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

Felicitas Schikora*

July, 2018

Abstract

This paper disentangles the effects of a recently introduced policy reform on participation measures and language development of refugees in Germany. The *residence rule* puts limitations on initial residence decisions for refugees with a permanent residence permit. Given that treatment intensity varies distinctly across states, I use this exogenous variation in a Differences-in-Differences approach.
I assess the reform's effect on participation in language courses and refugees' language skills. The results indicate that living in a state with strict statutory provisions has a positive effect on the probability to complete a language course and on certified language levels.

Keywords: Migration, Refugees, Language Acquisition, Placement Restriction, Residence Rule, Quasi-Experiment
JEL codes: J15, J60, K37, R23

*German Institute for Economic Research (DIW), Department Socio-Economic Panel, Mohrenstrasse 58, 10117 Berlin, Germany. Phone: 0049 (0)30 89789 326, Fax: 0049 (0)30 89789 115, Email: fschikora@diw.de

1 Introduction

In 2015 and 2016 alone, more than 1 million refugees[1] came to Germany (Federal Ministry of the Interior, 2017). As a consequence of this sudden influx of refugees[2], the Federal German Government has implemented several policy reforms that propose targeted integration measures for beneficiaries of protection. To facilitate the planning of integration activities in receiving and emitting municipalities and to distribute financial burdens more evenly (The Expert Council of German Foundations on Integration and Migration, 2016, p.4ff), policy makers thereby introduced the *residence rule ('Wohnsitzauflage')*. The *residence rule* puts additional restrictions on initial location choices for refugees with a permanent residence permit if certain criteria are fulfilled.

Understanding how initial residence of immigrants is linked to their language development and prospective labor market performance is essential in order to design and/or improve integration policies or refugee resettlement programs. If, and only if, policy measures are designed targeted and effectively, they serve as a valuable tool to reduce income transfers to immigrants and dependency on social welfare. This is particularly relevant considering that many refugees have been granted a permanent protection status and will stay in their host countries for an extended period of time, or permanently. Consequently, this paper disentangles the effects of the *residence rule* on participation in integration measures and language development of refugees in Germany.

To account for selective migration patterns, the empirical analysis relies on a quasi-experimental setting: As a result of the reform, a group of

[1] In public dispute, the terms *refugee* and *migrant* are often used interchangeably. Still, it is essential to explicitly differentiate between them. For the remainder of this paper, I will use the term *migrant* to describe an individual moving to another country expecting to live there for a certain period of time. Only in the recent German context, i.e., immigrants arriving in Germany as part of the latest refugee wave, I will adopt the term *refugee*.

[2] For visual proof, please refer to Figure 1. It provides the number of asylum applications in Germany from 1995 to 2018 (data for 2018 includes the first quarter only).

refugees, that was formerly able to choose its place of residence freely, now faces severe location restrictions. Yet, there exists substantial variation in the implementation of the *residence rule* across states in Germany. While beneficiaries of protection may move freely *within* some federal states, a couple of states have decided to apply the residence rule much more rigorously, such that refugees are obliged to stay in a certain district. I exploit resulting temporal as well as spatial variations in a Differences-in-Differences (DiD) estimation design.

This paper is related to two branches of literature. One strand of the literature studies the effect of initial placement restrictions on determinants of economic assimilation and immigrants' labor market performance. In this context, however, it is essential to account for selective migration patterns: If immigrants choose their residence 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.[3] To account for endogenous sorting of immigrants, existing evidence is largely based on so called *settlement policies* in the Scandinavian countries which determined immigrants' place of residence without considering 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) show that being placed in an ethnic enclave increases immigrants' earnings significantly, in particular for immigrants in the lower tail of the skill distribution. This finding is strengthened by Damm (2009), who indicates that this positive impact is primarily driven by network effects. Edin et al. (2004) use a DiD design and investigate a policy change in Sweden. The authors show that policies which choose income support over reintegration measures have detrimental effects on immigrants' earnings. Additionally, Rosholm and Vejlin (2010) find that lowering public income

[3] To 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.

transfers has a positive effect on the probability to find employment in a competing risk framework. Åslund and Rooth (2007) finally emphasize that local as well as national labor market conditions play a crucial role in determining long-term integration outcomes.

The second branch of literature is related to the relationship of language skills as "an important form of human capital" (Chiswick and Miller, 1995, p.248) and labor market performance of immigrants. Chiswick (1991) shows the relevance of self-reported language skills on immigrants' earnings. In his paper, the author emphasizes the importance of reading rather than speaking fluency with respect to labor market performance of immigrants. Using data on Australia, the 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) extend these analyses and account for measurement errors in self-reported language skills and correlated heterogeneity: They demonstrate that simple ordinary least squares (OLS) estimates are subject to substantial biases and find that - if both of these problems are taken into account - estimated effects on earnings are considerably larger than the OLS baseline. 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. Lastly, Bleakley and Chin (2004) exploit a quasi-experimental setting, 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.

While previous analyses of *settlement policies* have mainly focused on their effect on labor market performance, the *residence rule* has been introduced to provide refugees better access to integration measures and language courses. In their paper, Chiswick and Miller (1995, p.251) point out that refugees may acquire the host country's language less efficiently than other migrants. In light of this argument and considering the strong link between language skills and prospective labor market outcomes of im-

migrants, this paper therefore provides an alternative angle and investigates the impact of initial placement restrictions on language acquisition of refugees in Germany.

Besides this new perspective, the contribution to the existing literature is twofold: While the data sets employed to analyze the impact of *settlement policies* on labor market outcomes of immigrants are rich and of high quality, it is impossible to identify the formal status of an immigrant. The authors usually proxy refugee status by country of origin, which may lead to imprecise estimates. Secondly, this is one of the first studies to analyze immigrants who arrived in Europe as part of the latest refugee waves and who are substantially different to previous cohorts in a number of characteristics.[4] Using an innovative data set, the IAB-BAMF-SOEP Survey of Refugees in Germany, this paper intends to close the gap in the existing literature and extend previous analyses.

Preliminary results suggest that a more restrictive implementation of the *residence rule* proves to be effective in providing integration measures and language courses at the district level: Living in a state with a higher treatment intensity increases the probability to complete an integration course by approximately 6 percentage points and the average German skill level by 0.114 units. This effect is statistically significant and robust to the inclusion of additional covariates as well as to the definition of various subsamples.

The remainder of this paper is structured as follows. Section 2 describes the institutional background and illustrates the policy reform of interest. After a short description of the data set, Section 3 provides the definition of the working sample as well as descriptive statistics. Section 4 outlines the empirical strategy and defines treatment and comparison group. Section 5 reports preliminary results and some robustness checks and Section 6 provides guidance on mechanisms. Section 7 summarizes the findings.

[4] Furthermore, it is particularly insightful to focus on Germany, the country that has been affected most heavily by immigration of refugees in recent years (Organization for Economic Co-operation and Development, 2017, p.17).

2 Institutional Background

Asylum Procedure. — In Germany, individuals seeking for political asylum have to register at a state authority (such as a border or security authority) upon arrival (BAMF, 2016, p.8f). They will then be distributed to an *initial reception center* nearby based on the *Königstein key*. The *Königsstein key* is a distribution mechanism that allocates refugees at state level based on a state's tax revenue and population size.[5] In practice, the *Königsstein key* strives to ensure an equal distribution of refugees across federal states in Germany without considering individuals' preferences.[6] Hence, initial placement of refugees into a specific state is exogenous and as such immune to self-selection.

Once assigned to an initial reception center, asylum seekers may formally pose their asylum request (BAMF, 2016, p.11ff). Until a final decision on their claim has been reached, they must reside in the initial reception center (and, ergo, the initially assigned state). Importantly, this placement restriction for asylum seekers has been unaffected by the *residence rule*.

(Table 2)

Overall, there exist four different classes of protection in Germany (The Expert Council of German Foundations on Integration and Migration, 2017, p.4). While refugees may be entitled to *political asylum* based on basic constitutional law (Grundgesetz), the majority of immigrants seeking protection is granted protection based on the *Geneva Refugee Convention*.[7] Once

[5] For further information, please refer to `http://www.bamf.de/EN/Fluechtlingsschutz/AblaufAsylv/Erstverteilung/erstverteilung-node.html` (last downloaded on May 27, 2018). Table 1 presents the exact splitting rule from 2010 to 2017.

