Permanent Residency and Refugee Immigrants' Skill Investment

Jacob Nielsen Arendt, *ROCKWOOL Foundation Research Unit*

Christian Dustmann, *University College London, Centre for Research and Analysis of Migration (CReAM), and ROCKWOOL Foundation Berlin (RFBerlin)*

Hyejin Ku, *University College London, CReAM, and RFBerlin*

We analyze an immigration reform in Denmark that tightened refugee immigrants' eligibility criteria for permanent residency to incentivize their labor market attachment and acquisition of local language skills. Contrary to what the reform intended, the overall employment of those affected decreased, while their average language proficiency remained largely unchanged. This was caused by a disincentive effect, where individuals with low prereform labor market performance reduced their labor supply. Our findings suggest that stricter permanent residency rules, rather than incentivizing refugees' skill investment, may decrease the efforts of those who believe they cannot meet the new requirements.

I. Introduction

Asylum applications submitted in the European Union (EU) reached a record-high level of 1.2 million in both 2015 and 2016, imposing major economic and political challenges for receiving countries. Refugee immigrants'

We thank the ROCKWOOL Foundation for funding this project. Christian Dustmann also acknowledges support from the European Research Council

Submitted May 24, 2021; Accepted June 23, 2023; Electronically published January 27, 2025.

Journal of Labor Economics, volume 43, number 2, April 2025.
 https://doi.org/10.1086/726433

poor labor market attachment is among the most substantial challenges. Their labor force participation and earnings are persistently lower than those of other immigrant groups (see Brell, Dustmann, and Preston 2020; Fasani, Frattini, and Minale 2022). Many receiving countries have therefore implemented policies to incentivize refugee immigrants to work and acquire the local language. These investments are required for benefit receipt and permanent residency.[1] Since the latest wave of refugees to EU countries is still quite nascent, we lack the requisite data on their medium- to longer-term outcomes to assess such policies' effectiveness in incentivizing refugee immigrants' skill investment.

To shed light on this debate, we take advantage of Denmark's experience with refugee migration in the 2000s and its experimentation with permanent residency rules.[2] In particular, we focus on a 2007 reform that tightened refugee immigrants' eligibility criteria for permanent residency to incentivize their labor market attachment and acquisition of local language skills. The reform—while extending the existing legislation that requires at least 7 years of (temporary) residency and the completion of a mandatory integration program for permanent residency eligibility—newly required at least 2.5 years of cumulative employment experience (in full-time equivalents [FTE]) and raised the bar for the existing language test from the previous basic level to the intermediate level.

The 2007 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 sec. II for details). Our empirical strategy thus compares the labor market trajectories of immigrants receiving temporary residency in

(advanced grant MCLPS 833861) and the Deutsche Forschungsgemeinschaft (grant 1024/1-2 AOBJ:642097). Contact the corresponding author, Christian Dustmann, at c.dustmann@ucl.ac.uk. Information concerning access to the data used in this paper is available as supplemental material online.

[1] All of 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, the Netherlands, and Hungary have limited access to social benefits, while Austria, Poland, and Czech Republic have made permanent residence conditional on 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 2018). For further discussion on Denmark's experience with refugee migration during this earlier period, see studies such as Clausen et al. (2009), Kilstrom, Larsen, and Olme (2018), Dustmann et al. (2024), and Foged et al. (2024). A recent survey by Arendt, Dustmann, and Ku (2022) focuses specifically on the labor market consequences of refugee policies in Denmark.

January–October 2003 (who could complete the 3-year integration program by the policy cutoff date) with those of immigrants receiving temporary residency in January–October 2004 (for whom the completion of the 3-year integration program by the cutoff date was not possible). Specifically, 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 their third year since temporary residency but not before, whereas the earlier-admitted (i.e., control) cohorts continue to be applied the old criteria throughout. Importantly, both the treated and the control cohorts had been residing in Denmark for at least 2 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 follow the treated and control cohorts for 7 years and examine their labor market trajectories and language skills.

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% drop in employment (measured in annual FTE). Moreover, we find no significant reform effect on the overall language proficiency (measured by passing of the Danish language test at the intermediate or higher level). This is in contrast to what the reform was intended to achieve, namely, to incentivize refugee immigrants' labor market integration and skill investment.

To rationalize these seemingly counterintuitive responses to the reform, we draw on the literature on the incentive effects of graduation standards on students' effort provision in school settings (see, e.g., Costrell 1994; Betts and Costrell 2001). In a stylized model, we illustrate that the 2007 reform may have either incentive effects or disincentive effects on immigrants' skill investment depending on their underlying capacity to fulfill the new requirements. In particular, individuals with high productivity (or low cost of effort) who can meet the new requirements relatively easily may indeed be incentivized to fulfill the tougher criteria. In contrast, for individuals with low productivity (or high cost of effort), the reform may result in a disincentive effect if the new requirements are deemed too costly to fulfill.

We investigate this conjecture by taking advantage of the fact that individuals in both the treatment group and the control group had been residing in Denmark for at least 2 years before the reform implementation in 2007. On the basis of the prereform labor market performance of the treated and the control cohorts, we classify individuals who have accumulated a nonnegligible amount of employment experience as "high performance" and those who have not as "low performance." We then examine whether the effect of reform exposure differs for high- versus low-performance individuals. Similarly, we consider their prereform language proficiency as an alternative measure to classify individuals into high- versus low-performance types.

We find that the negative overall employment effect discussed above is mainly driven by the responses of low-performance individuals who decrease their labor supply in response to the reform. For those with high prereform labor market performance, we find evidence of increased language proficiency but not labor supply (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.

By evaluating the 2007 reform in Denmark exploiting comprehensive register data and based on a research design that makes use of the unique reform features, we provide much-needed evidence on the causal effects of tougher permanent residency rules—as considered in many EU countries today—on refugee immigrants' labor market integration and skill investment in the host country.

