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# 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 on labor market outcomes focusing on a recent cohort of Syrian and Iraqi refugees entering Germany between 2013 and 2016. My empirical analysis exploits a sudden and unpredictable change in the assessment of the Federal Agency responsible for asylum claims to grant full refugee status in accordance with the Geneva convention to refugees from these two countries in March 2016. Using data from the IAB-BAMF-SOEP survey of refugees and exploiting 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

In the last 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). In light of the increasing evidence that refugee migrants perform particularly worse in labor markets in Western Europe (Fasani et al., 2018; Brell et al., 2020) - even in the medium and long run (Bratsberg et al., 2014) - with potential negative consequences for future generations (Bauer et al., 2013), policies that improve the labor market integration of refugee migrants have the potential to bring large benefits to refugee migrants as well as their host countries. The literature on the economic assimilation of economic migrants suggests that the duration and permanence of stay is an important determinant of immigrants' economic integration as investments in destination country-specific skills largely depend on the time period in which immigrants can benefit from their investments (Dustmann, 1993, 1999, 2000; Cortes, 2004; Dustmann and Görlach, 2016).<sup>1</sup> This might be particularly important for refugee migrants whose relocation decisions are not, or only to a minor extent, based on economic considerations, which makes them less economically selected than economic migrants, 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 due to long waiting times for asylum claims or the lack of a clear perspective on permanent residence (Hainmueller et al., 2016; Dustmann et al., 2017). In particular, it has been noted that the various forms of protection that refugee migrants receive in the host country might be an influential factor for labor market integration as they typically differ in terms of the time frame they offer to refugee migrants to obtain permanent residence in the host country (Dustmann et al., 2017).

In this paper, I empirically investigate the link between different types of protection statuses and labor market outcomes, focusing on a recent cohort of refugee migrants from Syria

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

and Iraq who received one of the two most prevalent protection statuses - refugee status in accordance with the Geneva convention or subsidiary protection status - in Germany.<sup>2</sup> The German asylum system provides an interesting case for the goal of this study as both statuses offer refugee migrants equivalent access to the labor market and social security system. However, both statuses differ considerably in terms of the requirements to receive permanent residence in Germany. While Geneva convention refugees can apply for permanent residence 5 years after arrival in Germany if they meet minor requirements such as sufficient knowledge of the German language (A2 level) and being able to cover at least 50 % of their costs of living, subsidiary protection refugees need to prove that they have an acceptable command of the German language (B1 level), the ability to cover all of their costs of living, *and* contributed at least 60 months to the German social security system.<sup>3</sup> Considering that granted protection statuses are regularly checked and can be revoked if the reasons for obtaining a protection status are not applicable any more, the additional requirements for subsidiary protection refugees might severely affect their perception of the likelihood of permanent residence in Germany, with potential consequences for their integration efforts and labor market outcomes. On the other hand, the meritocratic elements included in the requirements might counteract such negative effects as they give higher incentives to be employed for subsidiary protection refugees who are willing to stay in Germany (Schammann, 2019).

The empirical analysis of this paper exploits a sudden and unpredictable change in the assessment of the German Federal Agency for Migration and Refugees (German: Bundesamt für Migration und Flüchtlinge, BAMF) to grant full refugee status in accordance with the Geneva convention to refugee migrants from Syria and Iraq. Asylum seekers qualify for full refugee status in accordance with the Geneva convention if the cause of flight was due to individual persecution resulting from an innate trait or membership of a social group (see also the discussion in Section 2.1). While Syrian and Iraqi asylum seekers who received notification about their asylum claim in the year 2015 or the first three month of the year 2016 were almost en-

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<sup>2</sup>As explained in Section 2.1, the German asylum system offers more than one form of subsidiary protection where one form of subsidiary protection is termed “subsidiary protection.”

<sup>3</sup>In general, German residents contribute to the social security system if they are employed and earn more than 450 Euros per month.

tirely granted full refugee status in accordance with the Geneva convention, the assessment of BAMF changed suddenly in the following months and around one-fifth of the asylum seekers receiving notification in April 2016 were granted only subsidiary protection. The share of refugee migrants who were granted subsidiary protection status remained high in the following months. As refugee migrants cannot precisely influence the timing of the decision on their asylum claim, and applied for asylum many months before they received the notification about their protection status, the change in the assessment of the BAMF provides valuable, and plausibly exogenous, variation in the likelihood to receive subsidiary protection status for refugee migrant who receive notification about their asylum claim close to change in the decision-making practice of the BAMF.

My empirical analysis is based on the comprehensive longitudinal IAB-BAMF-SOEP survey of refugees, which provides extensive information about the asylum procedure and socioeconomic background for a sample of refugee migrants who entered Germany in the years 2013 to 2016. Most importantly for this study, the survey collects information about the year and month at which refugee migrants received the notification about their asylum claim as well as their current protection status and labor market outcomes. This allows me to exploit the change in the assessment of the BAMF in a regression discontinuity (RD) design, which generates causal estimates of the effect of the policy change on current subsidiary protection status 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. Based on a sample of Syrian and Iraqi refugees who reported to have either protection status in accordance with the Geneva convention or subsidiary protection status, I estimate a significant decline in the probability of being in any employment, in full-time employment, as well as in monthly labor earnings two and one-half years after the policy change for refugee migrants who received notification after March 2016. The drop in employment by around 9 pp is almost entirely driven by the reduction in full-time employment, which suggests that the policy change had an effect

