Assessing the Role of Asylum Policies in Refugees' Labor Market Integration:
The Case of Protection Statuses in the German Asylum System*

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Abstract

I study the effect of refugees' protection status—Geneva Convention or subsidiary protection status—on labor market outcomes, focusing on a cohort of Syrian and Iraqi refugee migrants entering Germany between 2013 and 2016. My empirical analysis exploits a sudden and unpredictable March 2016 policy change of the asylum claim-handling federal agency, reducing the likelihood of receiving Geneva Convention refugee status for refugee migrants from these two countries. Using data from the IAB-BAMF-SOEP survey of refugees, I exploit the policy change in a fuzzy regression discontinuity design. Estimation results indicate a substantial negative effect of subsidiary protection status on earnings and employment.

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1 Introduction

Over the past decade, the European Union (EU) has experienced a sizeable increase in the number of refugee migrants from outside the European continent (Dustmann et al., 2017). Recent studies find that these refugee migrants perform worse than economic migrants in labor markets in Western Europe (Bratsberg et al., 2014; Fasani et al., 2018; Brell et al., 2020). As unemployed refugee migrants are more likely to be involved in violent crime (Couttenier et al., 2019), might mobilize populist right-wing movements (Ivarsflaten, 2008), and do not contribute to the local economy or tax revenues, the low labor market attachment of refugee migrants has immediate societal costs in host countries. Additionally, as children of unemployed refugee migrants have more difficulty integrating into host country societies, high unemployment rates among refugee migrants have potential negative consequences for future generations (Bauer et al., 2013). Given these negative effects, policies that improve the labor market integration of refugee migrants bring large benefits to both refugee migrants and host country societies.

The literature on the economic assimilation of economic migrants suggests that the duration and permanence of stay is an important determinant of economic integration. Investments in host country-specific skills—such as language skills, knowledge of the host country's institutions and production methods—largely depend on the period in which immigrants can benefit from their investments (Dustmann, 1993, 1999, 2000; Cortes, 2004; Dustmann and Görlach, 2016).[1] That the length of that period affects such investments might be particularly important for refugee migrants, whose relocation decisions are not—or very little—based on economic considerations, thereby making them less economically selected than economic migrants and resulting in lower levels of host country-specific human capital upon arrival (Becker and Ferrara, 2019; Brell et al., 2020). However, refugee migrants are confronted with a considerable amount of uncertainty about their future settlement in the host country, e.g., long waiting times for asylum decisions or the lack of a clear perspective on permanent residence

[1]This result follows from a standard dynamic human capital model (e.g., Ben-Porath, 1967). Chiswick (1978) provided an early contribution, showing that human capital differs across countries and that newly arrived immigrants have an incentive to invest in destination country-specific human capital. For a survey, see Duleep (2015).

(Hainmueller et al., 2016; Dustmann et al., 2017). As the protection status of refugee migrants typically differs in terms of the waiting time for permanent residence in the host country, the type of protection status might be an influential factor for refugees' labor market integration (Dustmann et al., 2017).

In this paper, I empirically investigate the link between different types of protection status and labor market outcomes. I focus on a 2013–2016 cohort of refugee migrants to Germany, from Syria and Iraq, who received one of the two most prevalent types of protection status—refugee status according to the Geneva Convention (hereafter, "Geneva Convention status") or subsidiary protection status, a form of protection that makes obtaining permanent residency more difficult. As both types of status offer refugee migrants equivalent access to the labor market and German social security system but greatly differ in the requirements necessary for receiving permanent residence, the German asylum system provides an ideal case for studying the relationship between types of protection status, perceived duration of stay, and labor market integration of refugees.

The empirical analysis in this paper exploits a sudden and unpredictable March 2016 policy change in the German Federal Agency for Migration and Refugees (German: Bundesamt für Migration und Flüchtlinge, BAMF) to grant Geneva Convention status for refugee migrants from Syria and Iraq. Syrian and Iraqi asylum seekers who received notification of the asylum decision in 2015 or the first three months of 2016 were almost entirely granted full Geneva convention status. However, when the March 2016 policy change took effect in April, the BAMF suddenly began granting only subsidiary protection to about one-fifth of the Syrian and Iraqi asylum seekers. Yet refugee migrants cannot precisely influence the timing of the decision on their asylum application, and they apply for asylum months before they receive the decision on their protection status. Therefore, the BAMF policy change provides valuable and plausibly exogenous variation in the likelihood of being granted subsidiary protection status for those refugees receiving their asylum decision close to the change in the BAMF's decision-making practice.

My empirical analysis is based on the comprehensive longitudinal IAB-BAMF-SOEP survey of refugees. This data provides extensive information about the asylum procedure and socio-economic background for a sample of refugee migrants who entered Germany between 2013 and 2016. Most importantly for this study, the survey collects information about the year and month in which refugee migrants received their asylum decision, as well as their current protection status and labor market outcomes. This information allows me to exploit the change in the BAMF policy in a regression discontinuity (RD) design, which generates causal estimates of the effect of the policy change on their status protection type and labor market outcomes under relatively weak identification assumptions (Hahn et al., 2001; Lee and Lemieux, 2010).

The results of my empirical investigation clearly indicate a substantial negative effect of subsidiary protection status on various labor market outcomes. Drawing on a sample of Syrian and Iraqi refugees who reported having either Geneva Convention or subsidiary protection status, I estimate a significant decline in the probability of being in any employment, in full-time employment, and in monthly labor earnings two and one-half years after the policy change for refugee migrants who received their asylum decision after March 2016. The drop in employment by around 9 percentage points (pp) is almost entirely driven by the reduction in full-time employment. This result suggests that the policy change influenced both the employment probability and the percentage of full-time employment among employed Syrian and Iraqi refugees.

Estimates for monthly labor income confirm this finding: Local linear regression at both sides of the threshold suggest a drop in monthly labor income of around 140 Euros for the entire sample and around 220 Euros among employed individuals. Using current reported protection status as the outcome variable, the RD design estimates indicate that exposure to the new BAMF policy regime increased the percentage of refugee migrants granted only subsidiary protection status by around 25 pp. Under the assumption that the exclusion restriction is satisfied, this result suggests that the effect of subsidiary protection status on labor market outcomes is four times as large as the previously discussed reduced form estimates.

My estimates of the effect of subsidiary protection status on labor market outcomes represent the local average treatment effect (LATE) for the subgroup of compliers, i.e., those refugee migrants who have only subsidiary protection status due to the change in the BAMF policy. It is plausible to assume that the policy change targeted a specific group of refugee migrants, thereby suggesting that the group of compliers differs from other groups of refugee migrants. To deal with this concern, I complement my baseline results with a complier analysis and find that the change in the BAMF decision-making practice more strongly affected refugee migrants who are more likely to be employed—e.g., male, younger, not married, and no children in the household.

The negative effect of subsidiary protection status on labor market outcomes is consistent with the proposed causal mechanism that subsidiary protection status reduces the perception of likely permanent residence in Germany, a perception that lowers refugee migrants' incentives to invest in country-specific human capital and the probability of being active in the labor market.

In the final part of the paper, I test this causal mechanism and find no evidence that subsidiary protection status reduces refugees' investments in country-specific human capital. While my estimation results indicate that subsidiary protection status increases refugee migrants' worries that they cannot remain in Germany, it also positively affects participation in integration classes and hours spent studying German. These puzzling results suggest that the negative effect of subsidiary protection status on labor market outcomes might be driven not by labor supply side factors but instead by labor demand side factors.[2] For example, if the employment of refugee migrants requires costly on-the-job training, firms prefer to hire refugee migrants with better prospects of staying, so as to regain their investment costs.

My empirical analysis, which provides quasi-experimental evidence based on microdata, confirms the existence of an economic and political trade-off in asylum policies (Dustmann et al., 2017). Although granting permanent residence to refugee migrants presumably induces political costs, it also provides economic and social benefits by reducing unemployment among

[2]While most of the literature on the economic assimilation of immigrants focuses on supply side factors, research shows that the demand side is also an influential factor in explaining immigrants' employment (Åslund and Rooth, 2007; Azlor et al., 2020).

refugee migrants in the society. This trade-off becomes particularly important if refugee migrants who are initially offered only temporary protection end up staying longer in the host country, e.g., because the grounds on which they received temporary protection have not changed. The results of my paper suggest that optimal asylum policies need to take into consideration the likelihood that the reasons for granting refugee migrant temporary protection will remain in place longer than the original policymakers intended.

This paper contributes to the increasing literature on asylum policies and the labor market integration of refugee migrants. A closely related study, drawing on data from the European Labor Force Survey, is Fasani et al. (2018), who exploit variation in refugees' exposure to high Geneva Convention status rates (a) across entry cohorts within a country and (b) across countries within a cohort, to study refugees' labor market integration. My findings are in line with theirs, that the exposure to high Geneva Convention status rates is associated with better labor market outcomes of refugees. However, instead of using variation in protection status at an aggregate level, my empirical analysis exploits within-country variation in types of protection status, based on high-quality microdata. Moreover, the difference-in-differences analysis employed by Fasani et al. (2018) relies on demanding common trend assumptions, while the results of my paper are based on the relatively weak identification assumptions of the RD design.

Various studies focus on other asylum policies. Fasani et al. (2018) show that the dispersal policies of refugees have a negative impact on labor market integration and, for the question of dispersal policies, Brücker et al. (2020) show that residence requirements reduce employment rates among refugee migrants in Germany. Rosholm and Vejlin (2010) study the effect of a reduction of welfare payments for refugee migrants in Denmark, finding that lower income transfers increase the job-finding rates of refugees. Hainmueller et al. (2016), analyzing the effect of the length of asylum procedures, find that they are negatively associated with labor market performance in Switzerland. Arendt et al. (2020) show that a Danish reform expanding language classes for refugees positively affected employment and income, and Battisti et al. (2019) provide evidence that job-search assistance is conducive for refugee migrants' employment prospects. My paper adds to this literature by investigating the effect of an asylum policy that governments can implement with low administrative costs, and provides similar positive returns.

More broadly, this paper also contributes to the literature on citizenship or legal status and the labor market outcomes of immigrants. Recent contributions in this area argue that citizenship and the legal status of immigrants are conducive to immigrants' labor market outcomes by giving private sector employers incentives to invest in a foreign employee (Gathmann and Keller, 2018). They find that faster access to citizenship increases the employment outcomes of immigrants in Germany, and Devillanova et al. (2018) provide quasi-experimental evidence from an amnesty program in Italy, showing that the legal status of immigrants increases employment rates. The findings of my paper confirm the overall positive effect of a more permanent status on employment rates and show that this association is also important for refugee migrants.

The remainder of the paper is structured as follows. Section 2 provides background information on the German asylum system and the BAMF policy change to grant full refugee status to asylum seekers from Syria and Iraq. Section 3 presents my data set based on the IAB-BAMF-SOEP survey of refugees. Section 4 gives the main identification strategy and discusses the validity of the RD design. Section 5 illustrates the baseline results. Section 6 provides various robustness tests of the RD design. Section 7 discusses the effects of subsidiary protection status on other integration efforts. Section 8 concludes.

2 Institutional Background

2.1 Asylum system and protection statuses in Germany

The BAMF is an agency of the German Federal Ministry of the Interior.[3] Its responsibilities include managing the German asylum procedure, starting from the first registration of asylum seekers upon entering Germany through the final decision on the asylum application. Upon

[3]This subsection is based on online information from the BAMF (www.bamf.de) and Tiedemann (2014).

arrival and first registration, asylum seekers are sent to one of several reception centers, where they file an asylum application with the closest branch office of the BAMF. If Germany is responsible for the asylum application under the Dublin III regulation, the asylum applicant will be invited to an individual hearing organized by the BAMF case worker responsible for the final decision. During this hearing, asylum seekers state the reasons for fleeing their country of origin and the kind of persecution they experienced. The individual hearing is of highest priority for asylum seekers, because the information and evidence they give will be the main basis for the case worker's decision.