Besides contributing to the contemporary policy debate, this study also adds to the literature on immigrant assimilation (see, e.g., Chiswick 1978; Borjas 1987; Friedberg 2000; Edin, Fredriksson, and Åslund 2003; Bleakley and Chin 2004; Cortes 2004; Lubotsky 2007; Cadena, Duncan, and Trejo 2015) on the one hand and refugee integration (see, e.g., Sarvimäki and Hämäläinen 2016; Brell, Dustmann, and Preston 2020; Fasani, Frattini, and Minale 2021, 2022; Arendt, Dustmann, and Ku 2022) on the other. Moreover, by focusing on the role of permanent residency rules on refugee immigrants' skill investment, we contribute to the growing literature that relates immigrant outcomes to their citizenship or permanent residency status (see, e.g., Bratsberg, Ragan, and Nasir 2002; Gathmann and Keller 2017; Hainmueller, Hangartner, and Pietrantuono 2017; Hainmueller, Hangartner, and Ward 2019; Felfe, Rainer, and Saurer 2020; Khourshed and Méango 2020; Govind 2021; Wang 2021). Our key departure from the literature is that we focus on the ex ante skill investment behavior of refugee immigrants in the face of tougher or more lenient permanent residency rules, whereas the literature is primarily concerned with the ex post outcomes of being granted citizenship or permanent residency.

II. Denmark's Refugee Policy and the 2007 Reform

Persons fleeing from war can be granted protection in Denmark in the form of (temporary) residency through asylum, subsidiary protection arrangements, or family reunification.[3] Temporary residency is granted for 1–3 years and must be renewed by application.[4] After a certain period of residency in

[3] The legal grounds for asylum are described in secs. 7 and 8 in the Danish Aliens Act. Family reunification is described in sec. 9.

[4] A renewal requires that the grounds on which the refugees or family members were provided residency still hold. Residency can also be withdrawn before renewal

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 asylum after February 28, 2002, eligibility for permanent residency requires (at least) 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.[5] The integration program lasts 3 years and consists of a language course (1.2 years of full-time studies) and employment support if unemployed. Documentation of language proficiency is acquired by passing a standardized test. The language test is available at three levels of proficiency: basic, intermediate, and high.[6] The test is offered twice yearly and can be repeated against a payment.[7]

The 2007 reform, the focus of this study, was intended to incentivize refugee immigrants' investment in the local language skills and engagement with the Danish labor market by conditioning the granting of permanent residency 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] 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."[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

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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 Danish Aliens Act, secs. 33(1) and 33(2).

[5] For those who applied for asylum before February 28, 2002, eligibility for permanent residency required 3 years of stay in Denmark and participation in the integration program but no language test requirement. See Kilstrom, Larsen, and Olme (2018) for an analysis of the 2002 reform.

[6] 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).

[7] The cost of the language test is DKK 600 (or USD 92 based on a 2020 exchange rate of 6.5). Benchmarked against the welfare benefits for a single person in 2002 (DKK 5,100 or USD 784), this is not a substantial cost.

[8] The speech made by the minister of foreign affairs and integrations at the proposal can be found here (in Danish): https://www.ft.dk/samling/20061/lovforslag/L93/fremsaettelsestale.htm.

[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 379 of April 25, 2007, on amendments of the act on integration of immigrants in

two new requirements in addition to the requirements of 7 years of residency 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 FTE). The second is a 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 completed the standard 3-year mandatory integration program by November 29, 2006.

Figure 1 illustrates the timing of the reform and the arrival cohorts unaffected and affected by the 2007 reform. Because the integration program must be initiated within 1 month of 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, completing the program by the cutoff date was impossible. We therefore focus on the cohorts receiving temporary residency in January–October 2003 and January–October 2004 as our control and treatment groups, respectively, with respect to the 2007 reform.[10]

III. Theoretical Considerations

The 2007 reform imposed stricter standards for permanent residency with the objective to incentivize refugee immigrants' labor market attachment and acquisition of local language skills. However, depending on the underlying capacity of individuals relative to the stricter criteria, the reform may increase or decrease investment. To illustrate this, we draw on the insights from Costrell (1994) and Betts and Costrell (2001) on the incentive effects of education standards and present a stylized model adapted from Costrell (1994).

Individual i has preferences $U^i(L_i, w_i)$ defined over leisure L_i and future earnings w_i.[11] Individuals can increase their future output through investment in

Denmark (Ministry of Integration and Foreign Affairs: https://www.retsinformation.dk/eli/lta/2007/379 [in Danish]).

[10] For the cohorts receiving temporary 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.

[11] The utility function U^i is a standard concave function in both arguments but is individual-specific, since different individuals face different trade-offs or costs of investing in human capital, for instance, because of different abilities or discount rates.

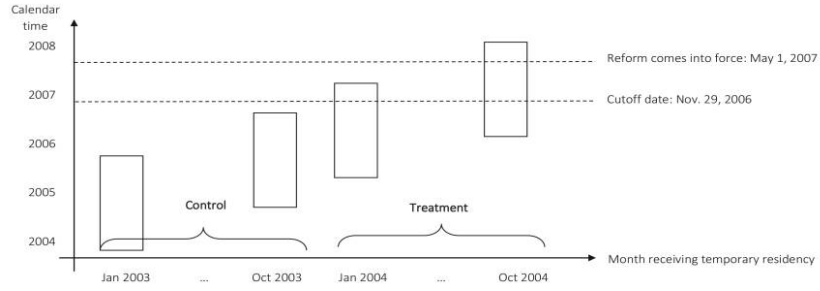

FIG. 1.—Timeline of the 2007 reform and the treatment and control groups. The boxes show the duration of the 3-year integration program for each cohort. Cohorts who have not yet completed the integration program by November 29, 2006, are subject to the new permanent residency rules coming into force 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.

human capital. The skill production function is a concave and decreasing function of leisure such that

$$y_i = h(L_i), h', h'' < 0, \tag{1}$$

where y_i is human capital (or output) of worker i. Let L_0 be the maximum amount of leisure and $y_0 = h(L_0)$ be the zero-effort output level. Assume that human capital (or output) directly translates into wages such that $w_i = y_i$. In the status quo, everyone is eligible for permanent residency.

In figure 2, we depict the optimal choice of effort and wage levels for two individuals, i and j, given the budget constraint in the $L - w$ space. In this example, person i opts for higher effort and wage levels than person j. Assume that there is a continuum of immigrants of mass 1, ordered by their optimal output y_i^* in the status quo (i.e., prereform). We interpret this continuum as reflecting the distribution of ability. Let the density and distribution functions be $g(y_i^*)$ and $G(y_i^*)$, where $g(y_i^*) > 0$ on $(y_0, y_{\max})$.

