

# Uneven Rules: The Struggle for Refugee Family Reunification <sup>\*</sup>

Katia Gallegos Torres<sup>†</sup>

Andreas Steinmayr<sup>‡</sup>

Valentin Wett<sup>§</sup>

*This version: October 21, 2024.*

## Abstract

We examine a policy change for family reunification of refugees. The policy strongly restricted family reunification for subsidiary-protected refugees and allowed convention refugees to apply without those restrictions only immediately after receiving protection. Regular family reunification almost stopped for subsidiary-protected refugees. However, we find suggestive evidence of an almost offsetting increase in irregular family reunification. Convention refugees expedited their reunifications and overall increased the likelihood of sponsoring family members. Moreover, we find positive labor market effects for convention refugees but not for subsidiary-protected ones.

JEL classifications:

Keywords: immigration, refugees, uncertainty, integration

---

<sup>\*</sup>We thank Jan Stuhler, and participants of internal seminars at the ZEW, IAB, and the University of Innsbruck for helpful comments and suggestions. Andrea Buchacher and Karolina Hanger provided valuable research assistance. We acknowledge the financial support by the Faculty of Economics and Statistics of the University of Innsbruck. Gallegos Torres acknowledges financial support from the German Federal Ministry of Labor and Social Affairs through the Fördernetzwerk Interdisziplinäre Sozialpolitikforschung (FIS) under grant number FIS.00.00140.19. The computational results presented here have been achieved using the LEO HPC infrastructure at the University of Innsbruck. All remaining errors are our own.

<sup>†</sup>IAB Nuremberg, ZEW Mannheim, and University of Innsbruck. [katia.gallegos-torres@iab.de](mailto:katia.gallegos-torres@iab.de)

<sup>‡</sup>University of Innsbruck, CESifo, CReAM and IZA. [andreas.steinmayr@uibk.ac.at](mailto:andreas.steinmayr@uibk.ac.at)

<sup>§</sup>University of Innsbruck. [valentin.wett@uibk.ac.at](mailto:valentin.wett@uibk.ac.at)

# 1 Introduction

Many European countries have seen a significant influx of asylum-seekers in recent years, with peaks in 2015 and 2022. A highly debated policy question is how to manage and redirect migrant flows in light of these increased arrivals. This question is controversial and emotionally charged, fueled by ongoing reports of immigrants arriving in Europe by boats and illegally crossing borders. According to a recent poll, half of Europeans support stronger border controls ([Euronews, 2024](#)). The dissatisfaction with the EU and national migration policies has led countries to implement reforms to reduce these arrivals. Some countries have already implemented policies aimed at reducing “pull-factors” such as welfare benefits ([Dustmann et al., 2024](#); [Agersnap et al., 2020](#)), while some others, such as Germany and Austria, have restricted the possibilities for family reunification for refugees. Although family migrants represent about 40% of migration flows to OECD countries ([OECD, 2017](#)), there are no exact estimates of how many of those came as refugees.

So far, there is scant evidence that restricting legal family reunification reduces refugees’ arrivals. On the one hand, stricter family reunification rules might deter the primary migrant from choosing a particular destination, i.e., a redirection of the inflows. On the other hand, stricter rules make it harder to sponsor family members, but those might also find ways to arrive by other means. Additionally, there is little evidence on the role of family reunification on the integration of migrants and refugees themselves (e.g., [Bratsberg and Raaum, 2023](#); [Poeschel, 2020](#); [Löbel, 2020](#); [Löbel and Jacobsen, 2021](#)). Understanding how individuals react to policies and learning whether these had the intended impact is crucial for designing effective measures ([Adda et al., 2022](#); [Foged et al., 2024](#)). Policy reports from [UNHCR Österreich \(2017\)](#) and [Council of Europe \(2017\)](#) based on qualitative interviews with refugees describe positive integration effects of family reunification. However, there is a lack of causal estimates.

By exploiting an amendment of the asylum law in Austria in 2016 restricting the possibilities for regular family reunification, we provide causal estimates on how the reform affected refugees’ family reunification decisions and their posterior (labor market) integration.<sup>1</sup> The policy affected subsidiary-protected and Geneva Convention refugees differently. [Table 1](#) summarizes the amendments to the law by type of asylum granted. For convention refugees, an income requirement was introduced for applications filed more than three months after receiving protection, but no such requirement was necessary within the first three months. Thus, incentives to apply for family reunification within the first three months increased. For subsidiary-protected refugees, the new legal requirements reduced the possibilities to zero for the first three years and introduced hard-to-reach income requirements for applications thereafter. These policy differences arise from the Council Directive 2003/86/EC on the right to family reunification of the European Union for the convention refugees that each country member is free to apply ([Council of the European Union, 2003](#)). Although the guidelines for applying the directive suggest that the member states should not apply the three-month limitation for convention refugees, Austria decided to do so ([European Commission, 2014](#)). For both statuses, convention, and subsidiary-

---

<sup>1</sup>Family reunification refers only to the core family: partner/spouse, children, or parents of minor children. We distinguish between regular and irregular family reunification: regular reunification follows established immigration laws, while irregular reunification occurs outside of them, i.e., without an official procedure.

protected refugees, the amendment intended to make Austria unattractive for asylum-seekers (Czech, 2016). While convention refugees typically receive permanent protection, subsidiary-protected refugees must renew their status and are expected to leave Austria once their home country is deemed safe. Thus, the three-year waiting period aimed to enable family reunification only for those already well-established subsidiary-protected refugees with sufficient income in Austria.

The amendment, enacted on April 27, 2016, and effective from June 1, 2016, did not apply to refugees who applied for family reunification before that date. Using a regression discontinuity design (RDD) in time, we estimate the amendment’s causal effect on the likelihood of family reunification and labor market integration indicators. We show that one year after protection, potentially eligible convention refugees who got asylum after the amendment were 7.5 pp. more likely to have a family reunified member in Austria than those not affected by the reform, which primarily reflects a change in the timing, i.e., family reunification happens earlier. This increase aligns with the incentives created by the law for this group. On the contrary, for subsidiary-protected refugees, we estimate a reduction in the probability of regular family reunification by 7.1 pp. after the amendment. An alternative to regular family reunification is for the family members to arrive irregularly—without a visa—to Austria, apply separately for asylum, and later apply for family reunification within the country. We find suggestive evidence that irregular family reunification increased by 6.5 pp. for the subsidiary-protected refugees—although imprecisely estimated—hinting at a partial substitution from regular to irregular family reunification.<sup>2</sup> While we acknowledge that our measure for family reunification is not perfect, even an imprecise proxy is sufficient to identify treatment effects on a group level. The reform also imposed stricter income requirements for all subsidiary-protected and for convention refugees whose families applied for reunification three months or more after the sponsor got protection. Thus, we look at the sponsors’ employment and monthly labor earnings in Austria. In line with the hypothesis that having one’s family around boosts labor market integration, we find that treated convention refugees earn, on average, 97 euros more per month one year after protection. Their employment probability, although positive, only becomes statistically significant at the 5% from the third year after protection onwards. Three years after protection, treated convention refugees are 7 pp. more likely to be employed and earn, on average, 220 euros more. While the policy aimed at a similar income effect for subsidiary-protected—higher income was required to apply—, we find no statistically significant effect on either employment or earnings. Importantly, the new income requirements depended on the number of family members planning to apply. These income requirements were set so high (on average, 3,000 euros monthly gross income for a family of five) that it was almost impossible for subsidiary-protected refugees to reach this threshold.<sup>3</sup>

In addition, welfare benefits receipt did not count towards the income threshold, but refugees

---

<sup>2</sup>In our data, we do not have information on actual family reunification applications. We proxy this variable by the time difference between international protection of the sponsor and the arrival of a coinsured family member in Austria. See Section 3.2 for more details.