on the employment probability as well as on the share of full-time employment among employed refugees. This is also reflected in the estimates for monthly labor income, where 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. Additionally, I show that the change in the assessment of the BAMF to grant refugee status in accordance with Geneva convention to Syrian and Iraqi asylum seekers can still be seen two and one-half years after the policy change. Using current reported protection status as outcome variable, RD design estimates indicate that the exposure to the new policy regime of the BAMF increased the share of refugee migrants with 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 for the subgroup of compliers, i.e., those refugee migrants who have subsidiary protection status only due to the change in the assessment of the BAMF. It seems plausible to assume that the change in the assessment of the BAMF targeted a specific group of refugee migrants, which suggests that the group of compliers differs from other refugee migrants. To address such concerns, I complement my baseline results with a complier analysis, and show that the change in the decision-making practice by the BAMF affected, indeed, more strongly refugee migrants with background characteristics that are commonly attributed to improve labor market outcomes for refugee migrants - such as male gender, younger age, 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 the likelihood of permanent residence in Germany, which, in turn, lowers the incentives to invest in country-specific human capital and the probability to be active in the labor market. Additionally, the size of the estimated effect suggests that the counteracting effect due to the meritocratic elements in the criteria to obtain permanent residence are only of minor

importance. In the final part of the paper, I test this causal mechanism, and show that there is no evidence that subsidiary protection status reduces refugees' investments in country-specific human capital. While my estimation results indicate that subsidiary protection status increases worries of refugee migrants 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 not necessarily be driven by labor supply side factors but instead by labor demand side factors.<sup>4</sup> For instance, if the employment of refugee migrants requires costly on-the-job training, firms prefer to hire refugee migrants with better prospects of staying to regain their investment costs. While the data at hand does not allow me to investigate this link, further research in this area might help understand the causal mechanism that explains the observed negative consequences of subsidiary protection status on labor market outcomes.

My empirical analysis provides quasi-experimental evidence based on micro data to confirm the existence of an economic and political trade-off in asylum policies as noted by Dustmann et al. (2017). Granting permanent residence status to refugee migrants presumably induces political costs, but provides economic and social benefits by reducing unemployment among refugee migrants in the society. This trade-off becomes particularly important if refugee migrants who are initially offered only temporary protection end up staying for longer in the host country because, e.g., the reasons for providing temporary protection do not change any time soon (Dustmann et al., 2017). The results of my paper suggest that optimal asylum policies should take into consideration the likelihood that the reasons for granting temporary protection status remain over a longer time period (5 to 10 years), and, in case the likelihood is high, to offer the same time frame to obtain permanent residence as for beneficiaries of full refugee status in accordance with the Geneva convention. To be aware of such a trade-off is also important in light of the efforts of the EU to harmonize the European asylum system, where to this day a large heterogeneity in granted protection statuses across countries exist

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<sup>4</sup>While the majority of the literature on the economic assimilation of immigrants focuses on supply side factors, it has been shown that the demand side is also an influential factor in explaining immigrants' employment (Åslund and Rooth, 2007; Azlor et al., 2020).

(Dustmann et al., 2017), and similar criteria to grant protection might be desirable.<sup>5</sup>

This paper contributes to the increasing literature studying the effect of asylum policies on labor market outcomes of refugees. Most closely to the question of this study, Fasani et al. (2018) exploit variation in refugees' exposure to high full refugee status recognition rates (measured by the ratio of the number of refugees receiving Geneva convention protection status to total number of decision made) across entry cohorts and within country or within entry cohort and across countries based on data from the European Labor Force Survey, and find that the exposure to high full refugee status recognition rates improves labor market outcomes. Another finding of Fasani et al. (2018) is that dispersal policies of refugees have a negative impact on labor market integration and, related to the question of dispersal policies, Brücker et al. (2020) show that residence requirements reduces 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 and find that lower income transfers increase job-finding rates of refugees, and Hainmueller et al. (2016) analyze the effect of the length of asylum procedure and find that they are negatively associated with labor market performance in Switzerland. This paper further relates to studies that investigate other policies that are intended to facilitate refugees' labor market integration such as language and integration courses. Arendt et al. (2020) show that a Danish reform that expanded language classes for refugees positively affected employment and income, and Battisti et al. (2019) provide evidence that job-search assistance is conducive for employment prospects of refugees. More broadly, this study also relates to the literature on citizenship or legal status and labor market outcomes of immigrants. In particular, Gathmann and Keller (2018) find that faster access to citizenship increases employment outcomes of immigrants in Germany, and Devillanova et al. (2018) provide quasi-experimental evidence from an amnesty program in Italy and show that legal status of immigrants increase employment rates.

The rest of the paper has the following structure. In Section 2, I provide background information on the German asylum system (Section 2.1) and the change in the assessment of the

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<sup>5</sup>For a discussion of the harmonization of European asylum policies and potential benefits, see Hatton (2015).



BAMF to grant full refugee status to asylum seekers from Syria and Iraq (Section 2.2). In Section 3, I present my data set based on the IAB-BAMF-SOEP survey of refugees. In Section 4, I introduce the main identification strategy (Section 4.1) and provide a discussion of the validity of the RD design (Section 4.2). The baseline results are illustrated in Section 5. Robustness tests of the RD design (Section 6.1), and results of an alternative identification strategy (Section 6.2) can be found in Section 6. In Section 7, I discuss effects of subsidiary protection status on other integration efforts, and Section 8 concludes.

## **2 Institutional Background**

### **2.1 Asylum system and protection statuses in Germany**

The German Federal Agency for Migration and Refugees (German: Bundesamt für Migration und Flüchtlinge (BAMF)) acts in the field of duties of the German Federal Ministry of the Interior.<sup>6</sup> Part of its responsibilities is the management of the German asylum procedure, starting from the first registration of asylum seekers after entering the country until the final decision about the asylum application is made. Upon arrival and first registration, asylum seekers are distributed among a number of reception centers in Germany where they file an asylum application with the closest branch office of the BAMF. If Germany is responsible for the asylum application in accordance with the Dublin III regulation, the asylum applicant will be invited to attend an individual hearing.<sup>7</sup> The individual hearing is organized by the case worker of the BAMF that is responsible for the final decision of the asylum application, and might be attended by an interpreter and possibly a lawyer or another person if requested by the asylum seeker. During the individual hearing, asylum seekers state the reasons why they decided to flee and what kind of persecution they experienced. The individual hearing is of highest priority for asylum seekers as the information and evidence put forth in the hearing

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<sup>6</sup>This subsection is based on online information from the BAMF ([www.bamf.de](http://www.bamf.de)) and Tiedemann (2014).

