

# **EMPLOYMENT EFFECTS OF A JOB-FIRST POLICY FOR REFUGEES**

JACOB NIELSEN ARENDT

# Employment Effects of a Job-First Policy for Refugees

---

Study Paper No. 139

Published by:

© The ROCKWOOL Foundation Research Unit

Address:

The ROCKWOOL Foundation Research Unit

Ny Kongensgade 6

1472 Copenhagen, Denmark

Telephone +45 33 34 48 00

E-mail: [kontakt@rff.dk](mailto:kontakt@rff.dk)

<https://www.rockwoolfonden.dk/en>

April 2019

# Employment Effects of a Job-First Policy for Refugees

Jacob Nielsen Arendt

The ROCKWOOL Foundation Research Unit, Ny Kongensgade 6, 1472 Copenhagen K,

email: [jar@rff.dk](mailto:jar@rff.dk), phone: +45 6116 3373

## Abstract

This study estimates the effects of a job-first policy for a group that have a very weak attachment to the labor market: refugees who have recently arrived in a new country. The policy requires refugees to actively search for jobs and to participate in on-the-job training immediately upon arrival in the host country. The job-first focus was added to an existing policy that emphasizes human capital investments based on counselling and language training. The relative effects between the two policies are identified by comparing monthly cohorts who settled in their new country before and after the policy was implemented. The results show that the policy has a significant and large employment effect for men 10 to 16 months after their arrival. The policy was implemented as intended and raised on-the-job training for both men and women but nevertheless, the policy has no effect on employment for women.

Keywords: Immigrant, Unemployment, Job-First, Employment Program

JEL code: J61, J64, J68

## **INTRODUCTION**

The large inflow of refugee immigrants that occurred in Europe in 2015 and 2016 emphasized the political and economic challenges that most Western countries face from humanitarian immigration<sup>1</sup>. One significant challenge originates from the low employment rate of these immigrants. Recent studies have shown that refugees have much lower employment rates than other immigrants and host-country nationals. Moreover, they often do not catch up with these two groups, even after having lived for several years in the host country (Bevelander 2016; Bakker et al. 2017; Schultz-Nielsen 2017; Bratsberg et al 2017; Dustmann et al. 2017; Fasani et al. 2017). As I describe below, these findings are particularly disturbing since only few causal studies exist that can aid decision makers in their efforts to close the gap and thereby reduce the economic pressure, and potentially also the social pressure, from the influx of large numbers of refugee immigrants.

This study seeks to fill part of this lack of evidence by estimating the effect of a job-first policy for refugees. The policy was adopted in 2016 and therefore addresses recent challenges. I use a quasi-experimental design to estimate the policy's impact by exploiting the fact that only immigrants who arrived after the third quarter of 2016 were subject to the job-first policy.

The policy evaluated here introduced a requirement that unemployed individuals actively search for a job and subjected immigrants to immediate and continued on-the-job training upon their arrival after they had received a residence permit. Thus, the policy shifted the focus of integration efforts from a human capital policy that primarily emphasized language-course participation to a job-first policy. Similar job-first strategies are in place in several countries, also for asylum seekers; however, to my knowledge, no study has estimated the causal impact of this approach (Martín et al. 2016; Konle-Seidl 2016; Garibay and De Cyuper 2013; Desidero 2016).

---

<sup>1</sup> I use the term refugee immigrant as a common term that refers to individuals who have been 1) granted asylum in accordance with the Geneva Convention, 2) resettled through the UNHCR, 3) provided subsidiary protection, or 4) reunified with a family member who has been granted asylum.

The empirical analysis is based on Danish administrative register data that contain information on all refugees that arrived in Denmark in the period of consideration, including the date of their arrival and weekly information on their participation in labor market programs as well as monthly information on their labor market status.

The results show that overall the job-first policy succeeded in increasing both the share of refugees who are subject to early job-search requirements and the share of refugees who participate in on-the-job training in the first six months following their arrival in Denmark. Moreover, the job-first policy had a large employment effect for men 10 to 16 months after their arrival, but had no employment effect for women. Finally, the job-first policy also reduced participation in language training, and those who did participate showed greater progress and completed the course faster.

There is a long-lasting empirical tradition for comparing job-first policies with human capital policies for unemployed welfare recipients (Hotz et al. 2006; Card et al. 2017). General findings from these studies suggest that a job-first policy has a positive effect in both the short and longer run. In contrast, the human capital policy has negative effects in the short run (lock-in effects), but it may prove beneficial in the longer run. However, these findings are not easily generalized to refugees for a number of reasons.

Refugees arrive in Western countries under difficult circumstances and with other resources than other immigrants. It is therefore to be expected that they react differently to employment policies than host-country nationals and other immigrants. On the one hand, early labor market support and contact to the labor market may be beneficial for some refugees in that it helps retain their motivation and prevents further deterioration of their human capital. On the other hand, most refugees have a substantial skill deficit, both with regard to host-country language skills and academic skills. Therefore, a potential trade-off exists between a human capital policy and a job-first policy.

I am aware of six studies that examine the effects of active labor market programs for newly arrived refugees. Even though some of these six studies consider either on-the-job training or human capital policies based on language investments, none of them consider job-first policies directly. Åslund and Johansson (2011) examined the effect of intensified coaching in Sweden but found no employment effect of the program. Joonas and Nekby (2012) examined the impact of reducing the caseload for caseworkers at job centers in Sweden and found that this raised employment levels for men, but not for women. Joonas et al. (2016) examined the impact of the Swedish Establishment Reform, the objective of which was to centralize the active labor market program within the Public Employment Service and provide intensified coaching to unemployed individuals. Joonas et al. found positive employment effects of the establishment reform for both men and women. Sarvimäki and Hämäläinen (2016) examined the effect of a reform that required an individualized job plan for each refugee. They found that the reform increased the earnings of the refugees substantially over a ten-year period. Clausen et al. (2009) found that subsidized employment has a positive impact on the transition from unemployment to self-support for newly arrived refugees in Denmark, whereas other types of on-the-job training and classroom training have no effect. This is in concordance with Butchek and Walter (2014), who review the literature on active labor market programs for immigrants in general. The evidence on human capital investments made through language training is still limited. Clausen et al. (2009) found that language training in the introduction program postpones labor market entry. This is supported by Lochmann et al. (2018), who found no short-run effects on earnings and employment for refugees who participate in language courses in France.

Looking at immigrants more generally, Hayfron (2001) found no effects on earnings of participation in language training in Norway. A few studies have found positive employment effects of participation in language training for immigrants in general in the longer run (Kennerberg and Åslund 2010 in Sweden; Orlov 2017 in Canada). Although these results may not be generalizable to refugees,

they do suggest that the negative impact found for refugees in the short run may be caused by a lock-in effect.<sup>2</sup>

## **INSTITUTIONAL BACKGROUND**

When asylum seekers receive asylum in Denmark, they are allocated to a municipality. The municipality is responsible for providing housing and for offering an introduction program to the refugee and their family members. The program must be initiated within a month after arrival, and participation is strongly incentivized: If a refugee declines the offer or fails to participate in parts of the program, they are sanctioned financially. Participation in the program is also required if the refugee is to obtain permanent residence. Refugees and their family members are eligible for welfare benefits that are roughly half the size of the welfare benefits to which Danish nationals are entitled (Act 1000 2015).

The introduction program has existed since 1999 and comprises an extensive Danish language course corresponding to 1.2 years of full-time participation and employment support (Act no. 474, 1998). Employment support can either be in the form of on-the-job training, classroom training and other training. Classroom training can take the form of vocational training or more basic skills training, e.g. in literacy or math. ‘Other training’ refers to a mixed group of courses ranging from job-search courses to courses in health coping and social skills training. The type of training that is provided is based on an assessment of the employability of the refugee, i.e. whether they have the skills and the ability to work within a short span of time. This is called the job-readiness assessment. This assessment must be conducted every third month by the municipal job center caseworker. Refugees who are assessed to be ready to work must actively search for a job and participate in active labor

---

<sup>2</sup> A much larger literature has examined either the effect of language proficiency on labor market status or the effect of participating in language training on language proficiency, see Chiswick and Miller (2014) for a review.

market programs when required to. Moreover, they are required to post their résumé on a public job database and to log their job search. Refugees who are assessed as not being ready for employment are not subject to the same job search and activation requirements.

## **THE JOB-FIRST POLICY**

In April 2016, the government, municipalities, trade unions and employer organizations agreed to revise the introduction program for refugees. The primary purpose of the revision was to expedite entry into the labor market (Act no. 665, 2016). In June 2016, a reform of the introduction program was announced, and the reform was enforced by law in July 2016.

The job-first policy is made up of two components, which I refer to as the *early job-search requirement* and the *early on-the-job training requirement*. These two components were only implemented for refugees that arrived in Denmark after 1 October 2016, which allows us to separate the effect of the job-first policy from other elements of the reform.<sup>3</sup>

The *early job-search requirement* states that all participants in the introduction program must be treated as being “job ready” from the time of their arrival. This means that they are subject to active job search requirements and must participate in on-the-job training. Before October 2016, immigrants could be declared as not being job-ready if they had language barriers (which was the case for the majority of immigrants), or if they were unable to log on to the public job database to post their CV

---

<sup>3</sup> The reform changed the duration of the integration program, reducing it from three years to one year. However, if the participant has not found employment or begun an ordinary education program, the program is extended for up to five years. In addition, the reform also made the requirement for housing of refugees more flexible, altered a required universal health examination to a targeted examination, and introduced a requirement for municipalities to delegate the responsibility for integrating refugees into society to a specific public management department.



and document their job search, as required. This type of exemption was explicitly ruled out by the job-first policy.

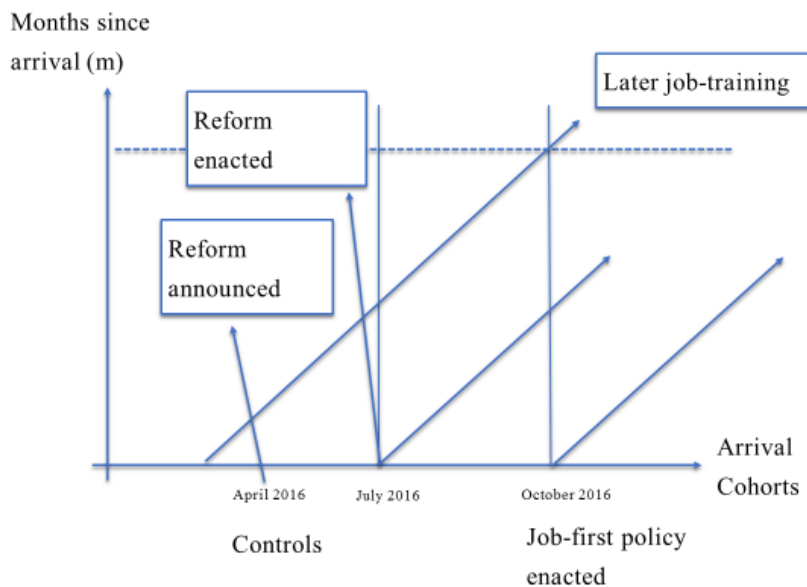
The *early on-the-job training requirement* states that on-the-job training should be initiated within one month upon arrival and is encouraged to start within two weeks. On-the-job training primarily consists of internships at private companies, which last up to 13 weeks. The internee receives welfare benefits, so the employer has no wage costs. This requirement replaced a more general participation requirement, which in practice was often fulfilled by participation in language training (Bolvig and Arendt 2018).

