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migrant labor supply – quasi-experimental
evidence from Germany*

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Heterogeneous displacement effects of migrant labor supply – quasi-experimental evidence from Germany

Mario Scharfbillig*, Marco Weißler[†]

July 30, 2019

Abstract

We provide estimates of the effect of migrant labor supply on resident employment. We exploit variation in the number of asylum seekers eligible to the suspension of a major hiring restriction implemented in a subset of German counties. Our difference-in-difference design allows us to provide evidence from a labor supply shock of migrants on local markets net of their additional spending at arrival that might mask labor market displacement effects. Despite this, we do not find a negative effect on employment growth of natives but only on other foreign residents. This also holds for unskilled employees. Therefore, our findings can be interpreted as the consequence of differential substitutability of different subgroups, where asylum seekers are substitutes to other immigrants but not natives - even when they are similarly qualified.

Keywords: asylum seeker, displacement, skill complementarity
JEL-Codes: J22, J61, R23

1 Introduction

There has been conflicting evidence on the question how the host country population is affected by migrant labor supply.¹ Even when studying the same incident like the Mariel boat lift, the literature finds heterogeneous results. While for instance Card (1990), Card (2009) and Peri and Yasenov (2019) find no significant effect of refugees on employment or wages, Borjas (2017) and Borjas

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¹We follow the convention in the literature using forced migration as a special case for general migration. Note that refugees may be different from other migrants, e.g. in terms of planned time horizon of stay (Cortes 2004). However, most differences pertain to forced migrants themselves and less so to their impact on the host country population.

and Monras (2017) find a negative effect on the employment and wages of natives. These contrasting results show that the findings are very sensitive to the estimation method used, with potential disagreements on the choice of control group (Peri and Yasenov 2019), survey choice (Clemens and Hunt 2019), the underlying model tested (Dustmann et al. 2016) and the validity of instruments, especially the shift-share designs (Adão et al. 2018; Borusyak et al. 2018; Broxterman and Larson 2018; Goldsmith-Pinkham et al. 2018; Jäger et al. 2018). Accordingly, the conflicting results may be explained by different specifications and or data availability, each with potentially violated assumptions. It is therefore essential to find clean and causal evidence for the influence of (forced) immigrants on the employment of other residents.

We are able to shed light on this important question using a quasi-experiment in Germany. In particular, we assess the effect of a sudden policy-induced migrant labor supply shock on employment of different subgroups in the labor market. In 2016 a subset of German counties suspended the "priority review" policy for recently arrived asylum seekers² that constituted a major labor market entry hurdle. The policy required an employee to obtain a permit by the German Federal Employment Agency testifying that no direct displacement of an EU citizen, nor non-EU citizen with permanent residence permit, occurs due to the hiring. Additionally, the policy prohibits employment of asylum seekers in temporary employment agencies.

The policy change allows us to employ a difference-in-difference approach to assess the displacement effect of dropping these restrictions for asylum seekers on employment growth in the county after the suspension relative to counties that did not drop the restriction. We show that the counties experienced a similar economic performance and nearly parallel employment growth rates prior to the policy change. We also follow Dustmann et al. (2016) and eliminate county-specific heterogeneity between suspending and non-suspending counties by estimating the effect on growth rates controlling for county-specific trends and monthly time effects.

Furthermore, the more than 200,000 asylum seekers who were affected by the policy change were randomly allocated across counties already prior to the policy change. While they were allocated immediately after arrival, they were allowed access to the labor market only three months after their arrival after passing the "priority review" (see Section 2 for details). Therefore, we are able to provide evidence on the impact of a large labor supply shock for migrants who already lived in a predetermined location in Germany. As there are implicit and explicit restrictions to movement of asylum seekers even after allocation, our results should be less plagued by endogenous location choice.

This also means our setting provides us with the unique opportunity to identify solely the effect of a labor supply shock net of demand effects of arrival. Due to the lag between allocation and the policy change we do not eval-

²The term "refugee" is often used as a summary term for displaced individuals. In this paper, the term is used for individuals who received a decision on their asylum requests and were officially recognized by their host country. The term "asylum seeker" is used for individuals who requested asylum but are still waiting for the decision on their refugee status.

uate the migration effect, but a pure labor market effect of migrants who were already residents of the county. The additional spending by and on arriving migrants might drive additional demand effects, which could mask any potentially adverse effect of labor market competition.³ Instead, our estimates include only consumption effects due to a higher income directly from employment.

More specifically, we exploit two sources of variation in the number of asylum seekers eligible to the policy change for identification. First, some states decided not to suspend the hiring restriction in a subset of counties. This opt-out decision was mostly based on the current unemployment and long-term unemployment rate in the respective state. However, this opt-out decision was not taken on the federal level, i.e. not for all of Germany, but on the state level and limited to a subset of states. This allows us to use the rest of the German counties as a valid control group with similar economic conditions. We further estimate our model controlling for level and trend differences between counties and validate our approach using placebo tests to verify that we are not picking up diverging trends prior to the policy change.

Second, the number of eligible asylum seekers varies randomly across counties. Asylum seekers are allocated randomly across states according to fixed quotas and irrespective of their individual characteristics. While the allocation *between* states is based on tax potential and population of that state, *within* states, county population was the only relevant criterion (see Czerny 2018). The total number of eligible asylum seekers varies across counties as it depends on individual characteristics like nationality, age and arrival date that are not criteria for allocation. Hence, controlling for state characteristics, random within-state variation of eligibility can be used to analyze heterogeneous intensities of the labor supply shock.

We make use of these two types of variation between suspending and non-suspending counties to estimate the total effect of asylum seekers' labor market access on employment growth of natives as well as other foreign residents. In order to answer our question, we employ registry data for all of Germany, such that we are immune to survey composition problems (Clemens and Hunt 2019). Using this data, which includes all dependent employment in Germany besides public servants, we are able to look at the impact on several subgroups, such as employees with different skill levels and nationalities. Hence, we are able to analyze employment effects for resident foreigners, which might be closer substitutes for migrants.

We find heterogeneous effects for different subgroups, shedding new light on the previously conflicting findings in the literature. While we do not find a negative effect on employment growth of natives, employment growth of res-

³When immigrants arrive their immediate consumption, e.g. of food and housing, can lead to employment growth due to additional local demand (e.g. Bodvarsson et al. (2008) on the Mariel boatlift). Their consumption is mostly financed by government transfers. The effect on local demand and employment is analyzed by a growing literature on local fiscal multipliers (e.g. Serrato and Wingender 2016; see Fuchs-Schündeln and Tarek (2016) for an overview). See Czerny (2018) for an estimate of the various multiplier effects of the recent refugee wave in Germany.

ident foreigners is negatively affected by the policy change. More precisely, we find that for up to twelve months after the policy change treated counties experience an additional monthly employment growth of asylum seekers of 0.66 percentage points (pp) or 8.2pp annually. This is a sizable effect relative to an average monthly (annual) employment growth rate of 3.61% (53.0%) in non-suspending counties in the treatment period. The policy change leads to a decline of employment growth of other foreigners by around 0.12pp monthly or 1.49pp annually relative to a positive annual growth of 10.3% in non-suspending counties. We validate our results with a series of robustness checks to confirm that the effect was solely driven by the policy change.

In addition, we compare workers across similar skill levels to assess a more direct labor market competition among similarly qualified employees. We find that the effects of the policy change are most pronounced for unskilled asylum seekers and foreigners. These foreigners likely compete in similar labor market segments as most asylum seekers and might have similar (high) levels of substitutability. The effect still remains insignificant for natives.

Using the random variation in the eligibility of asylum seekers across counties as weights, we are able to identify a weighted treatment effect in a similar fashion as Acemoglu et al. (2004). For skilled natives an increase in the treatment intensity significantly increases the employment effect. This means that asylum seekers seem to be rather complements to skilled workers. Nevertheless, the weighted effect for unskilled natives is also positive and marginally significant, strengthening our findings that there seems to be no displacement even of low skilled natives.

Overall, our results suggest that the heterogeneous displacement effect is not only driven by foreigners working in different labor market or skill segments than natives. Instead, they are also closer substitutes for employees with similar qualification. Therefore, our findings can be interpreted as the consequence of differential substitutability of different subgroups, where asylum seekers are closer substitutes to immigrants than natives even among similar skill levels.

Our results contribute to the literature in three ways. First, we provide causal evidence on the debate of whether migrants or refugees are close substitutes to native employees or not without the need to shift-share instrumentation (Peri and Yasenov 2019; Borjas and Monras 2017; see Tumen (2016) for evidence on refugees). We provide evidence for the latter, showing that immigrants are closer substitutes to other immigrants rather than natives (see also Card 2009), which is in line with findings in Ottaviano and Peri (2012), Peri (2012) and Peri and Yasenov (2019). The result is even more important because we cannot find a displacement effect on natives despite the absence of migration-induced consumption increases. Reasons for our findings may be different levels of substitutability and complementarity based on language differences (Clausen et al. 2009; Foged and Peri 2016; Lewis 2011) or differently valued human capital (Peri and Sparber 2009), for which we provided tentative evidence later in the paper.

Secondly, we contribute to the literature by analyzing intensity effects, which

is not possible using shift-share designs. When using the intensity estimates, we show that skilled natives benefit from immigrant employment even more if there are more asylum seekers on the labor market. This finding is in line with other findings on job complementarity of low skilled immigrants and natives (e.g. D’Amuri and Peri 2012).

Finally, to the best of our knowledge, we are the first to show an employment substitution effect in the labor market between recently arrived migrants and resident foreigners for different skill groups within a local labor market. Because most other papers in this area are limited to survey data or the overall share of foreigners in the labor market is small, they lack power to identify heterogeneous effects for foreigners and natives (see e.g. Glitz 2012). In Germany, foreigners make up on average 10.4% of all employees at the time of the policy change. It is therefore feasible to run separate regressions on foreigners using registry data for all of Germany. This may also explain the difference between our findings and other papers. Because the foreign population is large compared to other shocks analyzed, they might absorb the shock without natives being affected.

In the next section, we explain the institutional background of the allocation and labor market access of asylum seekers in Germany. This is crucial for our identification procedure, which we explain in more detail afterwards. Then we describe our regression design and the data we use. Finally, we present our main results and the results of various robustness checks. A short conclusion summarizes our findings and discusses opportunities for additional research.

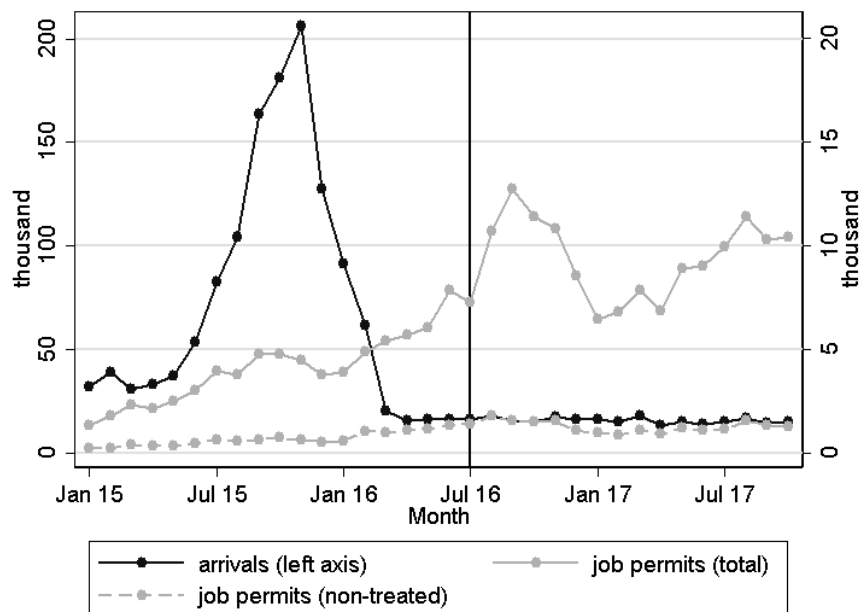
2 Labor market access for asylum seekers

Among European countries Germany received most asylum seekers in the recent refugee wave. Figure 1 shows the inflows of asylum seekers into Germany over time and number of employment permits of individuals from the eight most important countries of origin of asylum seekers. The inflow peaked in the second half of 2015, with more than 200,000 arrivals in November 2015. After 2015 the numbers dropped considerably and leveled off at prior arrival rates of around 15,000 per month. Overall, around 1.1 million individuals applied for asylum in Germany in 2015 and 2016 (BAMF 2017).