<sup>7</sup>The Dublin III regulations state that the asylum application must be processed in the country that the asylum seeker entered first. The Dublin III regulations are a EU law and applied to all EU member states except Denmark plus Island, Norway, and Switzerland.

will be the main basis of decision-making for the case worker of the BAMF and - in case of an appeal against the decision of the BAMF - the only accepted evidence from the asylum seeker in front of a court.

Based on the information from the individual hearing and additional research on the credibility of the claim of the asylum seeker, the case worker of the BAMF makes a decision about the protection status of the individual. The case worker of the BAMF checks if one of the four protection status - (i) political asylum in accordance with the German constitution, (ii) refugee status in accordance with the Geneva convention, (iii) subsidiary protection status, (iv) suspension of deportation - can be granted to the applicant. If this is the case, the applicant will receive a positive notification about the asylum application as well as a temporary residence permit. If not, the applicant will receive a rejection letter and is obliged to leave Germany within 14 or 30 days depending on the reason for rejection. In case the rejected applicant does not obey to the obligation to leave Germany, he or she will be deported. If obstacles to deportation exist - such as missing documents - the rejected applicant might be tolerated to stay in Germany for a short period of time (usually around 3 month), after which the rejected applicant will be, again, obliged to leave the country.

As I will show below, the two most prominent protection status in Germany are protection status in accordance with the Geneva convention and subsidiary protection status. Political asylum in accordance with the German constitution is rarely granted as it requires asylum seeker to enter Germany from a country that is not considered a “safe” country. As all German neighbouring countries are considered “safe,” political asylum can only be granted to asylum seekers entering Germany by plane. To receive protection in accordance with the Geneva convention, asylum seekers need to prove that they have been persecuted because of either their race, religion, nationality, political opinion or membership of a particular social group. It must be an innate trait (e.g., skin color, gender) or a characteristics of the individual that is so crucial for his or her 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 to obtain full refugee status in accordance with the Geneva convention, they might obtain

subsidiary protection if they fear death, torture or other inhuman treatment in their country of origin. In case of subsidiary protection, persecution does not need to relate to specific traits but can be the result of violence in the course of civil wars. Lastly, if asylum seekers cannot be granted either of these three protection statuses, they might be suspended from deportation. This might be the case if deportation would lead to other human rights violations or the risk of life or freedom, e.g., if asylum seekers have a disease that cannot be treated in their country of origin.

It follows from this discussion that the main element that determines if asylum seekers obtain either refugee status in accordance with the Geneva convention or subsidiary protection status - the two protection status studied in this paper - is that asylum seekers can prove that individual persecution was due to an innate trait or a crucial characteristic of their identity. As I will discuss in the next subsection, the assessment of whether this criteria is fulfilled for the large number of asylum seekers from Syria or Iraq that entered Germany between 2014 and 2016, changed suddenly and noticeably in March 2016. The type of protection status that asylum seekers receive has important consequences. While both statuses allow for immediate access to the German labor market and social security system, they considerably differ in terms of the prospects to receive permanent residence in Germany.

Refugees in accordance with the Geneva Convention receive preferential treatment when applying for permanent residency.<sup>8</sup> With the notification about the decision about the asylum application, Geneva convention refugees receive an initial temporary residence permit for three years which can be prolonged for 2 additional years each time the residence permit expires and the reasons for granting the protection status are still applicable. Three years after arrival in Germany, Geneva refugee migrants can apply for a permanent residence permit if they can prove that they have a good command over the German language (C1 level) and are able to cover at least 75 % of their cost of living. Otherwise they can apply for permanent residency after five years if they show sufficient knowledge of German (A2 level) and are able

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<sup>8</sup>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.

to cover at least 50 % of their cost of living. Subsidiary protection refugees receive an initial residence permit of only one year which can be also prolonged for 2 additional years each time the residence permit expires and the reasons for granting the protection status are still applicable. Subsidiary protection refugees do not have a fast track to apply for permanent residency, but can apply also five years after arrival in Germany. However, the requirements they need to meet are more advanced as they need to show that they have acceptable command of the German language (B2 level), are able to cover all of their cost of living, and have contributed for at least 60 month to the German social security system.

## **2.2 Increase in irregular migration and changes in asylum policies**

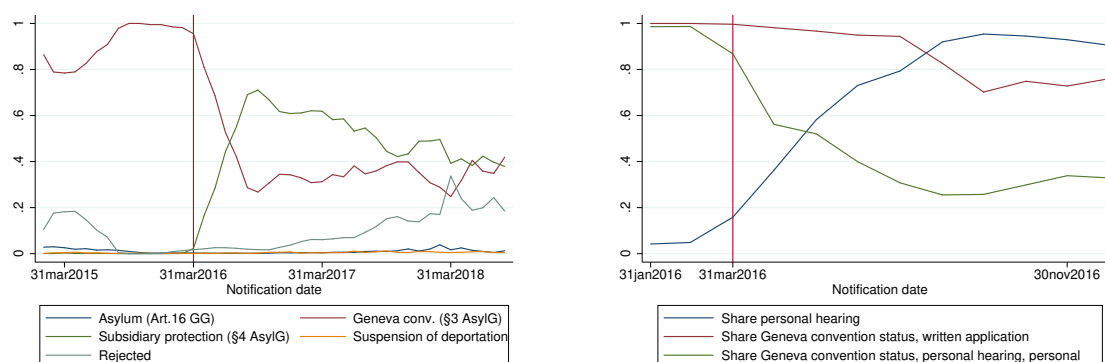
In the years 2014 to 2016, an unprecedented number of refugee migrants entered the EU with the intention to apply for asylum. The majority of these migration flows originated from Syria, Afghanistan, Iraq, and Pakistan, and first encountered terrain of the EU in Greece by crossing the Eastern Mediterranean sea by boat.<sup>9</sup> Many of these refugee migrants wanted to continue their journey to Northern member states of the EU - such as Germany, France, the UK, and Sweden, by transiting countries of the Western Balkan. According to the Dublin III regulations asylum seekers are required to apply for asylum in the country which they first entered. However, Southern countries of the EU - and in particular Greece - were overwhelmed by the number of refugee migrants, and let the majority of these refugee migrants pass their Northern borders - many times without registration. The large number of refugee migrants crossing European borders revealed the weakness of the European asylum system and its unequal distribution of refugee migrants among member states, putting pressure on Northern European countries to accept a larger number of refugee migrants to apply for asylum in their territory. In particular, Germany unilaterally suspended the Dublin III regulations and started to process a considerable share of the asylum claims that resulted from these migration flows.