Drawing on the information from the individual hearing and additional research on the credibility of the asylum seeker's claim, the case worker determines whether one of four types of protection status—(a) political asylum under the German constitution, (b) refugee status under the Geneva convention, (c) subsidiary protection status, and (d) suspension of deportation—can be granted the applicant. If so, the applicant will receive a positive protection decision and a temporary residence permit. If not, the applicant will receive a rejection letter and must leave Germany.

The two most common types of protection status in Germany are Geneva Convention status and subsidiary protection status. To receive Geneva Convention status, asylum seekers need to prove that they have been persecuted either because of their race, religion, nationality, or political opinion or because of belonging to a particular social group. The reason must be an innate trait (e.g., skin color) or an individual characteristic so crucial to the individual's identity or conscience (e.g., religion, sexuality, political opinion) that the individual cannot be forced to live without it. If asylum seekers do not fulfill the criteria for obtaining Geneva Convention status, they may obtain subsidiary protection if they fear death, torture, or other inhumane treatment in their country of origin. For subsidiary protection, the reason for persecution does not need to relate to specific traits but may also be the result of violence during civil wars.

The type of protection status that asylum seekers receive has crucial consequences. While both types of status allow immediate access to the German labor market and social security system, they differ considerably in terms of the individual's prospects for receiving permanent residence in Germany. Geneva Convention refugees receive preferential treatment when applying for permanent residency.[4] With the asylum application decision, Geneva Convention refugees receive an initial three-year temporary residence permit, which can be prolonged for two additional years each time it expires, as long as the reasons for granting the protection status remain applicable. Three years after arrival in Germany, Geneva refugee migrants can apply for a permanent residence permit if they can show a good command of the German language (level C1 of the Common European Framework of Reference for Languages (CEFRL) and are able to cover at least 75 % of their living costs. Otherwise, they can apply for permanent residency after five years if they show only a basic knowledge of German (CEFRL level A2) and are able to cover at least 50 % of their living costs.

In contrast, subsidiary protection refugees receive an initial residence permit of only one year, which can be extended for two years each time it expires, as long as the reasons for granting the protection status remain applicable. Rather than having a fast track to apply for permanent residency, these refugees can apply for it only after five years in Germany. However, the requirements are more difficult, as they need to show an acceptable command of the German language (CEFRL level B2), can cover 100 % of their living costs, and have contributed for at least 60 months to the German social security system.[5]

2.2 Increase in irregular migration and changes in asylum policies

In 2014–2016, an unprecedented number of refugee migrants entered the EU with the intention of applying for asylum. Most of these migration flows originated in Afghanistan, Iraq, Pakistan, and Syria, and they first entered the EU in Greece by crossing the Eastern Mediterranean Sea by boat.[6] Many of these refugee migrants wanted to continue their journey to northern EU member states by transiting Western Balkan countries (e.g., Croatia, Macedonia, Serbia).

[4] Geneva convention refugees are treated in accordance with §26 Act on the Residence, Economic Activity and Integration of Foreigners in the Federal Territory (AufenthG). Subsidiary protection refugees are treated in accordance with §9 AufenthG.

[5] German residents contribute to the social security system if they are employed and earn more than 450 Euros per month.

[6] Drawing on data from Frontex, the European agency for border control, Dustmann et al. (2017) show that around 38 % of illegal crossings to Europe between 2009 and 2015 were of individuals from Syria (Afghanistan 20 %; Iraq 5 %; Pakistan 5 %).

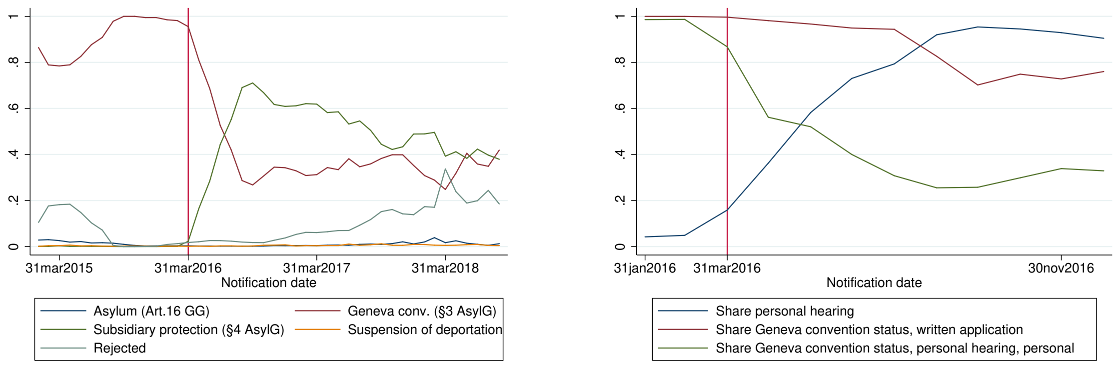
The Dublin III regulations require asylum seekers to apply for asylum in the country they first enter. However, southern EU countries—in particular, Greece—were so overwhelmed by the number of refugee migrants that they allowed the majority to pass through their northern borders. The large number of refugee migrants crossing EU borders revealed the weakness of the EU asylum system and its unequal distribution of refugee migrants among member states. It also put pressure on northern EU countries to accept a larger number of refugee migrants to apply for asylum in their territory than they would normally have accepted. Germany in particular unilaterally suspended the Dublin III regulations and started processing a large percentage of the asylum claims resulting from these migration flows.

Not surprisingly, the German asylum system was not prepared for the huge number of refugee migrants, and Germany had to undertake major legislative actions to increase its capacity for processing the large number of asylum claims (Bertoli et al., 2020). To speed up the asylum procedure for these refugee migrants, the German Federal Ministry of the Interior ordered the BAMF in autumn 2014 to determine the type of protection status for asylum seekers from Syria and Iraq on the basis of written applications, not individual hearings. Starting in 201, this order was rescinded, after which the standard procedure of individual hearings gradually took effect again (Deutscher Bundestag, 2016).

The change in the type of asylum application had critical consequences for the protection status of the Syrian and Iraqi refugee migrants. Figure 1a shows the type of protection status decision received by Syrian asylum seekers per month. As the figure shows, almost all Syrian refugee migrants were granted Geneva Convention refugee status if they received their asylum decision before March 2016 (rejections not considered), with the percentage receiving subsidiary protection status noticeably increasing thereafter.[7] At the same time, as the blue line in Figure 1b shows, the percentage of decisions from individual hearings was also increasing. More importantly, the decision to grant Geneva Convention status based on individual hearings (green line in Figure 1b) also changed significantly after March 2016, indicating that the sudden increase in the percentage of these migrants being granted only subsidiary pro-

[7]While less pronounced, the same pattern can be seen for asylum applicants from Iraq as illustrated in Figure A1b in the Appendix.

Figure 1:
Protection status and notification date, Syrian asylum seekers



(a) Received protection statuses **(b)** Type of application

Note: Left plot illustrates the type of protection status received by month of notification date of Syrian asylum seekers. Source: Own calculations based on monthly published data from BAMF (data available upon request). Equivalent plot for Iraqi asylum seekers can be found in Figure A1b in the Appendix. Right plot shows for Syrian asylum seekers (i) the share of decisions made by month of notification date on basis of personal hearings (blue line), (ii) the share of asylum applicants that were granted Geneva convention status on basis of written applications by month of notification date (red line), and (iii) the share of asylum applicants that were granted Geneva convention status on basis of personal interviews (green line). Source: Deutscher Bundestag (2017).

tection was driven by the change in the application process from written to in person.[8] This finding suggests that this sudden change was not due to compositional differences in the group of asylum applicants, but rather that the individual hearings led case workers to make different decisions on the question of individual persecution.[9]

As I explain in more detail in Section 4, an important assumption of the identification strategy in this paper is that receiving the asylum decision after March 2016 so greatly diminished the refugees' chance of receiving Geneva Convention status that the primary effect was on their likelihood of obtaining subsidiary protection status. This assumption rules out the possibility of any other asylum law change affecting the refugee migrants differently, depending on whether they received notification shortly before or after the policy change.

To further speed up asylum procedures, in mid-March 2016 the German government passed several reforms of the German asylum law. As most of these reforms were targeted at rejected

[8]Unfortunately, no data exists on the percentage of decisions based on personal hearings from Iraqi asylum seekers.

[9]In Section 4.2, I provide a detailed discussion of the validity of the RD design, which involves comparing refugee migrants who received notification about their asylum claim shortly before and after March 2016. From observable characteristics, I find no differences among the two subpopulations.

asylum applicants, they did not depend on the asylum decision—except for one: All asylum seekers granted subsidiary protection status became subject to a temporary ban on family reunification. Thus refugee migrants who entered without their spouse or children and received the asylum decision after the policy change were affected not only by a higher likelihood of obtaining subsidiary protection but also by the ban on family reunification. To avoid attributing changes in labor market outcomes to subsidiary protection status rather than to being affected by this ban, I exclude from my sample the Syrian and Iraqi refugee migrants who were banned from reuniting with their families.[10]

3 Data

The main data source of the paper is the longitudinal IAB-BAMF-SOEP survey of refugees (SOEP refugee panel) which provides an excellent source to study questions regarding the labor market integration of refugees in Germany.[11] The SOEP refugee panel is based on a sample of individuals who entered Germany between 2013 and 2016 with the intention to apply for asylum, as well as their household members. All individuals of the sample above the age of 18 are interviewed annually, and the first wave was conducted in the year 2016. The empirical analysis of this paper is based on the latest wave of SOEP refugee sample from the year 2018. I restrict the SOEP refugee sample to Syrian or Iraqi individuals in working age (18 to 65) who applied for asylum and had received notification about the asylum application before the time of interview.

Based on this sample of 2,061 individuals, around 81 % of the respondents reported to have either currently protection status in accordance with the Geneva Convention or subsidiary protection status. The other roughly 20 % consist of refugee migrants who either did not obtain protection in Germany, received protection for humanitarian reasons (suspension of deportation), or already have permanent residency in Germany. To increase the precision of my

[10] An alternative approach would be to disentangle both effects in a difference in discontinuity design as proposed by Grembi et al. (2016). However, as only a relatively small number of refugee migrants entered Germany without their spouse and children, the approach does not provide fruitful results in my application.

[11] The data set can be ordered online at the research data center SOEP of the DIW: https://www.diw.de/soep. For detailed information about the study design, see Kroh et al. (2016).

first stage estimates based on the discontinuity induced by the policy change, I exclude these individuals from the data set. This results in a sample size of 1,683 refugee migrations who either have protection status in accordance with the Geneva Convention or subsidiary protection status.[12] As explained above, the new policy regime did not only affect the likelihood to obtain subsidiary protection status but also included a ban on family reunification for refugee migrants with subsidiary protection status that received notification after the policy change. Around 71 % of the respondents are married of whom 18 % reported to have entered Germany without their spouse or children, and would have been affected by the ban if they obtained subsidiary protection status and received notification after the policy change. As it seems to be likely that being affected by the ban on family reunification also influences labor market outcomes, I also exclude these individuals from the sample, which results in a final data set with 1,470 observations.