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

[7] Indeed, protection rates differ considerably across protection classes (Compare Table 2 (BAMF, 2018, p.10)). For example, political asylum is granted in less than one

either of these states is granted, refugees receive a permanent residence permit for at least three years. Refugees who are allotted a *subsidiary protection status* receive a residence permit for one year only which can be extended several times. Lastly, *tolerated foreigners* face deportation restrictions after their asylum application has been rejected.[8] The Organization for Economic Co-operation and Development (OECD) defines people who have successfully applied for asylum and have been granted some sort of protection as *humanitarian migrants* (Organization for Economic Co-operation and Development, 2016, p.7). This paper is following the OECD's definition and hereinafter refers to all refugees who are granted a protection status of category one to three as *humanitarian migrants*. *Tolerated foreigners* are discarded from the empirical analysis.

The Residence Rule, §12a AufenthG. — While it is prohibited to relocate until a final decision on an asylum request has been reached, humanitarian migrants had been free to choose their place of residence in the past. The new regime, in contrast, partly enforces severe restrictions on initial place of residence (The Expert Council of German Foundations on Integration and Migration, 2016, p.20ff). As those are obligatory under certain circumstances only, the *residence rule* evidently applies to a subgroup rather than the total of humanitarian migrants.

With introduction of the residence rule, humanitarian migrants are obliged to take their place of residence in the state in which they filed their asylum request for up to three years after acceptance unless a legally defined exemption rule applies. This exemption rule states that:

> *"Sentence 1 [placement restriction on federal state level, ed. notes] 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 secu-*

percent of the asylum applicants, while approximately 20 percent receive protection based on the Geneva Refugee Convention.

[8] To add to this complexity, Germany has no distinct Immigration Act that explicitly rules immigration other than asylum requests.

> *rity 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 Ministery of Justice and Costumer Protection, 2016, §12a AufenthG, Art.1)

Clearly, the *residence rule* limits mobility between states if humanitarian migrants do not satisfy the so called exemption rule. Mobility within states, however, is only affected if states enforce additional legislation (Comp. Section 2 - *Additional Legislation at the federal state level*). A location restriction at state level may be considered a marginal change only. Especially in economically less developed states, however, this restriction may nevertheless be effective for humanitarian migrants targeted by the reform. Even though humanitarian migrants are free to leave a state if they find employment/training conditions that satisfy the exemption rule, competing with native workers in a different local labor market is challenging. This argument is strengthened by the fact that the *residence rule* applies to a group of humanitarian migrants which is less attached to the labor market in the first place.[9] The *residence rule* may therefore severely restrict freedom of movement for humanitarian migrants.

The residence rule was introduced as part of the Integration Act in summer 2016. Even though the law has been publicly announced in July, it was introduced with retroactive effect to January 1, 2016. Nevertheless, all federal states but North Rhein-Westphalia (NRW) decided to apply the rule for decisions made in/after August 2016 only in order to avoid confusion ('*Härtefallregelung*') (Ministry of the Interior and Federal Affairs, Schleswig-Holstein, 2016, p.4). Consequently, the *residence rule* affects all decisions on residence permit that were taken in/after August 2016. For

[9] The exemption rule defined in §12a AufenthG explicitly excludes individuals who have taken up employment with full social security coverage as well as individuals who are in some form of training relationship (schooling, vocational training) from legally binding placement restrictions.

decisions taken in North Rhein-Westphalia, the legally defined cut-off date applies.[10]

Additional legislation at state level — With introduction of the Integration Act, states have further been given the possibility to apply additional regulations at state level (Federal Ministery of Justice and Costumer Protection, 2016, §12a Art. 9 AufenthG). As of January 2017, four states have imposed additional legislation in order to apply the *residence rule* more rigorously, including Bavaria, Baden-Wurttemberg, North Rhein-Westphalia and Saxony-Anhalt (Organization for Economic Co-operation and Development, 2017, p.49f). Even though allocation keys differ, these decrees[11] mandate humanitarian migrants to live in a specific municipality or district. In any of the other federal states, humanitarian migrants remain free to move within the assigned state.

Potential threats to identification. — Besides introducing the *residence rule*, policy makers also suspended the so called *priority review* in summer 2016.[12] One might therefore worry about confounding the estimated effects with other reforms or changes taking place in the same period. Since both reforms target different groups of immigrants (humanitarian migrants vs. asylum seekers), however, 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 controversial discussion about immigration in Germany at the time of the policy change, a second source of bias could arise if some individuals had anticipated the reform. Even if this was the case, asylum seekers generally have no possibility to influence the timing of their asylum

[10] For clarification, please refer to the timeline in Figure 2.

[11] Please refer to Bayerische Staatsregierung (2016); Ministerium für Inneres, Digitalisierung und Migration, Baden-Württemberg (2016); Flüchtlingsrat Sachsen-Anhalt e.V. (2016); Landesregierung Nordrhein-Westphalen (2016), Verordnungen available in German only.

[12] The *priority review* is a check mechanism by the Federal Employment Agency that evaluates if there is a native worker comparable to the migrant worker in the job application process (Federal Ministery of Labour and Social Affairs, 2017). Only if no German employee is available, an asylum seeker may be granted the job.

decision. What is more, due to the sudden influx of refugees into Germany in 2015 and 2016, many asylum seekers had to wait for a prolonged of time until they received a final decision. Ergo, anticipation effects are largely negligible.

3 Data and Descriptive Statistics

Data Set. — This paper's empirical analysis is based on the IAB-BAMF-SOEP Survey of Refugees in Germany which is collected as part of the Socio-Economic Panel (SOEP). This data set provides information on refugees, who arrived in Germany from 2013 onwards, and will be collected in the years of 2016, 2017 and 2018 (Brücker et al., 2016; German Institute for Economic Research, 2017). Figure 3 provides graphical evidence on the distribution of surveyed refugee households in Germany. Even though participation is voluntary, the first survey wave from 2016 (samples M3, M4) covers more than 4,000 adult refugees in Germany. Sample M5, which has first been surveyed in 2017, extends the number of observations by approximately 2,000 observations.[13] Kroh et al. (2017, p.6) underline that results are representative of the target population if design weighting procedures are applied.

Using this data source has several advantages in comparison with alternative sources of information: Firstly, the Survey of Refugees in Germany maintains detailed information on residence status and the exact date of receiving a residence permit. This information is essential in order to identify treatment and period indicator, but is not provided in other data sets such as administrative records of the Federal Employment Agency. Secondly, the SOEP collects thorough and unique information on individuals' migration, education and employment histories as well as a rich set of control variables. It is further possible to extract information on the current

[13] To increase the number of observations, I combine both survey waves and include samples M3, M4 and M5 in the empirical analysis.

place of residence at the district level. Consequently, the data set allows the researcher to identify treatment and control group in the pre- and post-treatment period.

Definition of Working Sample/Data Preparation. — For the empirical analysis of this paper, I define a working sample that is subject to certain restrictions. Since the reform is applicable to first-time asylum seekers only, I drop observations who pose a repeated asylum request. I further keep only observations who contain valid information on type of residence permit and timing of asylum request. This reduces sample size quite substantially.[14] Due to a limited sample size, I discard all observations that have reached a decision on their asylum claim before 2015 as well as individuals who are older than 67. Finally, I restrict the working sample to have non-missing values for all explanatory variables employed in the Differences-in-Differences analysis.

Besides providing information on individual refugees, the IAB-BAMF-SOEP Survey of Refugees in Germany allows a researcher to consider a survey participant's family characteristics. I use the additional household questionnaire to extract information on childrens' educational and vocational training status. As the SOEP is based on a household level and all members of the household are interviewed, I can further identify spouses living in the same household.

Consequently, it has been possible to create an indicator variable that takes a value of one if a survey participant is not obliged to the *residence rule* based on his or her spouse's characteristics. It has to be highlighted, though, that the exemption rule does also apply if one's spouse is living in a different German household. Because these cases cannot be identified

[14] The IAB-BAMF-SOEP Survey of Refugees in Germany explicitly surveys asylum seekers whose asylum claim is still pending. Since those observations clearly miss a decision date, I loose approximately 30 percent of the sample. What is more and in line with the arguments in Section 2.1, the empirical analysis considers only those individuals who are subject to the *residence rule* based on §12a AufenthG. Consequently, I discard observations who satisfy the legally defined exemption rule based on personal, but also based on their family's characteristics.

in the data, the empirical analysis excludes individuals who claim to be married, but whose spouse is living in a different household in Germany. Thus, 69 observations are dropped from the sample.

Taking these considerations into account, the defined working sample[15] consists of 1440 observations: 743 treated and 697 control individuals.