The German asylum system was not prepared for the large number of refugee migrants and

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<sup>9</sup>Based 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 %).

**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).

Germany had to undertake major actions to increase its capacity to process the large number of asylum claims (Bertoli et al., 2020). In addition to transferring workers from other branches of the government to the BAMF (Grote, 2018), the German Federal Ministry of the Interior gave order to the BAMF in autumn 2014 to decide asylum claims from Syrian and Iraqi refugee migrants on basis of written asylum applications instead of personal hearings, with the intention to speed up the asylum procedure for these refugee migrants. The order was retracted in the beginning of the year 2016, after which, the standard procedure of personal hearings gradually replaced written applications (Deutscher Bundestag, 2016). The change in the type of the asylum application had consequences for the protection status of Syrian and Iraqi refugee migrants. Figure 1a shows the type of protection status received by Syrian asylum seekers per month of notification about the asylum claim. As illustrated in Figure 1a almost all Syrian refugee migrants were granted refugee status in accordance with the Geneva convention if they received notification about their asylum claim before March 2016 (rejections not considered), but the share of refugee migrants who received subsidiary protection status increased

noticeably afterwards.<sup>10</sup> At the same time, the share of decisions made on basis of personal hearings was increasing, as illustrated by the blue line in Figure 1b. More importantly, the assessment to grant full refugee status for decisions made on basis of personal hearings, as illustrated by the green line in Figure 1b, changed also significantly after March 2016, which indicates that the sudden increase in the share of Syrian refugee migrants with subsidiary protection was driven by the change in the type of application.<sup>11</sup> This suggests that the sudden change in the share of refugee migrants with subsidiary protection status was not due to compositional differences in the group of asylum applicants. Instead, this suggests that personal hearings lead case worker to decide differently on the question of individual persecution.<sup>12</sup>

As I explain in more detail in Section 4, another important assumption of the identification strategy of this paper is that receiving notification after the change in the assessment of the BAMF in March 2016 only affected refugee migrants likelihood to obtain subsidiary protection status instead of Geneva convention protection. This rules out that there were any other changes in asylum law that affected refugee migrants differently, depending on whether they received notification shortly before or after the policy change. Indeed, in mid March 2016, the German government passed a number of reforms of the German asylum law to further increase the speed of asylum procedures. The legislative package included a series of laws that affected the live of refugee migrants in Germany such as reductions of benefits for asylum seekers and rejected asylum applicants, ease of terms to deport rejected asylum applicants, or penalties for refugees who violate residential obligations. Almost all of these changes were targeted on rejected asylum applicants, and did not depend on the notification date of the asylum claim, except of one. With the passage of the law, asylum seekers that receive subsidiary protection status were subject to a temporary ban on family reunification. Before the law was passed, Geneva convention refugees as well as subsidiary protection refugees were

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<sup>10</sup>While less pronounced, the same pattern can be seen for asylum applicants from Iraq as illustrated in Figure A1b in the Appendix.

<sup>11</sup>Unfortunately, there is no data on the share of decisions made based on personal hearings for Iraqi asylum seekers.

<sup>12</sup>In Section 4.2, I provide a detailed discussion of the validity of the RD design, which involves a comparison of refugee migrants who received notification about their asylum claim shortly before and after March 2016, and based on observable characteristics, I do not find any differences among both subpopulations.

allowed to bring their spouse and children below the age of 18 to Germany after they were granted protection. Hence, refugee migrants who entered without their spouse or children and received notification after the policy change were not only affected by a higher likelihood to obtain subsidiary protection but also by the ban of family reunification. To avoid that I wrongly attribute changes in labor market outcomes to subsidiary protection status instead of being affected by the ban on family reunification, I exclude these refugee migrants from my sample as explained below.<sup>13</sup>

### 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.<sup>14</sup> 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 first stage estimates based on the discontinuity induced by the policy change, I exclude these

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<sup>13</sup>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.

<sup>14</sup>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).

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.<sup>15</sup> 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, marital 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.

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<sup>15</sup>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:

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

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

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).  $\mathbb{1}[t_i > c]$  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 ( $c$ ).<sup>16</sup>  $f(t_i - c)$  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.  $\eta_i$  and  $\epsilon_i$  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,

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<sup>16</sup>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.

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 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.<sup>17</sup>

The parameter of interest in this paper is  $\beta_1$  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*).<sup>18</sup> 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

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<sup>17</sup>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).

<sup>18</sup>By disentangling the assumption of independence and exclusion from the exclusion restriction, I follow Angrist and Pischke (2008) and Imbens and Lemieux (2008)

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 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.<sup>19</sup> 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.

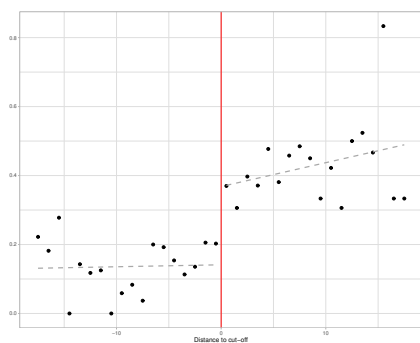
## 5 Results

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, subsidiary 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

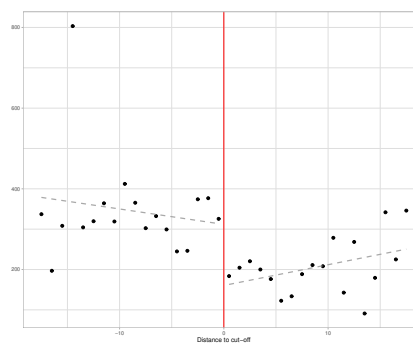
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<sup>19</sup>Figures A4a to A4m in the Appendix shows the corresponding RD plots. In Figures A4n to A4p in the Appendix, I follow Bauernschuster and Schlotter (2015) and show RD plots for predicted values of the treatment and outcome variables based 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.

