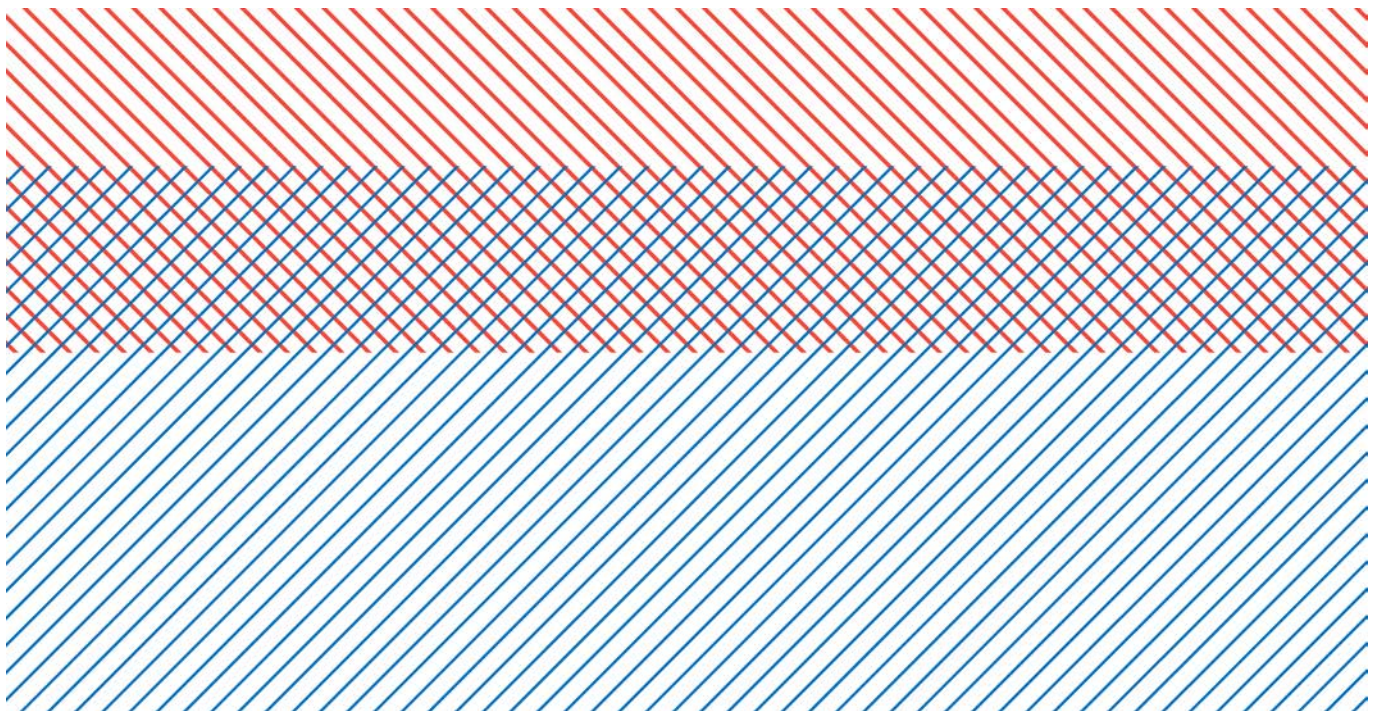


Working paper

Early labor market entry, language acquisition and labor market success of refugees



Jacob Nielsen Arendt and Iben Bolvig

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Hans Hummelgaard

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Abstract

This study investigates how early on-the-job training offered to refugees participating in language courses affects their subsequent language learning and longer-run labor market outcomes. To identify the causal effect of early on-the-job training, we utilize a gradual rollout of that training combined with a dispersal policy of refugees across municipalities in Denmark. We find that the on-the-job training in the year of arrival reduces standardized language tests scores after three to four years by 75% of a standard deviation. We also find that early on-the-job training has positive short-term effects on employment, but show that these effects are temporary. We show that time spent in job training replaces time spent in language courses and conjecture this may explain the negative effect on language acquisition and the lack of persistent employment effects.

Keywords: Refugee, Immigrants, Integration, Language acquisition, Skill investment, Labor market

JEL Code: J15; J24; J61; Z13

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1 Introduction

The number of people forced to leave their home country has been increasing steadily worldwide since 2012, with the number of refugees arriving in Europe reaching its highest level in 2015. Since 2015, most European countries have developed new strategies for integrating refugees into the labor market, and only by learning from these and earlier experiences are the relevant governments able to foster better integration for those who end up staying in their countries. However, even though the number of asylum applications in the EU has stabilized since 2017 at half the level of 2015, the integration of refugees into the European labor market remains a serious challenge (Dustmann et al., 2017; Fasani et al., 2018, 2020; Brell et al., 2020).¹

Two dimensions of refugee integration are predominant in the national integration policies in the EU: host country language knowledge and labor market participation. National integration strategies therefore often offer both language and employment support (Martín et al., 2016; Hernes et al., 2019). While language courses have been offered from the time of arrival for decades in some countries², a larger emphasis on “work-first” strategies has gained popularity in many Northern European countries, particularly in the aftermath of the events in 2015 (Martín et al., 2016; Konle-Seidl, 2016; Desiderio, 2016; Marbach et al., 2018; Fasani et al., 2020). Work-first strategies may include interventions such as early skills assessment, fast-track options, early on-the-job training, economic incentives for labor market participation, and work permits during the asylum-seeking phase.

This study compares two integration strategies for persons with asylum or subsidiary protection as well as their adult reunified family (henceforth referred to commonly as refugees): We focus on refugees who participate in language courses during the first year since arrival (93% in our context) and among them we contrast the refugees who participate in on-the-job training during the first year after arrival with those who do not. Since both strategies involve language courses, we refer to the joint strategy as the treatment and label it early job training, but please note that it is measured as an *addendum* to language courses. We examine how participation in early job training affects host country language acquisition, measured by standardized achievement test scores, as well as labor market outcomes in the longer run, i.e., four years after arrival.

We hypothesize that early on-the-job training may affect host country language acquisition and labor market outcomes directly, but also through indirect effects that operate through a substitution of time spent on the labor market with time spent in language courses. To be specific, the early on-the-job training may affect language acquisition through the two mechanisms: (1) a positive direct effect through higher exposure to host country natives and therefore to the host country language, and (2) a negative indirect effect through reduced time for language course participation. These conjectures are based on the literature on exposure to the host country language (Chiswick and Miller, 2014), which is further discussed below. Furthermore, early on-the-job training may affect labor market outcomes through the two mechanisms: (1) a direct effect (e.g., through job-specific training and an expanded network), and (2) an indirect

¹ https://ec.europa.eu/eurostat/statistics-explained/index.php/Asylum_statistics

² For example, language courses for newly arrived immigrants have been compulsory for unemployed refugees in the Netherlands from 1998-2006, in Denmark since 1999, in Norway since 2004, and in Germany since 2005 (Arendt, 2018a).

effect through altered language proficiency. These hypotheses are developed formally in Section 2, and in Section 6 we provide estimates of the net-effects and examine the size of the indirect mechanisms.

We apply administrative registry data covering all refugees aged 18-64 who received residency for the first time in Denmark from 2012 to 2016. The data contain information on monthly employment outcomes, scores on language tests, monthly participation patterns in language courses and job training, as well as detailed information on health care utilization. Health care information is important because poor health is prevalent among refugees (Porter and Haslam, 2005; Fazel et al., 2005; Peconga and Thøgersen, 2019) and may be an important barrier to labor market participation in destination country labor markets. All refugees are offered participation in language courses and job training during a three-year long integration program. Participation is highly incentivized since non-participation is sanctioned financially, and passing a language test at the end of the program is required to be eligible for permanent residency. It is left to the discretion of local municipalities how to combine language courses and job training. Historically, most municipalities have emphasized early language courses followed by job training during the end of the program (Bolvig and Arendt, 2018). We argue that a series of governmental recommendations paved the way for a gradual roll-out over time of the use of early on-the-job training for refugees in Denmark. This roll-out is not sufficient to identify the effect of job training, if refugees can choose where to live and select themselves into certain areas. To rule this out, we take advantage of the Danish dispersal policy for refugees, which allocate refugees across the country based on local municipality quotas. Such dispersal policies have previously been applied for identification of the effect of local characteristics on refugee employment (Edin et al., 2003, 2004; Damm, 2009; Azlor et al., 2020). We therefore base our identification on a random dispersal of refugees across the country, combined with within municipality variation over time in the propensity to use early on-the-job training.

We find that early on-the-job training reduces language acquisition at the end of the integration course considerably: a reduction in the grade point average in the tests at the end of the course of 35-40%, corresponding to a drop by 75% of a standard deviation. Although early on-the-job training has a positive effect on employment during the second year after arrival, the effect disappears after three and four years. Considering the mechanisms, early on-the-job training crowds out time spent in the language course during the integration program and increases time spent in additional job training. This substitution of time use seems to be a main mechanism behind the results and suggests that time spent in the labor market (job training or employment) cannot substitute time spent on language investments when it comes to the production of language proficiency. Examinations of effect heterogeneity show that the size and significance of the effects vary by gender and initial skill level, although they are never significantly different from each other across these groups.

Our study contributes to different strands of literature, each of which examines the integration and assimilation of refugees in the host country. One literature examines the main determinants of language proficiency. Language proficiency is an important outcome in its own right, and the literature on this has often stressed the three E's to language proficiency acquisition: exposure, economics, and efficiency (Chiswick & Miller, 2014). Post-migration exposure to the native language has mainly been studied indirectly: by years since migration and trips outside the country (Espinosa and Massey, 1997) or by the concentration of immigrants in the local neighborhood (Chiswick, 1998; Espenshade and Fu, 1997; Warman, 2007). We contribute to this literature by examining whether a new measure of exposure – time spent on the labor market – affects

language acquisition. Our study of refugees constitutes a valuable case for the study of language acquisition because refugees, in the current setting of a Scandinavian country, share the same lack of knowledge of the host country language upon arrival (limited previous exposure) and have free access to language training (limited economic barriers).

We also contribute to the literature that has emphasized host country language acquisition as an important determinant of integration in Western countries, both on the labor markets (Chiswick and Miller, 1995; Dustmann and van Soest, 2001; 2002; Dustmann and Fabbri, 2003) and for other dimensions of integration, such as parenting and participation in society (Chiswick, 1991; Dustmann, 1994; Chiswick and Miller, 2014; Zorlu and Hartog, 2018). The endogeneity of language proficiency is a central issue in this literature. Therefore, it is of great policy relevance that a few recent studies have shown that participation in host country language courses can have substantial positive long-run employment effects (Sarvimaaki & Hamalainen, 2016; Orlov, 2017; Arendt et al., 2020). We contribute to this literature by examining how indirect effects of job training – operating through language course participation – may affect two dimensions of integration: language proficiency and labor market outcomes.