Asylum seekers are not allowed to work directly after their arrival. Instead, they are allowed to work only three months after arriving in Germany.⁴ They require a permit by the German Federal Employment Agency (BA; “Bundesagentur für Arbeit”), which testifies that no direct displacement occurs. This so-called priority review (PR; “Vorrangprüfung”) requires each employer prior to hiring an asylum seeker to indicate that neither an EU citizen nor non-EU

⁴Given the prevalence of current inflows and similar inflows of asylum seekers in the past, many countries opted for restricting labor market access to asylum seekers. The duration asylum seekers have to wait to gain full labor market access varies across Europe from one day in Sweden up to twelve months in France and the UK (Hainmüller et al. 2016) with potentially negative effects on their integration in the long-run (Marbach et al. 2018 for evidence on Germany).

Figure 1: Inflow and job permits of asylum seekers



Note: Asylum seeker inflow into Germany as of end of a month based on EASY registrations (left axis) and accepted job permits of asylum seekers in Germany (right axis). Asylum requests in 2015 and early 2016 lagged behind actual arrival time due to delays in the possibility to officially request asylum during the initial peak of arrivals. Therefore, the figure counts registrations according to the EASY-system that only monitors country entries until 2017 and asylum requests afterwards. The black vertical line marks the last observation before the policy change. Job permits are in absolute numbers in all counties (total) and in non-suspending counties (non-treated). Source: German central registry of aliens (arrivals) and Statistics of the Federal Employment Agency (permits).

citizen with permanent residence permit (§ 39 AufenthG) can fill the position. The policy also entails a ban on working for temporary employment agencies, an important sector for migrants in the German labor market.⁵

At the same time, many newly arrived asylum seekers stated the importance of quickly finding a job to cover their immediate needs, e.g. for consumption and debt payments (Brücker et al. 2016b). 97% of men and 85% of women answer they “most certainly” or “likely” want to work in Germany in the future. The policy therefore was binding for many asylum seekers and induces costs and uncertainty for employers willing to fill a position with an asylum seeker.

The PR was obligatory for all asylum seekers only during the first 15 months after their arrival (as well as for individuals with a temporary “suspension of deportation status”). Afterwards, the PR was no longer required. However, the vast majority of asylum seekers arriving during the recent refugee wave was still affected by a policy change in 2016.⁶

Our identifying policy change occurred on 6th of August 2016, when the PR was suspended in 133 out of 156 districts of the BA, composed of 339 out of 401 counties⁷, for a preliminary period of three years.⁸ In suspending counties, asylum seekers and individuals with a temporary “suspension of deportation status” were allowed to work three months after their arrival in Germany without the requirement of a PR.⁹ Therefore, the policy change strongly reduced hiring requirements.

We observe an indicative reaction to the policy change in Figure 1 above. The figure shows approved employment permits on the right axis. There is an immediate surge of approved employment permits by asylum seekers after the policy change, which is sustained through the second half of 2016. At the same time, there was no significant increase in permits in counties that were exempt from the policy change (see dashed line in Figure 1). The numbers then drop considerably, partially due to seasonal factors. In the second quarter of 2017, employment growth increases again, mostly because for asylum seekers

⁵7% (12%) of all (unskilled) foreigners in June 2016 worked for a temporary employment agency, but only 2% (6%) of German employees.

⁶The average waiting time for an asylum decision in Germany was five months in 2015 and increased up to twelve months in the second half of 2017 (BAMF 2016). In addition, due to delays in the possibility to officially request asylum during the initial peak of arrivals, the filing of asylum requests in 2015 and early 2016 lagged behind actual arrival time. Hence, the duration between the arrival in Germany and the asylum decision was even longer.

⁷Germany is composed of 13 federal states and 3 city states (Hamburg, Bremen, Berlin). The states are composed of a total of 401 counties with an average size of more than 100,000 employees. The Federal Employment Agency has 156 local offices, which are responsible for multiple such counties.

⁸See “Integrationsgesetz” (law specifying the temporary suspension of PR; http://www.bgb1.de/xaver/bgb1/start.xav?startbk=Bundesanzeiger_BGB1&jumpTo=bgb116s1939.pdf) and “Vierte Verordnung zur Änderung der Beschäftigungsverordnung” (for the list of employment districts; <https://www.ewbund.de/files/2016-08-06-vierte-verordnung-aenderung-beschaef-tigungs-verordnung.pdf>)

⁹They still require an employment permit as the BA checks their potential job for “similarity of working conditions”, which is intended to prevent their exploitation by inspecting the enforcement of safety regulation and minimum wage payments.

arriving in 2015 the 15 month period until the PR is no longer required was over (see Figure A1). However, there were some exceptions to the eligibility of the policy change which we will make use of in our estimation below.

First, some counties were exempt from the PR suspension in order to "consider regional labor market conditions appropriately" (BAMF 2016). Apart from the original exceptions, no further counties have been exempted. Figure 2 shows the distribution of suspending counties within Germany. Treatment regions are slightly clustered, also because the exceptions were based on district level of the BA consisting of on average 2.6 counties. We acknowledge this by clustering standard errors on district level in the regressions. Furthermore, we run placebo regressions randomizing treatment districts across Germany to assess the validity of our results (see Section 6.1). In the next section, we further provide some evidence as to the question which economic factors were associated with the exceptions and how we are able to use the non-suspending counties as a valid control group.

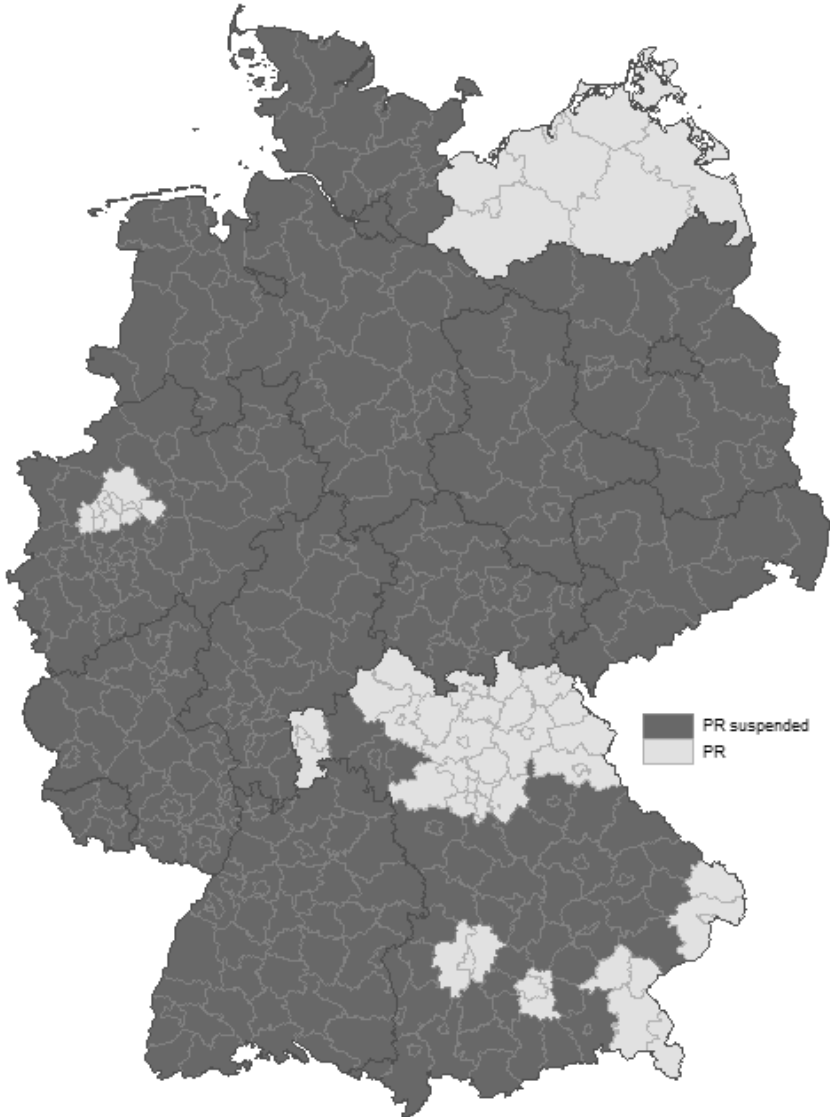
Second, individual characteristics of asylum seekers determined eligibility for the policy change and potential employment take-up. Eligibility depends on the arrival time (3-15 months prior to the policy change) and country of origin. Nationals from "safe countries of origin" ("sichere Herkunftsländer")¹⁰ were excluded from the policy change as they are not allowed to work during their asylum process. In addition, persons below the age of 15 are generally forbidden to take up employment in Germany. However, those characteristics did not determine their prior allocation to the counties affected by the policy change (see next paragraph). It was unknown to policy makers prior to the allocation as well as prior to the policy change, which asylum seekers were available for the labor market (e.g. because of their age, nationality or arrival time) and how they were qualified. Representative survey data were only available in November 2016, three months after the policy change took effect (Brücker et al. 2016a). Hence, we are able to use this variation in eligibility to assess the impact of different sizes of labor supply shocks.

We define the size of the labor market shock as the share of eligible asylum seekers in working age relative to total employment in a county at the time of the policy change. This is independent of labor market conditions as asylum seekers are not allowed to choose their county of residence, but were allocated to the 16 federal states within Germany by a pre-specified state quota. Afterwards, restrictions to movement and place of residence are in place.¹¹ The al-

¹⁰These included all countries of the European Union, Ghana, Senegal, Albania, Bosnia and Herzegovina, Kosovo, Macedonia, Montenegro and Serbia in August 2016.

¹¹Before they take up employment, asylum seekers have to remain available for the administration of their asylum request. In particular, there are residence obligations ("Residenzpflicht") in the respective state for the first three months of stay and residence requirements ("Wohnsitzauflage") for the first three years of stay if living costs are not covered without state assistance (e.g. by employment income). These regulations were further extended by the law regulating the suspension of the PR ("Integrationsgesetz"). In addition, many asylum seekers have very low wealth positions, which further hinders their movement (Brücker et al. 2016a). A survey among case workers in employment agencies finds besides a lack of skills, limited mobility to be a main constraint for labor market opportunities of asylum seekers (Dietz et al. 2018).

Figure 2: Counties affected by the policy change



Note: Map of German counties that did suspend PR (dark) after 6th of August 2016 and these who did not suspend it (light). Thick lines mark state borders. Source: German Federal Ministry of Labour and Social Affairs (BMAS).

location quota, the so-called "Königsteiner Schlüssel", is a weighted average of state population and tax payments in the previous year. Therefore, it depends on the economic situation of federal states. Within a state, asylum seekers are allocated to counties using a state specific key primarily based on population - with few exceptions unrelated to economic performance.¹² Hence, controlling for state-level differences insures an allocation exogenous to economic factors (see Section 3.2).

Figure 3 provides some indication for the effect of the policy change for asylum seekers. It shows the number of acceptances and rejections of asylum seekers' employment requests. In the year before the policy change more than 1,000 employment permits per month were rejected on the basis of PR regulation by the BA while 5,000 were granted.¹³ After the policy change, the number of rejections in PR suspension counties immediately dropped to zero. The number of accepted permits, which for PR states after August 6th 2016 represent the applications for the check of "similarity of working conditions" only, surged after the policy change.