Descriptive Statistics. — Table 4 presents descriptive statistics. Column 1 provides information on the treatment group, column 2 then adds information on the control group. To account for selection by design and potential self-selection, results are weighted by the corresponding weighting factor (Kroh et al., 2017).

(Table 4)

To begin with, Table 4 provides substantial evidence that treatment and control group are very similar in socio-economic characteristics: Both groups barely differ with respect to observable characteristics such as age, age at migration, average skill level of German[16], years of schooling as well as in their family status. Still, treated observations are less likely to be female (21 versus 23 percent) and to have children (36 versus 42 percent) than comparison units. Nevertheless, these differences are not statistically significant at conventional levels.

What is more, employment status at the time of the interview is comparable across groups. Differences in the rate of unemployment (91 percent for treated versus 94 percent for control units) may be attributable to higher rates of regional unemployment, which are more favorable in treatment states. What is more, treatment and control group are remarkably similar in other forms of labor market attachment. For example, 26 percent of individuals state that they have been actively looking for jobs in the last four weeks irrespective of their treatment status. Further, if asked about (planned) future labor force participation on a scale from 1 to 4 (1 "No, def-

[15] For an overview, please refer to Table 3.

[16] The variable `avg ger` provides an average on the individual's self-reported ability to speak, read and write the German language on a scale from 1 to 5 (1 "very well", 2 "well", 3 "averagely", 4 "not very well", 5 "not at all").

initely not", 2 "Unlikely", 3 "Probably", 4 "Definitely"), both treatment and control individuals show a strong preference for future employment (3.78 vs. 3.71).

Lastly, descriptive statistics suggest that there exists some variation with respect to country of origin. For example, while in the comparison group 77 (5) percent originally stem from Syria, Iraque or Iran (Afghanistan, Pakistan, India), 87 (2) percent of treated individuals report those as their countries of origin. This is in parts a result of a sorting mechanism by country of origin that has recently been applied in Germany (Organization for Economic Co-operation and Development, 2016). Consider, nevertheless, that refugees have no say in where to be placed during the asylum procedure (Compare Section 2 - *Asylum Procedure*), but are assigned to a particular state based on the *Königsstein key*. What is more, the DiD estimator may explicitly control for these differences as long as the differences are constant over time.

Repeated Cross-Section. — As the IAB-BAMF-SOEP Survey of Refugees has been first surveyed in 2016 and data on interviews from 2017 is not fully available yet, the empirical analysis relies on a single cross-section. To be precise, I include 2016-information on samples M3 and M4 as well as sample M5 from 2017. Yet, it is possible to split the data artificially into a pooled cross-section and to form *cohorts* using information on the year of asylum decision. The defined working sample consists of humanitarian migrants who received a positive asylum status in the years of 2015, 2016 and 2017. Considering that the reform investigated has been introduced in the summer of 2016, I further distinguish between individuals who received their status before and after the reform was introduced. Thus, I can differentiate between four cohorts: 2015, 2016-pre, 2016-post and 2017.

Please note that - because the interviews were conducted from July 2016 onwards - it is essential to formulate outcome variables which represent the outcome variable at the time of asylum decision rather than in the post-treatment period. Luckily, the data set provides detailed information

on humanitarian migrants' employment and language course histories since their arrival in Germany. It is therefore possible to extract information on the outcome variables in the year of asylum decision.

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 federal states as well as the fact that humanitarian migrants may not influence their place of initial residence.

Outcome Variables. — Considering that the *residence rule* was introduced to make integration measures more accessible to humanitarian migrants in need, I focus on the effect of higher treatment intensity on so called *integration courses (IK)*. In general, all migrants who arrive in Germany are highly recommended to participate in an IK if they face difficulties to make themselves understood in German (BAMF, 2017). Clearly, humanitarian migrants are eligible for participation. The Integration Act aims at improving access to those courses plus enables asylum seekers with high probability of acceptance and tolerated foreigners to participate for the first time. Regarding its content, an IK is split in two parts: a general course on life in Germany (100 lesson hours) as well as a language course (600 lesson hours). To investigate if a strict implementation of the *residence rule* has an effect on language acquisition of refugees in Germany, the empirical analysis focuses on three outcome variables: While the variable `BIK` measures the probability to take up an IK, `EIK` provides information on the probability to complete an IK in the year of the asylum decision. Lastly, the variable `IK Niv` measures certified and completed language skills in the year of the asylum decision on a scale from 0 to 3 (0 "No certified level", 1 "Level A1", 2 "Level A2", 3 "Level B1").

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 individuals over time (Lechner et al., 2011, p.168).[17] Hence, this quasi-experimental method may control for existing time trends as well as for differences between treatment and control group that exist permanently.

The *residence rule* was introduced in mid 2016, affecting all humanitarian migrants who received a decision on their asylum request since August 2016[18]. Ergo, the pre-treatment cohorts consist of all survey respondents who received a positive asylum decision before this legally defined cut-off date. On the other hand, observations with a positive decision on their asylum claim thereafter are considered post-treatment observations.

To define treatment and comparison group, I exploit regional variation in statutory requirements across states. §12 of the Integration Act provides federal states the choice to impose further requirements on initial placement of humanitarian migrants. If this is the case, beneficiaries of protection will face mobility restrictions *within* states, in addition to being unable to move *between* federal states. Bavaria, Baden-Wurttemberg, Saxony-Anhalt and North Rhein-Westphalia have passed additional decrees which provide 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 4 compares the development of the outcome variables for treated and comparison units disaggregated at a quarterly level (from Q1/2015 to Q4/2017) and contrasts their evolution over time. If the graphs develop similarly prior to the reform, this provides graphical evidence that the *common trend assumption* is met. Differences in the post-treatment period may

[17] Alternative, yet slightly different, presentations of the theoretical background can be found ,e.g., in Bertrand et al. (2004), Abadie (2005) and Imbens and Wooldridge (2009).

[18] for NRW: since January 2016

then be attributable solely to the policy change. Common time trends are evident regarding the probability to complete an integration course and the average level of language courses in the sample. With respect to the probability to take up an integration course in the year of asylum decision, however, graphical evidence is weaker: Especially in early 2015, Figure 4 suggests that treatment and comparison units differ substantially.

Exploiting differences in statutory regulations, I estimate a DiD model that can be defined as in the following equation:

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

where Y_{it} is the outcome of interest measured in the year of asylum decision, i indexes the treatment status and t provides a binary variable that takes a value of 0 (1) in the pre-treatment (post-treatment) period. To account for the fact that socio-economic characteristics are measured in the post-treatment period, I estimate three 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, speaking some German prior to immigration, a dummy for work-experience prior to emigration, as well as years of schooling (before immigration), interaction terms of being female and having children and being female and being married, a sample indicator, and country of origin fixed effects. Specification iii extends this list and adds the covariates number of children, age, age squared, type of residence permit, a dummy variable that takes a value of 1 if the survey participant wants to stay in Germany forever, a dummy variable that indicates having received help from authorities, the level of exposure to natives, the level of satisfaction with the current living situation, months since arrival, months since asylum decision as well as unemployment rate, population size, the number of vacancies at district-level and state fixed effects. The dummy variable $Treat_i$ is equal to 1 if an individual lives in the treatment area, Per_t 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.

In this setting, α_3 captures the causal effect of interest. Since the regression model estimates 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).

Alternative Specifications. — 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 that simulates treatment occurred before the actual treatment period, namely in August 2015. If indeed there exists no effect on the pre-treatment population, this placebo test should consistently return a zero effect.

5 Results and Sensitivity Analyses

Main Results — Table 5, 6 and 7 report preliminary results based on equation (1) for the respective outcome variables. To account for selection bias, I estimate three alternative specifications: (*i*) without a set of controls, (*ii*) including time invariant control variables only and (*iii*) including a full set of covariates.

(Table 5) (Table 6) (Table 7)

The results in Table 6 clearly indicate a positive and statistically significant effect of stricter treatment intensity on the probability to complete an integration course. Living in a state that enforces the *residence rule* more strictly increases the probability to conclude an IK in the year of one's positive asylum decision by approximately 6 percentage points. To be precise, estimation values range between 6.3 and 6.9 percentage points and are statistically significant at the one percent level. This is a very large effect considering that the mean of the outcome variable is 8.6 percent.

The results are remarkably similar if city states are excluded (column 4).

For a subsample that includes male refugees only, the effect is even more pronounced: Column 2 indicates that male refugees who live in a more restrictive state are 8.4 to 9.5 percentage points more likely to conclude a language course in the year of asylum decision, relative to a mean of 9.1 percent (ergo, nearly an increase of 100 percent).