<sup>3</sup>Anecdotal evidence from interviews we have conducted with the Austrian Red Cross and an integration teacher support these hypotheses. Additionally, Figures A.5 and A.6 in the Appendix show the evolution of real income since obtaining protection for both statuses and confirm how unlikely it is to reach 3,000 euros monthly income. Five years after protection, both subsidiary-protected and convention refugees earn on average 1,000 euros monthly, and only about 4% have an income above 3,000 euros.

could still rely on them. However, as refugees take up employment and earn labor income, their benefits will be reduced or even terminated if certain income thresholds are reached.<sup>4</sup> We do not find an effect on welfare benefits receipt for either subsidiary-protected or convention refugees.

Table 1: When is it possible to apply for family reunification?

	<b>For applications before June 1, 2016</b>	<b>For applications after June 1, 2016</b>
Geneva Convention refugees	Immediately after receiving protection without a deadline.	Need to apply within three months of receiving protection. Otherwise, need to fulfil more requirements.
Subsidiary-protected refugees	One year after obtaining protection.	Only after three years of obtaining subsidiary-protection. And need to fulfil more requirements regarding housing, income, and health insurance.

Overall, we find that the amendment had the intended effects for each group regarding family reunification rates. Regular family reunification decreased for subsidiary-protected and increased for convention refugees. However, subsidiary-protected seemed to have partially substituted the regular for irregular family arrivals. On the labor market integration, only convention refugees show higher and statistically significant employment and income effects, while there are no such effects for the subsidiary-protected.

With our findings, we contribute to various strands of the literature. First, to the evaluation of policies/reforms aiming at reducing “pull-factors” for refugees (Dustmann et al., 2024) or limiting their mobility (Fasani et al., 2022) and labor market access (Ahrens et al., 2024; Fasani et al., 2021). The paper closest to ours is Bratsberg and Raaum (2023) who evaluate a Norwegian policy that increased income requirements in 2003 for subsidiary-protected refugees but not for convention ones. Although the motivation of the Norwegian policy was to quicker integrate refugees in the labor market and not deter arrivals, they find a 20 pp. decrease in family reunification rates. Second, on top of looking at the effect of a relevant policy, our findings contribute to the literature on an understudied topic: the role of family reunification in the integration of immigrants. Poeschel (2020) finds that (economic) migrants in the US focus more on work when their spouse is absent. But long-term wages (of the sponsors) decrease by around 2% per year of delay. Bratsberg and Raaum (2023) also find increased employment and labor earnings for male refugees subject to the income requirements but only during the first five years. Lastly, introducing uncertainty (e.g., by changing the rules in place) may reduce the incentives to invest in host country-specific human capital such as language and education (Adda et al., 2022). Furthermore, the lesser human capital investment hinders social and labor market integration and might lead to worse labor market outcomes in the long run (Foged et al., 2024). Moreover, family separation can be a psychological burden for refugees (Löbel and Jacobsen, 2021). Although we cannot investigate the effect of the amendment on human capital investments and mental health, we complement these findings by looking at labor market outcomes.

The labor market and social integration of immigrants are highly politicized topics. Our

<sup>4</sup>The specific thresholds vary by state and depend on factors such as family size and living costs.

findings shed light on how policy changes affect the integration of refugees, providing valuable insights for policymakers. It is crucial for them to consider how policies interact when deciding what laws to change and how. The rest of the paper is organized as follows. Section 2 provides information on the family reunification process for refugees in Austria and explains the amendment’s changes. Section 3 describes the data, sample, and provides descriptive evidence. Section 4 describes the empirical strategy and identification assumption. Section 5 presents the results and robustness checks. Section 6 concludes and provides some policy suggestions.

## 2 Background

### 2.1 Family reunification for refugees

Austria provides two types of protection for refugees: those recognized as refugees under the Geneva Convention and those granted subsidiary protection. Asylum-seekers who have a “well-founded fear of persecution” are granted a Geneva Convention status. If an asylum-seeker does not meet the criteria for refugee status under the Geneva Convention due to a lack of personal persecution in their home country, but their life would still be at risk if they were to return, they may receive subsidiary protection.<sup>5</sup>

Until early 2016, refugees protected under the Geneva Convention were allowed to apply for family reunification after receiving a positive asylum decision. Subsidiary-protected refugees had to wait one year after receiving protection before the application. In either case, there were no further requirements for the application. The application is not submitted by the sponsor in Austria but by the family in an Austrian embassy abroad. The family must get an appointment at the embassy and then submit their application. Waiting times for appointments vary by country and are often uncertain, with no official reports on the duration. According to interviews we conducted with the Austrian Red Cross (ÖRK) in 2023, the embassy in Damascus has a waiting time of about twelve months but was much faster back in 2015/16. The embassies in Turkey and Pakistan have a waiting period of four months. The family must cover travel and application costs themselves, making the process a significant investment, especially if there is no embassy in their home country (e.g., Afghanistan). A prerequisite is to have a passport, which poses an additional burden on the families. The ÖRK estimates that getting an Afghani passport costs about 1,500 euros (including application and travel costs). Family reunification applications cost about 200 euros per adult and 100 euros per child. Additionally, the family must provide a DNA test if there are no official marriage documents and parenthood certificates.<sup>6</sup>

Embassies forward the applications to the Federal Office for Immigration and Asylum (BFA), which reviews the application and grants a decision that can be a positive or negative “prediction”. The BFA determines how likely it is that the applicants and the sponsor are indeed a family, whether there is sufficient accommodation, health insurance, and income for adequate family life in Austria, and whether it is likely that the members would get at least the same protection status as the sponsor.<sup>7</sup> If the prediction is positive, the family members receive a

---

<sup>5</sup>[https://www.oesterreich.gv.at/themen/menschen\\_aus\\_anderen\\_staaten/asyl-in-oesterreich/Seite.3210001.html](https://www.oesterreich.gv.at/themen/menschen_aus_anderen_staaten/asyl-in-oesterreich/Seite.3210001.html)

<sup>6</sup>The Federal Office for Immigration and Asylum (BFA) reimburses the DNA test cost upon arrival in Austria.

<sup>7</sup>Sufficient accommodation is benchmarked by standard Austrian housing standards, health insurance is typ-

visa to travel to Austria, where they have to apply for asylum themselves. There are no official statistics on how long it takes for the BFA to grant a decision. However, the ÖRK estimates that in 2015/16, it took about six to eight months from the time the family applied at the Austrian embassy abroad until they arrived in Austria.

## 2.2 The 2016 Reform

In response to the increased number of asylum-seeker arrivals in 2015, the Austrian government submitted a draft for restricting the Asylum Law (*AsylG 2005*) to the parliament in January 2016.<sup>8</sup> This draft was debated and passed by the parliament on April 27, 2016. These changes came into effect on June 1, 2016. The major goal of the amendment was to make Austria a less attractive destination for asylum-seekers (Czech, 2016).<sup>9</sup> We focus in the amendment on the paragraph regarding family reunification (*AsylG §35 Abs. 2*).<sup>10</sup>

The amendment affected convention refugees differently than subsidiary-protected ones. For both groups, applications submitted after June 1, 2016 are subject to the new rules. The changes for both groups were as follows:

- **Convention refugees (KON):** Under the old law, applications for family reunification could be submitted any time after the sponsor received protection—there were no income requirements. Under the new law, family members must apply for family reunification at an Austrian embassy abroad within three months after the sponsor receives protection in Austria to avoid facing income requirements. Applications submitted after this period have to fulfill additional requirements regarding adequate income (depending on the number of family members), housing, and health insurance must be met. Although the embassies abroad had long waiting periods, the ÖRK mentioned that a way to apply on time was to do it in written form (via letter or e-mail). All convention refugees who received protection after June 1, 2016, therefore had at most three months to apply before facing the strict requirements. Those who had received protection before June 1, 2016 effectively had three

---

ically provided through employment in Austria, while income requirements for self-sufficiency are outlined in §60 AsylG and §11 NAG.

<sup>8</sup>During 2014–2017, Austria received about 170,000 first-time asylum applications. With 4.8 applications per 1,000 inhabitants, on average, Austria was one of the countries in the EU that received the most applications per capita (Eurostat, 2023).

<sup>9</sup>To gain insights on the pro and con arguments of the debate, we reviewed the stenographic protocols from January 27–28, and from April 27, 2016. During the parliamentary debates of the amendment, other reasons than reducing and regulating arrivals were also mentioned, such as relieving the authorities' capacity for asylum procedures and integration programs. In this way, they could focus on first integrating the refugees already in Austria and manage their capacities more efficiently. Additionally, some representatives argued that the restrictions could hinder integration since family separation implies a psychological burden and can increase social tensions. Although other parliament members mentioned family reunification as an essential factor for successful integration, there was no evidence supporting either argument (Republic of Austria, 2016a,b). Additionally, the amendment was also based on a general security concern: the restrictions were intended to help ensure public order and security in Austria.

<sup>10</sup>The second larger change was “Asyl auf Zeit” where convention refugees, who applied for asylum after November 15, 2015, instead of getting an unlimited residence permit as before, now get only a three-year residence permit, subject to renewal approval. In a robustness check, we drop convention refugees who arrived in Austria after November 15, 2015. Other smaller changes to the law included an extension to the deadline for the decision of the asylum authorities (from 6 to 15 months), the possibility of getting an interpreter via video (instead of in presence), and a prolongation of the time of arrest in case of a negative decision (from 120 hours to 14 days) (Asylkoordination Österreich, 2016).



months plus the time between receiving protection and June 1, 2016. Overall, the new law created an incentive to apply more quickly to avoid facing the stricter requirements.

- **Subsidiary-protected (SUB):** Under the old law, applications could be submitted only after one year after the sponsor received protection status and there were no income requirements. Under the new law, family members can only apply for family reunification three years after the sponsor has been granted subsidiary protection. Additionally, the sponsor in Austria must meet requirements regarding adequate income (depending on the number of family members), housing, and health insurance. A sponsor needs a gross monthly income of around 3,000 euros to meet the eligibility criteria for a family of two adults and three children. Subsidiary-protected who received protection before June 1, 2015, could, in principle, still apply under the old law. Those who received protection afterward would automatically be subject to the new law.

Figures A.1–A.3 in the Appendix show timelines for both convention and subsidiary-protected refugees under the new and old law to illustrate the assignment to treatment.

The changes for convention refugees were based on the Council Directive 2003/86/EC on the right to family reunification of the European Union that each country member is free to apply (Council of the European Union, 2003). Although the guidelines for applying the directive suggest that the member states should not apply the three-month limitation for convention refugees, Austria decided to do so (European Commission, 2014).

Other arguments during the parliamentary debate for the three-month rule for convention refugees were linked to their longer-term prospects of staying in Austria. This shorter waiting time was in line with international and European human rights standards that emphasize the protection and unity of the family. The rationale is that the rapid family reunification within three months supports refugees' psychological and social stability, making their integration considerably easier. However, applying within three months of protection might be almost impossible for some families, given the waiting time at the embassies and the time they spend traveling to them. In theory, this hurdle aligned with the main intention of the amendment, which was to reduce arrivals. A way to circumvent this problem was to send the application in written form to the embassy, and policymakers had not considered this path when proposing the amendment.

In contrast, for those with subsidiary protection, the focus was not on the right to family unity but on their precarious status, which can be revoked if conditions in their home country improve and are deemed safe. Furthermore, the representatives argued that the three-year waiting period should contribute to a better integration of the subsidiary-protected before the arrival of their family.

Although the amendment specified the dates and waiting periods for both groups, in practice, there probably was a “grace period” for the convention refugees.<sup>11</sup> Those convention refugees who had obtained protection before June 1, 2016, and whose families had not applied until then, could still apply until September 1, 2016, due to the authorities' flexibility, as reported by the ÖRK. This “grace period” was not officially documented in the amendment, and according to

---

<sup>11</sup>In our interviews with the ÖRK, who help the refugees with their applications, they told us about this “grace period” for convention refugees.

the ÖRK, it did not apply to subsidiary-protected individuals. Figure A.2 shows the relation between month of protection and months the refugees had to apply without facing restrictions. The dashed-line shows this relation without the grace period, while the blue solid line shows the relation that actually applied (with the grace period). It becomes clear that for refugees who received protection before June 1, 2016, but had not applied for family reunification, would also be treated and have more time until applying than just the three months. Although, the ÖRK mentioned that there was no grace period for the subsidiary-protected refugees, our data suggests that those subsidiary-protected who received asylum within 90 days from June 1, 2015, behaved similarly to the control group. To account for this possible “grace period” for both groups, we implement the RDD with a (one-sided) 90-day donut as a robustness check.

### 2.3 Alternative ways of arriving to Austria

An alternative to officially applying for family reunification is for the family to come to Austria anyway with smugglers and not officially file an application abroad. They could then request asylum on their own and apply for family reunification once in Austria (§ 34 *AsylG*) by declaring that family members already live there. Unfortunately, there are no official statistics on the number of inland family reunification applications, and we cannot directly identify these cases in our data. However, the timing of arrivals and granting protection allows us to identify family reunification that can impossibly have occurred through the regular family reunification channel.

## 3 Data and Descriptive Evidence

### 3.1 Data sources and sample

We use administrative individual-level data from the Austrian Social Security Database (ASSD) and additional data provided by the Federal Ministry of Labour and Economy (BMAW). We can merge both datasets with unique individual identifiers.

**The Austrian Social Security Database (ASSD).** The ASSD is a linked employer–employee dataset covering all Austrian workers and containing detailed information on labor market statuses ((un)employment spells and other insurance episodes). Additionally, it contains basic demographics, employment sectors, labor income, and coinsurance relations (Zweimüller et al., 2009). Asylum-seekers get a special health insurance upon arrival, coded “O4” or “OE” in our data. This classification can identify which individuals came as asylum-seekers to Austria. In Appendix Table B.1, we show that our data closely trace the official reports on asylum applications and status’ granted for our years of analysis (2014–2016). Our data coverage is better for subsidiary-protected (columns 4–6) than for Geneva Convention refugees (columns 1–3). The data for 2015 has the best coverage, with 73% of the total convention refugees in our sample and 100% of subsidiary-protected. To identify family relations, we use the table on coinsurances. Family members can be coinsured when they do not have statutory health insurance on their own. To be coinsured, they must reside in Austria (*Lebensmittelpunkt*).

**Additional data from BMAW.** The second data source provides detailed information



on refugee status (Geneva Convention or subsidiary protection), date of asylum, welfare benefit receipt, and further individual characteristics of refugees who have ever registered at the Austrian Public Employment Services (PES), i.e., for about 85% of all refugees.<sup>12</sup> This dataset allows us to identify the type of status granted to a refugee (convention or subsidiary protection) and the asylum date, which are essential to determine the relevant rules for family reunification and the respective cutoff date for the implementation of the amendment.

**Sample.** We restrict our sample to men who were the first of their family to arrive, aged 18-50 years at arrival and who received a subsidiary protection or Geneva Convention status. This age group is more likely to have nuclear families (underage children and spouses) abroad. We exclude those who declared themselves single upon arrival, as they are less likely to have nuclear families abroad. Additionally, we exclude family members of people who arrived at the same time in Austria or before asylum was granted to the sponsor, coinsured persons who are Austrian nationals, and children born in the same year as the start of the coinsurance spell as they were probably born in Austria.

### 3.2 Identifying family reunification

Since our data does not allow for direct identification of reunified family members, we generate a proxy variable based on the arrival date of the coinsured family members and the date the sponsor received protection. We define a person as a regular “reunified family member” if they arrived in Austria at least six months after the sponsor was granted asylum (for convention refugees) and at least eighteen months after the sponsor was granted asylum (for subsidiary-protected refugees).

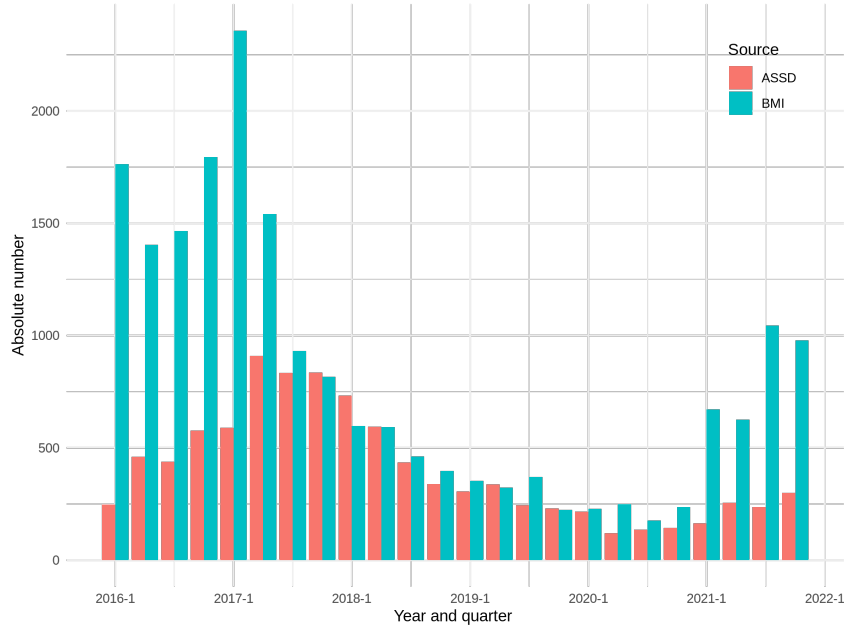
As previously mentioned, the ÖRK estimates that families typically arrive in Austria 6–8 months after submitting their applications abroad. Thus, for convention refugees, the earliest their family could arrive in Austria is six months after they received protection, assuming the application was submitted immediately. The earliest arrival time for subsidiary-protected refugees is 18 months after receiving protection, accounting for a 12-month waiting period to apply (as per the old law) plus an additional six months to arrive in Austria. We also create a variable to indicate whether the sponsor has a coinsured family member who arrived less than six months after receiving protection (for convention refugees) or less than eighteen months after receiving protection (for subsidiary-protected refugees). We interpret these variables as a proxy for “irregular” family reunification, as they suggest that family members arrived earlier than possible if they had arrived via regular family reunification. Importantly, family members we classify as “regular reunified family members” still might have come irregularly. However, it is unlikely that those classified as “irregular” went through the official family reunification scheme.

Figure 1 compares our proxy variable for regular family reunification based on the ASSD (red bars) and the official data on positive decisions from the Ministry of the Interior (BMI) for 2016–2022 by quarter (blue bars).<sup>13</sup> Both data series follow a very similar trend with a time lag,

<sup>12</sup>The incentives to register at the PES are large since many social benefits depend on doing so. However, if refugees found a job by themselves and never registered at the PES, we would be missing some characteristics of these people. Also, many refugees arriving via family reunification would probably not enter as “O4/OE” since they do not always enter the “Grundversorgung”.

<sup>13</sup>The BMI provided these data on “Prognoseentscheidungen gem. §35 Abs. 4 AsylG” upon request in March

Figure 1: Evolution of official positive family reunification decisions and the proxy based on ASSD data, 2016-2022, quarterly



*Notes:* The graph shows the evolution of positive decisions issued by the Ministry of the Interior (BMI, blue bars) and the headcounts of our proxy for reunified family members (red bars) during 2016–2022. The sample includes refugee men between 18 and 50 years old upon arrival in Austria who were not registered as “single” and were granted asylum between 2011 and 2021. We also eliminate those cases in which coinsurance was most likely based on a marriage in Austria, an arrival at the same time, or the birth of a child in Austria.

which is natural given the time between a positive decision and the arrival of family members, confirming that we can map family reunification with our proxy. The above-mentioned possible misclassification of irregular family reunification as regular family reunification might lead to an overestimation of regular family migration. But we also expect an undercount of family reunification, as we consider only cases from likely sponsors. Thus, we consider the similarity in the trends more informative than similarity in the levels.

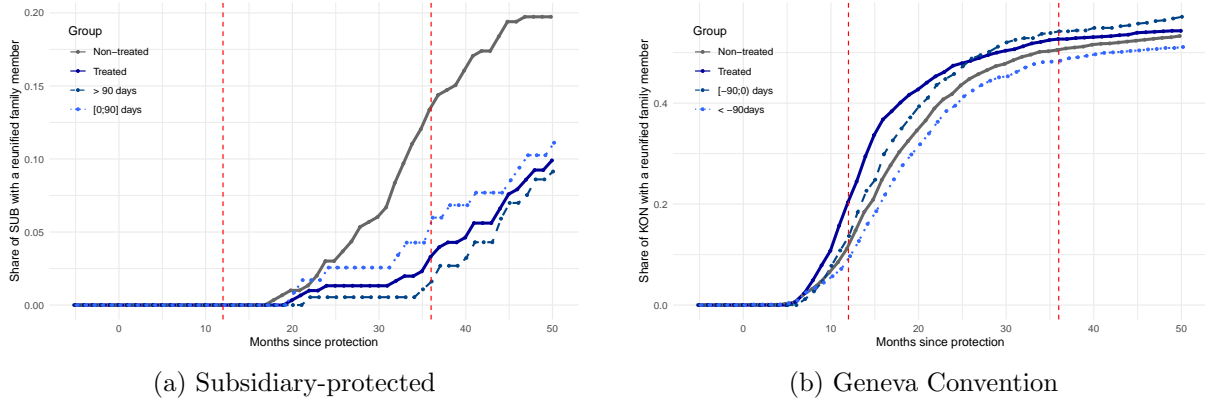
### 3.3 Descriptive evidence

We first examine the amendment’s impact on family reunification rates descriptively. Figure 2 shows the share of refugees by protection status (subsidiary or convention) in our sample with at least one reunified family member over time. We expect that subsidiary-protected refugees falling under the new law (treated group), i.e., those who got protection after June 1, 2015, will have fewer reunified family members than those falling under the old law (control group). If we could only capture regular family reunification and if the amendment was strictly enforced, we should see a flat line until month 36 after protection since subsidiary-protected refugees were banned from applying until then. In line with our expectation, Figure 2(a) shows that the control group (top line) has a higher level of family reunification shares than the treated group (bottom lines). The bottom three lines show the treated group (solid blue) and the treated group split by having received asylum within 90 days from June 1, 2015 (light blue dotted line)

2023. However, they told us no data on inland family reunification is available (§34 AsylG.).

or 90–255 days after June 1, 2015 (blue dashed line).

Figure 2: Evolution of proportion of subsidiary-protected and convention refugees with at least one reunified family member, 2014-2017, by treatment status



*Notes:* The graphs show the evolution of family reunification rates by protection status. The samples include all refugees who got protection  $\pm 255$  days from their relevant cutoff (June 1, 2015, for subsidiary-protected and June 1, 2016, for convention refugees). The control group received protection between day  $-255$  and  $0$ . The treated group from day  $0$  to  $255$  after the cutoff. Further, we split the treated group (for the subsidiary-protected refugees) and the control group (for the convention refugees) into those who got protection within 90 days of the cutoff (light blue dotted line), and after 90 days (for subsidiary-protected) or before 90 days (for convention refugees, blue dashed line). The first vertical red dashed line marks 12 months after protection, the second one 36 months.

By construction, the family reunification shares for subsidiary-protected refugees are zero until month 18 after protection. After that point, the shares for the control group quickly increase: Three years after protection, their family reunification rate is almost 14%. For the treated group, we still see some family reunification before month 36 after protection. Around 3% of treated subsidiary-protected refugees had a reunified family member, by our definition, before it was even possible to apply. But this value is significantly smaller than for the treated group (-10 pp. three years after protection). The gap is even larger if we consider only the subsidiary-protected who received their status from day 90–255 after the cutoff (dashed line). This large gap indicates that the change in the law had a sizeable impact on family reunification rates.

Figure 2(b) shows the descriptive evidence for the Geneva Convention refugees. For them, the amendment created a different incentive, i.e., to apply for family reunification within three months after protection. However, the families of those refugees who received protection before June 1, 2016, but did not apply until that date, could still apply until September 1, 2016. Hence, we would expect some treated individuals left of the cutoff, namely those who could have applied until June 1, 2016, but wanted to apply later and then needed to hurry up. The bottom grey solid line shows the averages for the control group. One year after protection, 10% of convention refugees in the control group had a reunified family member. For the treated group, this rate is 10 pp. larger (solid blue line at the top). Given the potential “late” applicants left of the cutoff, we split the control group in those refugees who were granted asylum up to 90 days before the cutoff (blue dashed line), and more than 90 days before that date (light blue dotted

line). The dashed line would represent the “late” applicants, for which we would expect a higher reunification rate than for the overall control group. Figure 2(b) shows that this is the case 12 months after protection and in all the years after as well.

## 4 Empirical Approach

To evaluate the causal effect of the amendment, we use a regression discontinuity design (RDD) in time—similar to, e.g., [Dustmann et al. \(2024\)](#); [Foged et al. \(2024\)](#)—where the running variable is the number of days between the date of receiving protection and the relevant cutoff for each group. We compare the outcomes for individuals who got protection before the cutoff date (control group) with those who got protection after the cutoff and are thus subject to the new law (treated group). This is a one-sided fuzzy design as individuals who could, in principle, apply under the old law but were not quick enough would be subject to the new law. Given that the new law affects all applicants for family reunification after June 1, 2016, we define assignment to treatment as follows:

1. For convention refugees: Refugees who received asylum from June 1, 2016, onwards are treated. Before the reform, their families could apply for family reunification immediately after the sponsor received protection. So, the relevant cutoff is the date when the law came into force.
2. For the subsidiary-protected: Individuals granted subsidiary protection from June 1, 2015, onwards are treated. Under the old law, the waiting period was one year after receiving protection. Therefore, the family of someone granted subsidiary protection on May 31, 2015, could have applied for family reunification one year later (before June 1, 2016). But someone who received subsidiary protection on June 1, 2015, would automatically be subject to the new law.

We estimate the effect of the amendment on outcome  $y$  of individual  $i$  for month  $t$  after protection using the following local linear regression model:

$$y_{it} = \alpha + \beta D_i + \gamma(z_i - c) + \delta D_i * (z_i - c) + X_i' \tau + \epsilon_{it}, \quad (1)$$

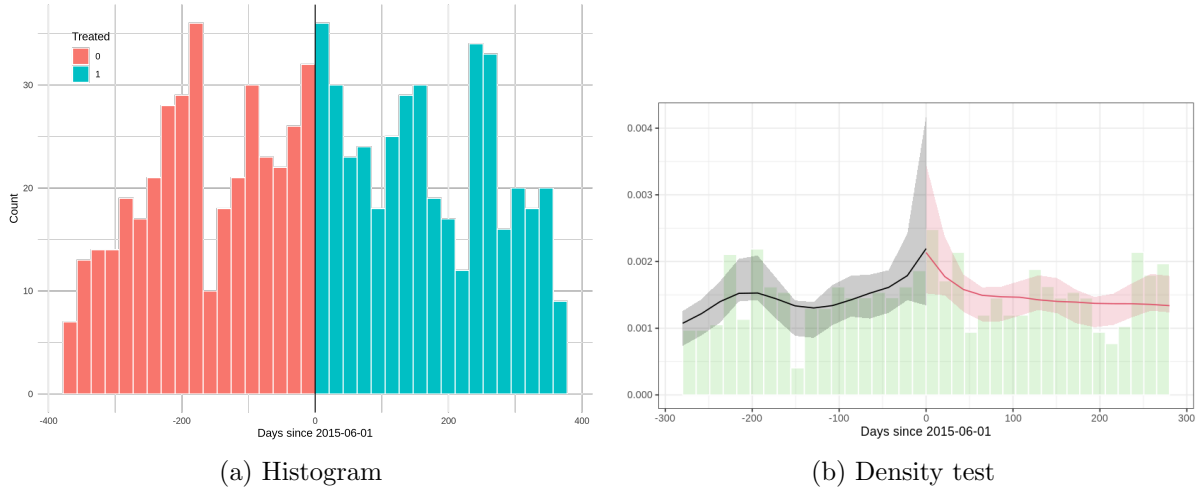
where  $D_i$  is a dummy equal one if the individual got protection after the cutoff,  $z_i$  is the running variable (date of protection) and  $c$  is the cutoff. Hence,  $(z_i - c)$  represents the centered running variable around zero.  $\beta$  is our parameter of interest. Following [Lee and Lemieux \(2010\)](#) to increase precision, we also include pre-treatment covariates for nationality, age at arrival, an indicator for being married, an indicator for having primary education or less, and days since immigration in  $X_i'$  as controls. We rely on non-parametric local linear regressions, using a uniform kernel and a bandwidth of 255 days around the cutoff. For the implementation, we use the `rdrobust` command in R ([Calonico et al., 2014, 2017](#)) and heteroskedasticity-robust standard errors.<sup>14</sup> The outcomes we look at are the probability of having a reunified family member—both

<sup>14</sup>While clustering the standard errors by the running variable was standard in the literature ([Lee and Lemieux, 2010](#)), [Kolesár and Rothe \(2018\)](#) shows that this approach can have substantially “worse coverage properties” than heteroskedastic-robust standard errors ([Cunningham, 2021](#)).

regular and irregular, a dummy for being employed (subject to social security contributions), monthly labor income, the number of jobs conditional on employment, a dummy for receiving welfare benefits, and a dummy for having a monthly income above 3,000 euros.

**Identifying assumption.** The primary assumption for our RDD strategy is that the potential outcomes (both treated and untreated) are continuous at the cutoff point. Since this assumption is untestable, we provide two tests supporting its plausibility.

Figure 3: Manipulation test - subsidiary-protected



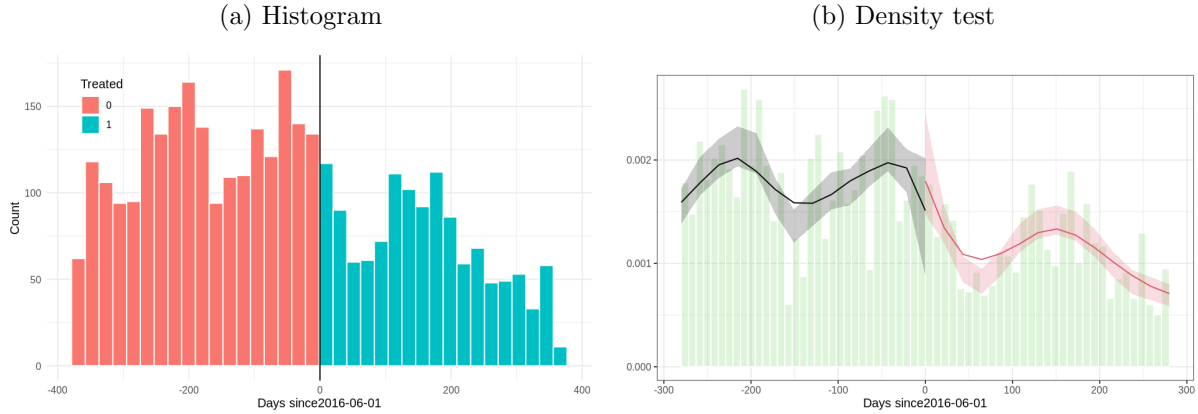
*Notes:* Panel (a) shows a histogram of the number of refugees granted subsidiary protection, grouped in 21-day bins, from one year before to one year after protection. Panel (b) displays the estimated density of this distribution. We use the `rddensity` command (Cattaneo et al., 2018) to conduct a density test and plot the estimated density. The test's p-value is 0.766, indicating that we cannot reject the null hypothesis of no manipulation.

First, we look at any signs of manipulation of the running variable, plotting a histogram and performing a density test (Cattaneo et al., 2018). Asylum-seekers have no influence on the processing time of their application. If caseworkers had intentionally wanted to delay decisions for asylum-seekers to fall under the new law, we would see a spike on the right side of the cutoff. Conversely, they could have sped up the decisions, and we would see a spike on the left side of the cutoff. These concerns do not apply to subsidiary-protected refugees since the relevant cutoff date was only decided in April 2016, i.e., almost a year after they received protection. Neither is the case. We do not see significant discontinuities around the cutoff for both protection statuses. Figures 3(a) and 4(a) depict the distribution of refugees by the running variable (days since the cutoff) from one year before until one year after the cutoff for subsidiary-protected and convention refugees respectively. Figures 3(b) and 4(b) plot the estimated densities for both distributions. We cannot reject the null of no manipulation from the density test in both cases.

Interestingly, we see a reduction in the counts for convention refugees after the cutoff (see Figure 4(a)). We believe this reduction could already reflect the fewer arrivals of asylum-seekers after the EU-Turkey agreement came into force in March 2016. The number of asylum-seekers in Greece who arrived through the Mediterranean Sea drastically reduced from 151,000 during January-March 2016 to only 5,200 during May-July (Knaus, 2024). The reduction in the number of protections granted happens gradually over several months. Still, in an RDD analysis that

also uses observations with some distance from the cut-off, this might create imbalances in the composition of the refugees, particularly by country of origin. An issue we investigate in more detail.

Figure 4: Manipulation test - convention refugees



*Notes:* Panel (a) shows a histogram of the number of refugees granted Geneva Convention protection, grouped in 21-day bins, from one year before to one year after protection. Panel (b) displays the estimated density of this distribution. We use the *rddensity* command (Cattaneo et al., 2018) to conduct a density test and plot the estimated density. The p-value of the test is 0.169, so we cannot reject the null hypothesis of no manipulation.

We use observations within 255 days from the cutoff in our estimations for two reasons. First, since the law was presented at the parliament in late January 2016, the subsidiary-protected refugees who got asylum by that date knew they would fall under the new law and could already have started to change their plans and bring their families sooner than expected via irregular ways. Second, in early March 2016, the European Union signed an agreement with Turkey to stop the irregular arrival of migrants. This decision could have also changed the plans of refugees already in Austria, especially the subsidiary-protected ones.

Our second assumption check is a discontinuity test for any difference in covariates. Our approach assumes no systematic differences between refugees who got protection right before and right after the cutoff. However, due to the reduction in arrivals due to the EU-Turkey agreement and the known seasonality in the arrival groups by country of origin, we do see a change in the composition of nationalities (see Figure A.4 in the Appendix). Thus, we perform all our analyses conditional on nationality. We also control for nationality when performing the covariate-discontinuity tests. We use the following pre-treatment covariates to perform this test: Age at immigration, primary schooling, marital status, and days between arrival in Austria and asylum granted.

Tables 2 and 3 show the mean value of pre-treatment characteristics of the main sample and the results of a test of covariates continuity around the cutoff using local linear regressions—as in equation 1 but controlling only for nationality dummies. Table 2 shows that on average, our subsidiary-protected sample was about 30.6 years old at arrival, 84.7% of them were married, about 50% had completed primary school or less, and spent on average 494 days since arriving in Austria until they received protection. There are no economically or statistically significant differences at the cut-off.<sup>15</sup>

<sup>15</sup>Figure A.7 in the Appendix shows RD plots for these covariates.



Table 2: Test of covariates discontinuities - subsidiary-protected

Covariate	Mean	Point Estimate	z-Statistic	P-value	95% CI
Age at arrival	30.63	-0.62	-0.56	0.58	[-2.79 ; 1.55]
Married share	84.74	1.42	0.32	0.75	[-7.37 ; 10.21]
Primary school share	50.07	1.60	0.20	0.84	[-13.99 ; 17.19]
Days since immigration	493.96	39.93	0.73	0.47	[-67.79 ; 147.65]

*Notes:* This table presents the results of a test of covariate continuity around the cutoff using a local linear regression and controlling for nationality. For each covariate, its mean value, the point estimate of the discontinuity at the cutoff, as well as the corresponding z-statistic, p-value, and lower and upper bound of the 95%-confidence interval, using robust standard errors, are displayed.

Table 3 shows that on average, convention refugees were about 33 years old at arrival, almost 84% of them were married, about 47% had completed primary school or less, and spent on average 352 days since arriving in Austria until they received protection. The differences at the cutoff are not statistically significantly different from zero at the 5% level, except for “days since immigration”. While this jump might raise concerns about the continuity assumption, the magnitude of the point estimate is relatively small compared to the mean. One reason for this significant difference in the days between immigration and asylum granted might be the lag in processing the increased number of applications from 2015.<sup>16</sup>

In addition to controlling for these variables, we show in the robustness section 5.3 that our main results are not sensitive to excluding these covariates.

Table 3: Test of covariates discontinuities - convention refugees

Covariate	Mean	Point Estimate	z-Statistic	P-value	95% CI
Age at arrival	32.69	1.01	1.66	0.10	[-0.183 ; 2.21]
Married share	84.02	2.45	0.88	0.38	[-3.030 ; 7.92]
Primary school share	47.28	-4.27	-1.09	0.28	[-11.962 ; 3.43]
Days since immigration	352.11	35.30	2.46	0.01	[ 7.172 ; 63.43]

*Notes:* This table presents the results of a test of covariate continuity around the cutoff using a local linear regression and controlling for nationality. For each covariate, its mean value, the point estimate of the discontinuity at the cutoff, and the corresponding z-statistic, p-value, and lower and upper bound of the 95%-confidence interval, using robust standard errors, are displayed.

**Fuzzy design.** The amendment to the family reunification rules creates a fuzzy RD design. Refugees whose families could have applied by June 1, 2016 under the old law but did not, would automatically fall under the new law. This means that there is only an increase in the probability of treatment assignment at the cutoff date. Since we do not know who applied for family reunification and when, we cannot estimate an IV. Hence, the results presented in this paper are reduced form estimates (ITT).

Additionally, our sample includes individuals who do not have family members abroad and for whom the change in the law is irrelevant. Although we try to approximate the relevant sample by excluding individuals who declared themselves single upon arrival and families who arrived together, we cannot know if the amendment was relevant for all individuals in our sample. Hence, we interpret our estimates as a lower-bound.

<sup>16</sup>Figure A.8 in the Appendix shows RD plots for these covariates.

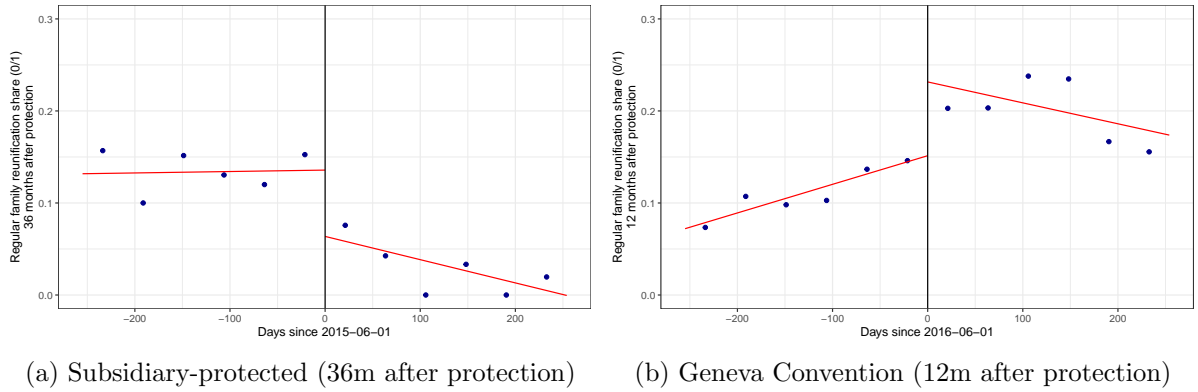
## 5 Results

This section presents our main results of estimating the effect of the amendment on the family reunification rates and the labor market integration of the sponsors.

### 5.1 Main results

**Regular family reunification rates.** To test the impact of the policy, we examine the effects on family reunification rates 36 months after protection for subsidiary-protected refugees and 12 months after protection for convention refugees.<sup>17</sup> Figure 5(a) shows the effect for subsidiary-protected. The results confirm the descriptive evidence shown in Figure 2. Subsidiary-protected refugees who received protection after June 1, 2015, were 7.1 percentage points less likely to have a reunified family member 36 months after receiving protection (see Figure 5(a) and column (2) of Table A.1 for year 3), although not statistically significant at conventional levels.

Figure 5: Regular family reunification rates, by protection status



*Notes:* The figures show regression discontinuity plots for regular family reunification shares of both subsidiary-protected and Geneva Convention refugees, using a uniform kernel and a bandwidth of 255 days. The red solid line shows a linear fit with different slopes at both sides of the cutoff. For subsidiary-protected refugees, the sample size includes 299 individuals on the left side of the cutoff and 303 on the right. For Geneva Convention refugees, the sample sizes are 1625 and 1042, respectively. Both models control for the covariates in Table 2 and nationality fixed-effects.

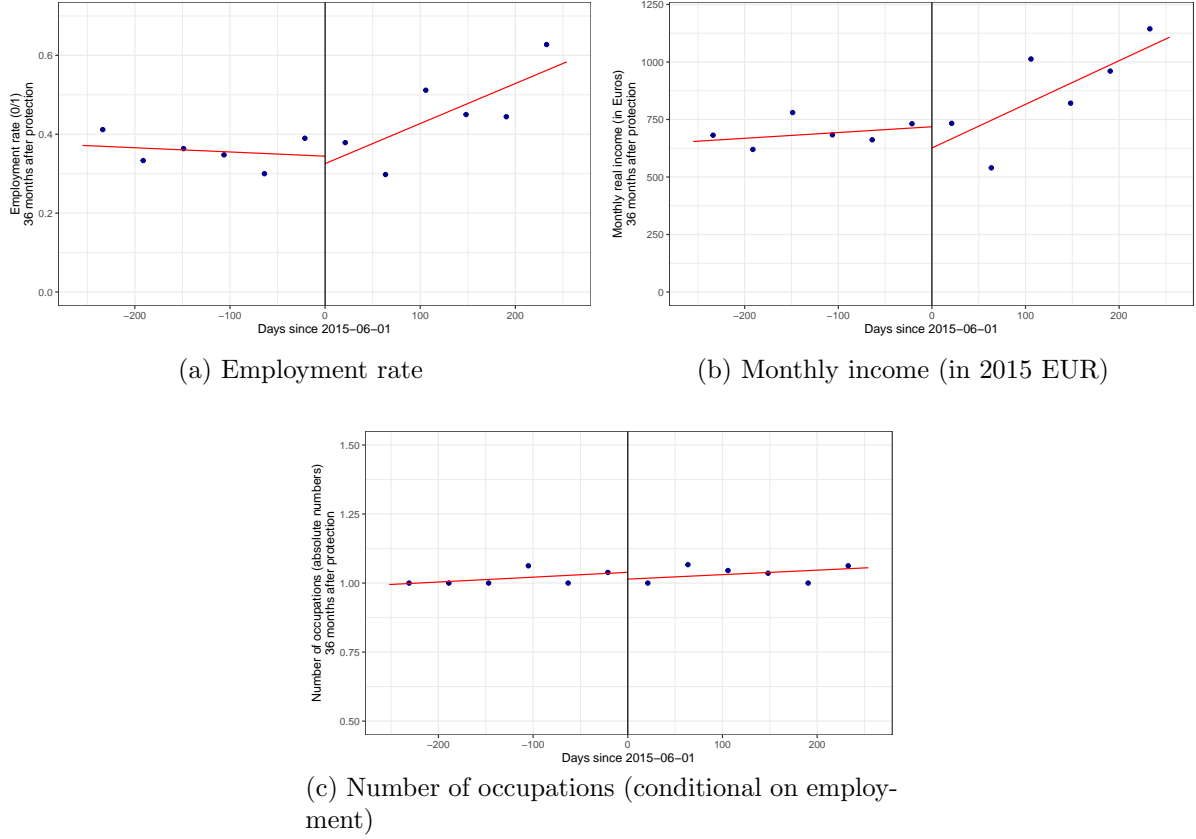
Considering the control mean of 13.4%, this reduction means a decrease of 53% in the family reunification rates after the amendment. This decrease is persistent and raises to approximately 13 pp. in years 4–5 after protection (see Table A.1).