3 Estimation strategy

3.1 Regression model

We employ a difference-in-difference (DID) strategy to identify the effect of the PR suspension on employment of asylum seekers, natives and other foreign nationals. We focus on employment instead of wages for multiple reasons. First and foremost, we do not have data on county level for average wages in all German counties. As some counties have only a small number of employed, wages are not reported by the BA due to data protection regulations. Second, we expect the downwards pressure on wages to be rather limited. As we look at a short time horizon of around one year after the policy change, wage changes might not be prevalent yet. In addition, observable downwards pressure on wages is limited by minimum wages¹⁴ as well as the checks for the "similarity of working conditions" by BA.¹⁵

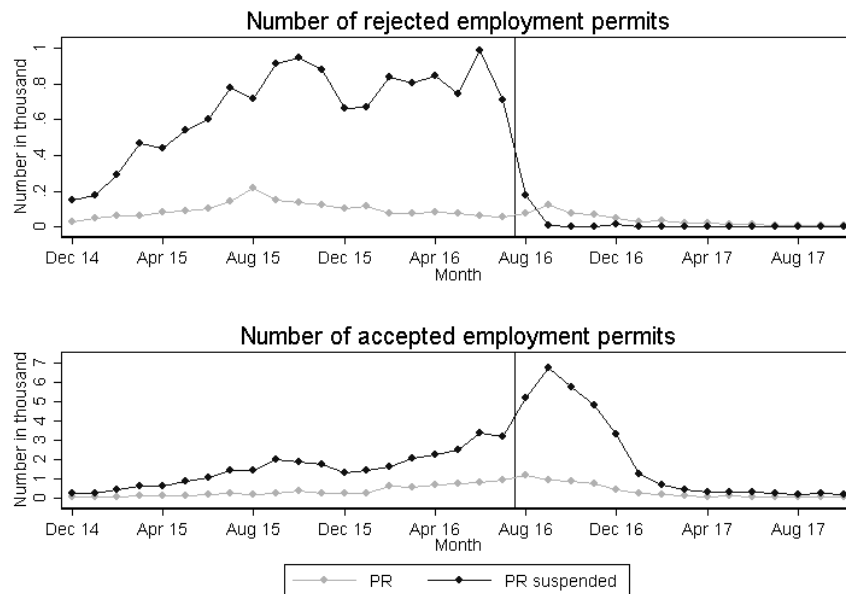
¹²In the state of Nordrhein-Westfalen and Brandenburg 10% of the allocation is based on the area, while in Mecklenburg-Vorpommern the allocation is adjusted for the presence of Jewish communities for historic reasons (see Czerny 2018). Hessen adjusts the allocation based on the share of foreigners living in the counties already, which might have an effect on our estimations. In a robustness check, we exclude Hessen without an effect on our results.

¹³This does not constitute an overall rejection rate as the granted applications are only the application with regards to PR. Work permits may be rejected for other reasons (e.g. "similarity of working conditions"). Additionally, some applications might be discouraged altogether due to the high bureaucratic burden. Hence, these numbers constitute a lower bound of the overall employment discouragement.

¹⁴Since 2015 there is a general minimum wage in Germany. In addition, there are minimum wage agreements in single firms (collective agreements) and different sectors, mostly in various handicrafts and unskilled services (e.g. temporary agency work).

¹⁵Accordingly, previous research has not found strong short-term wage effects of immigration shocks specifically for Germany (Glitz 2012; Brücker et al. 2014).

Figure 3: Rejected and accepted employment permits



Note: Number of rejected (§39(2) BeschV) and accepted (§32(1) BeschV) employment permits for asylum seekers between 3 and 15 months after arrival as of the end of a month. Employment permits represent combined audits of PR and similarity of working conditions. The black line represents the total number in all counties that suspended the PR, the gray line represents the total number within all other counties. The black vertical line marks the date of the policy change. Source: Statistics of the Federal Employment Agency.

More explicitly, we use monthly employment growth per county as our main outcome variable. We specify the model linearly and estimate it by OLS with fixed effects on county level:

$$\Delta L_{it}^g = \alpha_i^g + \theta_t^g + \beta^g D_{it} + \epsilon_{it}^g \quad (1)$$

where ΔL_{it}^g is the employment growth relative to the previous month of subgroup g (natives, asylum seekers, other foreigners) in county i at time t .¹⁶ D_{it} is the dummy variable for the suspension of priority checks and is one for counties suspending the priority review after July 2016 and zero otherwise. Hence, β^g is the difference-in-difference estimator. We run the regression separately for different subgroups. Therefore, unlike skill-cell approaches, we estimate the total effect of the policy change for each subgroup including indirect effects through wage changes or employment shifts.

Similar to Dustmann et al. (2016), we estimate the effect in growth rates to eliminate county-specific trends and weight observations by the total population in the county in 2015. Employing county- (α_i^g) and month- (θ_t^g) fixed effects enables us to flexibly account for unobserved heterogeneity in employment growth within subgroups. Because the treatment is on employment agency district-level, we use cluster standard errors on district level also allowing for serial correlation in the error terms.¹⁷

We use data from July 2014, two years prior to the policy change, until July 2017 one year after the policy change. We focus on the two years before the policy change in August 2016 as previous years might be affected by other policy changes.¹⁸ Furthermore, we end our analysis one year after the policy change as the overall effect is fading out over time. Due to the stark reduction in asylum seeker inflow in 2016, by July 2017 most asylum seekers stayed in Germany for more than 15 months and therefore were not subject to the PR even if they were allocated to non-suspending counties.¹⁹ We analyze the dynamics of this fading-out process in the results section.

In addition, we analyze labor market outcomes for counties with different treatment intensities (see e.g. Acemoglu et al. 2004). To this aim, we estimate

¹⁶Because the employment of asylum seekers is zero in a small number of counties especially at the beginning of our time period, we use the $\ln(x+1)$ transformation, to avoid varying numbers of observations. This imputation does not qualitatively change our results. As a robustness check, we use the inverse hyperbolic sine (IHS) transformation instead and find qualitatively unchanged results.

¹⁷We also try clustering the errors on county levels and find no qualitative differences.

¹⁸Restrictions on labor market access were considerably loosened in a first stage in November 2014 by allowing asylum seekers to work already 3 months after their arrival subject to a PR and dropping the additional requirement of a PR 15 months after their arrival ("Gesetz zur Einstufung weiterer Staaten als sichere Herkunftsstaaten und zur Erleichterung des Arbeitsmarktzugangs für Asylbewerber und geduldete Ausländer").

¹⁹Note that the policy change only affects asylum seekers before they receive a response on their asylum request. Therefore, due to the reduction of asylum seeker inflows, the power of the policy shock between affected and not affected counties fades out in the months after the policy took effect. Because most asylum seekers had received a decision on their asylum request by then, they no longer needed working permits.

the model with a weighted treatment term. Using the shares of eligible asylum seekers at the time of the policy change relative to total employment in a county as weights allows us to evaluate different intensities of the labor supply shock on employment growth.

This follows the specification of Dustmann et al. 2016 who set up a model where labor demand is an aggregate of different skill groups. They assume that natives and migrants are assumed to be perfect substitutes within each skill group, hence identification of labor supply elasticities comes through variation in skill supply by migrants and natives if wages are rigid. In contrast to their approach, we do not employ an instrumental approach for identification but exploit the differences in spatial variation of the policy change, captured by D_{it} , and the random variation in the eligibility of asylum seekers. Hence, we estimate the following specification:

$$\Delta L_{it}^s = \tilde{\alpha}_i^s + \tilde{\theta}_t^s + \tilde{\beta}^s D_{it} + \tilde{\beta}_w^s D_{it} w_i + \tilde{\epsilon}_{it}^s \quad (2)$$

where w_i is the demeaned share of eligible asylum seekers in June 2016 relative to total employment subject to social security contributions in the county in June 2016. Using this estimation strategy, we are able to identify the marginal effect of higher shares of asylum seekers on the labor market to assign part of the between-variation of counties to the size of the labor supply shock. The coefficient of interest here is $\tilde{\beta}_w^s$ which shows the effect of the size of the labor supply shock on employment growth of different subgroups.²⁰ We consider other definitions of the weighted labor supply shocks in various robustness checks.

3.2 Identification

In order to identify the effect, we need to show that conditional on our specification, counties would develop similarly in absence of the policy change. As we do not have this counterfactual data, we need to assure that conditional on our specification and given the information prior to the policy change we could expect the counties to develop similarly in absence of the policy change.

The employment agency districts exempt from the PR suspension were not randomly selected. They were selected by state governments based on labor market outcomes.²¹ Therefore, they had worse labor market conditions within their state but not within the whole country (see Section 4.3). This allows us to

²⁰As w_i does not vary within county, $\tilde{\beta}_w^s$ estimates only a between variation in $\tilde{\beta}^s$ across treated counties (i.e. if $D_{it} = 1$). Hence, a demeaned w_i leaves $\tilde{\beta}^s$ the same as in Equation (1).

²¹Exceptions were only made in three states: Nordrhein-Westfalen, Bayern and Mecklenburg-Vorpommern. In Nordrhein-Westfalen counties with an unemployment rate of more than 25% above the state average and with high long-term unemployment rates were exempt (<https://www.landtag.nrw.de/portal/WWW/dokumentenarchiv/Dokument/MMD16-13691.pdf>). In Bayern all counties with an above average unemployment rates were exempt (http://www1.bayern.landtag.de/www/ElanTextAblage_WP17/Drucksachen/Schriftliche%20Anfragen/17_0018660.pdf). In Mecklenburg-Vorpommern all counties were exempt from the suspension.

construct a valid control group by using all districts across Germany that were not affected by the PR suspension. We use growth rates of the dependent variable to eliminate any unobserved heterogeneity and level effects as e.g. structural differences, location or agglomeration effects that may have influenced the decision. In addition, we estimate the effect on county level. While overall an employment district might have worse labor market conditions, there is still variation within employment districts. Hence, we need to ensure that controlling for level differences leaves us with two comparable sets of counties in suspending and non-suspending employment districts.

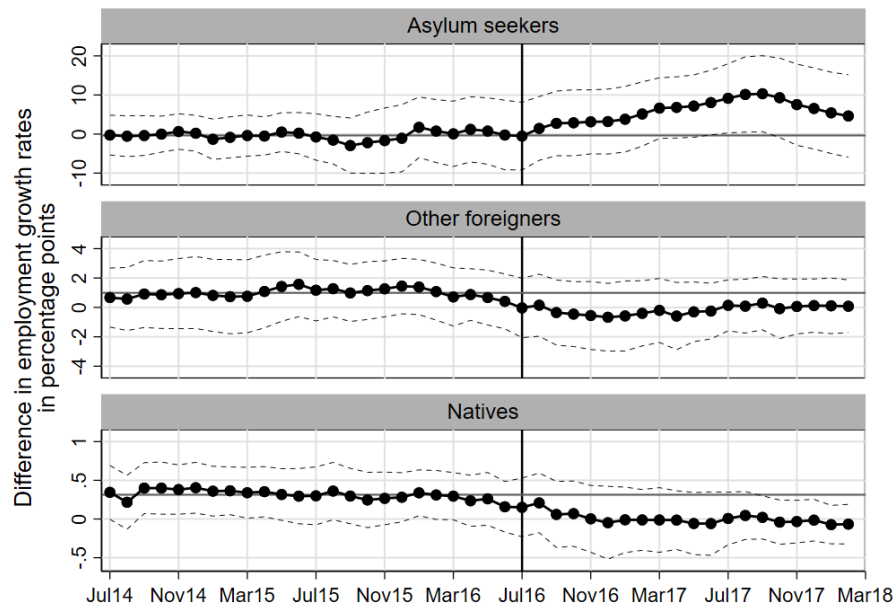
Because we use only a short period of analysis, it is unlikely that counties underwent structural changes apart from the policy change. Our approach is therefore valid if the policy decision is independent of (i) county trends potentially driving the PR suspension decision as well as (ii) the allocation of asylum seekers in Germany. In the following, we provide evidence to support the validity of our identification procedure.