The cohorts who arrived earlier than October 2016 were also subject to the job search requirements, when their job-readiness was re-assessed after October 2016. All on-the-job training spells can at most be interrupted by a six-week break, and the participant is required to work for at least 15 hours a week. This leaves time for other types of activities like language training and job searching.

Figure 1 summarizes the timing of the reform, and illustrates how the job-first component of the reform is implemented at a different point in time from the other elements of the reform. It shows that cohorts that arrived before October 2016 also receives a treatment in the form of intensified on-the-job training, but at a later point in time after their arrival once they pass October 2016.

To sum up, the job-first policy resulted in, that from October 2016, a group that would previously have been assessed as not being job-ready, mainly due to language barriers, was transferred from a policy based on initial human capital investments through language training to a job-first policy.

**Figure 1.** The timing of the policy reform



2

## DATA

I use register data collected by Danish public agencies for administrative purposes. Information on welfare benefit receipt and active labor market program participation is collected by the Danish Agency for Labour Market and Recruitment (STAR) on a weekly basis and the number of work hours is collected per month. I only include non-subsidized employment in the employment definition. The data from STAR are combined with other administrative data from Statistics Denmark that contain the variables gender, type of residence permit, time of first arrival, country of origin, age, and household composition at arrival.

The population covers all immigrants aged 18 to 64 who are either granted asylum or family members within the same age span who are reunified with a refugee for the first time. Asylum is provided either in accordance with to the Geneva convention, the UN quota agreements or as subsidiary protection. Family reunification covers unification with parents or cohabiting partners. I only include cohorts

who arrived in Denmark from September 2015 to December 2017, i.e. slightly more than one year before and after implementation of the job-first policy. Arrival is defined as settlement with legal residence in a municipality. This is when the integration program begins. Participation in active labor market programs and employment is observed for all cohorts for at least 19 months. This sample consists of 12,478 persons. There are different types of waiting periods for those who settle, which are not the focus of the current study. To get a more homogeneous sample, I restrict data to persons who received their residence permit in the year they settle or the year before (91%) and who subsequently received welfare benefits within five weeks after their arrival (81%). The final sample consists of 9,330 persons. Table 1 shows the average characteristics for this sample by gender and treatment status, i.e. whether they arrived before or after October 2016.

**Table 1. Characteristics of refugee participants in the introduction program by treatment status, September 2015-2017**

Treatment status	Has children	Age	Couple	Asylum	Syria	Eritrea	N
Men							
0	0.23	30.21	0.25	0.95	0.72	0.18	3,339
1	0.24	30.95	0.30	0.91	0.32	0.08	1,161
Total	0.23	30.40	0.27	0.94	0.62	0.16	4,500
Women							
0	0.68	31.29	0.74	0.50	0.83	0.08	3,292
1	0.67	30.36	0.78	0.40	0.57	0.17	1,538
Total	0.68	31.00	0.75	0.47	0.75	0.11	4,830

Note: The characteristics are measured in the arrival year. Treatment is defined as arrival in October 2016 or later.

The numbers in the table show that the treatment and control groups have equal demographic characteristics (children, age, and arrived as a couple), but that the country of origin changes over the period as the majority of the Syrians were admitted before October 2016. The second largest group is from Eritrea, and the size of this group also varies over the period, with more Eritrean men arriving

before October 2016 and more women arriving after.<sup>4</sup> Because a large share of the Syrians were granted asylum, the total share who are granted asylum also differs between the treatment and the control group. I show later that, despite these differences, the results are not very sensitive to the inclusion of covariates, nor is there any great heterogeneity across subgroups, e.g. Syrians and non-Syrians.

Table 2 shows the employment rates and cumulated work hours at a given number of months after arrival. Both measures of employment are higher for the treatment group than for the control group, and employment increases with time in the country. This holds true for both men and women, although the employment level is much lower for women.

**Table 2. Employment rate and cumulated work hours, by month after arrival and treatment status**

Treatment status	Employed after months:				Cumulated work hours:			
	9	12	15	18	9	12	15	18
<b>Men</b>								
0	0.11	0.18	0.24	0.30	34.5	83.1	153.4	245.1
1	0.21	0.31	0.33	0.38	76.6	166.9	274.8	385.7
Total	0.14	0.21	0.26	0.32	45.7	104.9	181.9	270.8
N	4500	4464	4318	4055	4357	4464	4318	4055
<b>Women</b>								
0	0.02	0.03	0.05	0.06	4.4	10.7	20.3	34.0
1	0.03	0.06	0.07	0.08	10.6	27.1	42.2	68.5
Total	0.02	0.04	0.05	0.06	6.4	15.8	26.1	41.4
N	4830	4754	4484	4184	4830	4754	4484	4184

*Notes:* Employment is recorded as at least one hour of work in a given month.