Section 2 underlines that North Rhein-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 Rhein-Westphalia - might be forced to return to a different state. This may take time and effort. Hence, estimation results may be biased. As a sensitivity analysis, I therefore exclude all observations who received a positive asylum decision from January to July, 2016 and live in North Rhein-Westphalia. If these observations are excluded from the empirical analysis, estimate values still range in the same ballpark 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 perform a placebo test that simulates the reform took effect in August 2015 rather than in August 2016. As column 5 indicates, this placebo test yields insignificant estimates for all specifications. What is more, estimates are generally much smaller.

Table 7 provides evidence on the reform's effect on refugees' certified language skills. The results show a pattern that is very similar to the one provided earlier. For the baseline sample, living in a federal state that applies stricter rules on initial place of residence increases certified language levels by 0.114 to 0.165 units, relative to a mean of 0.14 percent. 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 6, the placebo test consistently returns a small and statistically insignificant effect.

Lastly, it has to be pointed out that estimation results are not as clear

with respect to the probability to take up an integration course in the year of asylum decision. Table 5 provides suggestive evidence that living in a more restrictive state increases the probability to take up an IK by 7.2 to 11.3 percentage points, relative to a sample mean of 32.4 percent. For the baseline estimates, only specification *iii* returns a statistically significant effect at the 10 percent level. Columns 2 to 4 return a similar pattern. Moreover, the placebo test in column 5 returns a statistically significant effect for specification *i* and *ii*. This is consistent with graphical evidence provided in Figure 4. Thus, estimation results in Table 5 should be interpreted with extreme caution only.

Robustness Analyses — 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 downward biased". After a Monte Carlo simulation study in which the authors compare several alternative measures, they then suggest to employ 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 generate pseudo-samples of the residuals of the original regression using so called *Rademacher weights*. Subsequently, I re-estimate the regression equation based on the created pseudo-samples, while keeping the vector of control variables constant. In this setting, I test the null of no treatment effect.

Results gained from this estimation exercise confirm the findings provided in previous paragraphs. Using a total of 999 replications, the wild cluster bootstrap procedure returns a p-value of $p = 0.012$ for the probability to end an integration course[19]. While this p-value is slightly larger than the one from original cluster-robust inference, it clearly provides statistically significant evidence that living in a state which applies the *residence*

[19] The results reported are based on specification *iii* and the baseline sample.

rule more strictly has a positive effect on the probability to end an integration course. Similar findings hold for the level of certified language skills: Here, bootstrapping procedures provide a p-value of $p = 0.018$. For the probability to begin an integration course, however, the wild cluster bootstrap does not validate a statistically significant effect.

Lastly, and as a further robustness analysis, I run a simulation exercise. To do so, I randomly assign a higher treatment intensity to federal states as a first step and then run the regression analysis using the placebo treatment indicator rather than the original one. I repeat this procedure 10.000 times and save the estimation coefficients in an additional data set. Figure 5 displays these placebo coefficients in a histogram, while the ITT coefficient as in 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, 97.25 (96.18) percent of the estimates for the outcome variable `EIK` (`IK Niv`) lie below the estimated treatment effects.[20] This suggests that while estimated effects are rather large in percent, they are not unreasonably high. Furthermore, the graph provides some evidence that the effect is not driven by a single state alone but by the total of four states.

To conclude, estimation results indicate that higher treatment intensity has indeed had a positive and statistically significant effect on the probability to complete an integration course as well as the certified language level at the end of a course. This provides evidence for the fact that urging humanitarian migrants to stay in a particular district facilitates the planning and allocation of integration measures and, in turn, may improve quality of language courses. This is particularly relevant considering that higher skills in the host countries' language facilitates labor market access and are a powerful indicator for prospective labor market outcomes.

[20] For the outcome variable *Beginning an IK* this is true for 87.72 percent of the simulated estimates.

6 Mechanisms

Following a detailed discussion of treatment effects in Section 5, this section intends to provide an intuition for potential mechanisms. Using additional data provided by the federal statistical offices (*Regionaldatenbank Deutschland*) and the Federal Office for Migration and Refugees (BAMF), I give some proof on mechanisms that have already been well established in the empirical literature.

For one, network effects might play a role. There exists extensive literature on the importance of social networks, e.g., Bertrand et al. (2000) and Borjas (2000). To get a better understanding for the mechanisms involved, I distinguish between two types of networks: a network of natives and a network of co-nationals.

The IAB-BAMF-SOEP Survey of Refugees in Germany provides some information on social networks to the native population. For example, the survey reports the level of exposure to the native population on a scale from 1 to 6 (1 "Never", 2 "Less often", 3 "Every month", 4 "Every week", 5 "Several Times per Week", 6 "Every day") as well as a dummy variable that indicates if a refugee has received help from authorities in various areas (such as legal counseling, recognition of educational qualifications, finding employment etc.). Both of these variables are included as control variables in specification *iii*. I investigate the mean difference of these variables across treatment groups and test if differences are statistically distinct. Yet, variables are distributed similarly and differences are never statistically significant.

To examine networking effects among foreigners, I merge information provided by the federal statistical offices (Ausländerzentralregister) on the share of foreign population at the district-level. Descriptive evidence suggest that humanitarian migrants in treated states are placed in districts with a significantly smaller share of foreigners than humanitarian migrants in comparison states. This provides some evidence that high-intensity treatment states tend to avoid ethnic clustering. Since the data set does not

offer information on nationalities, I further exploit survey information on the level of exposure to people from the same country of origin (besides relatives): Similarly to the previous findings, I do not find a statistically significant effect using a two-sided t-test.

Secondly, strictly assigning humanitarian migrants to certain districts may facilitate anticipating the demand and supply for integration courses at a regional level. Refugees who live in states that abstain from applying the *residence rule* more rigorously may simply lack information on integration courses or face higher commuting costs. Hence, another important channel could be spatial mismatch (Kain, 1968).

The BAMF provides a table that lists all integration course providers with detailed information on their addresses including ZIP codes. From this line-up, I extract the number of course providers per ZIP code and aggregate this at the district-level. I can then generate a variable that captures the number of courses provided per district deflated by the number of ZIP codes per district.[21] Subsequently, I merge this information to my working sample. A two-sided t-test indicates that the number of course providers per district is considerably larger in treated than in comparison states (2.1 versus 1.6). This mean difference is statistically significant at the one percent level.

This pattern is confirmed if the number of course providers per district is evaluated as an outcome variable in the Differences-in-Differences setting from equation (1). Table 8 displays a statistically significant treatment effect of 0.4 additional course providers in high-treatment intensity states as a result of the *residence rule*.

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

[21] Deflating is essential, because the number of ZIP code areas per district differs considerably. For example, for the city of Berlin, there exist 191 ZIP codes in one district.

courses whenever needed. Consequently, the *residence rule* serves as a valid tool to match supply and demand for integration courses effectively.

7 Conclusion

There is currently a controversial debate about how to integrate immigrants best and fastest into European societies. This dispute has been fueled by the sudden and massive immigration of foreigners into Europe triggered by the destabilization of the MENA region in recent years. As a result, several European countries have changed their legislation in an attempt to foster integration measures and to allow for targeted integration of immigrants into local labor markets. Similarly, Germany has introduced a couple of policy reforms within the integration act framework.

This paper provides first evidence on the short-term effects of tight placement restrictions on refugees' language outcomes and participation in integration courses. To provide a causal effect, I exploit different treatment intensities across federal states that resulted from a policy change introduced in Germany in August 2016. The estimation results suggest that living in a state with strict placement policies has had a positive and statistically significant effect on the probability to complete an integration course as well as the certified German skill level at the end of the course. On the other hand, the results provide no clear evidence on the effect on the probability to begin an integration course.

Whether these results for Germany are applicable to other countries is an open question. Still, this paper is the first to shed light on participation in language courses and language effects of initial placement restrictions of refugees.

It is worth to perform further analyses to strengthen presented findings: In particular, it is essential to reevaluate results using the full set of interviews for 2017. If this will increase sample size, however, remains an empirical question. While I expect more individuals to receive a pos-

itive decision on their asylum claim, the Survey of Refugees suffers from high panel attrition. What is more, I intend to extend this paper in the following dimension: There exist several other possibilities to attend language courses, such as alternative commercial courses or privately organized ones. For a better understanding on the effect of the *residence rule* on language courses as a whole, I aim to extend the empirical analysis to the total of language courses.

References

Abadie, A. (2005): "Semiparametric difference-in-differences estimators," *The Review of Economic Studies*, 72, 1–19.