First, we check for parallel trends of employment growth in PR suspending and non-suspending counties before the policy change. In Figure 4, we show the differences in employment growth relative to the previous year in percentage points (pp). Employment growth differences for all groups vary narrowly and non-systematically around their respective means prior to the policy change. After the policy change, the employment growth difference for asylum seekers strongly increases from a pre-change average of -0.4pp to around 10.3pp 14 months after the intervention, only to revert back at the end of our time window. This reversion represents the diminishing number of eligible asylum seekers as their inflow ceases and their stock shifts closer to the 15 month limit at which the PR requirement is no longer binding even in counties without the PR suspension. For natives and other foreigners, the employment reaction points in the opposite direction. The figure suggests a substitutive nature between the employment of asylum seekers and other employees, which we further examine in the following sections. The overall much lower growth rates relative to asylum seekers are due to their higher absolute employment levels (see Table 3 below).

Second, we investigate the correlation of county differences with the policy decision prior to the enactment of the policy change. To this end, we use county characteristics from the end of 2015, and data on the stock of asylum seekers in July 2016. Details on the data are described in the data section below. The Logit estimation summarized in Table 1 shows average marginal effects of the potential drivers of the decision to suspend the PR. The dependent variable is a dummy equal to one if a county was affected by the policy change and zero otherwise. Model (1) and (2) test potential drivers of the suspension in levels while model (3) additionally includes the same control variables in one year differences (growth rates from 2014 to 2015), which is the relevant benchmark for us.

The share of asylum seekers in a county at the time of the policy change is measured relative to total employment subject to social security contributions in June 2016. Its coefficient is never significant and therefore the share

Figure 4: Employment growth trend differences



Note: Difference in growth rates of employment subject to social security contributions in PR and PR suspended counties by nationality in percentage points. Growth rates are as of the end of a month relative to the previous year. Dashed lines are 95% confidence intervals. Gray horizontal lines mark the average before the policy change; black vertical lines mark the last month before the policy change in PR suspending counties at the end of July 2016. Source: Statistics of the German Federal Employment Agency.

Table 1: Predicting priority review exemption

	(1)	(2)	(3)
Share Asylum Seekers (in %)	0.631 (0.6210)	0.544 (0.7234)	0.478 (0.7078)
Unemployment Rate (in %)	0.204 (0.1468)	0.132 (0.1274)	0.134 (0.1306)
Long-Term Unemployment (in %)	0.447** (0.1904)	0.467** (0.2033)	0.431** (0.2071)
Total Population (log)		0.012 (0.3271)	-0.014 (0.3194)
East Germany (d)		1.142 (0.7691)	1.527 (0.9567)
Urban County (d)		0.436 (0.4761)	0.344 (0.4994)
GDP per capita (log)		0.112 (0.6058)	-0.055 (0.6313)
Share Foreign Population		1.042 (4.7414)	-3.717 (4.6442)
Δ Long-Term Unemployment			3.890 (2.7974)
Δ Unemployment Rate			-1.118 (6.7681)
Δ Total Population (log)			88.110 (55.6447)
Δ GDP per Capita (log)			4.785 (4.4183)
Δ Share Foreign Population			-0.950 (1.4613)
N	388	388	388
Pseudo R2	0.043	0.062	0.100

Note: Average marginal effects from a Logit estimation. Dependent variable is a dummy equal to one if PR was suspended in a county. Share Asylum Seekers is the proportion of asylum seekers in a county at the time of the policy change relative to the employment subject to social security contributions in 2015. East Germany is a dummy equal to one if the county is in one of the five states in East Germany. Unemployment Rate is the within-county unemployment rate. Long-Term Unemployment is the share of individuals with an unemployment duration of more than 12 months in a county. Total Population is the logarithm of the county population, Urban County a dummy equal to one if a county is classified as a urban area by the German Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR). GDP per capita is the logarithm of the average GDP per capita in a county. Share Foreign Population is the share of individuals with a foreign nationality relative to the total population within a county. Variables with Δ are one year differences. All independent level variables are as of end of 2015. Standard errors in parentheses and clustered at the state level. The significance levels are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

seems not to be a driving force of the policy decision. This hints to the fact that, residual variation in the eligibility of asylum seekers was not a driving force of the policy decision. When including additional economic and other population related variables, only the long-term unemployment rate is significant. A higher rate is correlated with a lower probability to suspend the PR in a county. Hence, if asylum seekers are closer substitutes to long-term unemployed, we might overestimate the displacement effect. Finding a null effect on natives seems therefore even more surprising.²²

Adding these variables in one-year-differences to the model in column (3) of Table 1, the coefficients are never significant. Hence, besides level differences in long-term unemployment rates, previous economic trends seem not to be correlated with the policy decision. Hence, estimating our model on growth rates makes us confident that we do not capture non-random heterogeneity in our main regression.

Third, we investigate the driving forces of the allocation of asylum seekers and if they adhere to the legal requirements described in the part above. If more asylum seekers were systematically allocated to counties with worse (better) economic performance, we may over(under)-estimate any potential adverse effect of their employment growth on other labor market participants due to different dynamics. In Table 2, we assess the drivers of the allocation of asylum seekers on the county level in a simple OLS framework. The dependent variable is the logarithm of eligible asylum seekers per county and the independent variables are the same as in Table 1 above. Models (1) and (2) in Table 1 are estimated without and models (3) and (4) with state-fixed effects. State-fixed effects should capture differences in tax potential between states, which are part of the overall allocation key as explained above.

Throughout all models, total population is highly significant with a coefficient of around 1. This means that asylum seekers were on average distributed according to the population shares. When including additional control variables in model (2) of Table 1 East Germany counties and counties with a large GDP per capita received more asylum seekers relative to their population. This effect is due to the allocation across states (and therefore between East and West Germany) being partially based on tax revenues. When controlling for state-fixed effects, neither the East Germany dummy nor the GDP level are significant and all coefficients are closer to zero. Therefore, the allocation of asylum seekers seems to be consistent with legal requirements across Germany and variations not to be tied to economic conditions.

²²When randomizing treatment in Section 6.1 below, we still find the adverse effect on other foreigners to be marginally and the coefficient for natives not to be significant.

Table 2: Predicting the distribution of eligible asylum seekers

	(1)	(2)	(3)	(4)	(5)
Total Population (log)	0.992*** (0.0496)	1.028*** (0.0993)	0.998*** (0.0563)	0.986*** (0.1315)	
East Germany (d)		0.313** (0.1246)		-0.038 (0.0849)	0.014 (0.0157)
Urban County (d)		-0.000 (0.1035)		0.020 (0.0814)	-0.009 (0.0117)
Area (log)		0.034 (0.0651)		0.025 (0.0694)	-0.001 (0.0141)
Unemployment Rate (%)		0.007 (0.0210)		0.001 (0.0327)	0.002 (0.0072)
LT Unemployment (%)		-0.686 (0.6818)		0.110 (0.7934)	0.144 (0.1918)
GDP per capita (log)		0.402** (0.1634)		0.303 (0.1824)	0.052 (0.0379)
Share Foreign Population		-1.625 (2.3244)		-1.392 (3.5929)	-0.067 (0.4238)
State FE	No	No	Yes	Yes	Yes
N	388	388	388	388	388
R2	0.543	0.579	0.645	0.651	0.207

Note: Estimates from an OLS regression. Dependent variable is the logarithm of asylum seekers per county in columns (1)-(4) and the share of asylum seekers as of total population in column (5). Total Population is the logarithm of the county population. East Germany is a dummy equal to one if the county is in one of the five states in East Germany. Urban County a dummy equal to one if a county is classified as a urban area by the BBSR. Area is the log of the area of the county in square kilometer. Unemployment Rate is the within-county unemployment rate. LT Unemployment is the share of individuals with an unemployment duration of more than 12 months among all unemployed. GDP per Capita is the logarithm of the average GDP per capita in a county. Share Foreign Population is the share of individuals with a foreign nationality relative to the total population within a county. Models (3)-(5) include state-fixed effects. Standard errors in parentheses are clustered at the state level. The significance levels are * p<0.10, ** p<0.05, *** p<0.01.

4 Data

4.1 Employment

We analyze the effect of the PR suspension on employment growth by nationality using county-level administrative data. The German Federal Employment Agency (BA) provides this data on a monthly basis. The dataset covers the complete universe of dependent employment in Germany and contains a nationality variable allowing us to distinguish natives and individuals with other nationalities. It does not include data on self-employed, unpaid family workers and public servants, but it includes other employees in the public sector. Overall it covers around 80% of total employment in Germany. In our baseline specification we estimate the effect of the labor supply shock on employees who are subject to social security contributions.²³

The BA does not collect (comprehensive) information on the legal status of an employee, which hinders us from identifying asylum seekers directly in the employment data. Instead, we follow the approach of the BA using foreigners from the eight countries of origin with the highest numbers of asylum seekers (Afghanistan, Eritrea, Iraq, Iran, Nigeria, Pakistan, Somalia, Syria) as a proxy. Foreigners from these countries filed nearly 80% of all asylum requests in 2016 (BAMF 2017). If previously arrived individuals from these countries are close substitutes, we will therefore rather underestimate the true effect of the policy change on asylum seekers alone. However, this does not affect our estimates for other subgroups.

In addition, we analyze the effect on jobs with different skill requirements separately to find the group mostly affected by asylum seekers labor supply. Research that finds displacement effects of refugees' or immigrants' labor market access on natives often identifies a displacement of similar workers, for instance low qualified natives (Borjas and Monras 2017). At the same time, employment of complementary skill groups (e.g. high qualified natives) increases, which may be based on skill complementarities between these different skill groups.

Skill levels of jobs are reported by the employer. There are four different skill requirement levels which are based on the education level "usually required for the position" according to the most recent German occupation classification system (KldB 2010, "Klassifikation der Berufe 2010"): no requirement (unskilled), professional education, graduate education and post-graduate education. We aggregate the highest three skill levels (skilled) to be in line with Dustmann et al. (2016) and because the number of observations in the highest two categories on the county level are small (only 15.8% of all foreigners have

²³Employees subject to social security contributions make up the vast majority of the workforce (86% in 2015). The rest includes employees with very short job duration (less than three months or 70 days per calendar year) or earning less than 450EUR per month (which corresponds to 53 hours of work at the minimum wage of 8.50EUR). In June 2016 for instance 39% of all asylum seekers in such jobs were employed in the hospitality sector relative to only 17% of these with jobs subject to social security contributions.

a graduate education or more in June 2016).

In a robustness exercise, we compare these results with the respective ISCED education levels, which are also reported by employers. We follow Eurostat and combine ISCED 0 to 2 (No High School) and ISCED 3 and above (High School or more). However, the comparison of skills as defined by KldB 2010 and ISCED level is not one-to-one. In addition, relative to skill requirements, data on education levels are potentially less reliable, especially when reported inconsistently by different employers as is likely the case for asylum seekers. Furthermore, reporting is non-mandatory and hence there are many missing observations. In June 2016 skill levels are available for more than 99% of all employees, but ISCED levels only for 80% of all employees.

4.2 Treatment dummy and intensity

For the treatment variable D_{tj} we generate a dummy, which is one after August 2016 if the county is part of an employment agency district for which the PR was suspended. This allows us to estimate the overall policy effect of the suspension of the PR on the labor market outcomes in suspension counties relative to non-suspension counties.