A final 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 (Joonas et al., 2016; Clausen et al., 2009; Heinesen et al., 2011; Arendt, 2020a; Arendt, 2020b). Related studies show that early access to the labor market—as early as the asylum-seeking period—may matter in the long run (Marbach et al., 2018; Fasani et al., 2020). Therefore, early labor market entry may also be a stepping-stone towards higher employment in the long run. Yet, it is unknown whether similar effects can be obtained by on-the-job training. We therefore contribute to this literature by examining the effect of job training on employment in the longer run (where the longer run is four years since arrival).

The rest of the paper is organized as follows: In the next section, we present a stylized framework that aids the interpretation of mechanisms underlying the estimated effects, which is sketched under the hypothesis above. Section 3 presents the institutional settings governing refugee admission and participation in on-the-job training and language courses in Denmark. Section 4 describes the data and provides descriptive statistics for our main sample. Section 5 presents the empirical strategy, and Section 6 contains the results. Section 7 discusses implications of the results for policy makers and for future research, and Section 8 concludes.

2 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 with time spent on job training³.

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.

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 rise 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 in job training changes, and the effect of job training on language proficiency can be expressed as:

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

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:

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

This says that 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

³ We may also formally develop a model where the time spent on job training and language courses is determined by utility maximization, but note that utility maximization is determined under specific constraints, depending upon availability of job training slots, sanctioning of non-participation and national and local regulation of integration policies. This is beyond the scope of the current paper and is left for future research.

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:

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

The effect reflects a direct effect from job training, \mathbf{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, \mathbf{B}'_2 , times the net-effect of job training on language proficiency, discussed above: $\left(L'_1 + \frac{\partial C(T)}{\partial T} L'_2 \right) = \frac{\partial L(T,C)}{\partial T}$. We have that:

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

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

3 Institutional settings

This section describes the institutional settings that govern and implement the Danish immigration and integration policies for refugees and their families. A person who enters the country illegally can apply for asylum according to the UN convention or according to national regulation that provides subsidiary protection. The applicants live in decentralized asylum centers while the application is being evaluated, with access to minimal public care, and even though access to the labor market has been allowed since 2013 if the asylum decision takes more than 6 months, extremely few asylum seekers take advantage of this opportunity⁴. The waiting time in asylum centers for residency decisions varied between one half to one year during the period of consideration (Hvidtfeldt & Schultz-Nielsen 2018). Refugees may also be resettled from another country according to arrangements with the UNHCR. Once having been granted asylum, the immigrant can apply for family re-unification with family members and is eligible for public services, including welfare benefits.

Settlement across municipalities is determined by a public dispersal policy for refugees. The dispersal policy allocates refugees based on quotas determined by the number of immigrants from non-Western countries already residing in the municipality. The dispersal policy consists of a two-step process, where refugees are first allocated to five larger regions, after which the municipalities negotiate the internal allocation. If they do not agree upon the allocation, the government conducts the allocation on a local level. 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. This dispersal policy is important for our study, since it implies that refugees are close to being randomly allocated across the country and rules out the possibility of refugees choosing their initial location based on the type of language school or on employment opportunities. Other studies have exploited dispersion policies to estimate causal effects of local area characteristics (Edin et al., 2003; 2004; Damm 2009; Damm 2014; Azlor et al., 2020). Azlor et al., (2020) has stressed the potential problem of the municipalities being able to make requests for certain types of refugees and vice versa, thereby distressing the experimental setting (Azlor et al., 2020). We examine whether this is a problem in our context in a robustness analysis. We also conduct a robustness analysis that takes into account the fact that municipalities may choose an integration strategy that depends on local labor market characteristics. The robustness analyses are further described in Section 6.5.

3.1 The language course and the integration program

An important feature of the institutional setting in Denmark is that all immigrants must be referred to a language course within a month of arrival (Act No. 1062 of 2010)⁵. The language course has a duration corresponding to up to 1.2 years of full-time studies⁶ and is provided by public or private language schools. The local municipalities finance the courses by a fixed pay per student, but they are partly reimbursed by the national government.⁷ It is mandatory for

⁴ According to The Immigration Services, 277 of the persons seeking asylum in 2013-16 applied for the right to work and 78 were approved. 3.500 could have applied in 2016 alone (<https://www.dr.dk/nyheder/indland/radikale-giv-asylansoegere-lov-til-arbejde-fra-dag-et>). News from the national Danish television channel based on numbers from the Danish Immigration Service (in Danish)).

⁵ This subsection refers to legislation enacted prior to arrival, but holds for all arrival cohorts in our sample.

⁶ One full-time year corresponds to 37 hours per week for 46 weeks, which includes both lectures and homework (The Ministry for Refugees, Immigrants and Integration 2011, p. 30)

⁷ There are 98 municipalities of varying size, with 20,000 inhabitants on average. There are around 60 language schools that offer language courses for immigrants.

immigrants who receive welfare benefits to participate in the language courses, and non-participation lead to financial sanctions. 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). According to the Act on Danish language education for Immigrants, Level 1 is for immigrants with limited schooling who have not learned to read and write in their mother tongue and immigrants without knowledge of the Latin alphabet. Level 2 is for immigrants who have completed primary schooling or a short education and are expected to be slow learners of the Danish language. Level 3 is for immigrants with a medium or long education, who are expected to be faster learners of the Danish language. The minimum level of education for participants enrolled in Danish course level 3 is usually upper secondary education. Results from a test of reading pace can, however, move immigrants to a different course level (Ministry for Refugees, Immigrants and Integration 2003). The assignment to a course level is conducted by the institutions offering the language course. Each of the three language courses contains six modules that need to be passed consecutively to progress to the next level. The courses end with a standardized exam that is graded by an external examiner. Participation in the exam is free of charge⁸ and is incentivized since it is a condition for obtaining permanent residence to pass one of the three Danish courses (Act No 984 of 2012)⁹.

The language course is part of a three-year integration program for immigrants initiated within one month after receiving residency. In addition to the language course, the integration program includes a brief course in civic understanding and labor market related training activities for the unemployed. The language course can be broken up into parts throughout the three-year period (i.e. the course may be paused), and the municipalities may offer language classes during the day or as evening classes to accommodate working immigrants or immigrants participating in labor market training programs. The combination of training and language courses is under the discretion of the municipalities.

3.2 Rollout of early on-the-job training

A number of events have gradually paved the way for a larger focus on the work-first strategy emphasizing early contact with the labor market for refugees in the integration program. In 2004, the use of a new and cheap type of on-the-job training (internship) in the second year of the integration program was introduced. The use of internships was incentivized by raising the reimbursement for this type relative to other types of employment support (Wiberg & Andreasen 2003). The purpose of the internship was that particularly long-term unemployed and immigrants could obtain 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. 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 have documented that on-the-job training speeded up labor market entry particularly for immigrants, while other types of employment support were less effective in the short run (Clausen et al., 2009; Heinesen et al., 2011). 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

⁸ Although there is a small fee for re-examinations.

⁹ At the outset, asylum or family reunification provides temporary residency, which must be renewed every second year.

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). This recommendation was implemented in the Act on Integration in 2016, which made early on-the-job training compulsory for arrivals from October 2016 (Act No. 665 of 2016). The use of very early job training, within the first month after arrival, which was mandated by this reform, is studied in Arendt, (2020a). As we describe below, those immigrants for whom this legal requirement of very early job training applied are not included in the current study.

This 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 earlier 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 5. Empirical documentation of the gradual rollout is provided in the data section below.

4 Data and descriptive statistics

The data set used in this study is based on administrative registries from National authorities with information on the time and type of residence permit. We include all immigrants with asylum or subsidiary protection and their reunited adult family members who arrived in Denmark in the years 2012 to 2016. In the following, we will refer to this entire group as refugees, in short. The date on which the refugee is assigned to the municipality is the date the residence permit is issued and we refer to this date as the date of arrival. We restrict attention to the given period because the regulation of temporary and permanent residence is constant in this period. This is important as they provide incentives for early labor market entry and language school participation.

From this data, we select all refugees who at time of arrival are aged between 18 and 65. Everyone in this age range is entitled to unemployment benefits but not to public pension. This data set contains 27,093 refugees. Refugees can be exempt from participation in the introduction program, for instance if they suffer from severe chronic illness. Therefore, we exclude refugees who have not begun the language course within the first year of arrival (leaving 25,266 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 21,335 refugees, of whom 20,480 are still in the country after 42 months (96%). Note that this excludes refugees subjected to the very early job training required by a policy change in 2016, described in the previous section, which was implemented in October 2016. Finally, we exclude observations with missing data on the main outcomes. This leaves us with a main sample of 20,358 refugees. We include the sample of refugees who are observed for 48 months in supplementary analyses to look at the trend further ahead (15,962 refugees). In a robustness analysis, we follow the strategy of Azlor et al., (2020), who exclude refugees arriving during the first months of the year, in order to reduce the municipalities' ability to influence the settlement decision. For this robustness analysis we exclude all refugees arriving before at least 10 municipalities have filled their quotas, which reduces the sample to 12,079 refugees.

For the given population of refugees, we link administrative-individual level data from the language schools and municipalities. 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. The refugees must attend both an oral exam, a reading comprehension exam, and a written exam at the end of each course.¹⁰ The grade point average is based on all the exams, but if a student attends the same type of exam multiple times we use the latest exam taken. From the municipalities, we have weekly information on participation in on-the-job training (internships and jobs with a wage subsidy) and of income transfers received. Based on this data, we construct a monthly panel data set covering the months from the time of arrival till 2019.

To measure entry into regular employment, we use as a primary labor market measure a variable that counts each month the refugee has had more than 100 registered working hours

¹⁰ 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).

according to BFL or has received no unemployment benefit combined with a registered employer.¹¹ This variable is called “cumulative full-time employment”. Cumulative labor income is included as an outcome as it combines the measure of job quality and work intensity. 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 only available until 2018, however, and is therefore not observed for all refugees in the sample for 42 months. Because the majority of the refugees are in their twenties when they arrive, education is also a realistic alternative to employment. In concordance with the earlier stated hypotheses (Sections 1 and 2), early job training may affect educational enrolment directly (e.g., by providing incentives for skilled jobs) or indirectly through altered participation in language courses. We therefore supplement employment and income outcomes with information on education. The outcome horizon is too limited to observe completion for a large part of the group, and we therefore include information on both educational enrollment and completion at any level (less than 4% of the sample complete a formal education in the observation period). Educational enrollment is measured by receipt of public student grants, which are available from the upper secondary schooling level.¹² These formal educations are tuition free. A student grant is also available for students participating in further education for adults, some of which are tailored to immigrants, but they may require tuition fees.¹³ Information on educational completion is available by the date of completion from administrative education registries.

The data is merged with other administrative data containing information on demographic and individual characteristics, such as gender, age, number of children, date and reason for immigration, 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 and Haslam, 2005; Fazel et al., 2005; Peconga and Thøgersen, 2019). This might be a barrier to participation in language classes and the labor market. To proxy health problems, we merge administrative data on health care utilization such as general practitioner contacts, psychiatric care, and hospitalizations.

4.1 Descriptive statistics

¹¹ Based on the BFL data from Statistics Denmark and Dream data from the Ministry of Employment.

¹² The student grant in 2015 was around DKK 5,900 (approx. euros 790), www.su.dk, in Danish. The grant depends on parental income if the student is aged below 20 or is living with his or her parents.

¹³ The student grant for adults in 2015 varied from 60-100% of unemployment insurance level, DKK 17,918 (around 2,400 euro), www.ug.dk, in Danish.

Table 4.1 contains descriptive statistics for the main sample, measured one year after arrival. From this we see that less than 40% of the sample are women and that more than half the sample are married and have children at their time of arrival. Around 20% of the sample have family reunification status, implying that the rest have either asylum or subsidiary protection status. Around two-thirds are from Syria.

We also measure the use of publically financed health care in the year of arrival. Because there are no user fees for the use of primary and tertiary health care, we view the public costs of use of these services as rough indicators of their health. The numbers show that the annual costs of use of primary health care (general practice) are DKK 907, much lower for emergency and psychiatric care, and that 16% are hospitalized.

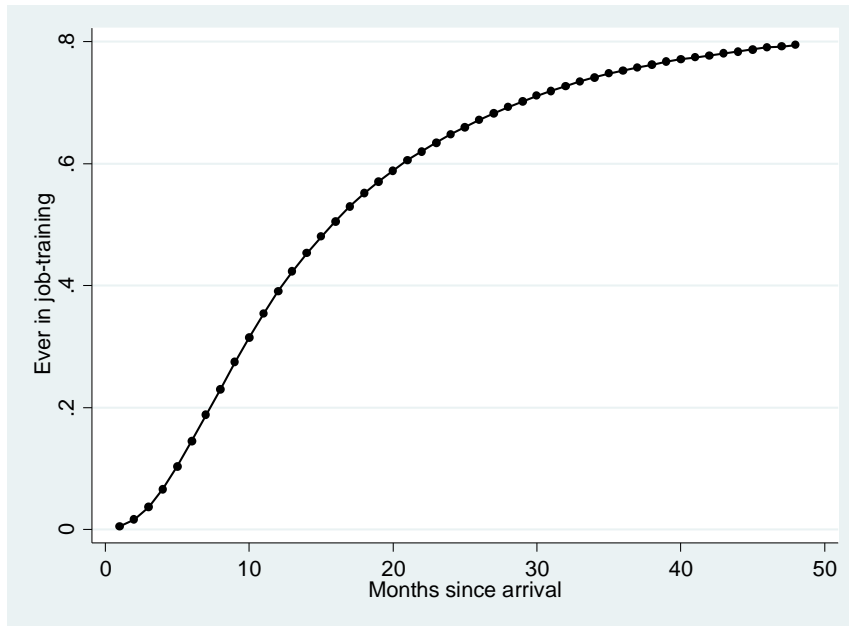
Table 4.1 Descriptive statistics measured 12 months after arrival.

	Mean
Women	0.37
Married	0.58
Maternity within first 12 months	0.11
Age at arrival	31.11
No children	0.47
1-3 children	0.43
Family-reunification stats	0.20
Danish course:	
Level 1	0.36
Level 2	0.53
Level 3	0.11
Health care utilization:	
General practitioner (costs, DKK)	906.85
Emergency care (costs, DKK)	22.47
Psychiatry (costs, DKK)	11.44
Hospitalized (days)	0.16
Country of origin:	
Eritrea	0.14
Somalia	0.05
Iran	0.05
Iraq	0.01
Syria	0.62
Year of arrival:	
2012	0.08
2013	0.12
2014	0.25
2015	0.46
2016	0.09
Observations	20,358

Notes: Characteristics at arrival for refugees and family reunified to refugees who arrived 2012-2016 at the age of 18-65.

Participation in early on-the-job training is measured within the first year of arrival. On-the-job training include subsidized employment as well as internships. Figure 4.1 shows the cumulative share of refugees who have participated in on-the-job training from the time of arrival. The share rises to around 40% within the first year and levels out during the third year at around 70%.

Figure 4.1 The cumulative share who have participated in on-the-job training, by month since arrival.



We look at three different outcomes of the language courses: hours of instruction attendance, exam attendance, and grade point averages at the exam. Figure 4.2 shows the share who have participated in a language course exam at a given time since arrival. It increases after the second year from less than 20% to around 40% after 3 years. Exam attendance continues to increase to around 60% after four years.

Figure 4.2 The cumulative share who have taken a language course exam, by month since arrival

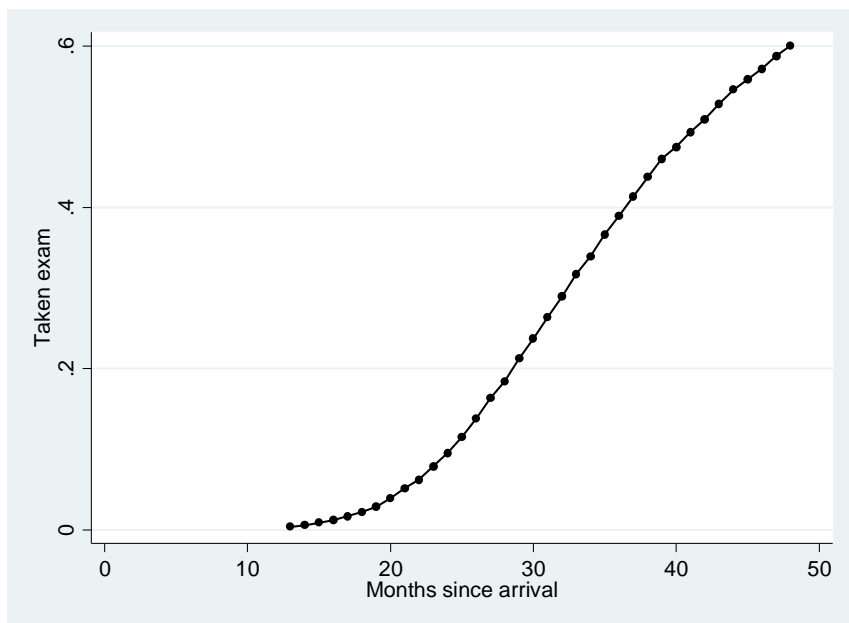


Table 4.2 below provides further summary statistics on language course and labor market outcomes at the end of the integration program, 24-48 months after their arrival. It shows that after three years each refugee has spent, on average, 867 hours in language school. After four years this number has increased to 960 hours, although 40% have already completed their exams after three years. The mean grade point averages (GPA) for those who attend the exam are also shown.¹⁴ The mean GPA decreases as more refugees finish their exam, indicating that the most skilled students take the exams sooner.

The labor market outcomes, shown in the middle of the table, reveal that 61% of language course participants gained employment during the first three years since their arrival. The cumulative sum of labor income since arrival is roughly DKK 36,500 (euros 4866) two years since arrival and it rises to just above DKK 146,000 (euros 19,500) after four years. Finally, at the bottom of the table two additional measures of labor market contact are presented. The monthly employment rate measures the share of refugees who have had a full-time job, and job training measures the share of refugees attending further job training during the given month. Together these two shares measure the overall percentage of refugees in contact with the labor market, showing a fairly stable contact share around 40% throughout the period with an increasing rate in formal employment and a decreasing rate in job training. Two years after arrival, around 18% are employed and after four years this number has increased to 32%.

Table 4.2 Descriptive statistics of language course and labor market outcomes measured 24-48 months after asylum was granted

	24 months	36 months	42 months	48 months
Sum of attended instruction time (hours)	733.12	866.68	885.57	960.01
Attended exam	0.10	0.39	0.51	0.60
GPA conditional on exam	6.12	5.48	5.29	5.19
Months employed since arrival	1.50	4.38	6.16	7.81
Sum of labor income since arrival (DKK)	36,492	95,084	126,765	146,123
Ever enrolled in education	0.07	0.20	0.25	0.29
Completed an education	0.01	0.02	0.03	0.03
Monthly employment rate	0.18	0.28	0.31	0.32
Job training during the month	0.23	0.14	0.11	0.09
Joint months in the labor market	0.41	0.42	0.42	0.41
Observations	20,358	20,358	20,358	15,962
Observations with GPA	1,428	5,756	7,491	6,891
Observations with income information	20,337	18,591	14,371	9,151

Note: The number of observations measured 48 months after arrival is considerably lower than the original sample size because the outcome cannot be measured for refugees arriving after September 2015. Likewise, labor income data are measured until December 2018 and therefore income can be measured four years later for refugees arriving before 2015 only. GPA is the grade point average across the different types of exams attended. If several exams of the same type are registered, results from the first exam are used.

¹⁴ The grades used are -3, 0, 2, 4, 7, 10, and 12. They correspond to the European ECTS system with grades from A-E, Fx, and F.

4.2 Gradual rollout

The percentage of refugees who receive on-the-job training within their first year in Denmark increases steadily during the period of observation. Figure 4.3 reveals that the rate is fairly stable around 20% up until 2014 and then increases to a level between 60% and 80% by the end of 2016. Nonetheless, the increase in early labor market contact is not the same for all municipalities. Figure 4.4 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 from below 20% to above 60%, whereas other municipalities have a fairly stable average share of early job training throughout the period.

Figure 4.3 Average rate of refugees who receive on-the-job training within the first year in Denmark – by year/month of arrival

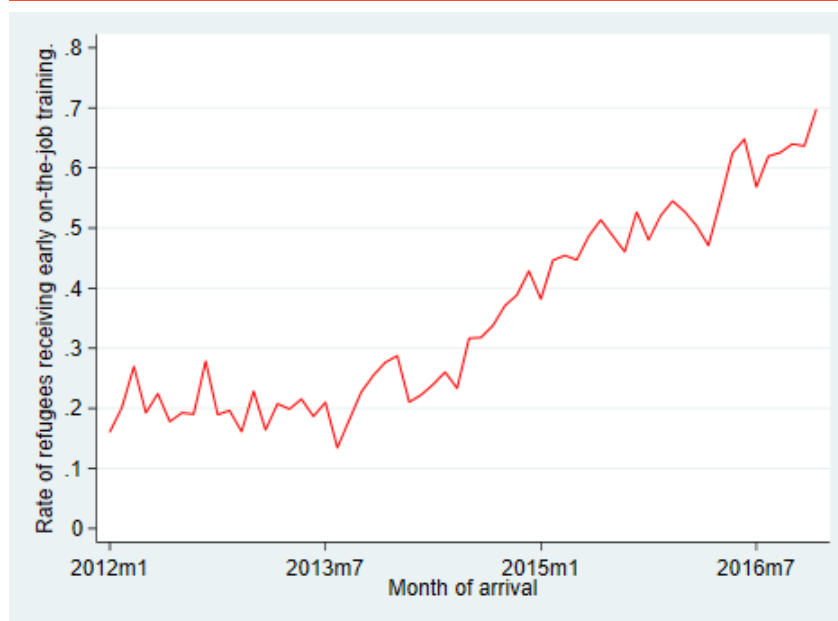
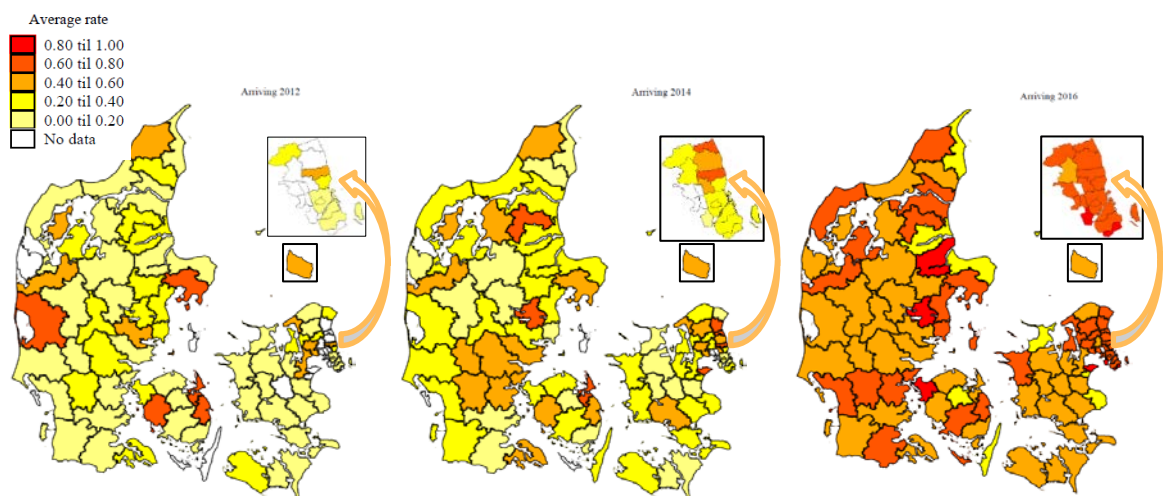