Åslund, O. and D.-O. Rooth (2007): "Do when and where matter? Initial labour market conditions and immigrant earnings," *The Economic Journal*, 117, 422–448.

BAMF (2016): "The stages of the German asylum procedure - An overview of the individual procedual steps and the lagal basis," Tech. rep., The Federal Office for Migration and Refugees.

BAMF (2017): "Instruction Sheet for Integration Course," Tech. rep., BAMF.

——— (2018): "Aktuelle Zahlen zu Asyl, Ausgabe März 2018: Tabellen, Diagramme, Erläuterungen," Tech. rep., Bundesamt für Migration und Flüchtlinge.

Bayerische Staatsregierung (2016): "Verordnung zur Durchführung des Asylgesetzes, des Asylbewerberleistungsgesetzes, des Aufnahmegesetzes und des §12a Aufenthaltsgesetzes (Aslydurchführungsverordnung - DVAsyl)," Tech. rep., Bayerische Staatsregierung.

Bertrand, M., E. Duflo, and S. Mullainathan (2004): "How much should we trust differences-in-differences estimates?" *The Quarterly journal of economics*, 119, 249–275.

Bertrand, M., E. F. P. Luttmer, and S. Mullainathan (2000): "Network Effects and Welfare Cultures," *The Quarterly Journal of Economics*, 115, 1019–1055.

Bleakley, H. and A. Chin (2004): "Language Skills and Earnings: Evidence from ChilChild Immigrants," *The Review of Economics and Statistics*, 86, 481–496.

Borjas, G. (2000): "Ethnic enclaves and assimilation," *Swedish Economic policy review*, 7, 89–122.

Brücker, H., N. Rother, and J. Schupp (2016): "IAB-BAMF-SOEP-Befragung von Geflüchteten: Überblick und erste Ergebnisse," *Politikberatung kompakt*, 116.

Cameron, A. C., J. B. Gelbach, and D. L. Miller (2008): "Boostrap-Based Improvements for Inference with Clustered Errors," *The Review of Economics and Statistics*, 90, 414–427.

Card, D. (1990): "The impact of the Mariel boatlift on the Miami labor market," *ILR Review*, 43, 245–257.

Chiswick, B. R. (1991): "Reviewed Work(s): Freinds or Strangers: The Impact of Immigrants on the U.S. Economy by George J. Borjas," *Journal of Economic Literature*, 29, 627 – 629.

Chiswick, B. R. and P. W. Miller (1995): "The Endogeneity between Language and Earnings: International Analyses," *Journal of Labor Economics*, 13, 246–288.

Damm, A. P. (2009): "Ethnic enclaves and immigrant labor market outcomes: Quasi-experimental evidence," *Journal of Labor Economics*, 27, 281–314.

Dustmann, C. and F. Fabbri (2003): "Language Proficiency and Labour Market Performance of Immigrants in the UK," *The Economic Journal*.

Dustmann, C. and A. van Soest (2001): "Language Fluency and Earnings: Estimation with Misclassified Language Indicators," *The Review of Economics and Statistics*, 83.

Edin, P.-A., P. Fredriksson, and O. Åslund (2003): "Ethnic enclaves and the economic success of immigrants. Evidence from a natural experiment," *The quarterly journal of economics*, 118, 329–357.

——— (2004): "Settlement policies and the economic success of immigrants," *Journal of Population Economics*, 17, 133–155.

Federal Ministery of Justice and Costumer Protection (2016): "Act on the Residence, Economic Activity and Integration of Foreigners in the Federal Territory; Residence Act," `http://www.gesetze-im-internet.de/englisch_aufenthg/englisch_aufenthg.html#p0180`, translation provided by the Language Service of the Federal Ministry of the Interior. lastly downloaded on 11.01.2018.

Federal Ministery of Labour and Social Affairs (2017): "Material für die Presse: Das neue Integrationsgesetz," `http://www.bmas.de/DE/Schwerpunkte/Neustart-in-Deutschland/Neustart-Arbeitgeber/integrationsgesetz.html`, lastly downloaded on 26.10.2017.

Federal Ministry of the Interior (2017): "280.000 Aslysuchende im Jahr 2016," `https://www.bmi.bund.de/SharedDocs/pressemitteilungen/DE/2017/01/asylantraege-2016.html`, lastly downloaded on 08.01.2018.

Flüchtlingsrat Sachsen-Anhalt e.V. (2016): "Informationen zur Wohnsitzregelung nach §12a AufenthG für Sachsen-Anhalt," Tech. rep., Flüchtlingsrat Sachsen-Anhalt e.V.

German Institute for Economic Research (2017): "IAB-BAMF-SOEP-Befragung Geflüchteter in Deutschland," https://www.diw.de/de/diw_02.c.244287.de/ueber_uns/menschen_am_diw_berlin/mitarbeiter/innen.html?id=diw_01.c.538695.de, lastly downloaded 23.10.2017.

Imbens, G. W. and J. M. Wooldridge (2009): "Recent Developments in the Econometrics of Program Evaluation," *Journal of Economic Literature*, 47, 5–86.

Kain, J. F. (1968): "Housing Segregation, Negro Employment and Metropolitan Decentralization," *The Quarterly Journal of Economics*, 82, 175–197.

Kroh, M., S. Kühne, J. Jacobsen, M. Siegert, and R. Siegers (2017): "Sampling, Nonresponse, and Integrated Weighting of the 2016 IAB-BAMF-SOEP Survey of Refugees(M3/M4)," *SOEP Suvey Papers*, 477 - Series C.

Landesregierung Nordrhein-Westphalen (2016): "Verordnung zur Regelung des Wohnsitzes für anerkannte Flüchtlinge und Inhaberinnen und Inhaber bestimmter humanitärer Aufenthaltstitel nach dem Aufenthaltsgesetz (Ausländer-Wohnsitzregelungsverordung - AWoV)," Tech. rep., Landesregierung Nordrhein-Westphalen.

Lechner, M. et al. (2011): "The estimation of causal effects by difference-in-difference methods," *Foundations and Trends® in Econometrics*, 4, 165–224.

Ministerium für Inneres, Digitalisierung und Migration, Baden-Württemberg (2016): "Vorläufige Anwendungshinweise des Ministeriums für Inneres, Digitalisierung und Migration zu §12a AufenthG," Tech. rep., Ministerium für Inneres, Digitalisierung und Migration, Baden-Württemberg.

Ministry of the Interior and Federal Affairs, Schleswig-Holstein (2016): "Praktische Umsetzung der gesetzlichen Wohnsitzverpflichtung nach §12a Abs.1 AufenthG," Tech. rep., Ministerium für Inneres und Bundesangelegenheiten, Schleswig-Holstein.

Organization for Economic Co-operation and Development (2016): "Making Integration Work: Refugees and others in need of protection," OECD Publishing.

——— (2017): "Finding their Way - Labour Market Integration of Refugees in Germany," Tech. rep., OECD.

Peri, G. (2016): "Immigrants, productivity, and labor markets," *The Journal of Economic Perspectives*, 30, 3–29.

Rosholm, M. and R. Vejlin (2010): "Reducing income transfers to refugee immigrants: Does start-help help you start?" *Labour Economics*, 17, 258–275.

The Expert Council of German Foundations on Integration and Migration (2016): "Ankommen und Bleiben. Wohnsitzauflagen als integegrationsfördernde Maßnahme," Policy Brief des SVR Forschungsbereichs 2016-6, The Expert Council of German Foundations on Integration and Migration.

——— (2017): "Fakten zur Aslypolitik - 1. Halbjahr 2017," Kurz und bündig, The Expert Council of German Foundations on Integration and Migration, aktualisierte Fassung.

Appendix

Section 12a, Residence rule

(Federal Ministery of Justice and Costumer Protection (2016), accentuation by the author)

(1) In order to promote their lasting integration into the way of life in the Federal Republic of Germany, foreigners who have been recognised as being entitled to asylum, having refugee status within the meaning of Section 3 (1) of the Asylum Act, who have been granted subsidiary protection within the meaning of Section 4 (1) of the Asylum Act or who have been granted an initial temporary residence permit pursuant to Section 22, Section 23 or Section 25 (3) shall be obliged to take up their habitual residence (place of residence) for a period of three years as from recognition or issuance of the temporary residence permit in that Land to which they have been allocated for the purposes of their asylum procedure or in the context of their admission process. *Sentence 1 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, or that person takes up or has taken up vocational training or is pursuing his studies or is in a training relationship.*

(2) A foreigner who is subject to the obligation under subsection 1 and who is living in a reception centre or other temporary accommodation may, within six months of recognition or admission, but no later than the expiry of the period referred to in subsection 1, be obliged, for the purpose of providing him with suitable accommodation, to take up residence in a specific place if this would not interfere with his lasting integration into the way of life in the Federal Republic of Germany. Insofar as, in an individual case, it was not possible to allocate suitable accommodation within six months, such allocation pursuant to sentence 1 may be made once within a further six months.