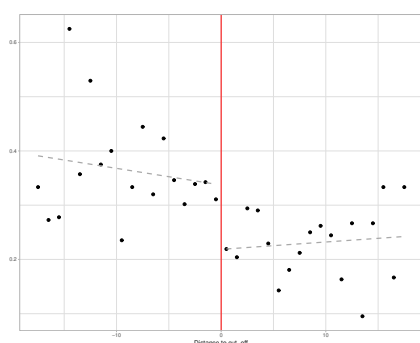
**Figure 2:**  
**RD plots, first-stage and outcome variables**



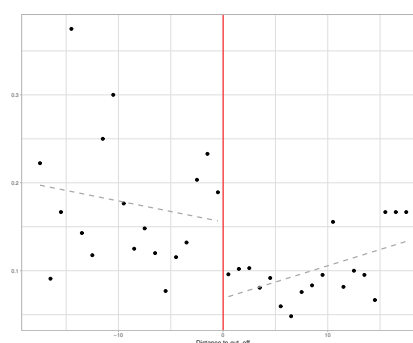
**(a)** Subsidiary protection



**(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.

side of the threshold.

Figure 2a illustrates the discontinuity in the share of refugees who received subsidiary 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.<sup>20</sup>

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.<sup>21</sup>

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<sup>20</sup> 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., Hulleger 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.

<sup>21</sup> 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.

**Table 1:**  
**First-stage and reduced form RD estimates**

	(1)	(2)	(3)	(4)	(5)
<i>First stage estimation</i>					
Subsidiary protection	0.24*** (0.04)	0.18*** (0.05)	0.21*** (0.04)	0.19*** (0.05)	0.15** (0.06)
F-statistic	40	12	27	17	5
<i>Reduced form estimation</i>					
Any employment	-0.09** (0.04)	-0.11** (0.05)	-0.10** (0.04)	-0.08* (0.05)	-0.05 (0.06)
Full-time employment	-0.09*** (0.03)	-0.11*** (0.04)	-0.10*** (0.03)	-0.10*** (0.04)	-0.13*** (0.05)
Monthly earnings (excl 0)	-222.98** (90.95)	-160.41 (127.90)	-214.69** (98.82)	-227.33** (115.23)	-248.20 (157.01)
Monthly earnings	-142.74*** (42.69)	-158.98*** (57.35)	-148.44*** (48.40)	-145.30** (56.91)	-152.76** (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 (excl 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

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 standard errors throughout the paper instead of clustered standard errors.

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, 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

**Table 2:**  
**OLS and fuzzy RD estimates**

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

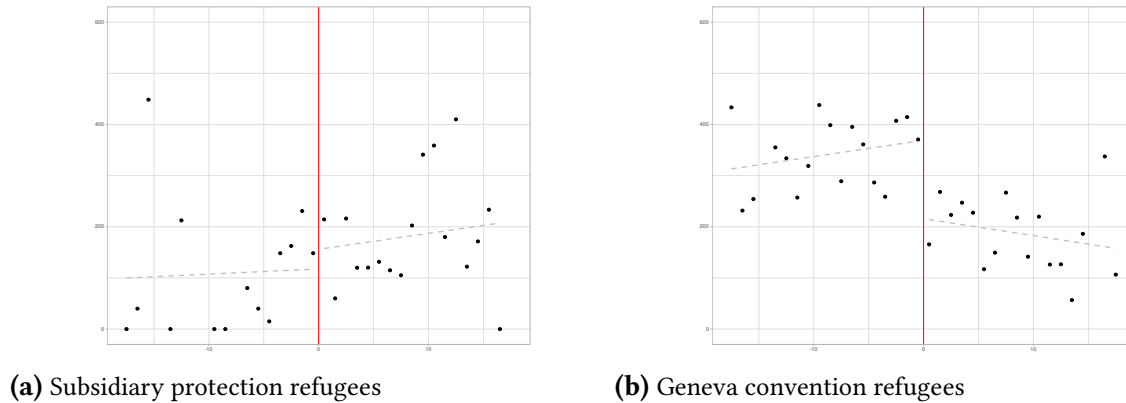
*Note:* OLS (column 1) and 2SLS (column 2) estimates of the effect of subsidiary protection status on various labor market outcomes. Each row reports results for a separate outcome variable. The first column reports OLS results of the effect of subsidiary protection status on labor market outcomes based 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: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

that receive subsidiary protection status due to the policy change.<sup>22</sup> 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 out-

<sup>22</sup>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**



*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.

comes 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 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 unconfoundedness 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

**Table 3:**  
**Complier characteristics**

	No	Yes
<i>Sample restricted to:</i>		
Female	0.26*** (0.05)	0.13* (0.07)
Age 30 or older	0.25*** (0.07)	0.19*** (0.05)
Married	0.37*** (0.07)	0.14*** (0.05)
Children in household	0.37*** (0.07)	0.12** (0.06)
Located in West Germany	0.28** (0.11)	0.20*** (0.04)
College graduate	0.21*** (0.05)	0.21** (0.09)
Without prior work experience	0.24*** (0.05)	0.18** (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: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

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 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 design, 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:

$$Sub_i = \delta_{ma} + \alpha_1 \mathbb{1}[t_i > c] + \eta_i \quad (3)$$

$$Y_i = \gamma_{ma} + \beta_1 \hat{Sub}_i + \epsilon_i \quad (4)$$

where  $\delta_{ma}$  and  $\gamma_{ma}$  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  $\alpha_1$  - which measures the effect of being notified about the protection status after  $c$  on the probability of having a subsidiary protection status - and  $\beta_1$  - 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),  $\beta_1$  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).<sup>23</sup> 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 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.<sup>24</sup> 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

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<sup>23</sup>Please note that the interview month varies between respondents which helps identifying the parameters in this approach.