Figure 4.4 Average rate of newly-arrived refugees who received on-the-job training during their first year in Denmark – average in the municipality in year of arrival



5 Method

We employ an instrumental variable (IV) approach as identification strategy for identifying and estimating the effect of receiving on-the-job training within the first year of arrival on language course and labor market outcomes for Danish refugees. Therefore, our empirical model is specified as follows:

$$(1) T_{i1} = \alpha_2 + \eta Z_{i1} + \pi_2 X_{i0} + \theta_2 U_{ms} + \gamma_{2m} + \gamma_{2s} + u_{i1}$$

$$(2) Y_{it} = \alpha_1 + \delta T_{i1} + \pi_1 X_{i0} + \theta_1 U_{ms} + \gamma_{1m} + \gamma_{1s} + \epsilon_{it}$$

where Y is an outcome measured t months after arrival ($t=13, \dots, 48$), T_i is a dummy for being treated in the first year, and X are individual characteristics measured in the year of arrival (see Table 4.1). Z is an instrumental variable, and γ_m and γ_s are municipality- and semester-of-arrival fixed effects. The fixed effects wipe out common time trends and time invariant unobserved determinants of the outcomes in a given municipality. U is the local unemployment rate measured at the municipality level in the year of arrival, to control for the possibility that the instrument is related to the possibility of getting a job (see discussion below), which may affect later language course and labor market outcomes.

The instrumental variable is a measure of the local inclination to use early on-the-job training as part of the integration program. For refugee i arriving in a municipality in a given month, the instrument 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 prior to refugee i 's arrival. We use municipality-month pairs where at least 10 refugees arrived during the previous 20 months. The number 20 has been chosen to ensure a sufficient number of municipality-month pairs. As described in Section 2 and showed in Section 3, early job training has been used increasingly over time in most municipalities, and we utilize this gradual rollout to identify its effect. Since we observe actual on-the-job training participation we can measure local average treatment effects rather than intent-to-treat effects using the instrumental variables estimator¹⁵. The identifying variation henceforth relies on the different growth-rates between municipalities in their inclination to use early job training as measured by the instrument. By relying on this rollout design, we are able to rule out any correlation driven by fixed municipality characteristics or general correlated time varying trends in outcomes and use of job-training.

5.1 Validation of the instrument

The validity of the instrumental variable requires it to be independent of the refugees' characteristics (the independence condition) but also that it affects the probability of participating in early on-the-job training (the first-stage condition). We argued in Section 2 that immigrants with asylum with no family members already living in the country are randomly allocated across municipalities due to the dispersal policy (see also Edin et al., 2003; 2004; Damm, 2009; Damm, 2014; Azlor et al., 2020).¹⁶ The plausibility of the independence assumption for the entire sample is examined using a balance test reported in Table 5.1.

¹⁵ Rollout designs have been used extensively to estimate the impact of various US welfare programs (e.g. Ludwig and Miller 2007; Almond, Chay and Greenstone 2007; Bailey and Goodman-Bacon 2015; Hoynes, Schanzenbach and Almond 2016; Johnson and Jackson 2017). In the absence of data on participation in the programs, most of these studies estimate intent-to-treat effects.

¹⁶ Immigrants with asylum based on reunification with refugees who are still in the integration period are also randomly allocated across municipalities, unless their application for reunification is depending on settlement location.

Table 5.1 Balance test: Regression of instrument on refugee characteristics measured one year after arrival.

	Incl. municipality FE and time trends		Without municipality FE		Without time trends	
	Coefficient	Std. error	Coefficient	Std. error	Coefficient	Std. error
Woman	-0.0018	(0.0024)	-0.0016	(0.0040)	-0.0002	(0.0028)
Woman with children	0.0037	(0.0031)	-0.0010	(0.0053)	0.0155***	(0.0037)
Married	0.0036*	(0.0016)	0.0039	(0.0027)	-0.0043*	(0.0019)
Maternity within first 12 months	0.0012	(0.0025)	0.0121**	(0.0042)	0.0024	(0.0030)
Age 18-25	-0.0033	(0.0055)	-0.0146	(0.0093)	0.0056	(0.0066)
Age 25-35	-0.0031	(0.0054)	-0.0114	(0.0092)	0.0036	(0.0065)
Age 35-45	-0.0026	(0.0056)	-0.0097	(0.0094)	0.0035	(0.0066)
Age 45-55	-0.0059	(0.0059)	-0.0105	(0.0100)	-0.0014	(0.0070)
Age 55 or more	0.0000	(.)	0.0000	(.)	0.0000	(.)
No children	0.0037	(0.0028)	0.0119*	(0.0047)	0.0105**	(0.0033)
1-3 children	0.0001	(0.0023)	0.0025	(0.0039)	-0.0001	(0.0027)
Family reunification status	-0.0005	(0.0020)	-0.0022	(0.0034)	0.0217***	(0.0024)
Country of origin:						
Eritrea	-0.0026	(0.0029)	-0.0048	(0.0048)	0.0378***	(0.0033)
Somalia	-0.0051	(0.0035)	-0.0087	(0.0058)	-0.0181***	(0.0041)
Iran	-0.0129***	(0.0035)	-0.0007	(0.0059)	-0.0069+	(0.0042)
Iraq	-0.0140+	(0.0072)	0.0034	(0.0121)	-0.0197*	(0.0086)
Syria	0.0033	(0.0021)	0.0138***	(0.0036)	0.0210***	(0.0025)
Health care utilization 1st Year:						
General practitioner (1000 DKK)	-0.0000+	(0.0000)	0.0000	(0.0000)	-0.0000	(0.0000)
Emergency care (1000 DKK)	-0.0000**	(0.0000)	-0.0000+	(0.0000)	-0.0000*	(0.0000)
Psychiatry (1000 DKK)	0.0000	(0.0000)	0.0000	(0.0000)	0.0000+	(0.0000)
Hospitalized (days)	-0.0007	(0.0006)	-0.0013	(0.0011)	-0.0033***	(0.0008)
Course module (within level) at arrival [†] :						
Level 1:						
Module 1	0.0915	(0.0645)	0.0381	(0.1088)	0.0412	(0.0769)
Module 2	0.0914	(0.0648)	0.0674	(0.1092)	0.0458	(0.0772)
Module 3	0.0763	(0.0660)	0.0623	(0.1114)	0.0227	(0.0787)
Module 4	0.0358	(0.0701)	0.0166	(0.1182)	-0.0440	(0.0836)
Module 5	0.0361	(0.0912)	-0.0558	(0.1537)		
Module 6						
Level 2:						
Module 1	0.0920	(0.0645)	0.0462	(0.1088)	0.0416	(0.0769)
Module 2	0.1016	(0.0646)	0.0689	(0.1090)	0.0603	(0.0770)
Module 3	0.1021	(0.0650)	0.0577	(0.1096)	0.0570	(0.0775)
Module 4	0.0856	(0.0668)	0.0482	(0.1129)	0.0333	(0.0797)
Module 5	0.0613	(0.0720)	0.0691	(0.1215)	0.0324	(0.0859)
Module 6	0.1047	(0.1114)	0.0986	(0.1883)	-0.0041	(0.1327)
Module 6	0.0920	(0.0645)	0.0396	(0.1088)	0.0328	(0.0769)
Level 3:						
Module 1	0.0857	(0.0645)	0.0814	(0.1094)	0.0502	(0.0774)
Module 2	0.0905	(0.0649)	0.0745	(0.1104)	0.0514	(0.0781)
Module 3	0.0992	(0.0655)	0.1149	(0.1153)	0.0673	(0.0816)
Module 4	0.1200+	(0.0684)			-0.0927	(0.1087)
Module 5						
Module 6						
Local unemployment rate	-0.0406***	(0.0029)	-0.0345***	(0.0012)	-0.0866***	(0.0018)
Observations	20303		20303		20303	
Adjusted R2	0.742		0.256		0.633	
Number of covariates	138		44		130	

Notes: OLS estimates with controls for municipality dummies and dummies for semester of arrival. † The three course levels are sub-divided into six sequential modules. Standard errors in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The presented coefficients are obtained from a regression of the instrument on characteristics of the refugees and the local unemployment rate at the time of arrival as well as municipality dummies and dummies for each arrival semester.

Table 5.1 shows that most of the individual characteristics that are likely to be strongly related to job finding abilities are unrelated to the instrument, once we have controlled for municipality and time fixed effects in column 1. Thus, neither demographics (gender, age, and children) nor use of most types of health care have a significant relationship to the instrument. Four characteristics are significantly related to the instrument on a 5% level, but given that the model contains 39 estimated coefficients this could be due to chance. In general, the number of significant correlations do not change considerably when the municipality fixed effects are excluded, whereas a number of individual characteristics become significant when the time trend measured as half-year (semester) fixed effects is excluded. This is probably due to changes in the composition of the refugee group during the period and it shows how important it is to correct for time trends.

It is potentially a larger concern that the local unemployment rate is strongly related to the instrument in all three specifications: The last row shows that a high unemployment rate is associated with a lower use of early job training. This may not only violate the independence assumption of the instrument but may also violate the exclusion assumption (Imbens and Angrist 1994). The exclusion restriction requires that being assigned to a municipality with a historical high level of on-the-job training among refugees has an impact only on outcomes that operate through the probability of being assigned to on-the-job training. In our case, we could imagine that municipalities with better general labor market conditions find it easier to find on-the-job training positions. If the better labor market conditions also affect future job and language acquisition, then the exclusion restriction is violated. To assess how important this is, we conduct a robustness analysis, where we include total time in the labor market during the first year, either in on-the-job training or regular employment, as the treatment. Then, the exclusion restriction is only violated if the fact that a refugee is assigned to a municipality with a high level of on-the-job training has an impact on outcomes, independently of the refugee's own labor market behavior, which seems more implausible. The exclusion restriction is also violated if the language course in municipalities with a higher use of early on-the-job training is of a different quality than in other municipalities. This is examined by inclusion of language course provider dummies in a robustness analysis at the end of the paper. As we shall see, both of these analyses show that the results are robust and that the correlation with the local unemployment rate therefore does not seem to be a concern.