(3) In order to promote their lasting integration into the way of life in the Federal Republic of Germany, foreigners who are subject to the obligation pursuant to subsection 1 shall be obliged, within six months of recognition or the

first issuance of a temporary residence permit, but no later than the expiry of the period applicable in accordance with subsection 1, to take up residence in a specific place if this can help them

1. acquire suitable accommodation,

2. acquire sufficient oral command of the German language equivalent to Level A2 of the Common European Framework of Reference for Languages and

3. enter paid employment, taking account of the local conditions on the vocational training and labour market.

(4) Foreigners who are subject to the obligation under subsection 1 may, in order to prevent social exclusion, also be obliged until the expiry of the period applicable under subsection 1, not to take up residence in a specific place, in particular if it is to be expected that they will not use German as their main language of communication at that place. This decision shall take into account the situation of the local vocational training and labour market.

(5) An obligation imposed or allocation made pursuant to subsections 1 to 4 must be revoked upon application by the foreigner

1. if the foreigner furnishes proof, in the event of an obligation or allocation pursuant to subsections 1 to 3 to take up residence at another place, or in the event of an obligation pursuant to subsection 4 not to reside at a place, that

a) he or his spouse, registered domestic partner or minor child is in employment with full social security coverage within the meaning of subsection 1, sentence 2, has an income which secures his subsistence or a vocational training place or has been accepted to a higher education institution; or

b) his spouse, registered domestic partner or minor, unmarried children reside elsewhere,

2. to prevent hardship; in particular, hardship shall exist where

a) the competent youth welfare office estimates that the local child and youth welfare benefits and measures pursuant to Book Eight of the Social Code would be negatively affected,

b) acceptance by another Land has been confirmed on other urgent, personal grounds or

c) comparable unreasonable restrictions would arise for the person concerned on other grounds.

In the event of revocation pursuant to sentence 1 no. 2, the foreigner must be subject to an obligation pursuant to subsection 3 or 4, at most until the period referred to in subsection 1 expires, account having been taken of his interests.

(6) Where dependants subsequently immigrate to rejoin a foreigner who is subject to an obligation or allocation pursuant to subsections 1 to 4, the obligation or allocation shall also apply to the dependants subsequently immigrating at most until the period applicable to the foreigner pursuant to subsection 1 expires, unless the competent authority has ordered otherwise. Subsection 5 shall apply accordingly to the subsequently immigrating dependants.

(7) Subsections 1 to 6 shall not apply to foreigners who were recognised or initially granted a temporary residence permit within the meaning of subsection 1 before 1 January 2016.

(8) Objections and actions filed against obligations pursuant to subsections 2 to 4 shall have no suspensory effect.

(9) *With regard to foreigners who are subject to the obligation pursuant to subsection 1, the Länder may, by way of statutory instruments of the Land government or other Land regulations, issue regulations specifying the organisation, procedure and suitable accommodation relating to*

1. *their distribution within the Land pursuant to subsection 2,*

2. *the procedure for allocation and obligations pursuant to subsections 2 to 4,*

3. *the requirements as to suitable accommodation within the meaning of subsections 2, 3 no. 1 and subsection 5, sentence 1, no. 1 (a), as well as the form of its proof,*

4. *the manner of furnishing proof of employment with full social security coverage pursuant to subsection 1, sentence 2, income which secures subsistence, and of having a vocational training place or being accepted to a higher education institution within the meaning of subsections 1 and 5, sentence 1, no. 1, (a),*

5. *the obligation to be taken up by the municipality determined as his place of residence and the admission process.*

7.1 Figures

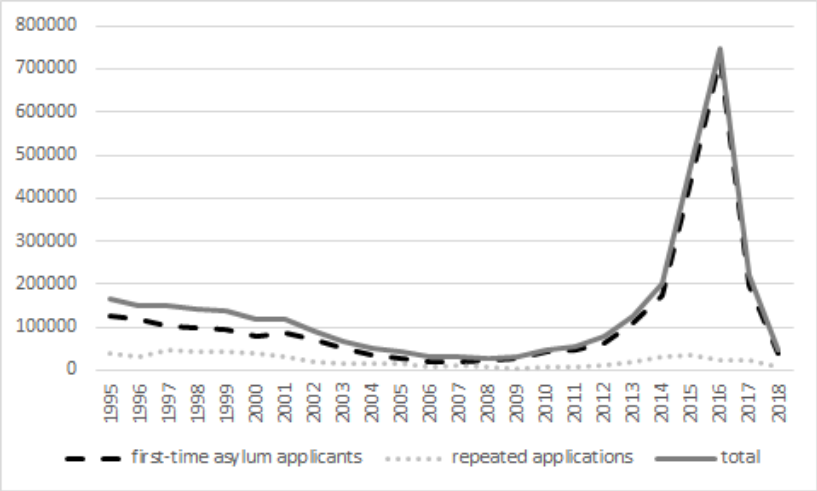


Figure 1: **Number of Asylum Applications**, BAMF (2018)