<sup>24</sup>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** (0.28)	0.60*** (0.20)	0.65** (0.28)	2.50*** (0.86)	2.33** (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: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

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 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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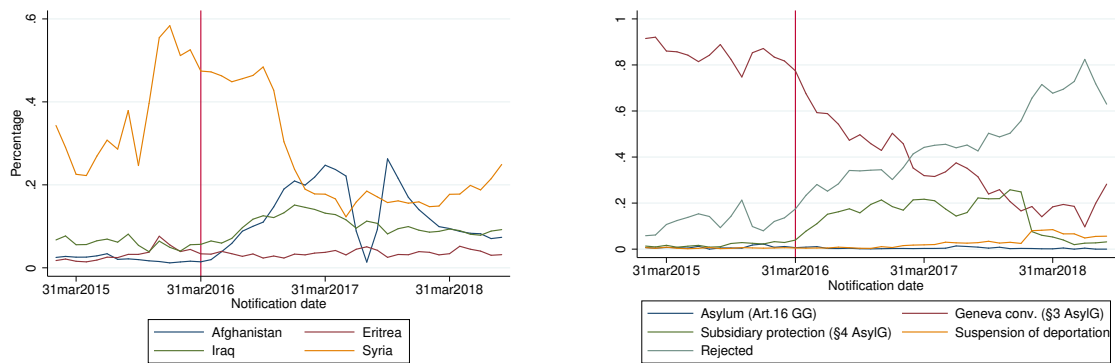
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**Figure A1:  
Protection status and notification date**

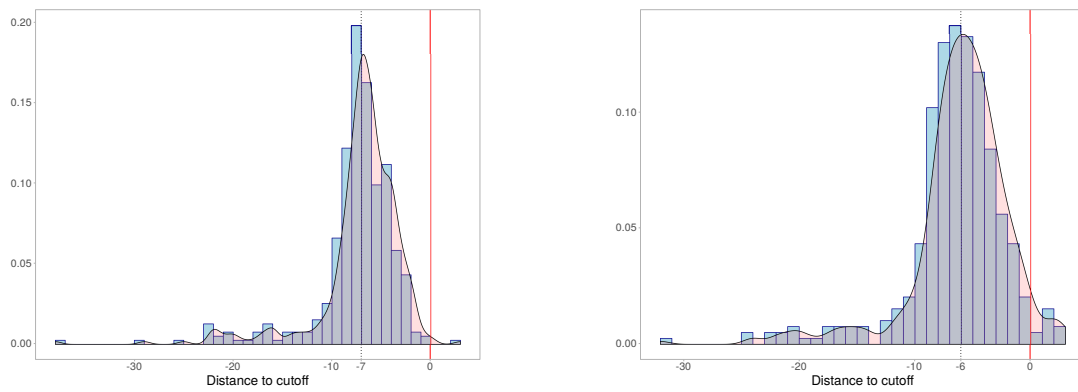


**(a)** Decisions by origin country (4 largest groups)

**(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**

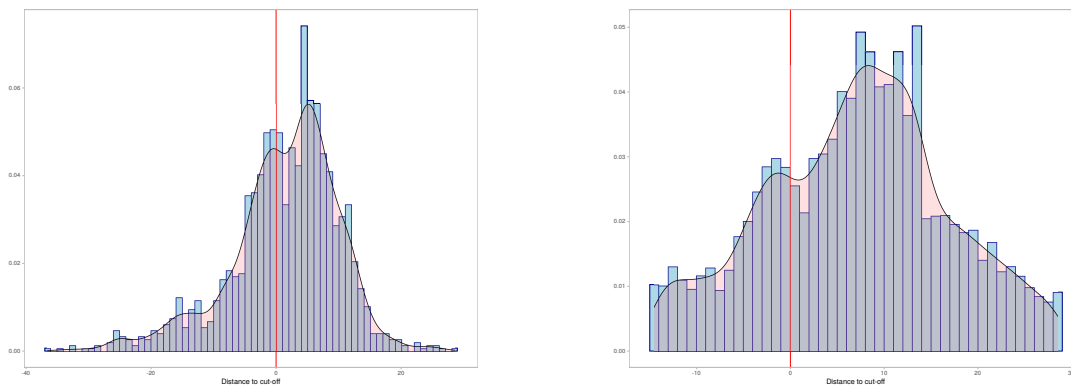


**(a)** Arrival month

**(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.



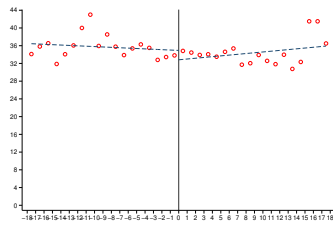
**Table A2:**  
**Validity of RD design: RD estimates, covariates**

	$E[X]$	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

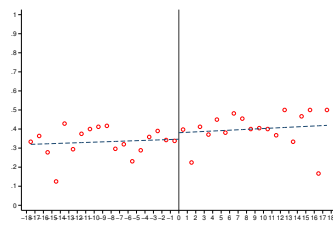
*Note:* Mean value of covariates and corresponding RD estimates. Significant estimates are indicated with stars based on Huber-White standard errors. See RD plots of covariates and predicted outcome variables in the Appendix.