For the treatment intensity w_i in Equation (2) we make use of different numbers of eligible individuals n_j in each county j in the first treatment month.²⁴ These are all asylum seekers and individuals with a temporary "suspension of deportation status" who arrived in Germany between 3 and 15 months prior to August 2016 and who are not nationals from "safe countries of origin" (Balkan²⁵, EU or other European nationalities). They were allowed to work but subject to the PR.

The Central Foreigner Register (AZR; "Ausländerzentralregister") of the Federal Office for Migration and Refugees (BAMF; "Bundesamt für Migration und Flüchtlinge") provides these data by nationality and arrival date as well as residence status, age and gender on county level. To assess different sizes of labor supply shocks, we concentrate on the asylum seekers who are more likely to work. Therefore, we consider individuals of working age (18-65 years). Furthermore, we weight the absolute number of eligible asylum seekers n_j by the total employment subject to social security contributions in the county. Hence, the shock is measured as a relative share or intensity.

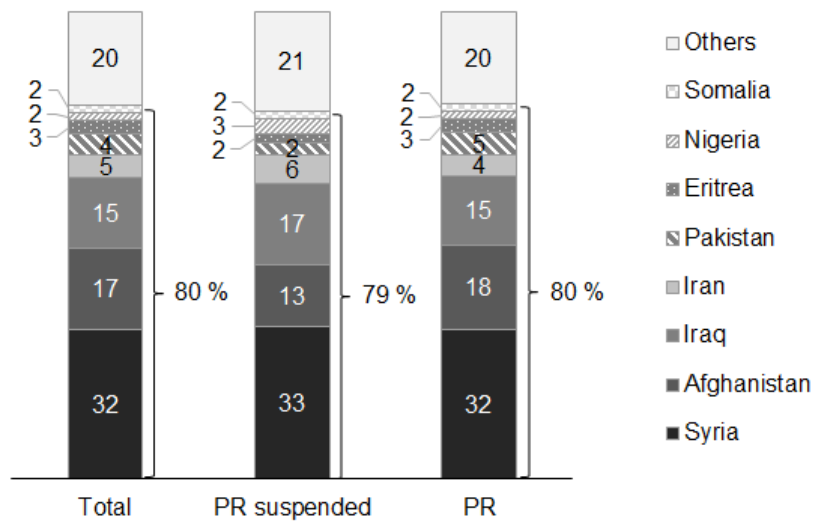
In absolute numbers, we identify around 234,000 individuals who arrived in the relevant period (3-15 months before the policy change) and are of work-

²⁴The exact number of asylum seekers is only available as of end of June 2016. After this date, each month more asylum seekers dropped out of our sample as they received their acceptance or rejection decision than new arrivals entering the dataset. Hence, when using June data instead of July data, we slightly overestimate the shock size and therefore underestimate the effect size.

²⁵Asylum seekers from the Western Balkans are not allowed to work during their asylum process as they have little chances to be granted refugee status as they mostly arrive from "safe countries of origin" ("sichere Herkunftsländer"). Furthermore, they were affected by a contemporaneous policy change. Hence, we exclude them from the main analysis. As a robustness check, we include them and find unchanged results (see Figure A2).

ing age (18-65).²⁶ 203,470 lived in PR suspension counties and therefore where affected by the policy change. Out of these 27% are female, 10% individuals with a temporary “suspension of deportation status” and 60% in-between age 25 and 54. Figure 5 displays the nationalities of the asylum seekers potentially eligible for the policy change due to their date of arrival and current status of their asylum process. The average distributions in PR and non-PR counties are very similar.

Figure 5: Nationalities of asylum seekers in PR and non-PR counties

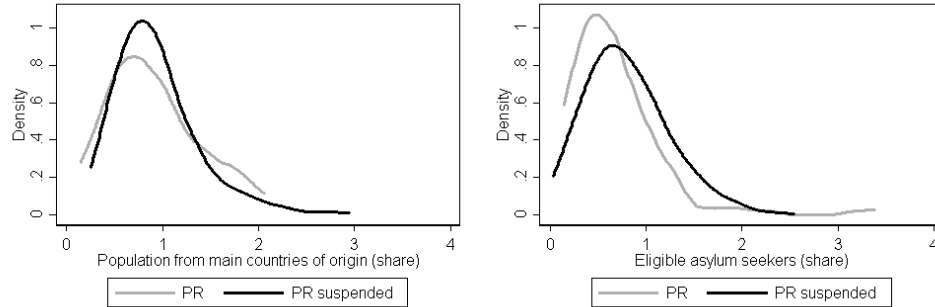


Note: Shares of asylum seekers in PR and PR suspending counties eligible to the policy change in August 2016 by nationality. Separate countries are the eight main countries of origin in Germany in 2015, while Others is a combined category for all other nationalities. Source: German central registry of aliens (AZR).

However, the share of eligible individuals ranges from 0.1% up to 3% of total employment subject to social security contributions in a county (see right panel in Figure 6), since important factors influencing eligibility and potential labor supply (arrival time, age and country of origin) were not considered in the allocation process or policy change. Hence, we end up with a considerable variation of eligible individuals across counties. The left panel in Figure 6 displays the total population from the eight main countries of origin relative to the overall population. Overall, the distribution of asylum seekers seems similar across PR and PR suspending counties. This also underlines that existing variation in eligibility and therefore treatment intensity might be uncorrelated

²⁶The 234,000 eligible asylum seekers in our sample are out of a total 1.1 million asylum applications in Germany in 2015/2016 (BAMF 2017). The size of the shock is in the medium range of previously studied refugee shocks, e.g. compared to 120,000 refugees in the Mariel Boat Lift, 250,000 refugees from the Yugoslavian War, 500,000 exodus Soviet Israelis, and 1.5 million refugees from the Algerian War (Borjas and Monras 2017).

Figure 6: Asylum seeker distribution across counties



Note: Fractions of counties according to total (left) and total eligible (right) number of asylum seekers. The panels show shares (in %) relative to total population (left) and relative to total employment subject to social security contributions (right) as smoothed kernel density estimates. Source: German central registry of aliens (eligible asylum seekers), German Federal Statistical Office (population) and Statistics of the Federal Employment Agency (total employment).

to the suspension decision.

4.3 Sample selection and description

We have to drop 13 of the 401 German counties due to data issues. We exclude all six counties in the state of Saarland, the county and city Kassel, Spree-Neise-Kreis and city Cottbus as they jointly manage and report the asylum seekers (as a matter of fact all foreigners) and therefore no county data on asylum seekers is available. Furthermore, we exclude the county Göttingen (city Göttingen and county Osterode am Harz) as there was a county reform in November 2016 that renders the data incompatible. In addition, we exclude the county Saale-Holzland-Kreis and the city Schweinfurt as they have by far the lowest (0% or 1 person) and highest (4%) shares of eligible asylum seekers relative to total employment due to specific conditions in these counties. However, as these counties are very small (0.09% and 0.17% of total employment in June 2016, respectively), including them in our sample does not change our results. Overall, we end up with 388 counties in our final sample, 327 of which suspended the priority review and 61 did not.

Table 3 shows the county characteristics for our treatment and control group. County characteristics are very similar for all variables across the two groups. The main significant difference is a higher average long-term unemployment rate among PR suspending counties. While there 34.1% of all unemployed are unemployed for more than one year, this share is only 30.6% in PR counties. Hence, as discussed above, although the exceptions were based on higher relative (long-term) unemployment shares in certain BA districts, overall the PR suspending counties were a more adversely selected sample. Besides this,

there is no evidence of an adverse selection of counties that kept the PR.²⁷

Below the employment shares and employment growth rates for the year before the policy change are shown. The vast majority of employees works in skilled jobs, with 84% and 83% in PR and PR suspending counties respectively. Although there are significant differences between both county groups (+/- 1pp), the economic significance of this difference is rather limited. The employment growth across treatment and control group is again similar, with large differences between the nationalities reflecting the different sizes of the population. For example, the average asylum seeker employment growth in PR (suspending) counties is 41% (40%), while it is only around 1% for natives in both groups.

5 Results

We report the main result of our paper in Table 4. The table shows the regression specification on the different treated groups from Equation (1). As in all the following tables, each coefficient reports the β^g from a separate regression on the respective subgroup. We find a strong positive effect of the suspension of PR on the employment growth of asylum seekers. Their employment growth rate increases on average by 0.66pp per month after the policy suspension, relative to an average growth rate of 3.61% in non-suspension counties. Annually, the effect translates into 8.2pp additional employment growth relative to 53.0% average growth rate in non-suspension counties. The coefficient may still understate the full effect for asylum seekers, because our asylum group also includes other nationals from these countries. Hence, it might also include prior migrants not subject to the policy change. Still, finding an effect here allows us to draw conclusions of potential substitution or complementarities between asylum seekers and the resident employees.

The effect on natives shows a negative sign but is not significant. This is despite the estimates are without direct positive employment effects out of additional consumption out of wealth and transfers by asylum seekers. Therefore, our estimates are closer to actual employment effects of asylum seekers (incl. additional consumption out of employment income) than previous studies that also captured potential demand effects of arrival. For foreigners, however, the effect is still negative and significant at the 5% level with an employment growth rate difference of -0.12pp relative to an average monthly growth rate of 0.82% in non-suspending counties. Annually, the effect is equivalent to a lower employment growth of 1.49pp per year relative to an average 10.3% employment growth in non-suspending counties. Because employment levels of natives are significantly larger (see Table 3), the employment growth between different groups are not directly comparable.²⁸

²⁷Similar results can be found for differences in trends (see Table A1). Besides a slightly lower population growth in non-suspending counties, there are also no differences in growth rates between 2013 and 2015.

²⁸Our results are robust to trimming of counties with the highest and lowest unemployment

Table 3: Sample characteristics

	no PR	PR	Diff.
Total Population (log)	12.00 (0.04)	11.85 (0.09)	-0.15*
East Germany (d)	0.20 (0.02)	0.13 (0.04)	-0.07
Urban County (d)	0.51 (0.03)	0.41 (0.06)	-0.10
Unemployment Rate (in %)	5.95 (0.15)	6.13 (0.45)	0.18
Long-Term Unemployment (in %)	34.13 (0.44)	30.55 (1.16)	-3.59***
GDP per Capita (log)	3.47 (0.02)	3.50 (0.05)	0.03
Asylum Seekers (share in %)	0.94 (0.02)	0.91 (0.06)	-0.02
Foreign Population (share in %)	9.24 (0.29)	9.44 (0.75)	0.20
<i>Employment Shares (by skill level)</i>			
- Unskilled	0.17 (0.00)	0.16 (0.00)	-0.01**
- Skilled	0.83 (0.00)	0.84 (0.00)	0.01**
<i>Employment Growth (by nationality)</i>			
- Asylum Seekers	0.40 (0.01)	0.41 (0.03)	0.01
- Natives	0.01 (0.00)	0.01 (0.00)	-0.00
- Other Foreigners	0.12 (0.00)	0.13 (0.01)	0.01
Observations	327	61	388

Note: The table shows county characteristics averaged over treatment (PR suspended) and control (PR) group as of end of 2015, except for Asylum Seekers (June 2016) and Employment Growth. Share Asylum Seekers is the proportion of asylum seekers in a county at the time of the policy change relative to the employment subject to social security contributions in 2015. Total Population is the logarithm of the county population. East Germany is a dummy equal to one if the county is in one of the five states in East Germany. Urban County a dummy equal to one if a county is classified as a urban area by the BBSR. Area is the log of the area of the county in square kilometer. Unemployment rate is the within-county unemployment rate. Long-Term Unemployment is the share of individuals with an unemployment duration of more than 12 months among all unemployed. GDP per Capita is the logarithm of the average GDP per capita in a county (in thousand Euro). Foreign Population is the share of individuals with a foreign nationality relative to the total population. Employment Shares are based on employment subject to social security contributions. Skill levels are based on BA classification (KldB 2010): no requirement (unskilled) and professional education or above (skilled). Employment growth is annual growth from 06/2015 to 06/2016. Asylum Seekers are all individuals working in a county from the eight main countries of origin. Natives are all German nationals. Other Foreigners include all other nationalities (excluding West Balkan nationals). The significance levels are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: DID regression on employment growth