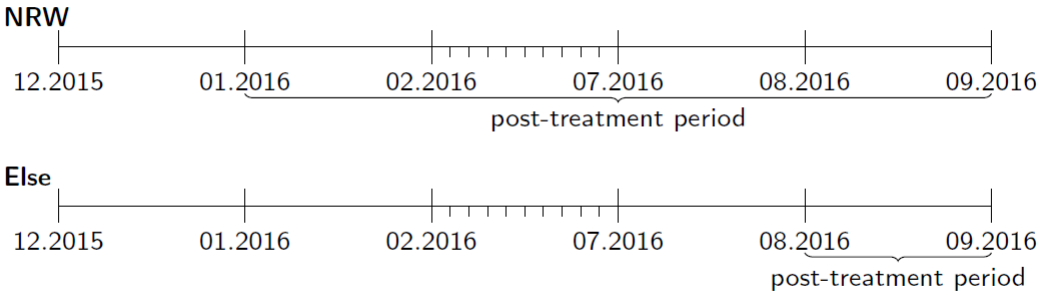


Figure 2: **Timeline: Introduction of the *residence rule***

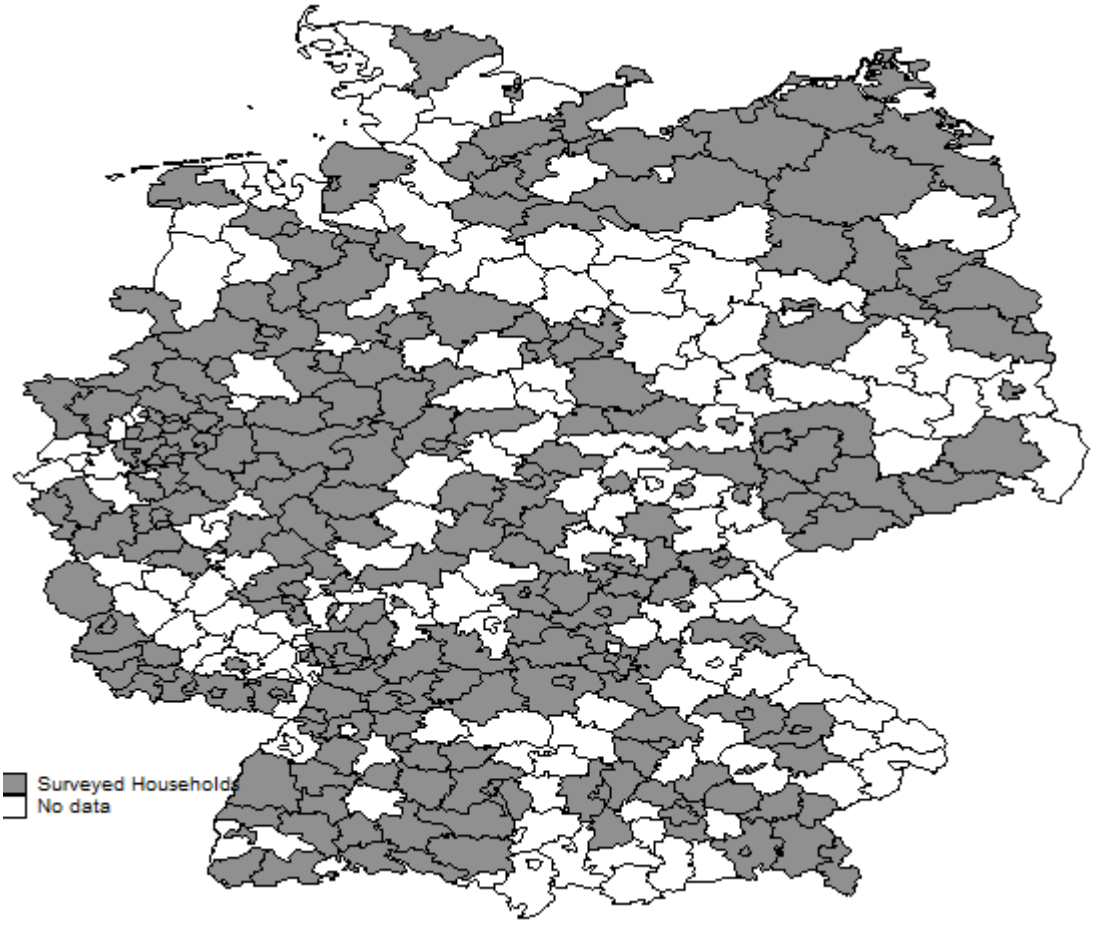


Figure 3: **Geographical Distribution of Households surveyed in the IAB-BAMF-SOEP Survey of Refugees**

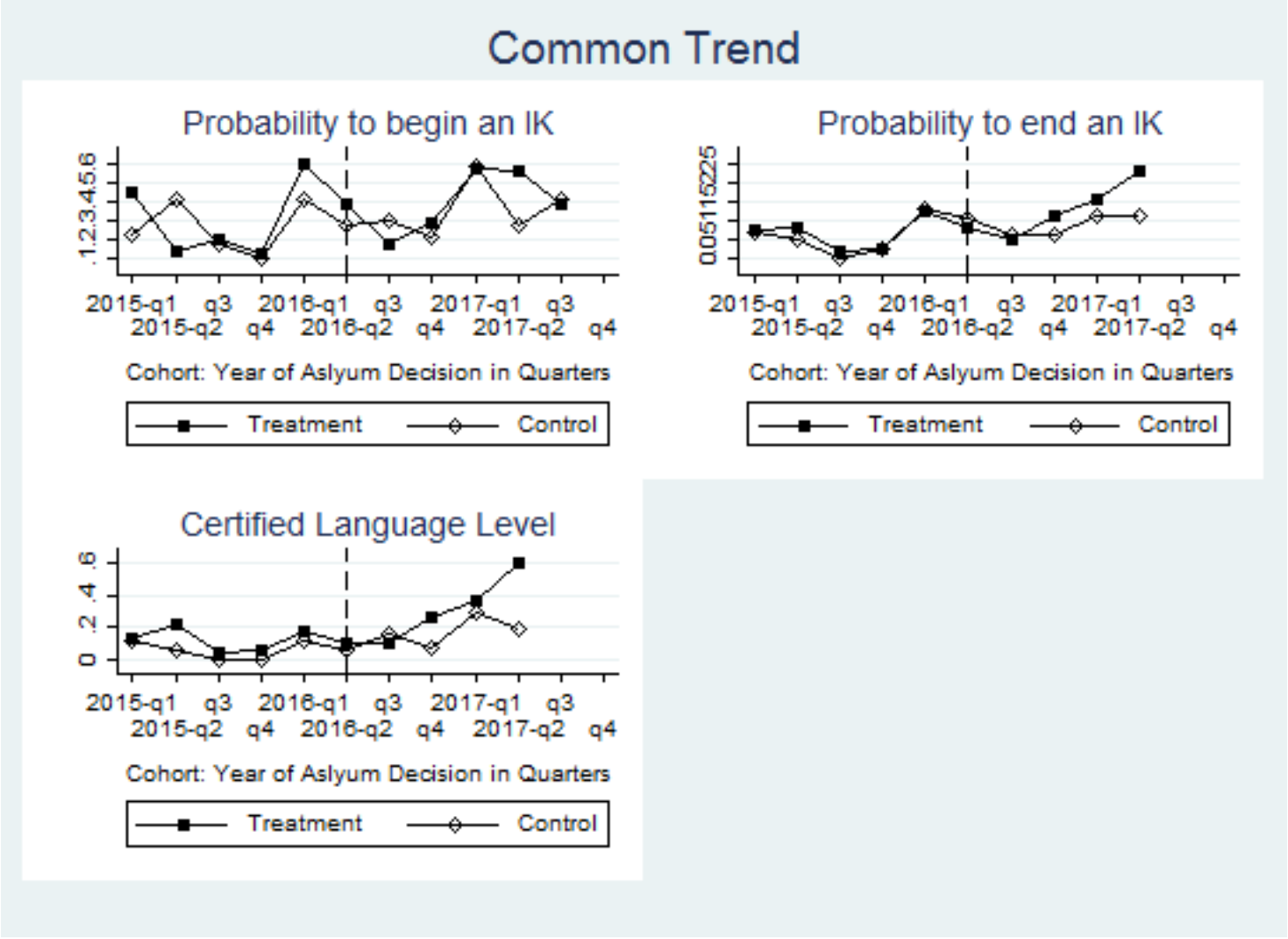
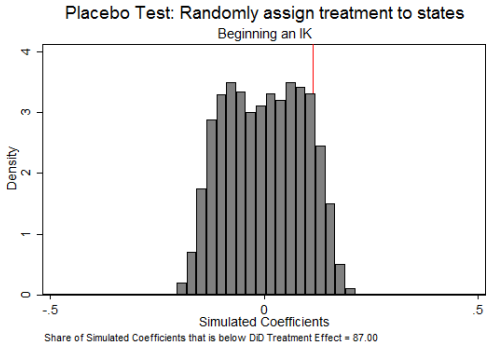
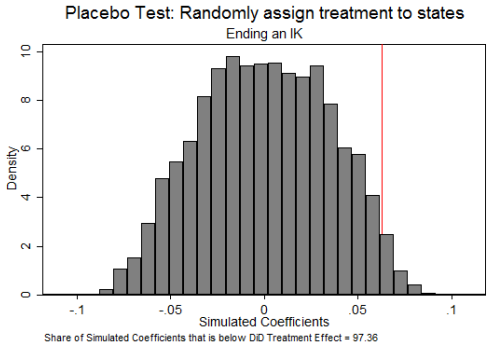


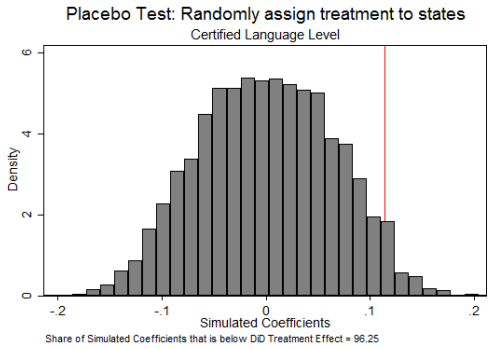
Figure 4: **Common Trend Assumption**



(a) Beginning an IK



(b) Ending an IK



(c) Language Level

Figure 5: **Placebo Test**

7.2 Tables

Table 1: **Königsstein Key, 2010-2017**

	2010	2011	2012	2013	2014	2015	2016	2017
Baden Wurttemberg	0,12804	0,12815	0,12931	0,13011	0,12975	0,12865	0,12967	0,13017
Bavaria	0,15123	0,15193	0,15225	0,15293	0,15330	0,15519	0,15533	0,15550
Berlin	0,05027	0,05038	0,05075	0,05025	0,05046	0,05049	0,05083	0,05093
Brandenburg	0,03122	0,03105	0,03072	0,03064	0,03081	0,03061	0,03037	0,03026
Bremen	0,00945	0,00931	0,00934	0,00947	0,00941	0,00957	0,00953	0,00951
Hamburg	0,02595	0,02545	0,02550	0,02544	0,02527	0,02530	0,02558	0,02558
Hessen	0,07205	0,07226	0,07302	0,07276	0,07316	0,07359	0,07399	0,07364
Mecklenburg-West Pomerania	0,02103	0,02082	0,02060	0,02051	0,02042	0,02029	0,02012	0,02002
Lower Saxony	0,09333	0,09314	0,09401	0,09362	0,09357	0,09321	0,09331	0,09366
North Rhein-Westphalia	0,21321	0,21442	0,21220	0,21235	0,21241	0,21210	0,21144	0,21144
Rhineland Palatinate	0,04816	0,04813	0,04808	0,04806	0,04835	0,04837	0,04831	0,04835
Saarland	0,01236	0,01231	0,01227	0,01230	0,01216	0,01222	0,01211	0,01203
Saxony	0,05225	0,05169	0,05144	0,05122	0,05101	0,05084	0,05056	0,05025
Saxony Anhalt	0,02968	0,02929	0,02908	0,02887	0,02858	0,02831	0,02799	0,02772
Schleswig Holstein	0,03345	0,03372	0,03364	0,03378	0,03388	0,03403	0,03391	0,03417
Thuringia	0,02833	0,02795	0,02779	0,02769	0,02748	0,02725	0,02695	0,02679

Source: `https://www.gwk-bonn.de/themen/finanzierung-von-wissenschaft-und-forschung/koenigsteiner-schluessel/`, last downloaded on July 2, 2018.

Table 2: **Classes of Protection in Germany**

	Class	Rate of Protection
1	Political asylum	0.70 %
2	Geneva Refugee Convention	20.50 %
3	Subsidiary protection status	16.30 %
4	Tolerated foreigners	6.60 %
	Rejections	55.90 %

Note: (BAMF, 2018, p.10)

Table 3: **Definition - Working Sample**

Original Sample: M3, M4, M5	6662
Missing information on spouse	-69
Repeated asylum request	-124
Missing: type of permit/timing of decision	-2805
Exemption rule	-1868
Asylum decision before 2015	-119
Missing values in explanatory variables	-237
N	**1440**

Table 4: **Descriptive Statistics**

	Controls		Treated	
	mean	Std.Dev.	mean	Std.Dev.
Female	0.23		0.21	
Age	30.69	10.54	29.87	10.25
Age at Migration	27.74	10.86	27.55	10.49
Dummy: Children	0.42		0.36	
Number of Children	1.16	1.80	0.99	1.73
Years of Schooling	9.52	3.98	9.42	4.10
Experience	0.74		0.72	
Future Employment	3.71	0.68	3.78	0.57
Active Search for Employment (4 weeks)	0.26		0.26	
Months since Arrival	16.06	5.95	16.87	5.78
Months since Asylum Decision	8.17	4.26	9.07	5.21
Some German before immigration	0.02		0.02	
Want to stay in Germany	0.95		0.92	
Exposure to Germans	3.47	1.91	3.31	1.85
UE Rate at regional level	7.67	2.85	7.6	3.53
Employment Status - Post-treatment				
Full Time	1.78		1.77	
Part Time	0.99		2.42	
Vocational Training	0.00		0.00	
Marginal Employment	2.59		2.53	
No Employment	93.82		91.23	
Internship	0.81		2.05	
Family Status				
Single	48.05		49.92	
Married	49.25		46.95	
Divorced	1.83		1.78	
Widowed	0.87		1.34	
Group of Countries				
[1] Syria, Iraque, Iran	77.08		86.84	
[2] Afghanistan, Pakistan, India	5.19		1.75	
[3] Africa	11.12		6.94	
[4] Western Balkan, former SU	0.11		0.30	
[5] Others	6.50		4.17	
Type of residence permit				
Political Asylum	10.35		8.09	
GFK	87.25		90.72	
§22 or 23	2.41		1.19	
N	697		743	

Table 5: **Effect on Beginning an IK**

Dep. Variable:	Baseline	Male	Without NRW	Without City States	Placebo
Beginning an IK	[1]	[2]	[3]	[4]	[5]
mean	0.3243056	0.3815789	0.3169479	0.3214551	0.3243056
[*i*]	0.086	0.142	0.072	0.089	0.188**
	(0.056)	(0.092)	(0.056)	(0.058)	(0.078)
Obs.	1440	988	1363	1347	1440
R^2	0.009	0.021	0.006	0.011	0.009
[*ii*]	0.072	0.131	0.065	0.069	0.159*
	(0.054)	(0.091)	(0.057)	(0.057)	(0.077)
Obs.	1440	988	1363	1347	1440
R^2	0.109	0.071	0.102	0.112	0.108
[*iii*]	0.113*	0.178*	0.086	0.111*	0.140
	(0.056)	(0.086)	(0.052)	(0.059)	(0.083)
Obs.	1440	988	1363	1347	1440
R^2	0.156	0.123	0.156	0.156	0.156

Notes: This table reports estimation results. The dependent variable measures the probability to take up an integration course in the year of asylum decision. For each sample, I estimate three alternative specifications: (i) without a set of controls, (ii) including time invariant control variables and (iii) including a full set of covariates.
Column (1) includes the baseline estimates using the full working sample. Column (2) explicitly focuses on male refugees only. Column (3) discards all observations in NRW who received a decision on their asylum claim from January to July 2017. Column (4) excludes city states. Column (5) includes a placebo test which simulates the reform was introduces in August 2015. Standard errors are clustered at the federal state level and given in parentheses. * 10 %, ** 5 %, *** 1 % significance level

Table 6: **Effect on Ending an IK**

Dep. Variable: *Ending an IK*	Baseline [1]	Male [2]	Without NRW [3]	Without City States [4]	Placebo [5]
mean	0.0861111	0.0910931	0.0829054	0.0838901	0.0861111
[*i*]	0.069***	0.095***	0.063***	0.069***	0.010
	(0.016)	(0.025)	(0.017)	(0.016)	(0.037)
Obs.	1440	988	1363	1347	1440
R^2	0.006	0.013	0.004	0.007	0.001
[*ii*]	0.067***	0.089***	0.063***	0.066***	0.011
	(0.016)	(0.024)	(0.017)	(0.018)	(0.035)
Obs.	1440	988	1363	1347	1440
R^2	0.029	0.030	0.027	0.033	0.024
[*iii*]	0.063***	0.084***	0.052***	0.068***	0.017
	(0.015)	(0.026)	(0.019)	(0.015)	(0.043)
Obs.	1440	988	1363	1347	1440
R^2	0.059	0.063	0.058	0.063	0.056

Notes: This table reports estimation results. The dependent variable measures the probability to complete an integration course in the year of asylum decision. For each sample, I estimate three alternative specifications: (i) without a set of controls, (ii) including time invariant control variables and (iii) including a full set of covariates.
Column (1) includes the baseline estimates using the full working sample. Column (2) explicitly focuses on male refugees only. Column (3) discards all observations in NRW who received a decision on their asylum claim from January to July 2017. Column (4) excludes city states. Column (5) includes a placebo test which simulates the reform was introduces in August 2015. Standard errors are clustered at the federal state level and given in parentheses. * 10 %, ** 5 %, *** 1 % significance level

Table 7: **Effect on Language Levels**

Dep. Variable: *Certified Language Levels*	Baseline [1]	Male [2]	Without NRW [3]	Without City States [4]	Placebo [5]
mean	0.14375	0.1487854	0.13573	0.1447661	0.14375
[i]	0.165***	0.197***	0.161***	0.170***	0.074
	(0.036)	(0.039)	(0.040)	(0.040)	(0.050)
Obs.	1440	988	1363	1347	1440
R^2	0.016	0.029	0.015	0.018	0.005
[ii]	0.160***	0.196***	0.156***	0.166***	0.056
	(0.040)	(0.042)	(0.043)	(0.046)	(0.049)
Obs.	1440	988	1363	1347	1440
R^2	0.050	0.053	0.048	0.055	0.042
[iii]	0.114***	0.141***	0.091**	0.125***	0.073
	(0.035)	(0.043)	(0.042)	(0.040)	(0.068)
Obs.	1440	988	1363	1347	1440
R^2	0.097	0.090	0.092	0.099	0.095

Notes: This table reports estimation results. The dependent variable measures the certified German language skills at the end of an integration course in the year of asylum decision. For each sample, I estimate three alternative specifications: (i) without a set of controls, (ii) including time invariant control variables and (iii) including a full set of covariates.
Column (1) includes the baseline estimates using the full working sample. Column (2) explicitly focuses on male refugees only. Column (3) discards all observations in NRW who received a decision on their asylum claim from January to July 2017. Column (4) excludes city states. Column (5) includes a placebo test which simulates the reform was introduces in August 2015. Standard errors are clustered at the federal state level and given in parentheses. * 10 %, ** 5 %, *** 1 % significance level

Table 8: **Effect on the Number of Course Providers per District**

Dep. Variable: *Number of course providers per district*	[i]	[ii]	[iii]
mean	1.853189	1.853189	1.853189
	0.626	0.645	0.402
	(0.288)	(0.269)	(0.207)
Obs.	1360	1360	1360
R2	0.04	0.05	0.29