Trade-offs between work-first and language-first strategies for refugees

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ABSTRACT

This study investigates how local integration strategies that prioritize a work-first approach affect refugee's participation in language courses and their medium run labor market outcomes. We utilize a gradual rollout of the work-first approach (on-the-job training within the first year after arrival) combined with a dispersal policy of refugees across municipalities in Denmark. We find that being placed in municipalities emphasizing the work-first approach is positively associated with later employment and earnings but show that the associations are temporary. A higher local propensity to use the work-first approach is negatively related to time spent in language courses, level of courses completed and the grade point average for language course exam attendees. We discuss whether the reduced language course attendance could be an explanation of the lack of a persistent labor market effects.

1. Introduction

The integration of refugees into the labor market remains a serious challenge in most Western countries, and unlike the case in the US many refugees in Europe never catch up with natives (Cortes, 2004; Dustmann et al., 2017; Zimmermann, 2017; Brell et al., 2020; Bevelander, 2020). Two dimensions of refugee integration are predominant in the national integration policies in the EU: host country language knowledge and labor market participation (Martín et al., 2016; Hernes et al., 2022). Yet, there is limited evidence as to which modalities foster better outcomes in these dimensions. While language courses have been offered to newly arrived refugees for decades in some countries[1], an emphasis on work-first strategies has gained popularity in many Northern European countries, particularly in the aftermath of the surge in the number of refugees from Syria in 2015 (Martín et al., 2016; Konle-Seidl & Bolitz, 2016). Work-first strategies may include interventions such as early skills assessment, fast-track options, early job training, economic incentives for labor market participation, and work permits during the asylum-seeking phase.

This study compares the work-first and language-first integration strategies for persons with asylum or subsidiary protection (henceforth

referred to commonly as refugees). We only include refugees who participate in language courses during the first year since arrival (93% of the refugees in the working age range in our context, because language course enrolment is compulsory) and among them we contrast the refugees who are placed in municipalities that emphasizes a work-first approach to a different extent. The work-first approach consists of the use of early on-the-job training during the first year after arrival. Since all refugees in our sample participate in language courses during the year of arrival, we refer to a higher use of early on-the-job training as the treatment. We examine how a higher use of early on-the-job training in the arrival municipality affects host country language course participation and completion, as well as labor market outcomes in the medium-long run; 3½ years after arrival.

We hypothesize that a more frequent use of early on-the-job training may affect formal language acquisition directly through increased exposure to the native language but that it may also have a negative indirect impact through a time constraint that leads to less time spent on language courses. Likewise, early on-the-job training may affect labor market outcomes directly, e.g., by providing training of job-specific skills and networks, or indirectly through changes in language acquisition. We therefore emphasize that work-first strategies may involve a

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[1] For example, language courses for newly arrived immigrants were compulsory for unemployed refugees in the Netherlands from 1998-2006, and have been so in Denmark since 1999, in Norway since 2004, and in Germany since 2005. They have, however, been offered to refugees in many countries for much longer, e.g., since the 1980s in the Scandinavian countries (Arendt, 2018).

trade-off between early labor market entry and other skills investments. These hypotheses are further developed formally in Appendix A.

We benefit from rich administrative registry data covering all refugees aged 18–64 who were granted a residence permit for the first time in Denmark in 2012 to the first quarter of 2016 and combine it with unique information on monthly employment outcomes, monthly participation patterns in language courses and job training and scores in the final language tests.

To identify the effect of a more intensive use of early job training, we use two sources of variation: First, we utilize the Danish dispersal policy for refugees, that allocate refugees to municipalities mainly based on the number of inhabitants, adjusted for the number of immigrants already residing in the municipality. This mitigates selection of refugees into areas with better labor market opportunities. Second, we argue that a series of governmental recommendations paved the way for a gradual rollout over time of the use of early on-the-job training for refugees in Denmark (see Section 2.2). We use this variation to separate the effect of local use of early on-the-job-training from local labor market conditions. Over a matter of a few years, the fraction of refugees who participated in on-the-job training in the first year since arrival increased from 20% to close to 50%, but with substantial differences in the take-up across municipalities[2]. We compare refugees who by chance end up in municipalities with a more frequent use of early job-training to refugees who end up in municipalities where refugees are primarily subjected to a language-first approach. Dispersal policies have previously been applied for identification of the effect of local characteristics on refugee employment (Edin et al., 2003, 2004; Åslund & Rooth, 2007; Damm, 2009; Beaman, 2012; Godøy, 2017; Marténs et al., 2019; Bratsberg et al., 2020; Azlor et al., 2020), but the use of the gradual roll-out of early on-the-job training is new.

We find that increased local use of early on-the-job training is positively associated with employment during the second year after arrival, but that the association disappears during the third and fourth year. The results condition on place and time of arrival as well as local unemployment and employment rate at arrival. We also find that increased use of early on-the-job training is negatively related to time spent in language courses and the final completed course level. Finally, we show that refugees who are placed in municipalities with a higher use of early on-the-job-training perform substantially worse at the final language course test, and that there are no significant effects on exam attendance. We present evidence that supports a causal interpretation of these associations but cannot rule out potential selection effects because use of early on-the-job training is correlated with the local employment rate.

Instrumental variables (IV) estimates, where individual participation in early on-the-job-training is instrumented by previous local use of early on-the-job-training, reproduce the same results: Significant but temporary positive labor market effects and a permanent significant reduction in language course participation. The IV results should be interpreted with care, as they are only causal under auxiliary assumptions. Nevertheless, the results suggest that increased use of a work-first strategy that emphasizes early participation in on-the-job-training only produces positive effects in the short run, at the expense of time spent in language courses. The absence of long-term effects in the labor market suggest that time spent in the labor market (job training or employment) is not superior to time spent on language courses, but that the two approaches are substitutes in the medium run.

Our study contributes to different strands of literature, each of which examines how different modalities of integration support can foster refugee integration. One strand of literature has shown that job search support and on-the-job training produce positive effects on labor market integration in the short run (Joona et al., 2016; Clausen et al., 2009;

Heinesen et al., 2013; Arendt, 2022; Foged et al., 2022b). All these studies consider the impact of on-the-job training for up to two years, and we add to these studies considering impacts for up to 3½ year. Another strand of literature has emphasized host country language acquisition as an important determinant of labor market integration in Western countries (Chiswick & Miller, 1995; Dustmann & van Soest, 2001, 2002; Dustmann & Fabbri, 2003; Budría & Martínez-de-Ibarreta, 2021). In this literature, exposure to the native language is considered a key determinant, though this has mainly been studied indirectly (Espinosa & Massey, 1997; Chiswick, 1998; Espenshade & Fu, 1997; Warman, 2007; Chiswick & Miller, 2014). Though we do not consider language proficiency per se, we contribute to this literature by examining a new measure of exposure – time spent in the labor market. The finding that time spent in the labor market reduces participation in language training is in accordance with findings in Arendt (2022) and Damm et al. (2022). We add to these results by showing that also language course level is also affected. Reduced language course participation and performance is potentially important in the longer term, since recent studies suggest that language course training may produce substantial long-term labor market effects (Sarvimäki & Hämäläinen, 2016; Foged et al., 2022a). Foged et al. (2022a) find that a reform that raised language course instruction hours by 200 h produced substantial and positive earnings effects 18 years after arrival. By comparison, refugees ending up in municipalities where all refugees are subjected to the work-first approach spent 152 fewer hours in language courses than refugee in municipalities that do not use the work-first approach. Therefore, even though the work-first and the language-first approach produce similar employment effects in the medium run, there is a risk that the work-first may produce negative earnings effects in the longer run if additional time spent in the labor market does not compensate the loss from less time spent in language training.

2. Institutional settings

A person who enters Denmark can apply for asylum according to the Refugee Convention or according to national regulation that provides subsidiary protection. The applicants live in decentralized asylum centers while the application is being evaluated, and during this time applicants have restricted access to public services. It is unlikely that any training offered during the application period plays a large role for applicants in the period of consideration, 2012–2016, because the biannual average waiting times among applicants who had their application granted during this period varied only between one and three quarters of a year (Hvidtfeldt & Schultz-Nielsen, 2017). Once granted asylum, the immigrant is settled in one of the 98 municipalities, and they become eligible for public services, welfare benefits and have full access to the labor market but are obliged to participate in an integration program (described in Section 2.1).

Importantly for our empirical analysis, settlement across the 98 municipalities in Denmark is determined by a spatial dispersal policy for refugees. The dispersal policy allocates refugees based on local quotas. The annual quota reflects the municipality's share of the total population adjusted for the share of foreigners already living in the municipality (Azlor et al., 2020). The dispersal policy involves a stepwise process where refugees are first allocated – based on regional quotas – to one of five larger regions, after which the municipalities negotiate the internal allocation. If the municipalities do not agree upon the allocation, the government conducts the allocation on a local level, so municipalities have a strong incentive to comply with the quotas in the first place. The local quotas are determined in the year prior to arrival, based on the number of applicants. Hence, the municipalities know their quota before the regional negotiations commence, and the number of locally allocated refugees correspond closely to the pre-determined quota (Azlor et al., 2020). Once allocated to a municipality, refugees are required to stay in that municipality during a three-year long period, where they participate in an integration program. They can only move if

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[2] From July 2016, participation in on-the-job training immediately upon arrival became compulsory. This period is excluded from the analysis and is the focus in the study Arendt (2022).

a new municipality agrees to take over the integration responsibilities and the costs of the integration program, which usually requires that the refugee finds a job in the new municipality. Immigrants who are granted a residence permit based on reunification with refugees who are already in the country are not subject to the dispersal policy. Therefore, we only include refugees and not their reunified family members in the analysis. This dispersal policy implies that refugees are allocated across the country independently of individual and local characteristics. The dispersal policy does allow, however, that municipalities can request certain types of refugees and vice versa, that refugees can request to live close to relatives, among people of their own nationality or in places with educational institutions or health care treatment options, thereby distressing the experimental setting. Azlor et al. (2020) find indicative evidence that random assignment can be compromised, but that this is not the case when restricting the focus to refugees who arrive after the first 10 municipalities have filled their quotas so that the option for making requests is more limited. Azlor et al. (2020) study another period than we do, and we therefore examine whether randomization has been compromised using a similar approach.

2.1. The integration program

The core components of the compulsory integration program are a language course and employment support, which may include on-the-job-training. The integration program commences by a compulsory referral to a language course within a month of arrival (Act No. 1062 of 2010)[3]. The language course has a duration corresponding to up to 1.2 years of full-time studies[4] and is provided by around 60 public or private language schools. The language course is financed by municipalities and the government and is free of charge for the refugees. It is compulsory for refugees who receive welfare benefits to participate in the language courses, and non-participation leads to financial sanctions. Refugees can be exempted from participation if they have good Danish language skills or for other reasons, usually because of severe health problems. They can, obviously, also choose not to receive welfare benefits and thus avoid being sanctioned for non-participation. Nevertheless, the majority enroll in the language course in the first year (during our period of interest it was 93%), but they can pause or adjust the intensity of participation to take time for job training and employment. To accommodate flexible participation, the municipalities are obliged by law to offer language classes during the day or as evening classes. The language courses are offered at three levels, depending upon prior educational background, knowledge of the Latin alphabet and literacy (Ministry for Refugees, Immigrants, and Integration, 2003; Act No. 1010 of, 2010). It is the municipality that determines the entry course level, based on initial interviews, education assessment and language screening. The course ends with standardized exams in three topics that are graded by an external examiner. Participation in the exam is free of charge[5] and is incentivized since a condition for obtaining a permanent residence permit is passing one of the three Danish courses (Act No 984 of, 2012)[6].

In addition to the language course, the integration program includes labor market-related training activities for the unemployed. These activities can either consist of on-the-job training or classroom training, such as job search courses or short specific skills training courses. On-the-job training is offered at public or private workplaces, either as subsidized employment or internships. Internships are by far the most common choice for newly arrived refugees (92% of the months with early on-the-job training are in internship). One internship lasts 13 weeks but can be extended to 26 weeks, and the refugee can participate in several internships. The purpose of the internship is to provide practical knowledge of the labor market, as a first step before being hired with a wage subsidy or in regular employment. In contrast to subsidized hiring, participants in internships do not receive pay from the company in which they are trained but continue to receive their welfare benefit. The combination of training and language courses is under the discretion of the municipalities, but it is compulsory to participate in labor market training once it has been offered by the municipality. The selection into job training is therefore to a large extent determined by job center caseworkers and the availability of job training slots. Since job training requires acceptance from an employer, it is likely that participants in early on-the-job training are a positively selected group in the sense that they – all else being equal – are better skilled and more employable than those not selected into job training. This also implies that they are likely to be better learners of the Danish language. There might be a group where selection works the other way, i.e., better skilled persons who avoid job training, e.g., if they find a job or enroll in education within the first year. Since this is a very small group, we expect positive selection into job training, on average.