The SOEP refugee panel provides detailed information about the asylum process. In particular, respondents were asked about the date (month and year) when they received notification about the decision of the asylum application, which allows to construct a variable that indicates whether an individual was affected by the policy change or not. The SOEP refugee panel provides also information about labor market outcomes. I use this information to construct two binary outcome variables that indicate whether an individual was (i) in any paid employed or (ii) in full-time employment at the time of the interview, as well as the reported monthly net labor income. Finally, I use the background information available in the SOEP refugee to construct an extensive set of control variables covering individual-specific characteristics such as age, gender, martial status, work experience before migrating, or time spent in Germany. This information is used to assess the validity of the RD design and is illustrated in Section 4.2.

[12]The main results of the paper are qualitatively not affected by the exclusion of these observations.

4 Identification

4.1 Empirical strategy

Estimating the effect of subsidiary protection status instead of protection in accordance with the Geneva convention on labor market outcomes poses considerable difficulties. As explained in more detail above, granted protection statuses target specific groups of asylum seekers and are not randomly distributed among refugee migrants impeding causal estimates of the effect of protection statuses on labor market outcomes. For instance, using cross-sectional variation in protection statuses among refugee migrants with one of the two protection statuses might lead to biased estimates of the true effect if there are individual-specific unobserved factors that explain labor market outcomes and the type of protection status simultaneously. These factors might be abundant in my setting and could relate to, e.g, the prevalence of economic motives to migrate, different experiences made when fleeing, or loss of valuable assets in the country of origin. In this paper, I overcome such endogeneity concerns by exploiting the discontinuity in the probability of receiving subsidiary protection status at the point in time when the BAMF changed its decision making practice. While before April 2016 basically all non-rejected applicants from Syrian and Iraqi were granted refugee status in accordance with the Geneva convention, this suddenly changed afterwards with a high and increasing share of refugees who only received subsidiary protection. Hence, the probability of receiving subsidiary protection changed noticeably for those receiving notification after the policy change in March 2016. I exploit this variation in the share of refugees with subsidiary protection status in a fuzzy regression discontinuity (RD) design using the date of notification about the asylum application as assignment variable. Under assumptions discussed in more detail below, a fuzzy RD design allows in my setting to identify the local average treatment effect (LATE) for a subgroup of refugee migrants by calculating the ratio between the estimated discontinuity of the labor market outcome variable and the jump in the share of refugee migrants with subsidiary protection status at the time of the policy change. The subgroup of refugee migrants for whom the LATE is identified consists of asylum seekers that (i) received notification about their asylum application at the time of the policy change and (ii) are compliers, i.e., refugee migrants who receive subsidiary protection status if they receive notification of their asylum application after the policy change but would receive protection status in accordance with the Geneva convention if they received notification before the policy change.

As suggested by Hahn et al. (2001) and Imbens and Lemieux (2008), I implement the fuzzy RD design by a two-stage least square (2SLS) estimation procedure using a binary variable indicating the policy change as the excluded instrument and the assignment variable as exogenous control variable. Formally, I estimate the following system of equations:

$$Sub_i = \alpha_0 + \alpha_1 \mathbb{1}\left[t_i > c\right] + \alpha_2 f(t_i - c) + \alpha_3 \mathbb{1}\left[t_i > c\right] f(t_i - c) + \eta_i \tag{1}$$

$$Y_i = \beta_0 + \beta_1 \hat{Sub}_i + \beta_2 f(t_i - c) + \beta_3 \mathbb{1}\left[t_i > c\right] f(t_i - c) + \epsilon_i \tag{2}$$

where Sub_i is binary variable indicating if individual i reported to have a subsidiary protection status in the last wave of the SOEP and $\hat{Sub}_i$ is the predicted values of Sub_i based on parameter estimates of Equation (1). $\mathbb{1}\left[t_i > c\right]$ is an indicator function equal to 1 if i's month of notification about his or her asylum application (t_i) was after the change in the decision making practice of the BAMF in March 2016 (c).[13] $f(t_i - c)$ is a function of the assignment variable, the distance between t_i and c, and Y_i is a measure of the labor market outcome of i as reported in the last wave of the SOEP. η_i and ϵ_i are error components capturing factors that influence the outcome variables Sub_i and Y_i and are not included in Equation (1) and (2), respectively.

I estimate Equation (1) and (2) based on a sample of refugee migrants from Syria and Iraq who reported in the last wave of the SOEP refugee panel to have subsidiary protection status or protection status in accordance with the Geneva convention. As standard in the literature, I employ local linear and polynomial regressions on both sides of the threshold and report results for various bandwidth selection choices. Following the suggestions by Imbens and

[13] I treat individuals who received notification about the asylum application in March 2016 as individuals who received notification before the change in the decision making practice even though the discussion in Section 2.2 suggests that some individuals were already exposed to the new decision practice in March 2016. In Section 6.1, I show that my results are robust to excluding those observations in a Donut RD design.

Lemieux (2008) and Lee and Lemieux (2010), I use a rectangular kernel which is equivalent to standard linear regressions on both sides of the threshold.[14]

The parameter of interest in this paper is β_1 and represents the LATE for compliers at the threshold under the following two assumptions (Imbens and Angrist, 1994; Hahn et al., 2001). The first assumption is *monotonicity* at threshold date, i.e., receiving notification shortly after c did not cause some individuals to receive protection status in accordance with the Geneva convention who would have obtained a subsidiary protection status in case they received notification shortly before c. Based on the discussion in Section 2.2, this assumption seems to be fulfilled as the new policy regime seems to be more strict in terms of granting a protection status in accordance with the Geneva convention and subsidiary protection status was very rare in the old policy regime. The second assumption is the *exclusion restriction* at the threshold date, i.e., receiving notification shortly after c did not impact Y except through Sub. This assumption requires that (i) the exposure to the new policy regime is "as good as randomly assigned" close at the threshold date (*independence*) and (ii) the exposure to the new policy regime did not affect labor market outcomes through other channels than an increase in the share of individuals with subsidiary protection status (*exclusion*).[15] Independence is fulfilled if there is imprecise control over the assignment variable - which is a standard assumption in RD designs - and its assessment is part of the following subsection. However, even if the exposure to the new policy regime is as good as randomly assigned close to the threshold, the exclusion restriction is violated if the exposure to the new policy regime affected labor market outcomes through other channels than the reception of subsidiary protection status. For instance, refugees with subsidiary protection who entered Germany without their spouse and received notification shortly after the threshold were affected by the ban on family reunification for which specific labor supply responses might be expected. Since I exclude asylum

[14] Imbens and Lemieux (2008, p. 625) state that "from a practical point of view, one may just focus on the simple rectangular kernel, but verify the robustness of the results to different choices of bandwidth" and Lee and Lemieux (2010, p. 319) write that "it is [...] simpler and more transparent to just estimate standard linear regressions [...] with a variety of bandwidths, instead of trying different kernels corresponding to particular weighted regressions that are more difficult to interpret." See also the discussion in Hinnerich and Pettersson-Lidbom (2014).

[15] By disentangling the assumption of independence and exclusion from the exclusion restriction, I follow Angrist and Pischke (2008) and Imbens and Lemieux (2008)

seekers that are affected by the ban on family reunification, this channel should not affect the identification strategy in this paper. Additionally, as the discussion in Section 2.2 has shown, there were no other changes in asylum policies that might have affected only asylum seekers who received notification shortly after to policy change, which suggests that I can rule out any other channel that might have affected labor market outcomes of refugees close to the cutoff.

4.2 Validity of the RD design

Lee (2008) shows formally for a sharp RD design that if individuals cannot *precisely* control the assignment variable, the variation in the treatment variable is as good as randomly assigned for observations with similar values of the assignment variable and, particularly, those observations close to the cutoff. It follows for a fuzzy RD design that imprecise control of the assignment variable implies random assignment of the instrumental variable for observations close to the threshold. If refugees were able to *precisely* influence the timing of the notification date of their asylum application, and if refugees have a benefit to be treated in accordance with the old or new policy regime, it is likely that refugees on one side of the cutoff differ systematically from those on the other side. For instance, assume refugees with better labor market prospects might be better informed about asylum policies and know about the change in decision making practice and others do not. If those refugees with better labor market prospects prefer to avoid the new policy regime with a higher chance of receiving subsidiary protection status, they would put effort into receiving the notification about the asylum application before the threshold while the others would not. The result of this thought experiment would be that refugees on both sides of the cutoff differ with respect to their labor market prospects independently of the protection status they received.

However, this scenario seems to be rather unlikely due to the following aspects. First, the change in the decision making practice has never been publicly announced, which makes it implausible that even well-informed refugees knew about this policy change. Moreover, those refugees who received notification of their asylum application close to the cutoff arrived in Germany and applied for asylum several month before. This is illustrated in Figure A2 in the

Appendix, which shows histograms of the arrival (left) and application month (right) relative to the policy change for refugee migrants who received notification 3 month before or after the policy change. Additionally, there is no recorded or anecdotal evidence that refugee migrants can influence the processing time of asylum applications.

Nonetheless, selective sorting around the threshold could still be possible. Assume that caseworker responsible for the asylum application knew about the policy change and were selective about the refugees who would fall into the old policy regime by influencing the processing time of the application. If such a selection is correlated with factors that influence labor market outcomes, this would invalidate the RD design.

An intuitive approach to assess the prevalence of sorting is to investigate the density of the assignment variable (McCrary, 2008). The intuition is that strategic sorting implies an unexpectedly high number of decisions made shortly before (or after) the threshold, resulting in a discontinuity of the density of the assignment variable at the cutoff. Additionally, a discontinuity of the density of the assignment variable might point to selective attrition as, for instance, in DiNardo and Lee (2004). Selective attrition means that refugees who receive subsidiary protection because of the change in decision making practice are more likely to drop out of the sample (e.g, because they left Germany or they do not want to participate in the interview). This threatens the validity of the RD design, in particular if the reason for dropping out of the sample is correlated with labor market outcomes.

Figure A3 in the Appendix plots on the left-hand side the density of the assignment variable for the SOEP refugee panel sample used in the empirical analysis and the plot on the right-hand side illustrates the same distribution for the official register data. The vertical lines in Figure A3 indicate the threshold date at the End of March 2016. Both plots show a very similar density of the assignment variable which highlights the good quality of the SOEP survey. Further, as the graph on the left is based on the survey participants of the SOEP in 2018 and the graph on the right is based on actual decisions made by the BAMF in each month, the similarity between both densities suggests that selective attrition might not be of importance in this study. Visually inspecting the density of the assignment variables in Figure A3, one might see a discontinuity shortly after the cutoff starting in May 2016. However, focusing only on the month before and after the cutoff, the density seems to be rather smooth. Additionally, I formally test the null hypothesis that the discontinuity of the density of the running variable is equal to zero as proposed by McCrary (2008) and cannot reject the null hypothesis (bin size: .460, bandwidth: 12.306, log difference in height: -0.093, standard error: 0.104).

A second test to check if the instrumental variable is "as good as randomly assigned" close to the cutoff is to compare pre-determined background characteristics of refugees who received notification about the asylum application before and after the threshold. While it is likely that those two groups differ in many dimensions for the overall sample, they should become more similar when restricting the sample to observations close to the cutoff. Table A1 in the Appendix shows mean values of selected pre-determined covariates for refugees who were not affected by the policy change ($t < c$) and those who were affected ($t > c$) as well as t-values of a two-sided mean comparison test. The first three columns refer to a sample that includes refugees who received notification about their application 18 months before or after the policy change and the last three columns further restrict the sample to three month before and after the policy change.

Focusing on the sample with a bandwidth choice of 18 months, Table A1 shows that refugees who received notification before the policy change are more likely to be male and slightly older than their counterparts who received notification after the threshold. Further, a higher percentage of those refugees had already acquired work experience before they moved to Germany and have spent, at the time of the SOEP interview, more time in Germany. The lower part of Table A1 shows also differences with respect to the outcome and treatment variables. In contrast, focusing on the last three columns in Table A1, the differences between both groups lose significance and the absolute difference between the mean values become much smaller - except for the treatment and outcome variables, which supports the hypothesis that selective sorting is not an issue in my setting.

