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The Effect of Initial Placement Restrictions on Refugees' Language Acquisition in Germany

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The Effect of Initial Placement Restrictions on Refugees' Language Acquisition in Germany

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Abstract

This paper analyzes the effect of a recently introduced policy reform on participation in integration courses and on certified language proficiency levels among refugees in Germany. The *residence rule* restricts initial residence for refugees with a permanent residence permit. Given that treatment intensity varies distinctly across states, I utilize this quasi-experiment and apply a difference-in-differences approach. Using an innovative data-set, the IAB-BAMF-SOEP Survey of Refugees, I find that stricter statutory provisions have a positive effect on the probability to complete a language course and on the level of certified language proficiency. The results indicate that this effect is driven partly by spatial mismatch.

Keywords: Migration, Refugees, Language Acquisition, Placement Restriction, Quasi-Experiment.

JEL codes: J15, J60, K37.

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1 Introduction

The destabilization of the MENA (Middle East and North Africa) region has triggered a major influx of migrants to Europe in recent years. Their large numbers pose challenges to national governments and create the need for targeted policy measures to foster integration. This is particularly urgent considering that many refugees¹ have been granted protection status and will likely remain in their host countries for an extended period, or even permanently.

Given the large influx of refugees (Figure A1), the German federal government introduced several integration measures for refugees with protected status, including the *residence rule* (*Wohnsitzauflage*). The *residence rule* puts severe restrictions on refugees' ability to choose their place of residence. Thereby, the policy aims at distributing financial burdens more evenly across municipalities and facilitating the planning of integration activities and language courses (SVR, 2016, p.4ff). In light of the strong link established in the literature between immigrants' language skills and prospective labor market outcomes², completion of integration courses may be considered a first essential step towards successful integration. This paper therefore disentangles the effects of the *residence rule* on participation in integration courses and language acquisition by refugees in Germany.

To the best of my knowledge, this is the first paper to investigate the effect of initial placement restrictions on refugees' language acquisition. I use an innovative data-set, the IAB-BAMF-SOEP Survey of Refugees, which

¹In the public debate, the terms *refugee* and *migrant* are often used interchangeably. Nevertheless, it is essential to distinguish them explicitly. The term *migrant* describes an individual moving to another country, expecting to live there for a certain period of time. In the recent German context, I use the term *refugee*, following the UNHCR's (United Nations High Commissioner for Refugees) definition. A *refugee* is someone who fears "being persecuted for reasons of race, religion, nationality, membership of a particular social group or political opinion, is outside the country of his nationality and is unable or, owing to such fear, is unwilling to avail himself of the protection of that country; or who [...] is unwilling to return to it" (United Nations High Commissioner of Refugees, 1951, p.14).

²E.g., Chiswick (1991); Chiswick and Miller (1995); Dustmann and van Soest (2001); Dustmann and Fabbri (2003); Bleakley and Chin (2004).

provides unique information on refugees who have arrived in Germany since 2013, a group that differs substantially from foreigners who arrived before. The paper thus sheds light on early integration of refugees in a very recent context. What is more, even though the data-sets employed in previous analyses are rich and of high quality, most have been unable to identify an immigrants' formal status.³ Because the IAB-BAMF-SOEP Survey of Refugees provides detailed information on the type of residence permit, I am able to circumvent this potential source of bias.

The empirical analysis relies on a quasi-experimental setting that exploits variation in statutory regulations across German states after a reform introduced in 2016. As a result of the *residence rule*, a group of refugees who were formerly able to choose their place of residence freely, now face severe restrictions on moving for an initial period of three years. Yet, there exists substantial variation in the implementation of the policy across states. While those with protection status may move freely within some states, a few states have decided to implement the *residence rule* more rigorously, requiring refugees to stay in a certain district.

Exploiting both temporal as well as spatial variation in a difference-in-differences (DiD) estimation design, I find that living in a state that restricts place of residence to the district (rather than the state) level has a substantial positive effect on the probability of completing an integration course and on the certified level of language proficiency. For example, residing in a high treatment intensity state increases the probability to complete an integration course in the year of the asylum decision by 7.0 percentage points, relative to a mean of 8.6 percent. This effect is quite sizable and robust to the definition of various subsamples and to the use of wild-cluster bootstrap t-procedures. Estimation results further suggest that stricter placement restrictions increase participation in other language courses as well, although the results are much less robust. Moreover, the results indicate that the effect is driven partly by spatial mismatch (Kain, 1968). Once I control for the supply of integration courses at the district level,

³Notable exceptions in the recent literature include Bratsberg et al. (2017) and Schultz-Nielsen (2017).

the effects decrease by about one-third and are much less robust, especially for language proficiency levels. This may indicate that states with stricter residence rules place humanitarian migrants in districts with a more abundant supply of integration courses.

The remainder of this paper is structured as follows. Section 2 describes how this research fits into the existing literature. Section 3 describes the institutional background and illustrates the relevant policy reform. Section 4 outlines the empirical strategy. After a short description of the data-set, Section 5 provides the definition of the working sample as well as descriptive statistics. Section 6 reports the main results including some robustness checks, while Section 7 provides guidance on mechanisms. Section 8 concludes.

2 Literature Review

This paper is connected to two branches of research. The first studies the effect of initial placement restrictions on determinants of economic assimilation and immigrants' labor market performance. In this context, it is essential to account for selective migration patterns. If immigrants choose to reside in a host country based on factors such as expected labor market outcomes (Card, 1990, p.245) or pre-existing ethnic enclaves (Edin et al., 2003, p.329), estimates may be seriously biased.⁴ In the existing literature, a number of studies have accounted for endogenous sorting of immigrants by studying reforms that are very similar to the *residence rule*. These *settlement policies*, introduced in Sweden and Denmark in the 1980s, determined immigrants' place of residence without considering their individual preferences and, as such, exogenously. Using the size of an ethnic enclave in the year of assignment as an instrumental variable (IV), Edin et al. (2003) showed that being placed in an ethnic enclave increases immi-

⁴To tackle this potential pitfall, Peri (2016) emphasizes the importance of exogenous variation in the empirical analyses and suggests to use natural experiments. The subsequent paragraph therefore reviews literature that relies on quasi-experimental settings only.

grants' earnings significantly, in particular for immigrants in the lower tail of the skill distribution. This finding was confirmed by [Damm \(2009\)](#), who reports that this positive effect is primarily driven by immigrant networks. [Edin et al. \(2004\)](#) used a DiD design and show that policies that choose income support over reintegration measures have detrimental effects on immigrants' earnings. Additionally, [Rosholm and Vejlin \(2010\)](#) report that lowering public income transfers have a positive effect on the probability to find employment in a competing risk framework. Finally, [Åslund and Rooth \(2007\)](#) emphasize that local as well as national labor market conditions play a crucial role in determining long-term integration outcomes.

This paper takes an alternative perspective by focusing on language acquisition as an early integration outcome. It is therefore also related to the research on the relationship between immigrants' language skills as "an important form of human capital" ([Chiswick and Miller, 1995](#), p.248) and their labor market performance. [Chiswick \(1991\)](#) showed the relevance of self-reported language skills on immigrants' earnings, and concludes that reading fluency is more important than speaking fluency. Using data on Australia, the United States (US), Canada, and Israel, a subsequent study provides evidence that this effect holds in an international context ([Chiswick and Miller, 1995](#)). [Dustmann and van Soest \(2001\)](#) build on the aforementioned studies by accounting for measurement errors in self-reported language skills and unobservable variables, which are correlated with language acquisition and respective outcome variables, such as ability: They demonstrate that simple ordinary least squares (OLS) estimates are subject to substantial biases and find that estimated effects on earnings are considerably larger once these factors are taken into account. Combining both a matching and an IV estimator, [Dustmann and Fabbri \(2003\)](#) find a positive effect of language skills on the probability to find employment in the UK, while the effect on earnings is less precise. Finally, [Bleakley and Chin \(2004\)](#) provide causal evidence on this topic, exploiting a quasi-experimental setting and using an interaction term of age at arrival and a dummy for non-English-speaking country of origin to solve the endogeneity

of language skills. Their results demonstrate that English proficiency affects wages of adults who arrived in the US as children, and what is more, they show that this effect is driven mainly by education.

3 Institutional Background

Individuals seeking political asylum in Germany have to register with a state authority upon arrival (BAMF, 2016b, p.8f). They are then assigned to an *initial reception center* and thereby to a particular state based on the *Königstein key*.⁵ The *Königstein key* is a distribution mechanism that allocates refugees to a state based on its tax revenue and population size.⁶ Since this distribution mechanism strives to ensure an equal distribution of refugees across states without considering individuals' preferences, initial placement in a specific state is exogenous and as such immune to self-selection.⁷

Once assigned to an initial reception center, refugees may formally place their application for asylum (BAMF, 2016b, p.11ff). Until a final decision has been reached, asylum seekers face severe restrictions on moving and must reside in the initially assigned state (*Residenzpflicht*).⁸ Importantly,

⁵In case there are several reception centers within the assigned state, the EASY quota system (Initial Distribution of Asylum Seekers) chooses the reception center located nearest to the authority where the refugee registered to minimize commuting costs.

⁶The formula is based on two-thirds on tax revenues and one-third on population size. For further information, please refer to <http://www.bamf.de/DE/Fluechtlingsschutz/AblaufAsylv/Erstverteilung/erstverteilung-node.html> (last downloaded on August 28, 2018). Table A1 illustrates the exact allocation scheme from 2010 to 2017. Table A2 then compares a state's assigned percentage share (*Königstein key*) to the actual distribution of asylum seekers across states from 2013 to 2017; the tables demonstrate that asylum seekers are indeed allocated to states according to the *Königstein key*.

⁷In practice, there is one exception: In compliance with basic constitutional law (Art. 6 GG), state authorities seek to place parents and their minor children in the same state if family members arrive at different points in time. Because family members generally apply for *family asylum*, this influences only 10 percent of placement decisions.

⁸While allocation to a particular state is regulated by federal law, states pass their own regulations determining how to distribute asylum seekers to specific districts. Most states have allocation schemes that are based primarily on the size of their districts' population. For an overview, please refer to Table A3.

this restriction for asylum seekers has remained unaffected by the *residence rule*. For clarification, Figure A2 illustrates the stages of applying for asylum in Germany schematically.

Whereas Germany has no distinct immigration act that explicitly regulates immigration other than asylum, it specifies four different categories of protection once a decision on an asylum claim has been reached. These categories are listed in Table A4, which also provides references to the underlying legal provisions, duration of residence permit, as well as some details on procedures. The Organization for Economic Co-operation and Development (OECD) defines people whose application for asylum has been approved and those granted some sort of protection as *humanitarian migrants* (Organization for Economic Co-operation and Development, 2016, p.7). Following the OECD's definition, this paper hereinafter refers to all refugees who have been granted a protection status of category 1 to 3 as *humanitarian migrants*. Foreigners with *tolerated status*, who are legally required to leave Germany eventually, are excluded from the empirical analysis.