Table 5.2 First stage: The effect of the instrument on the probability of receiving on-the-job training in the first year

	Early job training
Instrument [#]	0.489***
	(0.0336)
Observations ^{##}	20,303
Cragg-Donald Wald F test	212.3

Notes: OLS estimates with controls for background characteristics, municipality dummies and dummies for semester of arrival.

[#] The instrument is calculated as the degree of job training in the municipality of arrival in the 20 months prior to arrival.

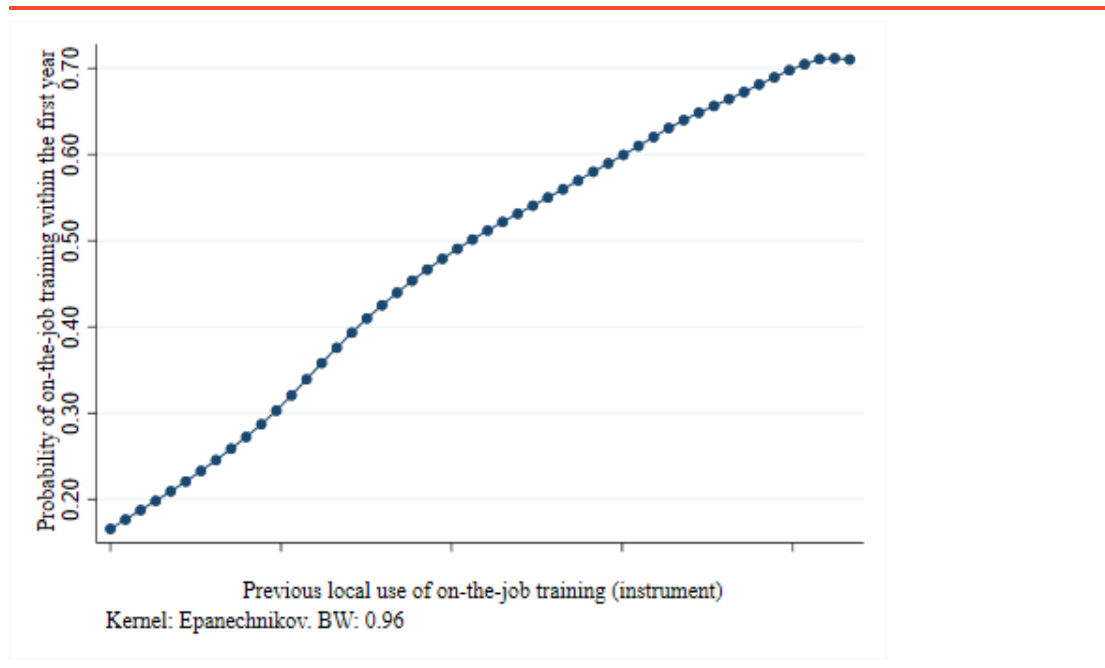
^{##} The regression is based on the sample of refugees who can be followed for at least 42 months. P-value in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 5.2 tests whether the instrument affects the treatment (the first-stage condition). The table contains the first-stage coefficients on the instrument, i.e. the coefficient η in equation (1). It shows that the instrument is strongly correlated with the likelihood of participating in early job training.¹⁷ We also report the Cragg-Donald Wald F statistic for weak identification. It is very large and therefore there is no indication that the instrument is weak (Stock and Yogo 2005).

To be able to interpret the IV estimates as local average treatment effects (LATE), an assumption of monotonicity of the instrument is also required (Imbens and Angrist 1994). The assumption rules out the presence of defiers. As the first-stage coefficient is positive, this assumption requires that the likelihood of receiving early job training is not lower for an individual when 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. Figure 5.1 plots a local linear regression of the probability of participating in early on-the-job training in period t on the rate of use of early on-the-job training in the municipality in the previous period (i.e., the instrument). The function shows a highly monotonic relationship between the instrument and the refugees' probability of receiving on-the-job training, which indicate that the first stage is not merely driven by outliers, which again supports the assumption that there are no defiers.

Figure 5.1 Local linear regression of the probability of early on-the-job training on the instrument



¹⁷ The coefficient increases by around 50 percent when municipality dummies and annual local unemployment rates are excluded, which suggests that municipality-specific factors play a sizeable role for the correlation between the instrument and treatment.

6 Results

In this section, we present the estimates of the effect of early on-the-job training on 1) language acquisition and 2) labor market outcomes. We use exam attendance and grade point average at the final language school exam as the main language course outcome (section 6.1). We use cumulative number of months in ordinary employment and cumulative income as the main labor market measures (section 6.2). As argued earlier, early job training may also affect educational enrolment – either directly (e.g., by providing incentives for skilled jobs) or indirectly through altered participation in language courses. We therefore supplement the analyses with two outcome measures on education: ever being enrolled in an education in the Danish educational system and having completed such an education.

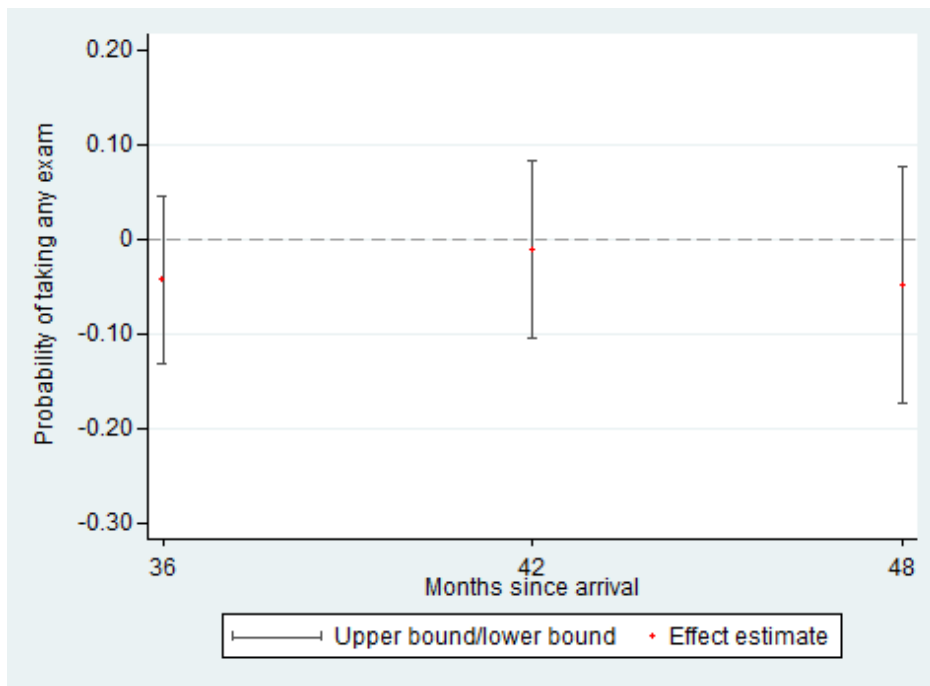
We hypothesize that the effect on language acquisition runs through time spent in either school or the labor market and therefore we consider two potential mechanisms: language school participation measured as the sum of attended instruction time, and contact with the labor market before taking the exam measured by the sum of months beyond the first year spent in job training or in paid employment (section 6.3).

For ease of interpretation we present the results from estimates of δ in equation (2) across the multiple outcomes and time periods in a series of graphs showing the IV estimates 18-48 months after arrival in the country. The estimates are from models including a full set of controls, i.e., the characteristics described in Table 1 as well as municipality fixed effects and dummies for each semester of arrival.

6.1 Language acquisition

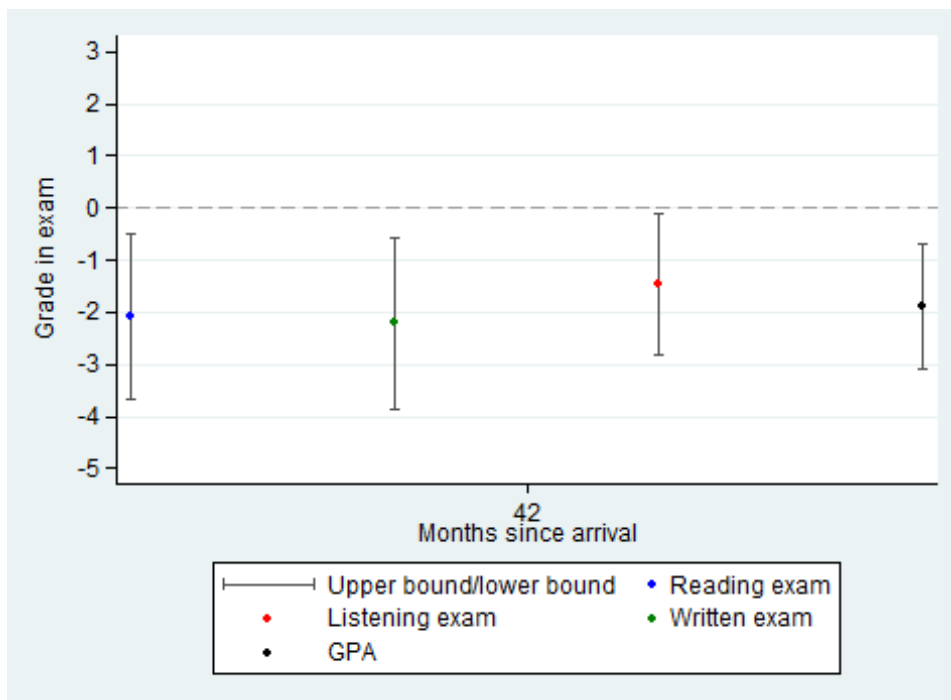
Figure 6.1 presents results on exam attendance measured 36, 42 and 48 months after arrival, and we find that the attendance is not affected by participation in early on-the-job training. This result enables us to evaluate the effects at the average grades without accounting for a selected sample.

Figure 6.1 The effect on exam attendance three-four years after arrival.



Notes: Each dot represents a Separate IV estimate from model (2) of the effect of early on-the-job training, with control for background characteristics, municipality dummies, and dummies for semester of arrival. Bars are 95% confidence intervals.

Figure 6.2 The effect on subject grades and GPA measured 42 months after arrival.



Notes: Each dot represents a Separate IV estimate from model (2) of the effect of early on-the-job training, with control for background characteristics, municipality dummies, and dummies for semester of arrival. Bars are 95% confidence intervals.

Figure 6.2 shows the effects on the results of the final exams of the language course. It shows the subject grade for each type of subject: reading comprehension, listening and writing, as well as the average across types of subjects. For all types of subjects we find that early job training lowers the average grade by 1.5 to 2 grades. The effect corresponds to almost a 40% reduction or about 75% of a standard deviation.