As a final step, I check if pre-determined characteristics show a discontinuity at the threshold. If such pre-determined characteristics are not continuous around the threshold, I might wrongly attribute changes in labor market outcomes to changes in protection status if such discontinuities around the threshold were responsible for the changes in labor market outcomes. Table A2 in the Appendix shows RD estimates for various specifications (bandwidth choice and polynomial order) on various covariates.[16] For almost all covariates, I cannot reject the null hypothesis that the estimated discontinuity is equal to zero for all specifications. If I find significant effects for some variables, these are not robust across all specifications. The most worrisome discontinuity can be found for the variable *month since migration*. However, the effect is relatively small compared to the sample mean, which suggest that the resulting bias should be negligible.

In sum, the fact that refugee migrants cannot affect the timing of the decision of the asylum application as well as the three tests of the independence assumption around the threshold due to imprecise control of the assignment variable suggests that selective sorting does not play a major role in my setting.

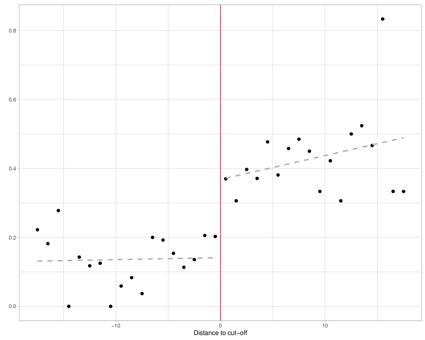
5 Results

Before discussing the estimates of the main identification strategy, I will first provide a visual inspection of the the relationship between the notification date of the asylum application, subsidary protection status, and the outcome variables. Figure 2 shows binned scatter plots between the notification date and subsidiary protection status as well as the three main outcome variables. Each dot in Figure 2 shows the mean value of the corresponding outcome by the month of the notification date. The red vertical line indicates the threshold date between March and April 2016, and the dashed lines are linear fits based on the mean values of each side of the threshold.

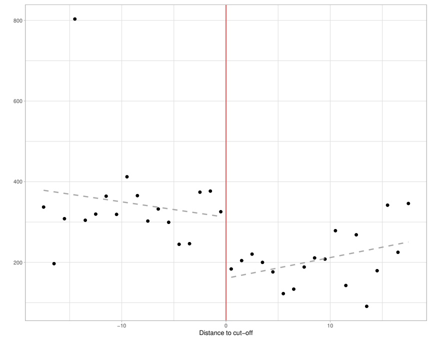
Figure 2a illustrates the discontinuity in the share of refugees who received subsidiary

[16]Figures A4a to A4m in the Appendix shows the corresponding RD plots. In Figures A4n to A4p in the Appendix, I follow Bauernschuster and Schlotter (2015) and show RD plots for predicted values of the treatment and outcome variables based on separate regressions of these variables on the full set of control variables. If predicted variables show a discontinuity at the threshold, this would indicate that differences in observable characteristics might be responsible for discontinuities of the treatment or outcome variables at the threshold. However, as shown in Figures A4n to A4p, this is not a concern here.

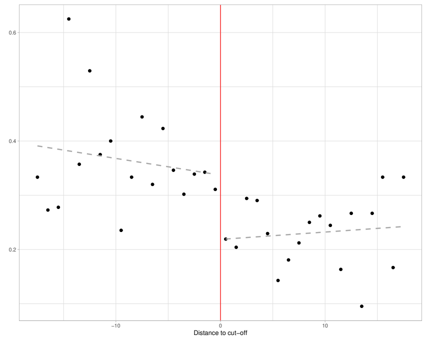
Figure 2:
RD plots, first-stage and outcome variables



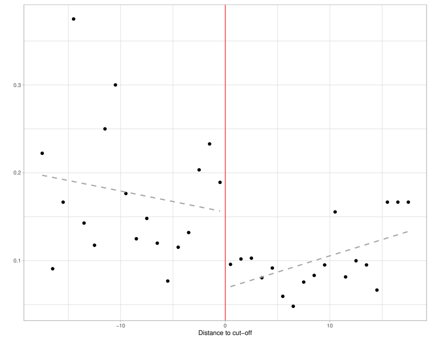
(a) Subsidiary protection



(b) Monthly labor earnings



(c) Any employment



(d) Full-time employment

Note: Mean of selected variables by value of the assignment variable with fitted lines on both sides of the threshold. Selected Bandwidth: 18 months.

protection status after the policy change. The observations left of the threshold indicate that the share of refugees who report in the last wave of the SOEP to have subsidiary protection status is almost entirely below 20 % before the policy change. On the other hand, this share increased to more than 35 % directly after the policy change and remains significantly higher afterwards. However, contrary to what discussion of the administrative data of the BAMF in Section 2.2 suggested, the share of refugees with subsidiary protection is considerably above zero before the threshold. A possible explanation for the sizeable mismatch might be that the administrative data illustrates the share of protection statuses issued in each month based on first-time decisions and the survey data refers to the protection status during the last wave of the SOEP. As rejected refugees and refugees who do not obtain a protection status in accordance with the Geneva Convention can take court action against the decision, which might result in receiving a protection status or receiving a better protection status, this might explain the discrepancies between the administrative data and the data from the SOEP.[17]

Turning next to the relationship between the notification date and monthly labor earnings as shown in Figure 2b, again, a striking discontinuity around the cutoff can be observed. Average labor earnings were almost entirely above 300 Euros before the change in decision-making practice, which changed suddenly to around 200 Euros afterwards. A similar pattern - while less pronounced - can also be seen for the binary outcome variables *Any employment* in Figure 2c. While the average share of refugees with any employment is most of the time in the range between 30 and 40 % in the old policy regime, this pattern changes in the new policy regime where mean employment lies between 20 and 35% percent most of the time. A similar picture emerges when turning to the outcome variable *Full-time employment* in the Figure 2d. Here, the average share of refugees reporting to have full-time employment during the last wave of the survey drops significantly at the cutoff from 15 to 20% to around 10% or less after the policy change.

Table 1 reports results of the first-stage and reduced form estimates of the baseline instrumental variables estimation discussed in Section 4. The first row of Table 1 shows results for the estimates based on Equation (1) for various bandwidth and selections of the order of polynomial for the assignment variable. The second to fifth rows show the same estimation specification using the outcome variables as dependent variable instead of the treatment variable. Inference is based on Huber-White standard errors which are shown in parentheses.[18]

[17] Another explanation could be measurement error in the treatment and/or assignment variable due to misreporting. Measurement error in the treatment variable - which in case of a binary treatment variable would lead to an upward bias in a simple 2SLS procedure (Kane et al., 1999; Jiang and Ding, 2019) - seems to be unlikely as respondents are explicitly asked to check their German identification card which states the protection status on the backside. Measurement error of the assignment variable might be more important here as respondents might not remember the exact month of the notification of the asylum application. Measurement error of the assignment variable might lead to difficulties in identifying the LATE as the discontinuity in the assignment variable might vanish (see, e.g., Hullegie and Klein, 2010; Pei and Shen, 2016; Davezies and Le Barbanchon, 2017). However, as Figure 2 illustrates a sizeable discontinuity, I conclude that measurement error of the assignment variable is not a concern in this study.

[18] A large part of the literature uses standard errors clustered at the value of the running variable in RD designs with a discrete running variable as suggested by Lee and Card (2008) to account for model misspecification. Kolesár and Rothe (2018) show that such standard errors "do not guard against model misspecification, and that they have poor coverage properties." In particular, they show that clustered standard errors are substantially smaller than Huber-White standard errors in case of small to moderate bandwidths and that the actual coverage rate of confidence intervals based on clustered standard errors with nominal level 95 % might be as low as 58 %, while confidence intervals based on Huber-White standard errors have coverage much closer to 95 %. Since clustered standard errors are much smaller than Huber-White standard errors in my setting, I use Huber-White

Table 1:
First-stage and reduced form RD estimates

	(1)	(2)	(3)	(4)	(5)
First stage estimation					
Subsidiary protection	0.24***	0.18***	0.21***	0.19***	0.15**
	(0.04)	(0.05)	(0.04)	(0.05)	(0.06)
F-statistic	40	12	27	17	5
Reduced form estimation					
Any employment	-0.09**	-0.11**	-0.10**	-0.08*	-0.05
	(0.04)	(0.05)	(0.04)	(0.05)	(0.06)
Full-time employment	-0.09***	-0.11***	-0.10***	-0.10***	-0.13***
	(0.03)	(0.04)	(0.03)	(0.04)	(0.05)
Monthly earnings (excl 0)	-222.98**	-160.41	-214.69**	-227.33**	-248.20
	(90.95)	(127.90)	(98.82)	(115.23)	(157.01)
Monthly earnings	-142.74***	-158.98***	-148.44***	-145.30**	-152.76**
	(42.69)	(57.35)	(48.40)	(56.91)	(74.63)
Bandwidth selection	none	none	18	12	6
Polynomial order	1	2	1	1	1
Observations	1470	1470	1399	1238	782

Note: First stage and reduced form RD estimates for various polynomial orders and bandwidth selection choices. Each row shows estimation results for a separate outcome variable. Estimates for the outcome variable *Monthly earnings (exlc 0)* are based on a restricted sample of employed individuals. Huber-White standard errors are reported in parentheses.
Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The estimates shown in Table 1 overall confirm the conclusions drawn from the visual inspection. The estimated discontinuity in the likelihood of being a beneficiary of subsidiary protection induced by the policy change is positive and sizable across all specifications. Referring to the estimate in column (1) where all observations are included, the estimation result suggests that the policy change lead to an increase in the share of refugee migrants with subsidiary protection status by around 24 pp. The estimated coefficient becomes smaller when using a higher polynomial order of the assignment variable or only observations around the threshold within a selected bandwidth. However, I can reject the null hypothesis that the estimated coefficient is equal to zero in all specifications.

The estimated effect of the policy change on the likelihood of being in any employment or full-time employment is negative throughout all measures and specifications. Interestingly,

standard errors throughout the paper instead of clustered standard errors.

Table 2:
OLS and fuzzy RD estimates

	OLS estimate	IV estimate	Control complier mean
Any employment	-0.07	-0.37**	0.64***
	(0.05)	(0.17)	(0.15)
Full-time employment	-0.09**	-0.40***	0.45***
	(0.04)	(0.13)	(0.12)
Monthly earnings (excl 0)	-250.87**	-770.57**	1327.70***
	(116.31)	(341.75)	(284.57)
Monthly earnings	-137.92***	-603.92***	761.01***
	(52.19)	(196.81)	(182.36)
Observations	396	1470	1470

Note: OLS (column 1) and 2SLS (column 2) estimates of the effect of subsidiary protection status on various labor market outcomes. Each row reports results for a separate outcome variable. The first column reports OLS results of the effect of subsidiary protection status on labor market outcomes based on subsample of observations close to the threshold (Bandwidth: 3 month). The second column reports instrumental variable estimates that corresponds to specification (1) in Table 1. The estimation of the corresponding mean of the control complier group follows suggestions by Cohodes (2020, p. 139-140). Huber-White standard errors are reported in parentheses.
Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

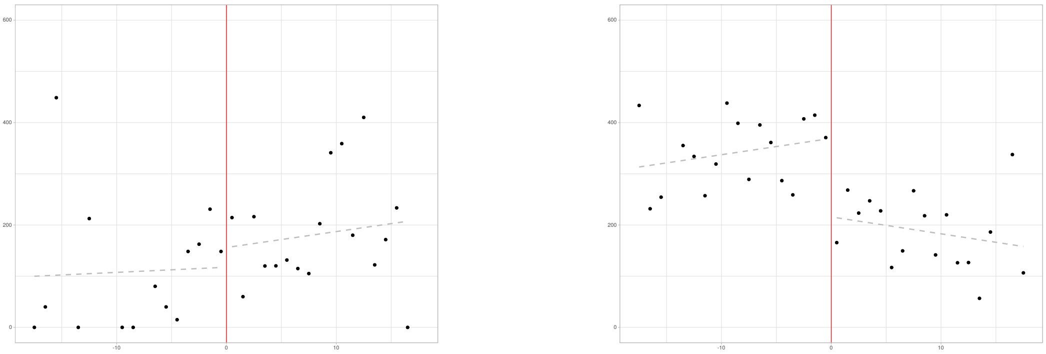
while the effect is slightly smaller in some specifications, the overall drop in employment by 9 percentage points (column 1) seems to be entirely driven by the drop in full-time employment. This result indicates that the policy change had an effect on the employment probability as well as on the share of full-time employed among all employed refugee migrants. The consequences of these two effects can also be seen in the change of monthly labor earnings as shown in the fourth and fifth row of Table 1. The fourth column of Table 1 shows estimation results for a sample of employed individuals. If the policy change would not have affected the composition of - in general, better paid - full-time and non-full-time employment among refugee migrants, I would expect the effect to be zero in this case. However, the effect is large and significant throughout almost all specifications and suggests that monthly labor earnings dropped by around 220 Euros per month among employed refugee migrants. When using the entire sample, as shown in the fifth column of Table 1, I also obtain negative effects of the policy change on monthly labor earnings, which is in line with the estimated negative consequences of the policy change on the overall employment probability.

The second column of Table 2 reports corresponding 2SLS estimates for specification (1) from Table 1. As explained above, the 2SLS procedure identifies the average treatment effect for the group of compliers, i.e., those refugee migrants who obtained subsidiary protection status only due to the policy change of the BAMF. To facilitate the interpretation of the effect, Table 2 also reports the mean of the control complier group, which estimates the mean values of potential outcomes of not having subsidiary protection status for the group of compliers. Focussing first on the estimated treatment effect in the second column of Table 2, the 2SLS procedure reveals large and significant negative effects of having subsidiary protection status on the likelihood of being employed as well as on monthly earnings. Subsidiary protection status reduces the likelihood of having any employment by 37 percentage points for the group of compliers, which implies that unemployment is twice as likely for those refugee migrants that receive subsidiary protection status due to the policy change.[19] Table 2 also makes clear that subsidiary protection status has an effect on the type of employment. While the share of full-time employment is at around 70 % (45/64) among employed refugee migrants for the untreated complier group, this number shrinks to 19 % ((45-40)/(64-37)) for those employed refugee migrants who received subsidiary protection status due to the policy change. The change in the composition of employment results in a significant drop in monthly labor earnings from around 1,330 Euros to 560 Euros among employed refugees in the complier group or from 760 Euros to 160 Euros among all refugees.

The first column of Table 2 reports coefficient estimates of a linear regression of each of the labor market outcomes on subsidiary protection status based on a subsample of individuals close to the policy change (bandwidth: 3 month). The OLS estimates give the average treatment effect of subsidiary protection status on labor market outcomes for individuals close to the threshold if the unconfoundedness assumption holds (Rosenbaum and Rubin, 1983), i.e., subsidiary protection status is not correlated with other variables that affect labor market outcomes such that treatment status is as good as randomly assigned for individuals close to the threshold. In general, this assumption is not fulfilled in fuzzy RD designs as individuals self-select into treatment based on incentives derived from the effect of the treatment variable on the outcome (Heckman et al., 1999). While self-selection seems to be not of a concern in this

[19] Unemployment in control complier group: 1-0.64=0.36. Unemployment in treated complier group: 1-(0.64-0.37)=0.73.

Figure 3:
Testing external validity of fuzzy RD design



(a) Subsidiary protection refugees **(b)** Geneva convention refugees

Note: Mean of monthly labor income by value of the assignment variable with fitted lines on both sides of the threshold conditional on protection status. Figure on the left (right) includes only refugee migrants who reported to have subsidiary protection status (protection status in accordance with the Geneva Convention). Selected bandwidth: 18 month.

study as treatment status is determined by a third party, there might be still systematic differences between refugee migrants with subsidiary protection status and Geneva convention refugees at the threshold if the granting of subsidiary protection status is targeted at a specific subgroup of individuals that might have different labor market perspectives. For instance, the unconfoundedness assumption is violated if asylum seekers with more dominant economic motives of migration or better labor market perspectives have a higher likelihood of receiving subsidiary protection status. Assuming that economic motives of migration affect labor market outcomes positively irrespective of protection status, OLS estimates will be upward biased, which implies under a constant treatment effect model, i.e., the effect of subsidiary protection does not vary across individuals, that IV estimates are larger than OLS estimates in absolute terms, which is in line with the results reported in Table 2.

In a heterogeneous treatment model, OLS and IV estimates might not only differ due to the violation of the uncounfoundedness assumption, it could also be the case that the average effect on compliers differs from the average effect on the other two subpopulation of *always-taker* and *never-taker* (Imbens and Angrist, 1994). Always-taker are refugee migrants who always receive subsidiary protection status irrespective of the policy regime to which they are exposed. On the contrary, never-taker are refugee migrants who receive Geneva convention protection status in the new and the old policy regime. A plausible procedure to assess the external validity of IV estimates is to compare average outcomes across compliance groups, i.e., of always-taker and treated complier and of never-taker and untreated complier (Angrist, 2004). If the average outcomes between these groups are not equal, this suggests that complier and always-taker (or never-taker) are substantially different and external validity of the IV estimates might be unlikely. I assess the external validity of the IV estimates in a fuzzy RD design in Figure 3, following Bertanha and Imbens (2020), and plot discontinuities of the outcome variable *monthly labor income* at the threshold conditional on protection status. In Figure 3a, observations close but left of the threshold consist of the subgroup of always-taker, and observations close but right to the threshold consist of always-taker and treated complier. In Figure 3b, observations close but left of the threshold consist of never-taker and untreated complier, and observations close but right to the threshold consist of never-taker. The discontinuity in average monthly labor income at the threshold is very small in Figure 3a, indicating that there is no substantial difference between always-taker and treated compliers. On the other hand, the large discontinuity in Figure 3b suggests substantial differences in labor market outcomes between never-taker and untreated complier. Since average income of untreated complier and never-taker are considerably larger than those of never-taker alone, it follows that untreated complier performing much better than never-taker in terms of labor market outcomes. These results suggests that the IV results are not informative for never-taker, and are consistent with the notion that the subgroup of complier consists of refugee migrants with *a priori* better labor market perspectives.

To further characterize the subgroup of complier, I report in Table 3 split sample estimates of the first stage equation by various characteristics. If compliers have, on average, better labor market perspectives, I would expect first stage estimates to differ for characteristics that are commonly attributed to increase labor market performance. In sum, the results reported in Table 3 support this view. First-stage estimates are much larger for subsamples restricted to males than for females, and younger individuals who are not married or do not have children in their household in comparison to their counterparts. Interestingly, first-stage estimates do

Table 3:
Complier characteristics

	No	Yes
Sample restricted to:		
Female	0.26***	0.13*
	(0.05)	(0.07)
Age 30 or older	0.25***	0.19***
	(0.07)	(0.05)
Married	0.37***	0.14***
	(0.07)	(0.05)
Children in household	0.37***	0.12**
	(0.07)	(0.06)
Located in West Germany	0.28**	0.20***
	(0.11)	(0.04)
College graduate	0.21***	0.21**
	(0.05)	(0.09)
Without prior work experience	0.24***	0.18**
	(0.05)	(0.07)

Note: Split sample estimates of first-stage equation by subgroup. Estimates correspond to specification (1) in Table 1. Huber-White standard errors are reported in parentheses.
Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

not differ with respect to education measured by being a college graduate but are slightly larger for individuals with prior work experience before migration than for individuals without prior work experience. The only result reported in Table 3 that does not support the view that compliers consists of individuals with better labor market perspective refers to the location, where the results of Table 3 suggest that the group of compliers is larger among refugee migrants located in East Germany than in West Germany.

6 Robustness

6.1 Robustness of the RD design

In this section, I provide a number of robustness tests for the RD design estimates of this paper. First, I construct a placebo sample of refugees who do neither have a protection status in accordance with the Geneva convention nor subsidiary protection status. This sample consists mainly of rejected asylum seekers or accepted refugees who are accepted on humanitarian reasons (suspension of deportation). If the discontinuity in labor market outcomes at the threshold for Geneva convention refugees and refugees with subsidiary protection was caused by the increasing share of refugees with subsidiary protection, I would not expect to see a similar discontinuity at the threshold for the placebo sample. Figure A5 and Table A3 in the Appendix show the relationship between the notification date and labor market outcomes and the estimates of the RD design for the placebo sample, respectively. As expected, the plots in Figure A5 do not show a clear and sizable discontinuity at the threshold. Moreover, the discontinuity is - if anything - positive. A similar conclusion can be drawn using the reduced form RD estimates in Table A3. In comparison with the estimates for the baseline sample, the placebo sample estimates are smaller and less precisely estimated across all specifications.

Second, I estimate the main results based on alternative definitions of the threshold. That is, instead of setting c equal to March 2016 in Equation (1) and (2), I use each month within an 18 month corridor around the original threshold in separate RD regressions as alternative cutoffs. If the result is indeed driven by the policy change, we would not expect to see similar large effects for alternative definitions of the threshold. Figure A6 shows coefficient plots of the estimated reduced form coefficients for the alternative cutoffs. Since the sample is relatively small, I see significant effects also for some of the alternative thresholds. However, the estimated effects become - except for one specification for the outcome variable *Any employment* - smaller. This is very reassuring for the identification strategy applied in this paper.

Third, I apply a Donut RD design. In a Donut RD desing, the observations close to the threshold are excluded. The idea of this design is to avoid biased estimates due to sorting around the cutoff. While the discussion of the validity of the RD design above suggests that sorting is unlikely to be of importance in this study, it cannot be ruled out entirely. Hence, it is informative to what extent the results are driven by the observations close to the cut-off. Table A4 in the Appendix contrasts the baseline results from the section above with those obtained by a Donut RD design. As can be seen in Table A4, the estimates are hardly affected.

Finally, I additionally control for the full set of pre-determined control variables. While a valid RD design does not require the inclusion of covariates in the regression, it might increase precision of the estimates. On the other hand, if I do not find significant effects after including a set of control variables, this might hint to non-random allocation of the instrument around the cutoff, indicating that the baseline effects from the Section above might be caused by differences in pre-determined variables around the cutoff. Table A5 shows the reduced form estimate after controlling for the set of control variables and indicates that the estimation results are hardly affected.