Now consider an immigration reform that newly introduces a minimum standard for human capital investment $\hat{y} \in (y_0, y_{\max})$ that individuals must meet to be eligible for permanent residency. Individuals who meet the standard and obtain permanent residency will continue to receive wages equal to their output (i.e., $w_i = y_i$) as before. However, individuals not eligible for permanent residency receive a fixed wage of y_0 regardless of their human capital, as depicted in figure 3.[12] The implication of the reform is illustrated by the new budget constraint in the $L - w$ space in figure 3.

[12] The low wage y_0 may represent a variety of disadvantages stemming from not having permanent residency. Although refugees and their family members can renew their temporary residency in Denmark, they can do so only as long as the grounds on which the asylum was granted still hold (see n. 4). If that is not the case,

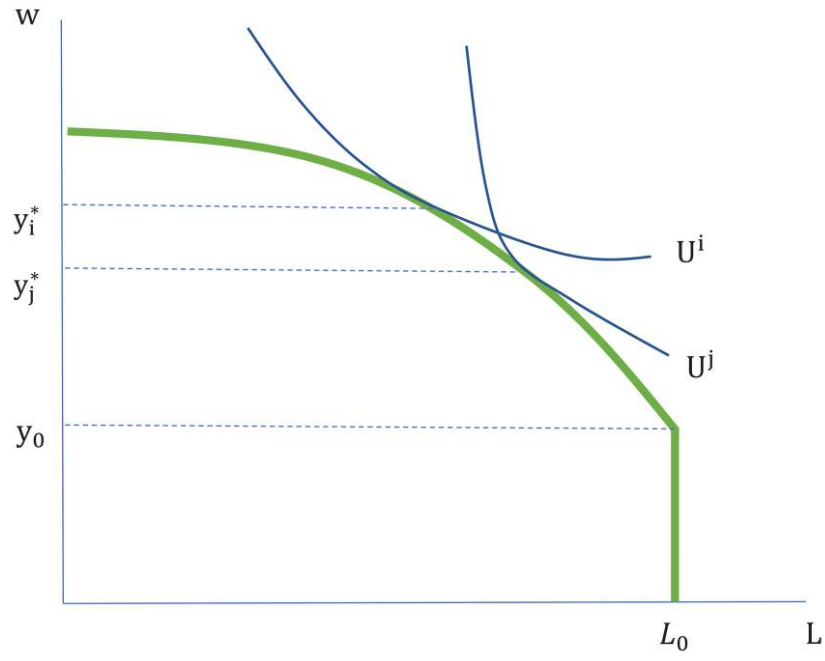

FIG. 2.—Preferences, constraints, and optimal choice before the reform.

Denote by $\tilde{y}_i$ the maximum investment that individual i will make such that

$$U^i(h^{-1}(\tilde{y}_i), \tilde{y}_i) = U^i(L_0, y_0).$$

Individuals of the highest ability whose prereform optimal output level is above the standard, $y_i^* > \hat{y}$, will continue to choose this level of investment after the reform. However, for individuals whose prereform optimal investment is below the standard, $y_i^* < \hat{y}$, their responses to the reform will depend on their maximum investment levels, $\tilde{y}_i$ (which vary by individual ability). Take the example of a high-ability individual (i) illustrated in figure 3*A*. Her maximum investment $\tilde{y}_i$ is above $\hat{y}$, the standard set by the reform. Hence, she will now increase her effort (from y_i^* to $\hat{y}$) and meet the standard.

the renewal of their temporary residency permit will not be granted, and they can be deported to their origin country. That creates uncertainty about future options, which on its own likely creates disutility. Moreover, such uncertainty impacts the anticipated migration duration, which in turn may lead to suboptimal investment into host-country-specific human capital (see Adda, Dustmann, and Görlach 2022). Finally, even if immigrants could continue to renew the temporary residency and stay in the host country, without permanence, they may be subject to less favorable conditions in the labor market.

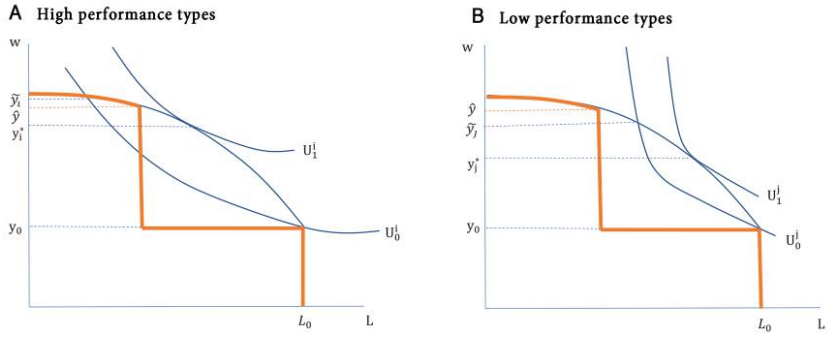

FIG. 3.—Heterogeneous responses to the reform by performance types. *A*, High-performance types. *B*, Low-performance types.

Figure 3*B* shows the case for a low-ability individual (j). Her maximum investment $\tilde{y}_j$ is below the threshold $\hat{y}$. Therefore, she will choose (L_0, y_0) and forego permanent residency, since she finds the effort needed to reach $\hat{y}$ too costly relative to the expected benefits of permanent residency. For such individuals, the reform will decrease output (from y_j^* to y_0).

The net effect on the aggregate human capital (or equivalently average human capital, since workers are of mass 1) thus depends on how high the standard $\hat{y}$ is set relative to the distribution of abilities (or preferences) in the target population. The overall effects and the heterogenous effects by worker types are the focus of our empirical investigation below.

IV. Empirical Strategy

We investigate the 2007 reform's effect on annual employment (in FTE) and language proficiency (measured by passing the Danish language test at the intermediate level or higher).[13] Individuals receiving (temporary) residency in January–October 2004 are our treatment group, and those receiving (temporary) residency in January–October 2003 are our control group (see sec. II). We follow each cohort for 7 years since temporary residency (YSR).[14] The 2007 reform came into effect on May 1, 2007. Therefore, our treatment group is exposed to the reform beginning in YSR = 3 but not before. Regardless of the reform, our control group continues to be applied the old rules.

[13] Once a person passes the language test at a given level, we consider that person to be proficient at that level in all subsequent periods.

[14] We focus on the first 7 years, since our primary interest is to identify the behavioral effect of the 2007 reform on the skill investment of refugee immigrants independently of the effect of eventual permanent residency status.