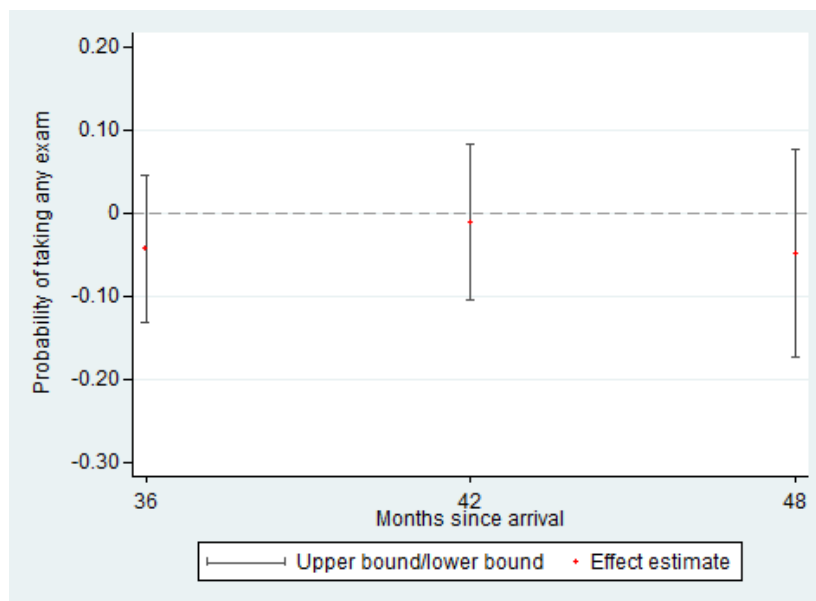
6.2 Labor market outcomes

In Figures 6.3-6.4 below, we present the effects of participating in early on-the-job training on employment and labor income. As a supplementary outcome, we also examine the effect on attending and completing ordinary education (Figure 6.5-6.6).

Looking at the effects on the cumulative number of months employed in Figure 6.3, we find a positive –although significant only after 18 months–effect during the second year in the country. However, this effect disappears after 30 months, indicating that those who do not enter early job training catch up with those who do.

The results therefore confirm the results from previous studies that have found on-the-job training to be an effective strategy for non-western immigrants in the short run (Clausen et al., 2009; Heinesen et al., 2011; Arendt, 2020a), but adds to these findings by showing that, at least in the current setting, the effects are temporary.

Figure 6.3 The effect on cumulative months in full-time employment.

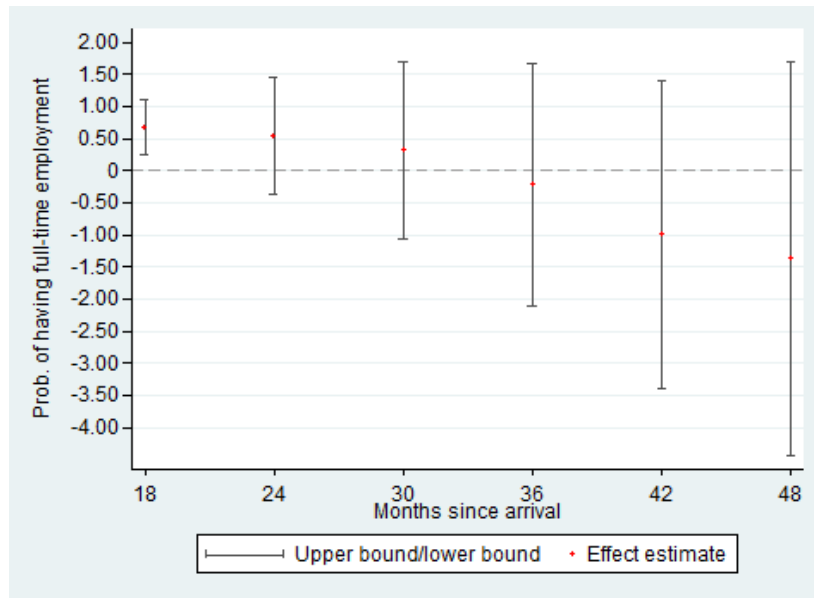


Notes: Each dot represents a Separate IV estimate from model (2) of the effect of early on-the-job training, with control for background characteristics, municipality dummies, and dummies for semester of arrival. Bars are 95% confidence intervals.

Looking at the effects on labor income in Figure 6.4, the effects mirror those on employment: there is an increased income level after 18 months, where they roughly have earned DKK 20,000 (2,700 euros) more since arrival, but as with the employment effect it disappears after 24 months. There is therefore no indication of a spillover from the excess employment rate

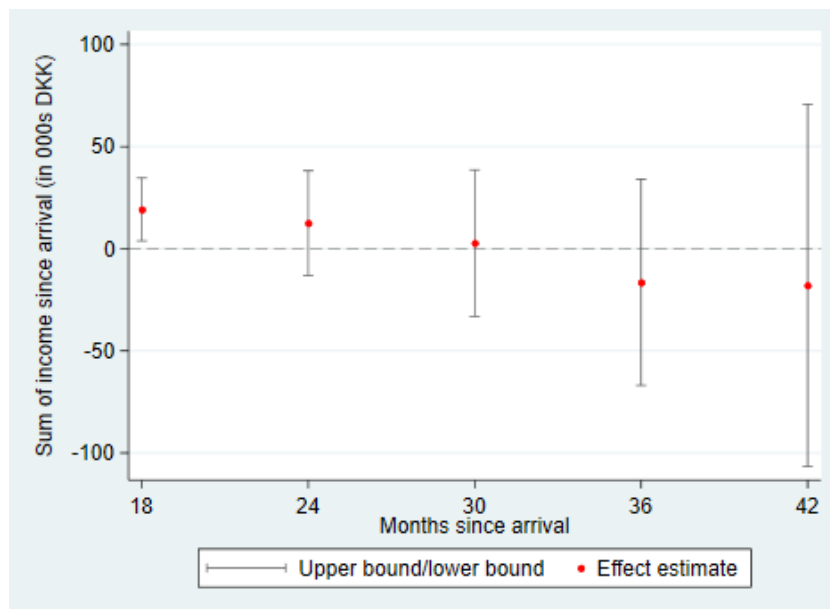
during the second year into a higher cumulative income beyond the first 18 months. This confirms the previous result that the labor market effects are rather short-lived. Nonetheless, we do not find any negative employment or income effect.

Figure 6.4 The effect on cumulative months in full-time employment



Notes: Each dot represents a Separate IV estimate from model (2) of the effect of early on-the-job training, with control for background characteristics, municipality dummies, and dummies for semester of arrival. Bars are 95% confidence intervals.

Figure 6.5 The effect on the cumulative labor income since arrival

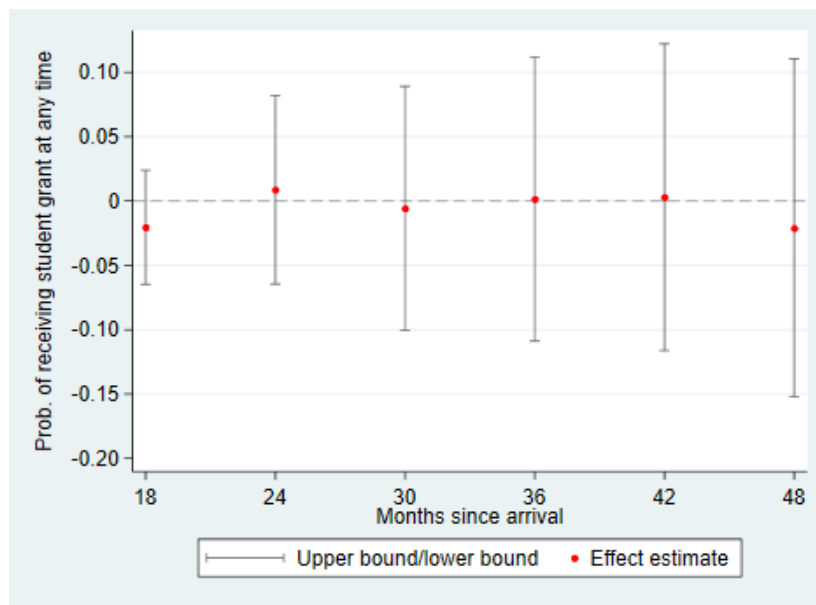


Notes: Each dot represents a Separate IV estimate from model (2) of the effect of early on-the-job training, with control for background characteristics, municipality dummies, and dummies for semester of arrival. Bars are 95% confidence intervals.

Figure 6.6 shows that there is no effect on the probability of enrollment in ordinary education, whereas Figure 6.7 shows a short-run negative effect on the probability of completing an education in the Danish school system. The effect is significant only 24-36 months after arrival,

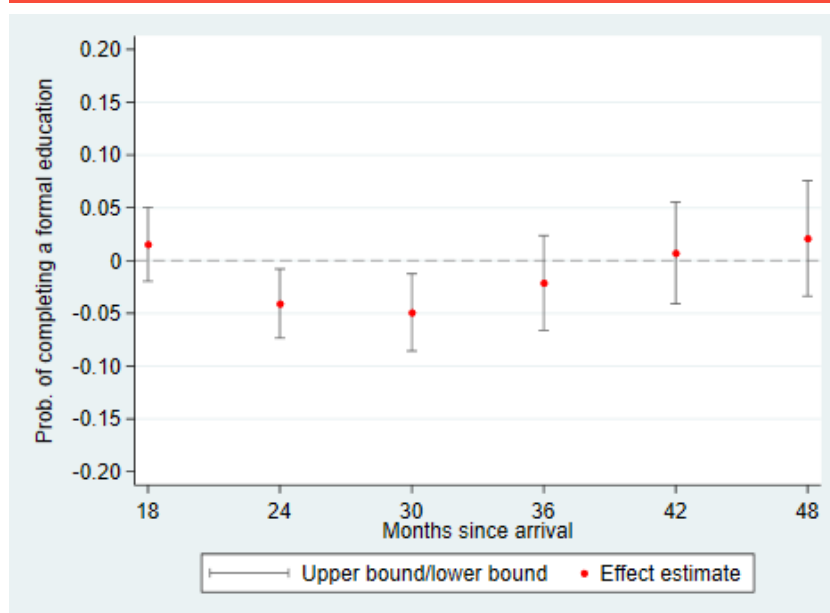
which indicates that it is based on educational programs of short duration, e.g., preparatory courses.¹⁸ This also explains why we see no effect on participation, since our measure is based on student grants available mainly from the upper secondary level.

Figure 6.6 The effect on ever being enrolled in the ordinary education system (measured as receiving student grant).



Notes: Each dot represents a Separate IV estimate from model (2) of the effect of early on-the-job training, with control for background characteristics, municipality dummies, and dummies for semester of arrival. Bars are 95% confidence intervals.

Figure 6.7 The effect on the completing an education within the Danish school system.



Notes: Each dot represents a Separate IV estimate from model (2) of the effect of early on-the-job training, with control for background characteristics, municipality dummies, and dummies for semester of arrival. Bars are 95% confidence intervals.

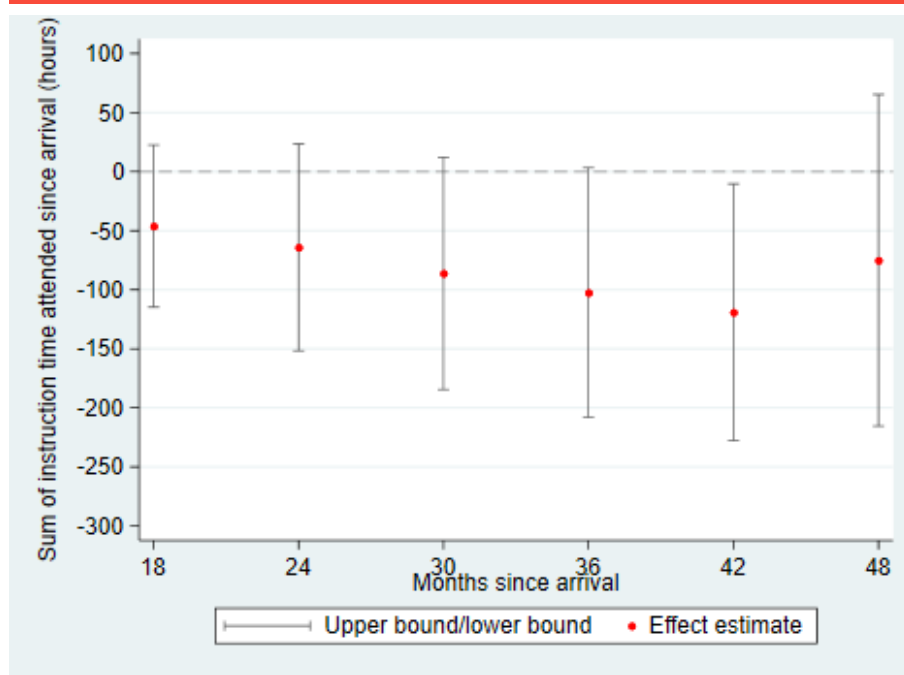
¹⁸ A frequency table of the type of education confirms this picture with 70% of refugees completing a formal education within four years after arrival.

6.3 Mechanism

In this section we take a look at some of the potential indirect mechanisms that may be driving the negative effect on language acquisition and the temporary effect on labor market outcomes: time in language courses and total time on the labor market (either in job training or in regular employment).

Figure 6.8 presents the results concerning the hours of instruction attendance.¹⁹ The refugees who receive on-the-job training early in the integration period attend substantially fewer Danish course lectures throughout the remaining period of the three-year integration program. The effect is not significant the first couple of years, but it increases in absolute size throughout the period and reaches a significant effect of 120 fewer attended hours among job training participants 42 months after arrival. This corresponds roughly to a 15% reduction in the cumulative hours of attended instruction time.

Figure 6.8 The effect on cumulative hours of attended instruction time

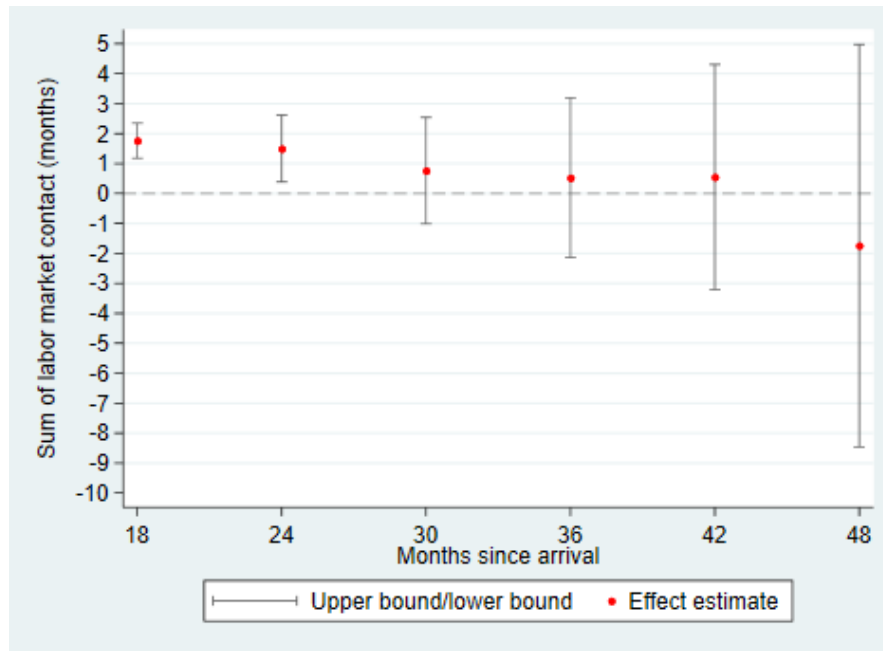


Notes: Each dot represents a Separate IV estimate from model (2) of the effect of early on-the-job training, with control for background characteristics, municipality dummies, and semester of arrival dummies. Bars are 95% confidence intervals.

Next, in Figure 6.9 we investigate whether the reduced time spent in school has been spent on the labor market, either through more job training or through ordinary employment. Among refugees attending early job training we see a clear tendency of increased labor market participation. Early job training mainly seems to affect later contact with the labor market immediately after the first year, which might partly be explained by the job training started within the first year not ending until the second year after arrival, and partly by the temporary effect on ordinary employment.

¹⁹ The measure is calculated as the hours of planned instruction time minus the hours of absence.

Figure 6.9 The effect on cumulative time spent on the labor market (months).



Notes: Each dot represents a Separate IV estimate from model (2) of the effect of early on-the-job training, with control for background characteristics, municipality dummies, and semester of arrival dummies. Bars are 95% confidence intervals.

6.4 Heterogeneity

In this section we examine potential effect heterogeneity by examining whether effects differ by gender and language course level. Previous analyses have shown that male and female refugees often react differently to programs and incentives that encourage or support refugees' labor market entry (e.g., Andersen et al., 2019; Arendt, 2020a; Arendt, 2020b), and we thus add to these findings. The examination by language course level provides an assessment of whether the impact of early on-the-job training on language acquisition differs between skill levels. As mentioned in Section 3, refugees are divided into three groups based on their skill levels on arrival when they enroll in the language course.

Table 6.1 presents the results measured after 24 and 42 months by language course level (columns 2-4) and by gender (columns 5-6). We have re-examined the strength of the instrument for each subgroup and, as the F-test of weak instrument reveals, the instrument is not very strong among refugees on course level 3. This prevents us from estimating the causal relationship between early on-the-job training and the output of interest for refugees at the highest skill level. We make another note of caution before presenting the results: Even though size and significance of the effects may differ across groups, these differences are never significantly different across these groups.

Among refugees on language course level 2, we find the same significant effects of early on-the-job training on language acquisition (row 2) and labor market outcomes (rows 3-6) as in the overall sample. However, early on-the-job training also seems to decrease the probability of taking a language course exam (row 1). The effect on the grade point average can therefore reflect an effect on exam achievement as well as a selection effect through attendance.

By contrast, there are no significant effects on language acquisition or labor market outcomes for language level 1 participants. However, there are no effects on instruction attendance either, so the results are consistent with our hypothesis that potential effects on language acquisition running through reduced time for language course participation. Instead we observe an increase in the probability of enrollment in ordinary education after 42 months (column 2). As mentioned above, simple cross-tabulations show that 70% of the observed completed educations in this group are at the lower secondary level, which mainly consists of preparatory courses. These are likely needed for the refugees to be able to get ordinary employment or pursue further education.

Splitting the analyses by gender reveals that whereas the negative effect on language acquisition is much larger (in absolute value) and only significant for male refugees, the temporary positive employment effect is twice the size and only significant for female refugees. In concordance with these findings and our hypotheses of indirect effects, early on-the-job training only has a significant (negative) effect on language instruction attendance for men and a significant (positive) effect on time in the labor market for females during the first three years in the host country.

Table 6.1 The effect of early job training or employment), by gender and language course level

	All	Level 1	Level 2	Level 3	Men	Women	Month
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1. Attend exam	0.0101 (0.0723)	0.140 (0.108)	-0.202+ (0.105)	0.193 (0.608)	-0.0456 (0.0841)	0.0712 (0.131)	42
2. GPA	-1.893** (0.613)	-1.377 (1.304)	-1.710* (0.706)	-6.447 (5.748)	-2.283** (0.704)	-0.603 (1.096)	42
3. Cumulative full-time employment (months)	0.546 (0.462)	0.475 (0.629)	0.905 (0.657)	-3.006 (5.376)	0.448 (0.623)	0.987+ (0.529)	24
	-0.997 (1.226)	0.107 (1.703)	-1.148 (1.745)	-11.21 (13.79)	-1.602 (1.612)	0.771 (1.530)	42
4. Cumulative labor income (DKK)	12570.5 (13039.1)	4821.4 (16405.5)	30549.8+ (18304.3)	-89038.0 (171144)	10743.4 (18002.6)	24200.5+ (12497.2)	24
	-17947.3 (45294.7)	-13527.0 (58508.8)	43455.4 (61787.0)	-968920 (1228479)	-50047.8 (56185.2)	10472.4 (62565.5)	42
5. Ever enrolled in formal education	0.00879 (0.0374)	0.0304 (0.0200)	-0.0209 (0.0576)	0.222 (0.536)	0.0397 (0.0459)	-0.0781 (0.0635)	24
	0.00299 (0.0608)	0.153* (0.0607)	-0.101 (0.0958)	-0.289 (0.641)	-0.00656 (0.0688)	0.0118 (0.114)	42
	-0.0407* (0.0167)	-0.0164 (0.0223)	-0.0275 (0.0243)	-0.322 (0.247)	-0.0586** (0.0211)	-0.0143 (0.0265)	24
6. Completed formal education	0.00716 (0.0246)	-0.00954 (0.0292)	0.0381 (0.0361)	-0.0852 (0.275)	-0.0113 (0.0301)	0.0307 (0.0395)	42
7. Cumulative in-struction attendance (hours)	-64.07 (44.68)	53.47 (70.11)	-152.4* (62.28)	-811.6 (608.4)	-79.47 (52.29)	-85.57 (80.09)	24
	-119.1* (55.47)	-17.58 (90.22)	-212.9** (75.14)	-790.5 (596.0)	-141.1* (64.54)	-116.7 (100.6)	42
8. Cumulative labor market exposure (months)	1.495** (0.567)	1.499+ (0.835)	1.506* (0.766)	-7.657 (44.11)	0.843 (0.720)	2.783** (0.862)	24
	0.550 (1.915)	1.914 (2.188)	0.426 (3.182)	-33.69 (62.10)	-1.393 (2.522)	4.517+ (2.607)	42
All:							
Observations	20283	7302	10849	2132	12752	7531	
F test (1 st stage)	204.4	91.52	101.6	2.467	134.4	76.61	42
With exam:							
Observations	10381	2632	6351	1398	7002	3379	
F test (1 st stage)	100.9	22.51	68.12	2.19	76.48	31.84	42

Notes: Each column represents a separate IV estimate from model (2) of the effect of early on-the-job training, with control for background characteristics, municipality dummies, and semester of arrival dummies. Standard errors in parentheses. + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001.