<sup>4</sup> 81 % are from either Eritrea or Syria. The remaining population are from many different countries with Iran as the single largest, followed by Afghanistan, Iraq, and Somalia as the only countries with more than 100 refugees.

Table 3 shows mean rates of participation in the different parts of the introduction program within six months after arrival. I focus on the first six months because this is where the treatment is expected to affect participation. In the data source used for this table, classroom training includes language training. The table shows that, in the treatment group, nearly half the men and a third of the women are declared to be job ready three weeks after their arrival, whereas this is only the case for 3-4% of the men in the control group. The share of refugees in the treatment group who are declared job ready increases to 80% for men and 70% for women within six months after their arrival in Denmark. In the control group, this figure only rises to 30% for men and to 20% for women.

**Table 3. Participation in different parts of the introduction program**

Treatment status	Job ready after week:			On-the-job training after week (cumulated):		
	3	13	26	3	13	26
<b>Men</b>						
0	0.04	0.14	0.28	0.21	0.74	3.77
1	0.43	0.82	0.80	0.84	2.58	8.06
Total	0.14	0.32	0.42	0.38	1.23	4.91
<b>Women</b>						
0	0.03	0.11	0.21	0.05	0.25	1.42
1	0.32	0.67	0.71	0.22	0.87	3.62
Total	0.13	0.29	0.38	0.11	0.45	2.15
Treatment status	Classroom training after week (cumulated):			Other training after week (cumulated):		
	3	13	26	3	13	26
<b>Men</b>						
0	2.38	5.60	13.39	0.84	1.40	2.56
1	2.45	4.78	10.16	0.97	1.45	2.32
Total	2.40	5.39	12.53	0.88	1.42	2.50
<b>Women</b>						
0	1.31	3.55	11.03	0.52	0.94	1.92
1	1.45	3.53	10.08	0.69	1.18	2.07
Total	1.36	3.54	10.72	0.58	1.02	1.97

Notes: Classroom training includes language training.

The next set of columns shows that the treatment group also experiences a rise in on-the-job training participation: After half a year, men in the treatment group have participated in five more weeks of on-the-job training than the control group, and the women in the treatment group have participated in two more weeks. The bottom part of the table shows that, in contrast, participation in classroom training is reduced in the treatment group, relative to the control group. There is no difference in the participation in other types of training between the treatment group and the control group.

## EMPIRICAL MODEL

I estimate the following linear model for each month after arrival (suppressing individual subscripts) to handle the timing of the policy reform and to disentangle the treatment effect from a calendar time effect:

$$(1) \quad Y_{cm} = \alpha + \gamma_1 t + \gamma_2 t^2 + \pi X_c + \delta_{1m} C_1 + \delta_2 C_2 + \beta_m T + \theta_r + \epsilon_{cs}$$

$$C_1 = 1(c_2 > c \geq c_1), C_2 = 1(c + m > c_2), T = 1(c \geq c_2)$$

Where  $Y_{cm}$  is the outcome of interest in the  $m^{\text{th}}$  month after arrival for individuals who arrived in month  $c$ ,  $c_2$  is October 2016, and  $c_1$  is July 2016. Therefore,  $T$  is the treatment indicator,  $C_1$  denotes cohorts who arrive in the quarter immediately before October 2016, and  $C_2$  controls for the fact that cohorts who arrive before July 2016 ( $c < c_1$ ) are affected by the job search requirement and continued training requirement once they reach October 2016.  $X_i$  contains observed characteristics at arrival: country of origin, age, marital status, couple and number of children in the household.  $t$  is calendar time and  $\theta_r$  are fixed effects for the municipality at arrival, taking into account, among other things, different program implementation patterns and job opportunities in local labor markets.

The estimated coefficient  $\beta_m$  can be interpreted as intent-to-treat effects of the job-first policy relative to treatment under the old introduction program. A causal interpretation can be given when arrival cohorts are identical (eliminating cohort effects), when calendar time affects everyone equally, and

when there are no anticipation effects. These are not innocuous assumptions, and sensitivity analyses are therefore conducted to assess their importance.

## **RESULTS**

The results from estimates of model (1) are presented in Table 4. I use the indicator of being employed as well as the total number of work hours cumulated from the time of arrival as dependent variables. The results are presented for every third month after arrival and they show that the job-first policy increases the employment rate for men by six percentage points after 12 months, but that the effect is only significant on a ten percent level. The effect is slightly larger after ten months and significant at the five percent level (not shown). In contrast, the effect on total hours worked (cumulated from the time of arrival) is large and significant at 12 and 15 months after arrival for men. The men in the treatment group have worked 74 hours more than the control group cumulated over the first 15 months after their arrival. This corresponds to two weeks of full-time work, or an increase of roughly 50% relative to the level for the control group (see Table 2). Both effects on employment rates and total work hours are small and insignificant for women.

The effect for men is also relatively large at 18 months after arrival, although it is smaller than at 15 months. The statistical uncertainty is larger due to a smaller sample size. Eventually all cohorts in the control group are treated, so the effect may drop either because the effect is temporary or because the control group catches up when treated. The employment rates in the control group shown in Table 2 did not suggest a sudden shift after 15 months, but I explore this further in the robustness analyses below.

**Table 4. The effect of the job-first policy on employment and work hours cumulated over time after arrival**

	Months after arrival					
	3	6	9	12	15	18
<b>Men</b>						
Employed	0.005 (0.008)	-0.010 (0.019)	0.047 (0.031)	0.058* (0.034)	-0.002 (0.041)	0.0003 (0.056)
Total work hours	0.84 (1.18)	-0.26 (4.93)	15.04 (12.49)	51.68*** (19.67)	74.12** (31.61)	32.55 (53.38)
N	4453	4452	4445	4410	4266	4006
<b>Women</b>						
Employed	-0.005 (0.003)	0.001 (0.007)	0.006 (0.013)	0.008 (0.015)	-0.013 (0.020)	-0.007 (0.027)
Total work hours	-0.399 (0.491)	-1.193 (1.642)	0.991 (4.491)	-2.303 (7.303)	10.20 (10.97)	-2.911 (19.23)
N	4775	4774	4772	4695	4422	4129

Note: Separate monthly OLS estimates of effects of T from model (1). The regression includes the following control variables at time of arrival: age, couple, children, country of origin, residence permit type, municipality fixed effects, a time dummy for when the control group passes October 2016, and a quadratic function of calendar time. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors in parentheses.

I also note that both the indicator variables  $C_1$  and  $C_2$  are insignificant.  $C_1$  captures potential differences between the treatment group and the cohorts that arrive from July to September 2016.  $C_2$  captures a mean impact of later job-readiness assessment and intensified on-the-job training after October 2016 for the control group.

Table 5 collects estimates from model (1) with other outcomes measured at 15 months after arrival. Similar conclusions are obtained for outcomes measured 12 months after arrival (available upon request). The first column contains results for an indicator of any employment up till the given point. The coefficients are insignificant for both men and women implying that the same share of the groups find regular employment at a given point in their assimilation process. The second column shows that there are no effects on the number of weeks that immigrants receive public student grants after 15 months. The student grant is available to all adult students enrolled in post-secondary education in Denmark<sup>5</sup>. The amount of money received with a student grant is only slightly lower than the welfare

<sup>5</sup> Participation in higher education, however, requires documentation of a high level of language proficiency.



assistance for immigrants without children. Finally, I show that there are no significant effects on sample attrition rates (due to death, illegal residence or emigration) either.

**Table 5. The effect of the job-first policy on different outcomes in month 15 after arrival**

	Not yet employed	Cumulated weeks with student grant	Sample attrition
Men	0.001 (0.040) 4401	-0.045 (0.155) 4401	0.001 (0.006) 4423
Women	-0.006 (0.021) 4684	-0.570 (0.368) 4684	0.005 (0.005) 4703

Note: Separate monthly OLS estimates of effects of T from model (1), see also notes to Table 4. \* p < 0.1, \*\* p < 0.05, \*\*\* p < 0.01. Standard errors in parentheses.

### **First-stage results: Characterizing treatment**

I have shown that the job-first policy has an employment effect for men, but not for women. To understand what may drive the effect for men, I look at the first stage, i.e., the effect of the job-first policy on the integration program components. The components are job readiness, on-the-job training class room training (including language courses) and other labor market programs (e.g. counselling and job search courses).

The effects on the number of cumulated weeks of participation in each of the three different interventions and on weeks assessed as job ready are shown in Table 6.

The results show that the job-first policy lifts the share that is declared job ready. The effect is lower after 52 than after 26 weeks. This might be because some of persons in the control group are also assessed as being job ready under the new requirements, or because the persons in the treatment group are re-assessed and some are found not to be job ready.

**Table 6. Effects of the job-first policy on participation in the integration program**

Weeks after arrival:	Job ready		On-the-job training		Classroom training		Other labor market programs	
	26	52	26	52	26	52	26	52
Men	11.35*** (0.477)	6.913*** (1.035)	1.535*** (0.481)	2.299** (0.992)	-1.161** (0.570)	-2.316** (1.119)	-0.369 (0.350)	-0.384 (0.671)
N	4421	4328	4421	4328	4421	4328	4421	4328
Women	9.337*** (0.543)	9.123*** (1.149)	1.395*** (0.304)	0.671 (0.739)	-0.796 (0.559)	0.213 (1.114)	-0.0583 (0.296)	0.551 (0.565)
N	4753	4585	4753	4585	4753	4585	4753	4585

Note: Separate monthly OLS estimates of effects of T from model (1), see Table 4.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors in parentheses.

The job-first policy also lifts the number of weeks that the treatment group participates in on-the-job training. The effect on on-the-job training is persistent after 52 weeks for men: They have participated in more than two weeks of on-the-job training and have been assessed to be job ready for close to seven weeks more. A similar pattern is observed for women, but the effect on on-the-job training is not significant after 52 weeks. The next set of results show that there is a negative effect of the job-first policy on classroom training for men. The effects are of a similar size as the effect on on-the-job training. This indicates that participation in on-the-job training (or employment) crowds out the use of this part of the integration program for men.

I look further into the effects on language course participation, using another data set that isolates participation in language courses. The results are shown in Table 7. A language course is divided into six modules and participants must pass a test after each module before they can proceed to the next module. The results show that participation starts earlier for persons in the treatment group. However, the results also confirm the previous findings: that the degree of participation is reduced during the first year. Participation is measured as hours of assigned language training. In agreement with this finding, four to seven percent fewer pass the first module in their first attempt. Yet, among those who

pass a module, they pass 10 to 14 weeks faster. The consequences of the job-first policy on human capital investment is therefore ambiguous: even though the number of hours of participation is reduced slightly, some participants progress faster through the course. Whether this is a result of participation in on-the-job training cannot be inferred from these data.

**Table 7. Effects of the job-first policy on language course outcomes**

	Weeks to course start	Hours of language training, year 1	Passed first module in first attempt	Weeks before passed first module
Men	-1.854** (0.756)	-22.03*** (6.831)	-0.0704*** (0.0165)	-13.75*** (3.410)
N	4384	3484	4386	3542
Women	-1.017 (0.739)	-21.27*** (5.900)	-0.0519*** (0.0151)	-10.15*** (3.481)
N	3542	4592	4605	3522

Note: Separate OLS estimates from cross-sectional models with the same covariates as in model (1), see Table 4.  
\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors in parentheses.

## Robustness analyses

This section presents an analysis of the robustness of the results by addressing the following questions:

- 1) Are the differences between the treatment group and control group, notably in terms of country of origin, sufficient to impede a causal interpretation of the estimated effects? I explore this question by looking at the sensitivity of the results to the inclusion of the covariates and by exploring results across subgroups.
- 2) Are the results sensitive with respect to how calendar time is controlled for, and to the use of control groups that are treated at different points after their arrival?

The results from these sensitivity analyses are presented in Table 8. The first row shows the effects across specific subgroups. The effects are almost identical for Syrians and non-Syrians. This result is

important because the share of Syrians showed the greatest difference between the treatment group and the control group. There are larger differences between the estimates for couples and non-couples (3-4), and between estimates for refugees and reunified families (5-6), but all the estimates are positive and significant. Moreover, the pairs of estimates based on country of origin, arrival as a couple or not, and type of residence permit are not significantly different from each other.

**Table 8. Robustness analysis, hours worked after 15 months, men**

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment group	63.32** (25.97)	69.03* (36.50)	45.17* (24.74)	80.41** (31.54)	46.51** (20.66)	146.2* (80.15)
Population	From Syria	Not from Syria	Single	Arrival as a couple	Refugee	Family, reunified
N	2742	1668	3248	1162	4151	259
	(7)	(8)	(9)	(10)	(11)	(12)
Treatment group	121.4*** (12.26)	100.1*** (28.33)	100.8*** (14.02)	79.90*** (27.34)	39.32 (39.39)	87.91* (46.19)
Control variables	No	No	Yes	Yes	Yes	Yes
Calendar time	No	Quadratic	No	Linear	Quadratic	Quadratic
N	4464	4464	4464	4464	2987	3508

Notes: See Table 4. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors in parentheses. All estimates are from model (1) with the same control variables as reported in Table 4, unless otherwise mentioned.

The second part of Table 8 shows the results with different sets of control variables and different control groups. The result denoted as (7) shows the estimate without controlling for calendar time or characteristics and shows that the treated cohorts have a much higher employment rate than the control group, as is also seen in Table 2. The estimate drops to 100 work hours when controlling for calendar time, but without covariates (8). An almost similar estimate is obtained when covariates are included, but calendar time is excluded (9). These estimates are not significantly different from the

main estimate of 74 work hours. In (10), I control for calendar time using a linear function of time. Here, the estimate is almost identical to the main estimate presented earlier: 79 work hours. I tried higher order polynomials in calendar time, but the models were infected by collinearity. Columns (11) and (12) contain the estimated effect of the job-first policy when the control group arrives before February or after January 2016. It is seen that there is only a significant effect for cohorts that arrive earlier and who are treated later. This may suggest that the effect drops after 15 months because a larger share of the control group is treated at this point.

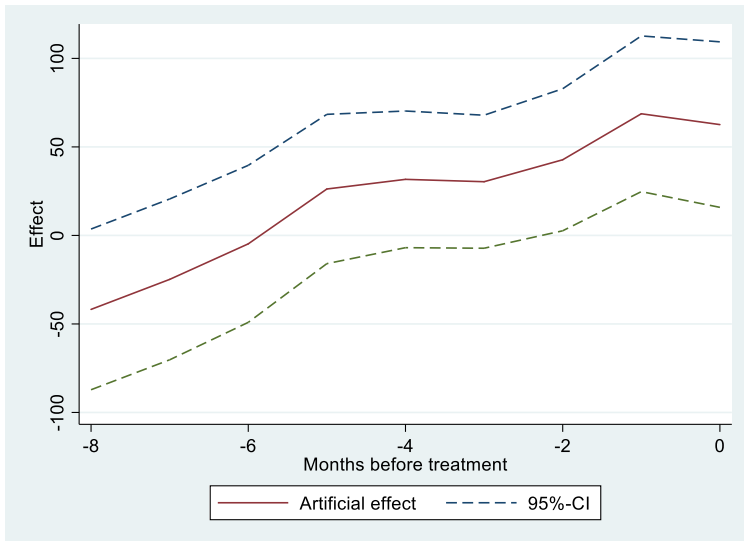
### **Placebo tests**

To provide further evidence on the plausibility of the results, I conduct two different placebo analyses. In the first, I estimate a model (1) with an artificial treatment dummy that is rolled backwards in time. The results are shown in Figure 1 for men, where time zero corresponds to cohorts who settled in October 2016. The analysis shows that the effect is only significant at the true treatment date (0 in the figure) and the two months before. The increase before the actual treatment suggests that some municipalities had initiated the treatment before the reform was introduced. Note that this does not affect my main estimates because I have dummied out the quarter prior to treatment: This is done because the cohorts who arrive in this quarter are potentially affected by other parts of the reform that were implemented in July 2016.

In the second placebo analysis, I estimate the model in a 10% random sample of Danish welfare benefit recipients. A cohort is defined by the start date for the welfare benefit spell. If the quadratic function of calendar time does not control adequately for improvements in the business cycle, I should see a positive effect for other groups who are not affected by the reform of the integration program. Figure 2 shows that the results are insignificant for both men and women. If anything, the results

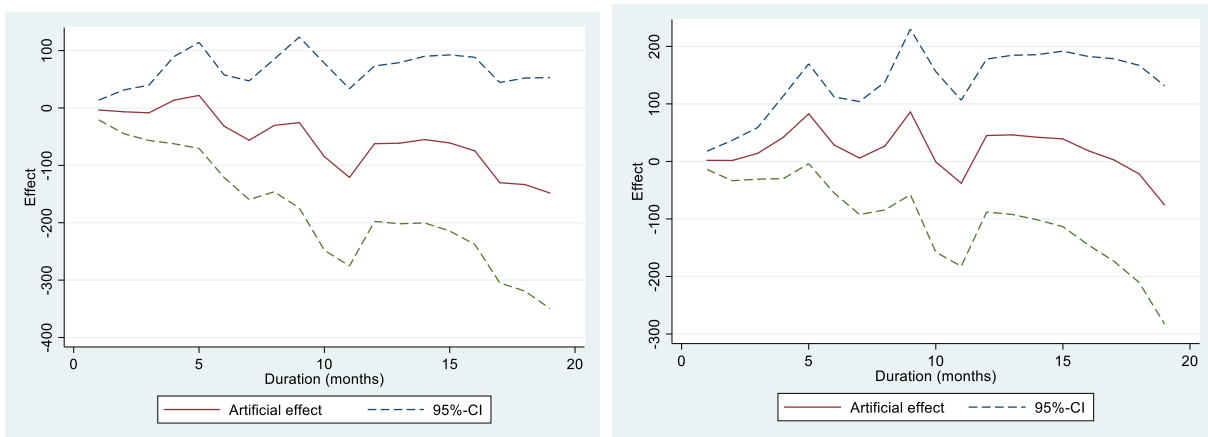
suggest that the model may in fact over control the business cycle, as the estimates are negative for men. This means that our estimates are downward biased.

**Figure 1. Placebo effects on hours worked after 15 months, men**



Notes: Estimates from model (1) with artificial treatments, see Table 4.

**Figure 2. Placebo effects on hours worked for a sample of Danish welfare recipients**



Notes: Estimates from model (1), see Table 4.

## **CONCLUSION**

The large influx of asylum seekers in Western Europe in 2015 led to substantial costs for national governments. This in turn resulted in a demand for effective solutions that could help refugees become self-reliant. Many countries are considering, or implementing, job-first policies to expedite immigrants' entry into the labor market. This approach stands in contrast to a long tradition that has emphasized human capital investments, primarily in the form of language training. Examples of the human capital investment approach are the Danish integration program from 1999, German legislation in the area introduced after 2005 and similar Dutch legislation introduced after 2007. In most countries, it has been left to the discretion of local job centers when to time employment support programs. To the best of my knowledge, no studies have documented whether the shift in policies has had the intended effect.

This study estimates the effects of a job-first policy that required refugees to actively search for jobs and participate in on-the-job training within one month of their arrival. From a policy point of view, this is an interesting experiment, both with regard to how to tackle refugee immigration and with regard to tackling unemployment for groups with a very weak attachment to the labor market.

I use the introduction of the policy as a unique quasi-experiment to estimate its effect on employment for refugees. I can only estimate a short-term effect, because the control group is also treated by the reform at a later stage after their arrival.

I show that the job-first policy has a large and positive employment effect for men in the short to medium term, but that the policy has no employment effect for women. This effect is reduced as expected when a larger share of the control is treated at a later point. The policy also reduces investments in language training, but at the time of writing I cannot distinguish whether this is a permanent reduction or merely a delay. To the extent that language training is effective, this will of course be important for the long-term consequences.

I find that the effect is stable across different groups of refugees. This is important because there are significant differences between the treated group and the control group in observed characteristics, notably in the share of refugees from Syria. It is also important because another policy that increased the economic incentive to find a job was implemented in April 2016. The policy placed a ceiling on the welfare benefits that a family could obtain. The policy only affected couples, not singles, and I find an effect of the job-first policy for both groups. The estimated effects are also robust towards different ways of controlling for the business cycle, and finally no effect is found in two types of placebo analyses.

The estimates may, however, depend on the specific circumstances under which the policy is implemented. In particular, the policy is implemented during an economic upturn, within a welfare regime that provides reduced welfare benefits for newly arrived immigrants, and where the employment of refugees has received substantial attention from the government, municipalities and employer organizations. Whether similar effects can be obtained under a different scenario is not known. Finally, I stress that the estimated effects are not the effect of the revised introduction program per se, but rather of the specific components related to the job-readiness requirements and on-the-job training requirements.



## REFERENCES

- Act 296 (2016). Acts of Parliament of 22/03/2016 on changes to the Act on Active Social Policy and the Act on Integration of foreigners in Denmark. The Ministry of Employment.
- Act 474 (1998). Acts of Parliament of 01/07/1998 on the Integration of foreigners in Denmark. The Ministry of Interior.
- Act 665 (2016). Acts of Parliament of 08/06/2016 on changes to Act on Integration of foreigners in Denmark. The Ministry of Transport, Construction and Housing.
- Act 1000 (2015). Acts of Parliament of 30/08/2015 on changes to the Act on Active Social Policy. The Ministry of Transport, Construction and Housing.
- Angrist, J., Imbens, G. and D. Rubin (1996). Identification of Causal Effects Using Instrumental Variables, *Journal of the American Statistical Association* 91: 444-472.
- Bakker, L., Dagevos, J. and G. Engversen (2017). Explaining the refugee gap: a longitudinal study on labour market participation of refugees in the Netherlands. *Journal of Ethnic and Migration Studies* 43(11): 1775-1791.
- Bevelander, P. (2016). Integrating refugees into labor markets. IZA World of Labor 269. doi: 10.15185/izawol.269.
- Bolvig, I. and J. N. Arendt (2018). Nationale ramme for integrationsindsatsen af flygtninge, 1997-2017. København: VIVE.
- Butschek, S. and T. Walter (2014). What active labour market programmes work for immigrants in Europe? A meta-analysis of the evaluation literature. *IZA Journal of Development and Migration* 3(48): 1-18.
- Card, D., Kluve, J. and A. Weber (2017). What works? A Meta Analysis of Recent Active Labour Market Program Evaluations. IZA Working Paper, No. 9236, revised 2017 version.
- Chiswick, B. R. and Miller, P. W. (2014). International Migration and the Economics of Language. IZA Discussion Papers, No. 7880.
- Clausen, J., Heinesen, E., Hummelgaard, H., Husted, L. and Rosholm, M., (2009). The Effect of Integration Policies on the Time until Regular Employment of Newly Arrived Immigrants: Evidence from Denmark, *Labour Economics* 16(4):409-417.
- Desidero, M. V. (2016). Integrating refugees into host country labor markets. Challenges and opportunities. Washington DC: Migration Policy Institute.
- Dustmann, C., Fasani, F., Frattini, T., Minale, L. and U. Schoenberg (2017). On the Economics and Politics of Refugee Migration. *Economic Policy* 32(91): 497-550
- Fasani, F., Frattini, T. and L. Minale (2017). (The Struggle for) Refugee Integration into the Labour Market: Evidence from Europe. CReAM working paper.
- Flores, C. A. and A. Flores-Lagunes (2013). Partial Identification of Local Average Treatment Effects with an Invalid Instrument. *Journal of Business and Economic Statistics* 31 (4).

- Garibay, M. G. and P. De Cyper (2013). The evaluation of integration policies across the OECD: A review. Policy Research Centre on Integration.
- Hayfron, J. E. (2001). Language training, language proficiency and earnings of immigrants in Norway. *Applied Economics* 33(15): 1971-1979.
- Hotz, V. J., Imbens, G. W. and J. A. Klerman (2006). Evaluating the differential effects of alternative welfare-to-work training components: A reanalysis of the California GAIN program. *Journal of Labor Economics* 24(3): 521-566.
- Heinesen, E., Husted, L. and Rosholm, M. (2013). The Effects of Active Labour Market Policies for Immigrants Receiving Social Assistance in Denmark, *IZA Migration Journal* 2(15): 1-22.
- Hvidtfeldt, C. and M. L. Schultz-Nielsen (2017). Flygtninge og asylansøgere i Danmark 1997-2016. Arbejdsrapport no. 50. København: ROCKWOOL Fondens Forskningsenhed.
- Joona, P. A. and L. Nekby (2012). Intensive coaching of new immigrants: an evaluation based on random program assignment. *The Scandinavian Journal of Economics* 114(2): 575-600.
- Joona, P. A., Lanninger, A. W. and Sundström, M. (2016). Reforming the Integration of Refugees: The Swedish Experience. IZA Working Paper No. 10307. Bonn: IZA.
- Kennerberg, L. and O. Åslund (2010). Sfi och arbetsmarknaden (Swedish for immigrants and the labour market). IFAU Working paper 2010:10.
- Konle-Seidl, R. and G. Bolitz (2016). Labor market integration of refugees: Strategies and good practices. EMP Study 2016-08. Policy Department A: Economic and Scientific Policy, the European Parliament.
- Konle-Seidl, R. (2018). Integration of refugees in Austria, Germany and Sweden. EMP Study 2016-23. Policy Department A: Economic and Scientific Policy, the European Parliament.
- Lochmann, A., H. Rapoport, and B. Speciale (2018). The Effect of Language Training on Immigrants' Economic Integration: Empirical evidence from France. IZA Working Paper No. 11331.
- Martín, I., Arcarons, Aumüller, J., Bevelander, P., Emilsson, H., Kalantaryan, S., MacIver, A., Mara, I., Scalettaris, G., Venturini, A., Vidovic, H., Welle, I., Windisch, M., Wolffberg, R. & A. Zorlu (2016). From Refugees to Workers Mapping Labour-Market Integration Support Measures for Asylum Seekers and Refugees in EU Member. Volume II: Literature review and country case studies. Bertelsmann Stiftung.
- Orlov, G. (2017). The Impact of Language Training on the Transfer of Pre-Immigration Skills and the Wages of Immigrants. Working Paper.
- Sarvimäki, M. and K. Hämäläinen (2016). Integrating Immigrants: The Impact of Restructuring Active Labor Market Programs, *Journal of Labor Economics* 34(2): 479-508.
- Åslund, O. and P. Johansson (2011). Virtues of SIN: Can Intensified Public Efforts Help Disadvantaged Immigrants? *Evaluation Review* 35(4): 399-427.