	Asylum	Native	Foreign
	(1)	(2)	(3)
DiD Overall	0.6585** (0.2810)	-0.0161 (0.0124)	-0.1247** (0.0601)
County FE	Yes	Yes	Yes
Date FE	Yes	Yes	Yes
N	14356	14356	14356
Unique N	388	388	388
Non-treated mean	3.61	0.09	0.82

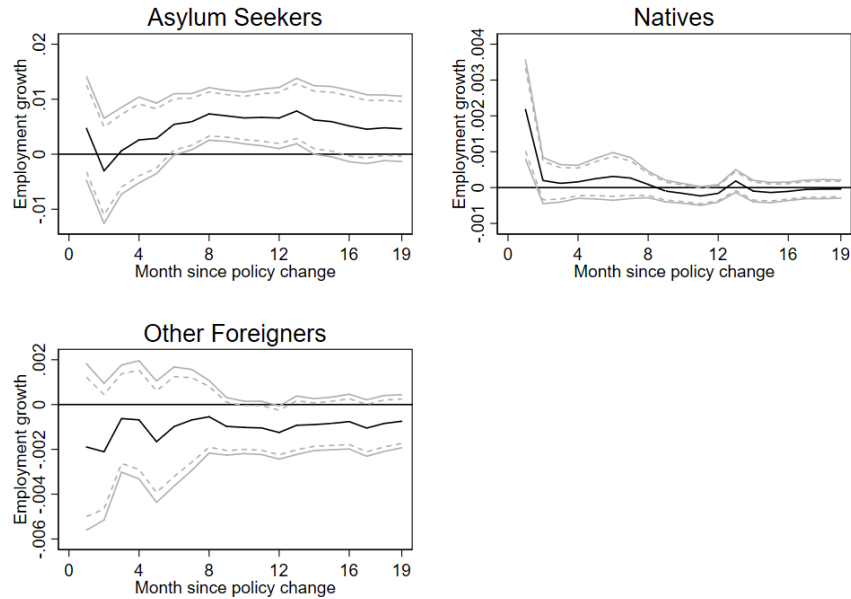
Note: Estimates from an OLS regression. The dependent variable is monthly employment growth in percentage points per nationality. Each column represents a different nationality group, column (1) for the eight main countries of origin, column (2) for Germans and column (3) for all other nationalities except Western Balkan. DID is a dummy variable equal to one in counties which suspended PR after the date of the policy change. All regressions contain county- and month-fixed effects. "Non-treated mean" are the average monthly employment growth rates in PR counties after the PR suspension. Observations are weighted by county population and from 07/2014 to 07/2017. Standard errors are in parenthesis and clustered on employment district level. The significance levels are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

What we can do instead is to calculate the counterfactual additional (reduced) employment of foreigners (asylum seekers) in case the PR was not suspended. However, our results are not precise enough to make an accurate statement on the direct displacement effect. Relative to a total employment of foreigners (asylum seekers) of 2,280,000 (84,000) in PR suspending counties just before the policy change, we would expect 34,000 (6,900) more (less) employed over one year after the policy change. However, if we consider the large confidence intervals, the range of additional employment of foreigners (1,900-65,000) and asylum seekers (1,100-13,000) allows for displacement ratios of asylum seekers to other foreigners of 100:15 up to 100:5,900.

In contrast to previous studies we are able to use monthly data. This enables us to assess the effect over time in the short run. This is particularly important as we might only have a temporary effect on employment growth due to the 15 months limit on the PR in all German counties. At the same time, the effect may last longer as forming employment matches is time-consuming and counties not affected by the policy change may lag behind. If the suspension gave firms a head start in dealing with asylum seekers, for example by providing firms with the opportunity to build up specific skills in hiring and processing asylum seekers applications and training, this could lead to a longer lasting effect. To this end, we provide estimates for a flexible range of the policy estimate for up to 19 months, which is the most recent data available.

rates and dropping the two largest city states Berlin and Hamburg which also accommodated most eligible asylum seekers.

Figure 7: Treatment effect over time



Note: Treatment effects on monthly employment growth for different nationalities with changing time length of the treatment period. Estimates from a difference-in-difference estimate with county- and time-fixed effects. The upper left panel is the effect for the eight main countries of origin, the upper right panel for natives and the lower left panel for all other nationalities (except Western Balkan). The solid black lines represent the estimates for a give month length, the gray lines represent the 90% (dashed) and 95% (solid) confidence intervals using clustered standard errors at the employment district level.

Figure 7 provides our estimates for different time windows. To this aim, we run consecutive regressions of model (1) and in each run add a further month of data to the end of the time frame.²⁹ For asylum seekers the effect picks up after the third month and becomes significant at the 5% level after the sixth month. Afterwards, it declines slightly towards the end of our time frame. For natives the effect is clearly not different from zero for the whole estimation period and frequently switches signs. This is further indication of no substitution effect between asylum seekers and natives. For other foreigners the effect is significant at the 5% level only one year after the policy change. Nevertheless, the point estimate is negative for the whole period but seems to reduce towards the end of the estimation period.

²⁹Figure A2 shows the monthly employment growth rates for each month in the first year after the policy change separately, instead of the aggregate results in Figure 7.

Table 5: DID regression on employment growth by skill level

	Asylum	Native	Foreign
	(1)	(2)	(3)
DiD Unskilled	0.7663* (0.4317)	-0.0540 (0.0581)	-0.2898* (0.1574)
DiD Skilled	0.4833** (0.2136)	-0.0146 (0.0127)	-0.0106 (0.0433)
County FE	Yes	Yes	Yes
Date FE	Yes	Yes	Yes
N	14356	14356	14356
Unique N	388	388	388
Non-treated mean (unskilled)	4.47	0.18	1.09
Non-treated mean (skilled)	3.08	0.07	0.67

Note: Estimates from an OLS regression. The dependent variable is monthly employment growth in percentage points per nationality and skill group. Each column represents a different nationality group, column (1) for the eight main countries of origin, column (2) for Germans and column (3) for all other nationalities except Western Balkan. Each row represents a different estimation for skill subgroups within the respective nationality. Skill levels are based on BA classification (KldB 2010): no requirement (unskilled) and professional education or above (skilled). DID is a dummy variable equal to one in counties which suspended PR after the date of the policy change. All regressions contain county- and month-fixed effects. "Non-treated mean" are the average monthly employment growth rates in PR counties after the PR suspension. Observations are weighted by county population and from 07/2014 to 07/2017. Standard errors are in parenthesis and clustered on employment district level. The significance levels are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.1 Results by skill level

We further split the employment of each group into skill levels in Table 5. We find that the effect of the policy on employment growth of asylum seekers is larger for unskilled and skilled, although only marginally significant for unskilled. Given the larger coefficient for unskilled asylum seekers and similar total employment of skilled asylum seekers (52.6% in June 2016) and unskilled (47.4%), the main labor supply shock is the additional employment of unskilled asylum seekers. This type of shock makes our effect similar to other migration shocks studied in the literature (e.g. Borjas and Monras 2017).

The coefficient signs on previous residents depend on the substitutability between different groups. Individuals present at the time of the shock and with similar skill levels than those from the employment shock, in this case the unskilled, should be negatively affected. This effect is predicted based on the assumption of direct job competition. At the other end of the skill distribution, residents could benefit from the supply shock due to skill complementarities.

For other foreigners the results seem to be consistent with previous findings. Unskilled foreigners bear the largest negative effect with -0.29pp monthly

employment growth, and the coefficient is marginally significant. The effect on the skilled foreigners is close to zero and not significant. For natives we find the same pattern, but it is never significant on any level. This means that, at least for our setting, there is no significant effect on employment growth of natives even when disaggregating employment into homogeneous skill levels. The result is important when contrasting it with previous findings in the literature. The implications of our findings are strengthened by the fact that there is no significant effect even when disregarding positive effects by additional consumption out of wealth and transfers due to migration, which are usually included in other settings. Substitutability between asylum seekers and the native population does not seem to be very high.

Overall, the results found in our main analysis confirm the empirical findings in papers referring to Peri and Yasenov (2019) rather than Borjas and Monras (2017). Facilitating labor market access for asylum seekers leads to substitution effects. Those are however primarily borne by other foreigners and apparently not by natives. This could either be due to similar skills or preferences of recent migrants and resident foreigners. As they have or lack similar human capital, e.g. language skills or networks, their competition on the labor market might be more direct. This can also be seen when looking at the sectors, in which asylum seekers take up employment. Figure A4 shows the correlation of the share of employed foreigners³⁰ with employment growth of asylum seekers by 2-digit economic sector. In particular for unskilled jobs, asylum seekers tend to take up employment in sectors with high foreigner shares, suggesting a more substitutive nature of those workers.³¹

5.2 Treatment intensity

In the second part of our analysis we study the effect of the size of the labor supply shock in order to identify potential heterogeneous treatment effects based on treatment size. According to Equation (2) described above, we assess the effect of different treatment intensities. Table 6 shows the results of the estimation with weighted dummies.³² The coefficients of interaction term $\hat{\beta}_w^g$ show the marginal effect of an increase in the supply shock intensity on employment growth in treated counties, while the coefficients of $\hat{\beta}^g$ remain unchanged (see Footnote 20).

Overall, we find positive significant interaction terms for natives. This implies that the larger the labor market shock, the more positive the effect becomes on other employees in particular for skilled natives. Figure 8 shows the

³⁰The five most important sectors for foreigners are hospitality, temporary agencies, maintenance, retail and construction. They employ around one third of all foreigners.

³¹Another potential explanation could be a relatively higher elasticity of labor supply of foreigners. When looking at different subgroups of foreigners in Table A2, we find that employment growth of EU nationals react stronger to the additional labor supply of asylum seekers. This could be due to their higher levels of mobility or dodging behavior due to the labor supply shock.

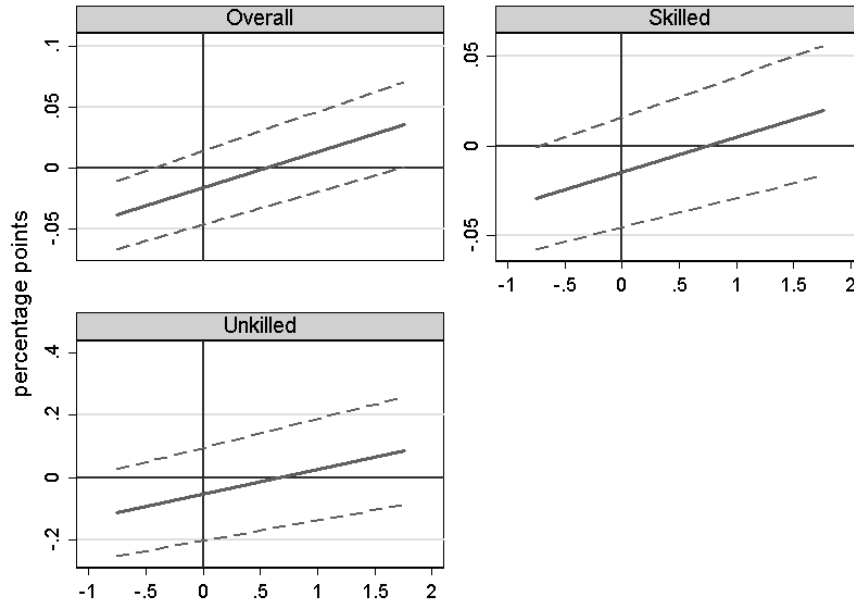
³²These results are also robust to alternatively weighting the treatment variable, e.g. using only eligible male asylum seekers between the age of 25 and 55, who are more likely to enter the labor market.

Table 6: Weighted DID regression on employment growth

	Asylum	Native	Foreign
	(1)	(2)	(3)
Weighted DiD Overall	-0.1239 (0.2665)	0.0296*** (0.0081)	-0.0626 (0.0538)
Weighted DiD Unskilled	-0.2464 (0.3732)	0.0791* (0.0433)	0.0445 (0.0814)
Weighted DiD Skilled	0.0154 (0.2040)	0.0194** (0.0081)	-0.1128 (0.0697)
County FE	Yes	Yes	Yes
Date FE	Yes	Yes	Yes
N	14356	14356	14356
Unique N	388	388	388

Note: Estimates from an OLS regression. The dependent variable is monthly employment growth in percentage points per nationality and skill group. Weighted DID is a weighted dummy variable. The dummy is equal to one in counties which suspended PR after the date of the policy change. The weight is the share of eligible asylum seekers at the time of the PR suspension relative to total employment in the county in June 2016. Each column represents a different nationality group, column (1) for the eight main countries of origin, column (2) for Germans and column (3) for all other nationalities except Western Balkan. Each row represents a different estimation for skill subgroups within the respective nationality. Skill levels are based on BA classification (KldB 2010): no requirement (unskilled) and professional education or above (skilled). The regression additionally contains county- and month-fixed effects as well as the DID dummy. "Non-treated mean" are the average monthly employment growth rates in PR counties after the PR suspension. Observations are weighted by county population and from 07/2014 to 07/2017. Standard errors are in parenthesis and clustered on employment district level. The significance levels are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 8: Treatment effect for natives by eligibility share



Note: Treatment effects on monthly employment growth for natives over the range of eligible asylum seekers relative to total employment in June 2016. The dashed lines mark 95% confidence bands. The upper left panel is the effect for natives overall, the upper left for skilled natives and the lower left panel for unskilled natives. The shares w_i are demeaned by their weighted mean level of 0.78. The lines show the range of coefficient estimates in the range of w_i observed in the data.

different treatment effects for natives over the range of affected asylum seekers relative to total employment. The average treatment effect as estimated by $\tilde{\beta}^g$ is at the horizontal line which marks the demeaned average of w_i , while the slope estimate $\tilde{\beta}_w^g$ is increasing for all groups. The point estimates even become positive for deviations from the mean of more than around 0.5 percentage points, i.e. for $w_j \geq 1.3$.

The findings from the weighted treatment variable further strengthen our results from before. For some natives the employment growth is even more positively affected if more than the average share of asylum seekers ($\bar{w} = 0.78\%$) are eligible to the policy change in a county, while the average effect from the policy change was insignificantly different from zero. Taken together, a displacement of natives by the reform seems unlikely while foreigners are displaced by asylum seeker labor supply. The latter effect may be explained by network effects that make it easier for firms to hire additional asylum seekers in counties with a larger share of asylum seekers in the labor force.

Table 7: Placebo DID regression on employment growth

	Asylum	Native	Foreign
	(1)	(2)	(3)
DiD Overall	-0.0163 (0.2543)	-0.0137 (0.0112)	-0.0894 (0.0622)
DiD Unskilled	-0.0727 (0.2891)	0.0019 (0.0505)	-0.0876 (0.1163)
DiD Skilled	0.0875 (0.2994)	-0.0156 (0.0121)	-0.0651 (0.0705)
County FE	Yes	Yes	Yes
Date FE	Yes	Yes	Yes
N	14356	14356	14356
Unique N	388	388	388

Note: Estimates from an OLS regression. The dependent variable is monthly employment growth in percentage points per nationality and skill group. Each column represents a different nationality group, column (1) for the eight main countries of origin, column (2) for Germans and column (3) for all other nationalities except Western Balkan. Each row represents a different estimation for skill subgroups within the respective nationality. Skill levels are based on BA classification (KldB 2010): no requirement (unskilled) and professional education or above (skilled). DID is a dummy variable equal to one in counties which suspended PR after the date one year prior to the policy change. The regression additionally contains county- and month-fixed effects. Observations are weighted by county population and from 07/2013 to 07/2016. Standard errors are in parenthesis and clustered on employment district level. The significance levels are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6 Robustness

6.1 Placebo regression

Next, we undertake a number of robustness checks to see whether our result holds for alternative specifications. We first run a number of placebo regressions to test whether we are just capturing already existing differences in employment growth between PR and PR suspending counties. First, we set a different timing of treatment (see Table 7). For our main effect, we assume a treatment one year before the actual treatment. We accordingly shift the analysis period one year ahead to July 2013 until July 2016 to end before the actual treatment.³³ There is no significant effect of the placebo dummy on any skill level subgroup.

In a second step, we randomize PR suspension assignment across employ-

³³As mentioned above, in 2014 some legal requirements for asylum seekers were changed in all of Germany, which is why we exclude the year prior from our main analysis. In order to have a comparable time window for the placebo test, we include the year in the placebo regression. Nevertheless, when we shorten the time window to 2014-2016, the results remain unchanged.

ment districts (see Figure A3). To this aim, we use the STATA routine provided by Heß (2017) and estimate our regression 2000 times with random treatment assignment (PR suspension) clustered on BA district level. Our estimates for asylum seekers and foreigners are at the tail of the distribution. The p-values (asylum: 0.04; native: 0.20; foreign: 0.09) are larger than in our main regression above (asylum: 0.02; native: 0.19; foreign: 0.04). However, besides a lower significance level of foreigners, the other estimates remain at the same level of significance. Hence, we are confident that clustered treatment assignment besides the clustering on BA district level does not drive our main results - particularly not the insignificant effect for natives.

6.2 Local spillovers

Another issue could be that individuals in counties not suspending the PR commute to suspending counties to circumvent their limited labor market access in the case of asylum seekers or the additional labor market competition for natives and other foreigners. This spillover effect could reduce the adverse effect of the policy change. Although such general equilibrium effects should implicitly be present in our estimates as are reactions of wages and labor supply decisions, we drop the neighboring counties of counties that did not suspend the PR. Such counties are more likely to be affected by potential spillover effects due to different migration or commuting behavior by migrants or natives. If the PR was suspended in the neighboring county, asylum seekers in the non-suspending county might avoid the regulation by commuting out. Therefore, this might increase the treatment effect in PR counties. However, excluding these counties also does not qualitatively influence our results (see Table 8). While the coefficient for low skilled asylum seekers becomes slightly smaller and insignificant, the coefficient for low skilled foreigners becomes slightly larger (in absolute terms) and significant at the 5% level.

6.3 Different skill measure

Finally, we analyzed possible displacement effects by education level (see Table 9). When we use education levels measured on ISCED levels, we are able to reproduce the findings from our job skill level analysis. The main effect on asylum seekers is almost exactly the same as in the skill requirement analysis. When disaggregating the results into different education levels, we find that asylum seekers with no high school education benefit the most from the policy change, although the coefficient is only marginally significant. For other foreigners the effect is also strongest for the education group, but in this analysis not significant anymore.

Table 8: DID regression on employment growth without neighboring counties

	Asylum	Native	Foreign
	(1)	(2)	(3)
DiD Overall	0.5868** (0.2826)	-0.0137 (0.0122)	-0.1399** (0.0624)
DiD Unskilled	0.6628 (0.4387)	-0.0384 (0.0567)	-0.3258** (0.1618)
DiD Skilled	0.4589** (0.2155)	-0.0137 (0.0128)	-0.0149 (0.0433)
County FE	Yes	Yes	Yes
Date FE	Yes	Yes	Yes
N	11988	11988	11988
Unique N	324	324	324
Non-treated mean	3.63	0.08	0.82
Non-treated mean (unskilled)	4.50	0.17	1.10
Non-treated mean (skilled)	3.08	0.07	0.67

Note: Estimates from an OLS regression. The dependent variable is monthly employment growth in percentage points per nationality and skill group. Each column represents a different nationality group, column (1) for the eight main countries of origin, column (2) for Germans and column (3) for all other nationalities except Western Balkan. Each row represents a different estimation for skill subgroups within the respective nationality. Skill levels are based on BA classification (KldB 2010): no requirement (unskilled) and professional education or above (skilled). DID is a dummy variable equal to one in counties which suspended PR after the date one year prior to the policy change. The regression additionally contains county- and month-fixed effects. "Non-treated mean" are the average monthly employment growth rates in PR counties after the PR suspension. Observations are weighted by county population and from 07/2014 to 07/2017. Standard errors are in parenthesis and clustered on employment district level. The significance levels are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: DID regression on employment growth for education levels

	Asylum	Native	Foreign
	(1)	(2)	(3)
DiD Overall	0.6645** (0.2813)	-0.0158 (0.0121)	-0.1257** (0.0595)
DiD No High School	0.9126* (0.5142)	-0.1071 (0.0802)	-0.1789 (0.1099)
DiD High School or more	0.2548 (0.2347)	-0.0167 (0.0126)	-0.0540 (0.0497)
County FE	Yes	Yes	Yes
Date FE	Yes	Yes	Yes
N	14356	14356	14356
Unique N	388	388	388
Non-treated mean	3.62	0.09	0.82
Non-treated mean (No High School)	4.12	0.02	0.91
Non-treated mean (High School or more)	2.98	0.23	0.94

Note: Estimates from an OLS regression. The dependent variable is monthly employment growth in percentage points per nationality and education level. Each column represents a different nationality group, column (1) for the eight main countries of origin, column (2) for Germans and column (3) for all other nationalities except Western Balkan. Each row represents a different estimation for skill subgroups within the respective nationality. Skill levels are based on ISCED classification. We combine ISCED levels 1 and 2 to the low education level (No High School) and all higher ISCED levels 3 and above to the high education level (High School or more). DID is a dummy variable equal to one in counties which suspended PR after the date of the policy change. The regression additionally contains county- and month-fixed effects. "Non-treated mean" are the average monthly employment growth rates in PR counties after the PR suspension. Observations are weighted by county population and from 07/2014 to 07/2017. Standard errors are in parenthesis and clustered on employment district level. The significance levels are * p<0.10, ** p<0.05, *** p<0.01.

7 Conclusion

In this paper we investigate the displacement effects on previous residents utilizing a quasi-experiment by a policy change that facilitated labor market access of asylum seekers. We exploit variation based on the opt-out decision of some counties and random assignment variation of asylum seekers within states. We find a significant increase in employment growth of asylum seekers, particularly in unskilled jobs. At the same time, employment of other foreigners grows less, while we cannot reject a null effect on native employment growth. This result holds when disaggregating employment into different skill levels.

Our results suggest that native and foreign employees have different degrees of substitutability, even when comparing employees with similar skill levels. More specifically, refugees and immigrants are closer substitutes than refugees and natives even on the same skill level. The finding has important implications for both theory and policy applications. Labor market models that assume perfect substitutability between immigrants and natives conditional on skill levels are not supported by our evidence. Many potential reasons may contribute to such a finding such as different levels of language proficiency or country and culture specific human capital formation or mobility.

Regarding policy implications, when discussing labor market access for asylum seekers or refugees and their impact on native employment, our result imply no or a very limited trade-off. As our results are based on a pure labor market supply shock - net of migration demand effects usually included in similar studies - we are able to assess the effect of labor market access of migrants alone. This policy decision is different from migration or asylum policies. However, they are closely linked - in particular in case of large scale migration waves as the one during our sample period.

However, our research design has a number of limitations. Due to data availability and prior confounding policy changes, our analyzed time window after the policy change is limited. It would be interesting to analyze the long-term effects in the facilitation of asylum seekers labor market access. However, we only can assess the effects of a facilitation of labor market access up to 1.5 years after the policy change, as the number of new arrivals decreased drastically in 2016 and therefore most of these arrivals are allowed unrestricted access to the labor market by the end of 2017. However, adverse labor market reactions to short-run immigration waves, like congestion externalities, might also fade out over time.