Significance levels: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

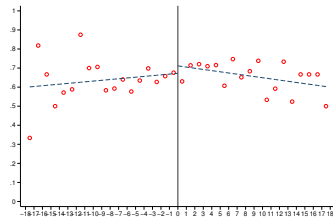
**Figure A4:**  
**Validity of RD design: RD plots, covariates and predicted outcomes**



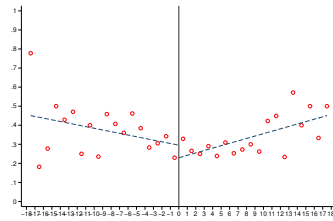
**(a)** Age



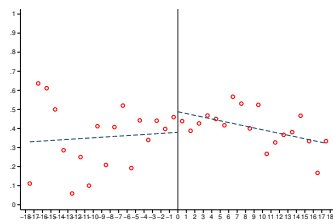
**(b)** Female



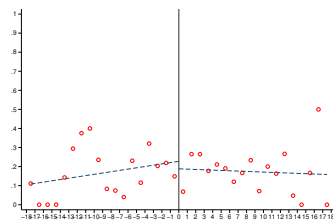
**(c)** Married



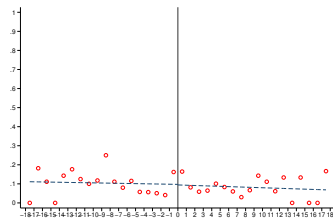
**(d)** No children in household



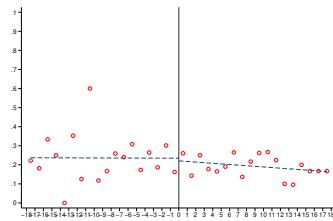
**(e)** Youngest child in household: 0 to 4



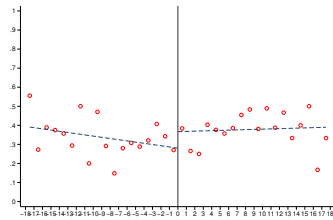
**(f)** Youngest child in household: 5 to 10



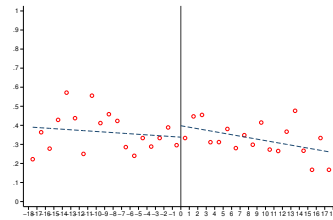
**(g)** Youngest child in household: 11 to 15



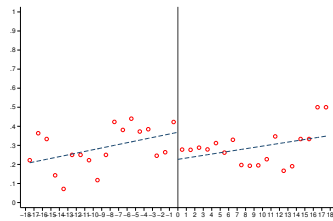
**(h)** College graduate



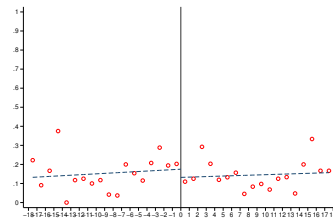
**(i)** No work experience prior migration



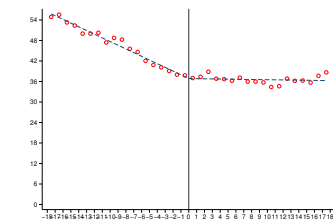
**(j)** Self-employed or blue-collar worker



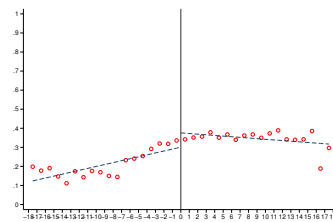
**(k)** White-collar worker



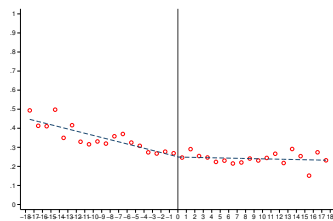
**(l)** Located in East Germany



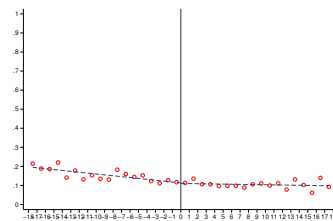
**(m)** No work experience prior migration



**(n)** Predicted subsidiary protection



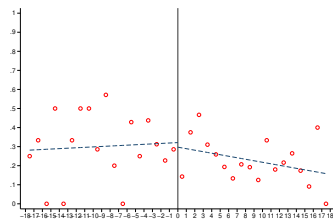
**(o)** Predicted employment (any)



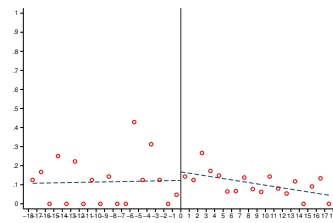
**(p)** Predicted employment (full-time)

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

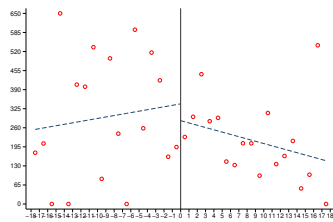
**Figure A5:**  
**Robustness: RD plots, placebo sample**



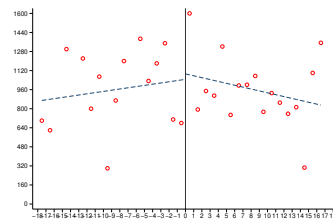
**(a) Any employment**



**(b) Full-time employment**



**(c) Monthly earnings**



**(d) Monthly earnings (excl 0)**

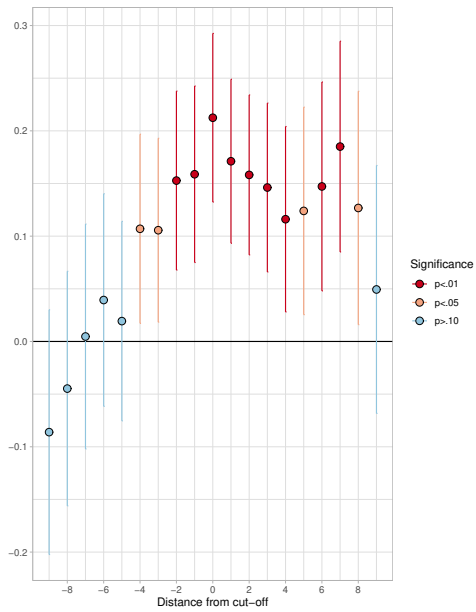
*Note:* Sample includes refugees who do not have an international protection status.