6.2 Exploiting time of asylum application

Fuzzy RD designs enjoy great popularity in applied economic research as they provide estimates of the LATE under a set of mild assumptions that can be credibly tested and visualized (Bertanha and Imbens, 2020). However, a potential downside of the fuzzy RD design is that the identification of the LATE depends heavily on observations close to the threshold. This might be particularly problematic in case of survey data where the number of observations are, in general, rather small. To address concerns that my estimates of the LATE are only driven by an unreliable small number of observations at the threshold, I propose a second identification strategy to estimate the effect of subsidiary protection status on labor market outcomes which exploits the policy change in an alternative setting. This identification strategy is based on a comparison of refugee migrants who entered Germany at the same month and applied for asylum in the same month but received notification about the decision of the asylum application before and after the policy change. The basic idea of this identification strategy is that whether applicant cohorts from the same arrival and application month receive notification about the asylum application before or after the policy change depends solely on factors that are unrelated to labor market outcomes of refugees - such as the number of applications a caseworker has to process. Formally, I estimate the following system of equations by 2SLS:

$$Sub_i = \delta_{ma} + \alpha_1 \mathbb{1}\left[t_i > c\right] + \eta_i \tag{3}$$

$$Y_i = \gamma_{ma} + \beta_1 \hat{Sub}_i + \epsilon_i \tag{4}$$

where δ_{ma} and γ_{ma} are month of arrival times month of application fixed effects and all other variables are defined as above. In Equation (3) and Equation (4), the inclusion of month of arrival times month of application fixed effects allows for any systematic variation in subsidiary protection status and labor market outcomes across cohorts that arrived in Germany in the same month and applied for asylum at the same month. Consequently, the estimation of α_1 - which measures the effect of being notified about the protection status after c on the probability of having a subsidiary protection status - and β_1 - which measures the effect of subsidiary protection status on labor market outcomes - is based on variation within cohorts that arrived in the same month and applied for asylum in the same month. Again, under the assumption that - conditional on the same arrival month and application month - receiving notification about the asylum application is as good as randomly assigned (independence) and does not affect labor market outcomes through other channels than protection status (exclusion), β_1 gives an estimate of the LATE for compliers.

Table A6 in the Appendix reports reduced form estimates of the effect of receiving notification about the asylum application after c on the probability of having subsidiary protection status (first row) as well as on labor market outcomes (second to fourth row) using month of application (column 1) and month of application times month of arrival (column 2) fixed effects. In column 3 of Table A6, I additionally add a set of control variables. Column 4 and 5 of Table A6 report results for the same specification using the placebo sample consisting of refugees without international protection as introduced above. Reported standard errors are clustered at the level of the arrival time application month. The first row of Table A6 makes clear that if decisions on asylum applications are made after March 2016, the likelihood of receiving subsidiary protection status is significantly higher even after flexibly controlling for the month of application, month of application times month of arrival, and adding control variables. The estimated results suggest that receiving notification after March 2016 lead to an increase in the probability to have subsidiary protection status by 23 percentage points. The corresponding F-test on the excluded instrument is between 62 and 45 which underlines the relevance of the instrument. Turning to the effects on labor market outcomes, Table A6 illustrates as expected the negative effects of receiving notification after March 2016 on the probability of being employed as well as monthly earnings. It is comforting to see that I do not find any effects for the placebo sample, which suggests that time until receiving notification does not *per se* affect labor market outcomes.

Table A7 in the Appendix reports the corresponding 2SLS estimates (column 2), estimation results of a linear regression of each of the labor market outcomes on subsidiary protection status controlling for arrival times application month fixed effects (column 1), and the estimation results obtained from the fuzzy RD design discussed above (column 3). While the 2SLS estimates from the fixed effect specification are smaller than the fuzzy RD estimates, they are still large and significant. The results suggest that subsidiary protection status reduces employment by 30 percentage points and monthly labor income by around 427 Euros. Similar to the results from the fuzzy RD design, the effect on employment seems to be largely driven by a reduction in full-time employment. Comparing the results with the OLS estimates in column 1, Table A7 illustrates, again, a large discrepancy, which suggests that the effect on complier might considerably larger as for other subpopulations.

Major concerns of this identification strategy are that the duration of the asylum procedure might have a direct negative effect on employment as suggested by Hainmueller et al. (2016), or that refugee migrants start only to look for employment after they receive the notification - which would reduce the time of job search for refugees receiving notification in the new policy regime in comparison to their counterpart - and might influence labor market outcomes directly. To assess if these concerns affect the estimation results, Table A8 in the Appendix shows estimation results of the IV strategy when additionally controlling linearly for (i) the time between application and receiving notification (column 2), (ii) the time between the notification and the interview date (column 3), or (iii) both (column 4).[20] The estimates reported in Table A8 show that including these control variables significantly reduces the power of the instrument and the IV results are estimated less precisely. However, as the point estimates become larger, this would suggest that both variables affect labor market outcomes positively.

In sum, the results shown in this section provide evidence that the RD design is robust to a

[20]Please note that the interview month varies between respondents which helps identifying the parameters in this approach.

number of checks and that the estimates of an alternative specification provide similar results to the fuzzy RD design which supports the obtained baseline estimates of a negative effect of subsidiary protection status on labor market outcomes.

7 Discussion

The negative effect of subsidiary protection status on labor market outcomes can be explained by changes in labor supply. Subsidiary protection status likely reduces the expected length of stay in Germany, which potentially affects integration efforts of immigrants (Dustmann, 1993, 1997, 1999; Cortes, 2004). Investments in host country-specific human capital - such as language skills, schooling and training, obtaining knowledge about the host country's institutions and production methods - might be of particular importance for refugee migrants as their relocation decision is not entirely based on economic considerations but often the result of *ad hoc* decisions triggered by violence and individual persecution, making refugee migrants less economically selected than economic migrants.[21] Consequently, refugee migrants' host country-specific human capital is, in general, lower than that of economic migrants upon arrival, which translates into lower levels of wages and employment (Brell et al., 2020). Lower level of human capital suggests high incentives for refugee migrants to invest in country-specific human capital as the costs of investments due to, e.g., forgone wages are lower and the rate on return of the investment might be higher (Chiswick, 1978). On the other hand, the uncertainty that refugee migrants face in terms of length of stay in the host country might counteract incentives to invest in country-specific human capital as it affects the time span that allows to reap the gains of the costly investment. Based on these considerations, more insecure protection statuses such as subsidiary protection status lead to lower investments in country-specific human capital and might worsen labor market outcomes.

On the other hand, there might also be labor demand side effects, which can explain the negative effect of subsidiary protection status on labor market outcomes, consistent with the increasing literature that shows that employment of immigrants is affected by labor demand

[21]For a survey of the adjustment of immigrants in labor markets, see Duleep (2015).

Table 4:
Fuzzy RD design estimates, perceived duration of stay and integration efforts

	Worries	Integration classes		Hours studying German	
	(1)	(2)	(3)	(4)	(5)
Subsidiary protection	0.59**	0.60***	0.65**	2.50***	2.33**
	(0.28)	(0.20)	(0.28)	(0.86)	(1.16)
Only unemployed	No	No	Yes	No	Yes
Observations	1454	1456	1060	1454	1061

Note: Fuzzy RD design estimates of the effect of subsidiary protection status on various outcomes measuring integration efforts. In column (1), the dependent variable is an ordinal response to the interview question: "Do you have worries that you cannot remain in Germany?" (1: no worries, 2: some worries, 3: a lot of worries). In column (2) and (3),the outcome variable is a binary variable whether a refugee migrant has attended a integration class in Germany. In column (4) and (5), the dependent variable is the number of hours an individual spends learning German per day. Estimates correspond to specification (1) in Table 1. Huber-White standard errors are reported in parentheses.
Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

side factors (Åslund and Rooth, 2007; Azlor et al., 2020). Kosyakova and Brenzel (2020) provide anecdotal evidence that German firms think that the conditions to hire refugee migrants are not clearly outlined, which might create uncertainty about the duration of a potential employment of refugee migrants. If employment of refugee migrants is costly, firms prefer to hire refugee migrants with better prospects of staying to regain their investment costs. This implies that firms might prefer to hire Geneva convention refugee migrants instead of subsidiary protection refugee migrants.

While the available data does not allow me to disentangle labor demand and labor supply side factors, I test in this subsection if subsidiary protection status also reduces refugees efforts to invest in host country-specific capital, which would be in line with the labor supply side explanation. Table 4 reports fuzzy RD design estimates for various outcomes measuring the refugees' uncertainty about the length of stay in Germany and investments in country-specific human capital. The dependent variable in column (1) is an ordinal measure of the answer to the interview question: "Do you have worries that you cannot remain in Germany?" ("no worries" is coded as 1, "some worries" is coded as 2, and "a lot of worries" is coded as 3). The dependent variable in columns (2) and (3) is a binary variable indicating whether a refugee migrant has attended an integration class in Germany, and the dependent variable in columns

(4) and (5) is a variable measuring the number of hours a refugee migrants spent studying German, and are intended to measure refugees' investments in country-specific human capital. While the estimate in column (1) of Table 4 is consistent with the idea that subsidiary protection status increases the uncertainty about the length of stay in Germany for refugee migrants, measures of investment in country-specific human capital are not negatively affected by subsidiary protection status. To test if this effect is driven by higher employment rates among refugee migrants with Geneva convention status, which might increase the opportunity cost to spend time in integration classes or studying German, I restrict the sample to those refugee migrants who are not employed at the day of interview in column (3) and (5). As the estimation results do not change for the restricted sample, I conclude that higher employment of Geneva convention refugees do not explain my findings. In sum, the results of Table 4 suggest that changes in refugees' labor supply due to subsidiary protection status do not explain the baseline findings of my paper, and labor demand side factors might be more important. A potential explanation for the positive effect on integration efforts could be that subsidiary protection refugees invest in host country-specific human capital to be able to prove in front of German authorities that they are willing to integrate in case their temporary residence permit might not be prolonged.

8 Conclusion

In this paper, I analyze the effect of refugees' protection status on labor market outcomes, focussing on a recent cohort of Syrian and Iraqi refugees. My empirical analysis exploits novel, plausible exogenous variation in the likelihood to receive subsidiary protection status due to a change in the assessment of the Federal Agency responsible for asylum claims to grant full refugee status in accordance with the Geneva convention. My results based on a fuzzy RD design suggest that subsidiary protection status has a substantial negative effect on labor earnings and employment probability, in particular, in the probability to be full-time employed. Further, I show in a detailed complier analysis that those refugee migrants who were affected by the policy change have *a priori* better labor market perspectives and have characteristics that are commonly attributed to improve labor market outcomes. My results are consistent with the causal mechanism that a reduction in the perception of permanent stay in the host country reduces refugees' willingness to invest in host country-specific human capital, which, in turn, reduces labor market performance. However, the results of the discussion section show that refugees with subsidiary protection invest even more in country-specific human capital, which suggest that there might exist also demand side factors that explain my results. In sum, my empirical analysis confirms the existence of an economic and political trade-off in asylum policies as granting permanent residence presumably induces political costs but provides economic and social benefits by reducing unemployment.

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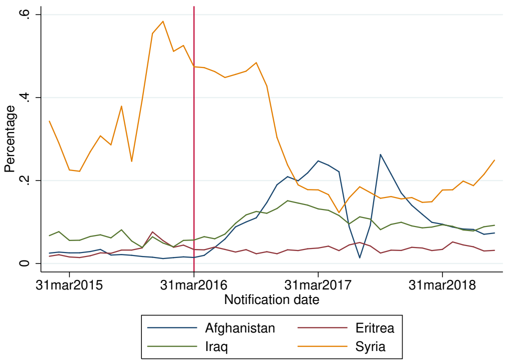
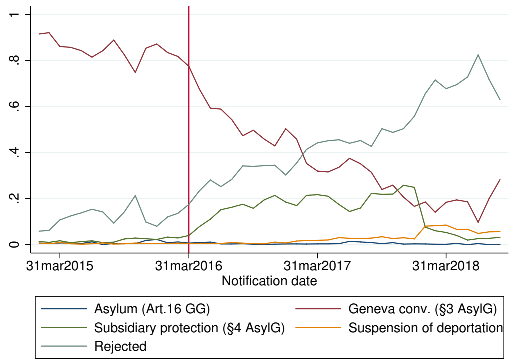
Figure A1:
Protection status and notification date



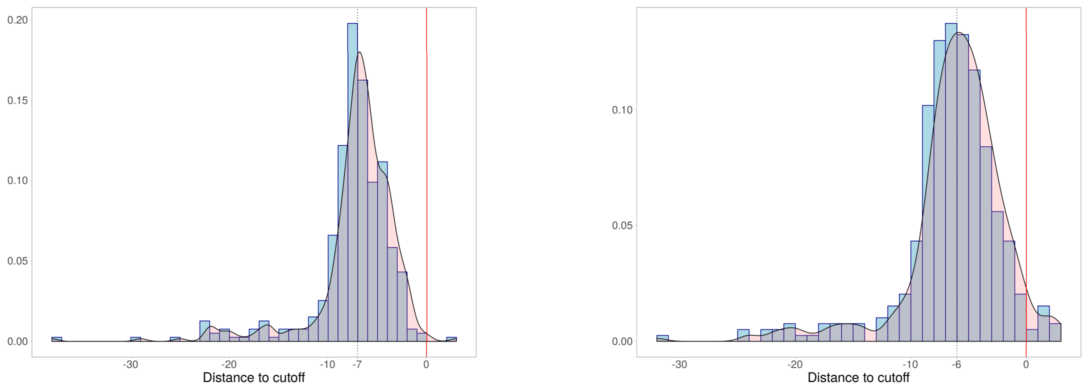
(a) Decisions by origin country (4 largest groups)



(b) Received protection status (Iraqi)

Note: Left plot shows the share of decisions made by the BAMF for asylum seekers of the four largest groups of asylum seekers by month of notification date. Right plot illustrates the type of protection status received by month of notification date for Iraqi asylum applicants. Source: Own calculations based on monthly published data from BAMF (data available upon request).

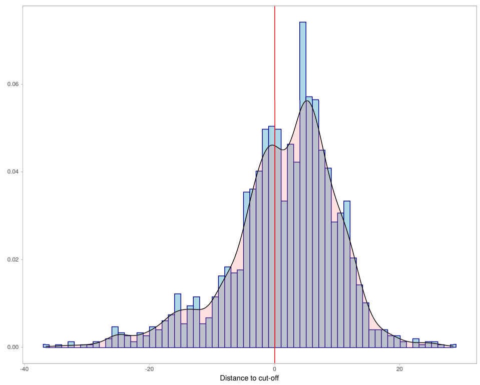
Figure A2:
Validity of RD design: arrival and application dates relative to policy change



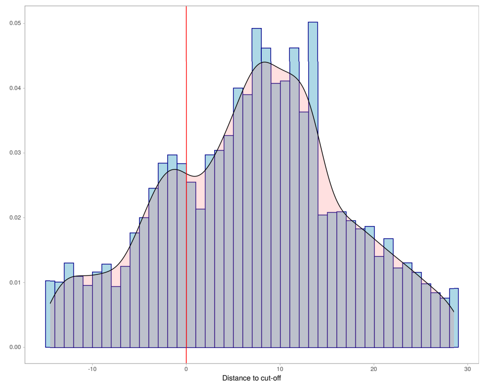
(a) Arrival month **(b)** Application month

Note: Normalized histogram and Gaussian kernel density estimate of the month of arrival (left) and application for asylum (right) - both relative to the time of the policy change (between March and April 2016) - for refugee migrants who received notification within a 3 month corridor before and after the policy change. Number of observations: 396. The dashed vertical lines indicate the (rounded) mean value of each plotted variable and the red vertical lines indicate the change in BAMF's decision making policy.

Figure A3:
Validity of RD design: density of assignment variable



(a) SOEP Sample



(b) Official asylum statistic

Note: Normalized histogram and Gaussian kernel density estimate of assignment variable month of notification about decision of asylum application (relative to cutoff). The red vertical lines indicate the change in BAMF's decision making policy. The graph on the left uses data from the SOEP. The graph on the right uses data from the official record of the BAMF.

Table A1:
Validity of RD design: mean differences, covariates and outcome

	BW: 18 month			BW: 3 month		
	$t < c$	$t > c$	t-val	$t < c$	$t > c$	t-val
Female	34	41	-2.6	35	36	-0.1
Age between						
18 and 35	54	61	-2.8	59	58	0.2
36 and 55	43	36	2.4	39	39	-0.1
55 and 65	4	3	1.2	2	2	-0.1
Married	64	67	-1.1	66	68	-0.6
No children in household	34	30	1.5	29	28	0.2
Age of youngest child in household between						
0 and 4	38	43	-1.8	43	42	0.2
5 and 10	18	18	-0.0	19	19	-0.0
11 and 15	10	8	0.8	9	11	-0.6
College graduate	23	20	1.2	22	23	-0.2
No work experience prior migration	33	39	-2.1	34	31	0.7
Work experience prior migration						
Self-employed or blue-collar worker	35	34	0.3	34	41	-1.3
White-collar worker	32	27	1.9	32	28	0.7
Located in East Germany	17	13	1.9	22	18	1.2
Years since migrating						
0 to 1	0	1	-1.6	0	0	1.0
2 to 3	76	97	-12.6	96	95	0.4
4 to 5	24	3	13.5	4	5	-0.7
Labor market outcomes						
Any employment	35	22	5.1	33	24	1.9
Full-time employment	17	9	4.6	21	10	3.0
Subsidiary protection	15	42	-10.6	18	36	-4.1
Observations	525	874		206	190	

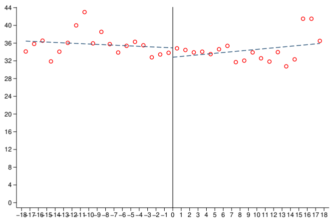
Note: Mean values of covariates (in percent) and t-values of mean-comparison test by value of the instrument for varying time spans around the cut-off.

Table A2:
Validity of RD design: RD estimates, covariates

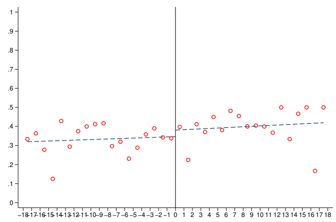
	$E[X]$	RD estimates				
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome:						
Age (in years)	34.37	-0.15	2.19*	0.39	2.12**	1.66
Female	0.38	0.05	0.02	0.04	0.04	-0.03
Married	0.66	0.06	0.04	0.04	0.06	0.01
No children in household (below 16)	0.32	-0.08*	-0.02	-0.05	-0.04	0.06
Youngest child in household: 0-4	0.41	0.06	-0.00	0.06	0.01	-0.04
Youngest child in household: 5-10	0.18	0.02	-0.03	-0.02	-0.00	-0.03
Youngest child in household: 11-15	0.09	-0.01	0.04	0.02	0.03	0.02
College graduate	0.21	-0.00	0.00	-0.00	-0.02	0.03
No work experience prior migration	0.37	0.04	-0.01	0.03	-0.01	-0.00
Self-employed or blue-collar worker	0.35	0.05	0.06	0.05	0.08	0.04
White-collar worker	0.29	-0.09**	-0.06	-0.08*	-0.07	-0.04
Located in East Germany	0.14	-0.03	-0.04	-0.05	-0.06	-0.07
Months since migrating	39.49	0.92**	0.88*	0.83*	1.69***	0.79
Bandwidth selection	none	none	none	18	12	6
Polynomial order		1	2	1	1	1
Observations	1470	1470	1470	1399	1238	782

Note: Mean value of covariates and corresponding RD estimates. Significant estimates are indicated with stars based on Huber-White standard errors. See RD plots of covariates and predicted outcome variables in the Appendix.
Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

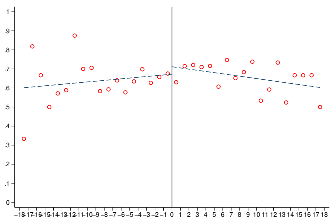
Figure A4:
Validity of RD design: RD plots, covariates and predicted outcomes



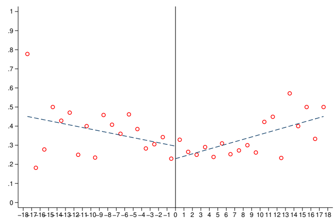
(a) Age



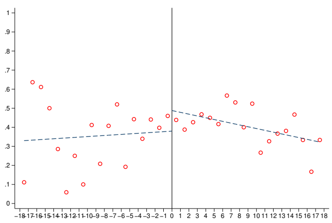
(b) Female



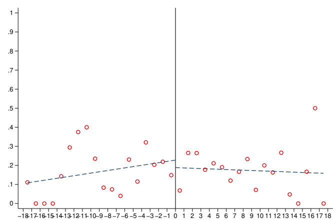
(c) Married



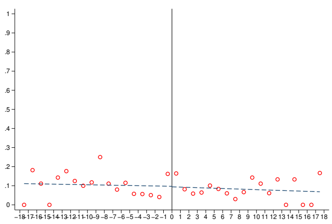
(d) No children in household



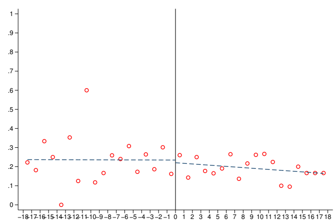
(e) Youngest child in household: 0 to 4



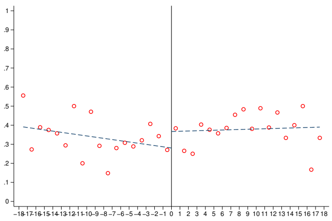
(f) Youngest child in household: 5 to 10



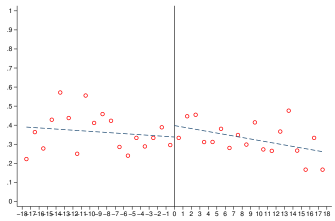
(g) Youngest child in household: 11 to 15



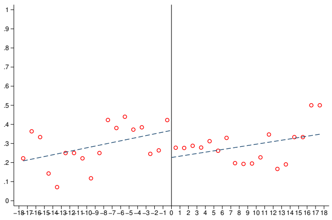
(h) College graduate



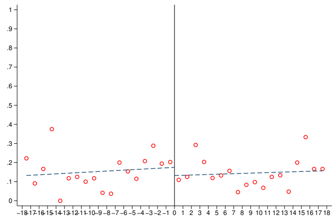
(i) No work experience prior migration



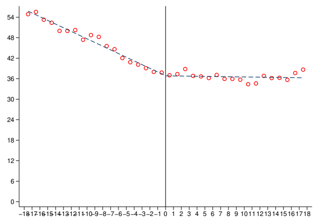
(j) Self-employed or blue-collar worker



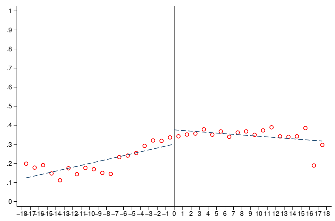
(k) White-collar worker



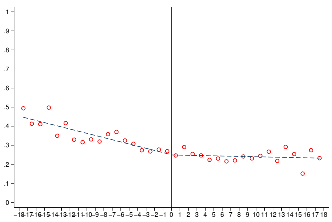
(l) Located in East Germany



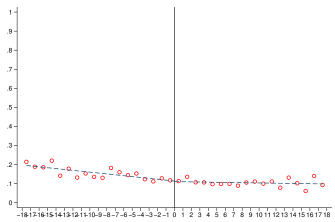
(m) No work experience prior migration



(n) Predicted subsidiary protection



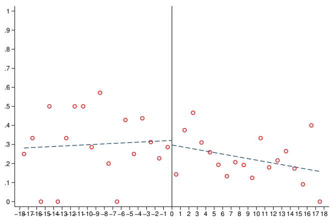
(o) Predicted employment (any)



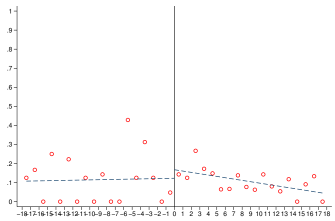
(p) Predicted employment (full-time)

Note: Mean of selected variables by value of the assignment variable with fitted lines on both sides of the cut-off.

