al. (Forthcoming) and Andersen, Dustmann, and Landersø (2019) provide several validity checks to ensure that the characteristics of refugees and of the location where they were first placed are randomly distributed between treated and control groups (i.e., around the cut-off date). Here we show in column 1 (for the welfare reform) and column 2 (for the language reform) of Table 2 the partial correlations between the dummy that indicates inclusion in the treatment and dummies capturing gender, age interval, single status, presence of children and continent of origin on arrival. The OLS coefficients in column 1 show that no individual refugee characteristics have a significant correlation (at 5% level) with the treatment status, and jointly we cannot reject at the 10% confidence level that they are all equal to 0 (p-value 0.13). Similarly column 2 shows no significant correlation between refugee characteristics and inclusion in the "language reform" treatment. In this case the joint significance of the coefficients is lower (p-value of 0.43).

Additional checks relative to randomness in number of applications processed, time to approval, and characteristics of initial location are performed in Foged et al. (Forthcoming) and Andersen, Dustmann, and Landersø (2019) and they show that the distribution of refugees between treatment and control groups is as good as random.

6.2.3 Main Results and Discussion

Panel (b) of Table 3 shows the main effect of the 50% cut in welfare benefits (equivalent to about USD6,000 yearly measured in 2015 prices) on employment probability and earnings of the average treated refugee in the short (1-5 years), medium (6-10 years) and long run (11-15). Two facts emerge. First there is a positive significant effect in the short run, as the probability of employment of treated refugees increases by about 5 percentage points and their yearly earnings increase by about USD1,900. When one analyzes the impact in each year after the reform, this effect is largest in years 1 and 2 (about 10%) and decreases

less precise for smaller bandwidths).

significantly already by year 5 (see Andersen, Dustmann, and Landersø, 2019, for a careful analysis of the short run). Second, the average employment and earnings effects are small and not significant in the medium and long term. In fact, the point estimates in the long run are negative and one can rule out a permanent effect of 5 percentage points.

When we capture both the intensive and extensive margin of work by measuring employment as a fraction of the full-time full-year worked (Appendix Table A.11), we find a smaller and less significant impact (+0.029 share of full employment in the short run). This is an indication that cutting welfare did not change the precariousness of the jobs of refugees. Note, also, that the increase in employment and earnings during the short run, when the refugee experiences a reduced amount of transfers by USD6,000, are far from replacing the transfer income with earnings, as they make up less than one third of the lost income (see Andersen, Dustmann, and Landersø, 2019, for details). A central finding in Andersen, Dustmann, and Landersø (2019) is the significant increase in the crime rate of treated refugees. The potential cost of such crime needs to be added to the lack of labor market benefits in the medium and long run to fully evaluate the effectiveness of this policy (we will return to this in Section 7).

Panel (c) of Table 3 shows the effects of the 1999 reform that increased language training (quantity and probably quality). The effects on the employment probability are shown in the same way as we did for the welfare benefit policy and there are several important features of these results. First of all, and unlike the welfare cut, increased language training has a significant permanent effect, increasing the employment probability by 5 to 6 percentage points from the short to the long run, and increasing earnings up to USD3,000 per year in the long run. The persistence and significance, especially of the effect on earnings, reveals that better language skills seem to genuinely increase the value of human capital and earning ability of refugees.

Foged et al. (Forthcoming) show several features that help understand the mechanisms.

The effects of the reform on the employment rate begin to emerge after the refugee has completed the language training. For younger individuals it encourages more formal schooling, mainly in the form of professional degrees. The language reform also shifted occupations held by refugees towards more communication intensive jobs. All these features suggest that the reform worked through improving language skills of refugees.

The permanent improvement in employment and earnings for refugees implies a very significant present discounted gain from this policy for the refugees (see Foged et al., Forthcoming, for details). Considering the persistent increase in employment and earnings experienced from year 1 to year 15 since asylum this policy increased employment and earnings by 12% and 15% of the total, respectively.

6.2.4 Heterogeneity Analysis: Gender, Original Language and Initial Placement

We analyze the impact of the reforms on specific groups of refugees. As we showed in Section 5.4, female refugees and refugees from countries with culture/language farther from Denmark experienced slower employment and income growth we asses here whether some specific reforms especially helped those groups.

Table 5 shows the estimated average treatment effects of the welfare reform separately for males and females (Panels a and b); for refugees from linguistically similar (Latin-based) or linguistically far countries (non-Latin based)[23] in Panels (c) and (d); and for those initially placed in labor markets with a high (above median) or a low (below median) employment rate of Non-Western immigrants. The estimates show that the statistically significant effects, namely those in years 1 to 5, are larger in magnitude for males (+7.7% in employment and +USD3,200 in earnings) and much smaller and not significant for women. No significant effect is detected for the more disadvantaged refugees from "Linguistically far" countries

[23]Foged et al. (Forthcoming) were able to use the mother tongue of the refugee for the definition of linguistically close and far. We cannot do that here, because this information is only available around the time of the 1999 reform.

(+0.4 employment effect and USD1,000 effect, not significant), while larger effects, though still not significant are estimated for refugees from linguistically closer countries.

We also separate the effect of welfare cuts on refugees initially placed in locations with better economic opportunities (higher average employment rates of immigrants) relative to those in locations with worse opportunities. Considering initial location as a dimension for heterogeneity analysis will shed light on the potential complementarity of cutting welfare and placing refugees in strong labor markets. The different estimates are reported in Panels (e) and (f). The positive short-run employment and earnings effects only accrue to refugees located in labor markets with better opportunities. This seems reasonable. As this policy did not provide any better skills or better information to refugees, but only pushed them into the labor market faster, the limited short-run effects only occur where local employment opportunities are adequate.

Table 6 shows the same heterogeneity analyses, as in Table 5, but for the effects of the language reform. The picture that emerges is very different.

First, females experienced the largest long-run effect on employment probability and earnings. While it took longer to accrue, the effects in the long run were almost twice as large on employment (+5.7 percentage points versus +2.9 percentage points) and 20% larger in earnings. Females may take longer to complete language training and longer to find a job afterwards possibly due to childbearing (median age is 31). It is also possible that they invest in additional education to a larger extent than men. In Foged et al. (Forthcoming) we show that language training significantly increases refugees' access to occupations like "assistance of elderly and disabled", which has a relevant communication component and is overwhelmingly done by women. Additionally, the language reform benefits more refugees from linguistically far countries in the long run. The impact on their earnings is almost +USD4,000 in the long run, which corresponds to a remarkable 40% of the full increase in earnings over the first 15 years of residence for this group. Finally, while the estimates be-

come less precise and significant, when split by economic opportunities in the initial location, there is weak evidence that refugees placed in locations with a higher immigrant employment rate get larger employment and earnings benefits from the reform. In the point estimate, the earnings effect in high employment areas becomes very large (+USD4,600) but also very imprecise.

Summarizing the results for the welfare and the language training policies we can say that the welfare cut did not show any significant effect except in the short run and for males and for those living in location with better employment opportunities. On the contrary, the expansion of language training shows significant long-run results on employment and, especially on earnings, and those are stronger for women and for those coming from countries linguistically more different from Denmark, which are the groups that tend to have weaker economic outcomes.

6.3 Initial Placement: Ordinary Least Squares with Conditionally Random Assignment

The type of policy whose impact on refugees' integration has been studied the most in all countries is the initial placement of refugees. This was not a policy targeted at the integration of refugees; rather, it was an administrative procedure aimed at avoiding excessive concentration of refugees in a few locations. However, as the economic and social characteristics of initial location may significantly affect refugees' labor market success, the analysis of this policy can generate important suggestions.

In many countries, at least during some periods, the locations where refugees were initially settled after they received their refugee status was determined by the authorities. While they were subsequently free to move, several initial integration policies (training, transfers, housing) were only offered in the municipality of first assignment. This generated temporary incentives to stay and the potential of the initial location to impact the economic fortunes of

refugees, at least for a while. Researchers have exploited the fact that authorities allocated refugees to locations. The idea is that the initial placement is as good as random conditional on the few observable characteristics that the authorities know when placing them, and therefore uncorrelated with their unobserved characteristics.

For Denmark, refugees arriving between 1987-1998 were subject to a Spatial Dispersal Policy in which the Danish Refugee Council (DRC) never met with them and only knew origin and family characteristics when assigning them to a municipality in Denmark. Therefore, conditional on these observable characteristics, they were distributing refugees quasi-randomly. Several authors (Damm, 2014; Damm and Dustmann, 2014; Damm and Rosholm, 2010) used the conditionally random initial allocation to assess the effect of several location characteristics on several outcomes of the refugees, mainly in the short run. In particular, many focused on the economic conditions of the initial location, often summarized by its employment rate. Other studies focused on the presence of co-nationals of the refugees (referred to as "network" or "enclaves") to analyze whether support of a co-ethnic network helped or hindered job finding and wages in the short run.

6.3.1 Empirical Estimation

In our estimates we adapt and follow the basic approach described in Damm (2014) and Damm and Rosholm (2010). Consistent with the preceding analyses, we consider the refugees' employment probability and earnings outcomes in the short, medium and long run. We regress them on some characteristics of the initial location, after controlling for the refugees' individual characteristics that could be observed by the authorities.

As, conditional on origin country and family characteristics, the initial location should be uncorrelated with observable and unobservable skills of refugees, we simply estimate the following regression using Ordinary Least Squares:

$$y_{is} = \beta_0 + \beta_1 empl\_rate\_nw_{mt} + \beta_2 share\_conationals_{mt} + X_{it}\beta_3 + \beta_4 cohort_i + \varepsilon_{is}. \qquad (4)$$

As usual, the outcome y_{is} is the employment probability or the yearly earnings for refugee i in year-interval s since being granted asylum. The refugee was initially placed in municipality m in year t. The dependent variables of interest are the municipal employment rate of Non-Western immigrants in municipality m in the year of first placement t, $empl\_rate\_nw_{mt}$, and the share of co-nationals in the population of municipality m in year of first placement t, $share\_conationals_{mt}$. Several studies have focused on these two variables and correlated measures of those variables, capturing the strength of the labor market (first measure) and the availability of support of co-nationals (the second variable). To interpret more clearly the magnitude of their coefficients both variables have been divided by their standard deviation. Note that a main difference from earlier studies that focus on either labor market tightness or networks is that we include both in the same equation.

Municipality characteristics are correlated. Therefore, while the location is conditionally random, this randomization does not allow us to differentiate between the impact of correlated local variables. Table A.12 in the Appendix shows the correlation among a set of relevant local characteristics. We see that the employment rate of Non-Western immigrants is positively correlated with the overall employment rate and average earnings in a location, so this seems like a good proxy for the strength of the local labor market. It is not correlated with a dummy for urban municipalities and it is negatively correlated with population density and co-national network presence. The share of co-nationals, is not correlated with the overall employment rate or earnings, but it is positively correlated with population density and especially with the presence of (Non-Western) immigrants in general. With the caveat that these two chosen variables are proxies for some important local characteristics that may affect refugees' integration, we chose those two variables as they were the most analyzed in the literature, and they seem to capture important local features.

X_{it} contains individual characteristics of the refugee on arrival and, in particular, information on his/her family and country of origin, which were observed by the authorities that placed the refugees (see Table 3 for details). $cohort_i$ is immigration cohort fixed effects, con-

trolling for the years when those refugees were first placed in a municipality in Denmark. We cluster standard errors by municipality, which is the dimension of variation of our variables of interest.

6.3.2 Validity

In Columns 3 and 4 of Table 2 we provide tests of the conditionally random assignment to municipalities. We show the coefficients on individual characteristics from a regression using the employment rate of Non-Western immigrants and the share of co-nationals in the municipality of first assignment of the refugee. Note, that the variables capturing the family structure and the country of origin of refugees are not (and are not supposed to be) uncorrelated with initial location characteristics as placement was conditional on those. However, the education measures on arrival, likely also correlated with unobservable skills, and available for this early group of refugees only (as later the initial data collection changed) are not correlated with the municipality characteristics we analyze. Additional validity checks were performed in Damm (2014) and Damm and Rosholm (2010) and confirm orthogonality of additional refugee characteristics and location characteristics, conditional on variables observed by the authorities.

6.3.3 Main Results

Panel (d) of Table 3 shows the impact of the Non-Western immigrant employment rate and the share of co-national in the initial municipality on the trajectory of employment and earnings of refugees in the following 15 years in five-year intervals. Several results are interesting. First, higher employment rates, but not larger networks of co-nationals, are associated with better employment and earnings outcomes in the long run. Second, the positive effect of strong local labor markets accrues over time and is significant already in the short run. This suggests that stronger local economies make it more likely for refugees to find jobs and early access to jobs could make it easier to have a better career.

Third, the magnitude of the local market effect is significant but not huge. Going from a municipality in the bottom 25% of the employment rate distribution to one in the top 25%, which implies an improvement of about 1.3 standard deviations, will generate a higher probability of employment in the long run by about 2% and higher yearly earnings of USD880. These effects are non-trivial but they are significantly smaller than those from the language training reform (which are more than double, for employment and more than triple for income). However, as no other policy that we analyzed shows long-run effects, besides the language training one, these results are important. Finally, the presence of co-nationals does not show any effect in the short or long run. The existing literature is mixed on the role of these types of networks. Some studies find positive short-term effects but no medium-run effects (Battisti, Peri, and Romiti, 2022), several studies show no negative short-run effects (Edin, Fredriksson, and Åslund, 2003) but no study analyzes long-run effects. We are the first to show a null long-run effect of networks on employment and earnings.

6.3.4 Heterogeneity Analysis and Mobility Post-Placement

We perform the same type of heterogeneity analyses for the effects of initial placement as we did for the other policies. Table 7 shows the estimated coefficients when splitting the sample between men and women in Panels (a) and (b) and between refugees from countries that are linguistically close or far from Denmark in Panels (c) and (d). We do not split along the municipality employment rate of Non-Western immigrants, as this is the variable randomized across refugees by the policy conditional on the demographic data that authorities had access to when placing the refugees.

First, we notice that the size of the local network (share of co-nationals) does not seem to show any long-run impact for any subgroup. In some cases, the coefficients for the short run are negative and occasionally significantly negative. Overall, our analysis does not show any support for the idea that the labor market success of refugees is enhanced by a local network of co-nationals in the long-run. Refugees may benefit from the network

because of personal relationships, amenities and other forms of support, but in terms of labor market outcomes we do not see positive effects. Second, the positive impact of the local employment rate is equally strong on male and female refugees' employment, and somewhat stronger for men's earnings. Additionally, the impact of the local employment rate seems to be concentrated among refugees from linguistically far countries, whose coefficients on employment and earnings are much larger and significant, while it is negligible for refugees from linguistically close countries. This suggests that refugees who have a harder time to finding employment may benefit disproportionately from strong conditions in the local labor market. It may also mean that these refugees, who are the more likely to stay in the initial location, benefit more extensively from its conditions. On average, about 36% of refugees remain in the municipality of first placement 15 years after arrival, and among those who are linguistically far, this percentage is 35.4%. Therefore, it is natural to think that better local conditions will result in better long-run opportunities for refugees including for this group.

Appendix Table A.10 shows that the positive effect of initial placement in strong labor markets is fully captured by the positive correlation of current employment probability and better refugee outcomes. Panel (a) in this table shows that controlling for current labor market conditions fully absorbs (eliminates) the positive initial effect. Additionally, Panels (b) and (c) of Table A.10 show no evidence that initial strong labor market implies initial selection by the refugee of better jobs (more complex – Panel b) or better firms (with high average skills of co-workers – Panel c). Most of the positive effect of initial placement in a strong labor market seems to be due to the higher probability of remaining in such a strong market in the long run.

Finally, as the effects of language training were also stronger for linguistically far refugees and for those in strong local labor markets (Table 6), this suggests that the combination of initial placement in strong labor markets and improvement in language skills could be a very powerful policy-combination.

Geographic mobility out of the municipality of initial placement, can be an important

channel for labor market success. Often people need to move to achieve better employment matches. Hence, whether a policy increases or decreases mobility out of the initial municipality can inform us about the channels through which that policy has an impact on refugees' outcomes. Table A.8 in the Appendix shows the impact of each of the policies we evaluated in the short, medium and long run, on the probability of moving out of the original municipality. While the welfare cuts seem to have no effects on mobility, the language reform and the employment rate of initial placement location seem to have opposing impacts on mobility in the short run.

This is interesting and it suggests different mechanisms at work for each reform. The language reform reduces mobility in the early years, and this seems to be related to the very nature of the reform, which provides language classes in the municipality of placement, that are not easily transferable elsewhere. As argued in (Foged et al., Forthcoming) refugees stayed in the initial municipality longer to take the language classes for an extended period of time and this improved their labor market outcomes in the long run. Their overall mobility out of the initial municipality in the medium and long run did not change, suggesting that the results of that reform on employment and earnings proceed from overall better skills and not just better matches with locations and firms. On the contrary, being initially placed in a municipality with strong labor markets, is associated with early employment success, and it seems to increase mobility over any time horizon, suggesting that the positive employment effects in the short run may generate more willingness to invest in moving again, possibly pursuing even better opportunities and better matches or better amenities, once some job success has been achieved. This can be an important channel for long-run success unlocked by early dynamism and better initial jobs. The last row of Table A.8 shows that the presence of co-nationals makes refugees less willing to move out of the initial location. This is consistent with non-labor market benefits from living near co-nationals (consumption amenities, emotional support) and possibly willingness to pay a labor-market success cost for that.

Finally, Table A.9 shows that the positive impact on mobility from the local employment rate and the negative impact from co-ethnic networks seem to be similar for men and women. Possibly the mobility effect is weaker for linguistically far refugees whose stronger labor market success, therefore, is even more driven by better local outcomes and the network effect has a stronger impact on reducing the mobility of this group, as they may need its support more than linguistically (culturally) more similar refugees.

7 Some Considerations on Policy Costs and Benefits

A full Cost-Benefit analysis that includes all the direct and indirect costs and benefits of each program is difficult. Here we limit ourselves to providing some simple evaluations that quantify the present discounted value of the most direct costs and benefits per refugee.

The ALMP intervention analyzed ("Industry Packages") requires some additional effort by the municipality in identifying the occupations with shortages and during the implementation phase in planning and providing some specific training. We do not have a clear quantification of this but these costs are not large.

On the other hand, cutting welfare benefits to immigrants in their first 7 years of residence saves the public coffers expenses of transfers to the unemployed (on average USD34,632 in present value terms using a discount rate of 3%) but is neutral to society as this is a redistribution.

The language training policy has a direct monetary cost which is the additional hours of language training. In Foged et al. (Forthcoming) we calculate the present discounted value of the cost, incurred in the first three years of stay to be USD2,762 per refugee.[24]

Finally, there seems to be no economic cost of placing all refugees in the quartile of

[24]The reform expanded language instruction time by 30% over an extended period of 18 months. We assume that the duration of the additional language training is equal to 6 months (30% of 18 months) and is split equally between the second and the third year after asylum.

In the calculations the operating cost per month is assumed to be equal to the amount paid from the state to the municipality per refugee per month he/she is enrolled in language training (Act on Integration No 474 of 1998, section 45(5-7) and section 59(3-4)).

municipalities with the highest employment rate of Non-Western immigrants (whose value we will calculate below). One issue would be the availability of housing, especially for a large and sudden inflow (akin to the one taking place now from Ukraine). One could evaluate the difference in rent as cost, but usually refugees use public housing and the outside value of that is not equal to the rent.

We can calculate the aggregate gains to the economy as the additional earnings that each refugee generates as a consequence of exposure to the policy. Using a societal discount rate of 3% and the impact on annual gross earnings each year from year 1 to 15 after asylum (including the non significant estimates), we find the following per refugee benefit. The present discounted value (PDV) of benefits from the ALMP, which we only assume for year 1 (as this is the only estimate for one year) is simply USD1,456 per refugee. The extra earnings from the welfare cut have a present discounted value of USD9,706 per refugee. The language training reform generated a PDV of USD30,045 in earnings for each refugee. This is by far the largest gain accruing to refugees and in aggregate. Finally, on average there would be a gain of USD6,623 per refugee if moving those placed in the bottom quartile to the top quartile of municipalities by employment rate of Non-western migrants.

Overall, while the welfare cut reduces transfers to refugees and, therefore, increases money available for other public interventions, in terms of aggregate returns to Danish GDP, and accounting for its costs, the language training policy seems by far the most beneficial. The simple difference between the present value of the causal impacts on gross earnings and the present value of the additional operating costs in year two and three suggests that the net present value of the reform is around USD27,000 per refugee over a 15-year time horizon. Applying this average net return to the 25,000 refugees who arrived in Denmark in the first two months of the war in Ukraine, this implies a present discounted value of 675 million dollars.

8 Conclusions

In this paper we use a common definition of employment and earnings outcomes, similarly defined populations of refugees and quasi-random policy assignments on arrival to estimate the short, medium and long-term impact of four policies adopted in Denmark in the last 30 years on the labor market assimilation of refugees.

While for the "Industry Package" policy, an ALMP that matched refugees with jobs in high demand, we are only able to identify some short-run positive effects on employment and earnings, we find strong and persistent positive effects in the long run from the expansion of language training, as well as for the initial dispersion policy when it randomly placed refugees in locations with high employment rates of Non-Western immigrants. We only found a small and short-run employment incentive effect from cutting initial welfare benefits, received by unemployed refugees in the early years after asylum. We do not find any positive effect of random placement of refugees in locations with a large presence of co-nationals.

We also find that placing refugees in high employment rate locations enhances the long-run labor market impact of learning the local language and especially helps refugees from countries that are linguistically far. Language training seems most beneficial to women and for refugees from linguistically far countries, which are among the very vulnerable and disadvantaged groups.

Placing refugees in the strongest labor markets, further improving their language training in the early years and possibly making an effort to target these policies at females and refugees from culturally far countries may maximize the positive impact on the long-run labor market outcomes of refugees. This will have large benefits in terms of present discounted value of their earnings, which may in part pay for its cost. We hope the findings of the present analysis can be incorporated into policies and further tested so as to improve our understanding and interventions for the success of these populations.

9 Figures and Tables

Figure 1: Timeline of Refugees' Experience and Policies

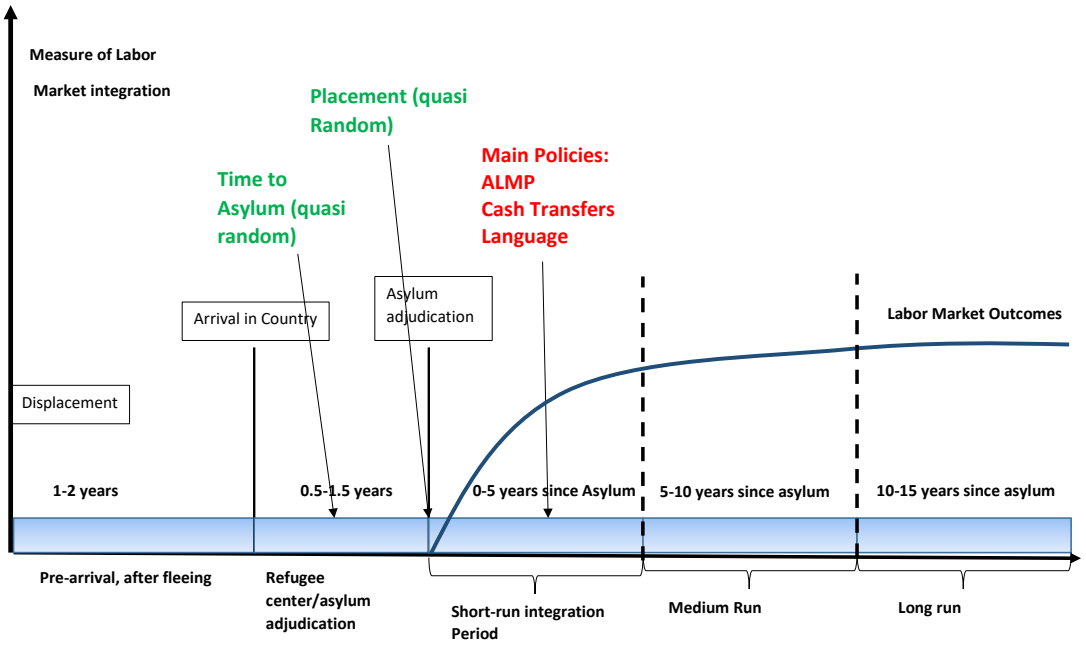
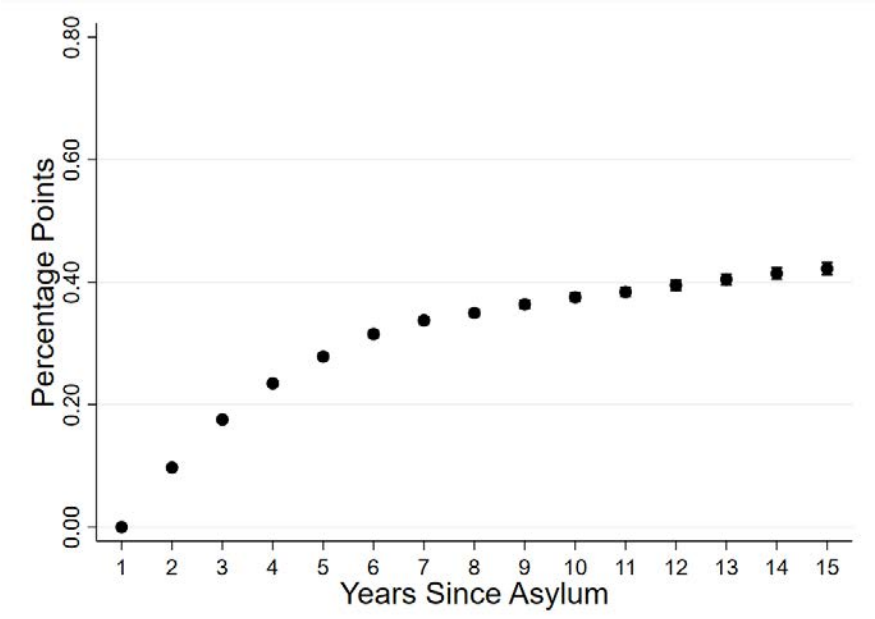


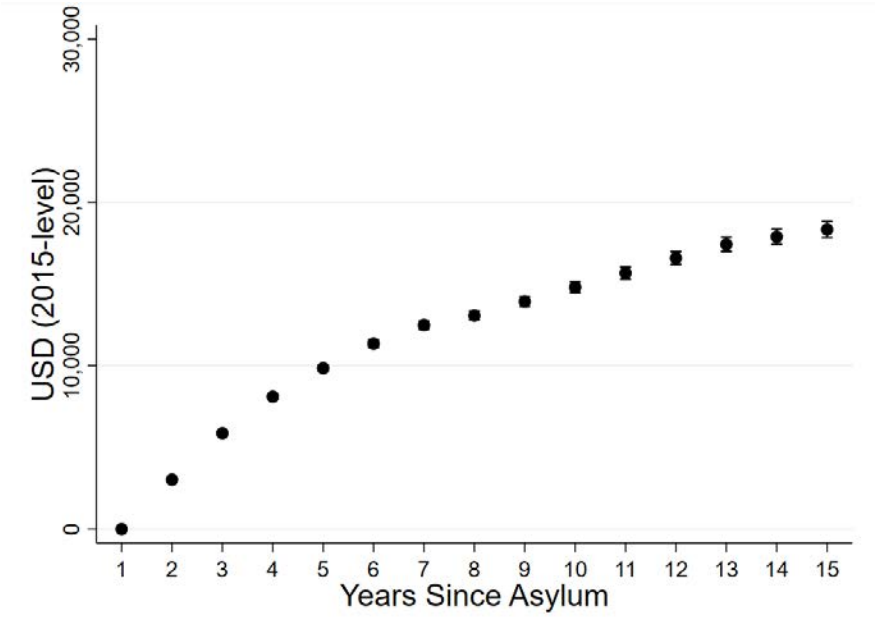
Table 1: Literature

Policy	Studies	Average Effects on Employment Rate			Average Effects on Yearly Labor Income		
		1-5 Years	5-10 Years	10-15 Years	1-5 Years	5-10 Years	10-15 Years
ALMP: Support to search, training, coaching	Average of 4 studies, see Table A.1	0.075	NA	NA	NA	NA	NA
Welfare: Cut in transfers: DK cut by 40%. US cut by USD 1200 per year	Average of 2 studies, see Table A.2	0.038	NA	NA	-USD 1940	NA	NA
Language training: increased hours and quality	Average of 3 studies, see Table A.3	0.017	0	NA	+728 euros	+728 euros	NA
Initial Location, Employment Rate of immigrants	Only one study could translate outcome: Azlor, Damm, and Schultz-Nielsen (2020)	+0.01 employment rate → 0-0.005 empl prob.	NA	NA	positive/0	NA	NA
Initial Location: Network	Many different ways of measuring network. See Summary Table A.4	0	NA	NA	positive/0	NA	NA

Notes: The point estimates reported in this table use those in Tables A.1-A.4 in the Appendix. The average effects are weighted by the inverse of their estimated standard errors, also reported in Tables A.1-A.4 in the Appendix.



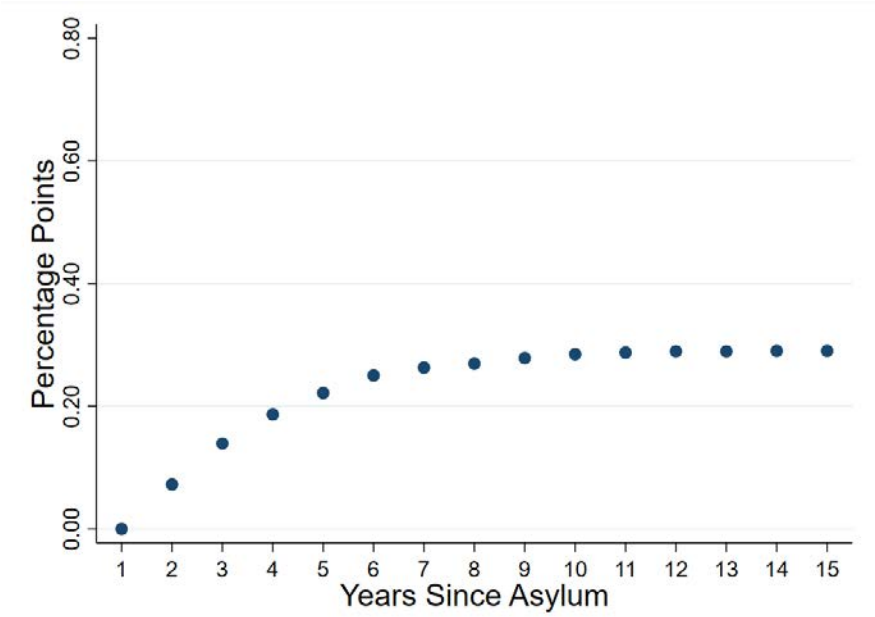
(a) Employment



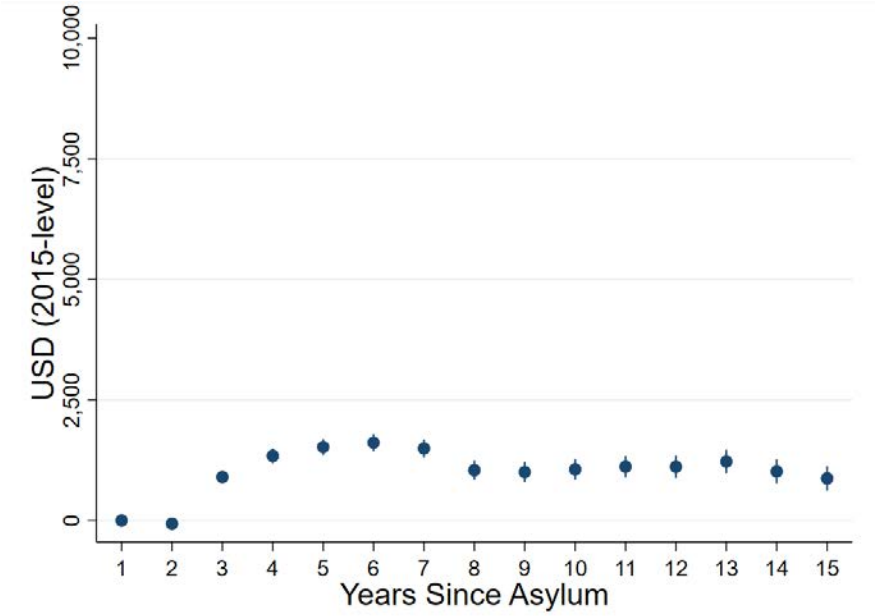
(b) Earnings

Figure 2: Refugee Employment and Earnings by Years Since Asylum

Notes: The graphs show the estimated annual employment and earnings for adult refugees with 95 percent confidence intervals (standard errors clustered by individuals). The estimates are conditional on age, gender, marital status, dummies for having children aged 0-2 and aged 3-17, population share in municipality (of country total), share of Non-Western immigrants in municipality, local employment rate for the full working age population, dummies for different refugee cohorts (1986-1996 is the reference group), origin country fixed effects, and municipality fixed effects.



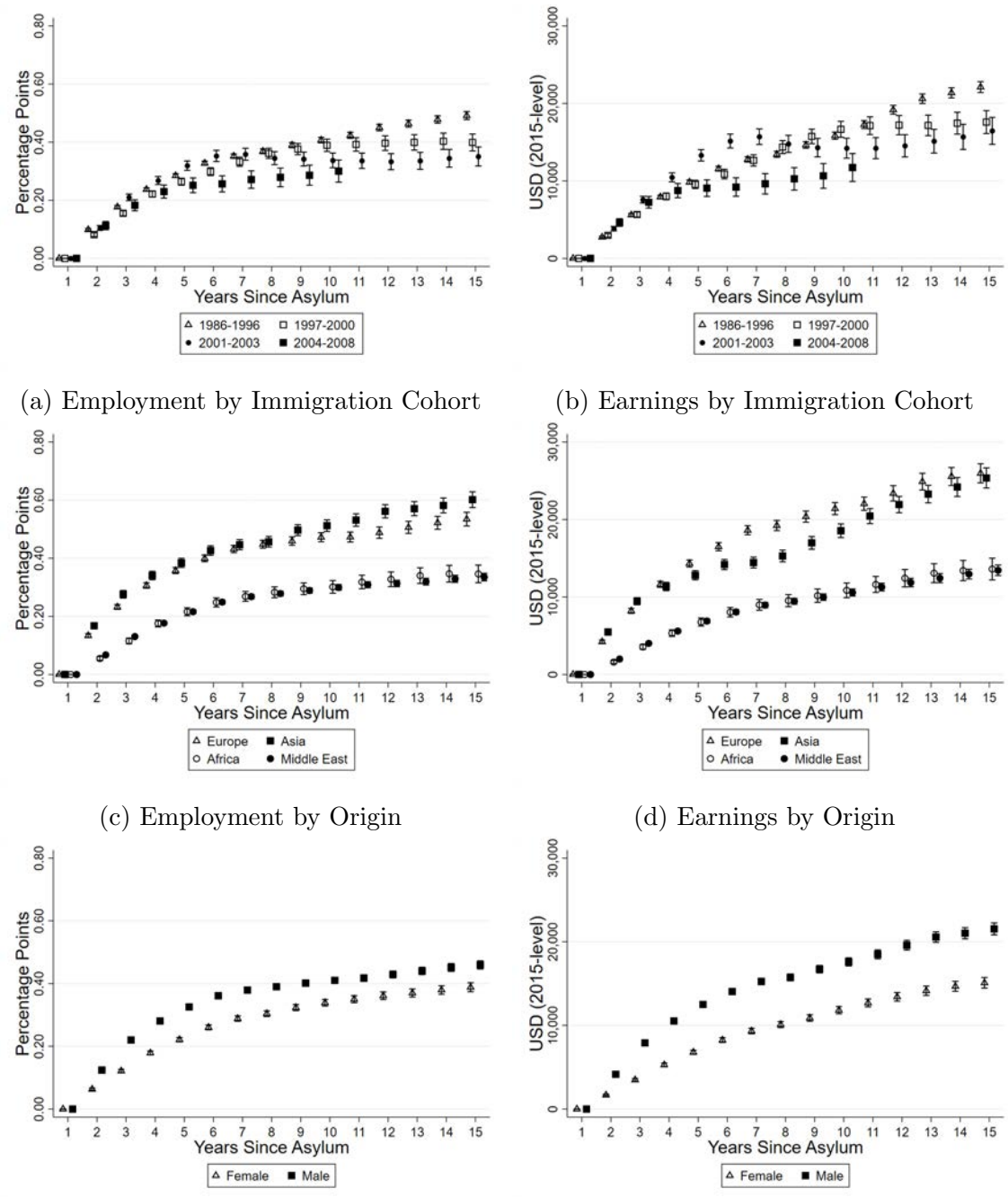
(a) Employment



(b) Earnings

Figure 3: Reduction in Refugee-Native Employment and Earnings Gaps by Years Since Asylum

Notes: The graphs show the estimated annual employment and earnings for adult refugees with 95 percent confidence intervals (standard errors clustered by individuals). The estimates are conditional on age, gender, marital status, dummies for having children aged 0-2 and aged 3-17, population share in municipality (of country total), share of Non-Western immigrants in municipality, local employment rate for the full working age population, dummies for different refugee cohorts (1986-1996 is the reference group), origin country fixed effects, and municipality fixed effects.



(a) Employment by Immigration Cohort

(b) Earnings by Immigration Cohort

(c) Employment by Origin

(d) Earnings by Origin

(e) Employment by Gender

(f) Earnings by Gender

Figure 4: Refugee Employment and Earnings by Immigration Cohort, Origin and Gender

Notes: The graphs show the estimated annual employment and earnings for adult refugees with 95 percent confidence intervals (standard errors clustered by individuals) from four separate regressions based on refugee immigration cohorts in 1986-1996, 1997-2000, 2001-2003 and 2004-2008 in Panels (a) and (b); from four separate regressions based on origin in Panels (c) and (d); and from two separate regressions based on gender in Panels (e) and (f). The estimates are conditional on age, marital status, dummies for having children aged 0-2 and aged 3-17, population share in municipality (of country total), share of Non-Western immigrants in municipality, local employment rate for the full working age population, municipality fixed effects, immigration cohort fixed effects and origin country fixed effects.

Table 2: Balancing Tests

	Welfare Benefits (1)	Language Training (2)	Employment Rate of Non-Western Immigrants (3)	Share of Co-Nationals (4)
Age 30-39y	-0.003	-0.010	0.033	-0.235***
	(0.009)	(0.009)	(0.026)	(0.045)
Age 40-49y	-0.012	0.010	0.011	-0.176***
	(0.011)	(0.010)	(0.037)	(0.067)
Age 50-55y	-0.020	0.006	0.104	-0.185
	(0.017)	(0.017)	(0.078)	(0.127)
Female	0.009	0.010	-0.001	0.085***
	(0.009)	(0.007)	(0.009)	(0.012)
Number of Children 0-2y	0.003	0.005	-0.002	-0.027*
	(0.011)	(0.011)	(0.014)	(0.015)
Number of Children 3-5y	0.000	-0.007	0.009	-0.031**
	(0.008)	(0.008)	(0.014)	(0.016)
Number of Children 6-12y	0.008*	0.001	0.015*	-0.042***
	(0.005)	(0.005)	(0.009)	(0.010)
Number of Children 13-17y	0.008	-0.001	-0.008	0.037**
	(0.006)	(0.006)	(0.013)	(0.018)
Single	0.002	0.013	0.025**	-0.055***
	(0.012)	(0.011)	(0.011)	(0.014)
Africa	-0.028	-0.027	-0.035	0.236
	(0.021)	(0.025)	(0.241)	(0.391)
Asia	-0.061*	-0.005	0.259***	-0.003
	(0.034)	(0.020)	(0.077)	(0.781)
Basic Education			0.003	0.019
			(0.019)	(0.026)
Academic Education			0.041*	-0.027
			(0.021)	(0.028)
Unknown Education			0.007	0.029
			(0.017)	(0.024)
N	4,785	5,888	19,160	19,160
Cohort FE	No	No	Yes	Yes
Origin Country FE	No	No	Yes	Yes
F	1.49	1.01	1.83	2.54
Pr $> F$	0.13	0.43	0.14	0.05

Notes: Robust standard errors in parentheses clustered at date of admission in columns (1) and (2) and clustered at family level in columns (3) and (4). $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. In column (1) the sample is refugees admitted in 2001-2003. In column (2) the sample is refugees admitted in 1997-2000. In columns (3) and (4) the sample is refugees admitted in 1987-1998. In columns (1) and (2) we show estimates from regressing a dummy for admission post-reform on individual refugee characteristics measured at immigration and controlling for admission date. In columns (3) and (4) we show the correlation between characteristics in the municipality of assignment and refugees' characteristics measured at immigration. F denotes the F-test statistic of joint insignificance of all individual characteristics in columns (1) and (2). In columns (3) and (4) F denotes the F-test statistic of joint insignificance of the dummies for educational attainment: basic education, academic education and unknown education (vocational education is the reference). Pr $> F$ denotes the corresponding p-value from the F-test.

Table 3: Estimated Impact of Policies on Employment and Earnings

	Year 1-5 (1)	Year 6-10 (2)	Year 11-15 (3)	Year 1-5 (4)	Year 6-10 (5)	Year 11-15 (6)
	Employment			Earnings		
			Panel a. ALMP			
Estimate	0.051***			1,600.622**		
	(0.016)			(646.504)		
N	8,556			8,556		
			Panel b. Welfare Benefits			
RD Estimate	0.048**	-0.010	-0.022	1,883.896**	423.012	-291.711
	(0.020)	(0.030)	(0.030)	(784.323)	(1,400.426)	(1,663.562)
N	4,787	4,642	4,558	4,787	4,642	4,558
			Panel c. Language Training			
RD Estimate	0.060***	0.063**	0.044*	2,265.991***	3,192.694***	3,213.577**
	(0.017)	(0.025)	(0.024)	(737.790)	(1,239.130)	(1,475.314)
N	5,888	5,717	5,518	5,888	5,717	5,518
			Panel d. Initial Placement			
Employment Rate Non-Western Immigrants	0.010**	0.008	0.015***	361.673**	237.828	675.081**
	(0.004)	(0.006)	(0.006)	(150.125)	(213.789)	(290.158)
Share of Co-Nationals	-0.001	-0.007*	-0.005	-9.288	-313.116*	-262.953
	(0.004)	(0.004)	(0.004)	(123.807)	(180.230)	(263.362)
N	19,160	17,858	16,598	19,160	17,858	16,598

Notes: Robust standard errors in parentheses clustered at municipality level in Panels (a) and (d), and clustered at date of admission in Panels (b) and (c). $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. The table shows the estimated impact of different policies on the average employment rate (columns 1-3) and average earnings (columns 4-6) in 5-year intervals for adults aged 18-55 on arrival, conditional on refugee characteristics on arrival: age (third order polynomial), gender, marital status, spouse in Denmark, dummies for having children aged 0-2 and aged 3-17, refugee (vs. family-reunified), and dummies for Eastern Europe and rest of the world (Muslim countries is the reference). In Panel (a) we further condition on year of immigration fixed effects, calendar month of immigration fixed effects and municipality of assignment fixed effects. In Panels (b) and (c) we show RD estimates using a triangular kernel to weight observations and a bandwidth of 548 days on each side of the cutoff. In Panel (d) we further condition on year of immigration fixed effects and origin country fixed effects (Afghanistan is the reference).

Table 4: Estimated Heterogeneous Impact of ALMP on Employment and Earnings

	Year 1 (1)	Year 1 (2)
	Employment	Earnings
	Panel a. Female	
Estimate	0.014	274.541
	(0.016)	(456.459)
N	3,197	3,197
	Panel b. Male	
Estimate	0.073***	2,250.219**
	(0.025)	(915.497)
N	5,359	5,359
	Panel c. Linguistically Close	
Estimate	0.058	2,123.950
	(0.093)	(2,273.163)
N	680	680
	Panel d. Linguistically Far	
Estimate	0.047**	1,573.764**
	(0.018)	(665.271)
N	7,876	7,876
	Panel e. Above Median Employment Rate	
Estimate	0.047	1,813.251
	(0.028)	(1,128.572)
N	3,551	3,551
	Panel f. Below Median Employment Rate	
Estimate	0.055**	1,579.298**
	(0.020)	(755.127)
N	5,005	5,005

Notes: Robust standard errors in parentheses clustered at municipality level. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. The table shows the estimated impact on the employment rate (columns 1-3) and earnings (columns 4-6) in year 1 for adults aged 18-55 on arrival, conditional on refugee characteristics on arrival: age (third order polynomial), gender, marital status, spouse in Denmark, dummies for having children aged 0-2 and aged 3-17, refugee (vs. family-reunified), dummies for Eastern Europe and rest of the world (Muslim countries is the reference), year of immigration fixed effects, calendar month of immigration fixed effects and municipality of assignment fixed effects. Each panel shows estimates from a regression on a sub-sample.

Table 5: Estimated Heterogeneous Impact of Welfare Benefit Reduction on Employment and Earnings

	Year 1-5 (1)	Year 6-10 (2)	Year 11-15 (3)	Year 1-5 (4)	Year 6-10 (5)	Year 11-15 (6)
	Employment			Earnings		
			Panel a. Female			
RD Estimate	0.031	-0.010	-0.027	880.239	117.480	-1,351.909
	(0.025)	(0.034)	(0.037)	(814.403)	(1,551.077)	(1,856.811)
N	2,372	2,314	2,280	2,372	2,314	2,280
			Panel b. Male			
RD Estimate	0.077**	0.008	-0.005	3,184.219**	1,337.679	1,511.204
	(0.032)	(0.044)	(0.046)	(1,417.963)	(2,269.064)	(2,630.747)
N	2,415	2,328	2,278	2,415	2,328	2,278
			Panel c. Linguistically Close			
RD Estimate	0.049	-0.042	-0.050	1,943.771	-3,260.400	-3,590.592
	(0.036)	(0.048)	(0.055)	(1,212.110)	(2,105.286)	(2,761.802)
N	1,395	1,278	1,217	1,395	1,278	1,217
			Panel d. Linguistically Far			
RD Estimate	0.024	-0.001	-0.019	1,040.602	1,452.700	30.355
	(0.026)	(0.037)	(0.036)	(1,145.850)	(1,865.740)	(2,067.342)
N	3,392	3,364	3,341	3,392	3,364	3,341
			Panel e. Above Median Employment Rate			
RD Estimate	0.065**	-0.027	-0.023	2,150.810*	470.076	-64.593
	(0.032)	(0.038)	(0.036)	(1,166.748)	(1,884.397)	(2,135.078)
N	2,330	2,258	2,221	2,330	2,258	2,221
			Panel f. Below Median Employment Rate			
RD Estimate	0.029	0.011	-0.018	1,480.350	464.919	-140.403
	(0.026)	(0.039)	(0.042)	(1,111.035)	(1,816.811)	(2,222.808)
N	2,455	2,378	2,323	2,455	2,378	2,323

Notes: Robust standard errors in parentheses clustered at date of admission. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. The table shows the estimated impact on the average employment rate (columns 1-3) and average earnings (columns 4-6) in 5-year intervals for adults aged 18-55 on arrival, conditional on refugee characteristics on arrival: age (third order polynomial), gender, marital status, spouse in Denmark, dummies for having children aged 0-2 and aged 3-17, refugee (vs. family-reunified), and dummies for Eastern Europe and rest of the world (Muslim countries is the reference). Each panel shows RD estimates from a regression on a sub-sample using a triangular kernel to weight observations and a bandwidth of 548 days on each side of the cutoff.

Table 6: Estimated Heterogeneous Impact of Language Training on Employment and Earnings

	Year 1-5 (1)	Year 6-10 (2)	Year 11-15 (3)	Year 1-5 (4)	Year 6-10 (5)	Year 11-15 (6)
	Employment			Earnings		
			Panel a. Female			
RD Estimate	0.034**	0.066**	0.057*	1,068.553*	3,371.597***	3,443.580**
	(0.017)	(0.027)	(0.032)	(631.364)	(1,284.331)	(1,643.834)
N	2,539	2,469	2,410	2,539	2,469	2,410
			Panel b. Male			
RD Estimate	0.089***	0.058	0.029	3,624.040***	3,072.734	2,956.758
	(0.024)	(0.036)	(0.034)	(1,069.663)	(1,896.027)	(2,176.131)
N	3,349	3,248	3,108	3,349	3,248	3,108
			Panel c. Linguistically Close			
RD Estimate	0.066*	0.048	0.036	3,219.344**	1,629.972	1,601.678
	(0.038)	(0.049)	(0.056)	(1,601.195)	(2,498.631)	(3,307.543)
N	1,322	1,272	1,211	1,322	1,272	1,211
			Panel d. Linguistically Far			
RD Estimate	0.057***	0.069**	0.050*	1,835.849**	4,001.456***	3,951.971**
	(0.016)	(0.028)	(0.028)	(724.352)	(1,438.381)	(1,730.759)
N	4,566	4,445	4,307	4,566	4,445	4,307
			Panel e. Above Median Employment Rate			
RD Estimate	0.047	0.094*	0.071	1,286.152	3,102.798	4,636.557
	(0.036)	(0.048)	(0.049)	(1,636.304)	(2,664.216)	(3,093.498)
N	2,143	2,084	2,012	2,143	2,084	2,012
			Panel f. Below Median Employment Rate			
RD Estimate	0.059***	0.046*	0.035	2,552.054***	2,989.407**	2,242.900
	(0.019)	(0.027)	(0.029)	(740.405)	(1,311.704)	(1,655.358)
N	3,745	3,631	3,501	3,745	3,631	3,501

Notes: Robust standard errors in parentheses clustered at date of admission. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. The table shows the estimated impact on the average employment rate (columns 1-3) and average earnings (columns 4-6) in 5-year intervals for adults aged 18-55 on arrival, conditional on refugee characteristics on arrival: age (third order polynomial), gender, marital status, spouse in Denmark, dummies for having children aged 0-2 and aged 3-17, refugee (vs. family-reunified), and dummies for Eastern Europe and rest of the world (Muslim countries is the reference). Each panel shows RD estimates from a regression on a sub-sample using a triangular kernel to weight observations and a bandwidth of 548 days on each side of the cutoff.

Table 7: Estimated Heterogeneous Impact of Initial Placement on Employment and Earnings

	Year 1-5 (1)	Year 6-10 (2)	Year 11-15 (3)	Year 1-5 (4)	Year 6-10 (5)	Year 11-15 (6)
	Employment			Earnings		
			Panel a. Female			
Employment Rate Non-Western Immigrants	0.018***	0.008	0.016**	385.177**	175.638	505.738
	(0.006)	(0.007)	(0.008)	(194.074)	(256.315)	(367.159)
Share of Co-Nationals	0.004	-0.007*	-0.006	113.560	-209.815	-409.475
	(0.005)	(0.004)	(0.006)	(175.245)	(179.601)	(281.943)
N	7,978	7,449	6,891	7,978	7,449	6,891
			Panel b. Male			
Employment Rate Non-Western Immigrants	0.009*	0.010	0.017**	469.493**	475.199*	955.612***
	(0.005)	(0.006)	(0.007)	(185.311)	(278.548)	(361.627)
Share of Co-Nationals	-0.004	-0.004	-0.000	-20.281	-252.522	54.293
	(0.004)	(0.006)	(0.006)	(144.405)	(294.869)	(416.533)
N	11,182	10,409	9,707	11,182	10,409	9,707
			Panel c. Linguistically Close			
Employment Rate Non-Western Immigrants	0.020***	-0.011	-0.003	623.929**	-378.789	-289.819
	(0.008)	(0.008)	(0.008)	(261.853)	(336.452)	(362.666)
Share of Co-Nationals	0.006	-0.002	-0.002	226.046	-192.259	-118.982
	(0.007)	(0.006)	(0.006)	(211.759)	(226.381)	(345.378)
N	6,893	6,331	5,557	6,893	6,331	5,557
			Panel d. Linguistically Far			
Employment Rate Non-Western Immigrants	0.004	0.016***	0.024***	205.047	592.348**	1,196.492***
	(0.004)	(0.006)	(0.007)	(137.431)	(254.609)	(393.417)
Share of Co-Nationals	-0.008***	-0.012**	-0.006	-243.859**	-425.185*	-342.552
	(0.003)	(0.005)	(0.005)	(116.569)	(248.401)	(332.559)
N	12,267	11,527	11,041	12,267	11,527	11,041

Notes: Robust standard errors in parentheses clustered at municipality of assignment. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. The table shows the estimated impact on the average employment rate (columns 1-3) and average earnings (columns 4-6) in 5-year intervals for adults aged 18-55 on arrival, conditional on refugee characteristics on arrival: age (third order polynomial), gender, marital status, spouse in Denmark, dummies for having children aged 0-2 and aged 3-17, refugee (vs. family-reunified), dummies for Eastern Europe and rest of the world (Muslim countries is the reference), year of immigration fixed effects and origin country fixed effects (Afghanistan is the reference). Each panel shows estimates from a regression on a sub-sample.

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A Appendix: Additional Tables and Figures

Table A.1: Summary of ALMP Interventions

Author	Identification	N	Country & Period	Initial Intervention?	Employment Effects 1-2 Years	Employment Effects $\geq$ 3 Years	Earnings Effects 1-2 Years	Notes
Arendt (2022)	Before-After	9,250	Denmark 2013-2018 Reform in Oct. 2016	Yes, job-search and training requirements	1 year (avg. 11-13 months): 0.077 (s.e. 0.0315).	NA	1 year (ave. 11-13 months): USD 222 (s.e. 98).	Non-significant effect on women
Battisti, Giesing, and Laurentsyeva (2019)	RCT	420	Germany 2014-2017	Yes, job-matching and information support	1 year: 0.119 (s.e. 0.069)	NA	NA	Includes not yet recognized refugees
Dahlberg et al. (2020)	RCT	140	Sweden 2017-2020 Intervention 2017-2020	Yes, language training, job search assistance and work practice	1 year: 0.17 (s.e. 0.07) 2 year: 0.19 (s.e. 0.08)	NA	NA	Significant language component.
Joona and Nekby (2012)	RCT Multinomial Logit estimation	1,288	Sweden 2006-2008	Yes, intensive coaching and counseling	1 year: 0.032 (s.e. 0.016) 2 Year: 0.058 (s.e. 0.022)	NA	NA	We consider average marginal effects from multinomial logit

Notes: The table lists only studies with well-defined control groups and interventions on immigrants during their early years, and that use a natural experiment (reform) or an RCT for identification. In order to be included, a study needs to report the effects on employment probability and/or earnings.

Table A.2: Summary of Welfare Generosity

Author	Identification	N	Country & Period	Intervention	Employment Effects 1-2 Years	Employment Effects $\geq$ 3 Years	Earnings Effects 1-2 Years	Notes
LoPalo (2019)	DID Across state increase in TANF	70,000	US. 2000-2016	Yes, state-level variation in welfare generosity. Effect per USD 1,200 yearly increase in TANF	0-4 years: 0 (s.e. 0.02)	NA	0-4 years: USD 1,940 $\sim$ -7.7% yearly income (s.e. USD 408)	Effect per USD 1,200 increase in TANF
Huynh, Schultz-Nielsen, and Tranæs (2007)	RDD Date of asylum recognition		Denmark. 2002 reform	Yes, 40% welfare cut $\sim$ USD 6,000 per year	year 1: 0.062 (s.e. 0.025) year 2: 0.058 (s.e. 0.023)	NA	NA	Sensitive to sample. Standard errors inferred from significance of the estimates
Rosholm and Vejlin (2010)	RDD Date of asylum recognition		Denmark. 2002 reform	Yes, 40% welfare cut $\sim$ USD 6,000 per year	NA	NA	NA	Effect on hazard rate of transitioning to employment

Notes: The table lists only studies with well-defined control groups and interventions on immigrants during their early years, and that use a natural experiment (reform) or an RCT for identification. In order to be included, a study needs to report the effects on employment probability and/or earnings.

Table A.3: Summary of Language Training

Author	Identification	N	Country. Period	Initial Intervention?	Employment Effects	Earnings Effects	Notes
Foged and van der Werf (2022)	IV (distance)	10,170	Denmark 2003-2013	Yes, Natural experiment affecting distance to language training from initial placement	Year 5: 0.009 (0.009) [Table 5, column 2]	NA	The coefficient is the impact of one hour shorter commute, which reduces attendance by 70 hours [Table 3, column 2]
Lochmann, Rapoport, and Speciale (2019)	RDD (language score)	2,100	France Residence: 2009 Outcomes: 2010-2013	Yes, 60 to 400 hours of language training	Years 0-4: 0.097 (s.e. 0.039) [Table 4, column 2 and Table 7]	NA	*Contrat d'accueil et d'intégration* introduced in 2007
Sarvimäki and Hämäläinen (2016)	RDD (date of asylum)	10,000	Finland Residence 1990-1999 Outcomes: 2000s	Yes, More language and less general skill and on-the-job training in year 2 [Table 5]	Years 3 to 13 avg.: -0.01 (s.e. 0.01) [Table 4]	Years 3 to 13 avg.: ITT 724 Euros (s.e. 309) and LATE 2,070 Euros (s.e. 911) [Table 3]	Reform came into effect May 1, 1999 and applied to those obtaining residence on May 1, 1997 or later

Notes: The table lists only studies with well-defined control groups and interventions on immigrants during their early years, and that use a natural experiment or an RCT for identification. In order to be included, a study needs to report the effects on employment probability and/or earnings. For Lochmann, Rapoport, and Speciale (2019) the estimates are taken from Table 4, Column 2; Those are estimates on labor force participation, which combined with insignificant effects on employment, conditional on participation in their Table 7, give us an impact on employment/population ratio. For Sarvimäki and Hämäläinen (2016) they are taken from Tables 3 and 4. Foged and van der Werf (2022) report estimates from three similar specifications, we pick the intermediate value of the impact estimate in Table 3.

Table A.4: Summary of Initial Conditions

Author	Identification	N	Country & Period	Initial Intervention?	Employment Effects 1-2 Years	Employment Effects $\geq$ 3 Years	Earnings Effects 1-2 Years	Notes
				Panel a: Employment				
Azlor, Damm, and Schultz-Nielsen (2020)	Initial random placement after exhausting the quota	4,282	Denmark 1999-2010	Yes, Initial placement	Year 1-2: 1% higher empl. Rate→ 0.5% higher prob. empl.	Year 3-4: 1% higher empl. Rate→ 0.6% higher prob. empl.	NA	Non-western immigrants empl. rate effect 0.2%
Damm (2014)	Initial Random Placement		Denmark 1986-98	Yes, Initial placement	No effect	Year 5-6: Ln mean years of education of non-western → increases probability employment	Ln employment of co-nationals increases ln earnings	Most effects on employment are 0
Godøy (2017)	Initial random dispersal		Norway	Yes, Initial placement	NA	NA	Effect of initial employment on ln earnings year 3 to 6 after arrival	
Åslund and Rooth (2007)	Initial dispersal policy 1980-1990s		Sweden 1987-1991 cohorts of entry	Yes, Initial placement	1-5 years: 1% lower unemployment (rate)→ 0.05-0.06% increase probability of employment	6-10 years: same as short run	1% lower unemployment: 10% increase in earnings 1-10 years	

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Aksoy, Poutvaara, and Schikora (2021)	Initial dispersal		Germany 2016-18	Yes, Initial placement	1-2 years: 1 standard deviation decrease in unemployment increases probability of employment by 4%	NA	NA	Focus on effects of attitudes
Müller, Pannatier, and Viarengo (2022)	Initial dispersal		Switzerland 1998-2018	Yes	Year 1-5: 1% increase unemployment rate decreases empl. Probability by 2-3%	Year 5-10: 1% increase unemployment rate decreases empl. Probability by 2-3%	NA	Focus on attitudes
				Panel b: Network				
Edin, Fredriksson, and Åslund (2003)	Initial random placement	6,400	Sweden 1980-2000	Yes dispersal	NA	NA	Increase in size ethnic group, no effect on ln(earnings)	Effects looking at quality of local network
Battisti, Peri, and Romiti (2022)	Initial random placement		Germany		1-3 years: 1 std deviation increase in network, +10% employment probability	3-12 years: No effect	No effect	Effects on less educated

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Damm (2009)	Initial random assignment	Denmark 1986-98	Yes, dispersal	NA	Year 7: increase in ethnic network by one std deviation → no effect on probability of employment	Positive effect on ln (earnings)	Effects on less educated

Table A.5: Refugee Employment and Earnings Gaps to Natives by Years Since Asylum

	Any Employment (1)	Employment of Full Year (2)	Earnings (3)
2	0.072***	0.031***	-58.921
	(0.001)	(0.001)	(46.575)
3	0.139***	0.074***	906.260***
	(0.002)	(0.001)	(66.291)
4	0.187***	0.106***	1,342.604***
	(0.002)	(0.002)	(78.276)
5	0.222***	0.129***	1,528.529***
	(0.002)	(0.002)	(86.999)
6	0.250***	0.147***	1,619.511***
	(0.002)	(0.002)	(93.445)
7	0.263***	0.155***	1,496.246***
	(0.002)	(0.002)	(98.576)
8	0.269***	0.159***	1,044.736***
	(0.002)	(0.002)	(102.496)
9	0.278***	0.165***	1,006.341***
	(0.002)	(0.002)	(106.033)
10	0.285***	0.173***	1,060.458***
	(0.002)	(0.002)	(110.102)
11	0.288***	0.179***	1,109.295***
	(0.002)	(0.002)	(114.781)
12	0.290***	0.184***	1,100.958***
	(0.002)	(0.002)	(119.626)
13	0.290***	0.187***	1,201.665***
	(0.002)	(0.002)	(123.107)
14	0.292***	0.185***	989.729***
	(0.002)	(0.002)	(126.325)
15	0.292***	0.185***	834.385***
	(0.002)	(0.002)	(128.636)
Age	0.077***	0.144***	8,526.201***
	(0.000)	(0.000)	(22.747)
Age Squared	-0.002***	-0.003***	-176.043***
	(0.000)	(0.000)	(0.672)
Age Cubed	0.000***	0.000***	1.153***
	(0.000)	(0.000)	(0.006)
Female	-0.017***	-0.068***	-13,574.484***
	(0.000)	(0.000)	(26.569)
Single	-0.083***	-0.092***	-8,728.143***
	(0.000)	(0.000)	(27.713)
Children 0-2y	-0.008***	-0.010***	1,235.967***
	(0.000)	(0.000)	(19.518)
Children 3-17y	0.040***	0.044***	4,297.899***
	(0.000)	(0.000)	(20.874)
Refugees 1986-1996	-0.787***	-0.660***	-41,489.949***
	(0.114)	(0.096)	(4,542.926)
Refugees 1997-2000	-0.758***	-0.640***	-41,670.294***
	(0.114)	(0.096)	(4,547.271)
Refugees 2001-2003	-0.713***	-0.612***	-40,317.988***
	(0.114)	(0.097)	(4,549.659)
Refugees 2004-2008	-0.711***	-0.615***	-41,828.290***
	(0.114)	(0.097)	(4,557.192)
Population Share in Municipality	-0.490***	-0.729***	75,571.055***
	(0.031)	(0.032)	(3,187.860)
Non-Western Share in Municipality	0.320***	0.674***	207,018.257***
	(0.008)	(0.008)	(919.485)
Employment Rate in Municipality	0.947***	0.777***	54,867.353***
	(0.003)	(0.004)	(274.496)
Constant	-0.716***	-1.717***	-137,733.617***
	(0.005)	(0.005)	(427.488)
N	81,645,229	81,645,229	82,210,741
Origin Country FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes

Notes: Robust standard errors in parentheses clustered by individuals.$^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. The table shows the estimated annual employment and earnings for adult refugees, conditional on years since asylum, age (third order polynomial), gender, marital status, dummies for having children aged 0-2 and aged 3-17, population share in municipality (of country total), share of Non-Western immigrants in municipality, local employment rate for the full working age population, municipality fixed effects, origin country fixed effects and dummies for different refugee cohorts relative to all natives.

Table A.6: Summary Statistics

	Refugees	Natives	Other Immigrants
Age	31.32	36.53	34.90
Female	0.44	0.49	0.50
Syria	<0.01	-	<0.01
Eritrea	<0.01	-	<0.01
Iran	0.09	-	0.03
Afghanistan	0.07	-	<0.01
Iraq	0.19	-	0.01
Somalia	0.11	-	0.01
Lebanon	0.09	-	0.02
Bosnia	0.21	-	0.01
Sri Lanka	0.06	-	0.01
Yugoslavia	0.04	-	0.04
Vietnam	0.03	-	0.03
Myanmar	0.01	-	<0.01
Single	0.32	0.52	0.41
Has Children Younger Than 3y	0.18	0.11	0.17
Has Children 3-17y	0.41	0.48	0.46
Basic Education	0.54	0.41	0.47
Vocational Education	0.25	0.37	0.26
Academic Education	0.21	0.22	0.27
Missing Education Information	0.50	0.02	0.40
Any Employment	0.09	0.79	0.57
Employment as Fraction of Full Time	0.05	0.66	0.42
Annual Earnings (USD 2015-Level)	2,206.38	38,463.01	24,096.30
N	56,192	60,114,383	3,860,397

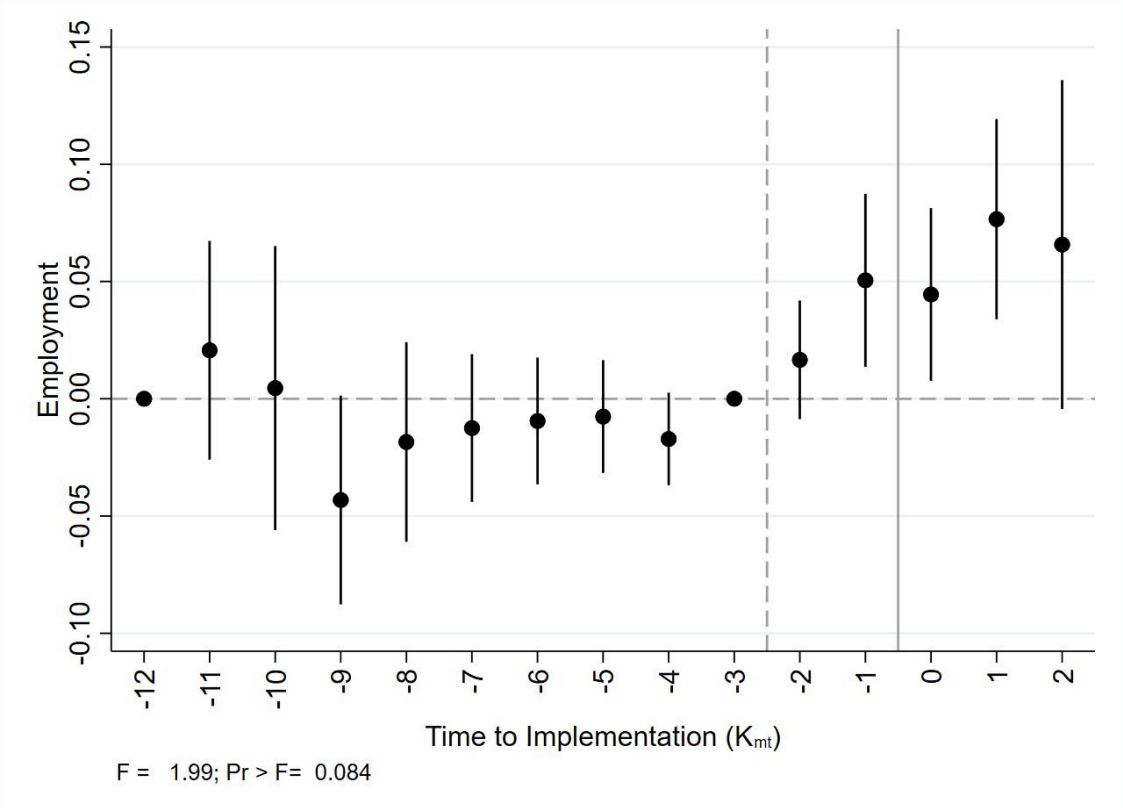
Notes: The table shows the means of different characteristics of adult refugees aged 18-55 on arrival and natives and other immigrants aged 18-55 in 1986-2008. The refugee characteristics are measured in the first year after arrival while the characteristics of natives and other immigrants are the population averages in 1986-2008.

Table A.7: Summary Statistics by Estimation Samples

	ALMP	Welfare Benefits	Language Training	Initial Placement
Age	35.10	33.63	32.56	29.48
Female	0.37	0.50	0.43	0.42
Syria	0.58	0.01	0.01	-
Eritrea	0.10	-	-	-
Iran	0.08	0.06	0.05	0.17
Afghanistan	0.06	-	0.14	0.04
Iraq	0.02	0.49	0.48	0.24
Somalia	0.02	0.13	-	0.23
Lebanon	-	0.01	0.01	0.18
Single	0.38	0.24	0.28	0.35
Has Children Younger Than 3y	0.13	0.18	0.18	0.18
Has Children 3-17y	0.39	0.51	0.44	0.31
Basic Education	0.36	0.32	0.45	0.21
Vocational Education	0.04	0.09	0.15	0.08
Academic Education	0.09	0.10	0.18	0.10
Missing Education Information	0.51	0.50	0.22	0.61
Any Employment	0.09	0.05	0.06	0.03
Employment as Fraction of Full Time	0.06	0.03	0.03	0.01
Annual Earnings (USD 2015-Level)	2,610.63	1,204.61	1,347.77	579.89
N	8,556	4,787	5,888	19,160

Notes: The table shows the means of different characteristics of refugees in the four different estimation samples. The refugee characteristics are measured in the first year after arrival.