The Residence Rule. — While asylum seekers face severe travel restrictions until a final decision on the asylum application has been reached, humanitarian migrants were previously free to choose their place of residence. The new regulation, in contrast, enforces severe restrictions on initial place of residence if certain criteria apply (SVR, 2016, p.20ff). Those who do not comply lose their social benefits. With introduction of the *residence rule* in summer of 2016, humanitarian migrants are required to take up residence in the state in which they filed their asylum request and remain there for a period of three years *unless* a legally defined exemption rule applies. This rule states that:

"Sentence 1 [placement restriction on state level, ed. noted] shall not apply where a foreigner, his spouse, registered domestic partner or minor child takes up or has taken up employment of at least 15 hours per week with full social security coverage (, on account of which that person has an income amounting to at least the average monthly needs for individual persons pursuant to Sections 20 and 22 of Book Two of the Social Code [€712,

ed. notes/), or that person takes up or has taken up vocational training or is pursuing his studies or is in a training relationship.” (Federal Ministry of Justice and Consumer Protection, 2016, §12a AufenthG, Art.1)

If humanitarian migrants do not satisfy the exemption rule, §12a of the Integration Act clearly limits mobility *between* states. Mobility *within* states, however, is only affected if states enforce additional legislation. A location restriction at the state level may be considered a marginal change only. Especially in economically less developed states, this regulation may nevertheless be particularly restrictive for humanitarian migrants targeted by the reform. Even though humanitarian migrants are free to leave the state if they find employment/training that satisfies the exemption rule, competing with native workers in a different local labor market is challenging. The *residence rule* may therefore severely restrict freedom of movement for humanitarian migrants.

Additional legislation at state level. — With the introduction of the Integration Act, states have further been given the possibility to impose additional regulations at state level (Federal Ministry of Justice and Consumer Protection, 2016, §12a Art. 9 AufenthG). As of January 2017, five states have decided to apply the *residence rule* more rigorously, including Baden Württemberg, Bavaria, North Rhine-Westphalia, Saarland, and Saxony-Anhalt (Organization for Economic Co-operation and Development, 2017, p.49f). These states mandate humanitarian migrants to live in a specific district.⁹ In all other states, humanitarian migrants remain free to move within the assigned state.

⁹Please refer to [Ministerium für Inneres, Digitalisierung und Migration, Baden-Württemberg \(2016\)](#); [Bayerische Staatsregierung \(2016\)](#); [Landesregierung Nordrhein-Westfalen \(2016\)](#); [Staatskanzlei Saarland \(2016\)](#); [Ministerium für Inneres und Sport, Sachsen-Anhalt \(2017\)](#); regulations available in German only.

4 Empirical Strategy

To estimate the effect of strict statutory requirements regarding place of residence on language outcomes of humanitarian migrants, I employ a DiD design, taking advantage of legal variation across states as well as the fact that humanitarian migrants have no influence on their initial assignment to a particular state.

Outcome Variables. — If migrants lack adequate command of German, it is strongly recommended that they participate in an *integration course (IK)* (BAMF, 2017). Humanitarian migrants are generally eligible for these courses. The curriculum is split in two parts, a general course on life in Germany (*orientation course*, 100 hours) and a language course (600 hours). Upon completion of this mandatory course curriculum, participants take a final exam including a language test and a quiz on the orientation course. While the exact duration of an integration course evidently depends on how classes are scheduled, the humanitarian migrants in the working sample took, on average, six months to complete an integration course with a standard deviation of three months.

The Integration Act aims at improving access to integration measures and language courses in particular. Hence, this paper investigates whether strict statutory provisions on initial place of residence have an effect on (1) the probability to begin an integration course in the year of the asylum decision, (2) the probability to complete an integration course in the year of asylum decision, and (3) certified language proficiency levels.¹⁰

Main Specification. — To identify the causal effect of a policy intervention on some relevant outcome variable, the DiD estimator takes *double differences*, i.e., compares the changes in the outcome variables of treated and control units over time (Lechner et al., 2011, p.168).¹¹ Hence, Lechner

¹⁰Language skills are measured on a scale from 0 to 3 (0 "No certified level", 1 "Level A1", 2 "Level A2", 3 "Level B1"). Cf. <https://www.coe.int/en/web/common-european-framework-reference-languages/home>, last downloaded on April 25, 2019.

¹¹Bertrand et al. (2004), Abadie (2005) or Imbens and Wooldridge (2009) present alternative perspectives on the theoretical background.

et al. (2011) argue that this quasi-experimental method controls for existing time trends as well as for permanent differences between treatment and control group.

The *residence rule* was introduced as part of the Integration Act in summer of 2016. Even though the law was publicly announced in July, it was introduced with retroactive effect to January 1, 2016. Nevertheless, all states but North Rhine-Westphalia (NRW) decided to apply the *residence rule* to decisions made after July 2016 only (Ministerium für Inneres und Bundesangelegenheiten, Schleswig-Holstein, 2016, p.4). Figure A3 illustrates the beginning of the post-treatment period for NRW and all other states respectively. On this basis, the pre-treatment period consists of all survey respondents who received a positive asylum decision before the legally defined cut-off date.¹² Observations with a positive decision thereafter are considered to be post-treatment observations.

To define treatment and comparison group, I exploit regional variation in statutory requirements across states. Based on §12a of the Integration Act, states may impose further restrictions on the initial placement of humanitarian migrants. In this case, beneficiaries of protection will face mobility restrictions *within* states, in addition to being unable to move *between* states. Baden Württemberg, Bavaria, North Rhine-Westphalia, Saarland, and Saxony-Anhalt have passed additional laws imposing severe limitations on residence decisions of humanitarian migrants. Consequently, I consider observational units in more restrictive states to be treated and all others to be comparison units.

Figure A4 illustrates which states impose stricter statutory regulations on initial place of residence as well as the precise date of instruction of the *residence rule* in each state. I use these sources of exogenous variation to estimate a DiD model that can be defined as follows:

$$Y_{it} = \alpha_0 + \alpha_1 Treat_i + \alpha_2 Per_t + \alpha_3 Treat_i Per_t + X'_{it} \gamma + \epsilon_{it}, \quad (1)$$

¹²For NRW: January 2016; for all other states: August 2016.

where Y_{it} is the outcome of interest measured in the year of the asylum decision measured for treatment status $i \in (0,1)$ in period $t \in (2015, 2016\text{-pre}, 2016\text{-post}, 2017)$. To account for the fact that socio-economic characteristics are measured in the post-treatment period, I estimate two alternative specifications: While specification i abstains from the inclusion of control variables, specification ii includes dummy variables for a respondent's sex, being married, having at least one child, and wanting to stay in Germany permanently. Furthermore, I control for age, age squared, months since arrival, months since asylum decision, as well as years of schooling (before immigration), interaction terms of being female and having children and being female and being married, and country of origin and state fixed effects. The dummy variable $Treat_i \in (0, 1)$ is equal to 1 if an individual lives in a treated state, $Per_t \in (0, 1)$ equals 1 if a decision has been made in the post-treatment period. ϵ_{it} is a mean zero term. Standard errors are clustered at the state level to allow for serial autocorrelation within federal states. Since the small number of clusters ($G = 16$) may bias standard errors downward, I also review the results using wild cluster bootstrap t -procedures (Cameron et al., 2008).

In this setting, α_3 captures the causal effect of interest. Since the regression model provides the reduced-form estimates of stricter statutory requirements on all humanitarian migrants who reside in the treatment area, I will interpret α_3 as an intention-to-treat effect (ITT).

Identifying assumptions. — Figure 1 compares outcome variables for treatment and comparison individuals disaggregated at a quarterly level and contrasts their evolution over time. If graphs develop similarly prior to the reform - that is, if individuals in treatment and comparison states are indeed equally likely to complete an integration course before the Integration Act was introduced - this provides graphical evidence that the *common trend assumption* is met. Differences in the post-treatment period may then be attributable solely to the policy change. While the graphs clearly support common time trends for the probability to complete an integration course in the year of the asylum decision and respective language

proficiency levels, graphical evidence on the probability to begin an integration course is much weaker. Especially in early 2015, the probabilities of treated and comparison units differ significantly.

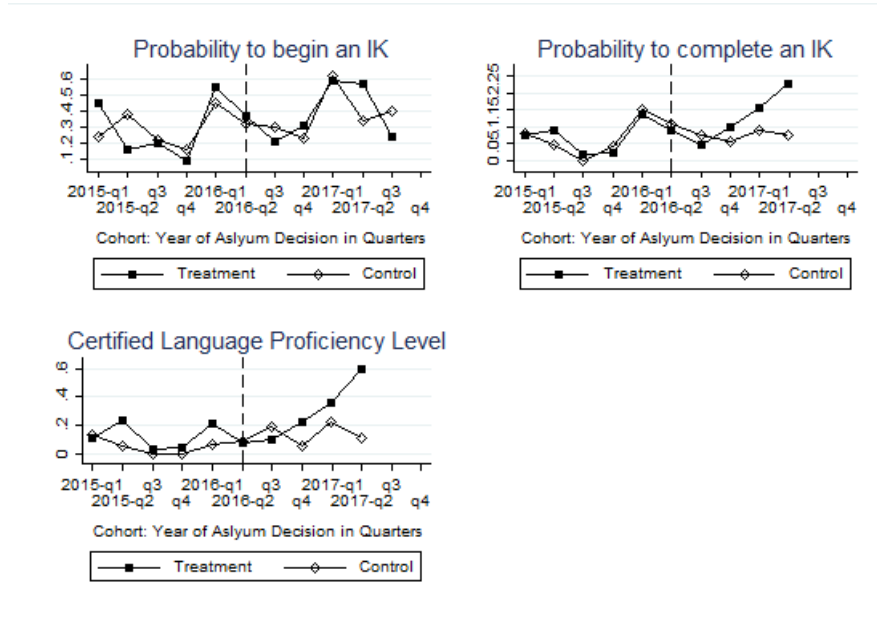


Figure 1: **Common Trend Assumption**

Notes: Data are from SOEP v33.2. Own computations.

To ”[rule] out behavioral changes of the treated that influence their pre-treatment outcome in anticipation of future treatment” (Lechner et al., 2011, p.178, ed. notes), I further perform a placebo test simulating that treatment occurred before the actual treatment period, in August 2015. If indeed there exists no effect on the pre-treatment population, this placebo test should consistently return a small and statistically insignificant effect.

Potential threats to identification. — Besides introducing the *residence rule*, policy-makers also suspended the *priority review* in summer of 2016.¹³

¹³*Priority review* is used by the Federal Employment Agency to evaluate whether there is a native comparable to the asylum seeker in the job search process (BMAS, 2017). Only if no German is available, can an asylum seeker be granted the job.

One might therefore worry about confounding the estimated effects with other reforms or changes taking place at the same time. Yet, since the two reforms target different groups of immigrants (*priority review*: asylum seekers; *residence rule*: humanitarian migrants), it is unlikely that the overlap in time will bias estimation results. I have not found any other significant reforms or policy changes that could be of concern for the results.

Due to the contentious public discussion over immigration in Germany at the time of the policy change, a second source of bias could arise if individuals had anticipated the reform. Even if this was the case, asylum seekers generally have no possibility to influence the timing of their asylum decision. What is more, due to the sudden influx of refugees to Germany in 2015 and 2016, many asylum seekers had to wait for a prolonged period of time until they received a final decision.¹⁴ As a result, anticipation effects are largely negligible.

Lastly, one might be concerned about selection into treatment. If treatment and control states exhibit different geographic, economic or political patterns, such differences may have led treatment states to impose additional regulations at the state level. Qualitative investigations show that treatment states justify imposition of additional legislation at the state level with arguments provided in §12a AufenthG.: placement restrictions at the district level may help humanitarian migrants to acquire suitable accommodations, to gain sufficient command of spoken German language, and to enter paid employment.¹⁵ Tables A6 and A7 provide quantitative evidence that treatment and comparison states share similar political and labor mar-

¹⁴Due to the massive influx of foreigners to Germany, waiting times on asylum decisions (measured in months from arrival in Germany) have increased steadily from 2015 to 2017. While asylum seekers in the working sample had to wait on average 6.3 months (SD 5.7) in 2015, this increased to 9.1 months (SD 5.5) in 2016 and 15.0 months (SD 4.6) in 2017.

¹⁵See also Minister of the Interior in Baden Württemberg Thomas Strobel's interview (<https://www.baden-wuerttemberg.de/de/service/presse/pressemitteilung/pid/wohnsitzaufgabe-wird-konsequent-umgesetzt-1/>, last downloaded on February 28, 2019) or the press release by Emilia Müller, former Minister of Employment and Social Order in Bavaria (<https://www.stmas.bayern.de/aktuelle-meldungen/pm1608-302.php>, last downloaded on February 28, 2019).

ket characteristics. Table A6 gives the proportion of second votes¹⁶ received by the major political parties by states and voter turnout of parliamentary elections in Germany in 2013 and 2017. The table shows that, on average, there are only marginal differences between treated and comparison states. For example, while in treatment states turnout was 75.3 percent in 2017, comparison states had a voter turnout of 74.9 percent on average. Besides a slightly higher share of second votes for the CDU/CSU, differences between treatment and comparison states are not statistically significant. What is more, the choice to apply additional legislation at state level did not depend on the political party in office. Table A7 shows additionally that while treated states tend to be slightly more economically successful, none of these differences are statistically significant. Likewise, treatment and comparison states exhibit a similar share of asylum seekers receiving government benefits relative to the overall state population. These comparisons hence suggest that treatment and comparison states share similar political and labor market characteristics, rebutting the suspicion of selection into treatment.

5 Data and Descriptive Statistics

The empirical analysis relies on the IAB-BAMF-SOEP Survey of Refugees which is conducted as part of the German Socio-Economic Panel (SOEP) (Göbel et al., 2018). This data-set provides information on refugees who have arrived in Germany since 2013, and is carried out in 2016, 2017, and 2018 (Brücker et al., 2016; DIW, 2017). It collects comprehensive information on individual refugees' migration, education, and employment histories as well as detailed information on their participation in language courses and overall living conditions in Germany (Figure A5). To date, the

¹⁶While the first vote represents the choice for a candidate in the constituency, the second vote is cast for a party's list of all candidates. For further information on the German electoral system, please check https://www.bundeswahlleiter.de/en/info/presse/mitteilungen/bundestagswahl-2017/20_17_zwei-stimmen.html, last downloaded on April 25, 2019.

IAB-BAMF-SOEP Survey of Refugees is the only data-base that allows for quantitative and empirical social research on refugees who were part of the most recent arrival cohort in Europe.¹⁷

Even though participation is voluntary, the IAB-BAMF-SOEP Survey of Refugees features an overall response rate of 48.7 percent, which is "an excellent response rate compared to other sub-samples in the SOEP" (Kroh et al., 2017, p.14). The first survey wave from 2016 (samples M3, M4) covers more than 4000 adult refugees in Germany. Sample M5, an add-on sample first surveyed in 2017, increases the number of observations by approximately 2000. Pooling these samples results in a total of 6662 observations.

Using this data source has several distinct advantages: First, the IAB-BAMF-SOEP Survey of Refugees maintains detailed information on residence status and the exact date when refugees received a residence permit.¹⁸ It is also possible to extract information on the current place of residence at the district level. Consequently, I can identify treatment and control group in the pre- and post-treatment period. Secondly, the IAB-BAMF-SOEP Survey of Refugees surveys individuals on a household level. Hence, it is possible to deduce information on respondent's family characteristics, such as children's educational or vocational training status, and to identify spouses living in the same household.

Definition of Working Sample. — I defined a working sample subject to several restrictions (Table 1). Since the reform applies to first-time asylum seekers only, I dropped observations that had submitted repeated asylum applications. I further kept only those observations that provided valid information on the type of residence permit and timing of the asylum application. This reduced the sample size quite substantially.¹⁹ Due to

¹⁷ Kroh et al. (2016, p.6) underscore that the results are representative of the target population if design weighting procedures are applied. For my empirical analysis, I abstained from using design weighting procedures, because the composition of the working sample differs slightly from overall refugee population in Germany (Table A5). Results are rather robust to including survey weights, however, and are available upon request.

¹⁸For clarification, Figure A6 in the appendix illustrates the exact wording and answer categories of the relevant survey questions.

¹⁹The IAB-BAMF-SOEP Survey of Refugees oversamples asylum seekers whose claim is still pending. Since those observations lack a decision date, I lose approximately 30

the limited sample size, I discarded all observations that had received a decision on their asylum claim before 2015 as well as individuals who were older than 67. Finally, I restricted the working sample to have non-missing values for all explanatory variables employed in the DiD analysis.

Table 1: **Definition of Working Sample**

Original Sample of Survey Respondents: M3, M4, M5	6662
Repeated asylum request	-124
Missing: type of permit/timing of decision	-1901
Exemption rule	-2772
Asylum decision before 2015/Older than 67	-119
Missing values in explanatory variables	-227
Missing information on spouse	-69
N	1450

Notes: Data are from SOEP v33.2. Own computations.

Based on the survey design, I created an indicator variable that takes a value of one if a survey respondent is not subject to the *residence rule* based on his or her spouse’s characteristics. The exemption rule does also apply, however, if the spouse is living in a different household in Germany. Because these cases cannot be identified in the data, the empirical analysis excludes individuals who claim to be married but whose spouse is living in a different household in Germany. Sixty-nine observations were therefore dropped from the sample.

Taking these considerations into account, the defined working sample consists of 1450 observations: 804 treated and 649 control individuals.

Descriptive Statistics. — Table 2 presents descriptive statistics. Column 1 provides information on the control group, column 2 then adds information on the treatment group.

Overall, the treatment and control group are very similar in socio-economic characteristics. To begin with, the two groups barely differ with

percent of the sample. What is more, the *residence rule* does not apply if certain criteria are met (Section 3). Consequently, I discarded individuals who satisfied the exemption rule based on personal but also based on their family’s characteristics. The vast majority of humanitarian migrants who are exempted from the *residence rule* in the SOEP have children in compulsory schooling (2300/2770 observations).

respect to observable characteristics such as age, age at migration, years of schooling, as well as family status. Further, I find little differences in gender and the probability to have children.

Table 2: **Descriptive Statistics**

	Controls		Treated	
	mean	SD	mean	SD
Female	0.33		0.30	
Age	30.30	10.00	31.49	11.16
Age at Migration	27.55	10.51	28.89	11.37
Dummy: Children	0.49		0.48	
Number of Children	1.31	1.93	1.43	2.09
Years of Schooling	9.26	4.12	9.12	4.10
Experience	0.70		0.69	
Future Employment	3.69	0.68	3.69	0.69
Active Search for Employment (4 weeks)	0.23		0.24	
Months since Arrival	18.52	6.32	18.09	5.91
Months since Asylum Decision	8.73	4.65	9.31	5.32
Some German before immigration	0.02		0.02	
Want to stay in Germany	0.94		0.93	
Exposure to Germans	3.40	1.94	3.28	1.84
UE Rate at regional level	7.66	2.79	7.08	3.26
<i>Family Status</i>				
Single	39.94		38.56	
Married	55.73		57.96	
Divorced	2.79		1.49	
Widowed	1.55		1.99	
<i>Group of Countries</i>				
[1] Syria, Iraque, Iran	75.85		87.94	
[2] Afghanistan, Pakistan, India	6.50		2.86	
[3] Africa	13.31		7.09	
[4] Western Balkan, former SU	0.15		0.25	
[5] Others	4.18		1.87	
<i>Type of residence permit</i>				
Political Asylum	9.13		8.08	
GFK	88.85		90.3	
§22 or §23	2.01		1.62	
N	646		804	

Notes: * 10 %, ** 5 %, *** 1 % significance level. Data are from SOEP v33.2. Own computations.

What is more, treatment and control groups are remarkably similar in various forms of labor market attachment: For example, 23 (24) percent of comparison (treated) individuals state that they have been actively looking for a job in the last four weeks. Further, if asked about planned future labor force participation on a scale from 1 to 4 (1 "No, definitely not", 2 "Unlikely", 3 "Probably", 4 "Definitely"), both groups show a strong preference for future employment.

Lastly, descriptive statistics suggest that there exists some variation with respect to country of origin. For example, while 76 percent of the comparison group originally stem from Syria, Iraq, or Iran, 88 percent of treated individuals report those as their countries of origin. On the other hand, 7 (3) percent of comparison (treated) individuals list Afghanistan, Pakistan, or India as country of origin. One reason for this disparity is that in order to reduce processing times, some regional offices of the Federal Office for Migration and Refugees (BAMF) have specialized in particular countries of origin ([Organization for Economic Co-operation and Development, 2017](#), p.48).²⁰ Consider, nevertheless, that refugees have no say in where they are placed during the asylum procedure but are assigned to a particular state based on the *Königstein key*. The DiD estimator may also explicitly control for these differences as long as the differences are constant over time.

Repeated Cross-Section. — The empirical analysis pools information on samples M3 and M4 (2016), as well as sample M5 (2017) and, therefore, relies on a single cross-section. Yet, the data can also be artificially split into repeated cross-sections by forming *cohorts* based on the year of an individual's asylum decision.

The working sample consists of humanitarian migrants who received a positive asylum status in the years of 2015, 2016, and 2017. As the *residence rule* was introduced in summer 2016, I further distinguish between individuals who received their decision before and after the reform was

²⁰While this may indeed cause clustering of nationalities to a certain extent, none of the countries of origin in the working sample were processed exclusively in treatment or control states.

introduced. Thus, I can differentiate between four cohorts: 2015, 2016-pre, 2016-post, and 2017.

Further, because interviews were conducted from July 2016 onwards, it was essential to reformulate outcome variables such that they represent outcomes at the time of the asylum decision. Fortunately, the data-set provides rich information on language course histories since year of the arrival in Germany. This made it possible to extract information on the outcome variables in the year of the asylum decision.

6 Results and Sensitivity Analyses

Main Results. — Tables 3 and 4 report estimation results based on equation (1) for the respective outcome variables.²¹

Table 3: **Effect on Completing an IK**

	Baseline	Male	W/o NRW	W/o City States	Placebo
	[1]	[2]	[3]	[4]	[5]
mean	0.086	0.924	0.84	0.085	0.086
[i]	0.080***	0.110***	0.074***	0.081***	0.023
	0.018	0.029	0.019	0.019	0.043
Obs.	1450	995	1372	1356	1450
R^2	0.006	0.013	0.005	0.007	0.000
[ii]	0.070***	0.093***	0.062***	0.073***	0.029
	0.018	0.030	0.021	0.018	0.048
Obs.	1450	995	1372	1356	1450
R^2	0.046	0.047	0.047	0.048	0.043

Notes: Standard errors are clustered at the state level and given in parentheses.

* 10 %, ** 5 %, *** 1 % significance level. Data are from SOEP v33.2. Own computations.

Table 3 illustrates that living in a state that enforces the *residence rule* more strictly increased the probability of completing an integration course in the year of asylum decision by approximately 7 percentage points. To be precise, estimation values range between 7.0 and 8.0 percentage points and are statistically significant at the one percent level. This is a very large

²¹I refrain from reporting estimation results on *Beginning an integration course*, since the common trend assumption is clearly violated. Further, to account for selection bias, I report two alternative specifications: (i) without a set of controls and (ii) including a full set of covariates.

effect considering that the mean of the outcome variable is 8.6 percent.

While placement restrictions may be less severe in city-states, the results are remarkably similar if these are excluded (column 4). Furthermore, for a subsample that includes male refugees only, the effect is even more pronounced: Column 2 indicates that male refugees in a high treatment intensity state are 9.3 to 11.0 percentage points more likely to conclude a language course in the year of asylum decision, relative to a mean of 9.24 percent. This is an increase of around 100 percent.

Section 4 underscores that North Rhine-Westphalia is the only state that insisted on the legally defined cut-off date, January 1, 2016. The reform itself was announced in July 2016 with retrospective effect. Thus, some humanitarian migrants - who had received a permanent residence permit in this particular period and moved to North Rhine-Westphalia - might be forced to return to a different state. This may take time and effort. Hence, as a sensitivity analysis, I exclude all observations that received a positive asylum decision from January to July, 2016, and live in North Rhine-Westphalia. If these observations are excluded, estimate values are still in the same range and are highly statistically significant (column 3).

To validate the assumption that treatment and control units would have followed similar paths in the absence of the reform, I performed a placebo test simulating that the reform took effect one year earlier, in August 2015. Column 5 shows that this placebo test yields insignificant estimates for all specifications. What is more, the estimates are generally much smaller.

Table 4 provides evidence on the reform's effect on refugees' certified level of language proficiency. Overall, the results show a very similar pattern. Living in a federal state that applies stricter rules on initial place of residence increases certified language proficiency levels by 0.132 to 0.161 units in the baseline sample, relative to a mean of 0.143. This effect is statistically significant at the one percent level. For a male subsample as well as a sample without city-states, estimates are even slightly larger. Similarly to Table 3, the placebo test consistently returns statistically insignificant effects.

Table 4: **Effect on Language Proficiency Levels**

	Baseline	Male	W/o NRW	W/o City States	Placebo
	[1]	[2]	[3]	[4]	[5]
mean	0.143	0.149	0.136	0.144	0.143
[i]	0.161***	0.203***	0.157***	0.166***	0.097
	0.038	0.042	0.042	0.041	0.059
Obs.	1450	995	1372	1356	1450
R^2	0.016	0.028	0.015	0.017	0.004
[ii]	0.132***	0.164***	0.117***	0.137***	0.118
	0.035	0.043	0.039	0.040	0.079
Obs.	1450	995	1372	1356	1450
R^2	0.077	0.070	0.075	0.075	0.075

Notes: Standard errors are clustered at the state level and given in parentheses.

* 10 %, ** 5 %, *** 1 % significance level. Data are from SOEP v33.2. Own computations.

Robustness Analyses. — Besides integration courses, there are many other language learning programs available in Germany, such as ESF-BAMF courses, entry-level German courses offered by the German Federal Employment Agency and other courses as well. In a final step, I therefore analyzed the effect of stricter placement restrictions on participation in *any* language course and acquired language proficiency levels within the year of the asylum decision.²² Results are presented in Table A8. Overall, estimates indicate that living in a high treatment intensity state increases the probability to participate in any language course as well as respective language proficiency levels. For example, living in a treatment state increases the probability to complete a language course by 7.2 to 8.1 percentage points. However, since mean values are generally much larger, the relative effect sizes decrease substantially. What is more, effects are not as robust to different sample specifications and/or lose significance if the respective control variables are included.

Cluster-robust inference is based on the assumption that the number of clusters goes to infinity. [Cameron et al. \(2008, p.414, ed. notes\)](#) show that “[with] a small number of clusters, the cluster-robust standard errors are

²²Please note that in this context, the IAB-SOEP-BAMF Survey of Refugees captures respective language proficiency levels on a scale from 0 to 6 (rather than 0 to 3), with 0 “No certified level”, 1 “Level A1”, 2 “Level A2”, 3 “Level B1”, 4 “Level B2”, 5 “Level C1”, and 6 “Level C2”.

downward biased.” The authors suggest employing cluster bootstrap-t procedures which provide asymptotic refinement. To account for the limited number of clusters in the German context ($G=16$), I follow the authors’ recommendation and apply wild cluster bootstrap as a robustness check (Cameron et al., 2008, p.416ff). Hence, I generated pseudo-samples of the original regression’s residuals using what are known as *Rademacher weights*. Subsequently, I reestimated the regression equation based on the created pseudo-samples, while keeping the vector of control variables constant. In this setting, I tested the null of no treatment effect.

The results of this estimation exercise confirm the findings above. Using a total of 999 replications, the wild cluster bootstrap procedure produces a p-value of $p = 0.006$ for the probability to complete an integration course in the year of the asylum decision.²³ While this p-value is slightly larger than the one from original cluster-robust inference, it provides statistically significant evidence that living in a state that applies the *residence rule* more strictly has a positive effect on the probability to complete an integration course. Similar findings hold for the certified level of language proficiency: Here, bootstrapping procedures provide a p-value of $p = 0.016$.

Finally, and as a further robustness analysis, I ran a simulation exercise. I randomly assigned a higher treatment intensity to federal states as a first step and ran the regression analysis using the placebo treatment indicator. I repeated this procedure 10000 times and saved the estimation coefficients in an additional data-set. Figure A7 displays these placebo coefficients in a histogram, while the ITT coefficient (Cf. equation (1)) is depicted by a red, vertical line. The graph clearly illustrates that estimates are centered around zero and are approximately normally distributed. What is more, for the probability to complete an integration course (the respective language proficiency levels), 97.55 (96.36) percent of the estimates lie below the estimated treatment effect. This suggests that while estimated effects are rather large in percentage terms, they are not unreasonably high. Furthermore, the graph provides some evidence that the effect is not driven by

²³The results reported are based on specification *ii* and the baseline sample.

a single state alone but by all the treatment states.

To conclude, the estimation results demonstrate that higher treatment intensity has indeed had a positive and statistically significant effect on the probability to complete an integration course and achieved certified language proficiency level at the end of the course. This provides evidence that urging humanitarian migrants to stay in a particular district facilitates the planning and allocation of integration measures. This is particularly relevant considering that higher proficiency in the host country language improves labor market access and is a powerful indicator of prospective labor market outcomes.

7 Potential Mechanisms

There exists extensive literature on the importance of social networks for immigrant assimilation (Borjas, 2000; Bertrand et al., 2000). It is theoretically ambiguous, however, what effect a change in the size of an ethnic enclave could have. For example, while a smaller network of co-nationals in a given area may increase an immigrant’s incentives to participate in language courses to learn the host country language (Lazear, 1999), immigrants in such areas may be less informed about potential course offerings (Bertrand et al., 2000). Hence, the effect of network size on refugees’ language abilities is not clear a priori.

To analyze whether *network effects* are at play, I exploited survey information on the level of exposure to people from the same country of origin, who are not related to the interviewee.²⁴ I investigated the mean difference of this variable across treatment groups using a two-sided t-test and examined whether differences are statistically distinct. I found that the level of exposure is similarly distributed across treatment groups and the

²⁴The respective survey question states *How often do you spend time with people from your country of origin who are not related to you?*: 1 "Every day", 2 "Several times per week", 3 "Every week", 4 "Every month", 5 "Less often", 6 "Never". I define humanitarian migrants to have a high exposure to co-nationals if they spend time with non-related co-nationals at least once a week.

differences are never statistically significant. What is more, including the network variable in the regression framework does not change estimation results significantly (Table A9, column 2).

To further examine networking effects based on ethnic enclaves (Borjas, 1998, 2000; Damm, 2009; Edin et al., 2003, 2004), I used additional data from the Federal Statistical Office, the Central Register of Foreigners (Ausländerzentralregister), which lists the most frequent foreign nationalities at a district level, and merged this with my working sample. Again, I performed a two-sided t-test to compare average values across groups. The results suggest that humanitarian migrants in treated states are placed in districts with a lower share of co-nationals. On average, only 8 percent of humanitarian migrants reside in districts where their nationality is the most frequent foreign nationality, while this holds true for 17 percent of humanitarian migrants in comparison states. This provides some evidence that high treatment intensity states tend to avoid ethnic clustering.

Assigning humanitarian migrants at district level may help to anticipate the demand for integration courses at a regional level and adjust supply accordingly. What is more, refugees who live in states that abstain from applying the *residence rule* more rigorously may lack information on integration courses or face higher commuting costs. Hence, another important channel could be *spatial mismatch* (Kain, 1968).

Each year the BAMF publishes a statistical report on integration courses (*Integrationskursgeschäftsstatistik*), which lists the total number of integration courses begun, the total number of courses completed, as well as the number of course graduates in each district of Germany. This external data base provides exact information on *local access* to integration courses. While the average numbers by district in treated and comparison states are quite similar, Figure A8 illustrates that districts only began to adjust the supply of integration courses starting in 2015.

I merged this information with my working sample using official district codes and the year of the asylum decision. As a first step, I compared the means of these three proxy variables deflated by the share of foreign-

ers versus natives in a district across groups. I can therefore relate these measures to the size of the relevant population in each district. The results show that the supply of integration measures is higher in treatment than in comparison states. For example, while on average 35 people per district completed an integration course in comparison states, there were approximately 43 course graduates in treatment states. This difference is statistically significant at the one percent level.

Subsequently, I included the deflated proxy variables in the regression framework (Table A10). While column 1 replicates the original estimates, columns 2 to 4 include each proxy individually. Column 5 then includes the number of integration courses begun, the number of integration courses completed, and the number of course graduates per district in the year of the asylum decision. I find that once supply-side differences are taken into account, estimates decrease by approximately 25 to 35 percent. This effect is most pronounced for certified language proficiency levels; some specifications produce statistically insignificant estimates.²⁵

These findings suggest that spatial mismatch of integration courses in non-treated states is by far a more important driver than potential networking effects. States that apply the residence rule more rigorously can assess the demand for integration measures clearly and provide integration courses whenever needed. Consequently, the *residence rule* serves as a valid tool to match supply and demand for integration courses effectively.

8 Conclusion

There is a contentious debate underway in Europe about how to integrate immigrants best and most quickly into their host societies. These questions, which have taken on added urgency with the recent arrival of large

²⁵Unfortunately, the IAB-BAMF-SOEP Survey of Refugees does not provide a history of moving patterns since arrival in Germany, but only reports the current place of residence and the district in which they were housed the longest in Germany. Because the data lack a history of moves, I cannot verify that humanitarian migrants still live in the district in which they attended the integration course. This should be kept in mind when interpreting estimation results.

numbers of refugees, have led several European countries to change their legislation to enhance integration measures and allow for targeted integration of immigrants into local labor markets. Germany's Integration Act is one such policy measure.

In this paper, I used an innovative, albeit small, survey data-set to address a topic of high policy relevance. As such, it provides initial quantitative and empirical evidence on the short-term effects of tight placement restrictions on refugees' participation in integration courses and language outcomes. I exploited different treatment intensities across states that resulted from the *residence rule* introduced in Germany in August of 2016 to yield a causal effect. The robust estimation results demonstrate that living in a state with strict placement policies has a positive and statistically significant effect on the probability to complete an integration course and on certified language proficiency levels. I further find suggestive evidence that states with stricter residence rules place humanitarian migrants in districts with a larger supply of integration courses. Hence, after controlling for the supply of integration courses at a district level, the estimates are smaller and in parts statistically insignificant.

It should be noted that this analysis is restricted to short-run effects and that results may differ in the medium or long run. For example, if humanitarian migrants in less restrictive states lack information on integration courses nearby, they may catch up eventually. Furthermore, it would be highly interesting to understand how the restrictions studied affect individual behavior in areas such as job search, employment and earnings. This is particularly relevant because treatment may increase incentives to meet the exemption rule. However, since the vast majority of the sample is not yet working, precision is too poor to be analyzed at a current stage. Hence, future research on this topic is needed to formulate more detailed policy recommendations.

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Appendix

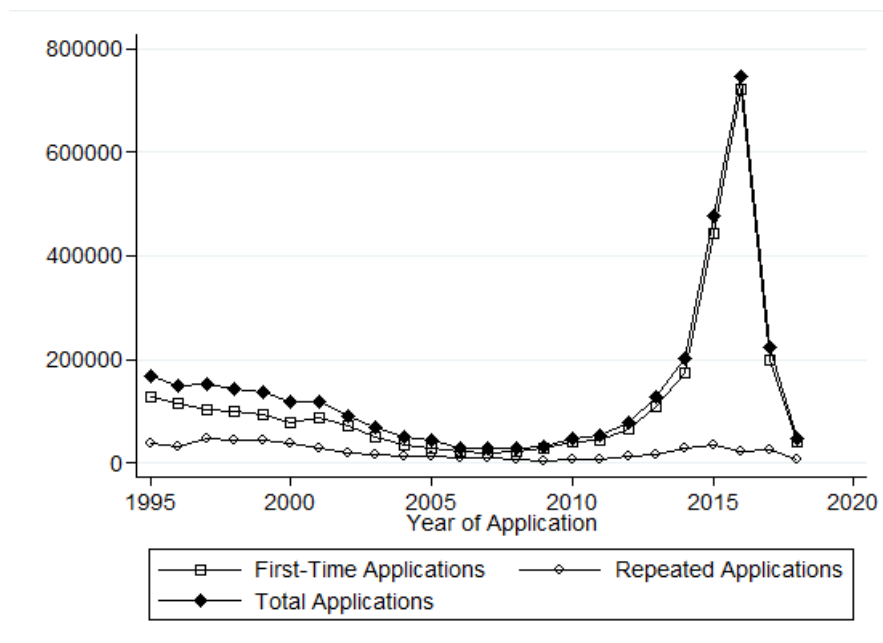


Figure A1: **Number of Asylum Applications**

Notes: This figure provides the number of asylum applications in Germany from 1995 to 2018 (data for 2018 includes the first quarter only). Data are from [BAMF \(2018\)](#). Own illustration.

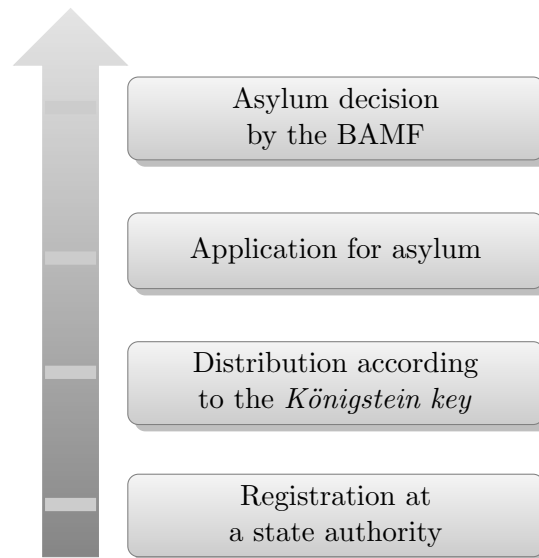


Figure A2: **Stages of the Asylum procedure**
Notes: Illustration based on [BAMF \(2016b\)](#).

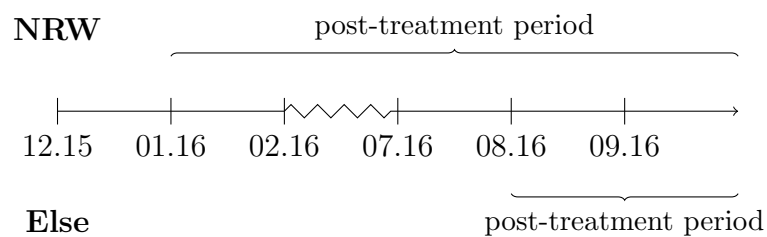


Figure A3: **Timeline of the *Residence Rule***
Notes: Illustration based on [Ministerium für Inneres und Bundesangelegenheiten, Schleswig-Holstein \(2016\)](#).

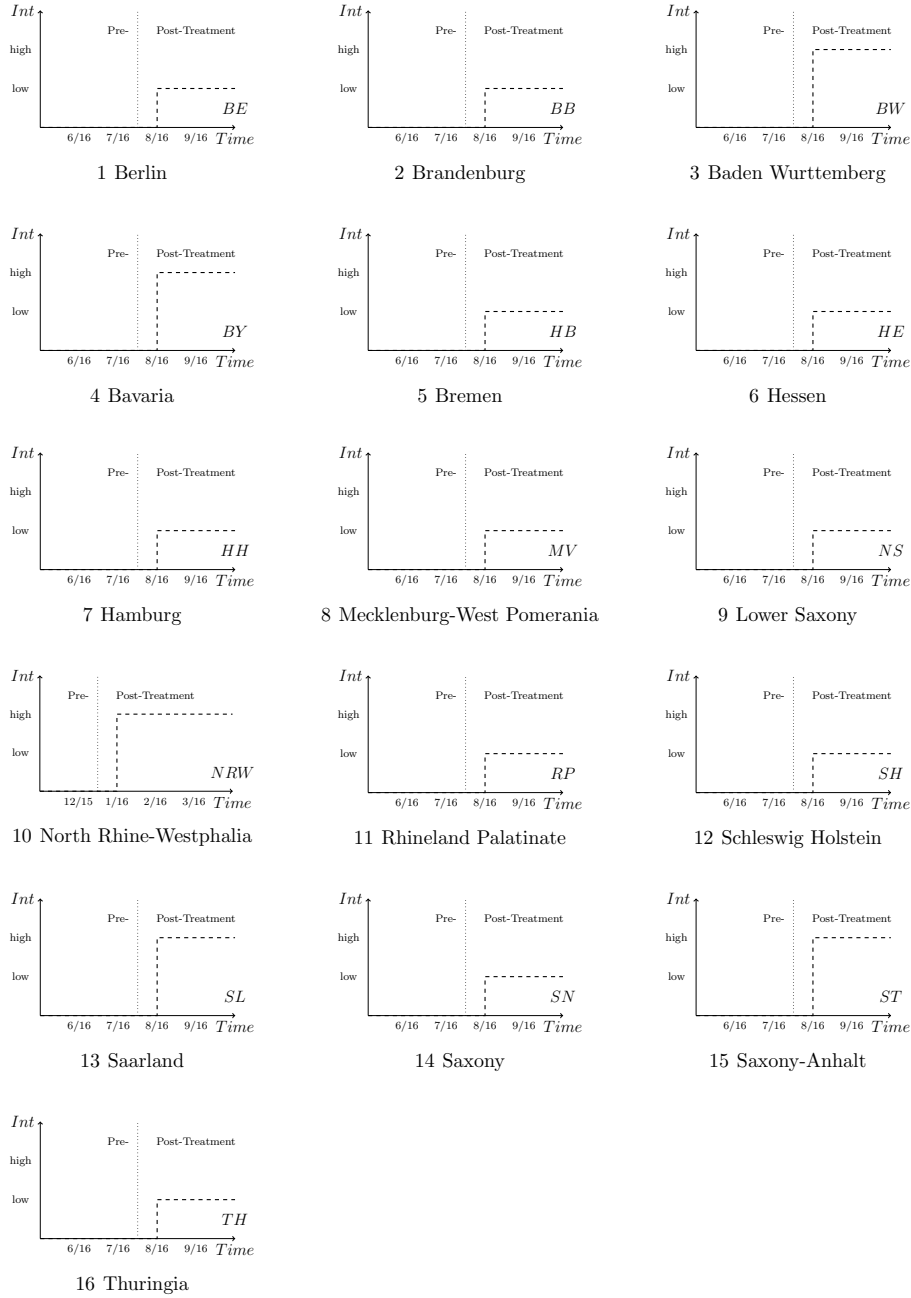


Figure A4: **Treatment Intensity across States**

Notes: Illustration based on [Ministerium für Inneres und Bundesangelegenheiten, Schleswig-Holstein \(2016\)](#); [Bayerische Staatsregierung \(2016\)](#); [Ministerium für Inneres, Digitalisierung und Migration, Baden-Württemberg \(2016\)](#); [Ministerium für Inneres und Sport, Sachsen-Anhalt \(2017\)](#); [Landesregierung Nordrhein-Westphalen \(2016\)](#); [Staatskanzlei Saarland \(2016\)](#).

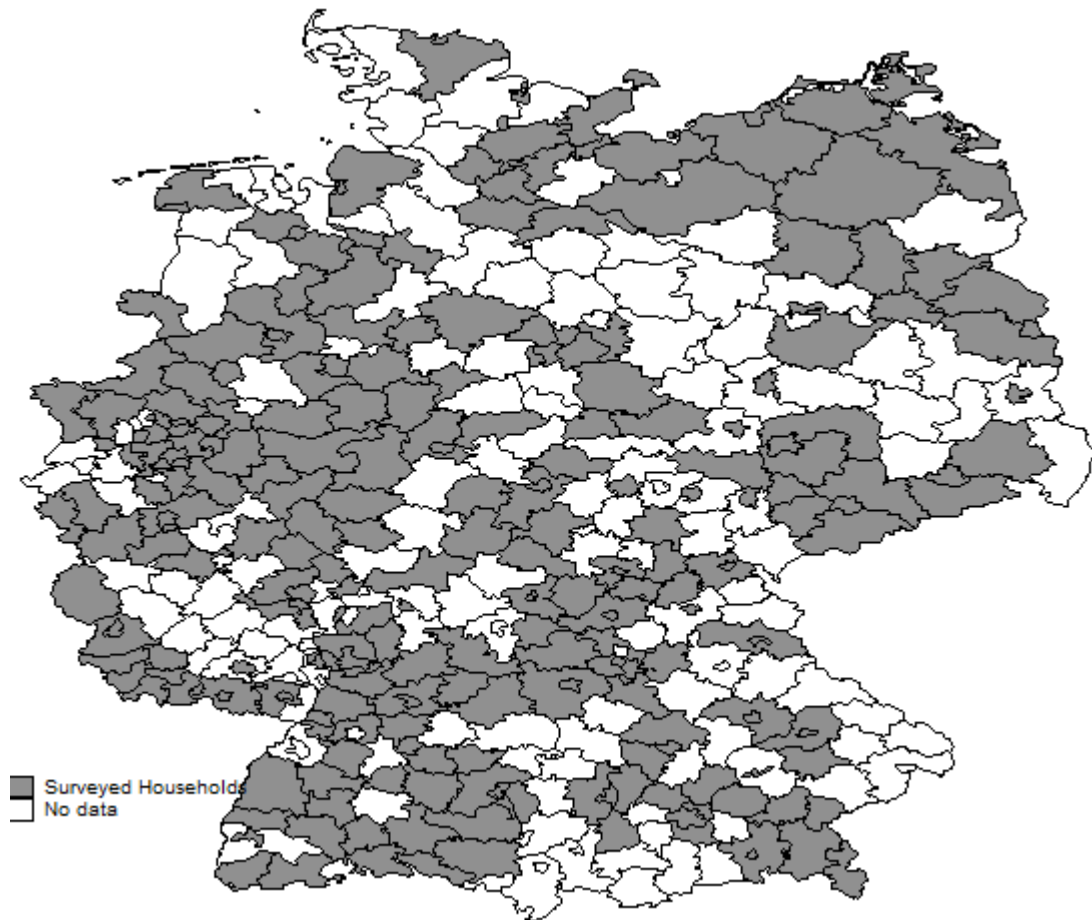


Figure A5: Geographical Distribution of Households surveyed in the IAB-BAMF-SOEP Survey of Refugees

Notes: Data are from SOEP v33.2. Own computations.

46 When was the decision regarding your application for asylum made?

Year

Month

No details

1 Timing of the asylum decision

50 Which residence title do you currently hold? If you are not sure which residence title you hold, please check the Immigration Authority's label in your passport.

Permission to stay pursuant to Section 55 of the German Asylum Law (asylum seekers)

A residence permit according to Section 25 sub-section 1 of the German Residence Act (persons entitled to asylum)

A residence permit according to Section 25 sub-section 2 of the German Residence Act (persons with refugee status)

A settlement permit according to Section 26 sub-section 3 of the German Residence Act

A temporary suspension of deportation according to section 60a of the German Residence Act

A residence permit according to Section 22 or Section 23 of the German Residence Act (admission on humanitarian grounds)

A residence permit pursuant to § 23a or § 25 sub-section 3, 4 or 5 of the German Residence Act (admission on other humanitarian grounds)

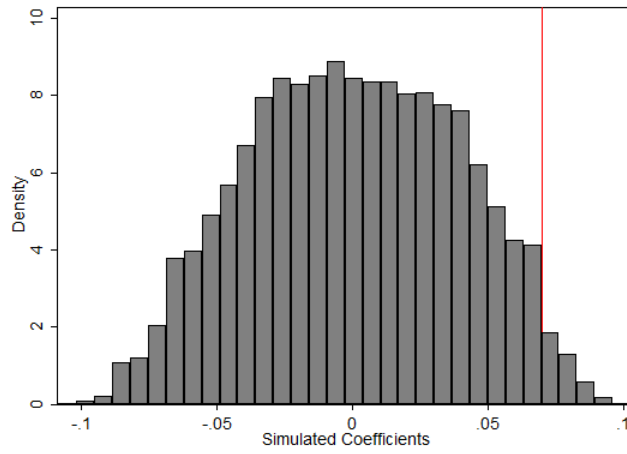
Another residence title

No details

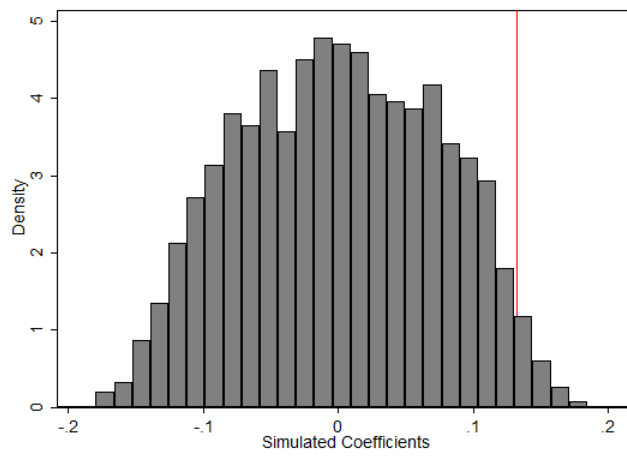
2 Type of residence permit

Figure A6: **Information on Residence Status and Timing of Residence Permit**

Notes: Survey questions and answers are taken from IAB-BAMF-SOEP Sample of Refugees in Germany: Personal Questionnaire 2016, Section B007 *Legal Status* (IAB-BAMF-SOEP, 2016, p.32ff).



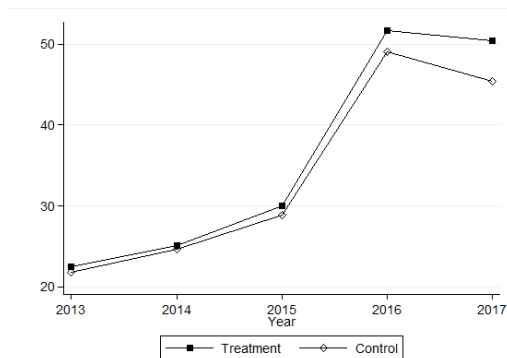
1 Completing an IK



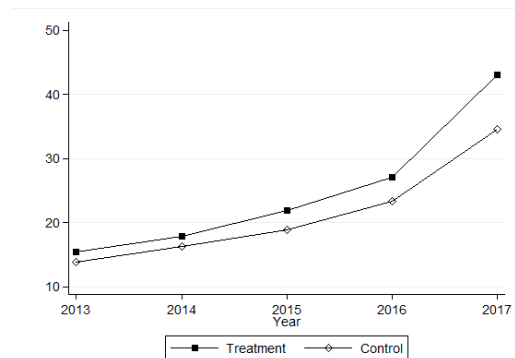
2 Certified Language Proficiency Level

Figure A7: **Placebo Test**

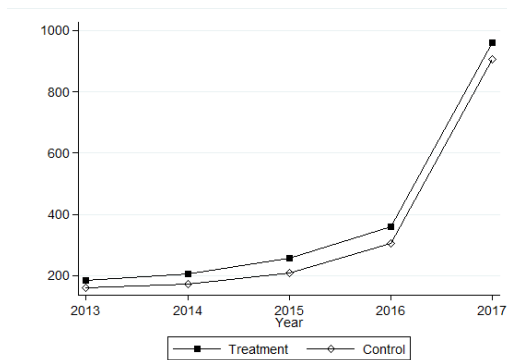
Notes: Data are from SOEP v33.2. Own computations.



1 Average number of courses begun



2 Average number of courses completed



3 Average number of course graduates

Figure A8: **Supply-side Indicators of Integration Courses**
 Notes: Data are from *Integrationskursgeschäftsstatistik* (2013-2017) and SOEP v33.2. Own computations.

Table A1: Königsstein Key, 2010 - 2017

	2010	2011	2012	2013	2014	2015	2016	2017
Baden Württemberg	0.128	0.128	0.129	0.130	0.130	0.129	0.130	0.130
Bavaria	0.151	0.152	0.152	0.153	0.153	0.155	0.155	0.156
Berlin	0.050	0.050	0.051	0.050	0.050	0.050	0.051	0.051
Brandenburg	0.031	0.031	0.031	0.031	0.031	0.031	0.030	0.030
Bremen	0.009	0.009	0.009	0.009	0.009	0.010	0.010	0.010
Hamburg	0.026	0.025	0.026	0.025	0.025	0.025	0.026	0.026
Hessen	0.072	0.072	0.073	0.073	0.073	0.074	0.074	0.074
Lower Saxony	0.093	0.093	0.094	0.094	0.094	0.093	0.093	0.094
Mecklenburg-West Pomerania	0.021	0.021	0.021	0.021	0.020	0.020	0.020	0.020
North Rhein-Westphalia	0.213	0.214	0.212	0.212	0.212	0.212	0.211	0.211
Rhineland Palatine	0.048	0.048	0.048	0.048	0.048	0.048	0.048	0.048
Saarland	0.012	0.012	0.012	0.012	0.012	0.012	0.012	0.012
Saxony	0.052	0.052	0.051	0.051	0.051	0.051	0.051	0.050
Saxony-Anhalt	0.030	0.029	0.029	0.029	0.029	0.028	0.028	0.028
Schleswig Holstein	0.033	0.034	0.034	0.034	0.034	0.034	0.034	0.034
Thuringia	0.028	0.028	0.028	0.028	0.027	0.027	0.027	0.027

Notes: Figures in percent. <https://www.gwk-bonn.de/themen/finanzierung-von-wissenschaft-und-forschung/koenigsteiner-schluesel/>, last downloaded on August 28, 2018.

Table A2: Actual versus Percentage Share of Asylum Seekers per State, 2013 - 2017

2013		No. applications	In percent	Königstein Key	2014		No. applications	In percent	Königstein Key
Baden Württemberg		13421	12.2	13.0	Baden Württemberg		16482	9.5	13.0
Bavaria		16698	15.2	15.3	Bavaria		25667	14.9	15.3
Berlin		6113	5.6	5.0	Berlin		10375	6.0	5.0
Brandenburg		3058	2.8	3.1	Brandenburg		4906	2.8	3.1
Bremen		1109	1.0	0.9	Bremen		2222	1.3	0.9
Hamburg		3207	2.9	2.5	Hamburg		5702	3.3	2.5
Hessen		8129	7.4	7.3	Hessen		12536	7.2	7.3
Lower Saxony		10225	9.3	9.4	Lower Saxony		15416	8.9	9.4
Mecklenburg-West Pomerania		2303	2.1	2.1	Mecklenburg-West Pomerania		4418	2.6	2.0
North Rhine-Westphalia		23719	21.6	21.2	North Rhine-Westphalia		40046	23.1	21.2
Rhineland Palatine		5481	5.0	4.8	Rhineland Palatine		8716	5.0	4.8
Saarland		1219	1.1	1.2	Saarland		2564	1.5	1.2
Saxony		5040	4.6	5.1	Saxony		6030	3.5	5.1
Saxony-Anhalt		3195	2.9	2.9	Saxony-Anhalt		5978	3.5	2.9
Schleswig Holstein		3756	3.4	3.4	Schleswig Holstein		7032	4.1	3.4
Thuringia		2722	2.5	2.8	Thuringia		4867	2.8	2.7
Unknown		182	0.2	.	Unknown		112	0.1	.
2015		No. applications	In percent	Königstein Key	2016		No. applications	In percent	Königstein Key
Baden Württemberg		57578	13.0	12.9	Baden Württemberg		84610	11.7	13.0
Bavaria		67639	15.3	15.5	Bavaria		82003	11.4	15.5
Berlin		33281	7.5	5.0	Berlin		27247	3.8	5.1
Brandenburg		18661	4.2	3.1	Brandenburg		18112	2.5	3.0
Bremen		4689	1.1	1.0	Bremen		8771	1.2	1.0
Hamburg		12437	2.8	2.5	Hamburg		17512	2.4	2.6
Hessen		27239	6.2	7.4	Hessen		65520	9.1	7.4
Lower Saxony		34248	7.8	9.3	Lower Saxony		83.024	11.5	9.3
Mecklenburg-West Pomerania		18851	4.3	2.0	Mecklenburg-West Pomerania		7273	1.0	2.0
North Rhine-Westphalia		66758	15.1	21.2	North Rhine-Westphalia		196734	27.2	21.1
Rhineland Palatine		17625	4.0	4.8	Rhineland Palatine		36985	5.1	4.8
Saarland		10089	2.3	1.2	Saarland		6865	1.0	1.2
Saxony		27180	6.2	5.1	Saxony		23663	3.3	5.1
Saxony-Anhalt		16410	3.7	2.8	Saxony-Anhalt		19484	2.7	2.8
Schleswig Holstein		15572	3.5	3.4	Schleswig Holstein		28982	4.0	3.4
Thuringia		13455	3.0	2.7	Thuringia		154222	2.1	2.7
Unknown		187	0.0	.	Unknown		163	0.0	.
2017		No. applications	In percent	Königstein Key	2018		No. applications	In percent	Königstein Key
Baden Württemberg		21371	10.8	13.0	Baden Württemberg		21371	10.8	13.0
Bavaria		24243	12.2	15.6	Bavaria		24243	12.2	15.6
Berlin		9369	4.7	5.1	Berlin		9369	4.7	5.1
Brandenburg		5547	2.8	3.0	Brandenburg		5547	2.8	3.0
Bremen		2495	1.3	1.0	Bremen		2495	1.3	1.0
Hamburg		4664	2.4	2.6	Hamburg		4664	2.4	2.6
Hessen		14676	7.4	7.4	Hessen		14676	7.4	7.4
Lower Saxony		18861	9.5	9.4	Lower Saxony		18861	9.5	9.4
Mecklenburg-West Pomerania		3954	2.0	2.0	Mecklenburg-West Pomerania		3954	2.0	2.0
North Rhine-Westphalia		53343	26.9	21.1	North Rhine-Westphalia		53343	26.9	21.1
Rhineland Palatine		12951	6.5	4.8	Rhineland Palatine		12951	6.5	4.8
Saarland		3099	1.6	1.2	Saarland		3099	1.6	1.2
Saxony		7389	3.7	5.0	Saxony		7389	3.7	5.0
Saxony-Anhalt		5118	2.6	2.8	Saxony-Anhalt		5118	2.6	2.8
Schleswig Holstein		6084	3.1	3.4	Schleswig Holstein		6084	3.1	3.4
Thuringia		5040	2.5	2.7	Thuringia		5040	2.5	2.7
Unknown		113	0.1	.	Unknown		113	0.1	.

Notes: Presentation based on BAMF (2014, 2015b,a, 2016a, 2017).

Table A3: Distribution of Refugees within States

State	Responsible unit	Legal basis	Basis of the quota
Baden Württemberg	Municipalities	§18 Abs. 1 FlüAG	Population
Bavaria	Administrative Districts in coop. with municipalities	§§6 and 7 DVAsyl	Statutory order (fixed allocation key)
Berlin	Federal government		Subsequent accommodation of humanitarian migrants involves non-governmental actors.
Brandenburg	Municipalities	§6 Abs. 4 LAufnG	Population
Bremen	Municipalities Bremen and Bremerhaven	§§3 Abs. 3 AufnG Bremen	Statutory order (fixed allocation key)
Hamburg	Federal government		Behörde für Inneres und Sport is responsible for allocating the refugees across municipalities. No statutory order exists.
Hessen	Municipalities	§2 Abs. 1 LAufnG	Population
Mecklenburg-West Pomerania	Municipalities	§6 Abs. 1 ZwFlüAGDLVO M-V	Population
Lower Saxony	Municipalities	§1 Abs. 1 Satz 2 i. V. m. §2 Abs. 1 AufnG	Population
North Rhine-Westphalia	Municipalities	§1 Abs. 1 i. V. m. §3 Abs. 1 FlüAG	Population and area
Rhineland Palatine	Municipalities	§2 Abs. 1 Nr. 2 i. V. m. §6 Abs. 1 AufnG RP	Population
Saarland	Municipalities	§§1 and 2 LAG	Population
Saxony	Municipalities	§2 Abs. 1 u. 2 i. V. m. §6 Abs. 3 SachsFlüAG	Population
Saxony-Anhalt	Municipalities	§1 Abs. 1 u. 3 AufnG ST	Population
Schleswig Holstein	Municipalities	§1 Abs. 2 LAufnG i. V. m. §7 Abs. 1 AuslAufnVO	Population
Thuringia	Municipalities	§2 Abs. 1 ThrFlüVertVO	Statutory order (fixed allocation key)

Notes: Presentation based on respective state regulations.

Table A4: Categories of Protection in Germany

Category	Rate of Protection in %	Legal basis	Duration of residence permit	Details
1 Political asylum	0.70	<i>Grundgesetz</i> Art. 16a	3 years	Reexamination of protection grounds after 3 years; permanent residence permit after 5 years (<i>Niederlassungserlaubnis</i>).
2 Geneva Refugee Convention (GFK)	20.50	GFK § 3 AsylG	1951, 3 years	Reexamination of protection grounds after 3 years; permanent residence permit after 5 years (<i>Niederlassungserlaubnis</i>).
3 Subsidiary protection status	16.30	§4 AsylG	1 year	Protection status can be extended several times.
4 Foreigners with tolerated status	6.60	§60 AufenthaltG	1 year	Deportation restriction can be renewed several times; still, <i>foreigners with tolerated status</i> have to leave Germany eventually.
Rejections	55.90			If the asylum claim has been ultimately rejected, the individual is obliged to leave Germany.

Notes: Presentation based on SVR (2017, p.4) and BAMF (2018, p.10).

Table A5: Distribution across Age Groups, Gender, and Countries of origin

Target Population	Males				Females			
	younger than 30	aged 30 and above	younger than 30	aged 30 and above	younger than 30	aged 30 and above	younger than 30	aged 30 and above
Syria	18.26	12.60	4.97	5.70	41.52			
Afghanistan	5.41	1.79	1.44	1.14	9.77			
Iraq	3.93	2.00	1.30	1.12	8.34			
Albania, Serbia, Kosovo	1.53	1.75	1.18	1.42	5.88			
Eritrea, Somalia	4.14	0.92	1.24	0.32	6.62			
Iran, Pakistan	2.13	1.50	0.35	0.57	4.55			
Others	9.55	7.22	2.82	3.71	23.32			
					100.00			

Survey Population	Males				Females			
	younger than 30	aged 30 and above	younger than 30	aged 30 and above	younger than 30	aged 30 and above	younger than 30	aged 30 and above
Syria	14.81	12.58	4.69	6.29	38.38			
Afghanistan	8.06	3.68	2.33	1.83	15.89			
Iraq	3.75	2.91	1.32	1.58	9.56			
Albania, Serbia, Kosovo	1.09	0.93	0.36	0.50	2.88			
Eritrea, Somalia	3.98	1.17	1.26	0.43	6.84			
Iran, Pakistan	3.26	3.21	0.30	1.08	7.86			
Others	7.76	5.26	2.18	3.39	18.60			
					100.00			

Working Sample	Males				Females			
	younger than 30	aged 30 and above	younger than 30	aged 30 and above	younger than 30	aged 30 and above	younger than 30	aged 30 and above
Syria	31.74	23.73	7.47	6.93	69.87			
Afghanistan	1.80	0.90	0.88	0.48	4.06			
Iraq	3.63	2.25	1.21	0.98	8.08			
Albania, Serbia, Kosovo	0.00	0.00	0.06	0.00	0.06			
Eritrea, Somalia	5.53	1.94	1.63	0.33	9.43			
Iran, Pakistan	0.90	1.36	0.39	0.25	2.91			
Others	2.38	1.70	0.38	1.12	5.59			
					100.0			

Notes: Presentation (panel 1) based on [IAB-BAMF-SOEP \(2016, p.7\)](#). Data for panel 2 and 3 are from SOEP v33.2, weighted results. Own computations.

Table A6: Political Characteristics of States

	Voter Turnout	CDU/CSU	SPD	GRÜNE	FDP	LINKE	AfD	Others
2017								
Schleswig Holstein	0.763	0.340	0.233	0.120	0.126	0.073	0.082	0.027
Hamburg	0.760	0.272	0.235	0.139	0.108	0.122	0.078	0.045
Lower Saxony	0.764	0.349	0.274	0.087	0.093	0.070	0.091	0.036
Bremen	0.708	0.251	0.268	0.111	0.093	0.134	0.100	0.043
North Rhine-Westphalia	0.754	0.326	0.260	0.076	0.131	0.075	0.094	0.038
Hessen	0.770	0.309	0.235	0.097	0.115	0.081	0.119	0.044
Rhineland Palatine	0.777	0.359	0.241	0.076	0.104	0.068	0.112	0.039
Baden Württemberg	0.783	0.344	0.164	0.135	0.127	0.064	0.122	0.045
Bavaria	0.781	0.388	0.153	0.098	0.102	0.061	0.124	0.075
Saarland	0.766	0.324	0.271	0.060	0.076	0.129	0.101	0.039
Berlin	0.756	0.227	0.179	0.126	0.089	0.188	0.120	0.071
Brandenburg	0.737	0.267	0.176	0.050	0.071	0.172	0.202	0.063
Mecklenburg-West Pomerania	0.709	0.331	0.151	0.043	0.062	0.178	0.186	0.049
Saxony	0.754	0.269	0.105	0.046	0.082	0.161	0.270	0.067
Saxony-Anhalt	0.681	0.303	0.152	0.037	0.078	0.177	0.196	0.057
Thuringia	0.743	0.288	0.132	0.041	0.078	0.169	0.227	0.065
Average Treated States	0.753	0.337	0.200	0.081	0.103	0.101	0.127	0.051
Average Comparison States	0.749	0.296	0.203	0.085	0.093	0.129	0.144	0.050
Difference		*						
2013								
Schleswig Holstein	0.731	0.392	0.315	0.094	0.056	0.052	0.046	0.044
Hamburg	0.703	0.321	0.324	0.127	0.048	0.088	0.042	0.050
Lower Saxony	0.734	0.411	0.331	0.088	0.042	0.050	0.037	0.041
Bremen	0.688	0.293	0.356	0.121	0.034	0.101	0.037	0.057
North Rhine-Westphalia	0.725	0.398	0.319	0.080	0.052	0.061	0.039	0.050
Hessen	0.732	0.392	0.288	0.099	0.056	0.060	0.056	0.049
Rhineland Palatine	0.728	0.433	0.275	0.076	0.055	0.054	0.048	0.058
Baden Württemberg	0.743	0.457	0.206	0.110	0.062	0.048	0.052	0.065
Bavaria	0.700	0.493	0.200	0.084	0.051	0.038	0.043	0.092
Saarland	0.725	0.378	0.310	0.057	0.038	0.100	0.052	0.065
Berlin	0.725	0.285	0.246	0.123	0.036	0.185	0.049	0.077
Brandenburg	0.684	0.348	0.231	0.047	0.025	0.224	0.060	0.065
Mecklenburg-West Pomerania	0.653	0.425	0.178	0.043	0.022	0.215	0.056	0.060
Saxony	0.695	0.426	0.146	0.049	0.031	0.200	0.068	0.080
Saxony-Anhalt	0.621	0.412	0.182	0.040	0.026	0.239	0.042	0.058
Thuringia	0.682	0.388	0.161	0.049	0.026	0.234	0.062	0.080
Average Treated States	0.703	0.427	0.243	0.074	0.046	0.097	0.046	0.066
Average Comparison States	0.705	0.374	0.259	0.083	0.039	0.133	0.051	0.060
Difference		*						

Notes: * 10 %, ** 5 %, *** 1 % significance level. Data on electoral outcomes are from *Statistisches Bundesamt*. Own computations.

Table A7: Labor Market Characteristics of States

State	Unemployment (UE) rate dependent civilian workforce	UE rate total civilian workforce	workforce civilian	workforce males	UE rate females	UE rate foreigners	Share of asylum seekers to population
Schleswig Holstein	7.0	6.3	6.3	6.7	5.8	20.0	0.01022788
Hamburg	8.1	7.1	7.1	7.6	6.5	16.1	0.00964739
Lower Saxony	6.6	6.0	6.0	6.2	5.8	19.2	0.00948955
Bremen	11.6	10.5	10.5	11.0	10.0	26.7	0.00903569
North Rhine-Westphalia	8.5	7.7	7.7	8.0	7.4	21.5	0.01069396
Hessen	5.9	5.3	5.3	5.4	5.2	12.8	0.01124626
Rhineland Palatine	5.6	5.1	5.1	5.3	4.9	13.4	0.00785528
Baden Württemberg	4.2	3.8	3.8	3.9	3.7	9.1	0.00820461
Bavaria	3.9	3.5	3.5	3.7	3.3	8.9	0.00720878
Saarland	7.7	7.2	7.2	7.6	6.7	24.6	0.00187026
Berlin	11.5	9.8	9.8	10.5	9.0	19.9	0.01154153
Brandenburg	8.9	8.0	8.0	8.5	7.4	24.3	0.00720342
Mecklenburg-West Pomerania	10.6	9.7	9.7	10.6	8.7	28.9	0.00483214
Saxony	8.3	7.5	7.5	7.8	7.0	25.7	0.00702438
Saxony-Anhalt	10.4	9.6	9.6	9.9	9.2	32.2	0.00626361
Thuringia	7.4	6.7	6.7	7.0	6.5	22.0	0.00556362
Average treatment states	6.94	6.36	6.36	6.62	6.06	19.26	0.00684824
Average comparison states	8.32	7.45	7.45	7.87	6.98	20.82	0.0085152
Difference							

Notes: * 10 %, ** 5 %, *** 1 % significance level. Data on labor market characteristics are from *Statistisches Bundesamt*. Own computations.

Column 6 measures the share of asylum seekers who receive beneficiaries according to the *Asylbewerberleistungsgesetz* to the state population.

Table A8: **Effects on all Language Courses**

	Baseline	Male	W/o NRW	W/o City States	Placebo
<i>Beginning a LC</i>					
mean	0.518	0.572	0.511	0.514	0.518
[<i>i</i>]	0.102**	0.089	0.086**	0.108**	0.021
	0.043	0.067	0.038	0.044	0.098
Obs.	1450	995	1372	1356	1450
R^2	0.006	0.009	0.004	0.007	0.000
[<i>ii</i>]	0.086**	0.095	0.058	0.082*	-0.043
	0.039	0.059	0.038	0.038	0.101
Obs.	1450	995	1372	1356	1450
R^2	0.156	0.118	0.160	0.156	0.167
<i>Completing a LC</i>					
mean	0.200	0.215	0.198	0.196	0.200
[<i>i</i>]	0.081**	0.058	0.075**	0.106***	0.004
	0.035	0.045	0.034	0.033	0.060
Obs.	1450	995	1372	1356	1450
R^2	0.005	0.004	0.005	0.006	0.001
[<i>ii</i>]	0.072*	0.032	0.055	0.100**	0.015
	0.040	0.049	0.040	0.044	0.069
Obs.	1450	995	1372	1356	1450
R^2	0.077	0.070	0.079	0.081	0.075
<i>Language Proficiency Levels</i>					
mean	0.194	0.21	0.185	0.192	0.194
[<i>i</i>]	0.145***	0.133**	0.130**	0.148**	0.034
	0.044	0.045	0.044	0.051	0.089
Obs.	1439	987	1364	1345	1439
R^2	0.008	0.010	0.007	0.009	0.000
[<i>ii</i>]	0.088**	0.051	0.063*	0.088**	0.072
	0.034	0.042	0.035	0.040	0.104
Obs.	1439	987	1364	1345	1439
R^2	0.088	0.076	0.082	0.087	0.088

Notes: Standard errors are clustered at the state level and given in parentheses.

* 10 %, ** 5 %, *** 1 % significance level. Data are from SOEP v33.2. Own computations.

Some individuals report either "I received a different certification" or "I received no certification" for language proficiency levels on German language courses other than integration courses. Because in these cases overall language proficiency levels are missing, N decreases from 1450 to 1439 in the baseline specification.

Table A9: **Effects accounting for Networks**

<i>Completing an IK</i>		[1]	[2]
mean		0.087	0.087
		0.070***	0.071***
		0.018	0.018
Obs.		1450	1446
R^2		0.046	0.046
<i>Language Proficiency Levels</i>		[1]	[2]
mean		0.143	0.143
		0.132***	0.133***
		0.035	0.035
Obs.		1450	1446
R^2		0.077	0.077

Notes: Standard errors are clustered at the state level and given in parentheses.
* 10 %, ** 5 %, *** 1 % significance level. Data are from SOEP v33.2. Own computations.

Table A10: **Effects accounting for Spatial Mismatch**

<i>Completing an IK</i>		[1]	[2]	[3]	[4]	[5]
mean		0.087	0.087	0.087	0.087	0.087
		0.070***	0.051**	0.052***	0.052***	0.052**
		0.018	0.017	0.017	0.016	0.019
Obs.		1450	1375	1375	1386	1375
R^2		0.046	0.048	0.049	0.046	0.050
<i>Language Proficiency Levels</i>		[1]	[2]	[3]	[4]	[5]
mean		0.143	0.143	0.143	0.143	0.143
		0.132***	0.081	0.083	0.086*	0.089*
		0.035	0.053	0.053	0.047	0.048
Obs.		1450	1375	1375	1386	1375
R^2		0.077	0.088	0.089	0.080	0.092

Notes: Standard errors are clustered at the state level and given in parentheses.
* 10 %, ** 5 %, *** 1 % significance level. Data are from SOEP v33.2. Own computations.