We estimate the following DID equation:

$$y_{i\tau} = \alpha_i + \psi_\tau + \delta \text{Post}_\tau \times T_i + X_{t(i\tau)}\beta + u_{i\tau}, \quad (2)$$

where $y_{i\tau}$ is the outcome of individual i in her τth YSR, α_i is an individual fixed effect (FE), and ψ_τ is a dummy indicating YSR. The variable T_i indicates whether individual i is in the treatment (vs. control) group, and Post_τ is a dummy taking the value of 1 if YSR $\geq$ 3 and 0 otherwise. Note that the level effect of T_i is subsumed in the individual FE, α_i. Also, the level effect of Post_τ is subsumed in the YSR FE, ψ_τ. Standard errors are clustered at the person level.

Our main coefficient of interest is δ, which shows the average difference in outcomes in the postreform period (i.e., YSR = 3–7) between our treatment and control groups relative to the difference in the prereform period (i.e., YSR = 1–2). Our identifying assumption is that without the 2007 reform, the outcomes of the treatment and control groups would have evolved in parallel. If the reform has an incentive (disincentive) effect on the effort choices of the treated individuals, δ will be positive (negative). In some specifications, we include $X_{t(i\tau)}$, a vector of additional controls that vary at the level of calendar years (t)—(i) the annual unemployment rate or (ii) dummies for each of the recession years (2009–11)—to account for aggregate labor market conditions in Denmark.

We also estimate an event study variant of formula (2), where we replace Post_τ with YSR indicators to follow the reform exposure's year-by-year effect. Specifically, using YSR = 2 as the reference year, we estimate the following equation:

$$y_{i\tau} = \alpha_i + \psi_\tau + \sum_{\kappa=1(\kappa\neq 2)}^{7} \delta^\kappa I(\tau = \kappa) \times T_i + X_{t(i\tau)}\beta + e_{i\tau}. \quad (3)$$

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

It is worthwhile clarifying that the key objective of the 2007 policy was to incentivize refugee immigrants' labor market integration and skill investment. Fulfilling the new requirements by YSR = 7 was not the policy's goal.[15] Therefore, we focus on the contemporaneous outcomes in every period,

[15] The minimum residency requirement is 7 years, but this does not mean that individuals must fulfill the employment and language criteria by YSR = 7 to ever be eligible for permanent residency.

not just the cumulative outcomes at YSR = 7. For instance, it matters whether language proficiency is attained in year 3 or 7, as this has an impact on the productivity of the individual over their remaining migration cycle and the degree of integration in the host country (which is a major concern). We first estimate formulas (2) and (3) in the overall sample, then examine potentially differential responses by individuals' performance types to explore the model's predictions from section III.

V. Data on Refugee Immigrants

We draw 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. Asylum seekers, upon arrival in Denmark, apply for asylum. Between arrival and the asylum decision, there are waiting periods. An individual may be granted asylum (and issued temporary residency) or rejected at the asylum decision. We focus on individuals who applied for asylum after February 28, 2002, to maintain common eligibility requirements for permanent residency in the absence of the 2007 reform (see sec. II).[16] We also restrict attention to individuals who were aged between 18 and 64 at the time of receiving temporary residency.

Our primary outcomes of interest are employment and language proficiency. Employment is recorded in annual FTE, where FTE for the whole year is equal to unity. We observe this information for all individuals in Denmark as of January 1 each year. For example, 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 receiving (temporary) residency sometime during 2003, we denote the year 2004 as YSR = 1, 2005 as YSR = 2, and so on, since we observe their employment only for 2004 onward but not for 2003 (except for those who received residency precisely on January 1, 2003).[17] We measure an individual's proficiency in the Danish language based on passing the standard language test at the intermediate or higher level. We observe individuals' language test results for YSR = 1 onward.

[16] In 2002 there was a reform called Start Aid. For those receiving temporary residency on or after July 1, 2002, the reform lowered their welfare benefits by about 40% (see Dustmann et al. 2024). In our sample, both our control and treatment groups receive temporary residency after July 1, 2002, specifically during January–October 2003 (for control) and January–October 2004 (for treatment). Therefore, besides the 2007 reform that we focus on, there were no differences in temporary residency criteria for the control and treated groups in our sample.

[17] The original variable we use in the register data records the cumulative employment in a given calendar year. Take a person obtaining temporary residency in June 2003. For this person, 2004 is YSR = 1. However, the variable shows the person's cumulative employment by 2004. So for YSR = 1 only (but not for subsequent YSR), it is possible that employment exceeds FTE = 1, since the cumulative employment variable includes their employment from year 2003.

Once a person passes the language test, say at the intermediate level, we treat that person to be proficient at the intermediate level in all subsequent periods.

Table 1 shows some descriptive statistics for our sample. Focusing on the overall sample, column 1 shows that at YSR = 1, immigrants are, on average, 33 years old, about 60% are male, 55% are married, and 48% have children. As of YSR = 2, an average immigrant works for about 0.23 FTE, and 16% of our sample have Danish language proficiency at the intermediate or higher level. By YSR = 7, average annual employment rises to 35% of FTE, and roughly 40% of individuals have language proficiency at the intermediate

Table 1
Individual Characteristics and Employment and Language Outcomes

	Overall		Treatment Group		Control Group	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)
	A. Individual Characteristics					
Male	.59	.49	.56	.50	.61	.49
Age	33.10	10.30	32.70	9.80	33.30	10.60
Married	.55	.50	.56	.50	.54	.50
Any children	.48	.50	.54	.50	.44	.50
Country of origin:						
Afghanistan	.13	.33	.12	.33	.13	.34
Iran	.06	.23	.09	.29	.03	.18
Iraq	.06	.24	.03	.17	.08	.27
Myanmar	.13	.34	.21	.41	.08	.27
Somalia	.06	.24	.06	.23	.07	.25
Former Republic of Yugoslavia	.05	.23	.09	.28	.03	.17
Other countries	.51	.5	.41	.49	.58	.49
	B. Outcomes at Various YSR					
Annual employment (FTE):						
YSR = 2	.23	.35	.27	.39	.2	.33
YSR = 7	.36	.43	.35	.44	.37	.43
YSR = 1–7	.31	.41	.31	.42	.31	.41
Language:						
YSR = 2	.16	.37	.17	.38	.16	.36
YSR = 7	.39	.49	.41	.49	.37	.48
YSR = 1–7	.27	.45	.29	.45	.26	.44
Cumulative employment (annual FTE):						
YSR = 2	.35	.60	.44	.70	.29	.52
YSR = 7	2.20	2.16	2.20	2.33	2.21	2.03
YSR = 1–7	1.08	1.53	1.14	1.63	1.04	1.46
Person-year observations	5,122		2,090		3,032	
Unique individuals	767		314		453	

NOTE.—Sample means and standard deviations in the overall sample and by treatment status. The treatment group received temporary residency in January–October 2004, while the control group received temporary residency in January–October 2003. Annual employment is measured in FTE, where FTE = 1 corresponds to full-time employment for the full year. "Language" is an indicator of having passed the Danish test at the intermediate or the high level.

or higher level. In terms of cumulative employment by YSR = 7, the mean (standard deviation) 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 with little engagement in the labor market, 2.5 years of cumulative employment experience may be a big challenge.

Table 1 also separately displays the characteristics of our treatment and control cohorts. While the baseline characteristics are similar, the share of immigrants from Myanmar is higher in our treatment group than in the control group.[18] In our robustness analysis, we therefore exclude individuals of Myanmar origin from our sample. Moreover, we consider matching the control group with the treatment group to make their prereform characteristics more comparable (see sec. VI.B).

It is worthwhile pointing out that since passing of the language test is an absorbing state, it is important that the share of individuals reaching that state in the preperiods is balanced between the treated and control groups (as otherwise the DID estimates will under- or overstate the treatment effect). In our data, those who pass the language test by YSR = 2 are only a minor part of our sample (17%), and their shares in the treatment and control groups are balanced (see table 1).[19]

VI. Permanent Residency and Immigrants' Skill Investment

A. Overall Effects

Table 2 shows the estimates of the reform effect (the parameter δ from eq. [2]) in the overall sample. Columns 1–3 report estimates of δ with annual employment (in FTE) as the outcome, while columns 4–6 use language proficiency (at the intermediate or higher level) 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 reduced their employment hours by about 30% ($-0.09/0.31$) in response to the reform, contrary to what the reform was intended to achieve. In column 4, the estimates show that the treated cohorts have a three percentage point or 11% (0.03/0.27) improvement in language proficiency, although estimates are statistically insignificant. When we include annual unemployment rates (cols. 2, 5) or dummies indicating each recession year in our sample, 2009–11 (cols. 3, 6), the results change only slightly.

[18] A large share of refugees from Myanmar are resettled by agreement with the UNHCR. Denmark started to resettle 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). See https://www.unhcr.org/resettlement-data.

[19] Moreover, when we exclude from our sample those who pass the language test by YSR = 2, our main results remain invariant.

Table 2
Effects of Reform Exposure on Employment and Language Skills of Refugee Immigrants

	Employment			Language		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	−.090***	−.072***	−.069***	.033	.031	.025
	(.025)	(.023)	(.024)	(.027)	(.027)	(.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	5,122	5,122	5,122	5,122	5,122	5,122

NOTE.—Estimates of eq. (2) are presented. "Treated" is an indicator of cohorts receiving temporary residency in January–October 2004 (vs. January–October 2003). "Post" indicates whether the YSR is 3 or later. "Employment" is annual employment in FTE. "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 (2009–11). Standard errors in parentheses are clustered at the individual level.

*** $p < .01$.

Overall, the stricter permanent residency rules negatively affected employment, with an insignificant positive effect on language proficiency.

B. Robustness Checks

Sample Restrictions and Matching

We conduct various checks to examine the robustness of these estimates (table 3). In our primary analysis, we include January–October 2003 and January–October 2004 cohorts as our control and treatment groups, 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. The result is similar to our main estimate (table 2, col. 1). In column 2, we restrict the sample to the balanced panel of individuals we observe for all 7 years (91% of our sample). The estimates are very similar to those based on the full sample, suggesting that differential attrition between treatment and control groups is not driving our main results. Furthermore, column 3 reports estimates based on a matched sample, where we choose the control group to match the prereform characteristics of the treatment group using propensity score matching.[20] The coefficient remains stable. The corresponding estimates for language as a dependent variable are provided in columns 4–6. The effects on language are small and insignificant,

[20] We use nearest-neighbor propensity score matching with replacement. The variables used for matching are age, gender, marital status, having any children, countries of origin, and county assigned when granted temporary residency. The characteristics of individuals in the treatment group and those of the matched control group are reported in table A1 (tables A1–A6 are available online).

Table 3
Effects of Reform Exposure on Employment and Language Skills of Refugee Immigrants: Robustness to Sample Restrictions

	Employment			Language		
	Include November–December 2003 (1)	Balanced Panel (2)	Matched Sample (3)	Include November–December 2003 (4)	Balanced Panel (5)	Matched Sample (6)
Treated × Post	−.094***	−.086***	−.081***	.035	.031	−.002
	(.024)	(.025)	(.027)	(.027)	(.028)	(.030)
Mean of dependent variable	.309	.313	.308	.274	.282	.274
YSR FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,474	4,886	4,174	5,474	4,886	4,174

NOTE.—Estimates of eq. (2) are presented. "Employment" is annual employment in FTE. "Language" is an indicator of having passed the Danish test at the intermediate or the high level. "Treated" is an indicator of cohorts receiving temporary residency in January–October 2004 (vs. January–October 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 restrict the analysis to the balanced sample where the individual is observed in all 7 YSR. Columns 3 and 6 are based on matched refugee sample where control individuals are matched to the treated individuals by nearest-neighbor propensity score matching with replacement. Matching variables include age, gender, marital status, having any children, countries of origin, and county assigned when granted temporary residency. Standard errors in parentheses are clustered at the individual level.

*** $p < .01$.

similar to the estimates based on the main sample in table 2. Although not reported in order to save space, similar results also obtain when we exclude individuals originating from Myanmar.[21]

Event Study

To investigate the timing of responses, we next estimate the event study specification in equation (3). The estimated coefficients are plotted in figure 4, where the coefficient for YSR = 2 is normalized to zero. Figure 4*A* shows the results for employment, and figure 4*B* shows the results for language proficiency. In the case of employment, the differential outcomes for the treatment group relative to the control group increase from YSR = 2 to YSR = 3 when the reform sets in and becomes significant from YSR = 4 onward. The smaller response at YSR = 3 is as expected, since treatment takes place only part of that year (from May onward; see sec. IV). In the case of language acquisition, small positive effects from YSR = 4 onward are visible, although they are not statistically significant.

[21] The estimated coefficients (standard errors) for employment and language are −0.101 (0.026) and 0.042 (0.030), respectively, where the sample size is 4,430.

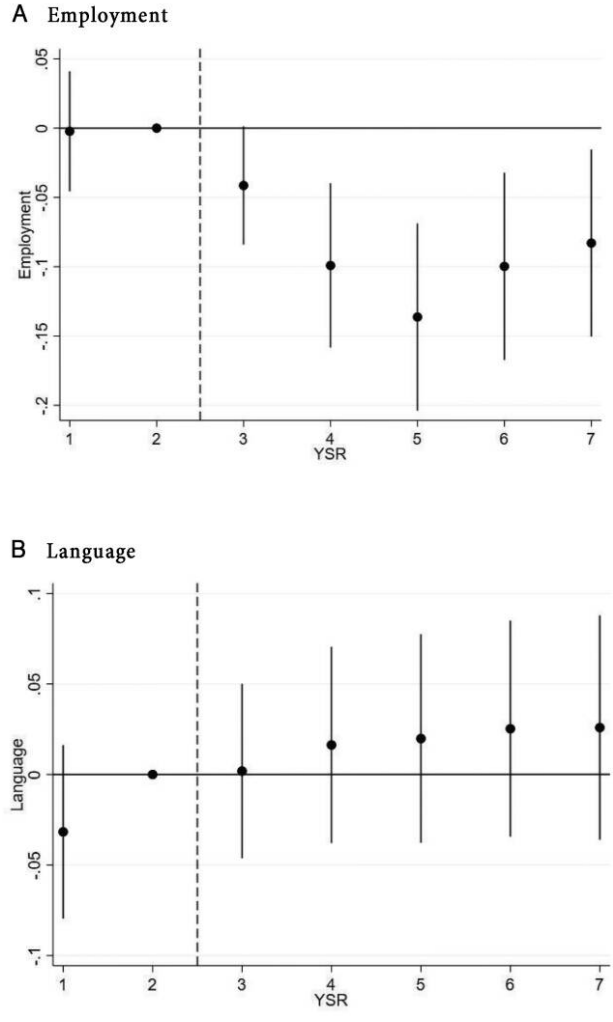

FIG. 4.—Event study estimates of the reform effect. *A*, Employment. *B*, Language. Estimated coefficients based on equation (3) are plotted, along with 95% confidence intervals. *A* uses annual employment (in FTE) as the dependent variable, while *B* uses passing of the language test at the intermediate or the high level as the outcome variable. The dashed vertical line shows the time when the reform comes into force in the middle of YSR = 3.

Comparison with Natives

A potential concern of our empirical strategy is that the treatment group, who enters the Danish labor market 1 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 deal with such business cycle effects, we use Danish natives with labor market profiles comparable to refugee immigrants in our sample and conduct a triple-difference analysis.

We match Danish natives to the refugee immigrants in our main sample based on propensity score matching. Specifically, we choose "treated" natives to match the characteristics of treated refugees in terms of YSR = 1 (2005) employment, age, gender, being single, having any children, and county of residence (or initial assignment for refugees) at YSR = 1.[22] Similarly, we choose "control" natives to match the characteristics of control refugees in terms of YSR = 1 (2004) employment and the other characteristics mentioned above. The characteristics of the matched native sample are provided in table A2.

We plot the trends in annual employment of the treatment versus control groups separately by the refugee and native samples (fig. 5). In the refugee sample (fig. 5*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 (fig. 5*B*), we do not find comparable weakening of performance by the "treated" group relative to the "control" group. These patterns suggest that what we observe in our refugee sample is unlikely to be driven by the recession effect.

In table 4, we present estimates using a triple-difference approach. Column 1 replicates the DID estimate for refugees in table 2, while column 2 shows the estimate of equation (2) for natives. Column 3 then displays the triple-difference estimate. 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.

Placebo Tests

To further ensure that the effects are specific to the 2004 (vs. 2003) cohorts 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 (2), where we use those receiving temporary residency 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 are no differential employment responses in YSR $\geq$ 3 by the later-arriving cohorts. This contrasts with our main estimate of the 2007 reform effect, shown in column 3.[23]

[22] Here we use the variable "single" instead of "married," since for Danes, "married" is not very meaningful because of the high prevalence of cohabitation.

[23] The sample size is larger in cols. 1 and 2 than in col. 3, since Denmark had a larger number of persons being granted residency in 2001 and 2002. We do not have information on language tests before 2004; hence, we cannot conduct placebo tests for language outcomes.

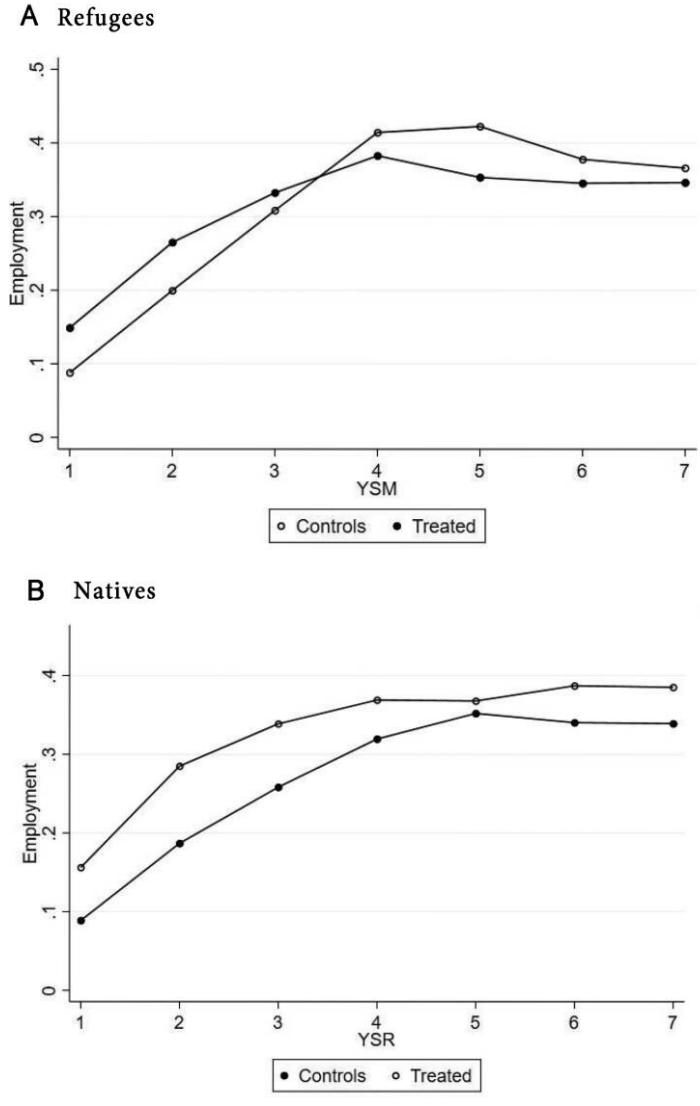

FIG. 5.—Trends in employment for refugees and matched natives. *A*, Refugees. *B*, Natives. This figure shows the trends in annual employment (in FTE) of the treatment versus control groups, separately by the refugee and native samples. Refugees are those underlying our main analysis in table 2. Natives are Danes matched to refugees by nearest-neighbor propensity score matching with replacement. Refugees in the control group are matched to natives in 2004, and refugees in the treatment group are matched to natives in 2005 (corresponding to the years where YSR = 1 for refugees). Matching variables include employment at YSR = 1, age, gender, being single, having any children, and county of residence (or initial assignment for refugees) at YSR = 1. "Treated" indicates the individual's treatment status within the refugee and native samples, respectively.

Table 4
Effects of Reform Exposure on Employment of Refugee Immigrants versus Danish Natives

	DID		
	Refugees (1)	Natives (2)	Triple Difference (Refugees − Natives) (3)
Treated × Post × Refugee			−.073**
			(.034)
Treated × Post	−.090***	−.017	
	(.025)	(.023)	
YSR FE	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Observations	5,122	5,143	10,265

NOTE.—The dependent variable is employment. Estimates of variants of eq. (2) are presented. Refugees are those underlying our main analysis in table 2. Natives are Danes matched to refugees by nearest-neighbor propensity score matching with replacement. Refugees in the control group are matched to natives in 2004, and refugees in the treatment group are matched to natives in 2005 (corresponding to the years where YSR = 1 for refugees). Matching variables include employment at YSR = 1, age, gender, being single, having any children, and county of residence (or initial assignment for refugees) at YSR = 1. Column 1 shows the baseline estimate from table 2 among refugees, while col. 2 reports the DID estimate in the matched native sample. Column 3 shows the results of the triple-difference estimates in a pooled sample of refugees and natives, i.e., the difference between cols. 1 and 2. Column 3 also includes Treated × Post. "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 FTE. Standard errors in parentheses are clustered at the individual level.

** $p < .05$.

*** $p < .01$.

This analysis shows that our main effect is indeed driven by the 2007 reform rather than picking up differential employment profiles that may be present between cohorts arriving in any two consecutive years.

Overall, the results suggest a disincentive rather than an incentive effect of the stricter permanent residency rules. We next examine whether worker responses to the 2007 reform differ by individuals' productivity (see sec. III).

C. Heterogeneous Responses by Performance Types

We consider two different performance measures relating to the new requirements stipulated in the 2007 reform: (i) cumulative employment by YSR = 2 and (ii) language proficiency (at the intermediate or higher level) by YSR = 2. As discussed above, at YSR = 2 neither the treatment group nor the control group has been affected by the 2007 reform. Hence, our classification of individuals into high versus low types is based on their prereform performance only.

Classifying Workers into Performance Types

In figure A1 (figs. A1, A2 are available online), we plot the distribution of cumulative employment (in FTE) by YSR = 2. As shown, most refugee immigrants have not worked at all after 2 years in the country. Given this, we classify an individual as "high performance" (high) if her cumulative employment

Table 5
Effects of Placebo Reforms on Employment of Refugee Immigrants

	Placebo 1 (1)	Placebo 2 (2)	Actual (3)
Treated × Post	−.010	−.019	−.090***
	(.012)	(.018)	(.025)
YSR FE	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Observations	23,998	11,159	5,122

NOTE.—The dependent variable is employment. Estimates of eq. (2) are presented using annual employment (in FTE) as the dependent variable. "Post" indicates whether the YSR is 3 or later. In col. 1, "Treated" is an indicator of cohorts receiving temporary residency in January–October 2002 (vs. January–October 2001). In col. 2, "Treated" indicates cohorts receiving temporary residency in January–October 2003 (vs. January–October 2002). Column 3 replicates the main reform effects from col. 1 of table 2. Standard errors in parentheses are clustered at the individual level.

*** $p < .01$.

by YSR = 2 is above 0.75 FTE; otherwise, she is classified as "low performance" (low). We choose the cutoff of 0.75, as it constitutes a nonnegligible part of the requirement of 2.5 FTE. With this threshold, 20% of our sample is in the high group, and 80% is in the low group. As figure A2 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 (the standard set by the 2007 reform). In contrast, a very substantial fraction (nearly 40%) of the low group has not met the standard by YSR = 7.

As a second dimension of performance, we consider a sample split by the immigrants' language skills by YSR = 2: those who already passed the Danish language test at the intermediate level (or higher) and those who did not. According to this measure, about 16% of individuals are classified as high.

Heterogeneity in Responses to the Reform

In table 6, we present the estimates of equation (2) for different subsamples of low- and high-performance (prereform) types. Columns 1–4 use employment (in FTE) as the dependent variable, whereas columns 5 and 6 use language proficiency (at the intermediate or higher level) as the dependent variable. Columns 1 and 2 split the sample by employment performance, and columns 3 and 4 split the sample by language performance, assessed at YSR = 2 (prereform), as discussed above. The significant and negative effect in column 1 suggests a disincentive effect of the stricter permanent residency rules for the low-performance individuals. Evaluated at the mean (0.22), the reform reduced their employment by 32% (0.07/0.22). We also find an adverse effect for high-performance individuals (col. 2), corresponding to a 7% reduction (0.05/0.66). Defining performance based on language proficiency at YSR = 2 (cols. 3, 4), we find that low-performance individuals reduce their annual employment by 37% (col. 3). In contrast, the reform has no

Table 6
Heterogeneous Responses to the Reform by Prereform Employment and Language Performance

	Employment				Language	
	Prereform Employment		Prereform Language		Prereform Employment	
	Low (1)	High (2)	Low (3)	High (4)	Low (5)	High (6)
Treated × Post	−.070***	−.050	−.109***	−.002	.001	.148**
	(.026)	(.059)	(.030)	(.065)	(.030)	(.061)
Mean of dependent variable	.223	.663	.295	.373	.263	.321
YSR FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,129	993	4,265	857	4,129	993

NOTE.—Estimates of eq. (2) are presented. Individuals are classified into high vs. low types based on prereform (YSR = 2) employment or language skills. With respect to prereform employment performance, an individual is classified as high if her cumulative employment (in annual FTE) by YSR = 2 is at least 0.75 and classified as low otherwise. With respect to prereform 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 classified as low otherwise. "Employment" is annual employment in FTE. "Language" is an indicator of having passed the Danish test at the intermediate or the high level. "Treated" is an indicator of cohorts receiving temporary residency in January–October 2004 (vs. January–October 2003). "Post" indicates whether the YSR is 3 or later. Standard errors in parentheses are clustered at the individual level.

** $p < .05$.

*** $p < .01$.

effect on employment for high-performance individuals (col. 4). Moreover, column 6 shows that individuals with high employment performance become more likely to improve their language skills in response to the reform. At the same time, there is no effect on language for individuals with low employment performance (col. 5).[24] According to columns 2 and 6, individuals with high prereform employment performance seem to allocate more effort to language acquisition while reducing employment. Therefore, at least for a subset of individuals in this group, the adverse employment effect may reflect temporary substitution rather than discouragement effects per se.

[24] In table A3, we consider language outcomes measured at different levels of proficiency: passing of the test at the basic level or above (panel A), at the intermediate level or above (panel B), and at the high level (panel C). The results confirm our previous findings; the 2007 reform incentivizes high-ability individuals' investment in language proficiency (but not beyond the standard set by the reform) while having little effect on the low-performance types. We also examine retaking of the language test as a possible mechanism; see table A4. We find that while overall the reform had no effect on the likelihood to retake the test, high-performance individuals become 2.6 times ((0.06 + 0.038)/0.038; col. 2) more likely than before to retake 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 retake the test in line with the interpretation that the 2007 reform had disincentive effects for individuals with a high cost of effort.

We now consider alternative ways of slicing the sample based on employment and language performance by YSR = 2. There are four possible combinations of prereform employment and language performance: low/low (68% of the sample), low/high (13%), high/low (16%), and high/high (4%). The group performing poorly on both dimensions constitutes more than two-thirds of the sample. Table A5 presents estimates of a variant of equation (2). 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 (col. 1). In contrast, 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 (col. 2).[25] This is consistent with our earlier conjecture that low-performance individuals for whom the cost of fulfilling the new requirements is high may give up entirely, whereas the reform may indeed incentivize those who have realistic chances of meeting the stricter requirements with some extra effort.

Next, we consider an additional measure to classify individuals into high versus low types without relying on their prereform performance. In particular, we use the level of language courses to which refugees are assigned as part of their mandatory integration program.[26] We classify those initially assigned to the highest level of language courses as high and the rest as low. This measure is correlated with the previous language measure we currently use to classify individuals (language attainment by YSR = 2, correlation = 0.311) but less so with the employment measure (correlation = 0.065). As shown in table A6, we again find a negative effect on employment for low types and a positive effect on language acquisition for high types, consistent with our findings in table 6.

D. Further Consequences of the Reform

To fully understand the reform's consequences, we conduct further analysis examining outcomes beyond employment (in FTE) and language (table 7). Column 1 replicates our main results on annual employment (table 2, col. 1). In columns 2 and 3, we decompose this employment outcome (in FTE) into extensive and intensive margins. The estimates show that both margins seem to matter, with individuals reducing their labor market participation and their hours worked conditional on working. Responding 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 postreform years (col. 4). Therefore, when we look at the incomes of refugee immigrants from different sources, we find that while exposure to the reform

[25] Notice that the highest-performing individuals in terms of language have already passed the postreform language requirement, by construction.

[26] The assignment to language courses is based on refugee immigrants' educational and linguistic background and not based on Danish language skills per se.

Table 7
Further Consequences of the Reform

	Employment				Income		
	In Annual FTE (1)	Extensive Margin (2)	Intensive Margin (3)	Welfare Receipts (4)	Labor Income (5)	Welfare Benefits (6)	Disposable Income (7)
Treated × Post	−.090***	−.117***	−.072***	.080***	−1.171***	.654***	−.086
	(.025)	(.0299)	(.039)	(.030)	(.299)	(.248)	(.068)
Mean of dependent variable	.293	.433	.712	.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	5,122	5,122	2,218	5,122	5,122	5,122	5,122

NOTE.—Dependent variables are employment and income. Estimates of eq. (2) 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 5–8 use log(1 + 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 January–October 2004 (vs. January–October 2003). "Post" indicates whether the YSR is 3 or later. Standard errors in parentheses are clustered at the individual level.

*** $p < .01$.

decreases labor income (col. 5), it increases income from welfare benefits (col. 6), leading to little effect on their overall disposable income (col. 7). Overall, the stricter permanent residency rules aimed at incentivizing refugee immigrants' labor market attachment seem to have achieved the opposite effect, a result driven by the fact that most refugee immigrants have low capacity to meet the new requirements.

VII. Conclusions

We study the effects of stricter permanent residency rules on refugee immigrants' skill investment, exploiting the 2007 reform in Denmark that, for permanent residency eligibility, newly required minimum years of labor market experience and raised the bar for the language proficiency requirement. Contrary to what the reform intended, the overall employment of the affected cohorts decreased after the reform, while their language proficiency remained essentially unchanged. The adverse employment effects are mainly driven by the response of "low-productivity" individuals (proxied by pre-reform levels of labor market attachment and language proficiency).

These findings highlight that setting a higher standard for obtaining permanent residency will effectively incentivize labor market engagement and investment in language skills only if the bar is set at an appropriate level. If the requirements are deemed too costly, the reform could be ineffective or result in disincentive effects. This is a particular concern for populations that

are ill prepared for the host country's labor market. This study advocates that policies aimed at better integration of refugee immigrants must carefully consider potential disincentives that may discourage individuals and possibly lead to outcomes inferior to those obtained in the absence of the policy.

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