Figure A.1: The Employment Rate by Event Time



Notes: The plotted estimates show 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 (Model 2), and 95-percent confidence intervals. F denotes the F-test statistic of joint insignificance of the pre-event dummies in event time -12 to -3. Pr$>F$ denotes the corresponding p-value from the F-test.

Table A.8: Estimated Impact of Policies on Mobility from Initial Municipality

	Year 1-5 (1)	Year 6-10 (2)	Year 11-15 (3)
	Panel a. Welfare Benefits		
RD Estimate	0.018	0.026	0.017
	(0.035)	(0.031)	(0.029)
N	4,787	4,642	4,558
	Panel b. Language Training		
RD Estimate	-0.195***	-0.007	0.029
	(0.031)	(0.033)	(0.033)
N	5,888	5,717	5,518
	Panel c. Initial Placement		
Employment Rate Non-Western Immigrants	0.076**	0.076**	0.081**
	(0.031)	(0.033)	(0.037)
Share of Co-Nationals	-0.119***	-0.126***	-0.123***
	(0.012)	(0.015)	(0.019)
N	19,160	17,858	16,598

Notes: Robust standard errors in parentheses clustered at date of admission in Panels (a) and (b), and clustered at municipality level in Panel (c). $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. The table shows the estimated impact of different policies on the mobility rate from assignment municipality in 5-year intervals for adults aged 18-55 on arrival, conditional on refugee characteristics on arrival: age (third order polynomial), gender, marital status, spouse in Denmark, dummies for having children aged 0-2 and aged 3-17, refugee (vs. family-reunified), and dummies for Eastern Europe and rest of the world (Muslim countries is the reference). In Panels (a) and (b) we show RD estimates using a triangular kernel to weight observations and a bandwidth of 548 days on each side of the cutoff. In Panel (c) we further condition on year of immigration fixed effects and origin country fixed effects (Afghanistan is the reference).

Table A.9: Estimated Heterogeneous Impact of Initial Placement on Mobility from Initial Municipality

	Year 1-5 (1)	Year 6-10 (2)	Year 11-15 (3)
	Panel a. Female		
Employment Rate Non-Western Immigrants	0.067**	0.078**	0.088**
	(0.030)	(0.034)	(0.040)
Share of Co-Nationals	-0.100***	-0.115***	-0.117***
	(0.013)	(0.016)	(0.021)
N	7,978	7,449	6,891
	Panel b. Male		
Employment Rate Non-Western Immigrants	0.083**	0.075**	0.079**
	(0.033)	(0.034)	(0.036)
Share of Co-Nationals	-0.135***	-0.137***	-0.129***
	(0.014)	(0.016)	(0.020)
N	11,182	10,409	9,707
	Panel c. Linguistically Close		
Employment Rate Non-Western Immigrants	0.086***	0.097***	0.109***
	(0.029)	(0.029)	(0.035)
Share of Co-Nationals	-0.102***	-0.103***	-0.099***
	(0.017)	(0.015)	(0.020)
N	6,893	6,331	5,557
	Panel d. Linguistically Far		
Employment Rate Non-Western Immigrants	0.065*	0.058	0.062
	(0.036)	(0.038)	(0.040)
Share of Co-Nationals	-0.143***	-0.156***	-0.152***
	(0.014)	(0.016)	(0.021)
N	12,267	11,527	11,041

Notes: Robust standard errors in parentheses clustered at municipality level. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. The table shows the estimated impact of initial placement conditions on the mobility rate from assignment municipality in 5-year intervals for adults aged 18-55 on arrival, conditional on refugee characteristics on arrival: age (third order polynomial), gender, marital status, spouse in Denmark, dummies for having children aged 0-2 and aged 3-17, refugee (vs. family-reunified), dummies for Eastern Europe and rest of the world (Muslim countries is the reference), year of immigration fixed effects and origin country fixed effects (Afghanistan is the reference).

Table A.10: Estimated Impact of Initial Placement on Employment, Earnings and Job Quality

	Year 1-5 (1)	Year 6-10 (2)	Year 11-15 (3)	Year 1-5 (4)	Year 6-10 (5)	Year 11-15 (6)
	Employment			Earnings		
	Panel a. Contemporaneous Employment Rate					
Employment Rate Non-Western Immigrants	0.001	0.000	0.001	12.139	-43.975	53.393
	(0.004)	(0.006)	(0.006)	(151.131)	(214.991)	(311.772)
Share of Co-Nationals	-0.001	-0.006	-0.002	17.179	-271.833	-157.591
	(0.004)	(0.004)	(0.004)	(123.450)	(183.733)	(266.669)
Contemporaneous Employment Rate Non-Western Immigrants	0.017***	0.018**	0.043***	603.294***	694.375**	1,929.060***
	(0.006)	(0.008)	(0.011)	(226.177)	(302.969)	(510.644)
N	19,160	17,858	16,598	19,160	17,858	16,598
	Panel b. Firm Quality					
	Log Average Hourly Wage in Establishment					
Employment Rate Non-Western Immigrants	0.011	0.005	0.005			
	(0.014)	(0.008)	(0.007)			
Share of Co-Nationals	0.002	0.001	0.003			
	(0.009)	(0.006)	(0.004)			
N	4,268	6,658	6,699			
	Panel c. Job Complexity					
	Complex Job			Non-Complex Job		
Employment Rate Non-Western Immigrants	-0.005	-0.006	0.003	0.018**	0.021***	0.021***
	(0.005)	(0.009)	(0.010)	(0.009)	(0.006)	(0.006)
Share of Co-Nationals	-0.009**	-0.007*	-0.009*	-0.004	-0.000	-0.001
	(0.003)	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)
N	19,154	17,855	16,586	19,154	17,855	16,586

Notes: Robust standard errors in parentheses clustered at municipality level. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. The table shows the estimated impact of initial placement conditions on average outcomes in 5-year intervals for adults aged 18-55 on arrival, conditional on refugee characteristics on arrival: age (third order polynomial), gender, marital status, spouse in Denmark, dummies for having children aged 0-2 and aged 3-17, refugee (vs. family-reunified), and dummies for Eastern Europe and rest of the world (Muslim countries is the reference), year of immigration fixed effects and origin country fixed effects (Afghanistan is the reference). In Panel (a) we include controls for the contemporaneous employment rate in the assignment municipality for Non-Western immigrants (calculated as 5-year averages). In Panel (b) we use the log of average hourly wages in the establishment conditional on employment (calculated as 5-year averages and excluding refugees) as the dependent variable. In Panel (c) we use dummies for having a complex job (ISCO 1-5) or a non-complex job (ISCO 6-9) as dependent variables.

Table A.11: Estimated Impact of Policies on Full Time Employment

	Year 1-5 (1)	Year 6-10 (2)	Year 11-15 (3)
	Panel a. ALMP		
Estimate	0.034**		
	(0.014)		
N	8,556		
	Panel b. Welfare Benefits		
RD Estimate	0.029*	-0.008	-0.016
	(0.015)	(0.026)	(0.028)
N	4,787	4,642	4,558
	Panel c. Language Training		
RD Estimate	0.043***	0.056***	0.037*
	(0.014)	(0.021)	(0.021)
N	5,888	5,717	5,518
	Panel d. Initial Placement		
Employment Rate Non-Western Immigrants	0.007**	0.003	0.011**
	(0.003)	(0.004)	(0.005)
Share of Co-Nationals	0.000	-0.005*	-0.004
	(0.003)	(0.003)	(0.004)
N	19,160	17,858	16,598

Notes: Robust standard errors in parentheses clustered at municipality level in Panels (a) and (d), and clustered at date of admission in Panels (b) and (c). $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. The table shows the estimated impact of different policies on the average employment rate (columns 1-3) in 5-year intervals for adults aged 18-55 on arrival, conditional on refugee characteristics on arrival: age (third order polynomial), gender, marital status, spouse in Denmark, dummies for having children aged 0-2 and aged 3-17, refugee (vs. family-reunified), and dummies for Eastern Europe and rest of the world (Muslim countries is the reference). In Panel (a) we further condition on year of immigration fixed effects, calendar month of immigration fixed effects and municipality of assignment fixed effects. In Panels (b) and (c) we show RD estimates using a triangular kernel to weight observations and a bandwidth of 548 days on each side of the cutoff. In Panel (d) we further condition on year of immigration fixed effects and origin country fixed effects (Afghanistan is the reference).

Table A.12: Correlation Matrix, Municipality Characteristics

	(1) Employment Rate	(2) Employment Rate, Non-Western Immigrants	(3) Annual Earnings	(4) Population Size	(5) Share of Non-Western Immigrants	(6) Share of Co-Nationals	(7) Urban Area
Employment Rate	1.00						
Employment Rate, Non-Western Immigrants	0.37***	1.00					
Annual Earnings	0.75***	0.28***	1.00				
Population Size	0.03	-0.08***	0.05***	1.00			
Share of Non-Western Immigrants	0.22***	0.02	0.35***	0.41***	1.00		
Share of Co-Nationals	-0.02	-0.06***	0.00	0.04***	0.22***	1.00	
Urban Area	0.18***	0.02	0.16***	0.39***	0.39***	0.03	1.00

Notes: The table shows the correlation matrix for municipality characteristics. Employment rate and earnings are measured as the municipality average within a year. For each refugee-sending country the share of co-nationals already residing in the municipality is calculated within municipality and year. We then calculate the average of the co-national shares across origin countries within municipality-year cells.

Figure A.2: Displaced People in Denmark

Notes: The difference between applications and admissions/permits under the special law for people displaced from Ukraine reflects processing time, which is exceptionally fast for this group. A close to 100% approval rate is expected. Citizens of Ukraine and people who were refugees in Ukraine on February 24 (first date of Russian invasion) are eligible. Refugees are individuals: 1) who risk persecution for reasons of race, religion, nationality, political opinion or membership of a particular social group in their country of nationality and are currently outside this country (inside the country they would be called internally displaced and cannot apply for asylum in another country) or 2) who qualify under subsidiary national rules. Sources: Statistics Denmark, VAN8A for refugees and VAN5 for asylum applications. Asylum applications include only individuals whose case is processed in Denmark ("registreringstallet"). Numbers on people displaced from Ukraine can be found here: `https://us.dk/tal-og-statistik/tal-vedr-saerloven/`.