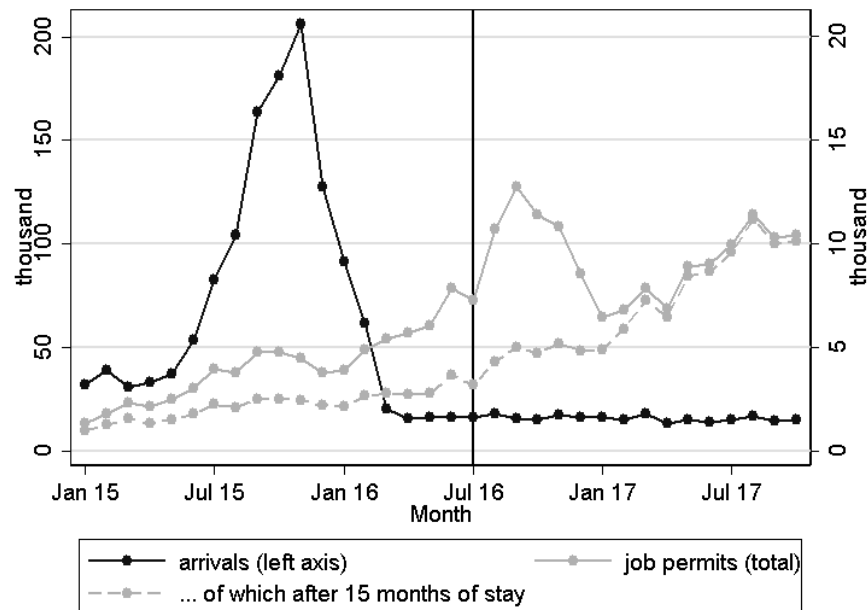
Another limitation might be that the size of our employment shock may simply be insufficient to detect any negative employment effect on the native population and we are not able to look at wage effects using our dataset. At the same time, the shocks seems large enough to detect a negative employment effect on other foreigners, a group that is much smaller than natives. Furthermore, the effect on natives does not seem to be consistent over time, in contrast to the effect for other foreigners. While the coefficient estimates for natives vary considerably around zero, we are able to identify a consistent displace-

ment effect of asylum seeker labor supply on employment of other migrants.

Overall, facilitating labor market access of asylum seekers leads to a short-run substitution effect on the labor market. However, the direct displacement is limited to other foreigners. Given the negative consequences of the waiting time till a decision on the asylum requests on their integration (Hainmüller et al. 2016), policy makers need to consider the trade-off between labor market outcomes for asylum seekers and resident foreigners on the labor market.

Appendix

Figure A1: Inflow and job permits of asylum seekers



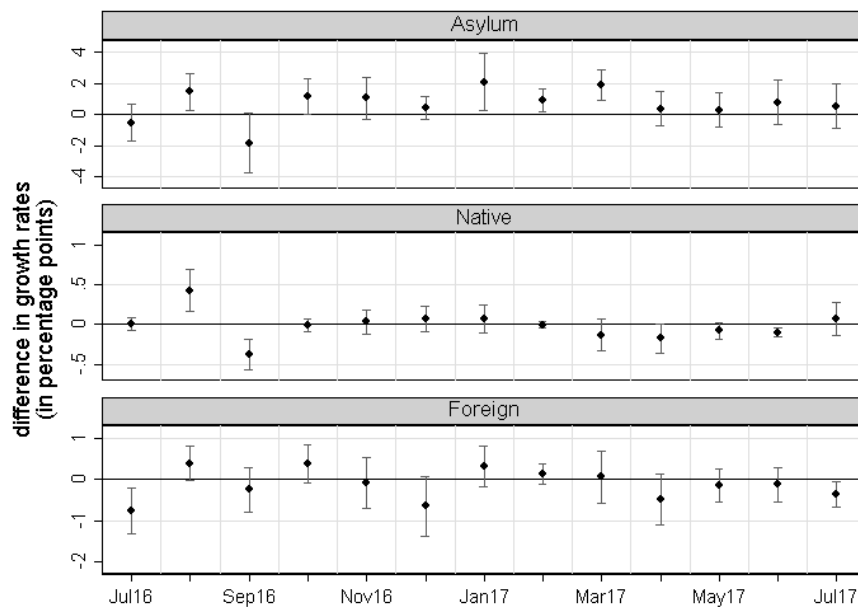
Note: Asylum seeker inflow into Germany as of end of a month based on EASY registrations (left axis) and accepted job permits of asylum seekers in Germany (right axis). Asylum requests in 2015 and early 2016 lagged behind actual arrival time due to delays in the possibility to officially request asylum during the initial peak of arrivals. Therefore, the figure counts registrations according to the EASY-system that only monitors country entries until 2017 and asylum requests afterwards. The black vertical line marks the last observation before the policy change. Job permits are in absolute numbers for all durations (total) and only after 15 months of stay. Source: German central registry of aliens (arrivals) and Statistics of the Federal Employment Agency (permits).

Table A1: Growth rates 2013-2015

	no PR	PR	Diff.
unemployment	-0.037 (0.002)	-0.033 (0.005)	0.004
long-term unemployment	-0.036 (0.003)	-0.040 (0.008)	-0.004
total population	0.011 (0.000)	0.009 (0.001)	-0.003**
foreign population	0.260 (0.010)	0.254 (0.027)	-0.005
GDP per capita	0.025 (0.002)	0.024 (0.003)	-0.002
Observations	327	61	388

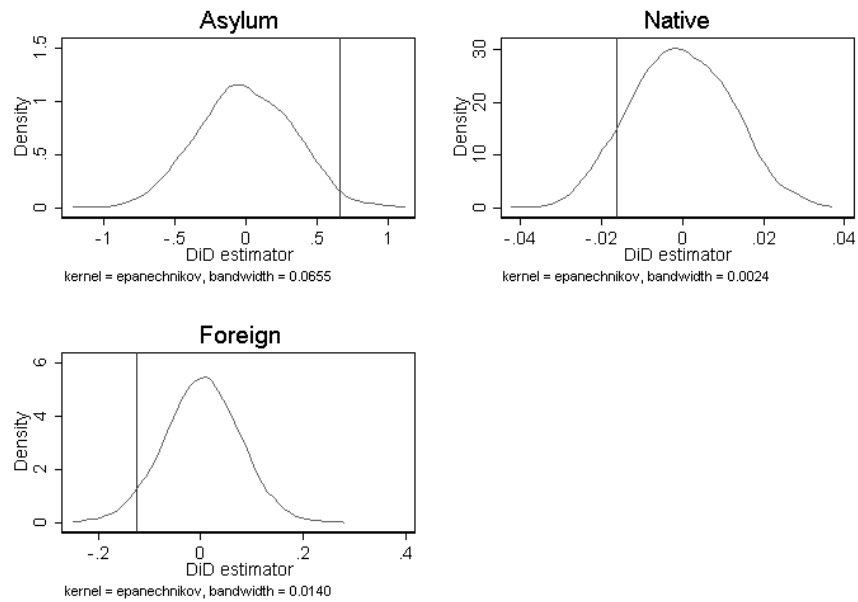
Note: Standard errors in parentheses. Stars denote significance levels of two-sided t-tests. * p < 0.1, ** p < 0.05, *** p < 0.01

Figure A2: Monthly employment growth rate differences



Note: Monthly employment growth difference between PR suspending and PR counties. Coefficients γ^s from an estimation of regression (1) with additional interaction terms of the difference term and time dummies, i.e. $\Delta L_{it}^s = \alpha_i^s + \theta_i^s + \beta^s D_{it} + \gamma^s D_{it} T_t + \epsilon_{it}^s$ with T_t monthly dummies for t after July 2016.

Figure A3: Distribution of coefficients with randomized treatment



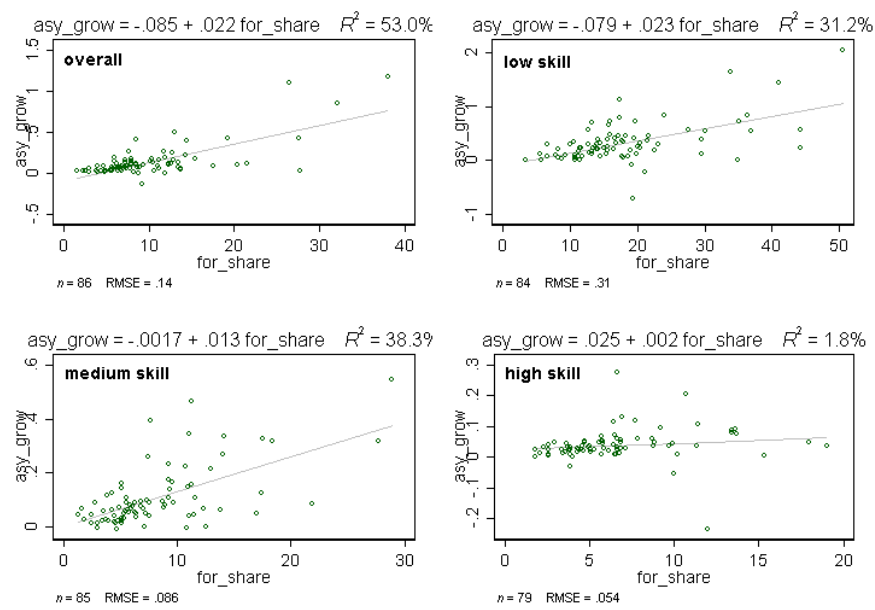
Note: This plot shows the distribution of difference-in-difference coefficients estimated 2000 times clustered on BA district level using the routine “ritest” (Heß 2017). The vertical line marks our previous main estimate (“DID Overall”). Each plot represents a different nationality group: the eight main countries of origin (top left), Germans (top right) and all other nationalities except Western Balkan (bottom left).

Table A2: DID regression on employment growth by foreigner definition

	Foreign	With Balkan	EU Foreign
	(1)	(2)	(3)
DiD Overall	-0.1247** (0.0601)	-0.1127** (0.0568)	-0.2079** (0.0878)
DiD Unskilled	-0.2898* (0.1574)	-0.2847* (0.1487)	-0.4182** (0.2101)
DiD Skilled	-0.0106 (0.0433)	0.0035 (0.0428)	-0.0567 (0.0647)
County FE	Yes	Yes	Yes
Date FE	Yes	Yes	Yes
N	14356	14356	14356
Unique N	388	388	388
Non-treated mean (total)	0.82	0.85	0.98
Non-treated mean (unskilled)	1.09	1.12	1.39
Non-treated mean (skilled)	0.67	0.71	0.75

Note: Estimates from an OLS regression. The dependent variable is monthly employment growth in percentage points per nationality and skill group. Each column represents a different nationality group, column (1) for for all foreigners except Western Balkan, column (2) for all foreigners including Western Balkan and column (3) only for foreigners with EU nationality. Each row represents a different estimation for skill subgroups within the respective nationality. Skill levels are based on BA classification (KldB 2010): no requirement (unskilled) and professional education or above (skilled). DID is a dummy variable equal to one in counties which suspended PR after the date of the policy change. All regressions contain county- and month-fixed effects. "Non-treated mean" are the average monthly employment growth rates in PR counties after the PR suspension. Observations are weighted by county population and from 07/2014 to 07/2017. Standard errors are in parenthesis and clustered on employment district level. The significance levels are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A4: Employment growth and foreigner share



Note: Linear prediction of employment growth of asylum seekers (“asy_grow”; in percentage points) from June 2016 to June 2017 by share of other foreigners in the sector in June 2016 (“for_share”; in %). Only 2-digit-sectors (“Wirtschaftszweige”) with positive employment of asylum seekers in June 2016 (total $n = 87$). Skill levels are based on BA classification (KldB 2010): no requirement (low level or “unskilled”), professional education (medium level), graduate and post-graduate education (high level). Source: Federal Employment Agency.

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