**Table A3:**  
**Robustness: Placebo RD estimates, reduced form**

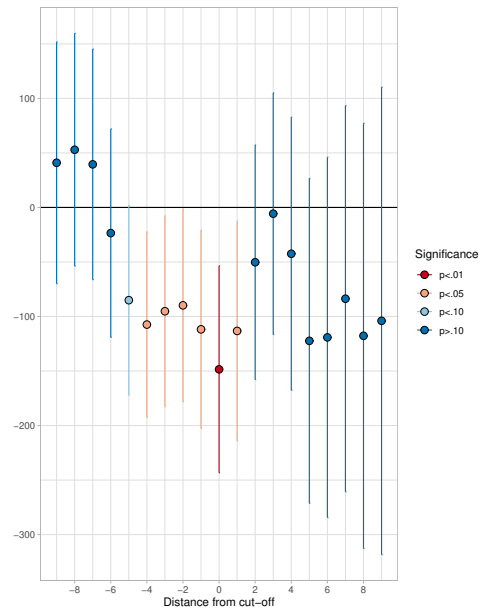
	(1)	(2)	(3)	(4)	(5)
Any employment	-0.05 (0.06)	0.04 (0.09)	-0.03 (0.07)	0.06 (0.09)	0.15 (0.15)
Full-time employment	0.04 (0.05)	0.09 (0.07)	0.06 (0.05)	0.07 (0.06)	0.27** (0.11)
Net earnings (excl 0)	24.29 (157.88)	-24.33 (227.40)	-2.02 (205.06)	37.62 (231.09)	377.88 (300.99)
Net earnings	-46.85 (77.84)	21.76 (109.63)	-35.96 (91.61)	60.10 (106.60)	252.93 (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: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

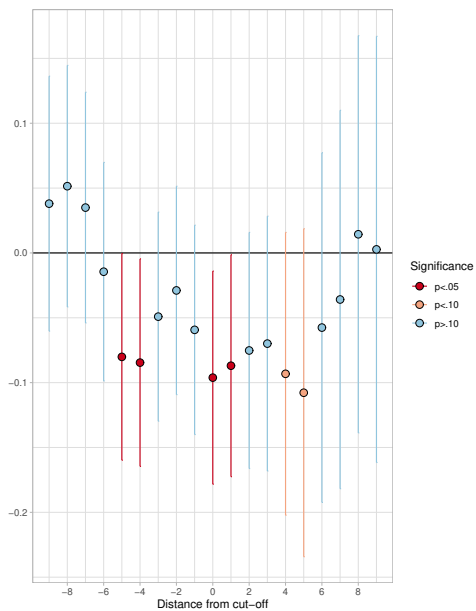
**Figure A6:**  
**Robustness: RD estimates, varying cut-off**



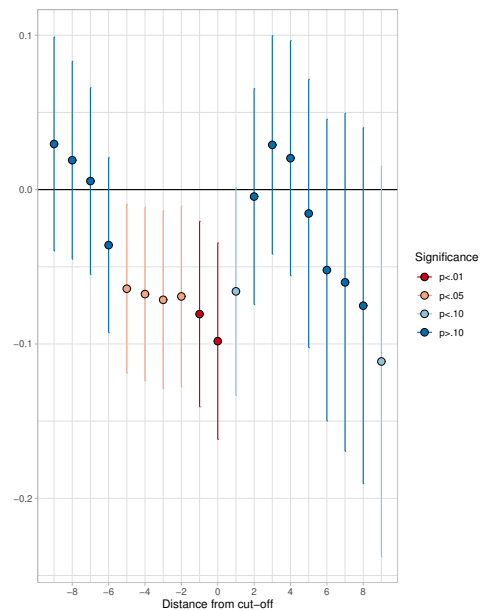
**(a) Subsidiary protection**



**(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* (0.17)	-0.37** (0.17)
Full-time employment	-0.36*** (0.13)	-0.40*** (0.13)
Net earnings (excl 0)	-790.38** (360.38)	-770.57** (341.75)
Net earnings	-549.66*** (200.08)	-603.92*** (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)
<i>First stage estimation</i>					
Subsidiary protection	0.19*** (0.04)	0.16*** (0.05)	0.19*** (0.04)	0.19*** (0.05)	0.16** (0.06)
F-statistic	24	11	21	17	6
<i>Reduced form estimation</i>					
Any employment	-0.07** (0.04)	-0.11** (0.05)	-0.09** (0.04)	-0.07 (0.05)	-0.04 (0.06)
Full-time employment	-0.09*** (0.03)	-0.11*** (0.04)	-0.10*** (0.03)	-0.11*** (0.04)	-0.15*** (0.05)
Monthly earnings (excl 0)	-236.62** (101.23)	-196.18 (130.18)	-247.44** (101.93)	-231.92** (117.77)	-209.28 (156.41)
Monthly earnings	-136.25*** (43.20)	-174.74*** (54.82)	-152.21*** (46.28)	-145.32*** (53.59)	-167.70** (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)
<i>First-stage</i>					
Subsidiary protection	0.23*** (0.03)	0.23*** (0.04)	0.24*** (0.04)		
F statistic	62.40	39.58	42.31		
<i>Reduced-form estimates</i>					
Any employment	-0.09*** (0.03)	-0.08** (0.03)	-0.07** (0.03)	0.06 (0.07)	0.00 (0.07)
Full-time employment	-0.06*** (0.02)	-0.07*** (0.02)	-0.07** (0.03)	0.01 (0.05)	-0.05 (0.05)
Monthly earnings	-116.75*** (35.66)	-108.31** (43.71)	-102.66** (40.51)	39.71 (77.67)	-31.63 (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 (0.03)	-0.30** (0.13)	-0.37** (0.17)
Full-time employment	-0.05** (0.02)	-0.28*** (0.10)	-0.40*** (0.13)
Monthly earnings	-81.30** (31.91)	-427.39*** (150.24)	-603.92*** (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** (0.13)	-0.42 (0.28)	-0.38 (0.27)	-0.43 (0.28)
Full-time employment	-0.28*** (0.10)	-0.52** (0.23)	-0.51** (0.23)	-0.52** (0.23)
Monthly earnings	-427.39*** (150.24)	-559.63 (341.78)	-510.49 (332.91)	-570.83* (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
Notification 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: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .