

PERMANENT RESIDENCY AND REFUGEE IMMIGRANTS' SKILL INVESTMENT

JACOB NIELSEN ARENDT

CHRISTIAN DUSTMANN

HYEJIN KU

Permanent Residency and Refugee Immigrants' Skill Investment

Study Paper No. 161

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

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

June 2021

Permanent Residency and Refugee Immigrants' Skill Investment*

Jacob Nielsen Arendt[†]

Christian Dustmann[‡]

Hyejin Ku[§]

May 2021

Abstract: We analyze an immigration reform in Denmark that tightened refugee immigrants' eligibility criteria for permanent residency by requiring minimum years of cumulative employment and raising the bar for the existing language test. Contrary to what the reform intended, individuals with low pre-reform labor market performance decreased their labor supply while those with high pre-reform labor market performance did become more likely to pass the language test at the required level. Our findings thus suggest that stricter permanent residency rules, while effective in incentivizing high-performance individuals, may have no or even opposite effects on low-performance individuals.

Keywords: immigrant assimilation, refugee integration, labor supply, language, human capital

JEL codes: J22, J24, J61

* We are grateful to the ROCKWOOL Foundation for funding this project. Christian Dustmann also acknowledges support from the European Research Council (ERC) Advanced Grant (MCLPS) – 833861 and the DFG – grant 1024/1-2 AOBJ:642097.

[†] ROCKWOOL Foundation Research Unit, Ny Kongensgade 6, Copenhagen, 1472, Denmark. jar@rff.dk

[‡] University College London, Department of Economics and CReAM, 30 Gordon Street, London, WC1H 0AX, UK. c.dustmann@ucl.ac.uk

[§] University College London, Department of Economics and CReAM, 30 Gordon Street, London, WC1H 0AX, UK. h.ku@ucl.ac.uk

1. Introduction

The number of asylum seekers in the European Union (EU) reached a record high level with around 1.3 million applicants in both 2015 and 2016, imposing major economic and political challenges for receiving countries. Among these challenges, the poor labor market attachment of refugee immigrants is one of the most substantial, with their labor force participation and earnings persistently lower than those of other immigrant groups (see Fasani et al. 2018; Brell et al. 2020). Many receiving countries have therefore implemented policies aimed at incentivizing refugee immigrants' labor force participation and acquisition of local language skills, requiring these investments as a precondition for benefit receipts and for obtaining permanent residency.¹ Nonetheless, to assess the effectiveness of such policies in incentivizing refugee immigrants' skill acquisition, we lack reliable data on the recent immigrants' medium- to longer-term outcomes since the latest wave of refugee migration to EU countries is still quite nascent.

To shed light on the consequences of policies aimed at the labor market integration of refugee immigrants, we take advantage of Denmark's experience with refugee migration in the 2000s and its experimentation with permanent residency rules.² In particular, drawing on detailed administrative data, we evaluate a 2007 reform that—while extending the existing legislation that offered permanent residency to all refugees who had been resident for at least 7 years—added two new requirements such that individuals needed to have accumulated at least 2.5 years of employment (in full-time equivalents) and have passed a Danish language

¹ All the Nordic countries altered integration and immigrant policies in a more restrictive direction in the aftermath of the refugee crisis in 2015 (see Hernes 2018). Moreover, in response to the refugee crisis, Austria, Netherlands and Hungary have limited access to social benefits while Austria, Poland, and Czech Republic have made permanent residence conditional upon passing a language test (OECD 2018). Passing a language test to become eligible for permanent residency has been required in Denmark since 2002, in Germany since 2005, in the Netherlands since 2010 and in Norway since 2013 (Arendt 2018).

² Denmark experienced a large inflow of refugee immigrants in the 1990s and 2000s partly due to a lenient immigration policy that was gradually tightened during the period (Hvidtfeldt and Schultz-Nielsen 2017). For further discussion on Denmark's experience with refugee migration during this earlier period, see studies such as Clausen et al. (2009), Kilström et al. (2018), Andersen et al. (2019) and Arendt et al. (2020).

test at the intermediate level (as opposed to the basic level required previously). The reform was proposed on November 29, 2006, passed by the parliament on April 19, 2007 and came into force on May 1, 2007. It applied to refugee immigrants who had not yet completed the standard 3-year mandatory integration program by November 29, 2006, the date of the reform proposal (see Section 2 for details).³

To assess the impact of tighter permanent residency rules on refugee immigrants' skill investment, we compare the labor market trajectories of immigrants receiving (temporary) residency in January-October 2003 (who could complete the 3-year integration program by the policy cutoff date) with that of immigrants receiving (temporary) residency in January-October 2004 (for whom completion of the 3-year integration program by the cutoff date was not possible), following each cohort for seven years. In particular, we employ a difference-in-differences (DID) strategy exploiting the fact that the later admitted (i.e. treated) cohorts are subjected to the new and more stringent eligibility criteria for permanent residency beginning in 2007 or their third year since residency (YSR), whereas the earlier admitted (i.e. control) cohorts continue to be applied the old criteria. Importantly, both the treated and control cohorts had been residing in Denmark for at least two years at the time of the reform proposal (November 2006) and implementation (May 2007), which makes their entry to Denmark, asylum applications, and asylum outcomes orthogonal to the 2007 reform.

We find that overall, the 2007 reform *decreased* employment for individuals subjected to the new and more stringent requirements for permanent residency. Evaluated at the mean, the estimated effect implies a 30 percent drop in annual employment (measured in full-time equivalents). Moreover, we find no significant reform effect on the likelihood to pass the language test at the higher required level. This is in contrast to what the reform was intended

³ The mandatory integration program commences within one month after a refugee immigrant is granted temporary residency and assigned to a municipality. Therefore, those who arrived before November 2003 could complete the 3-year integration program by the cutoff date of November 29, 2006.

to achieve, namely to incentivize refugee immigrants' skill investment through labor market attachment and language acquisition.

To rationalize these empirical patterns, we hypothesize that the reform may have differential effects on individuals depending on their underlying capacity to fulfill the new requirements (and obtain permanent residency). High productivity (or low cost of effort) individuals who can meet the new requirements relatively easily may indeed be incentivized to fulfill the employment and language criteria. In contrast, for low productivity (or high cost of effort) individuals, the reform may create a disincentive effect.

We investigate this conjecture by making use of a unique feature of the reform such that it affected individuals who had been residing in Denmark for at least two years prior to the reform implementation in 2007. In particular, we observe the *pre-reform* labor market performance of the treated and the control cohorts and classify individuals who have accumulated a non-negligible amount of employment experience as "high performance" and those who have not as "low performance". We then examine whether the effect of exposure to the 2007 reform differs for high- vs. low-performance individuals, as a proxy for their level of productivity (or cost of effort).

We find that the negative overall employment effect discussed above is mainly driven by the responses of low performance individuals while high performance individuals did become more likely to pass the language test at the required intermediate level (as they can readily fulfill the required level of cumulative employment). Our findings thus suggest that stricter eligibility criteria for permanent residency, while effective in incentivizing high performance individuals, may have no or even opposite effects for low performance individuals for whom the expected returns to fulfilling the new criteria are dominated by the steep rise in the cost of effort required. Moreover, given the importance of language skills for earnings as well as social assimilation of immigrants (Bleakley and Chin 2004, 2010), the heterogeneous responses by

the high and low performance types to the reform will likely reinforce the pre-existing differences between these groups.

By assessing the effects of a reform in Denmark that introduced substantial labor market engagement and tougher language tests as a new requirement for obtaining permanent residency, we provide much needed evidence on the incentive effects of tougher residency rules—as considered in many EU countries today—on the skill acquisition of refugee immigrants. In doing so, we contribute to the long literature on the determinants of immigrant assimilation and integration (see e.g. Chiswick 1978; Borjas 1987; Edin et al. 2003; Cortes 2004; Lubotsky 2007; Cadena et al. 2015; Sarvimäki and Hämäläinen 2016; Sarvimäki 2017; Albert et al. 2020). In particular, we add to the discussion on the expected duration of stay and immigrants’ investment in the host country-specific human capital in the spirit of Becker (1964) and Ben-Porath (1967).⁴ Moreover, by focusing on permanent residency rules as a key policy instrument, we also add to the growing literature that relates immigrant outcomes to their citizenship/residency status (see e.g. Bratsberg et al. 2002; Pinotti and Mastrobuoni 2015; Pinotti 2017; Gathmann and Keller 2017; Fasani 2018; Devillanova et al. 2018; Adda et al. 2020; Hainmueller et al. 2020; Felfe et al. 2020; Khourshed and Méango 2020).

In complementary recent work, Fasani et al. (2020) investigate the labor market effects of employment bans—commonly imposed on *asylum seekers* awaiting for a decision on their asylum application—and report that such employment bans are detrimental to the labor market integration of refugee immigrants in the long run. Our focus here is instead on the effect of tougher permanent residency rules—in particular, the requirement of minimum years of employment experience—on the skill investment of *refugee immigrants* who have already been granted asylum (and received temporary residency). The present study therefore departs from Fasani et al. (2020) in terms of the policy’s target population (i.e. refugee immigrants vs.

⁴ See Dustmann and Gorlach (2016) for detailed discussion of this literature.

asylum seekers) as well as the margin of intervention (i.e. *requiring* substantial labor market engagement for permanent residency vs. *allowing* non-zero amount of employment while awaiting for asylum decisions).

The remainder of the paper is structured as follows. In the next section we provide some background on the 2007 reform in Denmark. Section 3 presents a simple stylized model that motivates the potentially heterogeneous effects of the reform by individuals' performance types. Section 4 presents our empirical strategy while the data are described in Section 5. Section 6 provides the results of our empirical analysis. Some concluding comments are provided in Section 7.

2. Denmark's Refugee Policy and the 2007 Reform

Persons fleeing from war can be granted protection in Denmark in the form of (temporary) residency either through asylum, subsidiary protection arrangements or family reunification.⁵ Residency is granted for one to three years and must be renewed by application.⁶ After a certain period of residency in the country, immigrants can apply for permanent residency. The eligibility criteria for permanent residency are described in section 11 of the Danish Aliens Act. Specifically, for all immigrants who applied for (temporary) residency after February 28, 2002, eligibility for permanent residency requires 7 years of stay in Denmark, documentation of a basic (i.e. the lowest possible) level of Danish language proficiency, and completion of an integration program. The integration program lasts for three years and consists of a language course (1.2 years of full-time studies) and employment support if unemployed. Documentation

⁵ The legal grounds for asylum are described in section 7 and 8 in the Aliens Act of Denmark. Family reunification is described in section 9.

⁶ A renewal requires that the grounds on which the refugees or family members were provided residency still hold. Residency can also be withdrawn prior to renewal if it is judged to be secure to return to the home country. If residency is withdrawn or is not renewed, the refugee must return to her home country, typically within 1 month. See the Aliens Act, section 33(1) and (2).

of language proficiency is acquired by passing a standardized test. The language test is available at three levels of proficiency: A basic, an intermediate and a high level.⁷ The test is offered twice a year and can be repeated against a payment.

The 2007 reform, the focus of this study, was intended to incentivize refugee immigrants' skill investment by conditioning the granting of "permanence" on stricter attainment goals. Its stated objective was "*...to send a strong signal of how important it is to find employment and learn the Danish language. [...] It is important to stress that the integration exam [i.e. the two new requirements described below, our adding] is not introduced to keep the number of immigrants low. On the contrary, it is a tool that ensures that immigrants understand that it pays off to make an effort*" (the Minister of Foreign Affairs and Integrations).⁸ The law was proposed on November 29, 2006, passed by Parliament on April 19 2007, and launched on May 1, 2007.⁹ In particular, it introduced two new requirements, in addition to the existing requirements of 7 years of stay in the country and completion of the 3-year integration program. The first of the new requirements is a universal employment condition, which requires 2.5 years of cumulative employment (in full-time equivalents). The second is requirement of Danish language proficiency at the intermediate level (as opposed to the previous, basic level). The new rules applied to those who had not yet completed the standard 3-year mandatory integration program by November 29, 2006.

Figure 1 illustrates the timing of the reform and the arrival cohorts who were unaffected and affected by the 2007 reform. Because the integration program must be initiated within one

⁷ The basic language level corresponds to the A2 level on the "Common European Framework of References for Languages" scale, where an individual is able to "communicate about simple and routine tasks ... on familiar matters". The intermediate level corresponds to the B1 level, where the individual is able "to interact with a degree of fluency ... that makes regular interaction with native speakers quite possible ... and can produce clear, detailed text on a wide range of subjects (<https://www.coe.int/en/web/language-policy/cefr>).

⁸ The speech made by the Minister at the proposal can be found here (in Danish): <https://www.ft.dk/samling/20061/lovforslag/L93/fremsaettelsestale.htm>. Accessed on July 2020.

⁹ The law proposal can be found here: <https://www.retsinformation.dk/eli/ft/20061XX00103> (in Danish). The final amendment to the law is: Consolidated Act No. 379 of 25 April 2007 on amendments of the act on integration of immigrants in Denmark. The Ministry of Integration and Foreign Affairs: <https://www.retsinformation.dk/eli/fta/2007/379> (in Danish). Accessed on July 2020.

month upon being granted (temporary) residency, anyone who received (temporary) residency in October 2003 or before could safely complete the 3-year program by the cutoff date (November 29, 2006) and hence was not subject to the 2007 reform. In contrast, for those who received (temporary) residency after November 2003, completion of the program by the cutoff date was not possible. We therefore focus on the cohorts receiving residency in January-October 2003 and January-October 2004 as our control and treatment groups, respectively, with respect to the 2007 reform.¹⁰

[Figure 1]

3. Theoretical Considerations

The 2007 reform in Denmark, aimed at enhancing the skill acquisition of refugee immigrants, is based on the conjecture that the benefit of permanent residency (i.e. expected higher lifetime income) should always dominate the cost of investing in the host country-specific human capital (i.e. language course attendance and skill acquisition through labor force engagement). As we show below, however, for some low productivity (or high effort cost) individuals, stricter eligibility criteria may potentially backfire, by creating disincentive effects if these individuals find fulfilment of the new restrictions too difficult/costly relative to the expected benefits. Therefore, whether reforms such as the one studied here will lead to the intended incentive effects or the opposite disincentive effects depends on the underlying capacity of individuals to meet the new requirements.

¹⁰ For the cohorts receiving residency in November 2003, their treatment status with respect to the reform is ambiguous. If they initiated the integration program during November 2003, then they could complete it by the cutoff date (November 29, 2006). On the other hand, if they initiated the program only in December 2003, then it would not be possible to complete it before the cutoff date. Therefore, our baseline excludes November 2003 and December 2003 cohorts. In our robustness analysis, we include these cohorts as part of our control and treatment groups, respectively.

To illustrate this, we develop a stylized two-period model, where we distinguish between two types of individuals, those who incur high cost and those who incur lower cost, for sitting language courses and participating in the labor market, which hereafter is referred to as “effort” or “investment”. In period 1, individuals choose the level of effort. This affects their current earnings as well as the probability of obtaining permanent residency. If acquiring permanent residency, individuals enjoy some benefit (e.g. labor earnings) that depends on the human capital acquired in period 1. For simplicity, we assume that individuals get nothing in period 2 without permanent residency.¹¹ Also for simplicity, we set the discount factor at unity.

Each individual chooses the level of effort h (e.g. labor supply) to maximize the following utility:

$$(1) \quad u(h; \theta) = wh - c(h; \theta) + \phi(h)B(h),$$

where $w > 0$ is the wage rate in period 1 and $c(h; \theta)$ is the cost of effort for worker of productivity type $\theta \in \{\theta_L, \theta_H\}$, where $\theta_L < \theta_H$. Assume that $c'(h) > 0, c''(h) > 0$ and $c'(h; \theta_L) > c'(h; \theta_H)$ such that the marginal cost of effort is decreasing in productivity θ . With some probability $\phi(h)$, the worker is granted permanent residency, in which case she is entitled to the second period benefit of $B(h) > 0$ (e.g. labor earnings or transfer income granted to permanent residents), with $B' > 0$ and $B'' < 0$.

Before the 2007 reform. In the status quo, there is some constant probability of obtaining permanent residency (after 7 years of stay) that is common for all effort levels:

$$\phi(h) = \rho > 0.$$

Therefore, the utility in (1) can be stated as:

$$(1a) \quad u(h; \theta) = wh - c(h; \theta) + \rho B(h).$$

¹¹ We could assign non-zero income here, too, in which case the benefit granted in the event of permanent residency should be viewed as the extra income over and above that attainable without permanent residency.

For each productivity type $\theta \in \{\theta_L, \theta_H\}$, the optimal choice of effort $h^o(\theta)$ satisfies:

$$(2) \quad w - c'(h^o(\theta); \theta) + \rho B'(h^o(\theta)) = 0.$$

From (2) and that $c'(h; \theta_L) > c'(h; \theta_H)$ and $B'' < 0$, we know that

$$h^o(\theta_L) < h^o(\theta_H).$$

That is, a high productivity person exerts higher effort than a low productivity person.

After the 2007 reform. The 2007 reform states that (in addition to 7 years of stay) only those who have accumulated some minimum levels of employment and language skills are now eligible for permanent residency. Denoting the threshold by \hat{h} , the post-reform probability of obtaining permanent residency is such that:

$$\phi(h) = \begin{cases} 0 & \text{if } h < \hat{h}, \\ \rho & \text{if } h \geq \hat{h}, \end{cases}$$

which leads to the following utility:

$$(1b) \quad u(h; \theta) = \begin{cases} wh - c(h; \theta) & \text{if } h < \hat{h}, \\ wh - c(h; \theta) + \rho B(h) & \text{if } h \geq \hat{h}. \end{cases}$$

Assume that the new restriction is binding such that

$$(3) \quad h^o(\theta_L) < h^o(\theta_H) < \hat{h}.$$

That is, under the new regime, if individuals kept to their old effort choices—which were individually optimal pre-reform—both the high and low productivity individuals would no longer be eligible for permanent residency. The relevant question then is whether and when these individuals would find it worthwhile increasing their effort to \hat{h} to maintain their permanent residency eligibility.

Responses to the reform by productivity types. To make the distinction of productivity types relevant, assume that high productivity individuals prefer maintaining eligibility for permanent residency (through supplying effort \hat{h}) to becoming ineligible for it such that:

$$wh^*(\theta_H) - c(h^*(\theta_H); \theta_H) < w\hat{h} - c(\hat{h}; \theta_H) + \rho B(\hat{h}),$$

where $h^*(\theta)$ on the LHS denotes type θ person's optimal effort choice for $h < \hat{h}$ (see (1b)) that satisfies the following condition:

$$(4) \quad w - c'(h^*(\theta); \theta) = 0.$$

A low productivity individual will choose to supply effort \hat{h} and stay eligible for permanent residency only if:

$$(5) \quad wh^*(\theta_L) - c(h^*(\theta_L); \theta_L) < w\hat{h} - c(\hat{h}; \theta_L) + \rho B(\hat{h}).$$

This condition can also be stated as:

$$c(\hat{h}; \theta_L) - c(h^*(\theta_L); \theta_L) < w\{\hat{h} - h^*(\theta_L)\} + \rho B(\hat{h}),$$

where the expression on the RHS shows the contemporaneous and expected future returns to the higher effort/investment whereas the expression on the LHS shows the increase in the cost due to the higher effort requirement.

If the cost and benefit functions are such that condition (5) is not satisfied, then the low productivity individual will choose an effort level of $h^*(\theta_L)$ (that satisfies (4) for $\theta = \theta_L$) and forgo the possibility of permanent residency. Note that in that case, the post-reform effort is *lower* than the pre-reform effort (that satisfies (2)) such that

$$(6) \quad h^*(\theta_L) < h^o(\theta_L).$$

The intuition is that in both (2) and (4), the optimal effort equates the marginal cost $c'(h(\theta); \theta)$ with the marginal benefit, and the marginal benefit in (4) is smaller than that in (2) since the future benefit $\rho B'(h(\theta))$ is no longer relevant when the individual has foregone the possibility of permanent residency altogether.

Ultimately, when permanent residency rules are tightened, individuals face newly arising tradeoffs. While permanence provides individuals with a higher expected lifetime income, effort required to reach that is costly. Therefore, for low productivity individuals for whom the expected returns to permanent residency eligibility may not be sufficiently large to

justify the steep rise in the cost of the requisite investment, the reform may end up lowering their labor supply and/or language acquisition, contrary to what the policy intended to achieve. Our empirical analysis below examines whether such possibility may be present in the data and to what extent.

4. Empirical Strategy

The new and stricter rules stipulated in the 2007 reform applied to those who had not yet completed the integration program by November 29, 2006. In our baseline, we therefore focus on individuals receiving (temporary) residency in January-October 2004 as our treatment group and those receiving (temporary) residency in January-October 2003 as our control group, following each cohort for seven years after residency. The 2007 reform comes into effect on May 1, 2007. Therefore, our treatment group is exposed to the reform beginning in their third year since residency (YSR) but not before. Regardless of the reform, our control group continues to be applied the old rules. We therefore examine whether there are any possible changes in the labor market behavior of the treatment group relative to that of the control group from YSR=3 onwards but not before.

Specifically, we estimate the following differences-in-differences (DID) equation:

$$(7) \quad y_{it} = \alpha_i + \psi_{\tau(it)} + \delta Post_{\tau(it)} \times T_i + X_{it}\beta + u_{it},$$

where y_{it} is the outcome of individual i in year t such as employment or passing of the Danish language test, α_i is an individual fixed effect (FE) and $\psi_{\tau(it)}$ is a dummy indicating that year t is individual i 's τ -th YSR. The variable T_i indicates whether individual i is in the treatment (vs. control) group, and $Post_{\tau(it)}$ is a dummy taking the value of 1 if $\tau (= YSR) \geq 3$ and zero otherwise. Note that the level effect of T_i is subsumed in the individual FE, α_i . Also, the level effect of $Post_{\tau(it)}$ is subsumed in the YSR FE, $\psi_{\tau(it)}$.

Our main coefficient of interest is δ , which shows the average difference in outcomes over YSR=3 to 7 (relative to YSR=1 to 2) between our treatment and control groups that are affected and unaffected by the 2007 reform, respectively. If the reform has an incentive (disincentive) effect on the effort choices of immigrants, δ will be positive (negative). In some specifications, we include X_{it} , a vector of additional controls that vary at the person-year level: (i) dummies for each of the recession years (2009-2011) and (ii) the annual unemployment, to account for aggregate labor market conditions in Denmark.

We also estimate an event study variant of (7), where we replace $Post_{\tau(it)}$ with YSR indicators, so that we can examine the year-by-year effect of the reform exposure as opposed to the average effect in the post-reform period. Specifically, using YSR=2 as the reference year, we estimate the following equation:

$$(8) \quad y_{it} = \alpha_i + \psi_{\tau(it)} + \sum_{\kappa=1(\kappa \neq 2)}^7 \delta^{\kappa} I(\tau(it) = \kappa) \times T_i + X_{it}\beta + e_{it}.$$

Since the reform comes into force on May 1, 2007, only 8 (out of 12) months of 2007 count towards the post-reform period, whereas for subsequent years, all 12 months amount to the post period. Given the partial reform exposure in 2007, we expect a modest effect in YSR=3, if at all, which is then expected to increase in YSR=4 onwards. Moreover, under the parallel trend assumption and in the absence of any anticipation effect, we would expect $\delta^1 = 0$.

We first estimate (7) and (8) in the overall sample, and then examine potentially differential responses by individuals' productivity types to explore the predictions of the model from Section 3. We consider two different performance measures related to the 2007 reform's new eligibility criteria as proxies for "productivity": (i) cumulative employment by YSR=2 and (ii) language acquisition by YSR=2. As we discuss above, at YSR=2 neither the treatment group nor the control group has been affected by the 2007 reform, and hence our classification of individuals into high vs. low types is based on their *pre-reform* performance only.

5. Data on Refugee Immigrants

Our analysis draws on data from population, income, education, and immigration registers in Denmark, which are collected for administrative purposes and linked by a personal identifier in the Central Person Registry. We focus on individuals who applied for (temporary) residency after February 28, 2002 to maintain common eligibility requirements for permanent residency in the absence of the 2007 reform (as those who applied for residency before February 28, 2002 were subject to different permanent residency rules, whose effect is analyzed in Kilström et al. (2018)). We also restrict attention to individuals who were aged between 18 and 64 at the time of receiving (temporary) residency.

Our main outcomes of interest are employment and passing of the Danish language test at the intermediate level or higher. In our data, we observe employment in annual full-time equivalents (FTE), where full-time employment for the whole year is recorded as unity. We observe this information for all individuals present in Denmark as of January 1 each year. In particular, for a person in the country as of January 1, 2007, all work hours in 2007 are recorded as her employment for the year 2007. Therefore, for those who received (temporary) residency sometime during 2003, we denote year 2004 as YSR=1, 2005 as YSR=2, etc. as we only observe their employment for 2004 onwards but not for 2003 (except for those who received residency exactly on January 1, 2003). We also observe individuals' language test results for YSR=1 onwards.

Tables 1 shows some descriptive statistics for our sample, background characteristics measured at YSR=1 and employment and language outcomes measured at YSR=2 and 7 as well as the average outcomes during YSR=1 to 7. Focusing on the overall sample, the first column in the table shows that at YSR=1, immigrants are on average 33 years old, about 60 percent are male, 55 percent are married, and 48 percent have children.

As of YSR=2, an average immigrant works for about 0.23 full-time equivalents (FTE) and 16 percent of our sample have passed the Danish test at the intermediate or higher level. By YSR=7, average annual employment rises to 35 percent of FTE and roughly 40 percent of individuals have passed the Danish test at the intermediate or higher level. In terms of cumulative employment by YSR=7, the mean (*SD*) is 2.20 (2.16) FTE, which is slightly below the requirement of the 2007 reform, i.e. 2.5 FTE of cumulative employment. Hence, for the average individual, it seems feasible to fulfill the new employment requirement with some extra effort. As the large variance implies, however, for individuals who have little engagement in the labor market, 2.5 years of cumulative employment experience may be a big challenge.

[Tables 1]

Columns 2 and 3 show the characteristics of our control and treatment cohorts, respectively. Given that our identification strategy is based on DID, what we maintain is a parallel *trend* assumption, that is, in the absence of the 2007 reform, the progression (in employment and language tests) of the treatment and control groups would have been comparable, though the two groups need not have the same *level* in baseline characteristics or outcomes. Nonetheless, it is instructive to examine the level differences between the two groups. While the baseline characteristics are rather similar, the share of immigrants originating from Myanmar is substantially higher in our treatment group than in the control group.¹² In our robustness analysis, we therefore consider the role of excluding individuals of Myanmar origin from our sample.

¹² A large share of refugees from Myanmar are re-settled by agreement with the UNHCR. Denmark started to re-settle refugees from Myanmar in 2003. Like our numbers, the total number of persons from Myanmar who were resettled in Denmark doubled from 2003 to 2004 (from 59 to 199), <https://www.unhcr.org/resettlement-data>.

6. The 2007 Reform and Refugee Immigrants' Skill Investment

6.1 Overall effects

Table 2 shows the estimates of the reform effect (the parameter δ from equation 7) in the overall sample. Columns 1-3 report estimates of δ with annual employment (in FTE) as the outcome while columns 4-6 use passing of the language test at the intermediate level or higher as the dependent variable. In column 1, the estimate of -0.09 suggests that, evaluated at the mean reported in Table 1, the treated cohorts *reduce* their employment hours by about 30 percent (-0.09/0.31) in response to the reform – an effect that is opposite to the objective of the reform. Turning to column 4, the estimates show that the treated cohorts have a 3-percentage point or 11 percent (0.03/0.27) higher probability of passing the language test although estimates are statistically insignificant. When we include dummies indicating each of the recession years in our sample, 2009-2011 (columns 2 and 5) or annual unemployment rates (columns 3 and 6), the results change little.

[Table 2]

To investigate the timing of responses, we next estimate the event study specification in equation (8). The estimated coefficients are plotted in Figure 2, where the coefficient for YSR=2 is normalized to zero. Panel (a) shows the results for employment and panel (b) for language test results. In the case of employment, the differential outcomes for the treatment group relative to the control group increases from YSR=2 to YSR=3 when the reform sets in and becomes significant from YSR=4 onwards. The smaller response at YSR=3 is as expected since treatment only takes place in part of that year (from May onwards, see section 4). In the case of language acquisition, small positive effects from YSR=4 onwards are visible although they are not statistically significant. Again, this delay is to be expected, as language test results

(with exams taking place only twice a year) require sitting language classes, which are unlikely to be completed in the remaining months in YSR=3 after the reform has set in.

[Figure 2]

6.2 Robustness

Our empirical strategy boils down to a comparison of labor supply and language skills observed during YSR=3-7 (i.e. years 2007-2011) for the treatment cohort with the same outcomes observed during YSR=3-7 (i.e. years 2006-2010) for the control cohort (relative to the corresponding difference during YSR=1-2). A potential concern of this strategy is that the treatment group, who enters the Danish labor market one year later than the control group, may be hit more severely by the recession commencing in 2009 than the control group, following the last-in-first-out principles.

To address this concern, we use Danish natives who have labor market profiles comparable to that of refugee immigrants in our sample and conduct a triple difference analysis. We focus on Danes aged 20-40 who are unemployed for two consecutive years prior to 2004 (i.e. YSR=1 for the control group) and 2005 (i.e. YSR=1 for the treatment group), respectively. That is, natives in the control (treatment) group are unemployed during 2002-2003 (2003-2004) while employed or in education in 2001 (2002). In the register data, unemployment refers to the labor market status in November in the previous year. Therefore, an individual coded as “unemployed” in 2003 may still have high full-time equivalents (FTE) during that year. We thus additionally require the natives to have FTE=0 in 2003 for the control group and in 2004 in the treatment group. We then follow these natives for six years, beginning in 2004 for the control group and 2005 for the treatment group (i.e. YSR=1 for the control and treatment

groups, respectively).¹³ The summary statistics on these natives are provided in Appendix Table A1. The average annual employment in this native sample is 0.33 (in full-time equivalents), which is comparable to that for our refugee sample, 0.31 (see panel B of Table 1).

We plot the trends in annual employment of the treatment vs control groups, separately by the refugee and native samples (Figure 3). In the refugee sample (panel (a)), while the trends are largely parallel initially, between YSR=3 and YSR=4 (when the 2007 reform kicks in for the treatment group) the performance of the treatment group becomes weaker relative to that of the control group. In contrast, in the native sample (panel (b)), the weakening of performance by the “treated” relative to the “control” occurs between YSR=4 and YSR=5. That the treatment and control difference in trends happens at different YSR’s for the refugees and natives suggests that what we observe in our refugee sample is unlikely driven by the recession effect alone.

In Table 3, we present estimates using a triple-difference approach. Columns 1-3 replicate DID estimates for refugees in Table 2, while columns 4-6 show estimates of equation (7) but among natives. Columns 7-9 then display the refugee-native difference in the DID estimates. These estimates confirm that our DID estimates for the refugees are not driven by the differential effect of the recession on the treated (later entering) cohorts.¹⁴

[Figure 3]

[Table 3]

We conduct further checks, examining the robustness of our main finding to various sample restrictions (Table 4). Panel A displays results when using employment as an outcome

¹³ Due to data constraints in our native sample, we follow them up to six years instead of seven (which is the case for our refugee sample).

¹⁴ These results are robust to alternative choices of the Danish native sample, using for instance Danes unemployed for 1 or 3 consecutive years prior to YSR=1, instead of 2 consecutive years as used here.

whereas the corresponding results for language test results are provided in Panel B. In our main analysis, we include January-October 2003 and January-October 2004 cohorts as our control and treatment groups, respectively, since treatment status is ambiguous if arriving in November or December 2003. In column 1, we include November 2003 and December 2003 cohorts as part of our control and treatment groups, respectively. The result is very similar to our main estimate (column 1, Table 2). In column 2, we exclude Myanmar as their share in the treatment and control groups is rather unbalanced as we pointed out earlier. In column 3, we restrict the sample to the balanced panel of individuals whom we observe for all seven years (which is 91% of our sample). Both sets of estimates are very similar to that based on the full sample, suggesting that neither differential composition with respect to country of origin nor differential attrition between treatment and control groups are driving our main results. The corresponding estimates for language as dependent variable are provided in columns 4-6. The effects on language are similar to what we estimated based on the main sample in Table 2.

[Table 4]

To ensure that the effects are specific to the 2004 (vs. 2003) cohorts who were affected (vs. not affected) by the 2007 reform, we next conduct a series of placebo analyses, see Table 5. Column 1 shows the estimate of equation (7), where we use those receiving residencies in January-October 2001 and January-October 2002 as the control and treatment groups with respect to a hypothetical reform affecting the latter cohorts beginning in YSR=3. In column 2, we use January-October 2002 and January-October 2003 residency cohorts as the control and treatment groups. In both columns, there is no differential employment responses in YSR=3+ by the later arriving cohorts. This contrasts with our main estimate of the 2007 reform effect, which is shown in column 3.¹⁵ This analysis shows that our main effect is indeed driven by the

¹⁵ The sample size is larger in columns 1 and 2 than in column 3, since Denmark had a larger number of persons being granted residency in 2001 and 2002. We do not have information on language tests prior to 2004, hence we cannot conduct placebo tests for language outcomes.

2007 reform, rather than picking up differential employment profiles that may be present between cohorts arriving in any two consecutive years.

[Table 5]

Overall, the results so far suggest that the 2007 reform led to a decrease in employment hours while slightly increasing language test results among refugee immigrants who were subject to the tougher requirements for permanent residency. At first glance, the effects on employment and language test results seem at odds with each other. Moreover, the reduction in employment is contrary to what the reform was intended to achieve, suggesting a disincentive rather than an incentive effect. We next examine the possibility that the reform had heterogeneous effects along the distribution of refugees' productivity (proxied by performance), where those for whom compliance seems too costly simply give up on the prospect to achieve permanent residence (see Section 3).

6.3 Heterogeneous responses by performance types

We consider two different performance measures directly related to the new requirements stipulated in the 2007 reform: (i) cumulative employment by YSR=2 and (ii) language test results by YSR=2. We then examine whether worker responses to the 2007 reform differ by individuals' performance types.

Classifying workers into performance types. In Figure 4, we plot the distribution of cumulative employment (in FTE) by YSR=2. As the figure shows, a majority of refugee immigrants have not worked at all after two years in the country. Based on this pattern, we classify an individual as being in the "high performance" group (HIGH) if her cumulative employment by YSR=2 is above 0.75 FTE; otherwise she is classified as being in the "low performance" group (LOW). We choose the cutoff of 0.75 as it constitutes a non-negligible

part of the requirement of 2.5 FTE. With this threshold, 20% of our sample are in the HIGH group and 80% in the LOW group. As Figure 5 shows, based on the sample of the control group, those we classify in the HIGH group can easily fulfill the 2.5 FTE of cumulative employment by YSR=7, as required by the 2007 reform. In contrast, a very substantial fraction (nearly 40%) of the LOW group has not fulfilled the employment requirement by YSR=7.

[Figures 4 and 5]

As a second dimension of performance, we consider a sample split by the immigrants' language test results by YSR=2: those who already passed the Danish language course at the intermediate level (or higher), and those who did not. Only about 16 percent of individuals pass the Danish course at the intermediate level or higher at YSR=2. Among those who, as of YSR=2, have not yet passed the language course at the intermediate level, 27 percent pass it by YSR=7.

Heterogeneity in responses to the reform. In Table 6, we present the estimates of (7) for different subsamples of low and high (pre-treatment) performance types. The dependent variable in columns 1-4 is employment (in FTE), and in columns 5-6 language test results (passing the Danish course at an intermediate level or higher). Columns 1-2 split the sample by employment performance and columns 3-4 by language performance, both assessed at YSR=2 (pre-reform) as discussed above. Focusing on Panel A, the disincentive effects on employment are driven by the low performance individuals. Evaluated at the mean (0.22), the reform reduced their annual employment by 32 percent (0.07/0.22). In comparison, the insignificant negative effect for high performance individuals corresponds to a 7 percent reduction (0.05/0.66). Defining performance as having passed the language test at YSR=2 (columns 3 and 4), those who have not successfully done so reduce their annual employment by 37 percent (column 3), whereas the reform has no effect on employment for those who have

passed the language test by $YSR=2$ (column 4). Moreover, column 6 shows that individuals with high employment performance become more likely to pass the language test in response to the reform, while there is no effect on language test results for individuals with low employment performance (column 5).¹⁶ Based on columns 2 and 6, it seems that individuals with high pre-reform employment performance allocate more effort to language acquisition, as they are yet to fulfill the new language requirement, since they will have surpassed the new employment requirement in any case. For individuals with little labor market attachment prior to the reform, there is no sign of effort diversion to language acquisition (column 5) and rather, they seem to even withdraw from work (column 1).

[Table 6]

We now consider alternative ways of slicing the sample, based on both employment and language performance by $YSR=2$. There are four possible combinations of pre-reform employment and language performance: low/low (68% of the sample), low/high (13%), high/low (16%), and high/high (4%). The group who performs poorly on both dimensions therefore constitutes more than two-thirds of the sample. Table A3 presents estimates of a variant of equation (7). Although subject to the caveat that some of the groups become small, the table shows that the negative employment effect is driven by those with low performance in both dimensions (column 1), whereas the positive effect on language test results is driven by those who are on course for fulfilling the employment requirement but yet to fulfill the language requirement (column 2).¹⁷ This is consistent with our earlier conjecture that low performance individuals for whom the cost of fulfilling the new requirements is high may give

¹⁶ In Table A2, we examine re-taking of the language test as a possible mechanism. We find that while overall the reform had no effect on the likelihood to re-take the test, high performance individuals become 2.6 times $((0.06+0.038)/0.038$, column 2) more likely than before to re-take the language test, thus responding to the incentive effects induced by the reform. On the other hand, low performance individuals show – if anything – a reduction in their probability to re-take the test in line with the interpretation that the 2007 reform had disincentive effects for individuals with a high cost of effort.

¹⁷ Notice that the highest performing individuals in terms of language have already passed the post-reform language requirement, by construction.

up entirely whereas the reform may indeed incentivize those who have realistic chances of meeting the tougher requirements with some extra effort.

6.4 Overall Implications

So far, we have focused on employment measured by annual FTE and passing of the language test (at the intermediate or higher level) as the key outcomes of interest. For a fuller understanding of the reform's consequences, we conduct further analysis examining outcomes beyond language and employment, Table 7. Column 1 replicates our main results on annual employment (column 1 of Table 2). In columns 2 and 3, we decompose this employment outcome (in FTE) into the extensive and intensive margins. The estimates show that both margins seem to matter, with individuals reducing their labor market participation as well as their hours worked conditional on working. As a response to reduced labor market attachment induced by the reform, we find that the treated cohorts are more likely to receive welfare benefits than the control cohorts in the post-reform years (column 4). Therefore, when we look at the incomes of refugee immigrants from different sources, we find that while exposure to the reform decreases labor income (column 5) it increases income from welfare benefits (column 6), leading to little effect on their overall disposable income (column 7). Overall, the reform aimed at incentivizing refugee immigrants' labor market attachment seems to have achieved the opposite effect, a result driven by the fact that a majority of refugee immigrants have low capacity to meet the tougher employment and language requirements for obtaining permanent residency.

[Table 7]

7. Conclusions

We study the consequences of an immigration reform in Denmark that tightened refugee immigrants' eligibility criteria for obtaining permanent residency. In addition to 7 years of residence in the country, the reform newly required 2.5 years of cumulative employment experience (in annual FTE) as well as passing the Danish language test at the intermediate level (rather than basic level, as previously required). Contrary to what the reform intended we find that employment of the treated cohorts decreased relative to that of the control cohorts, a decline driven by the response of low productivity individuals, particularly those with jointly low employment and language potential, who constitute two-thirds of the sample.

The unintended consequence of the 2007 reform illustrates that policies that are aimed at improving refugee immigrants' skill acquisition by rewarding a specific performance will be effective only if the bar is set at an appropriate level. If the requirements are deemed too costly to fulfill, the reform could be ineffective or result in disincentive effects. This is a particular concern for populations that are badly prepared for the host country's labor market. Our paper advocates that policies aimed at improving immigrants' labor market outcomes need to carefully consider potential disincentives that may discourage individuals and possibly lead to outcomes inferior to those obtained in the absence of the policy.

References

- Adda, Jérôme, Paolo Pinotti and Giulia Tura (2020). There's more to marriage than love: The effect of legal status and cultural distance on intermarriages and separations. CEPR Discussion Paper No. DP14432.
- Albert, Christoph, Albrecht Glitz, and Joan Llull (2020). Labor market competition and the assimilation of immigrants. Working paper, Universitat Pompeu Fabra.
- Andersen, Lars Højsgaard, Christian Dustmann and Rasmus Landersø (2019). Lowering welfare benefits: Intended and unintended consequences for migrants and their families. The ROCKWOOL Foundation, Study paper 138.
- Arendt, Jacob Nielsen (2018). Integration and permanent residence policies – a comparative pilot study. The ROCKWOOL Foundation, Study Paper no. 130.
- Arendt, Jacob Nielsen, Iben Bolvig, Mette Foged, Linea Hasager and Giovanni Peri (2020). Integrating Refugees: Language training or work-first initiatives? NBER Working Paper No. 26834.
- Ben-Porath, Yoram (1967). The production of human capital and the life cycle of earnings. *Journal of Political Economy*, 75 (4, Part 1): 352-365.
- Becker, Gary S. (1964). *Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education*. University of Chicago Press: Chicago.
- Bleakley, Hoyt, and Aimee Chin (2004). Language skills and earnings: Evidence from childhood immigrants. *Review of Economics and Statistics*, 86(2): 481-496.
- Bleakley, Hoyt, and Aimee Chin (2010). Age at arrival, English proficiency, and social assimilation among US immigrants. *American Economic Journal: Applied Economics*, 2(1): 165-92.

- Borjas, George (1987). Self-selection and the earnings of immigrants. *American Economic Review*, 77 (4): 531-555.
- Bratsberg, Bernt, James F. Ragan and Zafar M. Nasir (2002). The effect of naturalization on wage growth: A panel study of young male immigrants. *Journal of Labor Economics*, 20 (3): 568-597.
- Brell, Courtney, Christian Dustmann, and Ian Preston (2020). The labor market integration of refugee migrants in high-income countries. *Journal of Economic Perspectives*, 34 (1): 94-121.
- Cadena, Brian C., Brian Duncan, and Stephen J. Trejo (2015). The labor market integration and impacts of US immigrants. In *Handbook of the Economics of International Migration*, vol. 1, pp. 1197-1259. North-Holland.
- Chiswick, Barry R. (1978). The effect of Americanization on the earnings of foreign-born men. *Journal of Political Economy*, 86 (5): 897-921.
- Clausen, Jens, Eskil Heinesen, Hans Hummelgaard, Leif Husted, Michael Rosholm (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.
- Cortes, Kalena E. (2004). Are refugees different from economic immigrants? Some empirical evidence on the heterogeneity of immigrant groups in the United States. *Review of Economics and Statistics*, 86 (2): 465-480.
- Devillanova, Carlo, Francesco Fasani and Tommaso Frattini (2018). Employment of undocumented immigrants and the prospect of legal status: Evidence from an amnesty program. *Industrial and Labor Relations Review*, 71 (4): 853-881.
- Dustmann, Christian and Joseph-Simon Görlach (2016). The economics of temporary Migrations. *Journal of Economic Literature*, 54 (1): 98-136.

- Edin, Per-Anders, Peter Fredriksson, and Oluf Åslund (2003). Ethnic enclaves and the economic success of immigrants—evidence from a natural experiment. *Quarterly Journal of Economics*, 118: 329–57.
- Fasani, Francesco (2018). Immigrant crime and legal status: Evidence from repeated amnesty programs. *Journal of Economic Geography*, 18 (4): 887-914.
- Fasani, Francesco, Tommaso Frattini and Luigi Minale (2018). (The struggle for) Refugee integration into the labour market: Evidence from Europe, IZA Discussion Papers, No. 11333.
- Fasani, Francesco, Tommaso Frattini and Luigi Minale (2020). Lift the ban? Initial employment restrictions and refugee labour market outcomes, CReAM Discussion Paper, No. 10/20.
- Felfe, Christina, Helmut Rainer, and Judith Saurer (2020). Why birthright citizenship matters for immigrant children: Short-and long-run impacts on educational integration. *Journal of Labor Economics* 38 (1), 143-182.
- Gathmann, Christina and Nicolas Keller (2017). Access to citizenship and the economic assimilation of immigrants. *The Economic Journal*, 128 (616): 3141-3181.
- Hainmueller, Jens, Dominik Hangartner and Giuseppe Pietrantuono (2020). Catalyst or crown: Does naturalization promote the long-term social integration of immigrants? *American Political Science Review*, forthcoming.
- Hernes, Vilde (2018). Cross-national convergence in times of crisis? Integration policies before, during and after the refugee crisis. *West European Politics*, 41 (6), 1305-1329.
- Hvidtfeldt, Camilla and Marie Louise Schultz-Nielsen (2017). Refugees and asylum seekers in Denmark 1992-2016 (in Danish). ROCKWOOL Foundation Research Unit, Arbejdspapir nr. 50.

- Khourshed, May, and Romuald Méango (2020). Language proficiency and economic incentives: The case of Syrian asylum seekers in Germany. MEA Discussion Paper No. 01-2020.
- Kilstrom, Matilda, Birthe Larsen and Elisabet Olme (2018). Should I stay or must I go? Temporary protection and refugee outcomes. working paper, Copenhagen Business School.
- Lubotsky, Darren (2007). Chutes or ladders? A longitudinal analysis of immigrant earnings. *Journal of Political Economy*, 115 (5): 820-867.
- OECD (2018). *International Migration Outlook 2018*, OECD Publishing, Paris, https://doi.org/10.1787/migr_outlook-2018-en.
- Pinotti, Paolo (2017). Clicking on heaven's door: The effect of immigrant legalization on crime. *American Economic Review* 107 (1), 138-68.
- Pinotti, Paolo and Giovanni Mastrobuoni (2015). Legal status and the criminal activity of immigrants. *American Economic Journal: Applied Economics* 7 (2), 175-206.
- Sarvimäki, Matti (2017). Labor market integration of refugees in Finland. VATT research reports 185.
- Sarvimäki, Matti, and Kari Hämäläinen (2016). Integrating immigrants: The impact of restructuring active labor market programs. *Journal of Labor Economics*, 34 (2): 479-508.

Table 1. Individual characteristics and employment and language outcomes

	Overall		Treatment group		Control group	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
<i>A. Individual characteristics</i>						
Male	0.59	0.49	0.56	0.50	0.61	0.49
Age	33.10	10.30	32.70	9.80	33.30	10.60
Married	0.55	0.50	0.56	0.50	0.54	0.50
Have children	0.48	0.50	0.54	0.50	0.44	0.50
Country of origin:						
Afghanistan	0.13	0.33	0.12	0.33	0.13	0.34
Iran	0.06	0.23	0.09	0.29	0.03	0.18
Iraq	0.06	0.24	0.03	0.17	0.08	0.27
Myanmar	0.13	0.34	0.21	0.41	0.08	0.27
Somalia	0.06	0.24	0.06	0.23	0.07	0.25
Former republic of Yugoslavia	0.05	0.23	0.09	0.28	0.03	0.17
Other countries	0.51	0.5	0.41	0.49	0.58	0.49
<i>B. Outcomes at various YSR (2, 7, and 1-7)</i>						
Annual employment (in FTE)						
YSR=2	0.23	0.35	0.27	0.39	0.2	0.33
YSR=7	0.36	0.43	0.35	0.44	0.37	0.43
YSR=1 to 7	0.31	0.41	0.31	0.42	0.31	0.41
Passed language requirement						
YSR=2	0.16	0.37	0.17	0.38	0.16	0.36
YSR=7	0.39	0.49	0.41	0.49	0.37	0.48
YSR=1 to 7	0.27	0.45	0.29	0.45	0.26	0.44
Cumulative employment (in annual FTE)						
YSR=2	0.35	0.60	0.44	0.70	0.29	0.52
YSR=7	2.20	2.16	2.20	2.33	2.21	2.03
YSR=1 to 7	1.08	1.53	1.14	1.63	1.04	1.46
Person-year observations	5122		2090		3032	
Unique individuals	767		314		453	

Notes: Sample means and standard deviations in the overall sample and by treatment status. The treatment group received temporary residency in Jan-Oct 2004 while the control group received temporary residency in Jan-Oct 2003. Annual employment is measured in full-time equivalents (FTE), where FTE=1 corresponds to full-time employment for the full year. "Passed language requirement" is an indicator of having passed the Danish course at the intermediate or high level. YSR is years since temporary residency.

Table 2. Effects of reform exposure on employment and language skills of refugee immigrants

	Dependent var.:					
	(1)	Employment (2)	(3)	(4)	Language (5)	(6)
Treated × Post	-0.090*** (0.025)	-0.072*** (0.023)	-0.069*** (0.024)	0.033 (0.027)	0.031 (0.0267)	0.025 (0.026)
Recession years			Yes			Yes
Unemployment rate		Yes			Yes	
YSR FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5122	5122	5122	5122	5122	5122

Notes: Estimates of equation (7) are presented. Treated is an indicator of cohorts receiving temporary residency in Jan-Oct 2004 (vs. Jan-Oct 2003). Post indicates whether the YSR is 3 or later. Employment is annual employment in full-time equivalents. Language is an indicator of having passed the Danish test at the intermediate or the high level. Unemployment rate is the annual gross unemployment rate. Recession years are dummies for each of the recession years (2009-2011). Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3. Effects of reform exposure on employment of refugee immigrants vs. Danish natives

	Dependent var.: Employment								
	<i>DID (refugees)</i>			<i>DID (natives)</i>			<i>DDD (refugees - natives)</i>		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated × Post × Refugee							-0.054*** (0.025)	-0.060*** (0.025)	-0.070*** (0.025)
Treated × Post	-0.090*** (0.025)	-0.072*** (0.023)	-0.069*** (0.024)	-0.036*** (0.004)	-0.012*** (0.005)	0.001 (0.005)			
Recession years			Yes			Yes			Yes
Unemployment rate		Yes			Yes			Yes	
YSR FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations		5,122			66,044			71,166	

Notes: Refugees are those underlying our main analysis in Table 2. Natives are Danes aged 20-40 who are unemployed for two consecutive years prior to 2005 (i.e. YSR=1 for the treatment group) and 2004 (i.e. YSR=1 for the control group), with FTE=0 the year before YSR=1. Columns 1-3 report estimates of equation (7) among refugees while columns 4-5 report that for natives. Columns 7-9 show the results of the triple difference estimates, i.e. difference between columns 1-3 and columns 4-6. Treated indicates the individual's treatment status within the refugee and native samples, respectively. Post indicates whether the YSR is 3 or later. Refugee indicates whether the individual is in our refugee (vs. native) sample. Employment is annual employment in full-time equivalents. Unemployment rate is the annual gross unemployment rate. Recession years are dummies for each of the recession years (2009-2011). Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4. Effects of reform exposure on employment and language skills of refugee immigrants:
Robustness to sample restrictions

	Dependent var.					
	Employment			Language		
	(1)	(2)	(3)	(4)	(5)	(6)
	Include Nov-Dec 2003	Without Myanmar	Balanced panel	Include Nov- Dec 2003	Without Myanmar	Balanced panel
Treated × Post	-0.094*** (0.024)	-0.101*** (0.026)	-0.086*** (0.025)	0.035 (0.027)	0.042 (0.030)	0.031 (0.028)
Mean of dep.var.	0.309	0.277	0.313	0.274	0.290	0.282
YSR FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5474	4430	4888	5474	4430	4888

Notes: Estimates of equation (7) are presented. Columns 1-3 use annual employment (in FTE) as the dependent variable while columns 4-6 use passing of the language test at the intermediate or the high level as the dependent variable. Treated is an indicator of cohorts receiving temporary residency in Jan-Oct 2004 (vs. Jan-Oct 2003). Post indicates whether the YSR is 3 or later. Columns (1) and (4) include the November 2003 cohort in the control group and December 2003 in treatment group. Columns (2) and (5) exclude individuals whose country of origin is Myanmar. Columns (3) and (6) restrict analysis to the balanced sample where the individual is observed in all seven YSR. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5. Effects of placebo reforms on employment of refugee immigrants

	Dependent var.: Employment		
	(1) Placebo 1	(2) Placebo 2	(3) Actual
Treated × Post	-0.010 (0.012)	-0.019 (0.018)	-0.090*** (0.025)
YSR FE	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Observations	23998	11159	5122

Notes: Estimates of equation (7) are presented using annual employment (in full-time equivalents) as the dependent variable. Post indicates whether the YSR is 3 or later. In column 1, Treated is an indicator of cohorts receiving temporary residency in Jan-Oct 2002 (vs. Jan-Oct 2001). In column 2, Treated indicates cohorts receiving temporary residency in Jan-Oct 2003 (vs. Jan-Oct 2002). Column (3) replicates the main reform effects from column (1) of Table 2. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6. Heterogeneous responses to the reform by pre-reform (YSR=2) employment and language performance

	Dependent var.:					
	Employment				Language	
	(1) LOW	(2) HIGH	(3) LOW	(4) HIGH	(5) LOW	(6) HIGH
<i>Pre-reform employment:</i>						
<i>Pre-reform language:</i>						
Treated × Post	-0.070*** (0.026)	-0.050 (0.059)	-0.109*** (0.030)	-0.002 (0.065)	0.001 (0.030)	0.148** (0.061)
Mean of dep.var.	0.223	0.663	0.295	0.373	0.263	0.321
YSR FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4129	993	4265	857	4129	993

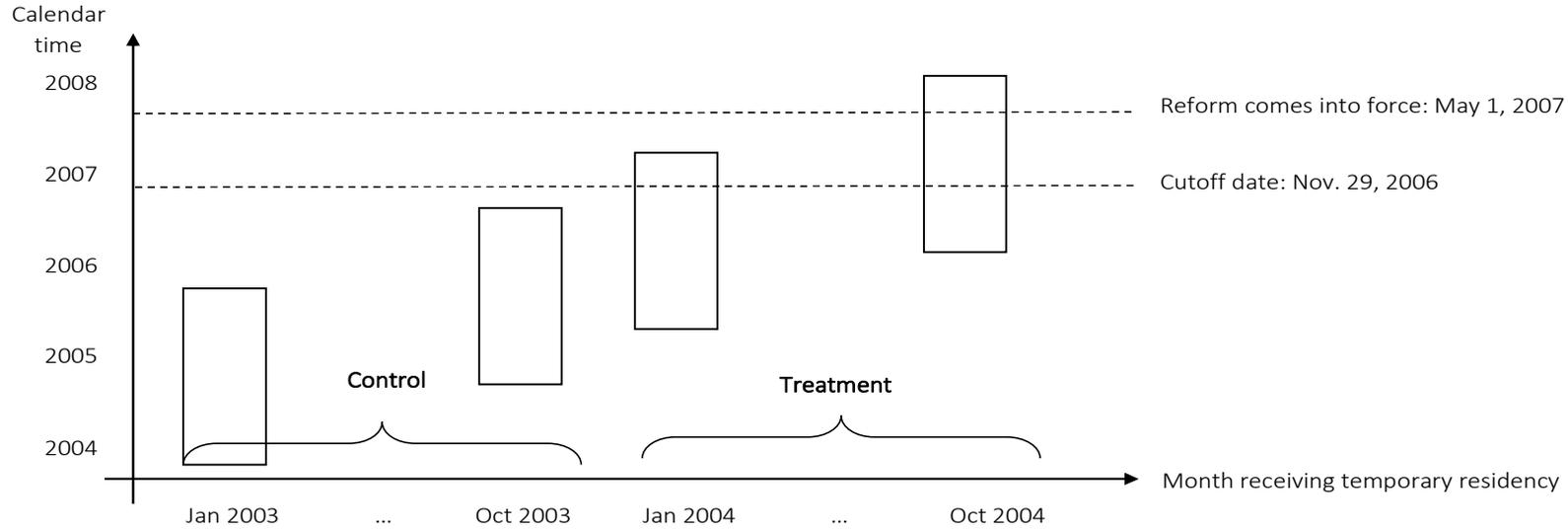
Notes: Estimates of equation (7) are presented. Columns 1-4 use annual employment (in FTE) as the dependent variable while columns 5-6 use passing of the language test at the intermediate or the high level as the dependent variable. Treated is an indicator of cohorts receiving temporary residency in Jan-Oct 2004 (vs. Jan-Oct 2003). Post indicates whether the YSR is 3 or later. With respect to pre-reform employment performance, an individual is classified as HIGH if her cumulative employment (in annual FTE) by YSR=2 is at least 0.75 and as LOW otherwise. With respect to pre-reform language performance, an individual is classified as HIGH if she passes the Danish language test at least at the intermediate level by YSR=2 and as LOW otherwise. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 7. Further consequences of the reform

	Dependent var.:						
		<i>Employment</i>			<i>Income</i>		
	in annual FTE	extensive margin	intensive margin	welfare receipts	labor income	welfare benefits	disposable income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated × Post	-0.090*** (0.025)	-0.117*** (0.0299)	-0.072*** (0.039)	0.080*** (0.030)	-1.171*** (0.299)	0.654*** (0.248)	-0.086 (0.068)
Mean of dep.var.	0.293	0.433	0.712	0.533	5.226	4.204	9.936
YSR FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5122	5122	2218	5122	5122	5122	5122

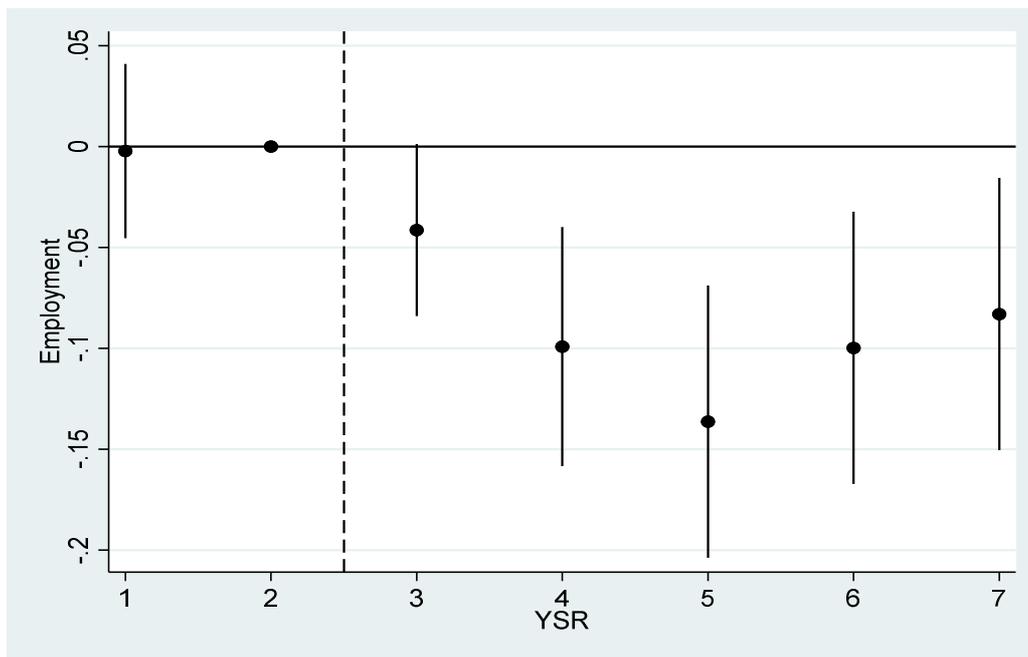
Notes: Estimates of equation (7) are presented, using different dependent variables. Column 1 uses employment in annual FTE. Columns 2 and 3 measure employment on the extensive and intensive margins, respectively, where the intensive margin is expressed in FTE. Column 4 uses an indicator for welfare receipts. Columns 5-7 use $\text{Log}(1+\text{income})$ as the dependent variable, where the income refers to labor income, welfare benefits and total disposable income, respectively. All incomes are annual income in 2018 USD. Treated is an indicator of cohorts receiving temporary residency in Jan-Oct 2004 (vs. Jan-Oct 2003). Post indicates whether the YSR is 3 or later. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 1. Timeline of the 2007 reform and the treatment and control groups

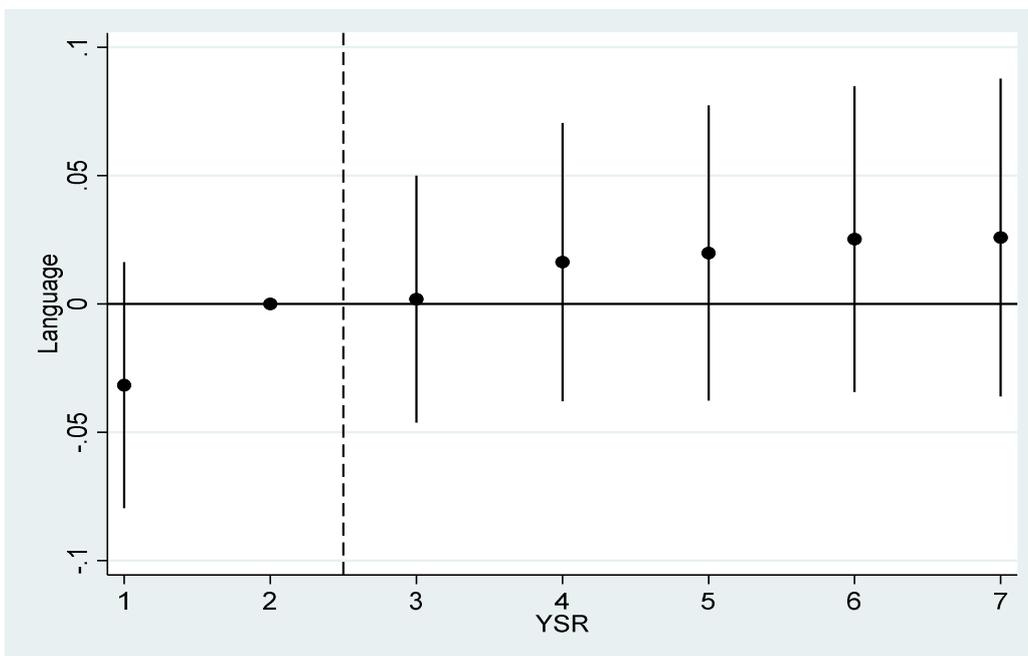


Notes: The rectangular boxes show the duration of the 3-year integration program for each cohort. Cohorts who have not yet completed the integration program by November 29, 2006 are subject to the new permanency residency rules coming into force from May 2007. Cohorts who have completed the integration program by the cutoff date of November 29 2006 continue to be applied the old permanent residency rules.

Figure 2. Event study estimates of the reform effect



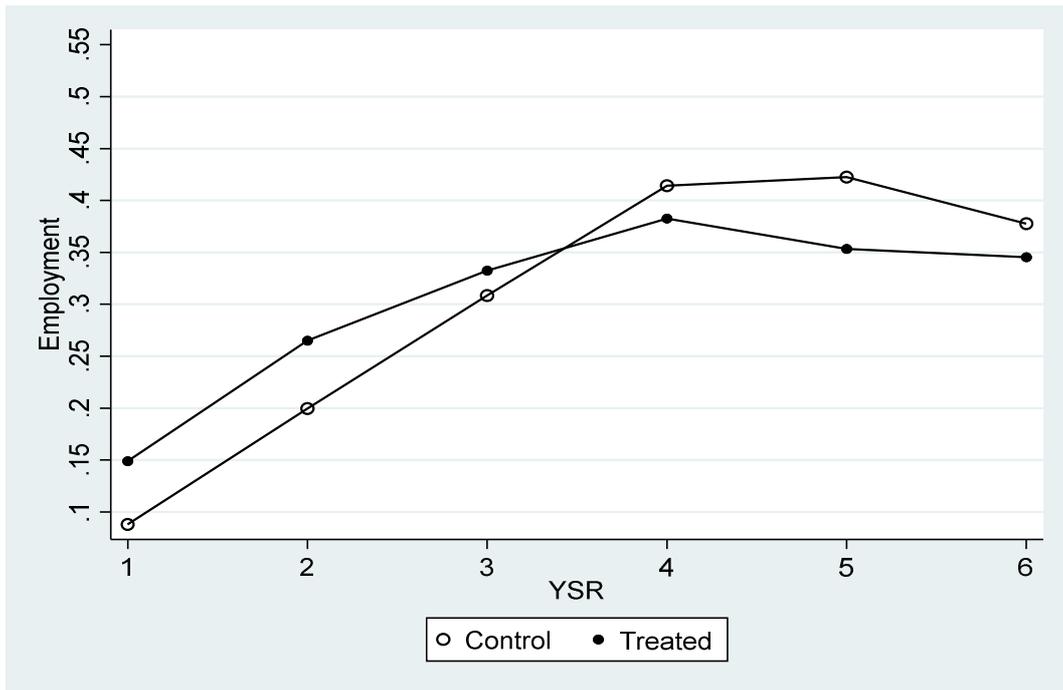
(a) Employment



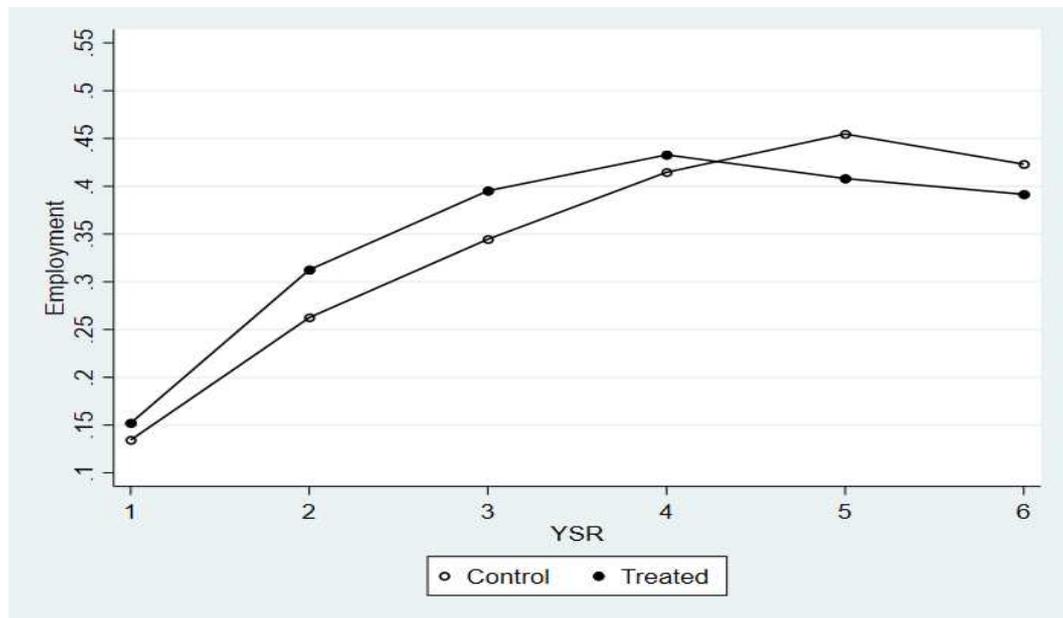
(b) Language

Notes: Estimated coefficients based on equation (8) are plotted along with 95 percent confidence intervals. Panel (a) uses annual employment (in full-time equivalents) as the dependent variable while panel (b) uses passing of the language test at the intermediate or higher level as the outcome variable. The dashed vertical line shows the time when the reform comes into force in the middle of YSR=3.

Figure 3. Trends in employment for refugees and natives



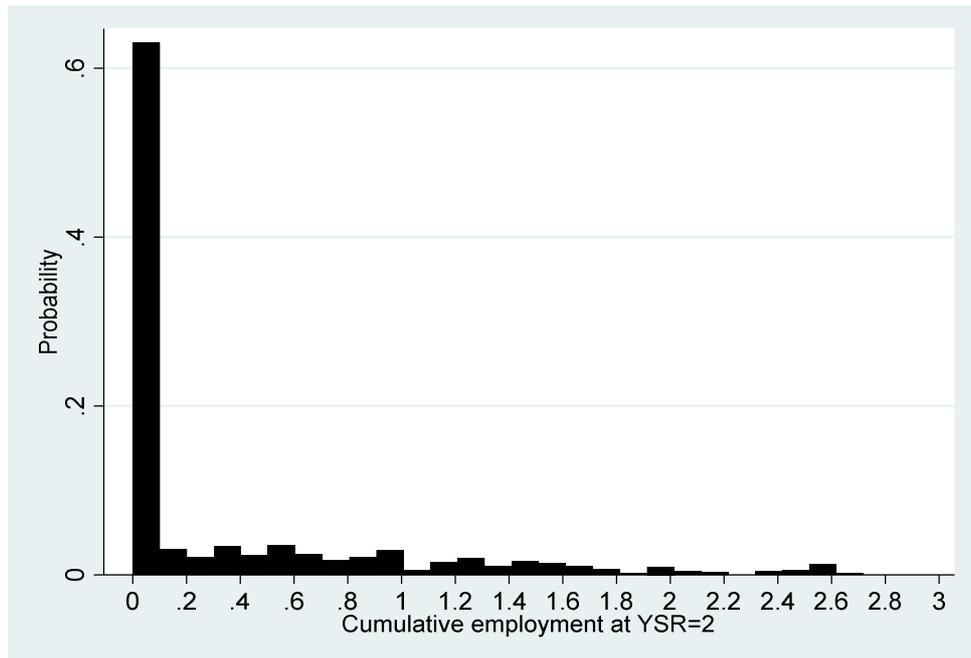
(a) Refugees



(b) Natives

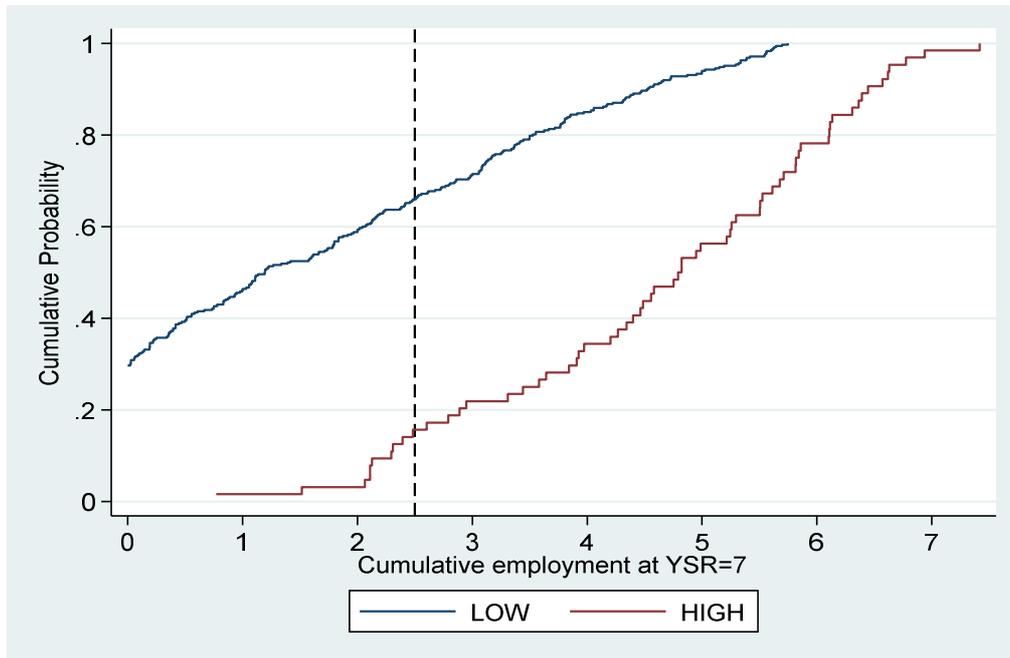
Notes: This figure shows the trends in annual employment (in FTE) of the treatment vs control groups, separately by the refugee and native samples. Refugees are those underlying our main analysis in Table 2. Natives are Danes aged 20-40 who are unemployed for two consecutive years prior to 2005 (i.e. YSR=1 for the treatment group) and 2004 (i.e. YSR=1 for the control group), with FTE=0 the year before YSR=1. Treated indicates the individual's treatment status within the refugee and native samples, respectively. For both refugee and native samples, YSR=1 corresponds to 2004 for the control group and 2005 for the treatment group.

Figure 4. Distribution of cumulative employment by YSR=2



Notes: This figure shows the distribution of cumulative employment (in annual FTE) in the sample as evaluated at YSR=2. The graph is truncated at 2 which is indicated by a diamond.

Figure 5. Distribution of cumulative employment by YSR = 7 by low vs. high pre-reform employment performance types



Notes: This figure shows the CDF of cumulative employment (in annual FTE) by YSR=7 by pre-reform employment performance types. The figure includes individuals in our control group only. An individual is classified as HIGH if her cumulative employment (in annual FTE) by YSR=2 is at least 0.75 and as LOW otherwise.

Table A1. Characteristics of natives

	Overall		Treated		Control	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Male	0.46	0.50	0.46	0.50	0.45	0.50
Married	0.29	0.45	0.30	0.46	0.28	0.45
Have children	0.46	0.50	0.47	0.50	0.45	0.50
Age	31.90	5.53	32.34	5.49	31.32	5.53
<i>Outcome:</i>						
Annual employment (in FTE)	0.33	0.40	0.34	0.41	0.32	0.39
Unique individuals	11,134		6,342		4,792	

Notes: Natives are Danes aged 20-40 who are unemployed for two consecutive years prior to 2005 (i.e. YSR=1 for the treatment group) and 2004 (i.e. YSR=1 for the control group), with FTE=0 the year before YSR=1. We follow the natives for six years beginning in 2004 (2005) for the control (treatment) group. Annual employment is measured in full-time equivalents (FTE), where FTE=1 corresponds to full-time employment for the full year.

Table A2. Reform exposure and re-taking of the language test

	(1)	(2)	(3)
<i>Pre-reform employment:</i>	All	HIGH	LOW
Treated × Post	-0.004 (0.014)	0.061** (0.025)	-0.020 (0.016)
Mean of dep.var.	0.037	0.038	0.037
YSR FE	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Observations	5122	993	4129

Notes: Estimates of equation (7) are presented, using re-taking of the language test as the dependent variable. Treated is an indicator of cohorts receiving temporary residency in Jan-Oct 2004 (vs. Jan-Oct 2003). Post indicates whether the YSR is 3 or later. With respect to pre-reform employment performance, an individual is classified as HIGH if her cumulative employment (in annual FTE) by YSR=2 is at least 0.75 and as LOW otherwise. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A3. Heterogeneity by combined pre-reform employment/language performance types

	Employment	Language
	(1)	(2)
LOW/LOW × (Treated × Post)	-0.082*** (0.028)	-0.024 (0.035)
LOW/HIGH × (Treated × Post)	-0.038 (0.069)	
HIGH/LOW × (Treated × Post)	-0.064 (0.066)	0.177** (0.070)
HIGH/HIGH × (Treated × Post)	0.020 (0.122)	
Individual FE	Yes	Yes
YSR-by-employment/language type FE	Yes	Yes
Observations	5122	5122

Notes: Estimates of a variant of equation (7) are presented. All regressions include YSR fixed effects fully interacted with the four employment/language types: LOW/LOW, LOW/HIGH, HIGH/LOW, and HIGH/HIGH. Treated is an indicator of cohorts receiving temporary residency in Jan-Oct 2004 (vs. Jan-Oct 2003). Post indicates whether the YSR is 3 or later. With respect to pre-reform employment performance, an individual is classified as HIGH if her cumulative employment (in annual FTE) by YSR=2 is at least 0.75 and as LOW otherwise. With respect to pre-reform language performance, an individual is classified as HIGH if she passes the Danish language test at least at the intermediate level by YSR=2 and as LOW otherwise. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$