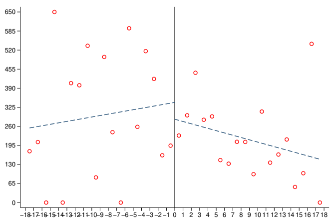
Figure A5:
Robustness: RD plots, placebo sample



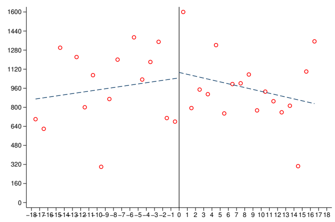
(a) Any employment



(b) Full-time employment



(c) Monthly earnings



(d) Monthly earnings (excl 0)

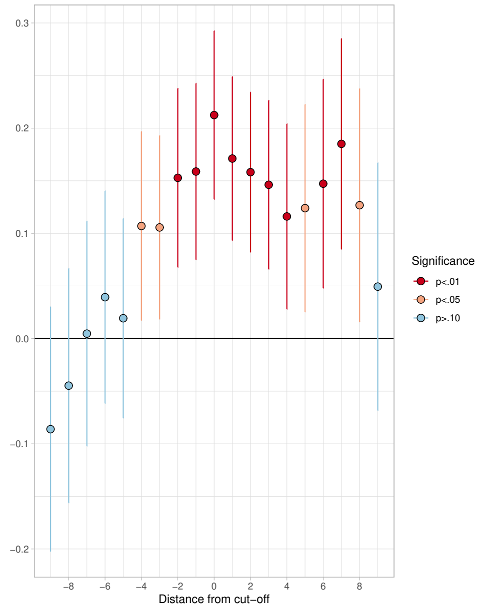
Note: Sample includes refugees who do not have an international protection status.

Table A3:
Robustness: Placebo RD estimates, reduced form

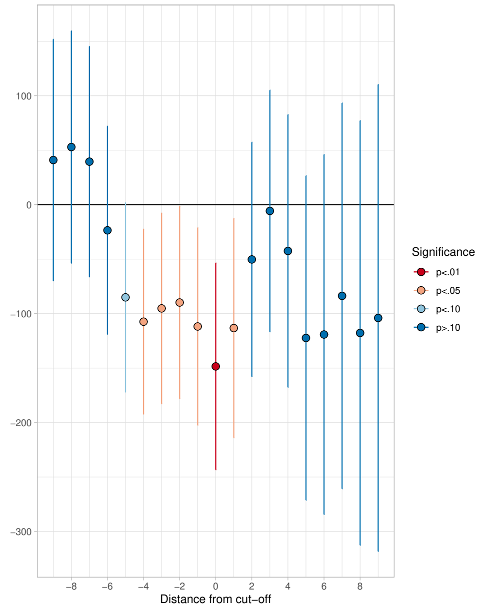
	(1)	(2)	(3)	(4)	(5)
Any employment	-0.05	0.04	-0.03	0.06	0.15
	(0.06)	(0.09)	(0.07)	(0.09)	(0.15)
Full-time employment	0.04	0.09	0.06	0.07	0.27**
	(0.05)	(0.07)	(0.05)	(0.06)	(0.11)
Net earnings (excl 0)	24.29	-24.33	-2.02	37.62	377.88
	(157.88)	(227.40)	(205.06)	(231.09)	(300.99)
Net earnings	-46.85	21.76	-35.96	60.10	252.93
	(77.84)	(109.63)	(91.61)	(106.60)	(159.45)
Bandwidth selection	none	none	18	12	6
Polynomial order	1	2	1	1	1
Observations	722	722	634	471	215

Note: Reduced form RD estimates for placebo sample. Huber-White standard errors are reported in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

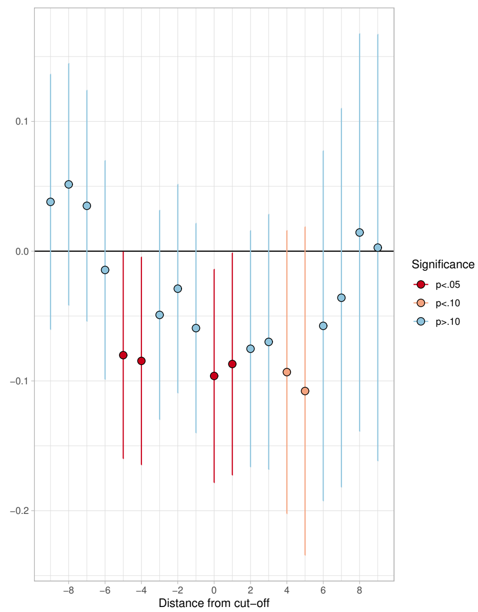
Figure A6:
Robustness: RD estimates, varying cut-off



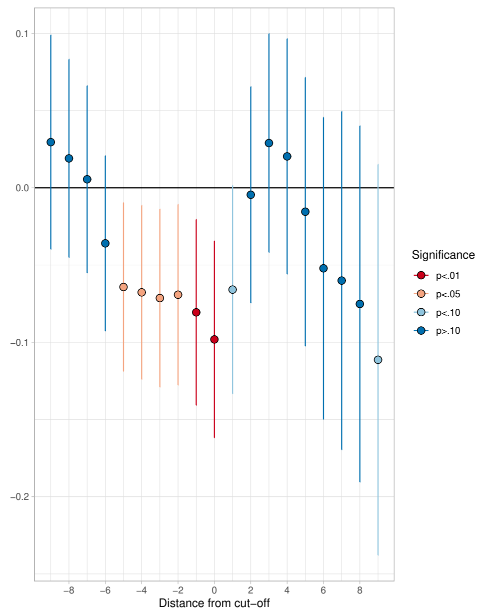
(a) Subsidiary protection



(b) Monthly earnings



(c) Any employment



(d) Full-time employment

Note: Plot of RD estimates and 95 % confidence interval for various cut-off based on baseline specification with first order polynomial and a selected bandwidth of 18 month.

Table A4:
Robustness: Donut RD estimates, 2SLS

	Donut IV estimate	IV estimate
Any employment	-0.33*	-0.37**
	(0.17)	(0.17)
Full-time employment	-0.36***	-0.40***
	(0.13)	(0.13)
Net earnings (excl 0)	-790.38**	-770.57**
	(360.38)	(341.75)
Net earnings	-549.66***	-603.92***
	(200.08)	(196.81)
Observations	1323	1470

Note: 2SLS estimates of the effect of subsidiary protection status on various labor market outcomes. Donut RD estimate is based on a sample that excludes observations one month before and after the cut-off (March and April 2016). Huber-White standard errors are reported in parentheses.
Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5:
Robustness: RD estimates, reduced form, covariates included

	(1)	(2)	(3)	(4)	(5)
First stage estimation					
Subsidiary protection	0.19***	0.16***	0.19***	0.19***	0.16**
	(0.04)	(0.05)	(0.04)	(0.05)	(0.06)
F-statistic	24	11	21	17	6
Reduced form estimation					
Any employment	-0.07**	-0.11**	-0.09**	-0.07	-0.04
	(0.04)	(0.05)	(0.04)	(0.05)	(0.06)
Full-time employment	-0.09***	-0.11***	-0.10***	-0.11***	-0.15***
	(0.03)	(0.04)	(0.03)	(0.04)	(0.05)
Monthly earnings (excl 0)	-236.62**	-196.18	-247.44**	-231.92**	-209.28
	(101.23)	(130.18)	(101.93)	(117.77)	(156.41)
Monthly earnings	-136.25***	-174.74***	-152.21***	-145.32***	-167.70**
	(43.20)	(54.82)	(46.28)	(53.59)	(71.13)
Bandwidth selection	none	none	18	12	6
Polynomial order	1	2	1	1	1
Observations	1470	1470	1399	1238	782

Note: 2SLS estimates of the effect of subsidiary protection status on various labor market outcomes. Huber-White standard errors are reported in parentheses.
Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6:
Robustness: Reduced form estimates, fixed effect specification

	Baseline sample			Placebo sample	
	(1)	(2)	(3)	(4)	(5)
First-stage					
Subsidiary protection	0.23***	0.23***	0.24***		
	(0.03)	(0.04)	(0.04)		
F statistic	62.40	39.58	42.31		
Reduced-form estimates					
Any employment	-0.09***	-0.08**	-0.07**	0.06	0.00
	(0.03)	(0.03)	(0.03)	(0.07)	(0.07)
Full-time employment	-0.06***	-0.07***	-0.07**	0.01	-0.05
	(0.02)	(0.02)	(0.03)	(0.05)	(0.05)
Monthly earnings	-116.75***	-108.31**	-102.66**	39.71	-31.63
	(35.66)	(43.71)	(40.51)	(77.67)	(77.35)
Application FE	Yes	No	No	No	No
Arrival x application FE	No	Yes	Yes	Yes	Yes
Control variables	No	No	Yes	No	Yes
Observations	1470	1470	1470	722	722

Note: Regression of subsidiary protection status (column 1) or labor market outcome on a binary variable indicating if an refugee migrant received notification of the asylum application after March 2016. Placebo sample consists of refugees who did not receive either Geneva protection status or subsidiary protection status. Cluster robust standard errors at the level of the arrival month time application month are reported in parentheses. Number of cluster: 371 (316, placebo sample).
Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A7:
Robustness: OLS and IV estimates, fixed effect specification

	OLS estimate	IV estimate	Fuzzy RD estimate
Any employment	-0.03	-0.30**	-0.37**
	(0.03)	(0.13)	(0.17)
Full-time employment	-0.05**	-0.28***	-0.40***
	(0.02)	(0.10)	(0.13)
Monthly earnings	-81.30**	-427.39***	-603.92***
	(31.91)	(150.24)	(196.81)
Month of arrival FE	No	No	
Month of application FE	No	No	
Arrival x application FE	Yes	Yes	
Control variables	Yes	Yes	
Observations	1470	1470	1470

Note: OLS and IV estimates of the effect of subsidiary protection status on various labor market outcomes. Excluded instrument in the IV estimation: binary variable indicating if refugee was notified about the decision of the asylum application after March 2016. The third column reports the fuzzy RD design estimates obtained in Table 2. Cluster robust standard errors at the level of the arrival month time application month are reported in parentheses. Number of cluster: 371.
Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A8:
Robustness: IV estimates, fixed effect specification

	(1)	(2)	(3)	(4)
Any employment	-0.30**	-0.42	-0.38	-0.43
	(0.13)	(0.28)	(0.27)	(0.28)
Full-time employment	-0.28***	-0.52**	-0.51**	-0.52**
	(0.10)	(0.23)	(0.23)	(0.23)
Monthly earnings	-427.39***	-559.63	-510.49	-570.83*
	(150.24)	(341.78)	(332.91)	(340.60)
F statistic	42.31	11.26	11.22	11.53
Month of arrival FE	No	No	No	No
Month of application FE	No	No	No	No
Arrival x application FE	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes
Application to decision (month)	No	Yes	No	Yes
Notfication to interview (month)	No	No	Yes	Yes
Observations	1470	1470	1470	1470

Note: IV estimates of the effect of subsidiary protection status on various labor market outcomes. Excluded instrument: binary variable indicating if refugee was notified about the decision of the asylum application after March 2016. Cluster robust standard errors at the level of the arrival month time application month are reported in parentheses. Number of cluster: 371.
Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.