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Silke Anger*, Jacopo Bassetto†, Malte Sandner‡

Abstract

While Western countries worry about labor shortages, their institutional barriers to skill transferability prevent immigrants from fully utilizing foreign qualifications. Combining administrative and survey data in a difference-in-differences design, we show that a German reform, which lifted these barriers for non-EU immigrants, led to a 15 percent increase in the share of immigrants with a recognized foreign qualification. Consequently, non-EU immigrants' employment and wages in licensed occupations (e.g., doctors) increased respectively by 18.6 and 4 percent, narrowing the gaps with EU immigrants. Despite the inflow of non-EU immigrants in these occupations, we find no evidence of crowding out or downward wage pressure for natives.

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1 Introduction

Immigrants experience worse labor market outcomes than natives in many destination countries (e.g., Borjas, 2015; Algan et al., 2010). A large part of this gap stems from institutional barriers to the transferability of human capital to the host country (Hendricks and Schoellman, 2018). These barriers lead to an under-utilization of immigrants' skills, implying high individual and society welfare losses (e.g., Friedberg, 2000; Mattoo et al., 2008; Dustmann et al., 2013). Previous research has shown that occupational recognition — the formal proof of the equivalence of a foreign certificate to its native counterpart — can enhance the transferability of qualifications and improve immigrants' labor market outcomes (Brücker et al., 2021; Sweetman et al., 2015).

In most contexts, however, access to occupational recognition is both non-standardized and costly for immigrants (OECD, 2017), leading to low application rates. In the U.S., for example, as no legal framework for recognition exists, credential evaluation services make recognition decisions on an individual, case-by-case basis (U.S. Department of Education, 2024). In Europe, the recent inflow of 4.2 million Ukrainian refugees shed new lights on the limits of recognition policies. Despite more than two thirds of refugees being highly qualified, the lengthy procedures to obtain occupational recognition have slowed down their labor market integration (Eurofound, 2024). Following these large migration inflows and persistent labor shortages, many destination countries have discussed ways to facilitate occupational recognition. Yet, little is known on the integration and labor market effects of such policies.

In this paper we address this gap exploiting a unique reform that aimed at enhancing skill transferability by drastically reducing institutional barriers to the recognition of foreign certificates. More precisely, we estimate the effects of the German Federal Recognition Act on immigrants' occupational recognition, employment opportunities, and wage assimilation. The Federal Recognition Act, passed by the German government in April 2012, (a) introduced a legal basis for recognition, (b) standardized and facilitated the proof of equivalence between German and non-German certificates, and (c) established numerous sources of information about recognition procedures. Crucially, and contrary to the pre-reform period, this new recognition framework applies to all immigrants with an occupational certificate (e.g., doctors, nurses, engineers) acquired abroad, independently of their country of origin. Given these characteristics, the Federal Recognition Act represents a potential blueprint to improve recognition policies worldwide.

Despite the clear goals of the reform, its effects on immigrants' occupational recognition and labor market integration are uncertain. First, a facilitated recognition framework does not automatically translate into higher application rates among eligible immigrants. While the costs of applying are lower, they may still exceed the expected gains from occupational recognition. Moreover, the reform may attract more applications from immigrants whose certificates do not meet the recognition standards, therefore lowering the share of *successful* recognitions. Second, even if the number of occupational recognitions increases, the reform effects on immigrants' labor market outcomes are ambiguous. One the one hand, a facilitated application process may encourage applications from immigrants who meet the quality standards to receive recognition but who posses lower observable and unobservable skills than pre-reform. In such cases, if employers do not trust the quality of recognized certificates, they may either avoid hiring immigrants or offer them lower-paying, less secure jobs. On the other hand, the reform may lower employers' uncertainty about recognized degrees and strengthen immigrants' outside options, potentially leading to better labor market outcomes.

To identify the effects of the reform on immigrants' labor market integration, we exploit the fact that, before 2012, immigrants from inside the EU were already subject to recognition procedures as those introduced with the Federal Recognition Act. Therefore, while it formally applies to all immigrants the new recognition framework lifted the barriers to recognition only for non-EU immigrants. This institutional setting allows us to apply difference-in-differences (DiD) designs in which EU immigrants represent the control and non-EU immigrants the treatment group. EU immigrants constitute a legitimate control group because they (a) must also have their home country certificates recognized to work in licensed occupations and (b) face language barriers similar to those of non-EU immigrants. Thus, our DiD design rules out the possibility that better labor market outcomes for non-EU immigrants post-reform are merely the result of third factors, such as better economic conditions or higher demand in licensed occupations that coincide with the reform. The identifying assumption is that, absent the Recognition Act, occupational recognition and labor market outcomes of EU and non-EU immigrants would have followed parallel trends. In support of this assumption we provide extensive evidence on the absence of pre-trends in the pre-reform years and test the robustness of our results to the use of alternative control groups.

We implement the DiD design using detailed German survey and administrative social security data from 2007 to 2017. The survey data allows us to analyze the reform effect on applications and recognitions because it provides unique retrospective information on occupational recognition outcomes for both EU and non-EU immigrants, as well as socio-demographic characteristics. The administrative social security data enables us to analyze the reform effects on employment probabilities, wages, and types of employment. The data includes all non-German individuals in the labor force, over 13 million individuals, between 2007 and 2017 (five years pre- and post-reform). Beyond demographics and labor market outcomes, social security records also contain a precise occupational classification, which allows us to identify employment spells in licensed occupations.

We obtain four sets of results. First, we demonstrate that lifting institutional barriers to occupational recognition led to a 4 percentage points increase in recognized certificates for non-EU immigrants. Relative to the pre-reform average among non-EU immigrants, recognition rates went up by 15 percent. The effect on recognized certificates is not driven by higher success rates in the recognition process (e.g., because of faster bureaucratic procedures), but rather by an increase in the number of applications for occupational recognition.

Second, we show that more occupational recognitions translate into higher employment rates in licensed occupations. Non-EU immigrants are 1.7 percentage points more likely to work in these occupations in the post-reform period, an increase of 18.6 percent relative to the pre-reform average. Compositional changes or selective in and out-migration do not drive these results. Additionally, we find that the reform had large employment effects also in a subset of non-licensed occupations for which occupational recognition is possible. This result highlights the importance of recognized certificates as mere proofs that immigrants acquired the required skills in their home country. Finally, we show that through its effects on employment in licensed and non-licensed occupations, the reform improved overall employment for non-EU immigrants.

Third, we study the reform's impact on the wage assimilation of non-EU immigrants in licensed occupations. We find that, in the post-reform period, the gap between non-EU and EU immigrant wages within regulated occupations decreased by 4 percent almost closing the existing gap. This convergence is not driven by compositional changes of non-EU immigrants or by a slowdown in EU immigrants' wage growth. Instead, a decline in the use of low-paying temporary contracts for non-EU immigrants who enter licensed occupations in the post-reform period can explain a large portion of the convergence. These findings are consistent with a decrease in employers' uncertainty about the quality of occupational certificates and a potential increase in non-EU immigrants' outside options.

Fourth, we estimate the effect of the reform on natives' employment and wages in licensed occupations. We find only small differences between natives in local labor markets exposed to either a large or small supply shock of non-EU immigrants in licensed occupations. These results are robust to alternative definitions of the supply shock measure. Wage rigidities, labor shortages, and skill complementarities between natives and non-EU immigrants can explain why natives' labor market outcomes were not sensitive to the size of the supply shock.

Taken all together, these results demonstrate that facilitating occupational recognition can generate large benefits both for immigrants, by enhancing their labor market integration, and for host countries, by reducing labor shortages in specific occupations. Despite political and economic concerns that these policies might crowd out native workers or put downward pressure on their wages, we find no evidence supporting these concerns.

Given these findings, the paper makes several contributions. First, we extend the literature on occupational recognition and skill transferability. Previous studies showed that occupational recognition substantially enhances immigrants' labor market outcomes (Kugler and Sauer, 2005; Tani, 2017; Brücker et al., 2021; Koumenta et al., 2022). However, when institutional barriers are in place, these gains are limited to a restricted group of immigrants. We show that lifting these barriers increases recognition rates and allows a larger number of immigrants to better integrate in the labor market of their host countries. Complementary to previous studies, we show that obtaining recognition improves employment outcomes not only in licensed, but also in non-licensed occupations. Additionally, we contribute to the literature on the determinants of wage assimilation (Dustmann and Glitz, 2011; Dustmann and Görlach, 2015; Borjas, 2015), showing that occupational recognition speeds up the convergence between non-EU immigrants' wages and those of EU immigrants and natives. This convergence does not only occur through occupational upgrading (Lessem and Sanders, 2020), but also within occupations.

Second, we contribute to the understanding of how immigrants make decisions about human capital utilization in the host country (Dustmann and Glitz, 2011; Adda et al., 2022). Our results indicate that bureaucratic hurdles and uncertainty about the outcome constitute important obstacles that prevent immigrants from seeking recognition of their qualifications. Given the high returns to recognition, this finding is surprising. However, it is in line with studies showing for other groups (e.g., students, welfare recipients, parents) that small changes in application procedures can strongly increase university, social program, or child care take-up (Bettinger et al., 2012; Hoxby and Turner, 2015; Bhargava and Manoli, 2015; Dynarski et al., 2021; Hermes et al., 2024). In the context of occupational recognition and skill transferability, we show that immigrants' decisions are highly sensitive to changes in application procedures. Lowering these barriers increases application and recognition rates, therefore closing the gaps in labor market outcomes between non-EU and EU immigrants.

Third, and more generally, we contribute to the extensive literature on policies aimed at improving immigrants' economic integration. Previous studies have evaluated interventions that provide additional skills to immigrants, such as language courses (Foged et al., 2022; Lochmann et al., 2019) and job search programs (Joona and Nekby, 2012; Sarvimäki and Hämäläinen, 2016; Battisti et al., 2019; Foged et al., 2024). Though often successful, these policies require immigrants to re-train and invest time and effort in the acquisition of new skills. Our paper is the first to evaluate a policy that facilitates the transferability of skills that immigrants already possess, but are not able to utilize in the labor market. We demonstrate that recognition policies can offer a cost-efficient way of improving integration for large groups of immigrants whose labor market potential would otherwise remain untapped.

Finally, we contribute to the literature on the effect of integration policies for immigrants on natives' labor market outcomes (Signorelli, 2024; Brinatti et al., 2023; Doran et al., 2022; Dustmann et al., 2017; Kerr et al., 2015), showing that the inflow of high skilled non-EU immigrants in licensed occupations did not displace natives, nor put downward pressure on their wages.

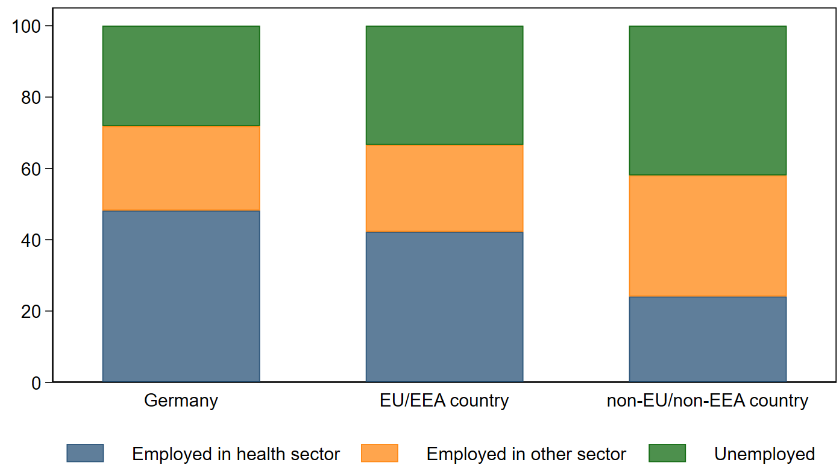
The rest of the paper is organized as follows. Section 2 describes the institutional setting in which the empirical analysis takes place. Sections 3 and 4 describe the data and the empirical framework, while Section 5.2 presents the main results. Section 6 provides additional analyses, and section 7 concludes.

2 Institutional Background and the Recognition Act

2.1 Institutional Setting and Recognition of Foreign Certificates

Immigrants fare worse than natives in the host country labor market with significant performance gaps based on the country of origin. For example, across European countries only 56% of non-EU immigrants with a foreign tertiary education work in highly skilled jobs, as compared to 66% of EU-educated immigrants and 80% of native-educated individuals (Frattini and Dalmonte, 2024). Figure 1 underscores this relationship, revealing similar disparities among German healthcare workers. Only 25% of non-EU immigrants with a foreign degree in healthcare work in the healthcare sector, as compared to 42% of foreign-trained EU immigrants, and 46% of domestic trained immigrants. Additionally, non-EU immigrants face higher unemployment rates than EU immigrants and natives.

Figure 1: Employment status of immigrants with a health care degree, by location of training



Notes: Figure 1 reports the employment status of healthcare graduates, separately for graduates that earned their degree in Germany, in the EU/EEA countries, or in non-EU/non-EEA countries. The sample includes all working age (18-65) individuals with a healthcare degree who participated in the German Microcensus between 2007 and 2010. The reported values are an average of the period 2007-2010. The German Microcensus is a 1% representative survey of the German population, which is used also for official statistics. Participation to the German Microcensus is mandatory. Source: German Microcensus, 2007-2010

Part of these gaps comes from occupational licensing rules which regulate the access to a wide range of occupations. In fact, working in a licensed occupation (e.g., doctors, nurses, engineers, architects, teachers) in Germany requires a domestic professional qualification or, for immigrants, the formal recognition of their foreign qualification.[1] Brücker et al. (2021) calculate that licensed occupations make up around 12 percent of total employment in Germany, of which 38 percent are in the health sector, 28 percent in the public sector, and 25 percent in the technical sector. Like in other countries (see for example Gittleman et al. (2018) for the US), in Germany licensed occupations exhibit on average higher wages and a steeper wage growth. Occupations can be licensed at the federal level (*Bundesebene*)

[1] As in Brücker et al. (2021) we use licensed and regulated occupations as synonymous words.

or at the state level (*Landesebene*) in Germany. Other than the responsible authority for the recognition process, these two groups of occupations hardly differ in their recognition procedures.

In contrast, entering a non-licensed occupation requires no formal recognition. Nonetheless, for most of these occupations, immigrants can apply for an official assessment of their home country occupational qualifications. If recognition is successful, that assessment becomes a legally binding document validating the equivalence with the German qualification. Examples of such unlicensed occupations are those requiring vocational training (e.g., office management clerks, electricians) and advanced training occupations (e.g., technician qualifications, certified financial advisors).

Despite the large number of eligible immigrants and the potential gains from recognition, applying for recognition in Germany before 2012 was an unstructured lengthy process for immigrants with qualifications from non-EU countries. Applicants had to face different authorities responsible for the recognition procedure depending on the occupation, the maximum duration of the process was not defined by a law, and guidelines about which documents were necessary for recognition did not exist. Additionally, applicants had no financial support to cover the administrative fees, which ranged between 100 and 600 euros (120-720 US-Dollars) depending on the occupation and the federal state in which the application was submitted (BMBF, 2017). Given these constraints, according to the German Microcensus[2], before 2012 only 20 percent of eligible non-EU immigrants applied for recognition of their home-country certificates, 10 percentage points less than eligible EU immigrants, for whom the recognition procedure was easier and more structured before 2012 (see Michel (2018) on the European Directive 2005/36/EC).

Data from the IAB-SOEP Migration Sample confirm that non-EU immigrants faced higher institutional barriers.[3] In Table A.1 we report the reasons why immigrants did not

[2]See Boehle and Schimpl-Neimanns (2010) for a detailed description of the German Microcensus.

[3]See Section 3 for details on the construction of the IAB-SOEP Migration Sample used for these statistics and the analysis.

apply for recognition before 2012, despite having obtained a qualification abroad and being eligible for recognition. Twenty-four percent of non-EU immigrants compared to only fifteen percent of the eligible EU immigrants reported that they did not apply due to administrative constraints. Furthermore, twenty percent of eligible non-EU immigrants without application stated that they saw no chance of obtaining recognition, compared to only fifteen percent of eligible EU immigrants.[4] Overall, these numbers indicate that reducing administrative hurdles and increasing information may increase application rates.

2.2 The Federal Recognition Act in 2012

To reduce bureaucratic hurdles and facilitate the process of occupational recognition for immigrants with a non-EU certificate, the German parliament passed the Federal Recognition Act (*Anerkennungsgesetz*) in April 2012. The Recognition Act harmonized the recognition process between EU and non-EU certificates introducing four major changes for non-EU immigrants. First, and most important, the new law created a legal basis for occupational recognition for all immigrants, independent of their country of origin. This legal base gave to administrative bodies binding rules for the recognition process and to migrants the right to legally enforce these rules.

Second, the reform restructured, standardized, and facilitated procedures for the assessment of equivalence between foreign and German certificates. Specifically, the new framework (1) allowed immigrants to send a standardized application form to administrative bodies with clear competent jurisdiction (also from abroad), (2) allowed that the proof of equivalence considered not only certificates but also work experience in the home-country,[5] (3) provided a guideline for all administrative bodies to make a decision within three months of the submission of an application.[6] Third, after the reform, the government offered and

[4]These survey data were collected in the pre-reform period, where already about 90 percent of all applications were successful.

[5]For example, doctors who acquired their qualification abroad can obtain recognition if they can prove that they worked several years in a hospital, although their certified training was shorter than what is required from the German training system.

[6]After applying, immigrants may receive three types of standardized decisions: fully recognized (the

advertised subsidies covering the costs of the application process. Fourth, from 2012 onwards the government established numerous sources of information about the recognition procedure (e.g., multi-language dedicated websites, mobile apps, hotlines), sources that could be accessed both in Germany and from abroad.[7]

While all legal changes apply to professional and vocational qualifications and to university degrees with a clear link to licensed occupations (e.g., physicians, dentists, pharmacists), they do not apply to the recognition of higher education qualifications that do not lead to a specific occupation (e.g., biologist, computer scientist or linguist). Nor does the new framework include the academic recognition of high-school diplomas. For occupations licensed at the state level (e.g., teachers, youth social workers, engineers, architects) each federal state passed its own Federal State Recognition Laws, between 2012 and 2014, which all follow the content of the Federal Recognition Act.[8]

Whereas before 2012 the German statistical offices barely kept records on the recognition process, since 2012 German authorities began a structured data collection on all applications. These records show that since the implementation of the Recognition Act, the number of applicants has steadily increased from 15,000 submissions, up to more than 60,000 per year (see Figure A.3).[9] Occupations licensed at the federal level received the largest number of applications, followed by non-licensed occupations and occupations licensed at the state level.

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only way for accessing a licensed occupation), partially recognized, and not recognized. For partial or non-recognition, applicants receive compensatory measures to help them reach full recognition.

[7]Figure A.1 displays an example from the website *www.anerkennung-in-deutschland.de*, the main web portal for immigrants interested in acquiring information on the recognition procedure.

[8]The timing of Federal State Recognition Laws is summarized in Figure A.2. Out of 16 federal states, 13 passed the Recognition Laws within 2.5 years since the Federal Recognition Act. The absence of never-treated federal states and the temporal concentration State Recognition Laws does not allow us to adopt a staggered difference-in-differences design. We therefore use only the timing of the Federal Recognition Act for the main analysis.

[9]The number of total applications rose to 420,000 by 2021, according to recent numbers from the Ministry of Education https://www.bmbf.de/bmbf/de/bildung/integration-durch-bildung-und-qualifizierung /anerkennung-auslaendischer-berufsqualifikationen/anerkennung-auslaendischer-berufsqualifikationen/node.html

3 Data and Sample Characteristics

Our main data source are the German social security records, which we use to analyze the effects of the reform on labor market outcomes. We complement these data with detailed survey data from the IAB-SOEP Migration Sample on immigrants' application and recognition outcomes.

Social Security Records For the analysis of immigrants' labor market outcomes we rely on the social security records, *Integrated Employment Biographies* (henceforth, IEB), for a random draw of 15 percent of the full population of immigrants in the German labor market.[10] The Institute of Employment Research (IAB) of the German Federal Employment Agency provides the data.[11] The dataset includes detailed daily administrative longitudinal information on nationality, occupation, educational background, industry, employment status, and earnings records of all individuals subject to social security in Germany.

Our main outcome is employment in licensed occupations defined as a binary variable that takes value 1 if individuals work in a licensed occupations and zero otherwise (in Section 5.2.1 we test the sensitivity of our results to alternative outcome definitions). To construct the outcome variable, we manually identify each 3-digit occupation as either licensed at the national level, licensed at the state-level or non-licensed following the European Commission List of Licensed Occupations for Germany.[12] In the baseline analysis we combine nationally and state-level licensed occupations.

To build the analysis sample, we create a yearly panel selecting all job spells referring to June 30 between 2007 and 2017, and restricting the sample to individuals with either vocational training or university degree, within the age range 23 to 55 years, and with either

[10] Given the smaller sample size when we consider only immigrants who move to licensed occupations, in Section 5.3 we use a random draw of 70 percent, the maximum allowed given the size of the resulting extraction and data protection requirements.

[11] For the description of a 2 percent random sample from the IEB, the *Sample of Integrated labor Market Biographies* (SIAB), see Antoni et al. (2019).

[12] For a complete list of licensed occupations by country at $https://ec.europa.eu/growth/tools-databases/regprof/$.

a EU or non-EU nationality. We identify whether an immigrant belongs to the EU and non-EU group by a nationality variable, which is reported for each job spell.[13] We exclude immigrants older than 55 because they are close to retirement and therefore more likely to leave the sample during the period.[14] In the main sample we also exclude immigrants who entered Germany after 2012 since other factors, such as the European Blue Card and the 2015 Refugee Crisis, may have affected both the number and the composition of incoming immigrants (we elaborate on this sample selection in Section 4). The final sample for the main analysis consists of an unbalanced panel of 489,789 observations for 75,138 immigrants arrived before 2012 in Germany.

One concern with the social security data is that we do not know precisely whether individuals acquired education abroad. This information is important to minimize the possibility of including immigrants who acquired education or training in Germany and are therefore unaffected by the Federal Recognition Act. To address this concern we restrict our sample to immigrants with non-German nationality whose first recorded educational level was either vocational training or tertiary education and who entered the register when they were older than 23 years. We discuss this approximation in detail and validate it with external data sources in Appendix B.

Table 1 presents socio-demographic characteristics and labor market outcomes for the main sample separately for EU and non-EU immigrants. The two groups are similar in terms of age, age at entry in the register and the time spent in Germany. Compared to EU immigrants, non-EU immigrants are more likely to be female (47 versus 39 percent) and are less likely to have acquired higher education (33 versus 39 percent). With respect to labor market outcomes in the pre-reform period, non-EU immigrants' employment rates are 18 percentage points lower than their EU counterparts. When employed, non-EU immigrants are less likely than EU immigrants to be employed in occupations eligible for recognition

[13] Given that individuals may naturalize or report more than one citizenship we use the most frequent nationality value. In alternative specifications we also use the first reported nationality variable.

[14] In Appendix B we provide additional details on the sample selection and the construction of nationality, education, and occupation variables.

Table 1: Descriptive statistics for the main samples of immigrants

	All		Non-EU		EU	
	Mean	SD	Mean	SD	Mean	SD
	Panel A: Social Security Records					
Demographic characteristics						
Female	0.43	0.49	0.45	0.50	0.39	0.49
Higher Education	0.37	0.48	0.33	0.47	0.44	0.50
Age	41.24	7.86	41.24	7.76	41.24	8.07
Age at entry	30.36	6.06	30.61	6.02	29.84	6.10
Years in Germany	10.38	6.99	10.13	6.58	10.91	7.74
Labor market outcomes (pre-reform)						
Employed	0.79	0.38	0.74	0.41	0.92	0.27
Employed in regulated occupations	0.11	0.34	0.09	0.32	0.14	0.36
Employed in non-regulated occupations	0.35	0.48	0.31	0.47	0.43	0.50
Index of occupational regulation	0.15	0.29	0.13	0.27	0.19	0.32
Daily wage (deflated)	84.43	56.30	70.46	48.78	105.98	60.19
Observations	489,789		329,666		160,123	
	Panel B: IAB-SOEP Migration Sample					
Demographic characteristics						
Female	0.54	0.50	0.54	0.50	0.54	0.50
Vocational training	0.52	0.50	0.48	0.50	0.57	0.50
University of applied sciences	0.18	0.38	0.18	0.38	0.17	0.38
University	0.28	0.45	0.32	0.47	0.23	0.42
PhD	0.02	0.15	0.02	0.14	0.03	0.17
Age	43.66	10.23	42.09	9.68	45.81	10.57
Age at entry	31.73	9.44	30.48	8.87	33.45	9.91
Years in Germany	9.18	5.05	8.85	5.03	9.64	5.05
Recognition outcomes (pre-reform)						
Applied for recognition	0.29	0.45	0.27	0.44	0.30	0.46
Obtained recognition	0.29	0.45	0.28	0.45	0.31	0.46
Observations	9,263		5,358		3,905	

Notes: Table 1 reports variable means and standard deviations for the sample of EU and non-EU immigrants in the Integrated Employment Biographies (Panel A) and the IAB-SOEP Migration Sample (Panel B). Demographic characteristics and number of observations refer to the whole sample. Labor market and recognition outcomes are computed from observations between 2007 and 2010.
Source: Integrated Employment Biographies (IEB) and IAB-SOEP Migration Sample.

(both licensed and non-licensed), and their daily wage is 35 euros lower than the daily wage of EU immigrants.

IAB-SOEP Migration Sample As a second data set, we exploit the IAB-SOEP Migration Sample (Brücker et al., 2014) to estimate to what extent the reform increased application and recognition rates. The IAB-SOEP Migration Sample is a unique panel dataset constructed on a sample of immigrants interviewed in 2013, 2014, 2015, and 2016. Respondents answered in addition to the standard German Socio-Economic Panel (henceforth, GSOEP) survey items, also questions about their nationality, immigration biography (e.g., year of arrival in Germany), and education obtained abroad. Crucial for our research question, for respondents with foreign education the data contain information about whether they applied for recognition (obtained recognition) and, if so, the month and year of application (recognition). We construct three outcomes as binary variables which take value 1 if individuals i) applied for recognition, ii) completed the recognition procedure and iii) successfully acquired recognition conditional on having completed the application procedure, and 0 otherwise.[15]

The analysis sample includes all individuals aged 18 to 65 who have a professional certificate or a higher education degree acquired abroad - and are therefore eligible for recognition - and who arrived in Germany between 1992 and 2012 (i.e., in the 20 years before the reform).[16] Since the survey asks retrospective questions on occupational recognition, for each individual we construct a yearly panel from the year they arrived to Germany to the last survey year available. The main sample consists of 9,263 yearly observations for 1,308 individuals.

Panel B in Table 1 reports socio-demographics characteristics and pre-reform outcomes for the full sample, and separately for EU and non-EU immigrants. Before the reform, application and recognition rates are respectively 27 percent and 28 percent for non-EU immigrants,[17] and 30 percent and 31 percent for EU immigrants. As for the social security

[15] In Appendix B, we describe the recognition variables in the IAB-SOEP Migration Sample in more detail, the construction of the panel, and test the validity of recognition variables in the GSOEP by comparing them with other data sources.

[16] Given the small sample size and the precise information on education attained abroad, we include here all individuals aged 18-65, while in the IEB analysis we include individuals aged 23-55. In the Appendix, we report all results restricting the age range to 23-55 (see Table A.7), and including also immigrants arrived after 2012 (see Table A.5).

[17] The share of applications is slightly higher than the data from the Microcensus (presented in Section

data, while similar in all other demographic characteristics, EU immigrants tend to be more highly educated than their non-EU counterparts.

4 Empirical Strategy

To estimate the effect of lifting institutional barries to occupational recognition on the integration of non-EU immigrants, we exploit the fact that the 2012 Federal Recognition Act eliminated differences in the recognition process between EU and non-EU immigrants.[18] Specifically, the reform introduced a formal recognition framework applying equally to all immigrants, regardless of their country of origin. While the new framework clearly improved non-EU immigrants' possibilities of obtaining recognition, it introduced no change for EU immigrants, who were already benefiting from a standardized recognition system. Specifically, in 2007 Germany implemented the 2005 European Directive EC/2005/36a, which required EU member states to introduce a standardized recognition process to facilitate the recognition of foreign credentials within EU and EEA states.This variation forms the basis of our DiD design, in which non-EU immigrants are treated and EU immigrants are the control group.[19]

In our main analyses, we estimate the following empirical model:

$$Y_{igt} = \beta Post_t * NonEU_g + \gamma NonEU_g + \eta Post_t + \lambda' X_{igt} + \psi'_{igt} + \epsilon_{igt} \tag{1}$$

where the dependent variable Y_{igt} is a recognition or labor market outcome as described

2). This is likely due to two reasons: first, the IAB-SOEP sample is drawn from the social security data, so that individuals who never appeared in the register were not sampled. Second, the data on recognition are retrospective, so that they do not account for attrition. Both factors bias upwardly the recognition rate.

[18] Agersnap et al. (2020) use a similar design to study the effect of a welfare reform in Denmark applying only to non-EU immigrants while leaving untouched the welfare benefits for EU immigrants. Elias et al. (2024) compare non-EU and EU to estimate the integration effects of a Spanish amnesty program that regularized 600,000 undocumented non-EU immigrants.

[19] To improve the validity of the control group, we exclude immigrants from countries that entered the EU during the last two European Union enlargements. After the 2004 Eastern Enlargement, EU15 countries were allowed to apply transitional restrictions to the free movement of the new EU workers. Germany lifted these restrictions in 2011 for the 13 countries which entered the EU in the 2004 Eastern Enlargement. This event might therefore confound the effects of the reform for immigrants from these 13 countries.

in Section 3, $NonEU_g$ is a binary variable that takes value 1 if the immigrant's nationality is from a non-EU country, $Post_t$ is a dummy equal to 1 for the post-2011 years. X_{igt} is a vector of individual characteristics, ψ_{igt} are group fixed effects and ϵ_{it} is the error term.[20] In all regressions, standard errors are clustered at the individual level to take into account the panel structure of the data. The parameter of interest β measures the effect of the reform on outcome Y_{igt} for non-EU immigrants.

We also present results of dynamic DiD regressions where, instead of a $Post_t$ dummy, we include interactions between the $Treat_g$ dummy and year fixed effects η_t:

$$Y_{igt} = \sum_{\tau} \beta_\tau NonEU_g * 1(t = \tau) + \gamma NonEU_g + \lambda' X_{igt} + \psi'_{igt} + \eta_t + \epsilon_{igt} \quad (2)$$

As in the social security data we do not directly observe which immigrants apply for or obtain recognition, our estimates for labor market outcomes should be interpreted as intention-to-treat effects. In Section 5.2 we discuss how the estimated effects on recognition and employment outcomes are related.

Our identification strategy relies on the assumption that, absent the Recognition Act, occupational recognition and employment outcomes of EU and non-EU immigrants would have followed parallel trends. This assumption may be violated, for example, if the composition and the size of the treated or control group changed over time, or if differences between the two groups would have set them on different trends also in the absence of the reform. As plausibility check for the parallel trends assumption, we provide extensive evidence on the absence of pre-trends in the pre-reform years, and check for anticipatory effects of the reform. Nonetheless, even if pre-trends are parallel, we cannot directly test that occupational recognition and employment outcomes of EU and non-EU immigrants would have followed pre-reform (parallel) trends also in the post-reform years.

[20] Individual controls include age, age squared, age at arrival (proxied by age at entry in the social security register for wage and employment models), age at arrival squared, nationality (as a proxy for country of origin), sex and education. To capture time-constant geographical trends and time-varying trends, we also include local labor market fixed effects, year fixed effects (or alternatively local labor market-year FE and education-year fixed effects).

As described in Section 3, one major threat to the parallel trend assumption is the presence of confounding policies and factors after 2012. Specifically, in 2012 the German Residence Act granted non-EU immigrants with specific advanced degrees a work permit (the so-called Blue Card) as long as German authorities recognized those degrees. The combination of the Blue Card Act and the Recognition Act might have affected not only the integration but also the selection of immigrants coming to Germany.[21] Moreover, from 2015 onwards Germany experienced a large inflow of refugees during the Refugee Crisis, dramatically changing the composition of non-EU immigrants. To isolate the effect of the Recognition Act on immigrants' labor market integration, our baseline analysis is restricted to immigrants who were in Germany already before 2012.[22]

Other threats to this assumption may persist even after restricting our sample to immigrants who arrived pre-reform. First, selective in- and out-migration or sample attrition could have changed the composition of treated and control groups over time. On the one hand, the reform might have affected immigrants' decision to leave Germany. On the other hand, economic shocks (e.g., the Great Recession) might have differently affected the labor market opportunities (e.g., self-employment periods, not covered in our administrative data) of non-EU and EU immigrants living in Germany. Similar concerns would apply if non-EU immigrants knew about the reform before and self-selected into migrating to Germany based on the perceived probability of recognizing their certificates. In Section 5, we address these additional concerns using balanced panels, matching approaches and alternative control groups.

[21] For example, in a recent paper Abarcar and Theoharides (2020) show that the expansion and contraction of U.S. visas for nurses in the 2000s changed accordingly both the number of foreign-trained nurses in the U.S. and the enrollment rates in nursing programs in the Philippines. Patt et al. (2021) provide evidence of immigrants' self-selection based on the occupational skills most rewarded in the host country labor market.

[22] In secondary analyses of Section 5, we include also immigrants who entered Germany in the post-reform period and report the estimated recognition and employment effects for this broader group.

5 Results

5.1 Effects on Occupational Recognition

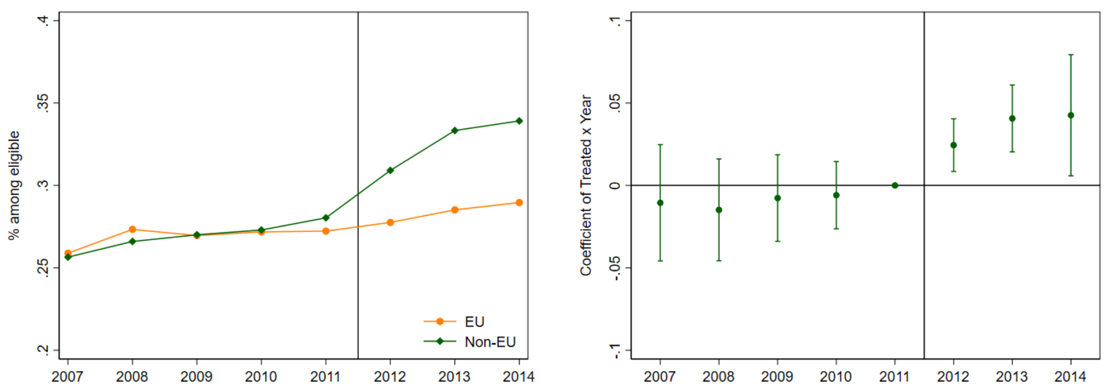
Using the IAB-SOEP Migration Sample, we estimate the effects of lifting institutional barriers to skill transferability on recognition outcomes. We use three binary outcomes (as defined in Section 3) which take value 1 if individuals obtained occupational recognition, applied for recognition, and successfully acquired recognition conditional on having applied, and 0 otherwise. In the main analysis we include only immigrants arrived before 2012, i.e., before the implementation of the Recognition Act.

Occupational recognition Figure 2a displays the share of non-EU and EU immigrants who obtained occupational recognition. The shares for EU immigrants are adjusted by its pre-reform mean difference (computed across 2007-2011) relative to the non-EU immigrants. Figure 2b displays the event study coefficients from estimating Equation 2 on the probability of obtaining occupational recognition. Both figures show that up to 2011 the difference in recognition rates between EU and non-EU immigrants remained stable. From 2012 onwards the coefficients for the interaction between time and the non-EU dummy are positive and statistically significant (see also Table A.4, column 1). Table A.3, Panel A, summarizes the post-reform effect, reporting the estimated β coefficients from Equation 1. Averaging across the post-reform period, the probability of obtaining occupational recognition increased by 3.8 percentage points for non-EU immigrants. The point estimate corresponds to a 14.4 percent increase relative to the pre-reform period.

Application and success rates In Figure A.4, Table A.3 (Panels B and C) and Table A.4 (Columns 2 and 3) we report the same results for two additional recognition outcomes, application rates and success rates (i.e., the share of successful occupational recognitions conditional on submitting an application). We find similar effect sizes on applications, and smaller not statistically significant coefficients for success rates. These results suggest that the increase in occupational recognitions among non-EU immigrants is driven by an increase

Figure 2: Effect of the Recognition Act on occupational recognition, event study plots

(a) Adjusted Levels (b) Differences



Notes: The figure displays recognition shares (panel a) and event study coefficients (panel b) for the probability of obtaining recognition (estimated by Equation 2). The baseline coefficient is the interaction between year 2011 and the dummy identifying non-EU immigrants. The vertical line indicates the year before the Recognition Act in 2012. The group of EU immigrants includes also ethnic Germans. These are immigrants with German origins that benefit from recognition procedures similar to EU immigrants. In Panels a) the outcomes for the EU immigrants are adjusted by its pre-reform mean difference (computed across 2007-2011) relative to the non-EU immigrants.
Source: IAB-SOEP Migration Sample, waves 2013, 2014, 2015, 2016

in applicants, and not by a higher chance of receiving a positive decision about recognition (e.g., because of changes in administrative procedures).

Robustness Results on recognition outcomes are robust to changing the set of individual controls and fixed effects (see Table A.3, Columns 1-4). Moreover, in Figure A.5, we expand the analysis' time window from 2007-2014 to 2000-2014 to address concerns that EU and non-EU immigrants were on differential trends before 2007 as the EU Directive 2005/36/EC only affected the recognition processes of *EU and EEA* immigrants. The figure demonstrates that trends for EU and non-EU immigrants have been consistently parallel throghout the extended pre-reform period. In Tables A.7 and A.5, we also show that results are virtually identical if we apply the same sample restrictions as in the employment analysis (described in Section 3) or if we include also immigrants who arrived in Germany after the reform. Finally, using data from Google searches in Germany,[23] we provide evidence against anticipatory

[23] Google Trend data have been already shown to proxy well for individual behaviors in other contexts, such as job search (Baker and Fradkin, 2017), migration decisions (Böhme et al., 2020), and domestic violence (Anderberg et al., 2022).

effects and in favor of an increased attention towards recognition opportunities. Figure A.7 clearly shows that the increase in Google searches for the word "*Anerkennung in Deutschland*" ("Certificate recognition in Germany") starts precisely around the time of the Recognition Act implementation in April 2012 - and not earlier - and keeps growing thereafter.

Taken together, the results on recognition outcomes show that administrative hurdles and the lack of information on occupational recognition prevented non-EU immigrants from applying. Lifting these institutional barriers had a large impact on immigrants' behavior, increasing applications - and successful recognitions. This increase is driven by immigrants who were already in Germany before the reform and were not fully utilizing their foreign-acquired skills. In the next section, we investigate whether the increase in occupational recognitions translates into better employment outcomes.

5.2 Effects on Employment

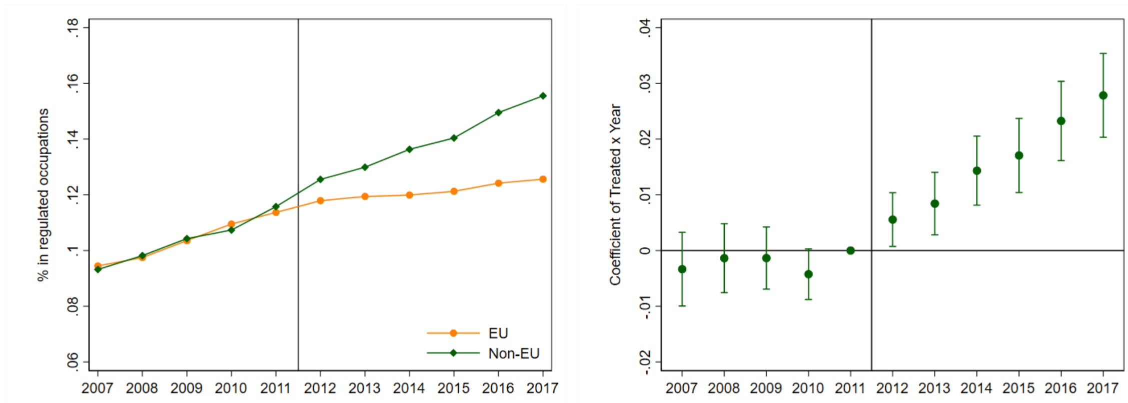
In this section we estimate Equations 1 and 2, using employment in licensed occupations as dependent variable that takes value 1 if individuals work in a licensed occupations and zero otherwise. As for Section 5.1, the baseline sample only includes immigrants arrived in Germany before 2012.

Baseline estimates Figures 3a and 3b report the yearly share of non-EU and EU immigrants in licensed occupations and the event study coefficients from Equation 2 (see also Table A.8). Shares for EU immigrants are adjusted by its pre-reform mean difference (computed across 2007-2011) relative to the non-EU immigrants. Both figures show that, in the post-reform years, non-EU employment in licensed occupations strongly increased, as compared to EU immigrants. In contrast, pre-reform coefficients (left of the vertical line) are not statistically different from zero and the adjusted levels run parallel, supporting the parallel trend assumption.

Table A.9 reports the estimated coefficients for the interaction between nationality and the reform dummy (Equation 1), summarizing the post-reform effect. In the post-reform

Figure 3: Effects of the Recognition Act on employment in licensed occupations, event study plots

(a) Adjusted Levels (b) Differences



Notes: Figure 3 displays levels and differences with the corresponding 95 percent confidence intervals for the regression model of Equation 2. The outcome is a dummy variable which is one if a migrant is employed in a licensed occupation and zero otherwise. The baseline year is 2011. In the left panel, the outcome for EU immigrants are adjusted by its pre-reform mean difference (computed across 2007-2011) relative to the non-EU immigrants.
Source: Integrated Employment Biographies (IEB).

period, the probability of working in licensed occupations increased for non-EU immigrants by 1.8 percentage points (Column 1). This point estimate corresponds to a 19.0 percent increase relative to the pre-reform baseline share (9.25 percent) of non-EU immigrants employed in these occupations. The coefficients barely change when the estimation includes a large set of individual control variables and group fixed effects (columns 2-3).[24]

How large are these effects in absolute terms? Our 15 percent sample includes 49,724 non-EU immigrants who entered Germany pre-reform and have qualifications eligible for recognition. The sample corresponds to 331,493 individuals in the entire population. Of these 331,493, 9.25 percent (30,663) worked pre-reform in licensed occupations. The 19.0 percent increase implies that, after to the reform, 5,826 non-EU immigrants work in licensed occupations and would not have done so without the reform. Given that in the pre-reform period (2007-2010) the maximum number of excess vacant jobs in licensed occupations was

[24]To capture the effect of immigrants entering employment in licensed occupations both from unemployment and from a different occupation, in the main specification we include both employed and unemployed immigrants. In Table A.10 we show results when the sample includes only employees and only full-time employees. The results are statistically significant and only slightly smaller in magnitude.

12,000 the effects are sizable in absolute terms.[25]

Table 2: Effects on employment by subgroups of licensed occupations

	(1)	(2)	(3)	(4)	(5)
	All	Top 15	Top 15 (Federal)	Top 15 (State)	Other
Post*Non-EU	0.0167***	0.0147***	0.0080***	0.0067***	0.002
	(0.0027)	(0.0023)	(0.0016)	(0.0017)	(0.0017)
Baseline	0.0925	0.0659	0.0418	0.0324	0.0317
R-Squared	0.0576	0.0543	0.0503	0.0241	0.0266
Observations	489,749	489,749	489,749	489,749	489,749
Individuals	75,138	75,138	75,138	75,138	75,138

Notes: Table 2 reports estimated coefficients and standard errors from Equation 1. In column 1, the outcome is the probability of being employed in any licensed occupation. In column 2 the outcome is employment probability in one of the 15 occupations (TOP 15) that received the highest number of applications overall (93% of all applications). Columns 3 and 4 distinguish between TOP 15 occupations licensed at the federal or state level. Column 5 reports the coefficient for the probability of being in the residual group of licensed occupations (receiving the remaining 7% of applications). The reported baseline is the average value of the dependent variable for the treated group (i.e. Non-EU immigrants) in 2007-2010. Only immigrants arrived in Germany pre-reform are included in the estimation. Individual controls include sex, age, age squared, age at entry, age at entry squared, nationality, educational level. LLM stands for "Local Labor Market". Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$. Source: Integrated Employment Biographies.

Subgroups of licensed occupations Table 2 reports effects on employment in different subgroups of licensed occupations (see Figure A.9). Column 1 shows the effects for the probability of being employed in any licensed occupation. In Column 2 the outcome is employment in licensed occupations that received the largest number of applications (hereafter, TOP 15).[26] Although the coefficient (1.5 pp) is smaller for occupations with the most applications compared to the baseline estimate, it corresponds to a relative effect of 22.3 percent, which is larger than the overall effect from Column 1. Columns 3 and 4 present the effects separately for TOP 15 occupations, licensed at the federal and state level. For both

[25] Own calculations from statistics of the Federal Statistical Office about vacancies and registered unemployed at the occupation-year level. The excess vacant jobs are computed as within-occupation difference between vacancies and registered unemployed who search for jobs in the occupation (in German, *Zielberuf*).

[26] Among licensed occupations, the top 15 occupations, in which before the reform 73 percent of all immigrants in licensed occupations worked, received about 93 percent of all applications, while for non-licensed occupations applications are less concentrated (around 50 percent in the top 15). Table A.2 displays the 15 occupations with the highest number of applications, licensed and non-licensed.

regulation types, the effects are significantly positive and the increase relative to the baseline (19.1 percent for federal and 20.7 percent for state) appears comparable. For occupations that received the remaining 7 percent of applications (Column 5), we find a small and not statistically significant effect.

Relating recognition and employment effects In the previous section we showed, using survey data, that the reform had an effect on the recognition rate of non-EU immigrants. However, as we do not directly observe recognition applications or outcomes in the administrative data, one concern is that the effect on employment could have also occurred without the increase in non-EU recognition rates. For example, non-EU immigrants could have obtained recognition of their certificates pre-reform but only started using them in the post-reform period.

Three of our findings provide evidence that the estimated employment effects can be reconciled with the increase in recognized certificates. First, the effects on recognition and employment are comparable in magnitude. Second, employment effects are concentrated only in occupations that received the majority of recognition applications. Third, in a back-of-the-envelop calculation we scale the effects on employment by the effects on recognition (1/0.04), and estimate that the effect of obtaining a recognized certificate increases the probability of being employed in licensed occupations by 42 percentage points. This effect is close in magnitude to the individual fixed-effects estimates in Brücker et al. (2021).

5.2.1 Robustness Checks

Alternative control groups If employment probabilities for EU immigrants changed as a result of either the reform or factors coinciding with it, the estimated reform effects may depend on the choice of EU immigrants as the control group. If EU and non-EU immigrants are close substitutes an increase of non-EU immigrants labor supply may directly reduce the employment of EU immigrants, leading to an overestimation of the reform effects. We therefore test the robustness of our results against using two alternative control groups, German citizens and non-EU immigrants with education acquired in Germany.[27]

The reform affects none of the groups directly, and both groups are less likely to be substitutes to foreign-trained non-EU immigrants than foreign-trained EU immigrants.[28] Using non-EU citizens with German education as a control group has the additional advantage that controlling for nationality allows us to compare the outcomes of treated and controls with the same ethnic background. Therefore, we rule out the possibility that changes in hiring behavior that are purely based on immigrants' nationality (e.g., stronger or weaker ethnic discrimination) drive our results.[29]

In Table 3, Columns 1 and 2, we report the results from regression models in which the outcome and the treated group (non-EU immigrants) are the same as in our baseline estimations, while the control groups are either Germans with vocational or university degrees (Column 1) or non-EU immigrants who completed vocational training or higher education in Germany (Column 2). When we use alternative control groups, the effects of the reform for non-EU immigrants who acquired their education abroad are remarkably similar to those estimated with EU immigrants who acquired their qualifications abroad as the control group. Furthermore, in Table A.11 we run placebo estimations and show that the reform effects are virtually zero when we use EU immigrants as the treated and Germans as the controls (Column 2), or when using EU immigrants educated abroad as the treated and EU immigrants with a domestic education as the controls (Column 4). Overall, these results provide evidence that the choice of our control group does not drive our main results.

Matching Although results are robust to alternative control groups, Table 1 in Section 3 shows that non-EU and EU immigrants are different in their baseline characteristics. These differences may affect the development of EU and non-EU immigrants' outcomes. As an

[27] This group is defined as non-EU immigrants who entered the register before they were 20 years old and with either vocational training or university as their highest educational level.

[28] For example, Signorelli (2024) shows that a selective immigration policy in France, aimed at increasing the hiring of non-EU immigrants in specific occupations, did not affect natives' employment. She explains this finding in terms of an imperfect degree of substitution between natives and non-EU immigrants.

[29] We do not use non-EU with German education as main control group because they do not need occupational recognition. Therefore, we are not able to estimate the reform effects on recognition outcomes using them as control group.

Table 3: Robustness checks

	(1)	(2)	(3)	(4)
	Alternative control groups		Alternative samples	
	Native Germans	Non-EU with German education	Matched	Balanced
Post*Non-EU	0.0170***	0.0156***	0.0133***	0.0152***
	(0.0018)	(0.0019)	(0.0029)	(0.0029)
Baseline	0.0926	0.0797	0.1136	0.0905
R-Squared	0.0349	0.0392	0.0698	0.0545
Individuals	2,457,501	728,942	343,952	229,587
Observations	329,995	102,284	52,924	23,982

Notes: Table 3 reports estimated coefficients for a series of robustness checks. In Column 1, we use native Germans as control group, while in Column 2 we use non-EU immigrants who received their education in Germany as control group. We proxy this group by including non-EU immigrants who entered the register before age 20 and obtained the highest education level (either vocational or university degree). In Column 3 we restrict the sample to a 1:1 matched sample of EU and non-EU immigrants, while in Column 4 to a balanced panel of immigrants who remained in Germany throughout the period 2007-2017. In all specifications we include individual controls, year and local labor market fixed effects. Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: Integrated Employment Biographies (IEB).

additional test, we therefore use a propensity score matching approach to create a sample of EU and non-EU immigrants which is balanced in terms of education, sex, time and age of arrival (Table A.13). Each non-EU immigrant is assigned to the EU immigrant with the closest propensity score without replacement. In Table 3, Column 3, we report employment effects estimated on this sample. Results are close in magnitude to those obtained with the unmatched sample. Additional estimations with the same sample but different sets of fixed effects are reported in Table A.14.

Alternative sample and outcome definitions As described in Section 3, due to limited information in social security records, we do not directly observe which immigrants received their certificates abroad or in Germany. We approximate this information with a combination of other characteristics (age at arrival, education, nationality). This approximation may bias our results due to measurement errors. Therefore, in Table A.12, we show that the baseline results from Table 2 are robust to alternative sample definitions, where we use other versions of the education and nationality variable. Moreover, in Appendix B we externally validate our sample selection.

In Table A.12 we also test the sensitivity of our results to alternative outcome definitions and functional forms. We report results from Equation 1, using the log number of workers in licensed occupations (Column 5), and the occupational-level regulation index (Column 6). The regulation index categorizes occupations as licensed at the 8-digit level and only in a second step creates a weighted regulation score at the 3-digits occupational level. The higher the number of licensed 8-digit occupations, the higher the score at the 3-digits occupational level.[30] We find a positive effect of the reform for non-EU immigrants on both alternative outcomes.

Balanced panel In the baseline analysis we use an unbalanced sample of immigrants arrived before the reform in Germany. Within this sample, patterns of in- and out-migration may vary between EU and non-EU, biasing our estimates.[31] To address this concern, in Table 3, Column 4, we restrict our baseline sample to immigrants who had an observation in each year between 2007 and 2017, so that over the 2007-2017 period our estimation sample is balanced (in Table A.15 we report coefficients from additional specifications). The estimated coefficients are virtually identical to the baseline, providing evidence that selective in- and out-migration in the years around the reform does not change the effects of the reform on non-EU immigrants employment probabilities in licensed occupations.

Inclusion of immigrants arrived after the reform As described in Section 4, our baseline sample includes only immigrants arrived before the reform. We apply this sample restriction because in the post-reform period there are two major confounding factors, the 2012 European Blue Card initiative and the 2015 Refugee Crisis. In Table A.5 we showed

[30] The index ranges from 0 to 1. (see Vicari, 2014, for more details). In Table 3, we use the continuous index including zeros for occupational aggregates without any regulation. In Figure A.8 we show the coefficient plots for different definitions of the regulation index, excluding zeros and constructing a binary variable that takes the value 1 if the regulation index is above 0.

[31] The average length of stay in the register between 2007 and 2017 is 7 years for EU and 8 years for non-EU, allowing for the possibility that our sample might be subject to changes due to selective in- and out-migration

that including immigrants arrived after 2012 in the analysis of recognition outcomes does not significantly alter the baseline results. Here, we expand the employment analysis to immigrants arrived after the reform and examine the potential impact of both confounding factors in separate regressions, applying two different sample restrictions: (a) excluding Blue Card non-EU immigrants (and their EU counterparts);[32] (b) excluding immigrants from Syria, Iran, and Iraq, the largest refugee home countries.

Table 4: Employment effects for the sample of immigrants arrived before and after the reform

	(1)	(2)	(3)	(4)
	All	No Blue Card	No Refugees	No Blue Card/ No Refugees
Post*Non-EU	0.0126***	0.0107***	0.0164***	0.0148***
	(0.0025)	(0.0025)	(0.0025)	(0.0025)
Baseline	0.0925	0.0925	0.0918	0.0918
R-Squared	0.0608	0.0594	0.0618	0.0602
Observations	639,155	634,673	600,029	595,827
Individuals	140,631	138,824	126,135	124,425

Notes: Table 4 reports the estimated coefficients from regression models with the full sample of immigrants, including those arrived both pre- and post-reform. The outcome variable is the probability of being employed in licensed occupations. In Column 2, we exclude immigrants who entered the social security data after 2012, with the first employment spell in licensed occupations, and whose hourly wage exceeded 14.95 Euros (as a proxy for being EU Blue Card holder). To make treated and controls comparable we exclude both EU and non-EU immigrants meeting these criteria. In Column 3, we exclude immigrants from Syria, Iraq and Iran. In Column 4, we exclude both groups. In all specifications we include individual controls, year and local labor market fixed effects. Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: Integrated Employment Biographies.

Table 4 reports the estimated coefficient from Equation 1 including immigrants arrived after the reform. Compared to the baseline estimates (0.0167) in Table A.9, the point estimate in Column 1 is positive but smaller, indicating that non-EU immigrants arrived in Germany post-reform benefited less than those already in Germany pre-reform. Excluding non-EU immigrants who likely entered Germany through the EU Blue Card (Column 2) lowers the reform effects of 0.2 percentage points (11.5 percent against 13.6 percent). On the contrary,

[32]Blue Card holders are identified as non-EU immigrants who arrived after 2012, whose first spell in the register is in a licensed occupation and whose initial gross hourly wage exceeds 14.92 euros (gross annual salary 42.969 euros).

excluding potential refugees from the sample (Column 3) increases the effect on employment by 0.4 percentage points (17.7 percent against 13.6 percent), suggesting that this group has lower probabilities of employment in licensed occupations (e.g., due to more restricting temporary work permits). Without refugees, the size of the reform effect is similar to that of the main results. Column 4 reports the coefficient after excluding both Blue Card holders and refugees, which is slightly lower than the baseline effect with only pre-reform immigrants. These findings suggest that once we account for confounding changes, the estimated positive reform effects can be generalized to immigrants arrived after the reform.

5.2.2 Heterogeneity across Labor Markets: Skill Shortage and Immigrant Networks

In this section we investigate how the employment effects of the reform vary according to pre-reform characteristics of local labor markets. On the one hand, previous research has shown that co-ethnic networks can improve immigrants' job search and labor market integration (Munshi, 2003; Dustmann et al., 2016; Battisti et al., 2019), so that non-EU immigrants may find more easily employment in licensed occupations in local labor markets with a large non-EU network. On the other hand, the effects may be concentrated only in local labor markets where workers in licensed occupations are more requested. Indeed, in recent years Germany - as well as many other European and non-European countries - has experienced skill shortages in specific occupations and regions (Peichl et al., 2022).

To shed light on the heterogeneity of the main effects, we characterize local labor markets by their pre-reform labor demand and the concentration of non-EU workers. As explained in detail in B, to quantify demand for specific occupations we first acquire data by occupational code, year, and district (*Kreis*) on job vacancies and registered unemployed from the Statistical Office of the Federal Employment Agency[33]. Second, we construct unemployment-to-vacancy ratios in licensed occupations at the local labor market level, averaging across pre-reform

[33] See footnote 25 and Appendix B for a complete definition.

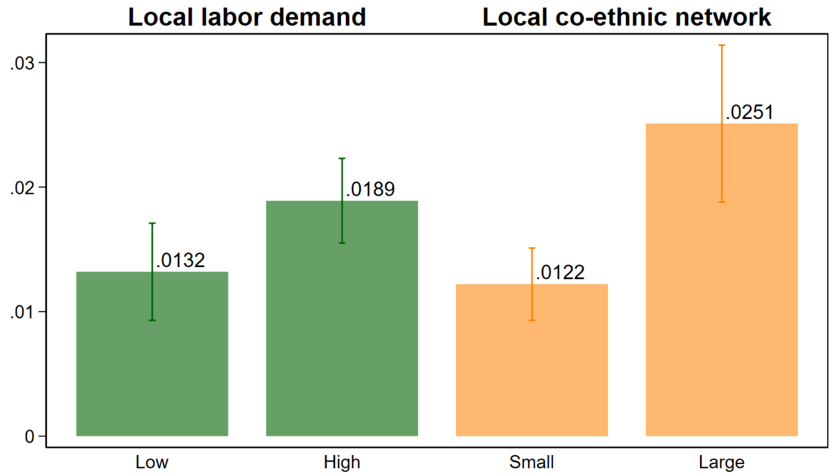
years (2007-2010). Third, we assign each local labor market to either high or low pre-reform demand based on whether their unemployment-to-vacancy ratio before the reform was below or above the median value across all local labor markets. Similarly, we split local labor markets according to the pre-reform share of non-EU immigrants with vocational training or university degree.[34]

Figure 4 displays the coefficients from separate regressions for the different types of local labor markets. The probability of entering licensed occupations for non-EU immigrants significantly increased in all local labor markets, independently from the pre-reform demand in these occupations. However, we find a larger coefficient in local labor markets with a stronger pre-reform skill shortage in licensed occupations. These results suggest that non-EU immigrants who were already in Germany before the reform were able to recognize their certificates in the post-reform period and contribute to relieve the skill shortage in licensed occupations. With respect to the non-EU network size in licensed occupations, we find stronger effects in local labor markets with a larger network of non-EU immigrants. This finding is in line with previous research on the positive role of co-ethnic networks on immigrants' labor market integration.

Finally, Figure A.10 and Table A.17 report estimated coefficients from Equation 1 for different subgroups of immigrants. While we find virtually no difference according to the time spent in Germany, the employment effects of the reform vary by sex and type of degree. Non-EU female immigrants and immigrants with a university degree acquired abroad are more likely to enter licensed occupations in the post-reform period. These results can be explained by the characteristics of licensed occupations, in which female workers are overrepresented (e.g., nurses and social workers) and for which a university degree is often required.

[34] The denominator is all workers - immigrants and natives - in the age range 23-55 and with vocational training or university degree

Figure 4: Heterogeneity of employment effects by local labor market characteristics



Notes: Figure 4 displays coefficients from separate regressions for local labor markets above and below the median value of pre-reform labor demand in licensed occupations and non-EU co-ethnic network in the local labor market. In all specifications we include individual controls, year and local labor market fixed effects. Standard errors are clustered at the individual level. Bars indicate 95% confidence intervals.
Source: Integrated Employment Biographies.

5.2.3 Effects of Employment in Non-Licensed Occupations

This section expands our main analyses by investigating the impact of the reform on occupation types beyond licensed occupations. These types include non-licensed occupations for which immigrants can apply for recognition (eligible non-licensed occupations) and occupations for which recognition is not available (non-eligible occupations). Furthermore, the section analyses the combined effect for licensed occupations, eligible non-licensed occupations, and non-eligible occupations.

Eligible non-licensed occupations are occupations for which a vocational training is required (*Ausbildungsberufe*).[35] Although entering these occupations does not formally require recognition, a recognized certificate may carry valuable information on immigrants' skills and experience, reducing employers' information asymmetries and thus improving immigrants' employment opportunities. Moreover, it might increase immigrants' perceived probability of success in the hiring process, therefore shifting their job search efforts towards

[35] Vocational training in Germany can be either in vocational schools or part of a dual system where trainees receive both on-the-job training and school training. Vocational training usually lasts two and a half years.

these occupations. In fact, Figure A.3 illustrates that the total number of applications for the recognition of vocational certificates (to enter eligible non-licensed occupations) steadily increased after 2012. In contrast to *eligible non-licensed* occupations, *non-eligible* occupations are occupations which usually require a higher education degree but are not subject to licensing (e.g., biologist, computer scientist or linguist, see also section 2).

We estimate Equation 2 using as outcomes employment in the four occupation types: a) licensed occupations, b) eligible non-licensed occupations, c) non-eligible occupations, and d) occupations in types a, b, or, c (overall employment). Table A.18 reports the results for the different outcomes, including in Column 1 the baseline outcome licensed occupations. We find that in the post-reform period employment in eligible non-licensed occupations increased by 13.5 percent (Column 2), while employment in *non-eligible* occupations only slighly declined by 2.4 percent (Column 3) relative to the pre-reform non-EU average. In Column 4, we then show the reform effects on overall employment. In the post-reform period overall employment for non-EU immigrants who were living in Germany already before 2012 increased by 6.8 percent (relative to a baseline employment rate of 73.2 percent). This increase in overall employment seems to be fully explained by an increase in employment in licensed or eligible non-licensed occupations.

5.3 Wage Assimilation

The previous sections presented strong evidence that the reform increased the employment of non-EU immigrants in licensed and unlicensed occupations, while leaving employment outcomes unchanged for EU immigrants. In this section, we ask whether the reform affected non-EU immigrant wages in licensed occupations, as compared to EU immigrants working in the same occupations. Addressing this question is important as a widening wage gap in licensed occupations may reflect a reduction in non-EU immigrants' quality following the easier access to occupational recognition.[36]

[36] Here and in the rest of the paper we use the term *quality* to define immigrants' skill sets which are not directly observable to the employer through immigrants' recognized qualifications. For this definition

The reform may have an ambiguous effect on non-EU immigrant wages. On the one hand, as more non-EU immigrants obtain recognition, their composition in terms of unobserved ability - though meeting the recognition standards - may decline. This negative selection of post-reform applicants may have occurred if more able non-EU immigrants applied before the reform when their higher expected returns to recognition compensated them for the high application costs. Furthermore, non-EU wages may decline even though no compositional change occurred if the increased supply of recognized certificates lowers their relative price, or if employers after the reform are more uncertain about the underlying ability of non-EU immigrants who obtained recognition. On the other hand, non-EU wages may increase if the reform reduced employers' uncertainty about the quality of non-EU job applicants (Blair and Chung, 2022), and strengthened the bargaining power of non-EU immigrants entering licensed occupations (e.g., through an increase in outside options).

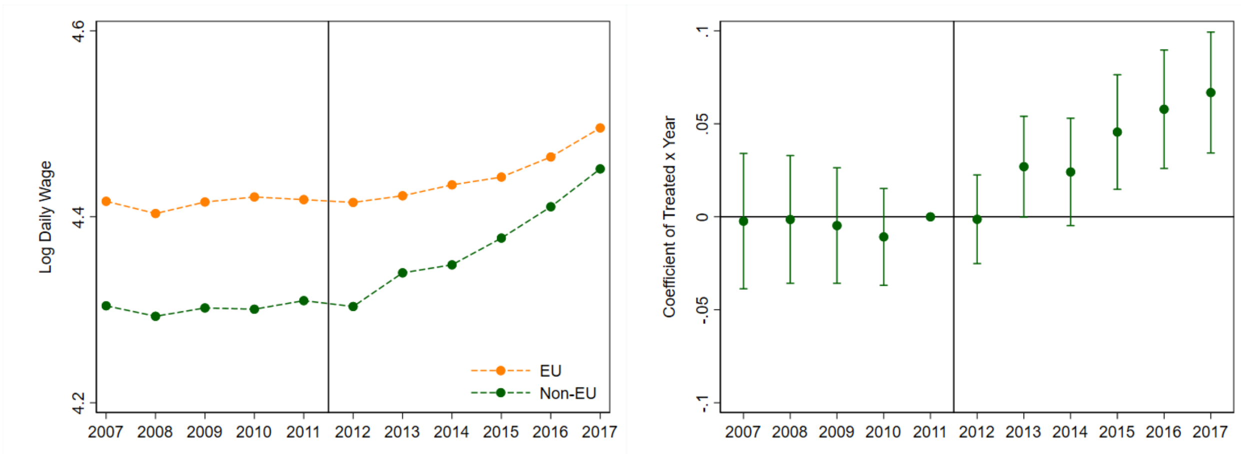
To answer this question, we use the baseline sample of non-EU and EU immigrants arrived before the reform and restrict this to workers in licensed occupations in the period 2007-2017. We then estimate Equations 1 and 2 using log daily wages as dependent variable. To control for differential sorting across occupations following the reform, we include occupation (3-digit) and industry (3-digit) fixed effects. Therefore, the estimated coefficients capture the reform effects on the difference between EU and non-EU immigrants' wages within licensed occupations and industries.

Figure 5 reports levels and event study coefficients from the estimation of Equation 2. We find that in the post-reform period, non-EU daily wages in licensed occupations increase faster relative to EU wages, leading to a convergence between the two. The average post-reform effect is a 4.15 log points increase in non-EU daily wages (Column 1 of Table A.20). In the next paragraphs we explore possible channels behind the converge of non-EU and EU wages in licensed occupations.

we follow the literature on immigrants' wage assimilation (see, for example, Borjas (2015) and Albert et al. (2021)).

Figure 5: Reform effects on log daily wages in licensed occupationsm event study plots

(a) Levels (predicted) (b) Differences



Notes: Figure 5 displays levels (predicted) and event study plots with the corresponding 95 percent confidence intervals for the regression model of Equation 2. The outcome is the log daily wage from any type of employment. Only immigrants arrived before the reform are included. Individual controls include sex, age, age at entry, tenure in the current occupation, educational level. Local labor market, industry and occupation FE are also included.
Source: Integrated Employment Biographies (IEB).

5.3.1 Understanding the Convergence in Non-EU and EU Immigrant Wages

Downward pressure on EU immigrant wages First, we test whether the reform created downward pressure on EU immigrant wages in licensed occupations, slowing down their wage growth relative to non-EU immigrants. As for employment outcomes (see Section 5.2.1), to understand whether a decrease in EU wages explains - at least partially - the reform effects on non-EU wages, we use natives as control group. While similarly unaffected by the reform, natives are less likely to be close substitutes than EU immigrants (Beerli et al., 2021; Albert et al., 2021). As alternative, we also use non-EU immigrants in non-eligible occupations, for which we showed in Section 5 that employment effects are close to zero. Table A.20 Columns 2 and 3 show that results with both alternative control groups are similar to the baseline coefficients in Column 1. These results suggest that the observed converge in EU and non-EU wages is due to an increase in non-EU wages.

Skill accumulation in previous jobs Second, we test whether compositional changes in non-EU immigrants entering licensed occupations can explain the wage increase. For example, while unable to work in licensed occupations, non-EU immigrants may have worked in non-licensed occupations which allowed them to acquire skills (technical/cognitive) highly valuable in licensed occupations. To test for this hypothesis, we consider all EU and non-EU transitions to licensed occupations in the period 2007-2017.[37] We first define the employment status of the spell before making the transition to a licensed occupation (from unemployment or employment). If immigrants were employed, we further characterize the transitions by the type of contract (part-time), the task complexity and the daily wage. We then estimate Equation 1 using transition characteristics as outcomes.

Table A.19 reports the results of this exercise. We find that non-EU immigrants who transition to licensed occupations after the reform were 5 percentage points more likely to be employed in the previous spell. However, we find no differences in the characteristics of this employment spell. They were not earning higher daily wages, nor working more full-time, nor performing more cognitive tasks. Therefore, we do not find evidence of positive self-selection of non-EU immigrants in terms of labor market experience accumulated before entering licensed occupations.

Initial contractual conditions Finally, if the reform strengthened immigrants' bargaining power and lowered employers' uncertainty, better initial contractual conditions may explain the increase in non-EU wages. We, therefore, test whether immigrants are less likely to be hired with marginal employment contracts.[38] Marginal employment contracts - like other types of fixed-term contracts - are cheaper for companies and are often used as screening devices before offering either a full or part-time permanent contract (Faccini, 2014; Booth et al., 2002). We estimate Equation 1 using as outcomes the probability of having a marginal employment contract, as compared to having a regular employment contract. We report results for two groups: immigrants who were working in licensed occupations already in

[37] Given the small number of transitions in the dataset used for the main analysis, we expand the original dataset to include 70% all immigrants working in Germany between 2007-2017.

[38] An employment spell is defined as marginal if recorded by employers as such (*geringfuegig*) or if the daily wage is below the marginal employment wage cap. The cap is time-varying and in our time-period it ranges between 13 and 14 Euros.

$t-1$ (defined as *incumbents*), and immigrants who were either unemployed or working in a non-licensed occupation in $t-1$ (defined as *new entrants*). Columns 1 and 2 of Table A.21 demonstrate that in the post-reform period the probability for non-EU immigrants to have marginal employment contracts is reduced. These effects are concentrated on new entrants (Column 2), and are robust to the use of alternative control groups (see Columns 3 and 4).

Overall, our results suggest that following the recognition reform, wages of EU and non-EU immigrants working in licensed occupations converged. This convergence can be explained by an improvement in initial contractual conditions for non-EU immigrants, while it does not seem to be related to either compositional changes in non-EU entering licensed occupations, or to a decline in average wages of EU immigrants working in these occupations.

6 Effects on Natives' Employment and Wages

Throughout the paper we showed that the introduction of a standardized occupational recognition framework improved markedly the labor market outcomes of non-EU immigrants, unlocking new employment opportunities in high-paying jobs. In this section we complement our analysis and investigate whether the supply shock of non-EU immigrants in licensed occupations hurt natives' labor market outcomes. A negative effect on natives' employment or wages would represent an indirect cost of the reform, and highlight a trade-off between integration policies and natives' welfare.

While some studies found that similar high-skilled migration shocks affect negatively natives' outcomes (Doran et al., 2022), recent research has shown that these effects may be absent or even positive (Beerli et al., 2021; Brinatti et al., 2023) if immigrants are imperfect substitutes for natives and if the latter reallocate across firms to reduce competition with immigrants (Brinatti and Morales, 2021). In our context, skill shortages in many licensed occupations (e.g., nurses and doctors) may have additionally sheltered natives from the increased competition (Signorelli, 2024).

To estimate the effect of the reform-induced supply shock, we use 1 percent of native workers in the social security records between 2007-2017 and follow an empirical strategy similar to Elias et al. (2024).[39] We exploit the variation at the local labor market level in the share of non-EU working in licensed occupations before (2007-2011) and after (2012-2017) the reform. For each of the 250 local labor markets in Germany, we compute the difference in the share of non-EU working in licensed occupations as

$$S_l = \Delta \frac{nonEU_{lt}}{All_{lt}} \tag{3}$$

where $nonEU_{lt}$ and All_{lt} are respectively the total of non-EU immigrants and all workers in licensed occupations in the local labor market l and in the period t (pre- or post-reform).[40] Δ indicates the difference between the post and pre-period. Taking the distribution of S_l, we create a treatment dummy $HighSupply_l$ which assigns a value of 1 to all natives in local labor markets with a value of S_l equal or higher than the median, and 0 otherwise.[41]

We then estimate the following equation

$$Y_{ilt} = \beta Post_t * HighSupply_l + \gamma HighSupply_l + \eta Post_t + \lambda' X_{ilt} + \psi'_{ilt} + \epsilon_{ilt} \tag{4}$$

where Y_{ilt} are native labor market outcomes (employment or log daily wages) overall or in licensed occupations and $Post_t$ is the time dummy, $HighSupply_l$ is an indicator for whether the labor market is above or below the median of S_l, X_{igt} is a vector of individual characteristics and ψ_{igt} are group fixed effects as in Equation 1. The parameter β identifies the reform effect of working (residing, for unemployed) in a local labor market highly exposed to the non-EU supply shock.

A crucial concern with this approach is that the size of the supply shock may be correlated

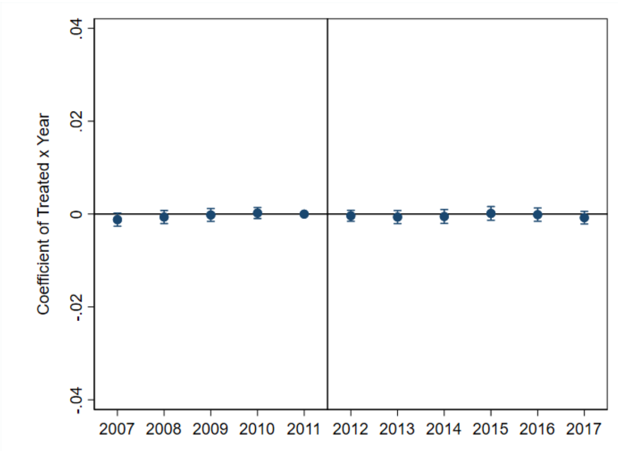
[39] To keep the group as comparable as possible to the non-EU immigrants, we further restrict the sample to individuals aged 23-55, with a vocational degree or tertiary education. We use the same sample as alternative control group in the previous sections.

[40] To construct the measure we do not restrict the sample to individuals arrived before the reform.

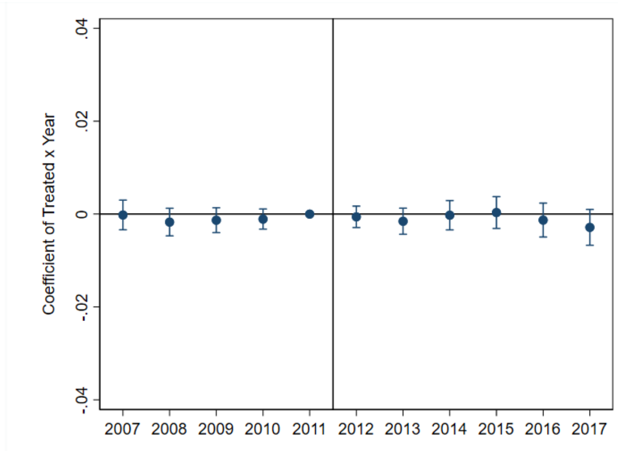
[41] S_l are time- constant within local labor markets.

Figure 6: Local non-EU labor supply and natives' employment and wages, event study plots

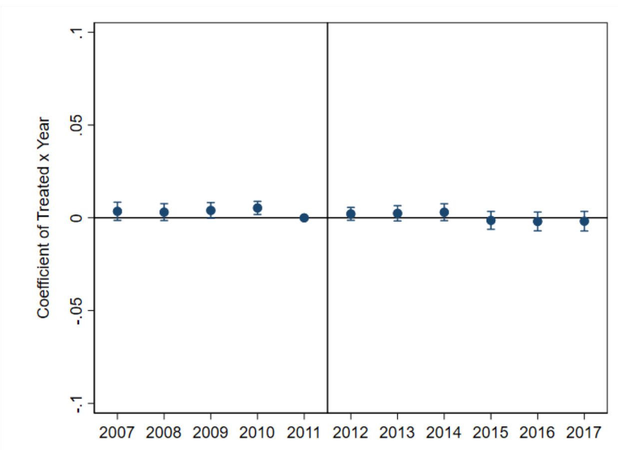
(a) Employment



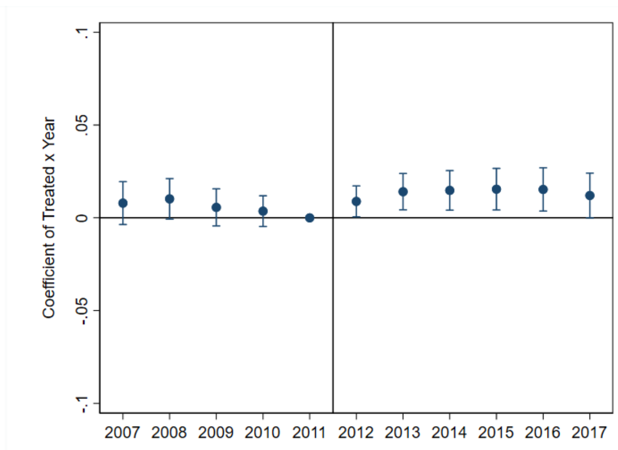
(b) Employment (licensed)



(c) Log Daily Wage



(d) Log Daily Wage (licensed)



Notes: Figure 6 displays the event study plots for natives' employment and wage outcomes overall (a and c) and in licensed occupations (b and d). In models with wage outcomes only employed workers are included. The treated group are natives in local labor markets with a large increase between post- and pre-reform period (above median) of non-EU workers in licensed occupations. The control group is natives in local labor markets with a small increase (below median). In all regression models we include individual controls, year and local labor market fixed effects. For models with wage outcomes, we additionally include tenure in the current occupation, industry and occupation fixed effects. Bars indicate 95 percent confidence intervals. Source: Integrated Employment Biographies.

with both the pre-reform skill shortage in licensed occupations and the labor supply increase related to the Eastern European Enlargement (Germany lifted migration restrictions in 2011). To address this concern, we correlate the continuous supply shock measure with these local labor market characteristics. Figures A.11 and A.12 show that local pre-reform skill shortages and changes in Eastern Europeans are not correlated with the supply shock measure.

Figure 6 depicts the event study plots from dynamic DiD regressions where, instead of the $Post_t$ dummy in Equation 3, we include interactions between year fixed effects and the $HighSupply_l$ dummy.[42] Visually, we find no evidence of differential pre-trends between individuals in local labor markets with a high and low supply shock. Only for wages in licensed occupations (Panel d) post-reform coefficients are positive, though the size of the difference remains small throughout the period.

The estimated coefficients from Equation 3 confirm these results. Table A.22 (Columns 1 and 2) report the effects of the supply shock on employment outcomes. We find a significant decrease of 0.0041 percentage points - 0.45 percent relative to the baseline - in overall employment and a marginally significant 0.0022 percentage points reduction - 1.4 percent - in employment in licensed occupations. Wage outcomes, reported in Columns 3 and 4, show that natives in high-supply local labor markets had a small negative effect on overall wages (-0.0045), and a small positive effect (0.0064) - though not significant - if employed in licensed occupations.

As a robustness check, we compute also an alternative measure of supply shock which uses as denominator all individuals in the register within each local labor market. We show results - which are close to the baseline estimation - in Columns 1-4 of Table A.23. Additionally, we estimate similar wage effects with alternative strategies comparing natives employed in licensed occupations and non-eligible non-licensed occupations (a similar approach to Signorelli (2024)), and natives in licensed occupations with a high or low non-EU inflow (see Columns 5 and 6 of Table A.23).[43]

The findings that the recognition reform did not affect native labor market outcomes align to previous studies showing negligible effects of high-skilled immigration on natives' employment (Brinatti et al., 2023; Signorelli, 2024), and no negative effects on natives' wages (Beerli et al., 2021). As in Beerli et al. (2021), the estimated effects on wages in licensed

[42] The coefficients come from regression models in which we control for individual characteristics and include local labor market fixed effects, as well as occupation and industry fixed effects for the wage regressions.

[43] As in Section 5.2 licensed occupations with a high inflow are occupations that received 93 percent of all applications for recognition.

occupations may be related to the existence of skill shortages and skill complementarities between natives and immigrants. Moreover, specific to the German context, the presence of strong unions and labor market regulations may lower wage flexibility and shelter natives Glitz (2012). Finally, our estimates may result from reallocation dynamics across firms (Brinatti and Morales, 2021) which take place, but we do not observe at our level of analysis.

7 Discussion and Conclusion

Immigrants perform worse than natives in the labor market, likely because of the low transferability of home-country professional qualifications. Standardizing the recognition of professional certificates in the host country is a key policy that can enhance skill transferability. This paper investigated the effects of a nation-wide recognition reform in Germany on immigrants' labor market outcomes. For non-EU immigrants - the treated group - the reform increased both occupational recognitions and their employment in licensed occupations, all of which require recognition. Additionally, the reform also increased employment in non-licensed occupations and reduced overall unemployment. Furthermore, despite the larger inflow of non-EU immigrants into these occupations, the average wages for non-EU immigrants did not decrease but even increased post-reform. Finally, we find no negative effects on natives employment or wages in licensed occupations. All results are stable up to five years post-reform.

Our results are highly valuable for policy makers worldwide, as many countries are discussing ways to improve skill transferability and immigrants' labor market integration, as well as to cope with skill shortages. Our results demonstrate that such policies are effective. Moreover, we address two prevailing concerns associated with integration policies for high-skilled immigrants. First, the quality in licensed occupations, for example the quality of health services, may decrease if more and possibly lower skilled immigrants obtain access to recognition. Second, if the reform induces a supply shock in licensed occupations, the increased competition may crowd out natives working in these occupations and exert downward pressure on their wages.

Regarding the first concern, our findings show that an increase in the number of recognized certificates does not necessarily lead to lower quality. If the quality of recognized certification had declined post-reform, we would expect employers to have observed this decline and have adjusted downward their labor demand for non-EU immigrants. On the contrary, employment effects increase with time and non-EU wages in licensed occupations converge to the levels of EU immigrants and natives. Regarding the second concern, we find no evidence that an inflow of immigrants in licensed occupations has harmed the labor market prospects of highly skilled native workers. This finding holds independently from pre-reform local skill shortages, indicating that other factors - such as collective agreements and reallocation dynamics - shield natives from the increased competition.

Taken together, our results point to the importance of removing formal barriers to the transferability of foreign-acquired human capital. Improving recognition procedures in terms of both the administrative burden and access to information may be a cost-efficient policy to integrate high-skilled immigrants into their host country's labor market, providing an alternative to other policies that foster immigrants' retraining and upskilling upon arrival.

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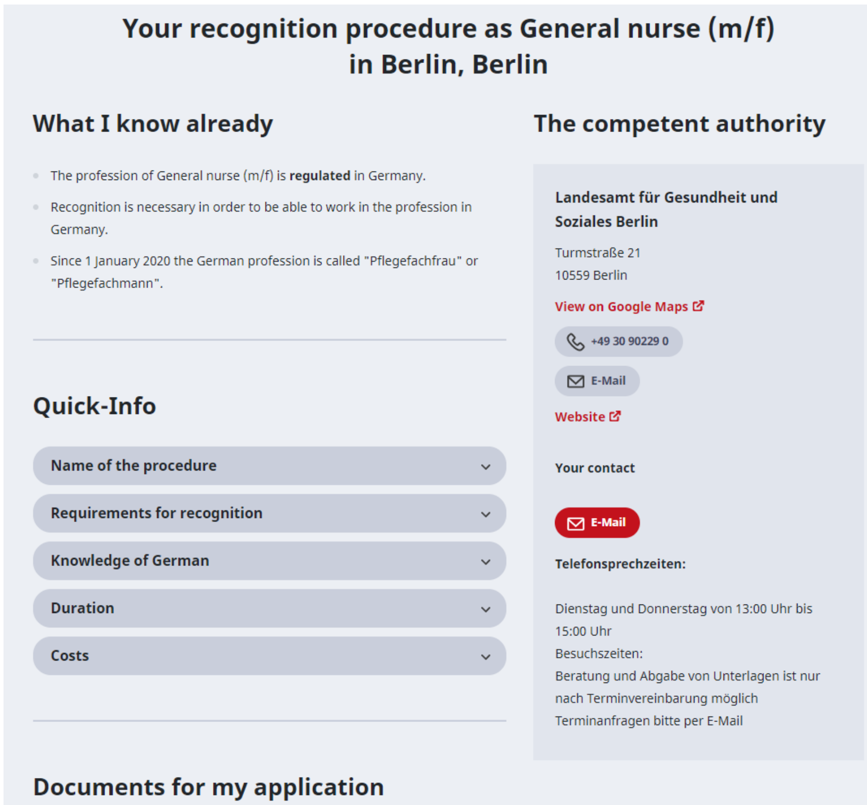
A Additional Figures and Tables

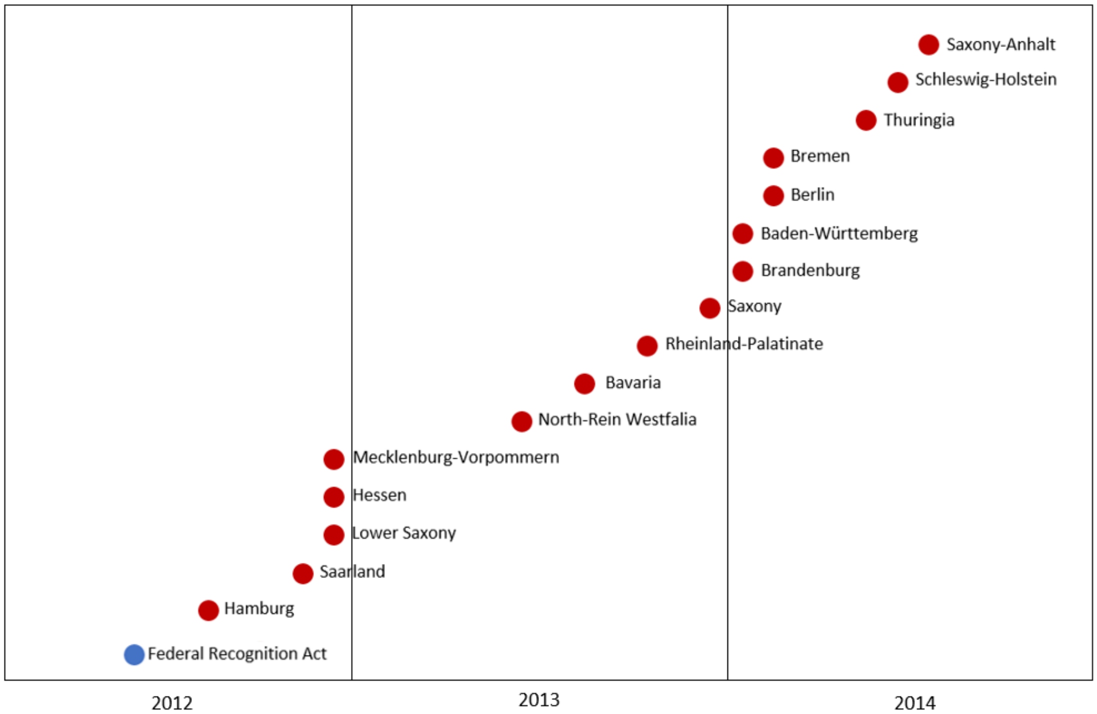
A.1 Figures

Figure A.1: Example of the available information on the website "Recognition in Germany"



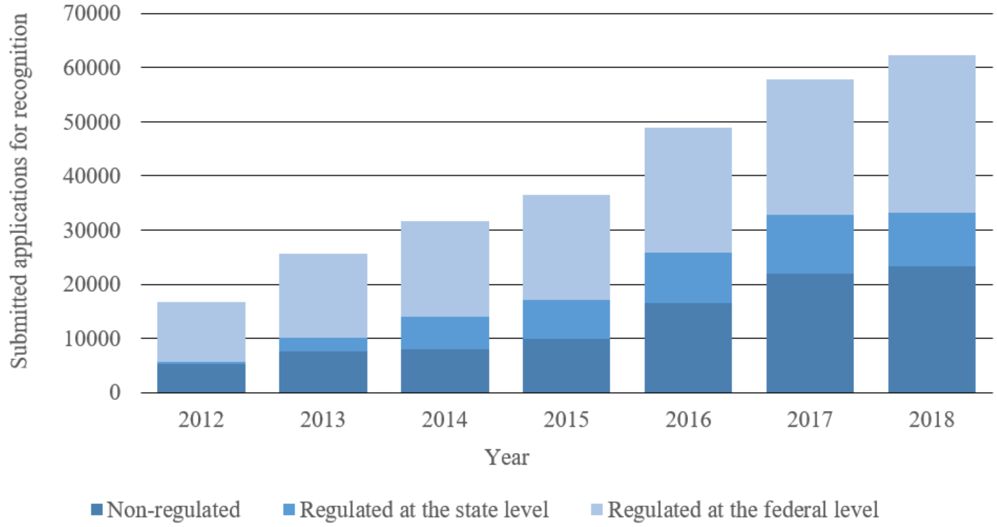
Notes: Figure A.1 is a screenshot of the webpage www.anerkennung-in-deutschland.de that results from the search of nursing jobs in Berlin. The webpage provides information on the type of certificate required and on the recognition procedure to follow. Source: website www.anerkennung-in-deutschland.de

Figure A.2: Timing of the introduction of recognition laws across federal states



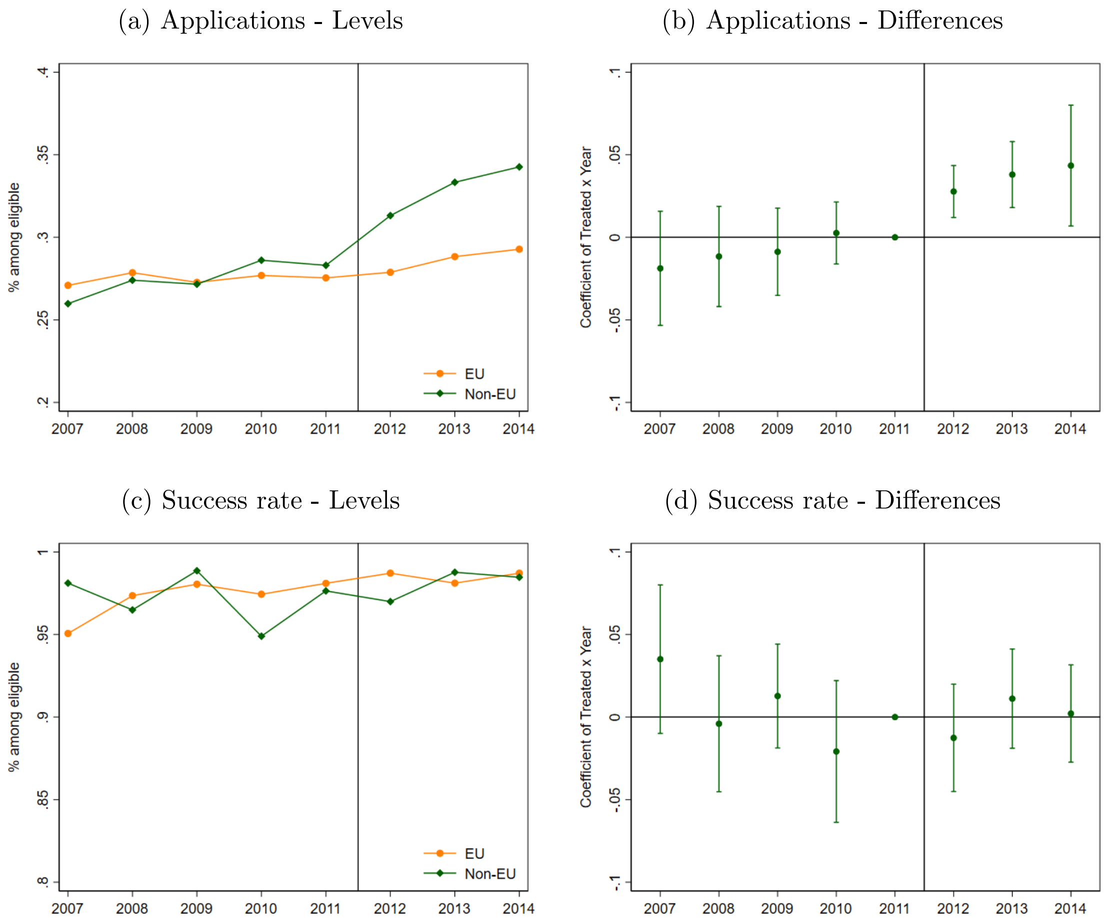
Notes: Figure A.2 displays the timing of state recognition laws from 2012 to 2014. The blue dot is the Federal Recognition Act (nation-wide recognition law). Source: Own graphical representation using data of the German Ministry of Education and Training (BIBB, 2015).

Figure A.3: Total number of applications by type of occupation for which recognition is requested



Notes: The figure shows the total number of applications by year and type of occupation for which recognition is requested. Recognition procedures are regulated at the federal or state level, or non-regulated (in case no licensing is needed). Non-licensed jobs for which recognition is possible include all vocational occupations (*Ausbildungsberufe*). Data on applications and recognition outcomes is not available before 2012.
Source: Ministry of Education and Training (BIBB), Official statistics on the Federal Recognition Act.

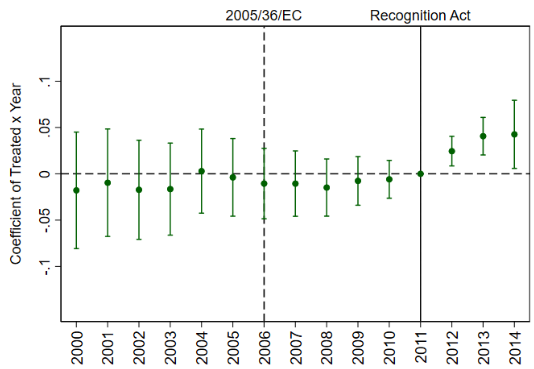
Figure A.4: Event study plots for the reform effects on applications, recognitions and success rates, Immigrants Arrived Before 2012



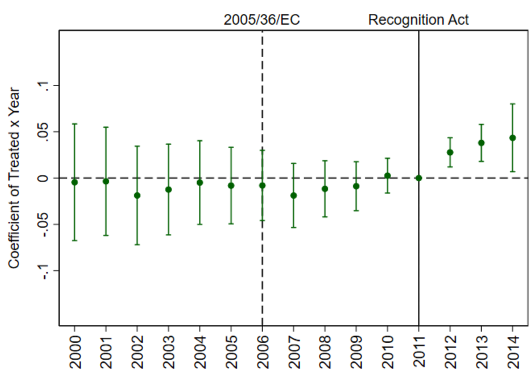
Notes: The figure displays the event study plots for the probability of being granted recognition, conditional on having applied (i.e., success rate). The baseline coefficient is the interaction between year 2011 and the dummy identifying non-EU immigrants. The vertical line indicates the year before the Recognition Act in 2012. The group of EU immigrants includes also ethnic Germans. These are immigrants with German origins that benefit from recognition procedures similar to EU immigrants. In Panel a) the outcome for the EU immigrants are adjusted by its pre-reform mean difference (computed across 2007-2011) relative to the non-EU immigrants.
Source: IAB-SOEP Migration Sample, waves 2013, 2014, 2015, 2016.

Figure A.5: Event study coefficients for the reform effects on applications, recognitions and success rates, immigrants arrived Before 2012, long pre-trends

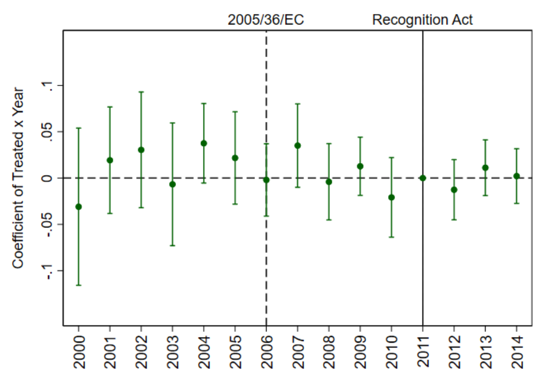
(a) Recognitions



(b) Applications



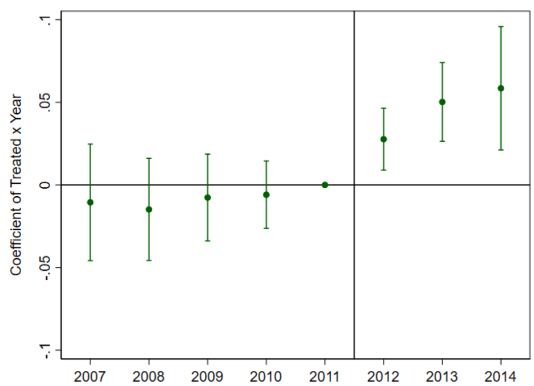
(c) Success rate



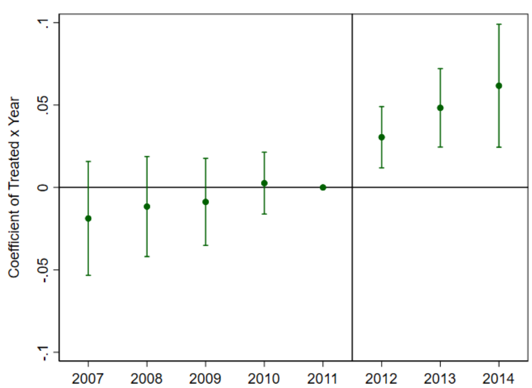
Notes: Figure A.5 displays the event study plot for the probability of obtaining recognition (panel a); the probability of applying for recognition (panel b); the probability of obtaining recognition conditional on having applied (i.e., success rate)(panel c). The baseline coefficient is the interaction between year 2011 and the dummy identifying non-EU immigrants. The dashed vertical line indicates the year before the implementation of the EU Directive 2005/36/EC, the solid line indicates the year before the Recognition Act adopted in 2012. Only immigrants arrived before the reform are included. The group of EU immigrants includes also ethnic Germans. These are immigrants with German origins that benefit from recognition procedures similar to EU immigrants. Bars indicate 95% confidence intervals.
Source: IAB-SOEP Migration Sample, waves 2013, 2014, 2015, 2016.

Figure A.6: Event study plots for the reform effects on applications, recognitions and success rates, all immigrants

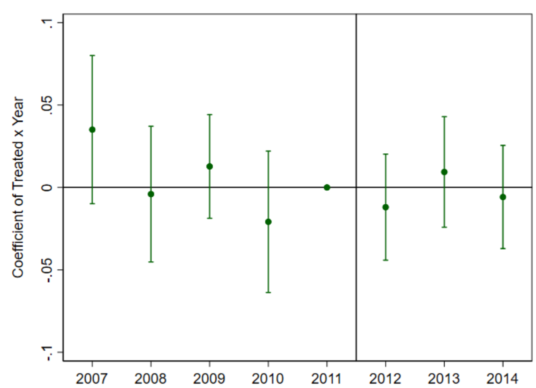
(a) Recognitions



(b) Applications

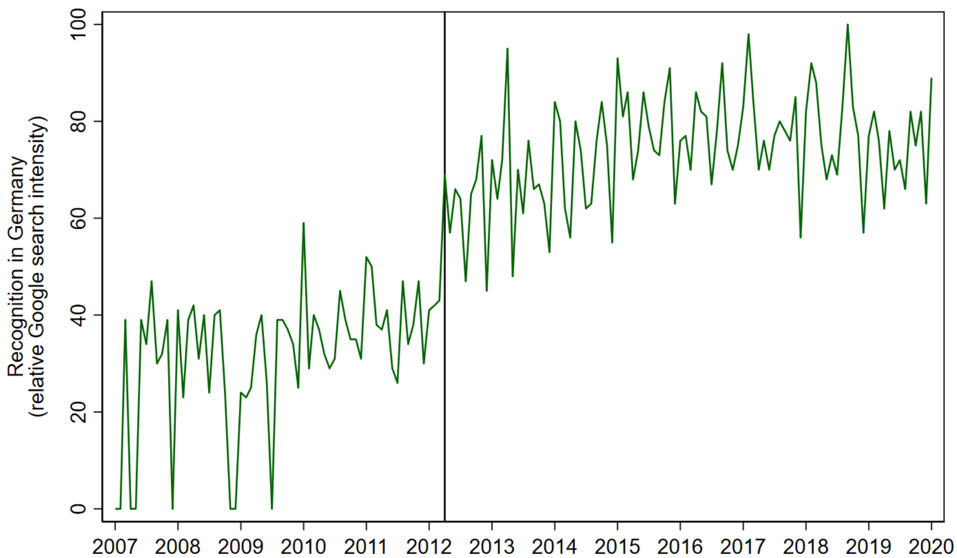


(c) Success rate



Notes: Figure A.6 displays the event study plots for the probability of receiving recognition (panel a); the probability of applying for recognition (panel b); the probability of being granted recognition, conditional on having applied (panel (i.e., success rate) c). All immigrants, both those arrived before (incumbents) and those arrived after the reform (new arrivals) are included. The baseline coefficient is the interaction between year 2011 and the dummy identifying non-EU immigrants. The vertical line indicates the year before the Recognition Act in 2012. The group of EU immigrants includes also ethnic Germans. These are immigrants with German origins that benefit from recognition procedures similar to EU immigrants. Bars indicate 95% confidence intervals.
Source: IAB-SOEP Migration Sample, waves 2013, 2014, 2015, 2016.

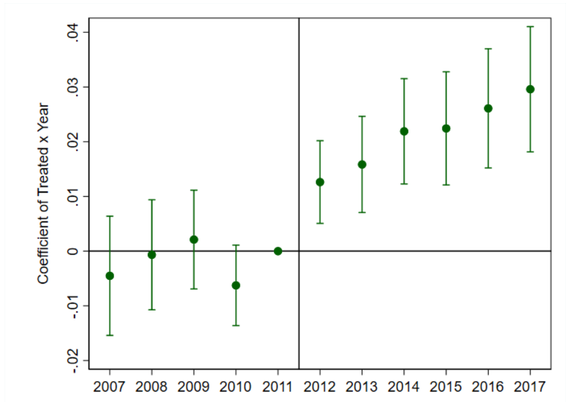
Figure A.7: Google Search intenstiy for the term "Recognition in Germany" (*Anerkennung in Deutschland*)



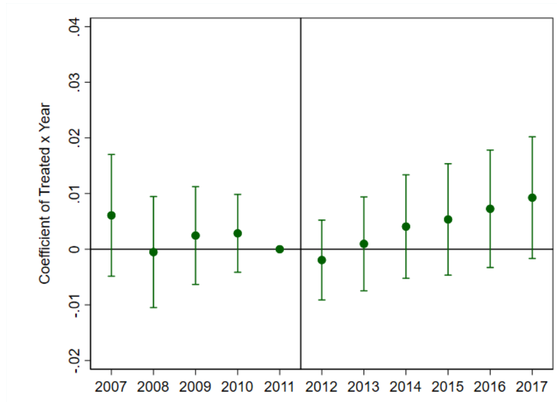
Notes: Figure A.7 displays the amount of google searches (search intensity) for the web search "Recognition in Germany" (*Anerkennung in Deutschland*) between 2007 and 2020. Data are restricted to web searches made in Germany. The monthly number of searches is normalized to 100, where a value of 100 is the peak popularity for the searched term. A value of 50 means that the term is half as popular. The black vertical line indicates the day in which the Federal Recognition Act came into force (April 1st 2012).
Source: Google Trends (searched on 02.01.2021).

Figure A.8: Event study plots for alternative definitions of the regulation index as outcome

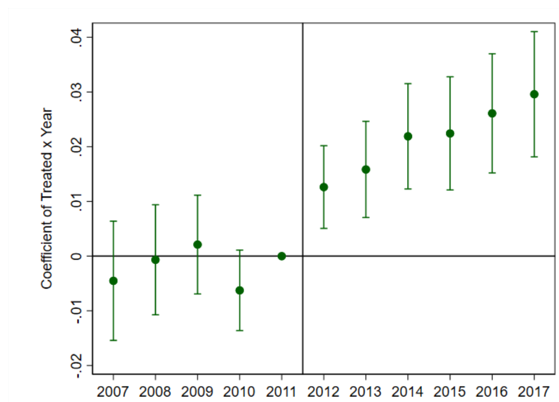
(a) Regulation index (continuous)



(b) Regulation index (continuous, without 0s)



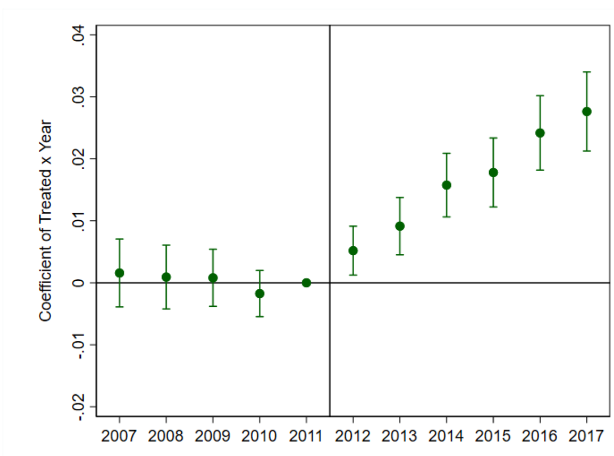
(c) Regulation index (binary)



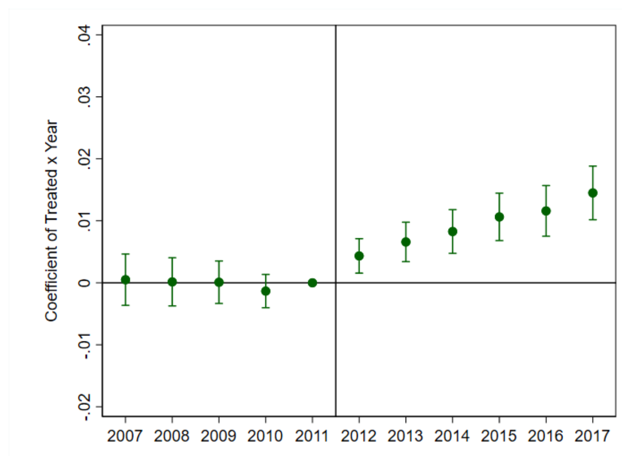
Notes: Figure A.8 displays the event study plot from Equation 2, where the outcomes are the continuous regulation index measure (panel a), the continuous regulation index without zeros (panel b), a dummy variable that takes value 1 if the regulation index is higher than 0, and 0 otherwise (panel c). Coefficients are estimated for each quarter pre- and post reform. The baseline coefficient is the interaction between year 2011 and the dummy identifying non-EU immigrants. Bars indicate 95% confidence intervals.
Source: Integrated Employment Biographies (IEB).

Figure A.9: Event study plots for different groups of eligible licensed occupations

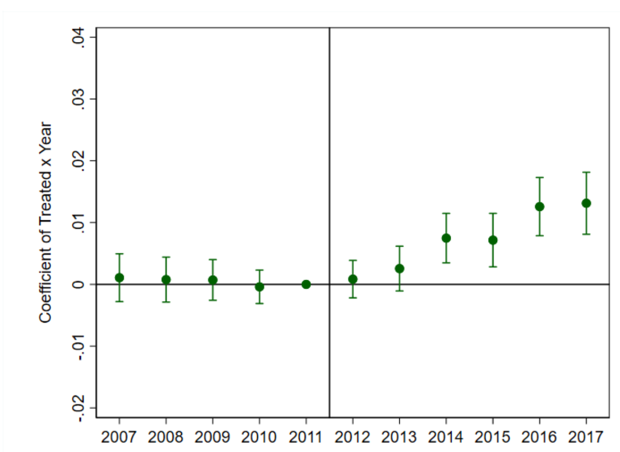
(a) Top 15 by number of applications



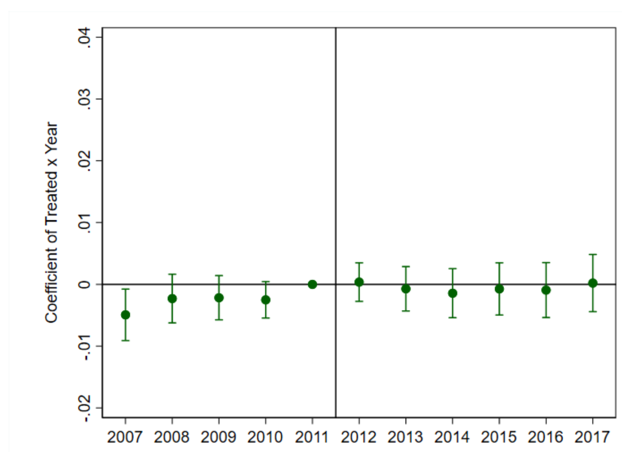
(b) Top 15 by number of applications, federally licensed



(c) Top 15 by number of applications, regionally licensed



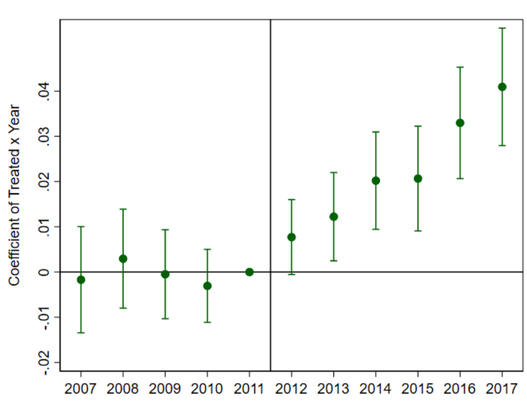
(d) Other eligible occupations



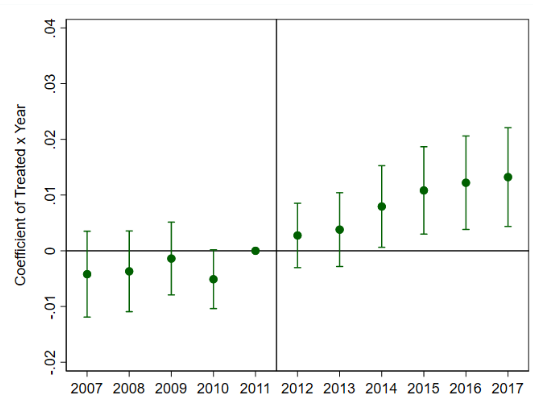
Notes: Figure A.9 displays the event study plot from Equation 2, using subgroups of licensed occupations to define outcomes. The subgroup is stated on top of each plot. The outcome variables are the probability of being employed in one of the 15 occupations (TOP 15) that received the highest number of applications for recognition (panel a), in a TOP 15 occupation licensed at the national level (panel b), in a TOP 15 occupation licensed at the state level (panel c), in a non-TOP 15 licensed occupation. The baseline coefficient is the interaction between year 2011 and the dummy identifying non-EU immigrants. Bars indicate 95% confidence intervals.
Source: Integrated Employment Biographies (IEB).

Figure A.10: Event study plots for heterogeneous effects across individual characteristics

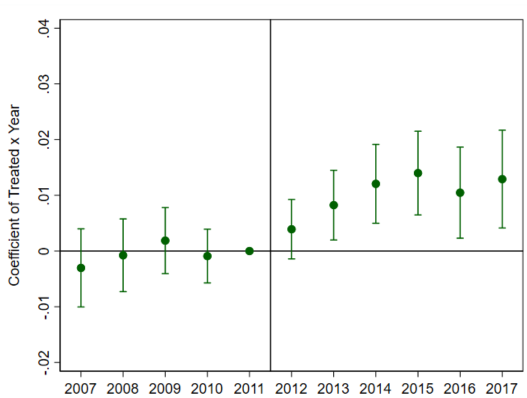
(a) Female



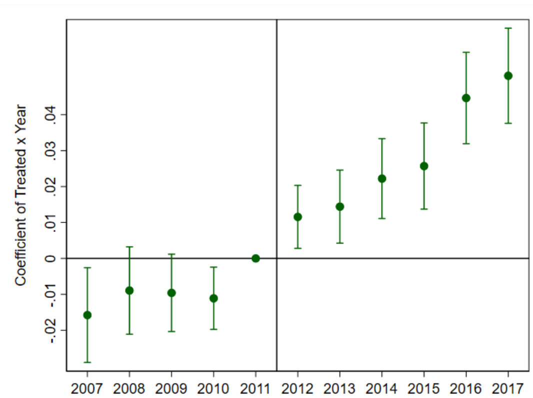
(b) Male



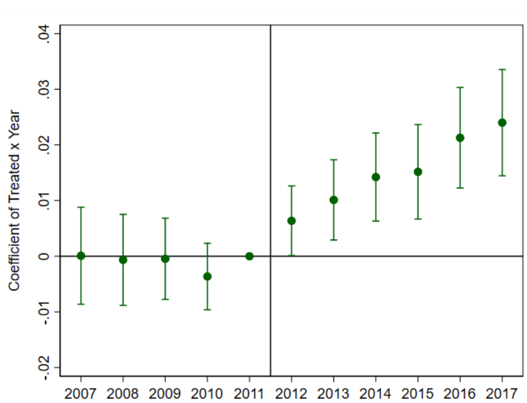
(c) Vocational education



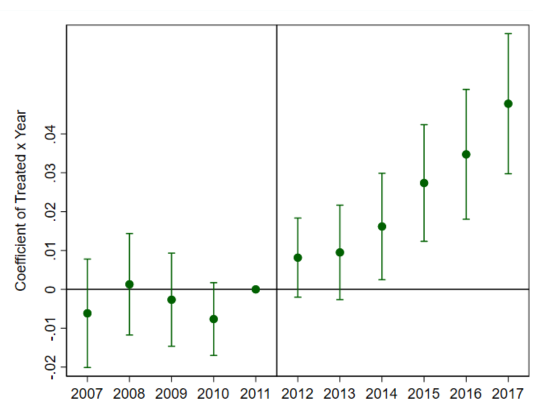
(d) University education



(e) Age at entry 25-34



(f) Age at entry 35-55



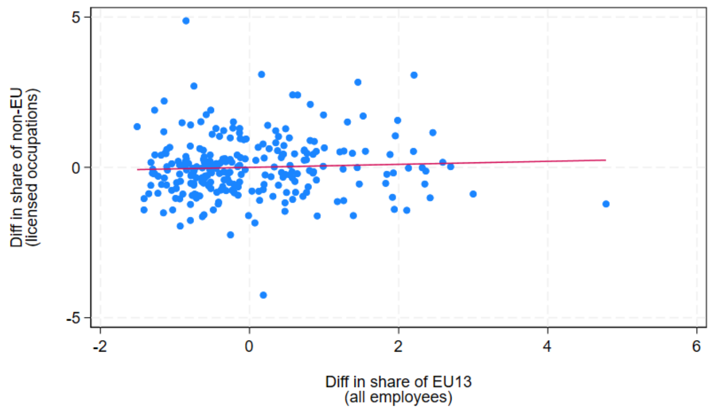
Notes: Figure A.10 displays the event study plot from Equation 2 for subgroups of the main sample, based on individual characteristics. The subgroup is stated on top of each plot. The outcome is the probability of being employed in licensed occupations. The baseline coefficient is the interaction between year 2011 and the dummy identifying non-EU immigrants. Bars indicate 95% confidence intervals.
Source: Integrated Employment Biographies (IEB).

Figure A.11: Correlation between non-EU supply shock measure and pre-reform local labor demand in licensed occupations

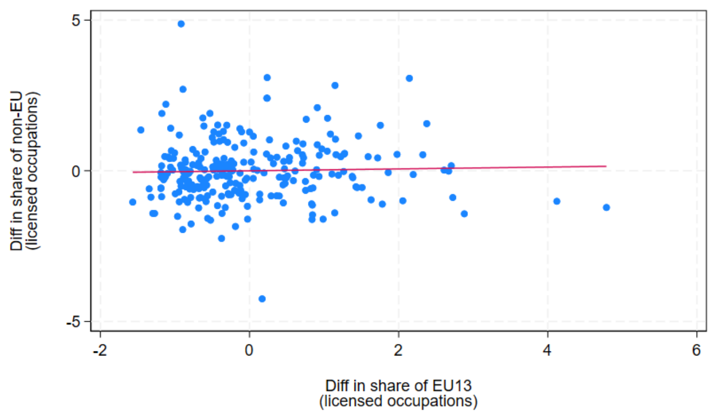
Notes: Figure A.11 displays the correlation (scatter plot and linear fit) between the supply-shock measure (see Equation 3 for the construction) and the average pre-reform (2007-2010) labor demand in licensed occupations. Each blue dot represents a local labor market.
Source: Integrated Employment Biographies (IEB), German Statistical Office (DESTATIS).

Figure A.12: Correlation between non-EU labor supply shock measure and EU13 labor supply

(a) EU13 in any occupation



(b) EU13 in licensed occupations



Notes: Figure A.12 displays the correlation (scatter plot and linear fit) between the supply-shock measure (see Equation 3 for the construction) and the change (pre-post 2012) in EU13 labor supply in any occupation (panel a) or in licensed occupations only (panel b). Each blue dot represents a local labor market.
Source: Integrated Employment Biographies (IEB), German Statistical Office (DESTATIS).

A.2 Tables

Table A.1: What prevents immigrants from applying for occupational recognition?

	(1)	(2)	(3)	(4)
	All immigrants		Arrived pre-reform	
	EU15	Non-EU	EU15	Non-EU
	in %		in %	
Administrative constraints	13.68	23.94	14.57	23.48
No perspective of recognition	14.74	19.69	14.57	20.00
Not important	38.42	32.43	35.76	33.48
Other reasons	33.16	23.94	35.1	23.04
Observations	190	259	151	230

Notes: Table A.1 reports the percentage of immigrants who would have been eligible for recognition but did not apply according to the reasons for no application aggregated in four groups: administrative constraints, no perspective of recognition, not important or other reasons. Responses come from a question included in all waves of the IAB-SOEP Migration Survey on the reasons why immigrants did not apply for recognition of their vocational or university certificate acquired abroad. In the first two columns all EU and non-EU immigrants for which the information is available are included. In the last two columns only EU and non-EU immigrants who entered Germany pre-reform are included.
Source: IAB-SOEP Migration Sample, waves 2013,2014,2015,2016.

Table A.2: Occupations with the largest number of applications, by type of occupation

Licensed occupations			Non-licensed occupations	
Occupation	Level of regulation	in %	Occupation	in %
Nurse	National	23.68	Electronics technician	12.67
Doctor	National	22.97	Office clerk	6.79
Teacher	State	12.07	Caregiver	3.89
Engineer	State	4.58	Trainer in office work	3.73
Social pedagogist	State	4.29	Commercial clerk	3.23
Social worker	State	4.15	Mechatronic technician	2.81
Children pedagogist	National	4.07	Machines mechanic	2.57
Physiotherapist	National	3.02	Office electrician	2.24
Pharmacist	National	3.02	Industrial electrician	1.82
Educator	State	2.57	IT-specialist	1.74
Architect	State	2.35	Sales clerk	1.66
Dentist	National	2.09	Metal technician	1.57
Children nurse	National	1.36	Cook	1.49
Ostetric	National	1.50	Heating technician	1.32
Nurse assistant	State	1.30	Hairdresser	1.24
Total		93.02		48.76

Notes: Table A.2 reports the licensed and non-licensed occupations that received the largest number of applications for occupational recognition after 2012. To identify these occupations, we collected data from the state statistical offices and selected the 15 occupations with the largest number of applications in 12 out of 16 federal states (data is incomplete for Hamburg, Saarland, Schleswig-Holstein, and Bavaria), distinguishing between licensed and non-licensed occupations. For licensed occupations we report whether the regulation is at the federal or state level. For all occupations we report applications as percentage of total applications. The percentages are computed based on the percentages for the state *Hessen* for which we obtained the number of applications separately by occupations (5-digit Kldb2010 classification). Since not all occupations have applications in all years from 2012 to 2018, we took the largest application number across all years for each occupation from the *Hessen* list and computed the total accordingly. Alternative calculations (e.g., the sum of all applications across all years) do not change the order.
Source: Regional Statistical Offices.

Table A.3: Reform effects on applications, recognitions and success rates, immigrants arrived in Germany before 2012

	(1)	(2)	(3)	(4)
	Panel A: Recognitions			
Post*Non-EU	0.0427***	0.0463***	0.0430***	0.0381***
	(0.0142)	(0.0135)	(0.0135)	(0.0137)
Baseline	0.2672	0.2672	0.2672	0.2672
R-Squared	0.0026	0.0657	0.0817	0.0845
	Panel B: Applications			
Post*Non-EU	0.0426***	0.0463***	0.0431***	0.0388***
	(0.0139)	(0.0132)	(0.0132)	(0.0134)
Baseline	0.2738	0.2738	0.2738	0.2738
R-Squared	0.0022	0.0645	0.08	0.0822
Observations	9,263	9,263	9,263	9,263
Individuals	1,308	1,308	1,308	1,308
	Panel C: Success rate			
Post*Non-EU	-0.0033	0.0017	0.0022	0.0064
	-0.0091	-0.0095	-0.0095	-0.0105
Baseline	0.9701	0.9701	0.9701	0.9701
R-Squared	0.0045	0.0317	0.0421	0.0863
Observations	2,793	2,793	2,793	2,793
Individuals	441	441	441	441
Individual controls	No	Yes	Yes	Yes
Year FE	No	No	Yes	Yes
LLM FE	No	No	Yes	Yes
LLM-Year FE	No	No	No	Yes
Education-Year FE	No	No	No	Yes

Notes: Table A.3 reports coefficients from Equation 1 using as outcome the probability of applying for recognition (Panel A), the probability of obtaining recognition (Panel B), the probability of obtaining recognition conditional on having applied (Panel C). Individual controls include sex, age, age at arrival in Germany, years in Germany, educational level and macroregion of origin. Year and state (*Land*) fixed effects are included. Only immigrants arrived before the reform (before 2012) are included. Ethnic Germans are included in the control group. Standard errors clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: IAB-SOEP Migration Sample, waves 2013,2014,2015,2016.

Table A.4: Event study coefficients for the reform effects on applications, recognitions and success rates, immigrants arrived in Germany before 2012

	(1)	(2)	(3)
	Recognitions	Applications	Success rate
Non-EU*t = -5	0.0004	-0.0113	0.0323
	(0.0174)	(0.0169)	(0.0247)
Non-EU*t = -4	-0.0132	-0.014	0.0146
	(0.0149)	(0.0147)	(0.0247)
Non-EU*t = -3	-0.0102	-0.0141	0.0199
	(0.0128)	(0.0129)	(0.0194)
Non-EU*t = -2	-0.0079	-0.003	-0.014
	(0.0096)	(0.0086)	(0.0239)
Non-EU*t = 0	0.0228***	0.0249***	-0.0002
	(0.0078)	(0.0078)	(0.0179)
Non-EU*t = 1	0.0362***	0.0316***	0.0246
	(0.0101)	(0.009)	(0.0167)
Non-EU*t = 2	0.0384**	0.0369**	0.0242
	(0.0185)	(0.0184)	(0.0161)
Baseline	0.2672	0.2738	0.9701
R-Squared	0.0789	0.0763	0.0888
Observations	9,263	9,263	2,793
Individuals	1,308	1,308	441
Individual controls	Yes	Yes	Yes
LLM-Year FE	Yes	Yes	Yes
Education-Year FE	Yes	Yes	Yes

Notes: Table A.4 reports coefficients from Equation 2 using as outcome the probability of applying for recognition (Column 1), the probability of obtaining recognition (Column 2), the probability of obtaining recognition conditional on having applied (Column 3). Individual controls include sex, age, age at arrival in Germany, years in Germany, educational level and macroregion of origin. Year and state (*Land*) fixed effects are included. Only immigrants arrived before the reform (before 2012) are included. Ethnic Germans are included in the control group. Standard errors clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: IAB-SOEP Migration Sample, waves 2013,2014,2015,2016.

Table A.5: Reform effects on applications, recognitions and success rates, all immigrants

	(1)	(2)	(3)	(4)
	Panel A: Recognitions			
Post*Non-EU	0.0524***	0.0453***	0.0401***	0.0363**
	(0.0148)	(0.0139)	(0.0139)	(0.0142)
Baseline	0.2672	0.2672	0.2672	0.2672
R-Squared	0.0011	0.0695	0.0853	0.0878
	Panel B: Applications			
Post*Non-EU	0.0529***	0.0465***	0.0413***	0.0379***
	(0.0146)	(0.0138)	(0.0138)	(0.0141)
Baseline	0.2738	0.2738	0.2738	0.2738
R-Squared	0.001	0.0669	0.0825	0.0845
Observations	9,788	9,788	9,788	9,788
Individuals	1,521	1,521	1,521	1,521
	Panel C: Success rate			
Post*Non-EU	-0.006	-0.0015	-0.001	0.0026
	(0.0096)	(0.0097)	(0.0098)	(0.0108)
Baseline	0.9701	0.9701	0.9701	0.9701
R-Squared	0.0045	0.0317	0.0421	0.0863
Observations	2,844	2,844	2,844	2,844
Individuals	469	469	469	469
Individual controls	No	Yes	Yes	Yes
Year FE	No	No	Yes	Yes
LLM FE	No	No	Yes	Yes
LLM-Year FE	No	No	No	Yes
Education-Year FE	No	No	No	Yes

Notes: Table A.5 reports coefficients from Equation 1 using as outcome the probability of applying for recognition (Panel A), the probability of obtaining recognition (Panel B), the probability of obtaining recognition conditional on having applied (Panel C). Individual controls include sex, age, age at arrival in Germany, years in Germany, educational level and macroregion of origin. Year and state (*Land*) fixed effects are included. All immigrants, arrived both before and after the reform, are included. Ethnic Germans are included in the control group. Standard errors clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: IAB-SOEP Migration Sample, waves 2013,2014,2015,2016.

Table A.6: Event study coefficients for the reform effects on applications, recognitions and success rates, all immigrants

	(1)	(2)	(3)
	Recognitions	Applications	Success rate
Non-EU*t = -5	-0.0117	-0.0001	0.032
	(0.0169)	(0.0174)	(0.0247)
Non-EU*t = -4	-0.0143	-0.0136	0.0144
	(0.0147)	(0.0149)	(0.0247)
Non-EU*t = -3	-0.0143	-0.0105	0.0198
	(0.0129)	(0.0128)	(0.0194)
Non-EU*t = -2	-0.0031	-0.0081	-0.0142
	(0.0086)	(0.0095)	(0.0239)
Non-EU*t = 0	0.0234***	0.0217**	-0.0006
	(0.0086)	(0.0086)	(0.0179)
Non-EU*t = 1	0.0283**	0.0322***	0.0232
	(0.0109)	(0.0109)	(0.0178)
Non-EU*t = 2	0.0385**	0.0369*	0.0118
	(0.0188)	(0.0188)	(0.0182)
Baseline	0.2738	0.2672	0.9701
R-Squared	0.0845	0.0878	0.0863
Observations	9,788	9,788	2,844
Individuals	1,521	1,521	489
Individual controls	Yes	Yes	Yes
LLM-Year FE	Yes	Yes	Yes
Education-Year FE	Yes	Yes	Yes

Notes: Table A.6 reports coefficients from Equation 2 using as outcome the probability of applying for recognition (Column 1), the probability of obtaining recognition (Column 2), the probability of obtaining recognition conditional on having applied (Column 3). Individual controls include sex, age, age at arrival in Germany, years in Germany, educational level and macroregion of origin. Year and state (*Land*) fixed effects are included.Immigrants arrived both before and after the 2012 reform are included. Ethnic Germans are included in the control group. Standard errors clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: IAB-SOEP Migration Sample, waves 2013,2014,2015,2016.

Table A.7: Robustness: Reform effects on applications, recognitions and success rates, alternative sample

	(1)	(2)	(3)
	Recognitions	Applications	Success rate
	Panel A: Arrived before 2012		
Post*Non-EU	0.0402**	0.0403**	0.0047
	(0.0173)	(0.0168)	(0.0124)
Avg Outcome Pre	0.2778	0.2849	0.9677
R-squared	0.083	0.081	0.0975
Observations	7,007	7,007	2,154
Individuals	1,053	1,053	359
	Panel B: All		
Post*Non-EU	0.0428**	0.0432**	0.0005
	(0.0172)	(0.0169)	(0.0128)
Avg Outcome Pre	0.2778	0.2849	0.9677
R-squared	0.0943	0.0915	0.1018
Observations	7,490	7,490	2,199
Individuals	1,250	1,250	382
Individual controls	Yes	Yes	Yes
LLM*Year FE	Yes	Yes	Yes
Education*Year FE	Yes	Yes	Yes

Notes: Table A.7 reports coefficients from Equation 1 using as outcome the probability of applying for recognition (Column 1), the probability of obtaining recognition (Column 2), the probability of obtaining recognition conditional on having applied (Column 3). Sample selection includes immigrants who arrived when they 23 years old or older, and have a age range 23-55. This sample selection resembles the one used in the analysis with social security records. Panel A includes only immigrants, arrived before the reform. Panel B includes all immigrants, arrived both before and after the reform. Individual controls include sex, age, age at arrival in Germany, years in Germany, educational level and macroregion of origin. Year and state (*Land*) fixed effects are included. Standard errors clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: IAB-SOEP Migration Sample, waves 2013,2014,2015,2016.

Table A.8: Event study coefficients for the reform effects on employment, immigrants arrived before 2012

	(1)	(2)	(3)
	All eligible	Top15	Other
Non-EU*t = -5	-0.0028	0.0032	-0.0060***
	(0.0035)	(0.0029)	(0.0023)
Non-EU*t = -4	-0.0011	0.0015	-0.0027
	(0.0033)	(0.0027)	(0.0021)
Non-EU*t = -3	-0.0001	0.0017	-0.0018
	(0.0029)	(0.0024)	(0.0019)
Non-EU*t = -2	-0.003	-0.0009	-0.0021
	(0.0024)	(0.0019)	(0.0016)
Non-EU*t = +0	0.0053**	0.0047**	0.0006
	(0.0026)	(0.0021)	(0.0017)
Non-EU*t = +1	0.0096***	0.0096***	0.0000
	(0.003)	(0.0025)	(0.0019)
Non-EU*t = +2	0.0152***	0.0163***	-0.0011
	(0.0033)	(0.0027)	(0.0021)
Non-EU*t = +3	0.0173***	0.0181***	-0.0008
	(0.0035)	(0.0029)	(0.0023)
Non-EU*t = +4	0.0221***	0.0236***	-0.0015
	(0.0038)	(0.0032)	(0.0024)
Non-EU*t = +5	0.0256***	0.0260***	-0.0003
	(0.0039)	(0.0034)	(0.0025)
Baseline	0.0925	0.0659	0.0266
R-Squared	0.0576	0.0544	0.0317
Observations	489,749	489,749	489,749
Individuals	75,138	75,138	75,138
Individual controls	Yes	Yes	Yes
LLM-Year FE	Yes	Yes	Yes
Education-Year FE	Yes	Yes	Yes

Notes: Table A.8 reports coefficients from Equation 2 using as outcome the probability of being employed in a licensed occupation. Individual controls include sex, age, age squared, age at entry, age at entry squared, nationality, educational level. Local Labor Market (LLM) times Year FE and Education times Year fixed effects are included. Standard errors are clustered at the individual level.
Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: Integrated Employment Biographies.

Table A.9: Reform effects on employment, immigrants arrived before 2012

	(1)	(2)	(3)	(4)
Post*Non-EU	0.0176***	0.0171***	0.0166***	0.0167***
	(0.0026)	(0.0026)	(0.0026)	(0.0027)
Baseline	0.0925	0.0925	0.0925	0.0925
R-Squared	0.0057	0.0457	0.0539	0.0576
Observations	489,749	489,749	489,749	489,749
Individuals	75,138	75,138	75,138	75,138
Individual controls	No	Yes	Yes	Yes
Year FE	No	No	Yes	Yes
LLM FE	No	No	Yes	Yes
LLM-Year FE	No	No	No	Yes
Education-Year FE	No	No	No	Yes

Notes: Table A.9 reports coefficients from Equation 2 using as outcome the probability of being employed in a licensed occupation. Each column presents a different specification. Individual controls include sex, age, age squared, age at entry, age at entry squared, nationality, educational level. Standard errors are clustered at the individual level.
Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: Integrated Employment Biographies.

Table A.10: Employment in licensed occupations (employed and full-time employed)

	(1)	(2)	(3)	(4)
	Panel A: All employed			
Post*Non-EU	0.0150***	0.0132***	0.0127***	0.0136***
	(0.0029)	(0.0029)	(0.0029)	(0.003)
Baseline	0.1263	0.1263	0.1263	0.1263
R-Squared	0.0029	0.0547	0.0666	0.0705
Observations	404,471	404,471	404,471	404,471
Individuals	68,571	68,571	68,571	68,571
	Panel B: Employed full-time			
Post*Non-EU	0.0143***	0.0100***	0.0092***	0.0099***
	(0.0035)	(0.0034)	(0.0034)	(0.0036)
Baseline	0.1233	0.1233	0.1233	0.1233
R-Squared	0.002	0.0632	0.0796	0.0853
Observations	274,211	274,204	274,197	274,190
Individuals	52,884	52,884	52,884	52,884
Individual controls	No	Yes	Yes	Yes
Year FE	No	No	Yes	Yes
LLM FE	No	No	Yes	Yes
LLM-Year FE	No	No	No	Yes
Education-Year FE	No	No	No	Yes

Notes: Table A.10 reports coefficients from Equation 1 restricting the sample to only employed immigrants (Panel A), and to only full-time employed immigrants (Panel B). The outcome variable is the probability of employment in licensed occupations. Individual controls and group fixed effects are the same as in the baseline specification. Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: Integrated Employment Biographies (IEB).

Table A.11: Alternative control groups: robustness and placebo estimations

	(1)	(2)	(3)	(4)
	Native Germans		German education	
	Treatment	Placebo	Treatment	Placebo
Post*Non-EU	0.0170***	0.0007	0.0156***	0.0033*
	(0.0018)	(0.0026)	(0.0019)	(0.0019)
Baseline	0.0926	0.1427	0.0797	0.0669
R-Squared	0.035	0.0318	0.0392	0.0249
Individuals	2,457,501	2,301,358	728,942	580,729
Observations	329,995	309,455	102,284	77,483

Notes: Table A.11 reports estimated coefficients for a series of robustness checks. In columns 1 and 2 native Germans are the control group. In column 1, we use non-EU immigrants as treated group. In Column 2 EU immigrants are the treated. In columns 3 non-Eu immigrants with foreign education are the treated and non-EU immigrants with German education are the control group. We proxy this group by including non-EU immigrants who entered the register before age 20 and obtained the highest level of education (either vocational or university degree). In Column 4 EU immigrants with foreign education are the treated, and EU immigrants with German education are the control group. In all specifications we include individual controls, year and local labor market fixed effects. Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: Integrated Employment Biographies (IEB).

Table A.12: Alternative sample and outcome definitions: Employment in licensed occupations

	(1)	(2)	(3)	(4)	(5)	(6)
	Alternative sample definitions				Alternative outcomes	
	Baseline	Education (Highest)	Education (mode)	Nationality (first)	Log	Regulation Index
Post*Non-EU	0.0166***	0.0183***	0.0190***	0.0166***	0.1476***	0.0096***
	(0.0026)	(0.0023)	(0.0025)	(0.0026)	(0.0348)	(0.0024)
Baseline	0.0925	0.0973	0.1114	0.0924	6.502	0.1009
R-Squared	0.0539	0.0493	0.0458	0.0558	0.6517	0.1257
Observations	489,751	698,329	543,443	496,100	8,262	392,858
Individuals	75,138	98,311	82,117	76,093		67,556
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes. Table A.12 reports coefficients from Equation 1 using alternative definitions of the sample. The dependent variable is the probability of being employed in licensed occupations. Column 1 reports the baseline results from Table A.9, column 3. In the baseline specification the sample includes EU and non-EU immigrants who entered Germany at age 23 or older, whose education level in the first spell is vocational or higher education and whose citizenship is non-German. In Columns 2 and 3 we change the education definition; first with the highest value and second with the modal value. In Column 4 we change the citizenship variable taking the first nationality. In Column 5, we report the results from aggregating the analysis at the education, nationality, age, gender, year and local labor market and using the log of individuals in licensed occupations as outcome. In Column 6, we use the occupation-level regulation index (described in Vicari (2014)) as outcome. Individual controls and group fixed effects (year and local labor market) are included. Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: Integrated Employment Biographies (IEB).

Table A.13: Robustness: balance of covariates in 1:1 matched sample

	EU	Reweighted Non-EU
Female	0.385	0.356
Higher education	0.460	0.511
Age at entry	41.279	40.139
Age at entry	30.057	30.007
Arrival cohort		
1975-1984	0.098	0.057
1985-1994	0.176	0.187
1995-2004	0.283	0.240
2005-2011	0.443	0.515
Individuals	26,462	26,462

Notes: Table A.13 reports descriptive statistics for the 1:1 matched sample of EU and non-EU immigrants. The matching was done through propensity score with no replacement. Matching variables: sex, age at entry, entry cohort, education.
Source: Integrated Employment Biographies (IEB).

Table A.14: Robustness: Regression coefficients for 1:1 matched sample

	(1)	(2)	(3)	(4)
Post*Non-EU	0.0211***	0.0156***	0.0153***	0.0133***
	(0.0029)	(0.0029)	(0.0028)	(0.0029)
Baseline	0.1136	0.1136	0.1136	0.1136
R-Squared	0.0033	0.0491	0.0642	0.0698
Observations	343,952	343,952	343,952	343,952
Individuals	52,924	52,924	52,924	52,924
Individual controls	No	Yes	Yes	Yes
Year FE	No	No	Yes	Yes
LLM FE	No	No	Yes	Yes
LLM-Year FE	No	No	No	Yes
Education-Year FE	No	No	No	Yes

Notes: Table A.14 reports descriptive statistics for the 1:1 matched sample of EU and non-EU immigrants. The matching was done through propensity score with no replacement. Matching variables: sex, age at entry, entry cohort, education.
Source: Integrated Employment Biographies (IEB).

Table A.15: Main specification with balanced panels

	(1)	(2)	(3)	(4)	(5)
	Unbalanced		Balanced across years:		
		2007-2017	2008-2017	2009-2017	2010-2017
Post*Non-EU	0.0166***	0.0152***	0.0142***	0.0141***	0.0154***
	(0.0026)	(0.0029)	(0.0028)	(0.0027)	(0.0025)
Baseline	0.0925	0.0905	0.0926	0.0949	0.0961
R-Squared	0.0539	0.0545	0.0535	0.0526	0.0535
Observations	489,751	229,587	225,917	220,015	211,922
Individuals	75,138	23,982	25,700	27,509	29,455
Individual controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes

Notes: Table A.15 reports coefficients from Equation 1 using alternative definitions of the sample. The dependent variable is the probability of being employed in licensed occupations. Column 1 reports the baseline results from Table A.9, column 3. The balanced panels include only individuals who are present in the data in all years throughout the time window. For example, in the balanced panel 2007-2017, we include only immigrants who were in the dataset in 2007 and remained through all years up to 2017. Individual controls and group fixed effects are the same as in the baseline specification. Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: Integrated Employment Biographies (IEB).

Table A.16: Heterogeneous effects by local labor market characteristics

	(1)	(2)	(3)	(4)
	Local Demand		Co-ethnic Network	
	Low	High	Small	Wide
Post*Non-EU	0.0132***	0.0190***	0.0122***	0.0251***
	(0.0039)	(0.0035)	(0.0029)	(0.0063)
Baseline	0.0944	0.0901	0.0873	0.0951
R-Squared	0.0615	0.0510	0.0900	0.0459
Observations	257,634	231,679	358,214	131,096
Individuals	44,727	40,382	59,613	24,397
Individual controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes

Notes: Table A.16 reports coefficients from Equation 1 for subgroups of immigrants based on the characteristics of the local labor market where they work (or live if they are currently unemployed). These characteristics are reported in the columns' headers, and described in Section 5.2. The outcome variable is the probability of being employed in licensed occupations Individual controls and group fixed effects are the same as in the baseline specification. Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: Integrated Employment Biographies (IEB).

Table A.17: Heterogeneous effects by socio-demographic characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Male	Female	Vocational	University	Entry age: 23-34	Entry age: 35-55
Post*Non-EU	0.0109***	0.0220***	0.0068**	0.0358***	0.0144***	0.0205***
	(0.0031)	(0.0044)	(0.0029)	(0.0047)	(0.0032)	(0.0059)
Baseline	0.0743	0.1164	0.0729	0.1376	0.0968	0.0854
R-Squared	0.0595	0.0544	0.0602	0.0794	0.0645	0.0684
Observations	279,467	210,280	309,562	180,185	300,395	111,360
Individuals	42,832	32,302	45,356	29,778	43,894	19,482
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table A.17 reports coefficients from Equation 1 for subgroups of immigrants based on individual characteristics. These characteristics are reported in the columns' headers. The outcome variable is the probability of being employed in licensed occupations. Individual controls and group fixed effects are the same as in the baseline specification. Standard errors are clustered at the individual level.
Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: Integrated Employment Biographies (IEB).

Table A.18: Reform effects on employment in non-licensed occupations

	(1)	(2)	(3)	(4)
	Eligible		Non-eligible	Overall
	Licensed	Non-licensed	occupations	employment
Post*Non-EU	0.0166***	0.0425***	-0.0009	0.0582***
	(0.0027)	(0.0037)	(0.0036)	(0.0023)
Baseline	0.0925	0.3071	0.3325	0.7321
R-Squared	0.0576	0.0561	0.0400	0.1271
Observations	489,749	489,749	489,749	489,749
Individuals	75,138	75,138	75,138	75,138
Individual controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes

Notes: Table A.18, from Equation 1 using as outcome variable the probability of being employed in eligible licensed occupations (Column 1), in eligible non-licensed occupations (Column 2), in non-eligible non-licensed occupations (Column 3), in any occupation (Column 4). Eligible non-licensed occupations are vocational training occupations(*Ausbildungsberufe*). Only immigrants who were in Germany pre-reform are included. Individual controls include sex, age, age squared, age at entry, age at entry squared, years in the register (and its squared transformation), nationality, educational level. Local labor market and year fixed efffects are included. Standard errors clustered at the individual level.
Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: Integrated Employment Biographies (IEB).

Table A.19: Characteristics of the last employment spell before moving to licensed occupations

	(1)	(2)	(3)	(4)
	Labor market outcomes in t-1			
	Employed	Part-time	Routine task	Log Daily Wage
Post*Non-EU	0.0576***	0.0159	0.0168	-0.0134
	(0.0125)	(0.0165)	(0.0159)	(0.0255)
Baseline	0.5218	0.5602	0.7304	1.7702
R-Squared	0.1387	0.1679	0.1761	0.2686
Transitions	32,951	15,741	15,741	13,672

Notes: Table A.19 reports the coefficient for regression models based on Equation 1 in which the outcomes are different characteristics of the last employment spell before moving to a licensed occupation. Transitions *within* these licensed occupations are excluded. In column 1 the dependent variable is the probability of being employed in the spell before moving to a licensed occupation with a high number of applications. In columns 2-5, the dependent variables are constructed using the characteristics of the previous employment spell. Column 2 shows the probability that the previous employment spell was part- or full-time. In Column 3 the outcome is the probability that the previous main occupational task was manual (routine or non-routine) compared to non-manual. Column 4 shows the previous log daily wage. The number of observations is lower in Column 4 because of missing values on wages. Baseline is the average pre-reform information for non-EU immigrants. Controls include sex, age, age squared, age at entry, age at entry squared, years in the register (and its squared transformation), nationality, educational level, year fixed effects, local labor market fixed effects. Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: Integrated Employment Biographies (IEB).

Table A.20: Alternative control groups for the effect on non-EU immigrant wages in licensed occupations

	(1)	(2)	(3)
	Log daily wage		
	Baseline	Natives	Non-EU in non-elig. occ.
Post*Non-EU	0.0415***	0.0306***	0.0431***
	(0.0113)	(0.0082)	(0.0091)
Baseline	4.230	4.2301	4.2301
R-Squared	0.496	0.3792	0.627
Observations	60,896	418,473	131,982
Individuals	13,711	76,653	27,580

Notes: Table A.20 reports the estimated coefficients from Equation 1 for log daily wages, restricting the sample to immigrants working in licensed occupations. Column 1 presents the baseline with EU immigrants as control group, Column 2 uses native Germans as control group, while Column 3 uses non-EU immigrants in occupations not affected by the recognition reform as control group. Individual controls include sex, age, age squared, age at entry, age at entry squared, years in the register (and its squared transformation), nationality, educational level. Local labor market and year fixed effects are included. Individual controls include also tenure in the occupation. Fixed effects include also industry fixed effects (3 digits) and occupation fixed effects (3 digits). Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: Integrated Employment Biographies.

Table A.21: Reform effects on marginal employment contracts in licensed occupations

	(1)	(2)	(3)	(4)
	Baseline		**German as control**	
	Incumbents	New entries	Incumbents	New entries
Post*Non-EU	-0.0082	-0.0463**	-0.0083**	-0.0268**
	(0.0052)	(0.0196)	(0.0041)	(0.0116)
Baseline	0.076	0.263	0.077	0.166
R-Squared	0.213	0.210	0.096	0.212
Observations	47,685	7,054	379,490	41,683
Individuals	10,794	6,042	67,987	37,801

Notes: Table A.21 reports the estimated coefficients from Equation 1 for the probability of having a marginal employment contract, restricting the sample to immigrants working in licensed occupations. Columns 1 and 2 is the baseline with EU as control group, Columns 3 and 4 use native Germans as control group. Incumbents are defined as individuals who were already working in t-1 in licensed occupations, new entrants are defined as individuals who were either unemployed or not working in a licensed occupation in t-1.Individual controls include sex, age, age squared, age at entry, age at entry squared, years in the register (and its squared transformation), nationality, educational level. Local labor market and year fixed effects are included. Individual controls include also tenure in the occupation. Fixed effects include also industry fixed effects (3 digits) and occupation fixed effects (3 digits). Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: Integrated Employment Biographies.

Table A.22: Effects of the non-EU immigrant labor supply shock on natives' employment and earnings overall and in licensed occupations

	(1)	(2)	(3)	(4)
	Employment		**Log Daily Wage**	
	All	Licensed	All	Licensed
Post*HighSupply	-0.0041***	-0.0022*	-0.0045**	0.0064
	(0.0009)	(0.0012)	(0.0018)	(0.0041)
Baseline	0.9017	0.1577	4.2794	4.3144
R-Squared	0.0322	0.0235	0.497	0.3764
Observations	2,286,279	2,286,279	2,007,430	381,378
Individuals	295,639	295,639	281,192	68,084
Individual controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
Occupation FE	No	No	Yes	Yes

Notes: Table A.22 reports estimated coefficients for regression models based on Equation 4. The sample includes native Germans aged 23-55 with either vocational training or university education. In Column 1, the outcome is the probability of being employed, in Column 2 the probability of being employed in licensed occupations, in Column 3 the log daily wage in any employment spell, and in Column 4 the log daily wage for workers in licensed occupations. Individual controls are sex, age, age squared and educational level. Regression models with wage outcomes also include the tenure in the current occupation. All regressions include year and local labor market fixed effects. Regression models with wage outcomes also include industry and occupation fixed effects. Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: Integrated Employment Biographies.

Table A.23: Alternative specifications for the effects on natives' employment and wages

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment	Employment (licensed)	Log Daily Wage	Log Daily Wage (licensed)		
Post*HighSupply (alternative)	-0.0016	0.0026**	-0.0042**	0.0064		
	(0.0009)	(0.0013)	(0.0019)	(0.0041)		
Post*EligibleOccupations					0.0029	
					(0.0026)	
Post*HighApplicantsOccupations						0.0067
						(0.0049)
Baseline	0.9017	0.1577	4.2794	4.3144	4.3098	4.3144
R-Squared	0.0336	0.0235	0.4969	0.3764	0.5431	0.3764
Observations	2,286,279	2,286,279	2,007,430	381,378	917,795	381,378
Individuals	295,639	295,639	281,192	68,084	160,933	68,084

Notes: Table A.23 reports estimated coefficients for regression models based on Equation 4. The sample includes native Germans aged 23-55 with either vocational training or university education. In Column 1, the outcome is the probability of being employed, in Column 2 the probability of being employed in licensed occupations, in Column 3 the log daily wage in any employment spell, in Columns 4 to 6 the log daily wage for workers in licensed occupations. Columns 1-4 use an alternative definition of Equation 3, where the denominator is all registered workers in the local labor market. Column 5 uses a dummy $EligibleOccupations$ which assigns a value 1 to natives in licensed occupations, and 0 if employed in occupations not affected by the reform. Column 6 uses a dummy $HighApplicantsOccupations$ which assigns a value 1 to natives in TOP 15 licensed occupations, and 0 if employed in other licensed occupations (which received only 7 percent of all applications for recognition). Individual controls are sex, age, age squared and educational level. Regression models with wage outcomes also include the tenure in the current occupation. All regressions include year and local labor market fixed effects. Regression models with wage outcomes also include industry and occupation fixed effects. Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: Integrated Employment Biographies

B Description of Datasets and Variables

B.1 IAB-SOEP Migration Sample

Sample construction For the analysis of the reform effects on application and recognition rates (Section 5.1) we use the 2013, 2014, 2015, and 2016 waves of the IAB-SOEP Migration Sample. The data were collected between 2012 and 2015 and contain retrospective information on immigrants' recognition processes. Specifically, the survey asks immigrants with a foreign-acquired education or professional qualification whether and when they applied for recognition and, if they applied, it asks for the result of the application and in which year they received the results (year of recognition). Additionally, the survey asks immigrants in which year they entered Germany for the first time.
Combining these pieces of information we construct a panel dataset for each immigrant, where the first observation is the year of arrival to Germany and the last one is the most current survey wave in which the respondent was interviewed. For example, if an immigrant arrived in Germany in 2000 and answered the survey questions in 2014, the panel will have yearly observations from 2000 to 2014.
To construct time-varying application and recognition variables we then proceed as follows. For the application variable, we use the year of application and assign a value of 1 to observations from the year of application onwards, and 0 to the years before application or if the immigrant has never applied. For the successful recognition variable, we use the year of recognition combined with the information on the recognition result and assign a 1 to observations from the year of recognition if the application was successful, and 0 if the application was not successful or if the immigrant has never applied. For example, if an immigrant from the 2014 survey wave arrived in 2000, applied for recognition in 2007 and received recognition results in 2008, then the application variable takes the value 0 from 2000 to 2006, and the value 1 from 2007 to 2014. If the result is positive (either full recognition or partial recognition), the successful recognition variable takes value 1 from 2008 to 2014.
Around 20 percent of the observations for which we have information on the application year and the application decision did not state the year when they received the decision. We deal with missing year information in the following way. We fill in the missing values assigning the year of application + 1. This assumption is reasonable, since before the reform the average distance in year between application and decisions is 1.5 years and the median 1.
Furthermore, for some immigrants who applied for recognition, the application was still pending at the time of the survey. We deal with these cases of not yet recognized certificates in the following way. We treat pending applications as successful applications. This is reasonable, since the share of successful applications in all applications is more than 80 percent both pre- and post-reform (as computed based on the IAB-SOEP migration sample), and more than 90 percent according to official statistics on recognition procedures from the BIBB.

Validation of recognition variables In this section we validate the recognition variables used for the estimation of the effects of the reform on recognition rates. Given that information on recognition procedures is asked retrospectively and might be therefore subject to measurement error, we exploit other data sources on recognition procedures and compare it with the one present in the IAB-SOEP Migration Sample. In detail, we first use the 2008 ad hoc module of the German Microcensus which focused on immigrants' integration and collected information on whether immigrants applied for recognition and on the outcome of the recognition procedures. We compute the percentage of immigrants in Germany before 2012 (i.e. before the Recognition Act) with recognition, with a failed or on-going recognition procedures and with no application for recognition. We also distinguish between different types of certifications (Figure B.1). Reassuringly, we find that the distributions in the two data sets are remarkably similar. Second, we gather information from official recognition statistics on the number of applications by regions of origin and aggregate SOEP immigrants according to the same regions of origin. Since official statistics refer only to recognition procedures after 2012, we consider SOEP immigrants who applied for recognition from 2012 onwards. We then compare the composition of applicants by regions of origin (Figure B.2). Also in this case, the distributions are closely comparable between the two data sources. Overall, these tables show that individual data on recognition from the SOEP are representative of recognition procedures.

Table B.1: Validation of application variable: by education

	(1)	(2)	(3)	(4)	(5)	(6)
	IAB-SOEP Migration			Microcensus		
	Recog	No recog	No app	Recog	No recog	No app
VET	17.5	9.9	72.6	14.1	8.6	77.3
Fachhochschule	34.5	12.3	53.2	36.6	10.0	64.4
University	30.3	9.1	60.6	27.4	8.2	64.4
PhD	47.8	8.7	43.5	40.0	-	60.0

Notes: Table B.1 shows the distribution of immigrants who obtained recognition (*Recog.*), applied but did not obtain recognition (*No Recog.*) and did not apply (*No app.*) within the same type of certification. Columns 1, 2, and 3 report the shares for immigrants in the IAB-SOEP Migration Sample who arrived in Germany in the pre-reform period, while columns 4, 5, and 6 display the percentages for immigrants in the German Microcensus 2008 Ad Hoc Module on immigrants' integration.
Source: IAB-SOEP Migration Sample and German Microcensus 2008 Ad Hoc Module.

Table B.2: Validation of application variable: by nationality

	(1)	(2)
	IAB-SOEP Migration	Register data (BIBB)
European Continent	77.8	81.0
Africa	4.0	5.5
Middle East and Asia	16.0	12.3
North and Central America	0.9	0.8
South America	1.3	1.3
Oceania and others	0.0	0.2
Total	225	17550

Notes: Table B.2 shows the distribution of applicants across regions of origin. In column 1 we report the shares for immigrants interviewed in the IAB-SOEP Migration Sample. In Column 2 we report the shares from the official statistics of the BIBB which were acquired from 2012 onwards to monitor recognition procedures after the implementation of the Federal Recognition Act. The regions of origin were pre-defined in the official statistics. To match the official statistics, in the SOEP computations we include all applicants who applied from 2012 onwards and recode countries of origin to the same regions in the BIBB data.
Source: IAB-SOEP Migration Sample and Official Statistics (BIBB).

B.2 Integrated Employment Biographies (IEB)

It is well known that some information collected through administrative sources is less reliable because employers have low incentives to correctly declare it. In particular, in the Integrated Employment Biographies both the nationality variable and the education variable may be problematic due to misreporting or underreporting behaviors of employers. Given the relevance of these two pieces of information for our sample selection and estimation, we explain below how we improved on the raw information and provide validating evidence on the quality of our variables.

Nationality We construct the nationality variable by taking the mode of the nationality value across all spells in the dataset. The value we assign to each individual is therefore the most frequent nationality their employers report. We then exclude all immigrants whose mode value of nationality is German and all who have no valid nationality values. While this might exclude immigrants who received citizenship early on in their employment careers, it allows to better identify the most likely foreign nationality. In alternative specifications we try also alternative definitions of nationality, that is based on the first valid nationality value and by including only immigrants who never had a spell as German natives. Results are not sensitive to this definition. Moreover, we show that the distribution across macro-regions of origin in the IEB data is almost identical to the distribution of origin countries constructed from the German Microcensus where we are able to identify more clearly both the time of immigration and the foreign nationality (in the German Microcensus it is asked explicitily whether they have German citizenship).

Education Two issues with the education variable may be relevant for our analysis. First, which is the true educational level of immigrants, and second whether they acquired education domestically (i.e. in Germany) or abroad. We address both issues by using the first available information on education and by restricting our analysis to immigrants who appear in the data after 23. We choose 23 as the cut-off age of entry as we assume that by 23 immigrants already plausibly acquired both a university degree or a vocational training. Moreover, in Germany many university students and vocational trainees enter the labor market already before the end of their educational career. The restriction based on the age of entry therefore allows us to reduce the concern that education might have been acquired in Germany (and that recognition wouldn't be necessary). Since in the administrative data we can only approximate the inclusion of individuals who acquired tertiary education and vocational training abroad, in Table B.3 we also show the same socio-demographic characteristics using immigrants in the German Microcensus (GMC). The GMC asks immigrant respondents both their year of immigration and the year they acquired their highest educational level. We can therefore more precisely identify immigrants who acquired their education abroad. The characteristics of immigrants in the IEB and the GMC are remarkably similar, with only the educational level being under-estimated in the IEB data. To address this issue we show in Table A.12 that results are not sensitive to changes in the definition of the educational variable. In particular, we run the main regression model using the highest level of education achieved instead of the first reported value. This includes immigrants for which employers might have falsely reported the level of education. Moreover, in case the bias from the measurement error is large, this would likely underestimate the positive effects on employment.

Occupational code Throughout the analysis we classify occupations using the 3-digits Kldb1988. For all employees, the employer encodes the employee's job in accordance with the "Classification of Occupations. Systematic and Alphabetical Directory of Job Titles" (published by the Federal Employment Agency, Nuremberg, 1988), which contains approx. 25,000 job titles. The occupational classification Kldb1988 consists of a 3-digit code and comprises about 330 values. In December 2010 the Federal Employment Agency introduced a new classification, Kldb2010, with 5-digits. This change brought a large number of firms to misreport or underreport the occupational variable in 2011. We fix this coding problem with the following approach. We exploit other pieces of information which were not subject to any reporting change from 2010 onwards, that is work and home location at the district (*Kreis*) level [44], industry code (WZ08 classification) and firm identification number. We then considered the last available occupational code before the reporting change and assigned this value to all subsequent employment spells, as long as work or home location, firm ID and industry code did not change. This procedure addresses both misreporting and underreporting errors. As an outcome of this procedure, missing values on the occupational code in 2011 starkly decline. With the fixed occupational code, we then move from the Kldb2010 to the Kldb1988 using a table provided by the Federal Employment Agency. This is particularly relevant to identify occupations with high and low numbers of applications since the statistics from the Regional Statistical Offices on the recognition procedures use the Kldb2010. It should be noticed that the Kldb1988 is a 3-digit classification and it is therefore more aggregated than the Kldb2010. As often occurs with occupational recodings, this recoding generated a jump in the data. In Figure 3 we follow Goos et al. (2014), and

[44] The *Kreis* level corresponds to the NUTS3 level of the NUTS geocode standard.

Table B.3: Socio-demographic characteristics of immigrants who entered Germany pre-reform, 2007-2017, German Microzensus vs Integrated Employment Biographies (IEB)

	(1)	(2)	(3)	(4)
	Non-EU		EU	
	IEB	Microcensus	IEB	Microcensus
Female	0.45	0.46	0.39	0.42
Higher education	0.33	0.43	0.44	0.64
Age	41.24	42.10	41.24	41.89
Age entry	30.61	31.96	29.84	32.33
Years in the register	10.13	10.62	10.91	10.05
Northern and Continental Europe			0.66	0.68
Southern Europe			0.34	0.32
Eastern Europe and Russia	0.25	0.27		
Balkans and Turkey	0.26	0.26		
Africa	0.09	0.08		
Middle East	0.10	0.10		
Asia	0.19	0.18		
North and Central America	0.06	0.06		
South America	0.04	0.04		
Oceania and others	0.01	0.01		
Observations	329,666	14,075	160,123	6,067

Notes: Table B.3 reports variable means for the Integrated Employment Biographies (IEB) sample and for a sample analogue in the German Microcensus. We pull all Microcensus waves from 2007 to 2017 together and compute variables as similar as possible to the IEB sample characteristics, while improving on some of the variables that the IEB does not include. In particular, we replace age at entry into the IEB with actual age at entry in Germany, and we replace the proxy for having acquired education abroad with actual information on acquired education abroad. Moreover, the nationality variable is more precise in the Microcensus. We consider only immigrants with reported year of entry earlier than 2011 to simulate the sample selection in the IEB. We exclude resettled immigrant groups with German origin, as they are likely to be registered with a German nationality in the IEB data. Source: Integrated Employment Biographies (IEB) and German Microcensus.

adjust the jump in the time series by taking the difference between 2011 and 2012 and applying this difference to all pre-reform years.

Local demand for licensed occupations We construct pre-reform demand for licensed occupations in local labor markets in the following way. We obtain from the Federal Employment Agency vacancy and unemployment totals by year, occupational code (3-digit Kldb1988) and district (*Kreis*). Unemployment data specific to an occupation come from the information on the occupation for which unemployed search for a job. This information is declared at the time of unemployment registration. The vacancy data report the numbers of positions open in each occupation as declared by firms. The unemployment to vacancy ratio captures therefore the extent to which firms are able to fill in their vacancies with local supply. We compute the unemployment-to-vacancy ratio in all districts and broad group of occupations (licensed occupations with large number of applicants) averaging the values for the years 2007-2010, the pre-reform period. We exclude 2011 due to its proximity to the reform. We then average the values across districts belonging to the same local labor market and assign to each individual the value according to its local labor market variable.