2.2. Rollout of early job training

Several events have gradually paved the way for a larger focus on the work-first strategy, which emphasizes early contact with the labor market for refugees in the Danish integration program. The internships were introduced in the second year of the integration program in 2004, as a cheaper form of on-the-job training than subsidized hiring. The use of internships was incentivized by raising the reimbursement for this type relative to the alternative of classroom training or subsidized employment (Wiberg & Andreasen, 2003). Subsequent research supported a work-first strategy: An evaluation of the integration program in 2006 documented that the language courses for immigrants have "lock-in" effects in terms of postponing or reducing labor market entry (e.g., Clausen et al. 2006), and two studies documented that on-the-job training speeded up labor market entry, particularly for immigrants, while classroom training was shown to be less effective in the short run (Clausen et al., 2009; Heinesen et al., 2013). In 2014, the government made an agreement with the labor market unions and employer organizations that emphasized early on-the-job training by initiating projects that supported simultaneous participation in on-the-job training and language classes (Bolvig & Arendt, 2018). An expert committee collected existing evidence in 2015 and recommended that immigrants should receive on-the-job training from the time of arrival (The Expert Committee's Report on The Active Labor Market Program, 2015)[7].

The consistent and continued emphasis on earlier labor market entry and simultaneous participation in on-the-job training and language classes resulted in a gradual rollout of early on-the-job training across Danish municipalities. We use this variation combined with the dispersal policy, described in the previous section, as our identification strategy. This will be further described in Section 4.

3. Data

The data set used in this study is based on administrative registries from National public authorities containing information on the time and type of residence permit. We include all immigrants with asylum or

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[3] This subsection refers to legislation enacted prior to arrival, but holds for all arrival cohorts in our sample.

[4] One full-time year corresponds to 37 hours per week for 46 weeks, including both lectures and homework (The Ministry for Refugees, Immigrants and Integration, 2011, p. 30)

[5] Although there is a small fee for re-examinations.

[6] As a rule, asylum or family reunification provides temporary residence permit, which must be renewed every second year.

[7] The Danish Act on Integration of 2016 made early on-the-job training compulsory for arrivals from October 2016 on (Act No. 665 of 2016). As we describe below, the immigrants for whom this legal requirement of very early on-the-job training applied are not included in the current study, since they cannot be followed for 42 months. They are, however, studied in Arendt (2022).

subsidiary protection who were granted a residence permit in Denmark in the years 2012 to the first quarter of 2016. In the following, we will refer to this entire group as refugees. The date on which the refugee is assigned to the municipality is the date that the residence permit is issued, and we refer to this date as the date of arrival. We restrict our attention to the given period because the regulation of temporary and permanent residency is constant in this period. This is important as these regulations provide incentives for early labor market entry and language school participation.

We select all refugees who at time of arrival were aged between 18 and 65. Everyone in this age range is entitled to unemployment benefit but not to retirement via public pension. This data set contains 21,417 refugees. Because refugees can be exempted from participation in the introduction program if they suffer from a severe chronic illness, we exclude refugees who have not begun the language course within the first year of arrival (leaving 20,284 refugees in the sample). Moreover, since we want to follow the refugees beyond the three-year integration period, we include only refugees for whom we observe outcomes for at least 42 months after arrival, which requires that they arrived before April 2016. This is the case for 17,809 refugees. Finally, we exclude observations with missing data on the main outcomes and where the instrument is calculated based on 10 refugees or less. This leaves us with a main sample of 16,157 refugees. We use the strategy of Azlor et al. (2020) to examine whether the option for municipalities and refugees to influence the settlement decision compromises randomization implicit in the dispersal policy. For this analysis, we exclude all refugees arriving before at least 5 or 10 municipalities have filled their quotas. The latter restricts the sample to 8,708 refuges for whom data is available for at least 42 months.

For the given population of refugees, we link administrative individual level data from the language schools, employment data from Statistics Denmark and the Ministry of Employment, and education data from Statistics Denmark. Based on this data, we construct a monthly panel data set covering the months from the time of arrival till the end of 2019.

From the language schools we have monthly information on the hours of instruction time and school absence rates, as well as exam dates and exam results. We combine the hours of instruction with absence rates to calculate the hours of instruction attendance. We also observe exam attendance and grades at the final exam. To pass the course, the refugees must attend both an oral exam, a reading comprehension exam, and a written exam at the end of each course[8]. The grade point average is based on all the exams taken, but if a student attends the same type of exam multiple times, we use the result from the latest exam. However, if early on-the-job-training affects the timing of exam attendance, results for exam results will be biased. As a supplementary measure, we utilize that each of the three language courses consist of 6 modules and that each module has been developed within the Common European Framework of References for Languages (CEFR). Thus, the language level of module 5 in the language course at the lowest level corresponds to the level of module 2 in the medium level course or module 1 in the high-level course (CEFR level A1). Correspondingly, different modules in different courses correspond to CEFR levels A2, B1, B2 and up to level C1. We number these levels from 1 to 5 and use the result as a proxy for completed language course level (see Fig. A.1 in the appendix). A small share has not completed any level at a given point in time, and we assign a 0 to these (3.5% after 24 months and 2.5% after 42 months). This measure has also been used in Foged and van der Werf (2020).

From the municipalities, we have weekly information on participation in on-the-job training (internships and jobs with a wage subsidy),

which is used to assess if job training begins within one year after arrival. To measure entry into regular employment, we use as a primary labor market measure an indicator variable taking the value 1 if the refugee has either worked at least one hour in that month combined with not participating in subsidized employment, or if the refugee has positive labor market income during that month. Labor income is included as an outcome as it combines the measure of work intensity with a measure of job quality. Labor income also has the advantage that it is likely to be measured with a higher validity than work hours, since only the former is based on information reported to the tax authorities. Labor income is available until 2018 only, however, and is therefore not observed for 42 months for all refugees in the sample.

Because most of the refugees are in their twenties or early thirties when they arrive, education is also a realistic alternative to employment and supplements the employment and income outcomes with information on education. The outcome horizon is too limited to allow observation of completion for a large part of the group, and we therefore include information on educational enrollment. Educational enrollment is measured by receipt of public student grants, which are available from the upper secondary schooling level[9].

The data is merged with other administrative data containing information on demographic and individual characteristics, such as gender, age, number of children, date and type of residence permit, country of origin, and family status. It is well documented that many refugees suffer from health problems such as trauma or physical injuries from torture (Porter & Haslam, 2005; Fazel et al., 2005; Peconga & Thøgersen, 2019). We therefore include administrative data on health care utilization, such as general practitioner contacts, psychiatric care, and hospitalizations. Appendix Table A.1 contains a full list of the registers and main variables used for the study.

4. Method

We take advantage of the dispersal policy and the gradual roll-out of early on-the-job training to mitigate selection bias in estimates of relationships between the local emphasis on the work-first approach and refugee outcomes. We estimate the effect of the work-first approach by comparing refugees who end up in municipalities that has emphasized the work-first approach with refugees that end up in municipalities that do not. For a given refugee, the emphasis on the work-first approach is calculated as the job training participation rate in the first year after arrival among the refugees who arrived at the same municipality during a certain period just prior to each individual refugee's arrival. We denote this measure by Z_{i0} and use municipality-month pairs where at least 10 refugees arrived during the past 20 months (i.e., pairs where the number is lower are excluded from the analyses). The number 20 has been chosen to ensure enough municipality-month pairs. We estimate the effect of the local propensity to use the work-first approach on future outcomes by use of the following model:

$$Y_{it} = \alpha_0 + \delta_R Z_{i0} + \theta_0 L_{ms1} + \pi_0 \, X_{i1} + \gamma_{0m} + \gamma_{0s} + \epsilon_{it} \tag{1}$$

Where Y_{it} is an outcome measured t months after arrival ($t = 13, 18, 24, \ldots, 48$), and γ_m and γ_s are municipality-of-arrival and semester-of-arrival fixed effects. The fixed effects imply that the identifying source of variation is regional differences across different arrival cohorts in the use of early on-the-job training (i.e., the gradual roll-out). We control for individual characteristics in the year of arrival, X, and the local labor market opportunities, L, proxied by the local unemployment rate as a fraction of the labor force, and the number of employed as a fraction of the working age population (aged 15-65) in the year of arrival. For given unemployment rates, municipalities may differ in the fraction being

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[8] Two grades are given after the exams for course level 1 (one joint grade for a written exam and an exam in reading comprehension, and one grade for an oral exam), while three grades are given after the exams for course level 2 and 3 (one for each of the exams, written, reading comprehension, and oral).

[9] The student grant in 2015 was around DKK 5,900 (approx. euros 790) per month, www.su.dk, in Danish, accessed December 2020. These formal educations are tuition free.

Table 1

Covariate balance for the instrument (the local propensity to use early job-training) across different samples.

	(1) All refugees		(2) Excl. 5 municipalities filled up		(3) Excl. 10 municipalities filled up	
Woman	-0.0022	(0.402)	-0.0035	(0.218)	-0.0033	(0.361)
Woman with children	0.0062	(0.140)	0.0066	(0.141)	0.0049	(0.350)
Married	0.0031	(0.252)	0.0026	(0.339)	0.0060^{+}	(0.088)
Parental leave	0.0016	(0.673)	0.0027	(0.526)	0.0028	(0.601)
Age < 25	-0.0035	(0.601)	-0.0052	(0.471)	-0.0065	(0.449)
25 ≤ Age < 35	-0.0038	(0.568)	-0.0054	(0.449)	-0.0083	(0.352)
35 ≤ Age < 45	-0.0038	(0.596)	-0.0051	(0.489)	-0.0079	(0.391)
45 ≤ Age < 55	-0.0067	(0.340)	-0.0059	(0.428)	-0.0076	(0.405)
No children	ref.	(.)	ref.	(.)	ref.	(.)
1-3 children	0.0010	(0.751)	0.0012	(0.716)	-0.0014	(0.729)
Eritrea	-0.0026	(0.327)	-0.0030	(0.296)	-0.0039	(0.261)
Somalia	ref.	(.)	ref.	(.)	ref.	(.)
Iran	-0.0028	(0.748)	-0.0081	(0.368)	-0.0158	(0.198)
Iraq	-0.0057	(0.511)	-0.0055	(0.507)	-0.0000	(0.996)
Syria	-0.0174^{+}	(0.050)	-0.0178*	(0.046)	-0.0154^{+}	(0.069)
Woman	-0.0140	(0.324)	-0.0146	(0.322)	-0.0212	(0.163)
Woman with children	0.0027	(0.647)	0.0040	(0.508)	0.0066	(0.283)
Low level course:						
- module 1	ref.	(.)	ref.	(.)	ref.	(.)
- module 2	-0.0045	(0.548)	-0.0075	(0.361)	-0.0068	(0.463)
- module 3	-0.0113	(0.471)	-0.0150	(0.331)	-0.0162	(0.421)
- module 4	-0.0301	(0.518)	-0.0301	(0.517)	0.0058	(0.877)
- module 5	-0.0481***	(0.000)	-0.0565***	(0.000)	-0.0605***	(0.000)
Medium level course:						
- module 1	0.0007	(0.763)	0.0007	(0.781)	-0.0011	(0.724)
- module 2	0.0105*	(0.028)	0.0078	(0.103)	0.0133*	(0.045)
- module 3	0.0087	(0.365)	0.0087	(0.389)	0.0166	(0.283)
- module 4	-0.0067	(0.740)	-0.0140	(0.483)	-0.0025	(0.927)
- module 5	0.0480*	(0.018)	0.0294*	(0.029)	0.0234	(0.209)
- module 6	-0.0038	(0.793)	-0.0086	(0.533)	-0.0207	(0.101)
High level course:						
- module 1	-0.0043	(0.201)	-0.0038	(0.337)	-0.0021	(0.620)
- module 2	-0.0056	(0.599)	-0.0101	(0.394)	-0.0052	(0.656)
- module 3	0.0211*	(0.031)	0.0159	(0.136)	-0.0099	(0.703)
- module 4	0.0391	(0.117)	0.0448^{+}	(0.077)	0.0880***	(0.000)
- module 5	0.0161	(0.851)	0.0202	(0.808)	-0.0869***	(0.000)
General practice costs (DKK)	-0.0000	(0.972)	0.0012	(0.297)	0.0014	(0.309)
Emergency care costs (DKK)	-0.0126**	(0.008)	-0.0241***	(0.000)	-0.0284***	(0.001)
Psychiatry costs (DKK)	0.0068^{+}	(0.077)	0.0054	(0.274)	0.0053	(0.261)
Days hospitalized	-0.0007	(0.289)	-0.0013	(0.159)	-0.0010	(0.386)
Starting month of language course	-0.0007	(0.499)	-0.0012	(0.313)	-0.0024	(0.109)
Local unemployment rate	-0.0003	(0.984)	0.0028	(0.840)	0.0038	(0.780)
Local employment rate	-0.0282*	(0.049)	-0.0270^{+}	(0.080)	-0.0326*	(0.039)
Constant	2.4741*	(0.018)	2.3817*	(0.034)	2.8115*	(0.015)
Observations	16157		13927		8679	

Linear regression with municipality and half-year time dummies included. Std. clustered at municipal level. The quotas are obtained from The Danish Immigrations Service (https://www.nyidanmark.dk/da/Numbers/visiteringskvoter), and the number of quotas filled counts the number of municipalities where the number of new arrivals is at least equal to the municipal quota in a given month. The analysis with 5 quotas filled has excluded the 5 municipalities, where quotas were first filled. ^{+}p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001.

employed because of differences in the fractions being outside the labor force. This is important in our context, because a large share of the population considered is outside the labor force[10]. The local labor market opportunities are included to control for the possibility that the use of early on-the-job training is related to the likelihood of getting a job, which may affect later language course and labor market outcomes. The gradual roll-out of early on-the-job-training creates an opportunity to separate its effect from the effect of local labor market opportunities, although results should be interpreted subject to the possibility that we have not controlled for all local labor market conditions. This possibility is evaluated using bounding estimates, as described below.

We cluster standard errors by the municipality level but we report alternative levels of clustering for the main results in the appendix

Table A.9 and reach similar results. Following Abadie et al. (2022), the alternative clusters are constructed at the level at which the treatment varies; municipality and month of arrival.

Eq. (1) provides intent-to-treat estimates of the effect of placement in municipalities with a more frequent use of early on-the-job training. If we are willing to make the additional assumption that – conditional on local employment and unemployment and municipality fixed effects - the only way that placement in such municipalities can affect refugee labor market outcomes is through increased risk of early job-training we can estimate the effect of actually participating in on-the-job training, T_{i1}, by the instrumental variables (IV) estimator:

$$T_{i1} = \alpha_1 + \eta Z_{i0} + \theta_1 L_{ms1} + \pi_1 X_{i1} + \gamma_{1m} + \gamma_{1s} + u_{i1} \qquad (2)$$

$$Y_{it} = \alpha_2 + \delta_{IV} T_{i1} + \theta_2 L_{ms} + \pi_2 X_{i1} + \gamma_{2m} + \gamma_{2s} + \epsilon_{it} \qquad (3)$$

Where the IV estimate is obtained as the reduced form estimate in Eq. (1) scaled by the first-stage coefficient, $\delta_{IV} = \delta_R / \eta$. To be more exact, if we are willing to invoke a monotonicity and an exclusion restriction, δ_{IV} has an interpretation of a local average treatment effect for the

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[10] Welfare benefit recipients in Denmark are categorized as being job-ready or not, and only the former group is counted as unemployed. Between 72-78% of welfare benefit recipients are therefore not counted as unemployed in the period of consideration (www.jobindsats.dk).
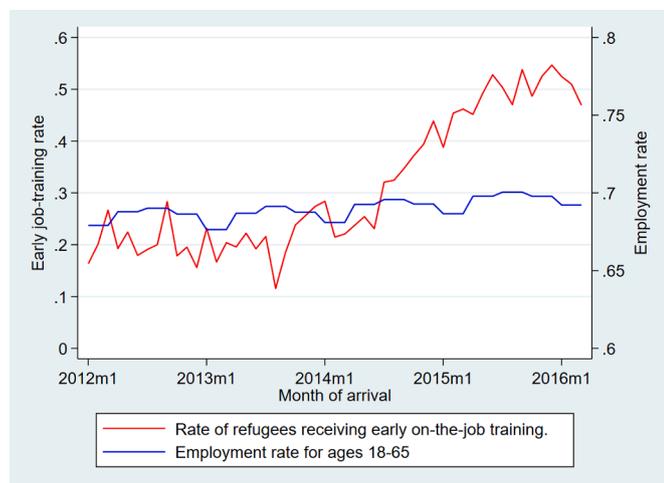
Fig. 1. Average rate of refugees who receive on-the-job training within the first year in Denmark and employment rates among all citizens aged 18–65, by year and month of arrival.

compliers who participate in early on-the-job training only if assigned to a municipality with a higher propensity to use that training (Imbens and Angrist, 1994). The monotonicity and exclusion restrictions are discussed further in the next subsection.

Previous papers have used dispersal policies for refugees to argue that size of local characteristics (ethnic network size or local unemployment rate) can act as instrument for their later values (Edin et al., 2003; Damm, 2009). In the light of results from previous studies of initial labor market conditions on future labor market outcomes (Åslund and Rooth, 2007, Godøy, 2017, Azlor et al., 2020), it is important that these estimates control for initial local labor market conditions.

4.1. Validation of the design

The key identifying assumption for the reduced form and instrumental variables estimates to have a causal interpretation, is that refugees are as good as randomly assigned to municipalities. This implies that, *conditional* on time and place of arrival, individual characteristics are unrelated to local characteristics. Table 1 presents balance tests for this assumption, where the treatment, local use of early on-the-job-training, are regressed on individual characteristics. We follow the approach of Azlor et al. (2020) and present results for all refugees in the main sample (cf. Section 3), as well as for refugees who arrived once the first 5 and 10 municipal quotas have been filled. The reasoning behind this procedure is that the refugees who arrives first have more municipalities to choose from (and vice versa) and therefore perhaps a higher likelihood of having a specific request fulfilled, which could compromise independence between individual and municipal characteristics.

The balance test in column 1 of Table 1 shows that when isolating the identifying source of variation by controlling for municipality and time of arrival, most of the correlations between the propensity to use early job-training and the regressors are small and insignificant. Thus, neither gender, age, having children nor most levels of start level of the Danish language course (which is a proxy for education from abroad) are significantly related to the propensity to use early job-training on a 5% significance level. Only five of 35 coefficients on individual characteristics are significant, and even the significant coefficients are small. There are some differences in the level of the language course (modules), but the differences show no clear ordering in the sense that, say, those starting at the lowest levels are in municipalities with a lower level of use of early on-the-job training (or vice versa).

The last two rows of Table 1 shows that the fraction of refugees participating in early on-the-job-training is lower in municipalities with higher fractions of the working age population in employment. Since

unemployment is controlled for, this means that early on-the-job training is higher when the fraction out-of-the labor force is higher. The results stress the need for controlling for the local labor market conditions when we examine the impact of early on-the-job-training.

Covariate imbalance is not reduced when we restrict the sample to refugees who arrive once the first five or ten municipal quotas have been filled (columns 2 and 3) along the lines of Azlor et al. (2020).[11] As a matter of fact, the balance seems to worsen, when excluding refugees arriving before 10 municipalities have been filled. Also, in 2015 and 2016 the quotas are high and consequently 65 percent of the refugees arriving in 2015 and all refugees arriving in 2016 are dismissed if the 10-municipality limit is applied. The propensity to use early job-training calculated for these refugees are based on refugees arriving during years of a great expansion in the use of early job-training (2014 and 2015) so by excluding these observations we lose valuable variation in the propensity to use early job-training. For these reasons, we will use the total sample in the main analyses, but stress that care should be taken when interpreting results.

A common approach for assessing omitted variable bias is to examine stability for the coefficient of interest, when gradually including more observed covariates. However, based on insights from Altonji, Elder and Taber (2005), Oster (2019) show that coefficient stability is only proportional to omitted variable bias when scaled by the changes in R-squared as observed covariates are included. Oster (2019) develops different tools to assess the size of omitted variable bias based on these insights. We present two sets of results based on Oster (2019): (1) The bounds developed under the assumption that the omitted variable biases from observed and unobserved covariates are alike ($\delta = 1$ in the terminology of Oster) and under assumptions of the size of the maximal R-squared in the model of interest, and (2) The amount of selection necessary (in terms of δ) that would drive estimates to zero. We discuss these results in the robustness section following the main results.

4.1.1. Gradual roll-out

To support the interpretation that we can separate the effect of the use of early on-the-job-training from that of local job opportunities, we examine the variation in the use of early on-the-job-training over space and time.

Fig. 1 shows that the use of early on-the-job training became increasingly frequent during the period of observation: The percentage of refugees who receive on-the-job training within their first year in Denmark is stable around 20% up until 2014 and then increases steadily to a level around 50% at the beginning of 2016. The figure also shows the monthly employment rate, which has been slowly increasing during the period from 68% to 69–70% (second axis to the right), but without the sudden rise from 2014 as seen for on-the-job-training.

Nonetheless, the increase in the use of early on-the-job training is not the same for all municipalities. Fig. 2 shows how some municipalities experience large increases in the average share of refugees who attend job training within their first year in the country from, for instance, below 20% (light yellow) to above 60% (orange-red), whereas the share in early on-the-job training is more stable in other municipalities. This supports the interpretation that early on-the-job training was rolled out gradually (and even rolled back in some places), independently of local job opportunities.

4.1.2. Instrumental variable estimates

To be able to interpret the IV estimate of early on-the-job-training causally, we need more assumptions than for the reduced form estimates. The key additional assumption is the exclusion restriction, which implies that conditional on the initial local employment and

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[11] We have also conducted balance test in an even narrower sample, for refugees who arrives once 12 municipal quotas have been filled, but the results are the same.
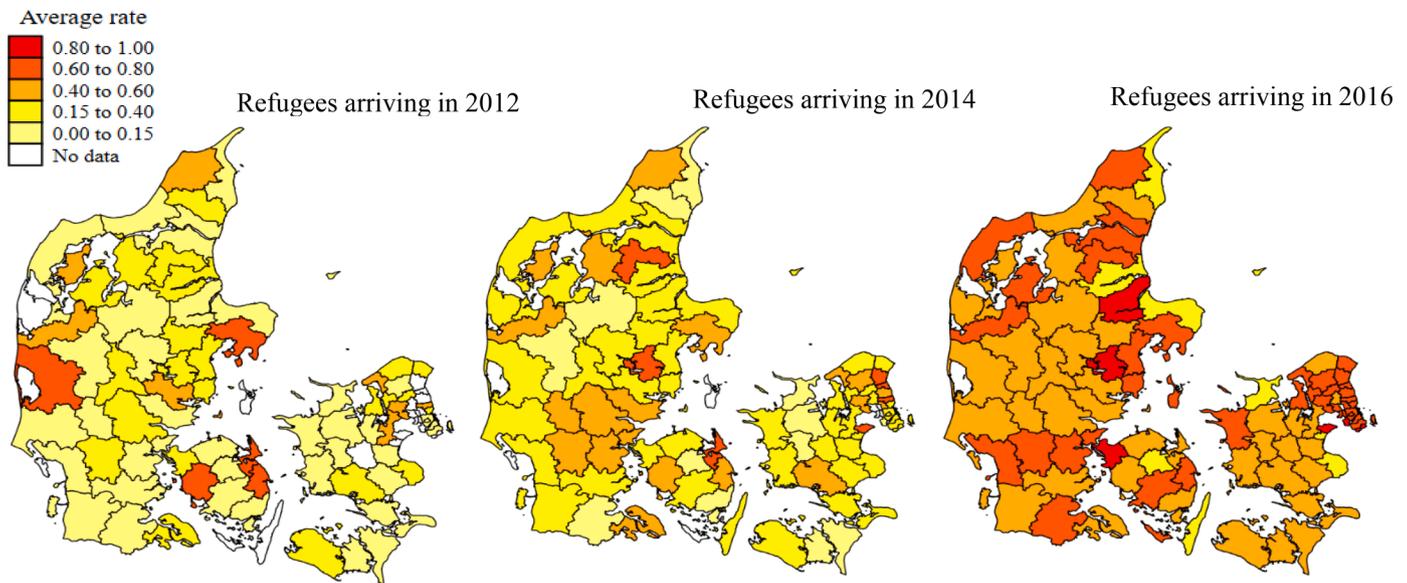
Fig. 2. Average rate of refugees who received job training during their first year in Denmark, by municipality and year of arrival.
Notes: Map of the municipalities in Denmark. No data exists for municipalities with less than 10 refugees arriving in the year.

Table 2
First stage: The effect of the instrument (local propensity to use early job-training) on the probability of receiving on-the-job training in the first year in Denmark.

	Full sample	Labor income sample
Instrument[#]	0.608***	0.575***
	(0.0549)	(0.0646)
Observations	16,157	11.970
Cragg-Donald Wald F test	122.7	79.04
Lee et al. 2021 s.e. inflation factor	1	1.03

Notes: OLS estimates with a dummy for participating in early job-training as outcome, and with controls for background characteristics, municipality dummies, and dummies for semester of arrival (included covariates are presented in Table A.1). Standard errors (in parentheses) clustered at the municipality level.
[#] The instrument is calculated as the degree of early on-the-job training among refugees in the municipality 20 months prior to arrival. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

unemployment level and calendar time, a higher propensity to use local on-the-job-training in the year of arrival only affect future outcomes through its impact on the individual probability of participating in on-

the-job-training. The exclusion restriction can be violated for the same reasons that the independence condition could be violated; if there are unobserved local characteristics that correlate with use of early on-the-job-training as well as future outcomes. For instance, additional labor market characteristics or spending on other types of support. Even when the independence condition holds, exclusion might be violated. For instance, it could be that when peer refugees are placed in job-training, it may create positive network effects or negative competition-for-jobs effects. Since the exclusion restriction is untestable, we should interpret results cautiously. For now, we examine the additional assumptions needed to interpret IV causally.

Table 2 tests whether the local propensity to use early job-training affects the individual probability of early job-training among later arriving refugees (coefficient η in Eq. (2)). It shows that the propensity to use early job-training is strongly correlated with the likelihood of participating in early on-the-job training. We also report the Cragg-Donald Wald F statistic for weak identification (Stock & Yogo, 2005). It is 122.7 in the full sample, which is above the threshold of 104.7 for a true 5% test (Lee et al., 2022) and therefore there is no indication that the instrument is weak. For the smaller labor income sample, the F statistics are slightly below the threshold. According to Table 3a in Lee

Table 3
Reduced form estimates of the effect of the local propensity to use early job-training.

Months since arrival	(1) Ever had a job		(2) Labor income (sum)		(3) Ever enrolled in education		(4) Completed language course level (CEFR)	
13	0.0937*	(0.0454)	8297.4	(5655.5)	-0.00724	(0.00796)	-0.0990	(0.0634)
	[0.05]		[3688.1]		[0.00]		[1.20]	
18	0.0898[+]	(0.0481)	15430.4[+]	(8296.6)	-0.0146	(0.0142)	-0.0641	(0.106)
	[0.12]		[8137.9]		[0.02]		[1.54]	
24	0.0509	(0.0444)	11704.0	(11983.4)	-0.00583	(0.0322)	-0.201[+]	(0.116)
	[0.23]		[19729.3]		[0.05]		[1.93]	
30	0.0263	(0.0451)	12586.6	(14024.2)	-0.0157	(0.0393)	-0.292*	(0.120)
	[0.33]		[38877.6]		[0.08]		[2.23]	
36	-0.0116	(0.0421)	3079.4	(15718.1)	-0.0507	(0.0461)	-0.309**	(0.113)
	[0.40]		[63859.9]		[0.09]		[2.39]	
42	-0.00198	(0.0376)	15802.0	(21820.2)	-0.0653	(0.0454)	-0.285*	(0.113)
	[0.40]		[93617.1]		[0.08]		[2.43]	
N	16157		16157††		16157		16157	

Notes: Reduced form estimates with controls for individual background characteristics, local labor market conditions, municipality dummies, and dummies for semester of arrival (included covariates are presented in Appendix Table A2). Standard errors (in parentheses) are clustered at the municipality level.
Mean outcomes for refugees not participating in early on-the-job training in []. [+] $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
†The panel is shorter when income is the dependent variable: $N = 14,957$ at 36 months and 11,970 at 42 months.

Table 4
Bounds for the effect of treatment.

Model	Bound 1 β_0 (baseline)	Controlled (from Table 3) $\widehat{\beta}$	Bound 2 β^*	δ Relative selection
Ever had a job after 18 months				
FE baseline	0.094	0.090	0.084	15.83
Empty baseline	0.356		-0.087	0.51
Ever had a job after 24 months				
FE baseline	0.059	0.051	0.045	10.18
Empty baseline	0.438		-0.082	0.384
Language course level (CEFR) after 42 months				
FE baseline	-0.306	-0.285	-0.280	57.49
Empty baseline	-0.767		-0.222	4.50

Notes: Bounds β^o and β^* from Oster (2019) based on Rmax = 0.29 in the employment model and Rmax=0.33 in the CEFR model. Bounds are based on model (1), where L and X are the "intermediate" control, W_1. L is a vector containing the local fraction in employment and the local unemployment rate and X is a vector containing individual characteristics (cf. Table A.1). δ is a proportionality parameter showing how much selection on unobservables relative to selection on intermediate controls should matter for estimates to be 0.

et al. (2022) the standard deviation of the effect estimate on labor income should then be inflated by 1.06. This factor does not alter any of the conclusions in the results section.

Another threat to the results is that IV estimates can be sensitive to a high instrument leverage (Young, 2022): If a few clusters generate the main variation in the instrument this alters IV inference. We have therefore calculated the instrument leverage using the clusters defined by municipality and semester of arrival. Its mean is 0.00028 and it never exceeds 0.007[12]. This is a very small leverage level, showing that instrument leverage does not affect our results.

If treatment effects are heterogeneous, interpretation of IV as local causal estimates requires an assumption of monotonicity of the instrument is fulfilled (Imbens & Angrist, 1994). As the first-stage coefficient is positive, this assumption requires that the likelihood of receiving early on-the-job training is not lower for an individual assigned to a municipality with higher levels of past use of early job training. This could be the case if, for instance, the number of on-the-job training positions is constrained, and a new refugee therefore has less on-the-job training options when arriving just after a period with a high level of on-the-job training. Plotting a local linear regression of the probability of participating in early on-the-job training on the propensity to use early job-training, we find a highly monotonic relationship (see Appendix Fig. A.2), which supports the monotonicity assumption.

Finally, the use of a two-fixed effect model can also produce problems in the presence of effect heterogeneity, since the two-fixed effect estimator produces a weighted average of effects with potentially negative weights (Goodman-Bacon, 2021; Imai & Kim, 2020; De Chaisemartin & D'Haultfoeuill, 2020). We examine whether this presents a problem using the tools developed in De Chaisemartin and D'Haultfoeuill (2020), based on De Chaisemartin et al. (2019).

5. Results

5.1. Reduced form results

Table 3 presents the OLS estimates of δ in Eq. (1) for four main outcomes, measured 13 and 18 months since arrival, and then for each

period of 6 months until 42 months since arrival. The estimates are from models including a full set of controls; individual characteristics, the local unemployment and employment rates, municipality fixed effects and dummies for each semester of arrival.[13]

For the labor income regressions, we have used the level of income and include observations with zero income but obtain qualitatively similar results when we use the inverse hyperbolic sine function transformation level of income, although they become insignificant (see Appendix Table A.2).

The reduced form estimates show that being placed in a municipality with a higher propensity to use early on-the-job training is associated with 9 percentage points higher employment rates after 13 and 18 months since arrival. The associations are large relative to the baseline probability of employment for refugees who do not participate in early on-the-job training (0.05 and 0.12), but the next rows show that the effects are temporary: The estimates become insignificant after 24 months and are much smaller than at 18 months, indicating that refugees who end up in municipalities that do not emphasize the work-first approach catch up with refugees placed in municipalities that emphasize the work-first approach.

Looking at the results for labor income in the second row, the estimated associations mirror those on employment: income levels more than double after 13 and 18 months, but only the estimates at 18 months are significant at a 10 percent level. As was the case for employment, the associations become smaller and insignificant from 24 months onwards. There is therefore no indication of a spillover from the excess employment rate during the second year into a higher cumulative income beyond the first 18 months. The results for labor income therefore confirm that the labor market effects are rather short-lived. Nonetheless, we do not find any significant negative associations with employment or income. The third row shows that there is no significant association with the probability of enrollment in ordinary education at the upper secondary or tertiary level either.

We do not know which type of job the refugees work in during job training, but we have examined the type of occupation they end up in if they find employment. We find that the four most frequent sectors in which the population is employed 42 months after arrival are the same whether they have participated in early on-the-job training or not. These sectors are manufacturing, hotel and catering, administrative assistance services, and trade, and employment in these sectors accounts for more than 60% of all employment.

The final model (4) shows that refugees who are placed in municipalities with a larger propensity to use early on-the-job-training pass language courses at a significantly lower level (i.e., where the required language proficiency to pass is lower). The estimate is significantly negative on a 10% level after 24 months since arrival and increases in absolute magnitude and becomes significant on a 5% level after 30 to 42 months since arrival. The association corresponds to a situation where every third refugee participating in early on-the-job training has passed a language course at one level lower on the CEFR scale (e.g., CEFR level A2 instead of B1).

5.2. Robustness

The results presented in Table 3 have a causal interpretation when refugees are randomly allocated across municipalities and early use of on-the-job training takes place independently of current and future employment opportunities. We have seen (weak) indications in Table 1 that these assumptions may be violated, and we therefore assess the robustness of the results from Table 3 in this section.

Even though we did not obtain a sample with better covariate balance when restricting it to refugees who arrived once municipal quotas

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[12] Instrument leverage is calculated as $\widetilde{Z}'_{(i)}(\widetilde{Z}'\widetilde{Z})^{-1}\widetilde{Z}_{(i)}$, where $\widetilde{Z}$ is the residual from a regression of the instrument on the covariates, and $\widetilde{Z}_{(i)}$ is the same residual when the i'th cluster is omitted.

[13] Appendix Table A2 shows the coefficients for the full covariate set for the employment outcomes and completed language course level.

Table 5
Reduced form estimates of the effect of the local propensity to use early job-training, mechanisms.

Months since arrival	(1) Instruction hours (sum since arrival)		(2) Months of on-the-job-training (sum after first year)		(3) Exam attendance		(4) GPA (std. diff. from mean)	
13	-31.66 [455.28]	(40.51)	0.274*** [0.06]	(0.0620)	0.00136 [0.00]	(0.00734)	NA	
18	-57.21 [610.43]	(52.57)	0.695* [0.87]	(0.301)	0.00372 [0.02]	(0.0180)	NA	
24	-93.16 [765.94]	(61.27)	0.733 [2.15]	(0.463)	0.0389 [0.09]	(0.0338)	-0.528+ [0.26]	(0.286)
30	-133.1* [872.81]	(66.08)	0.643 [3.36]	(0.521)	-0.0741 [0.22]	(0.0566)	-0.535** [0.14]	(0.187)
36	-153.3* [952.15]	(65.35)	0.855 4.44]	(0.545)	-0.0532 [0.38]	(0.0608)	-0.556*** [0.04]	(0.147)
42	-152.7* [1018.62]	(63.53)	1.249* [5.30]	(0.549)	-0.0173 [0.51]	(0.0541)	-0.471*** [-0.02]	(0.138)
N	16157		16157		16157		8518#	

Notes: Reduced form estimates with controls for individual background characteristics, local labor market conditions, municipality dummies, and dummies for semester of arrival (included covariates are presented in Appendix Table A.1). Standard errors (in parentheses) are clustered at the municipality level. Mean outcomes for refugees not participating in early on-the-job training in [].

The estimate for GPA is conditional on exam attendance, and the panel is shorter at earlier periods: $N = 1651$ at 24 months, $N=4035$ at 30 months, and $N=6594$ at 36 months. $^+ p < 0.1$, $^* p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$.

Table 6
Reduced form estimates of the effect of the local propensity to use early job-training. Heterogenous effects by gender and having children at the time of arrival.

Outcome:		Time since arrival: 13 months	18 months	24 months	30 months	36 months	42 months	N
Ever had a job	Female	0.0329 (0.0506)	0.0497 (0.0681)	0.0207 (0.0714)	-0.00420 (0.0635)	-0.00226 (0.0650)	-0.0146 (0.0633)	3954
	Male	0.118* (0.0558)	0.100+ (0.0548)	0.0611 (0.0513)	0.0319 (0.0534)	-0.0229 (0.0506)	-0.00750 (0.0466)	12203
	With children	0.0915* (0.0393)	0.157** (0.0504)	0.107* (0.0482)	0.0418 (0.0576)	0.00161 (0.0515)	-0.0326 (0.0468)	7219
	No children	0.0995 (0.0667)	0.0606 (0.0676)	0.0295 (0.0626)	0.0261 (0.0623)	-0.00631 (0.0568)	0.0180 (0.0500)	8938
	Female with children	0.0176 (0.0356)	-0.00814 (0.0424)	-0.0283 (0.0536)	-0.0706 (0.0575)	-0.0473 (0.0629)	-0.0801 (0.0693)	2439
	Female with no children	0.0679 (0.103)	0.190 (0.149)	0.101 (0.137)	0.0950 (0.132)	0.0883 (0.131)	0.0773 (0.123)	1515
	Male with children	0.133* (0.0605)	0.248** (0.0784)	0.201** (0.0759)	0.0874 (0.0850)	0.00378 (0.0729)	-0.0306 (0.0639)	4780
	Male with no children	0.101 (0.0710)	0.0368 (0.0675)	0.00734 (0.0672)	0.00701 (0.0662)	-0.0270 (0.0607)	0.00326 (0.0559)	7423
Completed language course level (CEFR)	Female	-0.100 (0.0821)	-0.0770 (0.128)	-0.362* (0.168)	-0.346* (0.153)	-0.406** (0.138)	-0.311* (0.152)	3954
	Male	-0.100 (0.0734)	-0.0452 (0.122)	-0.149 (0.131)	-0.271* (0.136)	-0.270* (0.133)	-0.294* (0.128)	12203
	With children	-0.109 (0.0875)	-0.0807 (0.107)	-0.185 (0.131)	-0.255+ (0.142)	-0.252+ (0.137)	-0.211 (0.148)	7219
	No children	-0.0898 (0.0869)	-0.0448 (0.132)	-0.227 (0.142)	-0.333* (0.147)	-0.357* (0.141)	-0.353* (0.139)	8938
	Female with children	-0.0978 (0.0900)	0.0484 (0.118)	-0.172 (0.188)	-0.280 (0.175)	-0.243 (0.167)	-0.102 (0.188)	2439
	Female with no children	0.0315 (0.164)	-0.203 (0.298)	-0.728** (0.253)	-0.554* (0.268)	-0.806** (0.262)	-0.778** (0.268)	1515
	Male with children	-0.107 (0.116)	-0.168 (0.156)	-0.237 (0.154)	-0.249 (0.174)	-0.254 (0.190)	-0.276 (0.185)	4780
	Male with no children	-0.109 (0.0934)	0.00108 (0.133)	-0.127 (0.160)	-0.297+ (0.172)	-0.280+ (0.158)	-0.301+ (0.154)	7423

Notes: reduced form models estimated for each subgroup separately with controls for background characteristics, municipality dummies, and dummies for semester of arrival (included covariates are presented in Appendix Table A.1). Standard errors (in parentheses) are clustered at the municipality level. $+ p < 0.1$, $^* p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$

have been filled, we show results for these samples. Table A.3 in the appendix shows that the estimated associations with employment and language course levels are qualitatively like those in Table 3, if we limit the sample size to refugees who arrived once the first 5 municipal quotas have been filled, whereas the coefficients are reduced by around 50% when the sample is limited to quotas filled in 10 municipalities. Since the sample restriction reduces the sample size by nearly 50% as well there is a substantial loss of power. As mentioned above, the reduction in coefficient size is likely linked to a loss of important variation in 2015

and 2016 where municipal quotas were filled up later during the year. Because covariate balance is as good in the main sample in this narrower sample, and because the estimated effect patterns are close to each other, we focus on the main sample with larger power in the following.

Next, we present results based on Oster (2019) to assess bias from unobserved covariates. Oster develops bounds based on an assumption of a maximum R-squared in the model of interest. Because our model describes how future outcomes depend on variables in the year of arrival, we follow the example in Oster (2019) and assume that the

Table 7

Reduced form estimates of the effect of the local unemployment rate (panel A) and local employment rate (panel B) during the first year since arrival.

Panel A: Effect of the local unemployment rate at arrival							
Months since arrival	(1) Ever had a job		(2) Labor income (sum)		(3) Ever enrolled in education		(4) Completed language course level (CEFR)
13	0.000305	(0.0151)	3679.3	(2583.0)	-0.0072	(0.00590)	-0.0237	(0.0144)
18	0.0114	(0.0155)	5750.1	(3655.6)	-0.0049	(0.00800)	-0.00526	(0.0301)
24	0.00221	(0.0156)	6994.2	(4959.6)	-0.0137	(0.00938)	-0.0167	(0.0388)
30	0.0169	(0.0159)	9349.8	(6175.9)	-0.0193+	(0.0111)	0.00459	(0.0337)
36	0.00119	(0.0142)	10347.7	(6942.9)	-0.0231*	(0.0107)	0.0159	(0.0305)
42	0.00919	(0.0119)	14089.0	(8852.5)	-0.0048	(0.0121)	0.0142	(0.0305)
Panel B: Effect of the local employment rate at arrival							
Months since arrival	(1) Ever had a job		(2) Labor income (sum)		(3) Ever enrolled in education		(4) Completed language course level (CEFR)
13	-0.00187	(0.0134)	484.7	(1459.6)	-0.00206	(0.00191)	0.0144	(0.0177)
18	-0.00333	(0.00728)	124.4	(1040.3)	-0.00043	(0.00353)	0.00580	(0.0142)
24	0.00351	(0.00549)	-296.6	(1478.0)	0.00321	(0.00397)	-0.00775	(0.0120)
30	0.00538	(0.00464)	57.34	(1981.9)	0.00938+	(0.00478)	0.00803	(0.0129)
36	0.00046	(0.00416)	680.0	(2373.3)	0.00788+	(0.00456)	0.0142	(0.0114)
42	0.00152	(0.00332)	1248.7	(2594.2)	0.00972*	(0.00397)	0.0144	(0.0114)
N	16157		16157†		16157		16157

Notes: Reduced form estimates with controls for individual background characteristics, local labor market conditions, municipality dummies, and dummies for semester of arrival (included covariates are presented in Appendix Table A2). Standard errors (in parentheses) are clustered at the municipality level.
†The panel is shorter when income is the dependent variable: $N = 14{,}957$ at 36 months and 11,970 at 42 months. $^{+}$ $p < 0.1$, $*$ $p < 0.05$, $**$ $p < 0.01$, $***$ $p < 0.001$

Table 8

Instrumental variable estimates of the effect of early on-the-job training.

Months since arrival	(1) Ever had a job		(2) Labor income (sum)		(3) Ever enrolled in education		(4) Completed language course level (CEFR)	
13	0.181*	(0.0874)	15991.5	(10633.2)	-0.0140	(0.0154)	-0.191	(0.124)
	[0.05]		[3688.1]		[0.00]		[1.20]	
18	0.172+	(0.0926)	29551.9+	(15650.3)	-0.0281	(0.0270)	-0.123	(0.203)
	[0.12]		[8137.9]		[0.02]		[1.54]	
24	0.0946	(0.0827)	21768.1	(21886.5)	-0.0108	(0.0594)	-0.374+	(0.218)
	[0.23]		[19729.3]		[0.05]		[1.93]	
30	0.0455	(0.0777)	21735.4	(23907.5)	-0.0271	(0.0679)	-0.504*	(0.204)
	[0.33]		[38877.6]		[0.08]		[2.23]	
36	-0.0195	(0.0696)	5098.6	(25659.2)	-0.0848	(0.0783)	-0.517**	(0.187)
	[0.40]		[63859.9]		[0.09]		[2.39]	
42	-0.00326	(0.0613)	27495.1	(37463.7)	-0.107	(0.0773)	-0.469*	(0.184)
	[0.40]		[93617.1]		[0.08]		[2.43]	
N†	16157		16157††		16157		16157	

Notes: Instrumental variable estimates with controls for individual background characteristics, local labor market conditions, municipality dummies, and dummies for semester of arrival (included covariates are presented in Appendix Table A2). Standard errors (in parentheses) are clustered at the municipality level. † Control group N=9330. †† $N = 14{,}957$ at 36 months and 11,970 at 42 months. The instrument is calculated as the degree of early on-the-job training among refugees in the municipality 20 months prior to arrival.
Mean outcomes for refugees not participating in early on-the-job training in []. $^{+}$ $p < 0.1$, $*$ $p < 0.05$, $**$ $p < 0.01$, $***$ $p < 0.001$

maximal R-squared is obtained in a model with outcomes in the first year since arrival as explanatory variables. Regressing employment in year 3 since arrival on employment in the first, we obtain a R-squared of 0.29, which will be applied below. We focus on two scenarios: One model where we allow time and municipality fixed effects in the baseline model (called the FE baseline), and one where only the instrument is included (called the Empty baseline). Table 4 shows the results for the three main outcomes: Ever had a job after 18 and 24 months, and on completed language level (CEFR) after 42 months.

The most intuitive bound is β_0 which is simply the treatment effect without a set of intermediate controls. It is seen that if we include time and municipality fixed effects (the FE baseline), leaving out the intermediate controls hardly alters the estimated association with the propensity to use early on-the-job training. For instance, the association with ever had a job after 18 months is 0.094 as opposed to an estimate of 0.090 when all controls are included, whereas in the empty model where only the local propensity to use early job-training is included, the association is 0.356. Therefore, we find that if the unobserved variables were to alter the effect as much as including both fixed effects and individual characteristics do, the lower bound of the employment effect might become negative (β^* is -0.087). δ is a parameter that describes

how much unobserved characteristics should affect the estimated effect, relative to the observed controls to drive the treatment effects to zero. If controls include the FE, this is seen to be 50% in the case of ever employed after 18 months ($\delta = 0.51$). If we, on the other hand, only look at individual and local labor market characteristics as intermediate controls, the table shows that selection on unobserved individual characteristics need to be 15 times that of these characteristics for the employment effect after 18 months to disappear ($\delta = 15.83$).

The effect on CEFR is bounded between -0.77 and -0.22 even with the empty model as baseline. In this case selection on unobservables need to be 4.5 times selection on all observed characteristics (including FE) to change the sign of the effect on CEFR. If we only look at individual and local labor market characteristics as intermediate controls, this number increase to 57 times selection on observed characteristics. The results therefore do not provide strong indications that selection on unobserved characteristics could be driving the results, and therefore support the previous findings that refugees are well balanced across municipalities, and estimated associations are causal.

In an additional robustness analysis, we examine whether the use of a two-way fixed effects model presents a problem because the estimate is a weighted average with potentially negative weights. De Chaisemartin

and D'Haultfœuill (2020) show that, under parallel trend assumptions, this is only a problem if the weights are correlated with treatment effects for the treated. We estimate the weights and their correlation with the model covariates (as predictors of the treatment effects) using the stata command twowayfeweights (De Chaisemartin et al., 2019). The results show that half the weights in the two-way fixed effect model are negative, both when we use the completed language level in month 42 and the indicator *Ever had a job* in either month 18 or month 42 as dependent variables (see Table A.4). The regression of the estimated weights on the set of covariates used shows that the covariates are not significantly related to the weights, except for two to three out of the 44 coefficients.

Finally, we test the results for alternative definitions of the instrument by applying two alternative cutoffs for the number months included when calculating the average use of early job-training in the municipality. We estimate the main results from Table 3 using, respectively 15 and 25 months as cutoffs and find all main conclusions to maintain (see Table A.5 in the appendix).

5.3. Mechanisms: participation patterns and heterogeneity

In this section, we examine the potential indirect mechanism that we hypothesized may be driving effects on employment and the completed language course level: Does early on-the-job training take away time from participation in language courses? Even though we do not observe language proficiency, we examine the effect on test results from the final language exam among those who taken an exam by the 42nd month. We also explore potential mechanisms running through further time in job training and by examining effect heterogeneity.

The results are shown in Table 5. Refugees who are placed in municipalities with a larger propensity to use early on-the-job training attend fewer instruction hours during the Danish course (column 1), and the deficit of attended instruction hours keeps increasing from month 13 to month 42 since arrival. The reduction in language course attendance indicates that early on-the-job-training substitute away time from language training. The estimate on accumulated attended instruction hours is not significant in the first 18 months but increases in absolute size throughout the period and is large and significant 42 months after arrival. Comparing refugees in municipalities where no-one participates in early on-the-job-training with refugees in municipalities where everyone historically has participated in early on-the-job-training yield a cumulated deficit of 152 h after 42 months, corresponding roughly to a 15% reduction in the cumulative hours of attended instruction time. At the same time, we see a clear tendency of increased participation in on-the-job training among refugees who are placed in municipalities with a historic larger fraction in early on-the-job-training. The outcome is measured as number of months with on-the-job training counted from month 13 and onwards. Therefore, the estimate of 0.27 indicates that living in high propensity municipalities has increased on-the-job training in month 13 by 0.27 months - this increase is more than four times higher than the level among refugees who did not participate in on-the-job training during the first year.

As was the case for employment, the effect on later on-the-job-training arises immediately after the first year, which might partly be due to the job training started within the first year not ending until the second year after arrival. It is therefore not the case that job training is shifted from the second to the first year for the treated, but they do genuinely receive more job training.

The third column in Table 5 shows that refugees who are placed in municipalities with a larger fraction in early on-the-job-training attend the language exam earlier, but the associations are not significant. The associations with GPA at the exam, conditional on exam attendance, are substantial, and the association is significant already after 24 months, but keeps increasing the following months. Even though we saw no significant association with exam attendance, we cannot rule out that the association on GPA is partly driven by selection into participation.

We have tested for heterogeneity by presence of children, age, and

gender without establishing any significant differences in the estimates between different groups. However, when splitting the models by the combination of gender and child, we find large positive employment associations for male refugees with children, whereas the associations for women with children are zero after 18 and 24 months, and significantly different from those for males with children. As examples, we present results for employment and completed language level by gender and presence of children in Table 6. Results by age groups are shown in the appendix Table A.6.

Table 6 shows that the temporary positive employment association is present for most groups, but only significant for males and refugees with children at entry. Likewise, the negative association on the completed language level CEFR persists for all groups, although it is insignificant for refugees with children, when splitting by both gender and children. Therefore, the results do not suggest that some groups substitute away more time from language schools when participating in early on-the-job training than others because they are more time-constrained, e.g., due to childcare responsibilities.

5.4. Mechanisms: Local labor market conditions

We have seen in Table 1 that the propensity to use of early on-the-job-training is correlated with the local labor market conditions. Even though we control for the local unemployment and employment rates, it is possible that early on-the-job training is simply picking up other unobserved local labor market characteristics. To get an impression of how large an impact the local labor market conditions have on the outcomes considered, we present the reduced form estimates for the local unemployment rate and the local fraction of employed citizens from the model where early on-the-job-training propensity is also included. The results are found in Table 7. It shows that there is no association between the local unemployment rate and labor market outcomes and language course participation outcomes, i.e., the effects are very small and insignificant (conditional on the early on-the-job-training propensity). The association between being enrolled in education and the fraction in employment is significant on a 5 or 10% level after 30 months, but the koefficients are very small. Table A.7 presents results for the outcomes presented in the section on mechanisms, and this too shows a limited impact of the local unemployment rate. An exception is that unemployment seems to impact later on-the-job-training.

5.5. Instrumental variable estimates

We have focused on the reduced form estimates of the intent-to-treat effect of the local propensity to use early job-training. We now turn to the instrumental variable estimates of the effect of actual participation in early on-the-job-training. As discussed above, bear in mind that they only have a local causal interpretation under the auxiliary assumptions of monotonicity and the exclusion restriction.

Table 8 shows the IV results, and since they are simply the reduced form estimates from Table 3 scaled by the first-stage coefficient in Table 2, it shows the same patterns as in Table 3. The important thing to note is that it also displays a similar pattern of significance:, temporary significant labor market effects and a reduced level of language course completion. The IV results for employment and language course completion are significantly different from OLS estimates (not shown) at 30 months since arrival onwards. The IV-estimates of effects on the mechanisms also mirror those of the reduced form model that we presented in Table 5 (see appendix Table A.8).

6. Discussion

The current study examines a work-first strategy for refugees that emphasizes early entry into the labor market by means of on-the-job training, as an addendum to early language training. We utilize the Danish dispersal policy of refugees to compare refugees that are quasi-

randomly assigned to municipalities with a high use of the work-first approach and municipalities with a lower level. We examine the consequences on two key dimensions of integration: formal language course completion and labor market outcomes and hypothesize that early on-the-job training may affect formal language acquisition and employment directly, but that it may also have a negative indirect impact through a time constraint that leads to less time spent on language courses.

The reduced form estimates show that – conditional on individual characteristics, municipality fixed effects and local employment and unemployment - being placed in municipalities that emphasize a work-first approach is associated with higher employment and labor market income in the second year since arrival. The positive short-term associations are large relatively to the levels in municipalities without a work-first approach and the results are in accordance with the findings of previous studies (Clausen et al., 2009; Heinesen et al., 2013; Arendt, 2022; Foged et al., 2022b). We add to these findings by showing that this association is short-lived and disappears in the third and fourth year since arrival. Therefore, from the perspective of labor market outcomes, the work-first and the language first-approach are substitutable in the medium run.

We also find that those who are placed in municipalities emphasizing a work-first approach are more likely to participate in job training later. Therefore, we can reject the possibility that the temporary effects of early on-the-job training on labor market outcomes arise because those who do not participate early just do so later, i.e., that the early treatment is merely a shift in the timing of job training.

We further show that the work-first approach is associated with less time spent attending language courses, thereby confirming our expectation that time spent in one type of training modality affect time spent in other types of training modalities. Refugees placed in municipalities with a work-first approach also end up completing lower language course levels, corresponding to a situation where every third refugee participating in early on-the-job training has passed a language course at one level lower on the CEFR scale (e.g., CEFR level A2 instead of B1). This is important not only because the CEFR level is a proxy for language proficiency, but also because completion of language courses at a high level is a prerequisite for obtaining permanent residency. We do not observe language proficiency directly but find a substantial reduction in test scores at the final language course exam (half a standard deviation of the GPA) for refugees in municipalities with a work-first approach, among those who attend the exam. We want to stress though that the negative association with test scores could partly be a selection effect, because a large fraction of refugee is not attending the exam. Whether the large negative association with formal language course acquisition is transferable to actual language proficiency is therefore uncertain, but it is supported by the lack of a permanent effect on labor market outcomes and is consistent with qualitative studies examining the use of host country language in different types of job training settings (Sørensen & Holmen, 2004; Sandwall, 2013; Bramm & Kirilova, 2018). Looking at examples of refugees and immigrants in job training, these studies all find that host country language is spoken very little in the course of a working day: Sandwall, (2013) find averages between 30 s and 2 min a day among Swedish immigrants, while Bramm and Kirilova, (2018) find averages between 37 and 104 s out of 1.5 h among Danish refugees in job training.

If our results carry over to formal language proficiency, they may indicate that early on-the-job training can have negative effects in the long run. In the current setting of refugee integration, it is well established that language proficiency is strongly related to labor market attachment in the long run (Chiswick & Miller, 1995; Dustmann & van Soest, 2001, 2002; Dustmann & Fabbri, 2003), and there is increasing evidence that language courses have large causal long-term earnings effects, 10-15 years after arrival (Sarvimäki & Hamalainen, 2016; Orlov, 2017; Foged et al., 2022a). Foged et al. (2022a) evaluated a

reform that extended Danish language courses by 200 instruction hours. In comparison, refugees in municipalities with a work-first approach have participated in 152 fewer hours of language instruction after 3½ years, than those without the work first approach. This corresponds to 75% of the increase in instruction time shown to produce long-run effects in Foged et al. (2022a). When early on-the-job training has no effects in the medium run and it decreases formal language acquisition, there is therefore a risk for negative effects on the labor market integration of refugees in the longer run, beyond the time horizon considered in the current study. This remains to be seen in future research.

If we are willing to invoke additional assumptions, all the significant results carry over to effects of participation in early on-the-job training when using instrumental variables where the local use of early on-the-job training is used as an instrument for the probability of individual participation in early on-the-job training.

Finally, we highlight that we have referred to estimates as associations because we cannot rule out that unobserved municipal characteristics are correlated with the propensity to use early on-the-job training as well as future outcomes. Therefore, results must be interpreted cautiously. We do show that we can bound the temporary employment effect and the more persistent effect on completed language course level away from zero using bounds from Oster (2019), and that selection on unobservables need to be substantial to be driving the effects.

7. Conclusion

Our findings show that a work-first approach to refugee labor market integration is positively associated with employment and earnings in the short run, but that the association fades out in the medium run of 3-4 years. This implies that a work-first and a language-first approach are substitutable in the medium run when it comes to labor market outcomes. However, the work first approach is also associated with reduced participation in language training and in the light of recent positive findings of long-term effects of language training, there is a risk that the work-first approach may produce negative results in the longer run. This remains to be seen in future research.

CRediT authorship contribution statement

Jacob Nielsen Arendt: Conceptualization, Methodology, Formal analysis, Writing – original draft, Writing – review & editing. **Iben Bolvig:** Conceptualization, Methodology, Formal analysis, Writing – original draft, Writing – review & editing, Project administration, Software, Visualization, Data curation.

Declaration of Competing Interest

None.

Data availability

The data that has been used is confidential.

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Appendix

Time-use and production technology

In this section, we consider a simple production technology that characterizes refugee's investment in labor income and language proficiency. The stylized model of the production technology shows how the empirically estimated effects of job training on labor income and language capital are sums of direct and indirect effects running through a potential substitution of time spent on language courses by time spent on job training.

When the refugee invests time in job training, T, and in language courses, C, she produces labor income (or employment), B, and language proficiency, L. To examine how the time investments affect the two outcomes, we assume the following simple production technology, with a triangular structure, where language proficiency is both an outcome and an input of labor income:

$$L = L(T, C), \ B = B(T, L)$$

We assume standard positive but marginally decreasing returns to each input: $L_1', L_2' > 0$ and $L_{11}'', L_{22}'' < 0$, $B_1', B_2' > 0$ and $B_{11}'', B_{22}'' < 0$. The marginal cross-products are left unrestricted. Here, L_1' reflects the marginal product of time spent on language acquisition through job training, e.g., through exposure to more natives, and L_2' reflects the marginal product of time spent on language acquisition through language courses. Likewise, B_1' reflects the marginal product of job training on labor income, e.g., through the experience and network that the training provides, and B_2' reflects the marginal product of language proficiency on labor income. Note that though we assume that language courses only affect labor income indirectly through its impact on language proficiency, this simplified framework allows language courses and job training to be either substitutes or complements in the production of labor market outcomes.

In the empirical analysis, we estimate the marginal effects of T on L and B. Since there is a natural constraint on time use, the time spent on job training cannot increase indefinitely without affecting time in the language course. We let $\frac{\partial C(T)}{\partial T}$ denote the marginal change of time-use in language courses when time-use on job training changes, and the effect of job training on language proficiency can be expressed as:

$$\frac{\partial L(T, C)}{\partial T} = L_1' + \frac{\partial C(T)}{\partial T} L_2'$$
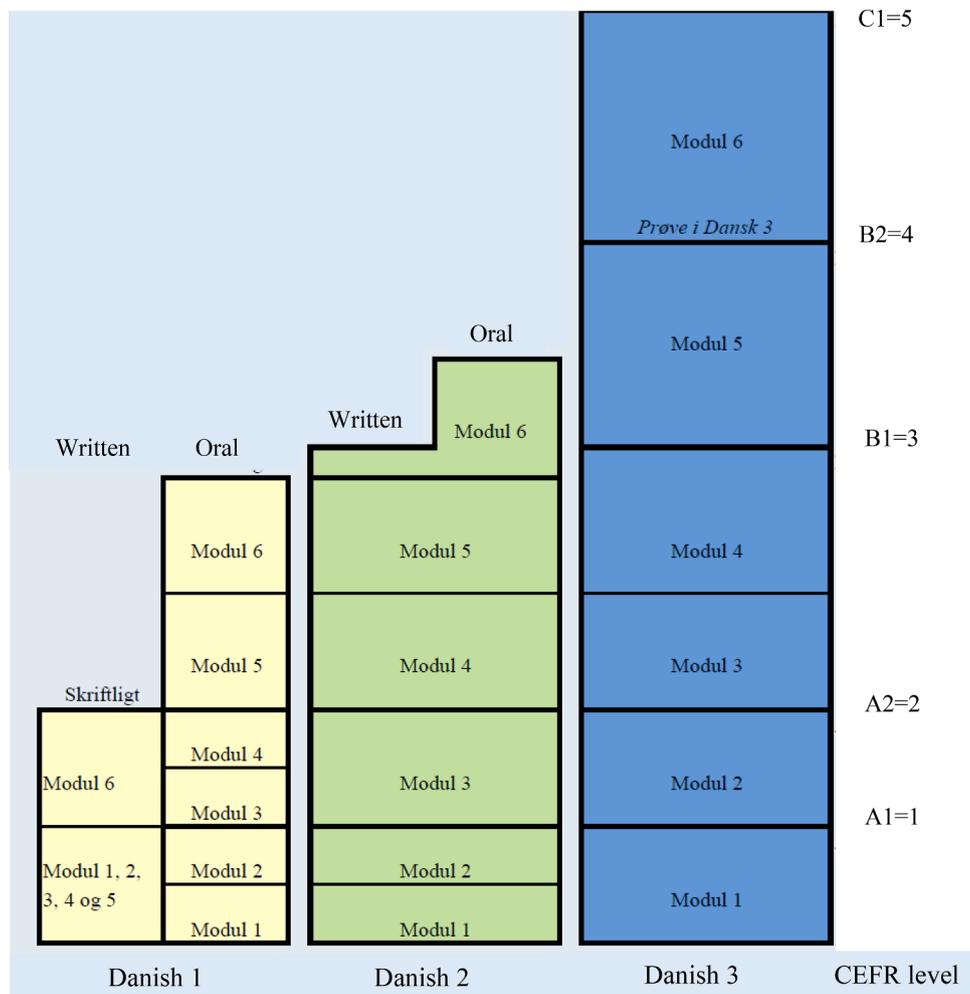

Fig. A.1. The relationship between Danish course modules and language proficiency based on the Common European Framework of Reference for Languages.
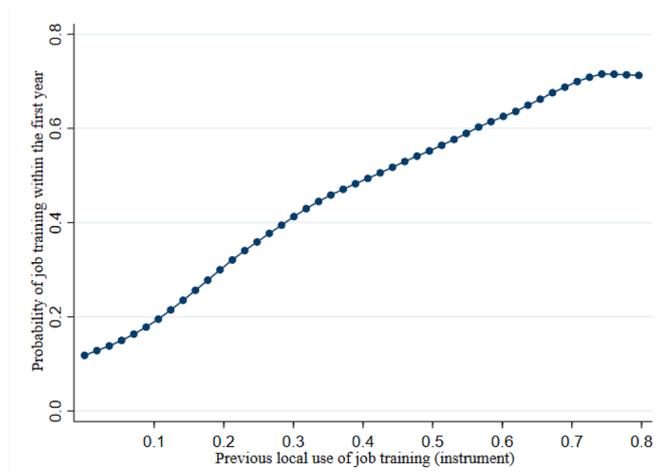
Fig. A.2. Local linear regression of the probability of early on-the-job training on the instrument.
Notes: Each dot represents a separate local linear estimate from a regression of a dummy for participating in early on-the-job training on the instrumental variable. Estimates obtained from an epanechnikov kernel and bandwidth of 0.96.

Table A.1
Main covariates and registers.

Covariate	Data source	Description
Female	BEF	
Female with child	–	Home-living child below 18 x female
Married	–	
Maternity leave first year	–	Increase in number of children from first to second year + female
Age	–	18-24, 25-34, 35-44, 45-54, 55-65
Children	–	Home-living children below 18: No children, 1-3 children, More than 3 children
Country of origin	–	Eritrea, Somalia, Iran, Iraq, Syria, Other
Language course initial level	Danish language school database (Danskuddannelsesdatabasen)	Danish 1 – module 1-6 Danish 2 – module 1-6 Danish 3 – module 1-5
Health care spending during first year	The National Health Insurance Service Registry (Sygesikringsregisteret)	Public spending in DKK using: General practice, Emergency call, Psychological practice
Hospitalization	National Patient Register (LPR)	Number of inpatient days of hospitalization
Local unemployment rate	www.statistikbanken.dk	Number of unemployed (full-time equivalents) as a fraction of the labor force in the municipality.
Local employment rate	www.statistikbanken.dk	Number of employed (full-time equivalents) as a fraction of the population aged 15-65 in the municipality.

Output measure	Data source	Description
Ever employed	BFL and DREAM	Some employment since arrival. Employment defined each month as positive income during the month or at least one registered working hour combined with no subsidized employment during that month
Labor income sum	BFL	Sum of wage income since arrival measured in DKK. Months without employment are included in the sum with a zero. Wage information is not available after 2018 and the measure is therefore not observed for 42 months for all refugees in the sample.
Ever employed	BFL and DREAM	Some employment since arrival. Employment defined each month as positive income during the month or at least one registered working hour combined with no subsidized employment during that month
Ever enrolled in education	DREAM	Receiving government financed education grant during the month
Attend exam	Danish language school database (Danskuddannelsesdatabasen)	Have participated in at least one language course exam.
CEFR	–	Common European Framework of Reference for Language: Level 1-5 according to Table A.1 A level has been reached if either a new level is started next month or if the relevant exam has been taken.
Hours of instruction attendance sum	—	Planned hours of instruction minus registered hours of absence. The sum of hours since arrival measured each month.
Sum of months with job-training	DREAM	Each month count as 1 if the individual has participated in job-training during the month. The sum of months with job-training since month 13.
GPA	Danish language school database (Danskuddannelsesdatabasen)	Grade point average: based on all the exams taken by month 42. If a student attends the same type of exam multiple times, we use the result from the latest exam.
Quota system		
Municipal quota	https://www.nyidanmark.dk/da/Numbers/visiteringskvoter	The quotas as announced by The Danish Immigrations Service.
Filled municipal quota	OPHG, BEF	We count the number of new arrivals of refugees in each municipality by arrival months. When this number is equal to or exceeds the quota, the quota is filled.

Table A.2
Reduced form effect on inverse hyperbolic transformed cumulative income.

| | Time since arrival: | | | | | |
	13 months	18 months	24 months	30 months	36 months	42 months
Labor income (inv. hyp. sine of sum)	0.812	0.818	0.312	0.134	-0.279	-0.120
	(0.542)	(0.568)	(0.556)	(0.565)	(0.532)	(0.593)
N	16157	16157	16157	16157	14957	11970

Standard errors (in parentheses) clustered at municipality level. $^+ p < 0.1$, $^* p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$.

Table A.3
Reduced form estimates of the effect of the local propensity to use early job-training, restricted sample to refugees arrived after municipal quotas are filled.

Panel A: Arrival after 5 municipal quotas are filled

Months since arrival	(1) Ever had a job		(2) Labor income (sum)		(3) Ever enrolled in education		(4) Completed language course level (CEFR)	
13	0.0650	(0.0474)	5276.9	(5738.5)	-0.00894	(0.00804)	-0.0899	(0.0650)
18	0.0802+	(0.0478)	11144.3	(8445.1)	-0.0186	(0.0148)	-0.0843	(0.108)
24	0.0464	(0.0465)	7784.2	(12573.2)	0.00617	(0.0327)	-0.184	(0.115)
30	0.0398	(0.0453)	8370.6	(14267.5)	-0.0188	(0.0416)	-0.294*	(0.113)
36	-0.0127	(0.0416)	-154.4	(15898.4)	-0.0474	(0.0462)	-0.281**	(0.106)
42	-0.00565	(0.0394)	9549.2	(22003.1)	-0.0713	(0.0458)	-0.259*	(0.104)

Arrival after 10 municipal quotas are filled

Months since arrival	(1) Ever had a job		(2) Labor income (sum)		(3) Ever enrolled in education		(4) Completed language course level (CEFR)	
13	0.0555	(0.0535)	5913.3	(7420.3)	-0.00140	(0.0138)	-0.0422	(0.0826)
18	0.0321	(0.0547)	10924.5	(10129.7)	-0.0285	(0.0201)	-0.0497	(0.119)
24	-0.0177	(0.0523)	5264.0	(15070.6)	0.0226	(0.0447)	-0.0603	(0.137)
30	0.0162	(0.0544)	1949.2	(17107.6)	0.0128	(0.0516)	-0.166	(0.140)
36	-0.0215	(0.0518)	-8555.9	(20427.7)	-0.0306	(0.0555)	-0.202	(0.126)
42	0.00259	(0.0531)	23047.6	(30837.8)	-0.0682	(0.0592)	-0.162	(0.121)
N	16157		16157†		16157		16157	

Notes: Reduced form estimates with controls for background characteristics, municipality dummies, and dummies for semester of arrival (included covariates are presented in Appendix Table A2). Standard errors (in parentheses) are clustered at the municipality level.
† $N = 14,957$ at 36 months and 11,970 at 42 months. $^+ p < 0.1$, $^* p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$

The effect therefore consists of a direct effect of job training on language proficiency, L'_1, and an indirect (substitution) effect, $\frac{\partial C(T)}{\partial T}L'_2$, that reflects altered time-use on the other input in the production of language proficiency (language course participation) times its marginal product. We denote the sum as the net effect. Assuming that $-1 \leq \frac{\partial C(T)}{\partial T} \leq 0$, we have that:

$$L'_1 < (>)L'_2 \ if \ \frac{\partial L(T,C)}{\partial T} < (>)0$$

According to this formula, if the net-effect of job training on language proficiency is negative, then one hour of job training cannot substitute an hour of language course participation when it comes to the production of language proficiency. When time-use is not binding, we may have that $\frac{\partial C(T)}{\partial T}$ is close to zero, and $\frac{\partial L(T,C)}{\partial T} \sim L'_1$. By a similar analogy, the effect of job training on labor income is:

$$\frac{\partial B(T,L)}{\partial T} = B'_1 + \frac{\partial L(T,C)}{\partial T}B'_2 = B'_1 + \left(L'_1 + \frac{\partial C(T)}{\partial T}L'_2\right)B'_2$$

The effect reflects a direct effect from job training, B'_1, and an indirect effect arising through changed time in language courses. The latter is the marginal product of language proficiency on labor income, B'_2, times the net-effect of job training on language proficiency, which is discussed above: $\left(L'_1 + \frac{\partial C(T)}{\partial T}L'_2\right) = \frac{\partial L(T,C)}{\partial T}$. We have that:

$$\frac{\partial B(T,L)}{\partial T} > 0 \ if \ \frac{\partial L(T,C)}{\partial T} > 0$$

On the other hand, the sign of $\frac{\partial B(T,L)}{\partial T}$ is ambiguous if $\frac{\partial L(T,C)}{\partial T} < 0$, i.e., a positive direct effect of job training can be countered by the indirect effect running through language proficiency, if this effect is negative.

Table A.4

Effect of covariates on estimated weights in two-way FE models for GPA and Employment measured 42 months after arrival.

	CEFR month 42		Ever had job month 18		Ever had job month 42	
	Estimate	Std. err	Estimate	Std. err	Estimate	Std. err
Municipality	-0.1651	1.7664	-0.2459	1.7468	-0.1651	1.7664
Semester of arrival	-0.4066*	0.1967	-0.4022*	0.1983	-0.4066	0.1967
Woman	0.0018	0.0010	0.0018	0.0010	0.0018	0.0010
Woman with children	0.0014	0.0010	0.0015	0.0010	0.0014	0.0010
Married	0.0028	0.0022	0.0025	0.0022	0.0028	0.0022
Maternity within first 12 months	0.0005	0.0009	0.0004	0.0009	0.0005	0.0009
Age 18-25	-0.0010	0.0014	-0.0010	0.0015	-0.0010	0.0014
Age 25-35	-0.0002	0.0012	-0.0002	0.0012	-0.0002	0.0012
Age 35-45	0.0007	0.0011	0.0007	0.0011	0.0007	0.0011
Age 45-55	0.0002	0.0005	0.0002	0.0005	0.0002	0.0005
Age 55+	0.0003	0.0004	0.0002	0.0004	0.0003	0.0004
No children	-0.0009	0.0015	-0.0016	0.0019	-0.0009	0.0015
1-3 children	0.0003	0.0013	0.0010	0.0016	0.0003	0.0013
Country of origin:						
Eritrea	0.0000	0.0000	-0.0041	0.0038	-0.0041	0.0038
Somalia	-0.0041	0.0038	0.0011	0.0011	0.0011	0.0011
Iran	0.0011	0.0011	0.0015	0.0013	0.0015	0.0013
Iraq	0.0015	0.0013	0.0001	0.0003	0.0001	0.0003
Syria	0.0001	0.0003	-0.0026	0.0029	-0.0027	0.0028
Health care utilization 1st Year:					0.0000	0.0000
General practitioner (1000 DKK)	6.8594*	3.0904	6.7000*	3.0706	6.8594*	3.0904
Emergency care (1000 DKK)	0.0028	0.2747	-0.0054	0.2754	0.0028	0.2747
Psychiatry (1000 DKK)	-0.0428	0.6861	-0.0035	0.6912	-0.0428	0.6861
Hospitalized (days)	0.0007	0.0007	0.0005	0.0016	0.0007	0.0007
Course level at arrival:						
Low level, module 2	0.0002	0.0003	0.0002	0.0003	0.0002	0.0003
Low level, module 3	0.0000	0.0001	0.0000	0.0001	0.0000	0.0001
Low level, module 4	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001
Low level, module 5	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Low level, module 6	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Medium level, module 1	-0.0021	0.0019	-0.0020	0.0019	-0.0021	0.0019
Medium level, module 2	0.0000	0.0005	0.0000	0.0005	0.0000	0.0005
Medium level, module 3	0.0001	0.0002	0.0001	0.0002	0.0001	0.0002
Medium level, module 4	0.0000	0.0001	0.0000	0.0001	0.0000	0.0001
Medium level, module 5	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Medium level, module 6	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
High level, module 1	-0.0005	0.0009	-0.0005	0.0009	-0.0005	0.0009
High level, module 2	-0.0001	0.0002	-0.0001	0.0002	-0.0001	0.0002
High level, module 3	0.0000	0.0001	0.0000	0.0001	0.0000	0.0001
High level, module 4	0.0000	0.0001	0.0000	0.0001	0.0000	0.0001
High level, module 5	0.0000	0.0000	0.0000	0.0001	0.0000	0.0000
Local unemployment rate	0.0330**	0.0107	0.0326**	0.0108	0.0330**	0.0107
Local employment rate	-0.0432	0.0275	-0.0446	0.0272	-0.0432	0.0275
Starting month of language course	-0.0081	0.0094	-0.0078	0.0093	-0.0081	0.0094
Share negative weights	0.511		0.507		0.511	
Sum positive weights	2.044		2.038		2.044	
Sum negative weights	-1.044		-1.038		-1.044	
Min. std. dev of ATT consistent with sum = 0	0.061		0.016		0.009	
Min. std. dev of ATT consistent with different sign	0.094		0.023		0.015	

Estimated using Stata program *twowayfeweights*. Clustered standard errors in parentheses. $^+ p < 0.1$, $* p < 0.05$, $** p < 0.01$, $*** p < 0.001$

Table A.5
Reduced form estimates of the effect of the local propensity to use early job-training, using the previous 15 months (Panel A) and 25 months (Panel B) to calculate the local propensity.

Panel A: Local propensity to use early job-training based on previous 15 months								
Months since arrival	(1)		(2)		(3)		(4)	
	Ever had a job		Labor income (sum)		Ever enrolled in education		Completed language course level (CEFR)	
13	0.0808+	(0.0439)	5861.5	(4852.4)	-0.00532	(0.00673)	-0.0878	(0.0589)
18	0.0966*	(0.0461)	12315.0	(7420.0)	-0.0150	(0.0134)	-0.0528	(0.0968)
24	0.0612	(0.0422)	9507.8	(10685.0)	-0.00004	(0.0295)	-0.187+	(0.106)
30	0.0400	(0.0433)	10006.0	(12556.9)	-0.0102	(0.0356)	-0.263*	(0.113)
36	-0.00052	(0.0388)	540.3	(14309.5)	-0.0545	(0.0414)	-0.275*	(0.110)
42	0.00932	(0.0369)	16234.9	(19801.5)	-0.0550	(0.0414)	-0.256*	(0.110)
N	16046		16046†		16046		16046	

Panel B: Local propensity to use early job-training based on previous 25 months								
Months since arrival	(1)		(2)		(3)		(4)	
	Ever had a job		Labor income (sum)		Ever enrolled in education		Completed language course level (CEFR)	
13	0.111*	(0.0476)	9395.6	(6168.7)	-0.00476	(0.00837)	-0.0883	(0.0677)
18	0.0946+	(0.0501)	16587.7+	(9031.5)	-0.0157	(0.0146)	-0.0280	(0.116)
24	0.0419	(0.0475)	10983.9	(13063.2)	-0.00304	(0.0337)	-0.209+	(0.121)
30	0.0288	(0.0486)	11606.5	(15786.3)	-0.00780	(0.0413)	-0.305*	(0.127)
36	-0.0151	(0.0456)	1154.8	(17952.0)	-0.0504	(0.0487)	-0.339**	(0.118)
42	-0.00007	(0.0393)	15012.1	(23716.5)	-0.0678	(0.0487)	-0.316**	(0.118)
N	16196		16196††		16196		16196	

Notes: Reduced form estimates with controls for background characteristics, municipality dummies, and dummies for semester of arrival (included covariates are presented in Appendix Table A2). Standard errors (in parentheses) are clustered at the municipality level.
†N = 14,848 at 36 months and 11,865 at 42 months. †† N = 14,996 at 36 months and 12,008 at 42 months. $^+ p < 0.1$, $* p < 0.05$, $** p < 0.01$, $*** p < 0.001$

Table A.6
Reduced form estimates of the effect of the local propensity to use early job-training. Heterogenous effects by gender, age and having children at time of arrival.

		Time since arrival:						
		13 months	18 months	24 months	30 months	36 months	42 months	N
Ever had a job	Below 30	0.0850	0.0759	0.0715	0.0491	0.0379	0.0538	8361
		(0.0617)	(0.0613)	(0.0568)	(0.0546)	(0.0467)	(0.0392)	
	Aged 30 and above	0.118*	0.125*	0.0440	0.0237	-0.0460	-0.0503	7796
		(0.0468)	(0.0594)	(0.0664)	(0.0612)	(0.0623)	(0.0582)	
Completed language course level (CEFR)	Below 30	-0.0810	-0.0387	-0.199	-0.317+	-0.321*	-0.299*	8361
		(0.0993)	(0.138)	(0.158)	(0.165)	(0.151)	(0.143)	
	Aged 30 and above	-0.115	-0.0815	-0.193	-0.269+	-0.278+	-0.263+	7796
		(0.0781)	(0.139)	(0.138)	(0.141)	(0.141)	(0.142)	

Notes: reduced form models estimated for each subgroup separately with controls for background characteristics, municipality dummies, and dummies for semester of arrival (included covariates are presented in Appendix Table A.1). Standard errors (in parentheses) are clustered at the municipality level. $+ p < 0.1$, $* p < 0.05$, $** p < 0.01$, $*** p < 0.001$

Table A.7
Reduced form estimates of the effect of the local unemployment and employment rates, mechanisms.

Panel A: Local unemployment rate at arrival								
Months since arrival	(1)		(2)		(3)		(4)	
	Instruction hours (sum)		On-the-job-training (sum)		Exam attendance		GPA (std. diff. from mean)	
13	2.348	-8.681	0.0228	(0.0154)	-0.0052+	(0.00268)	NA	
18	5.423	(11.72)	0.0942	(0.0893)	-0.0100+	(0.00594)	NA	
24	9.348	(14.58)	0.178	(0.147)	-0.0106	(0.00935)	0.0381	(0.0867)
30	9.302	(16.31)	0.266	(0.196)	-0.0007	(0.0140)	0.0426	(0.0592)
36	6.618	(18.11)	0.390+	(0.231)	0.0162	(0.0155)	0.0327	(0.0500)
42	1.651	(18.34)	0.432+	(0.254)	0.0206	(0.0166)	0.00826	(0.0470)
N	16046		16046		16046		8518#	

Panel B: Local fraction of employed citizens at arrival								
Months since arrival	(1)		(2)		(3)		(4)	
	Instruction hours (sum)		On-the-job-training (sum)		Exam attendance		GPA (std. diff. from mean)	
13	8.295	-9.213	0.0104	(0.0135)	-0.00244	(0.00161)	NA	
18	2.645	-6.067	0.0141	(0.0307)	-0.00385	(0.00332)	NA	
24	0.755	-4.836	0.0307	(0.0384)	-0.00365	(0.00334)	0.00183	(0.0265)
30	1.345	-4.552	0.0271	(0.0485)	0.00619	(0.00523)	0.0210	(0.0168)
36	0.962	-4.647	0.0546	(0.0542)	0.00752	(0.00488)	0.0143	(0.0138)
42	0.291	-4.586	0.0989+	(0.0568)	0.00755	(0.00514)	0.00734	(0.0131)
N	16196		16196		16196		8518#	

Notes: Reduced form estimates with controls for background characteristics, municipality dummies, and dummies for semester of arrival (included covariates are presented in Appendix Table A.1). Standard errors (in parentheses) are clustered at the municipality level.
The estimate for GPA is conditional on exam attendance, and the panel is shorter at earlier periods: N = 1651 at 24 months, N = 4035 30 months, and N = 6594 at 36 months. $^+ p < 0.1$, $* p < 0.05$, $** p < 0.01$, $*** p < 0.001$

Table A.8

Instrumental variable estimates of the effect of the local propensity to use early job-training, mechanism.

Months since arrival	(1) Instruction hours (sum)		(2) On-the-job-training (sum)		(3) Exam attendance		(4) GPA (std. diff. from mean)	
13	-61.02	(75.83)	0.528***	(0.0738)	0.00262	(0.0141)		
18	-109.6	(96.45)	1.332**	(0.484)	0.00712	(0.0342)		
24	-173.3	(107.7)	1.363+	(0.774)	0.0724	(0.0622)		
30	-229.8*	(106.7)	1.110	(0.844)	-0.128	(0.0978)	-0.916**	(0.330)
36	-256.5*	(100.9)	1.430+	(0.849)	-0.0891	(0.101)	-0.880***	(0.259)
42	-251.1**	(96.35)	2.054*	(0.850)	-0.0285	(0.0881)	-0.724**	(0.224)
N	16046		16046		16046		8518[#]	

Notes: IV estimates with controls for background characteristics, municipality dummies, and dummies for semester of arrival (included covariates are presented in Appendix Table A.1). Standard errors (in parentheses) are clustered at the municipality level.

[#] The estimate for GPA is conditional on exam attendance, and the panel is shorter at earlier periods: $N = 1651$ at 24 months, $N = 4035$ at 30 months, and $N = 6594$ at 36 months. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.9

Reduced form estimates of the effect of the local propensity to use early job-training. Standard errors clustered at municipality-arrival-month-level.

Months since arrival	(1) Ever had a job		(2) Labor income (sum)		(3) Ever enrolled in education		(4) Completed language course level (CEFR)	
13	0.0937** [0.05]	(0.0309)	8297.4* [3688.1]	(3552.3)	-0.00724 [0.00]	(0.00856)	-0.0990 [1.20]	(0.00856)
18	0.0898* [0.12]	(0.0354)	15430.4** [8137.9]	(5473.0)	-0.0146 [0.02]	(0.0129)	-0.0641 [1.54]	(0.0129)
24	0.0509 [0.23]	(0.0366)	11704.0 [19729.3]	(7993.1)	-0.00583 [0.05]	(0.0236)	-0.201* [1.93]	(0.0236)
30	0.0263 [0.33]	(0.0366)	12586.6 [38877.6]	(10519.0)	-0.0157 [0.08]	(0.0286)	-0.292*** [2.23]	(0.0286)
36	-0.0116 [0.40]	(0.0349)	3079.4 [63859.9]	(13808.7)	-0.0507 [0.09]	(0.0314)	-0.309*** [2.39]	(0.0314)
42	-0.00198 [0.40]	(0.0336)	15802.0 [93617.1]	(18841.7)	-0.0653 [0.08]	(0.0335)	-0.285*** [2.43]	(0.0335)
N	16157		16157††		16157		16157	

Mechanism:

Months since arrival	Instruction hours (sum since arrival)		Months of on-the-job-training (sum after first year)		Exam attendance		GPA (std. diff. from mean)	
13	-31.66+ [455.28]	(19.11)	0.274*** [0.06]	(0.0416)	0.00136 [0.00]	(0.00611)	NA	
18	-57.21* [610.43]	(24.81)	0.695*** [0.87]	(0.192)	0.00372 [0.02]	(0.0140)	NA	
24	-93.16** [765.94]	(29.70)	0.733* [2.15]	(0.307)	0.0389 [0.09]	(0.0272)	-0.528+ [0.26]	(0.272)
30	-133.1*** [872.81]	(32.89)	0.643+ [3.36]	(0.383)	-0.0741* [0.22]	(0.0376)	-0.535** [0.14]	(0.175)
36	-153.3* [952.15]	(35.04)	0.855+ [4.44]	(0.446)	-0.0532 [0.38]	(0.0409)	-0.556*** [0.04]	(0.127)
42	-152.7* [1018.62]	(36.63)	1.249* [5.30]	(0.498)	-0.0173 [0.51]	(0.0395)	-0.471*** [-0.02]	(0.112)
N	16157		16157		16157		8518[#]	

Notes: Reduced form estimates with controls for individual background characteristics, local labor market conditions, municipality dummies, and dummies for semester of arrival (included covariates are presented in Appendix Table A.1). Standard errors (in parentheses) are clustered at the municipality-arrival-months-level. Mean outcomes for refugees not participating in early on-the-job training in [].

† The panel is shorter when income is the dependent variable: $N = 14,957$ at 36 months and 11,970 at 42 months.

[#] The estimate for GPA is conditional on exam attendance, and the panel is shorter at earlier periods: $N = 1651$ at 24 months, $N = 4035$ at 30 months, and N=6594 at 36 months. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

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