6.5 Robustness analysis

In this section we address the concern that the estimates may be biased if the municipalities with a higher use of past on-the-job training are special. In particular, we address whether the results can be explained by scenarios where municipalities with a higher use of on-the-job

training have better employment opportunities or provide language courses of a different quality, e.g., if resources spent on on-the-job training crowd out resources spent on language schools.

Accounting for differences in early employment opportunities

Section 3 documented that the instrumental variable is related to the local unemployment rate, even when controlling for individual characteristics, time trends, and municipality fixed effects. This may violate the exclusion restriction, if arrival in a municipality with a low unemployment rate has lasting effects on future employment and language course outcomes. The key to the problem is that the causal pathway may operate through employment, instead of through on-the-job training. We therefore examine whether this is the case by examining the effect of early exposure to the labor market through either early on-the-job training or early employment. When employment is part of the treatment, the exclusion restriction is not violated even if low unemployment at time of arrival affects later employment chances.

Table 6.2 The effect of early labor market exposure (job training or employment) on language course outcomes

	Months since arrival					
	18	24	30	36	42	48
1. Attend exam	0.004 (0.004)	0.023** (0.008)	-0.011 (0.011)	-0.002 (0.012)	0.002 (0.013)	-0.002 (0.017)
2. GPA#				-0.401*** (0.121)	-0.303** (0.099)	-0.340** (0.107)
3. Cumulative full-time employment (months)	0.120*** (0.035)	0.096 (0.079)	0.057 (0.123)	-0.038 (0.172)	-0.177 (0.222)	-0.259 (0.299)
4. Cumulative labor income	3415.6** (1268.6)	2229.5 (2233.5)	493.6 (3235.6)	-3084.4 (4912.2)	-3504.3 (8963.5)	-11896.0 (28.923.4)
5. Ever enrolled in formal education	-0.004 (0.004)	0.002 (0.007)	-0.001 (0.009)	0.000 (0.010)	0.001 (0.011)	-0.004 (0.013)
6. Completed formal education	0.003 (0.004)	0.005 (0.004)	0.007+ (0.004)	0.002 (0.004)	-0.002 (0.004)	0.001 (0.005)
7. Cumulative instruction attendance	-8.227 (6.154)	-11.39 (7.801)	-15.31+ (8.789)	-18.71+ (9.441)	-21.18* (9.716)	-14.48 (13.68)
8. Cumulative labor market contact (months)	0.497*** (0.021)	0.797*** (0.040)	0.947*** (0.063)	1.113*** (0.010)	1.357*** (0.147)	1.389*** (0.238)
Observations	20,294	20,283	20,273	20,275	20,303	18,335
Observations with exam				7927	10,381	10,866
F test (1 st stage)	150.1	139.6	130.8	134.6	122.0	107.3

Notes: Separate IV estimates from model (2) of the effect of early labor market exposure (job training or employment) with control for background characteristics, municipality dummies, and semester of arrival dummies. Standard errors in parentheses + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. # We present results after 36 months only for the GPA outcomes, due to an insufficient number of observations.

We use the same sample and methodology as above and present the results in Table 6.2. The results confirm the previous results: Early exposure to the Danish labor market, now measured by either job training or ordinary employment, affects the language acquisition negatively, has only short-term positive employment effects, and time spent in the labor market substitutes away time spent in language courses.

Accounting for differences in quality of language course provider

As mentioned in the method section, it is possible that municipalities with excess tendency to use early job training might provide language courses of a different quality than other municipalities. If this is the case, then the exclusion restriction may also be violated. To assess the robustness to language school quality, we include language course provider dummies in the second robustness analysis, but otherwise use the same methodology as above²⁰. There are around 60 different providers during the period.

Table 6.3 The effect of early job training on language course and labor market outcomes – including language school dummies

	Months since arrival					
	18	24	30	36	42	48
1. Attend exam	0.0155 (0.0238)	0.136** (0.0467)	-0.0683 (0.0636)	-0.000184 (0.0721)	0.00809 (0.0738)	0.00180 (0.0908)
2. GPA				-2.107** (0.762)	-1.619* (0.634)	-1.966** (0.697)
3. Cumulative full-time employment (months)	0.660** (0.234)	0.448 (0.488)	0.207 (0.734)	-0.534 (0.998)	-1.274 (1.256)	-1.535 (1.600)
4. Cumulative labor income (DKK)	19476.2* (8360.0)	8961.2 (13714.4)	-869.5 (19090.6)	-21071.1 (26593.1)	-15984.7 (45919.2)	2608.3 (177631.1)
5. Ever enrolled in education	-0.0254 (0.0243)	0.00379 (0.0395)	0.0000250 (0.0504)	0.000871 (0.0579)	-0.0127 (0.0618)	-0.0257 (0.0683)
6. Completed formal education	0.0169 (0.0179)	-0.0396* (0.0169)	-0.0515** (0.0186)	-0.0226 (0.0220)	0.00242 (0.0233)	0.00400 (0.0268)
7. Cumulative instruction attendance (hours)	-84.21* (37.43)	-137.4** (47.39)	-179.8*** (53.02)	-182.0** (55.99)	-179.3** (56.80)	-141.3+ (73.26)
8. Cumulative labor market contact (months)	1.497*** (0.319)	0.827 (0.609)	0.191 (0.949)	-0.369 (1.413)	0.0962 (1.958)	-2.205 (3.477)
Observations	20,294	20,283	20,273	20,275	20,303	18,335
Observations with exam				7926	10381	9580
F test (1 st stage)	184.3	183.5	194.8	198.4	203.2	133.9

Notes: Separate IV estimates from model (2) of the effect of early labor market exposure (job training or employment) with control for background characteristics, municipality dummies, and semester of arrival dummies. Standard errors in parentheses * $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. # We present results after 36 months only for the GPA outcomes, due to an insufficient number of observations.

The results are shown in Table 6.3. In general, the results from the main model are again confirmed. Hence, early job training leads to lower language course attendance and a lower grade point average. Likewise, we find positive employment effects after 18 months in the country, which disappear after two to three years, and a negative effect on completing an education within the first couple of years.

Accounting for selection in arrival

In the last robustness analysis we address the possible problem that the municipalities are able to choose certain types of refugees and vice versa, and thereby distress the quasi-experimental setting (Azlor et al., 2020). Following the strategy of Azlor et al., 2020 we exclude all refugees

²⁰ In order not to eliminate too many degrees of freedom by including another set of dummy variables in the model, we estimate this version without dummies for semester of arrival and thus for year of arrival only.

arriving before at least 10 municipalities have filled their quotas in order to reduce the municipalities' possibility of influencing the settlement decision.

As Table 6.4 shows, the main result that early job training leads to a lower grade point average is confirmed, as are the short-run positive employment effects. For the remaining results, the size and magnitude of the coefficients are the same as the main model for most of the outcomes, but the reduced sample size means that most of the coefficients remain insignificant.

Table 6.4 The effect of early job training on language course and labor market outcomes among refugees arriving each year after at least 10 municipalities have filled their quotas.

	Months since arrival					
	18	24	30	36	42	48
1. Attend exam	0.0406 (0.0285)	0.179** (0.0586)	0.0150 (0.0787)	0.0554 (0.0908)	0.0803 (0.0940)	-0.0115 (0.138)
2. GPA				-2.490** (0.915)	-1.880** (0.712)	-2.046* (0.936)
3. Cumulative full-time employment (months)	0.557* (0.272)	0.511 (0.580)	0.241 (0.873)	-0.510 (1.210)	-1.444 (1.557)	-2.196 (1.924)
4. Cumulative labor income (DKK)	14929.4 (10060.6)	5187.5 (16494.2)	-7895.8 (22913.6)	-30977.8 (31064.2)	-86098.4 (165017.7)	-67590.8 (209699.3)
5. Ever enrolled in education	0.000240 (0.0283)	0.0657 (0.0473)	0.00465 (0.0613)	0.0329 (0.0713)	-0.00345 (0.0781)	0.00820 (0.0823)
6. Completed formal education	-0.00606 (0.0206)	-0.0139 (0.0224)	-0.0228 (0.0260)	-0.00973 (0.0252)	0.0329 (0.0300)	0.0238 (0.0316)
7. Cumulative instruction attendance (hours)	1.189 (45.52)	-18.67 (58.80)	-65.41 (65.81)	-74.62 (70.98)	-107.1 (73.80)	-50.50 (113.6)
8. Cumulative labor market contact (months)	1.879*** (0.381)	1.488* (0.756)	0.476 (1.186)	0.356 (1.778)	1.649 (2.614)	-1.955 (5.794)
Observations	12073	12067	12063	12061	12079	9498
Observations with exam				4744	6153	5709
F test (1 st stage)	129.9	124.1	132.9	132.2	131.8	62.46

Notes: Separate IV estimates from model (2) of the effect of early labor market exposure (job training or employment) with control for background characteristics, municipality dummies, and semester of arrival dummies. * $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. # We present results after 36 months only for the GPA outcomes, due to an insufficient number of observations.

7 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. We examine the consequences on different dimensions of integration; formal language acquisition, and labor market outcomes in the longer run. We hypothesize that early job training may affect language acquisition directly through increased exposure to the native language but that it may also have a negative indirect impact through a substitution effect on time spent on language courses. Likewise, early job training may affect labor market outcomes directly, by training job specific skills and providing a network, or indirectly through changes in language acquisition. We therefore emphasize that work-first strategies may involve a trade-off between early labor market entry and other skills investments.

The analysis shows that participating in on-the-job training in the first year in the host country leads to a reduction in language test scores after completion of the integration program. We also find that job training increases employment but show that the effect is short-lived and disappears two-three years after arrival. The positive short-run effect confirms previous studies (Clausen et al., 2009; Heinesen et al., 2011; Arendt, 2020a). We further show that time spent in early on-the-job training indeed substitutes time for attending language courses.

There is suggestive evidence that the results differ by gender and initial skill level, since a significant reduction in language proficiency is found only at an intermediate skill level (refugees who have completed lower secondary schooling, but not more), and the temporary positive effect on labor market outcomes is found for women only. We cannot, however, rule out that the effects are similar across groups.

One interpretation – which is consistent with our hypothesis – of the simultaneous findings of a temporary positive effect on employment, a substitution between time on the labor market and in language courses, and a negative effect on language acquisition, is that when job training starts to pay off in the labor market, the effect of reduced time in language courses has not yet materialized in reduced language proficiency, but once it does the direct and indirect effects cancel each other out.

Our findings suggest that it is imperative when launching new interventions to examine carefully what they replace. In the current setting of refugee integration, there is growing evidence that language courses have large long-term effects (Kennerberg & Åslund, 2010; Sarvimaaki & Hamalainen, 2016; Orlov, 2017; Arendt et al., 2020). While economic self-reliance is important for the host society as well as for the refugees, any effort to induce refugees to find jobs earlier should be considered against the risk that they drop out of language courses and other investments in skills that may support integration in the longer run.

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