For convention refugees, we expect family reunification rates to be higher for the treated since they have an incentive to apply as soon as possible as they can only apply for family reunification within three months after protection (otherwise, they need to fulfill stricter rules). Figure 5(b) shows this effect. Treated convention refugees were about 7.5 pp. more likely to have a reunified family member 12 months after protection. This gap increases to 8 pp. two years after protection and remains on average at 6 pp. for years 3–5 after protection (see Table A.7 in the Appendix). It is surprising to see a persistent effect and almost no catch-up from

<sup>17</sup>Results for different durations for both groups are shown in Tables A.1 and A.7 in the Appendix.

refugees below the cutoff. One explanation for this persistent effect could be the COVID-19 years. During these years (2020-2021)—which correspond to years four and five after protection for our sample—arrivals were relatively low compared to 2015–2016, and thus, a catch-up of the control group relatively unlikely.

Figure 6: Labor market outcomes for subsidiary-protected refugees 36 months after protection



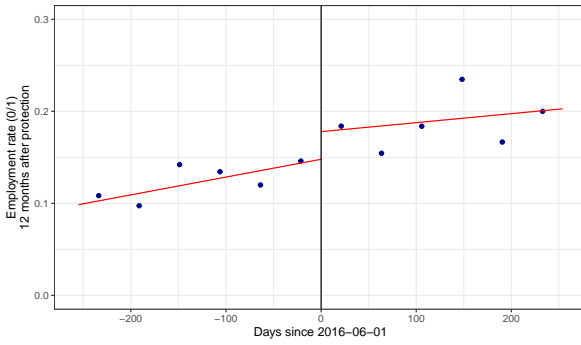
*Notes:* The figures show regression discontinuity plots for employment rates, monthly labor real income, and number of jobs of the subsidiary-protected refugees, using a uniform kernel and a bandwidth of 255 days. The red solid line shows a linear fit with different slopes at both sides of the cutoff. The sample size left of the cutoff is 299 individuals, and 303 right of the cutoff. All models control for the covariates in Table 2 and nationality fixed-effects.

**Employment and income.** Next, we analyze whether the amendment impacted the labor market outcomes of potential sponsors. If the stricter income requirements for subsidiary-protected refugees create an incentive for achieving higher earnings, we expect an increase in the employment probability and the amount of monthly earnings.

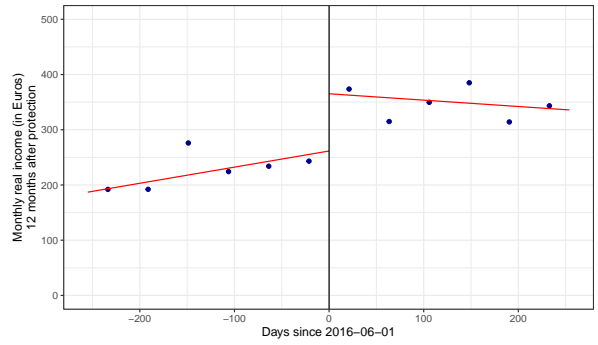
Figure 6 shows this was not the case for either outcome three years after protection. While we see a larger share of employed subsidiary-protected refugees who got asylum around 90 days after the reform, we do not find an increase right at the cutoff (Figure 6(a)). Figure 6(b) shows the result for monthly income, which mainly follows the pattern of the employment shares.

Recall that the incentive was to earn more to fulfill the income requirement. This higher income could be achieved by having several jobs that add to a particular monthly income or moving to better-paying jobs. We test the former in Figure 6(c) by looking at the number of jobs

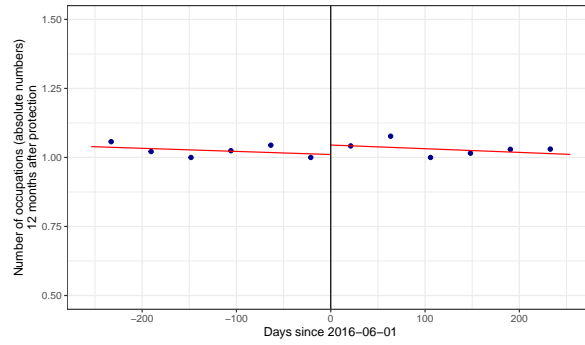
Figure 7: Labor market outcomes for convention refugees 12 months after protection



(a) Employment rate



(b) Monthly income (in 2015 EUR)



(c) Number of occupations (conditional on employment)

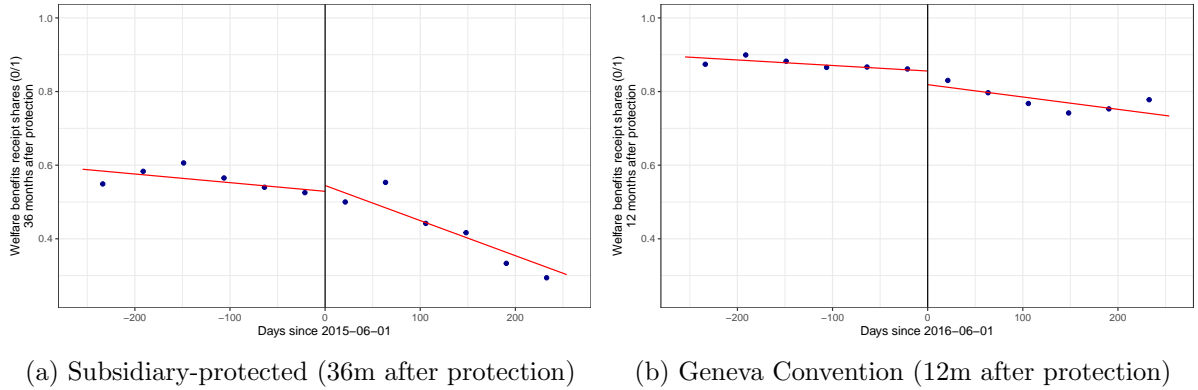
*Notes:* The figures show regression discontinuity plots for employment rates and monthly labor real income of convention refugees, using a bandwidth of 255 days. The red solid line shows a linear fit with different slopes at both sides of the cutoff. The sample size left of the cutoff is 1625 individuals, and 1042 right of the cutoff. Both models control for the covariates in Table 3 and nationality fixed-effects.

per worker, conditional on having any employment. The results do not show a discontinuous jump at the cutoff, so we rule out an adjustment by taking up more jobs. Since we also do not find an effect on monthly income, refugees did not move either to better-paying jobs. Given their likely lack of language and technical skills in the early years, we hypothesize that it was almost impossible to move to better-paying jobs. We will discuss this income restriction further in Subsection 5.2.

The amendment affected convention refugees in the opposite direction, i.e., they brought their families quicker to Austria. Additionally, they are not subject to the income requirement if their families apply within the first three months of protection. However, given that they now need to be able to support not only themselves but also their families, we also expect an increase in the employment probability and higher income one year after protection. Figure 7(a) shows a 2.8 pp. increase in the likelihood of being employed at the cutoff; however, this effect is not statistically significant. We find an increase of about 97 euros for monthly income, statistically significant at the 10% level (Figure 7(b)). These employment and income effects become larger and statistically significant at the 5% level for 3–5 years after protection. On average, treated convention refugees are 9 pp. more likely to be employed 3–5 years after protection and earn 200 euros more (see Tables A.8 and A.9 in the Appendix). Finally, we also look at the number of jobs—conditionally on having any employment—and find no effects (Figure 7(c)).

**Benefits receipt.** As refugees take up employment, they might stop receiving welfare benefits as they stop being eligible for them. At the same time, eligibility rules change for families relative to singles, which might also increase benefit receipt.

Figure 8: Welfare benefits receipt shares, by protection status



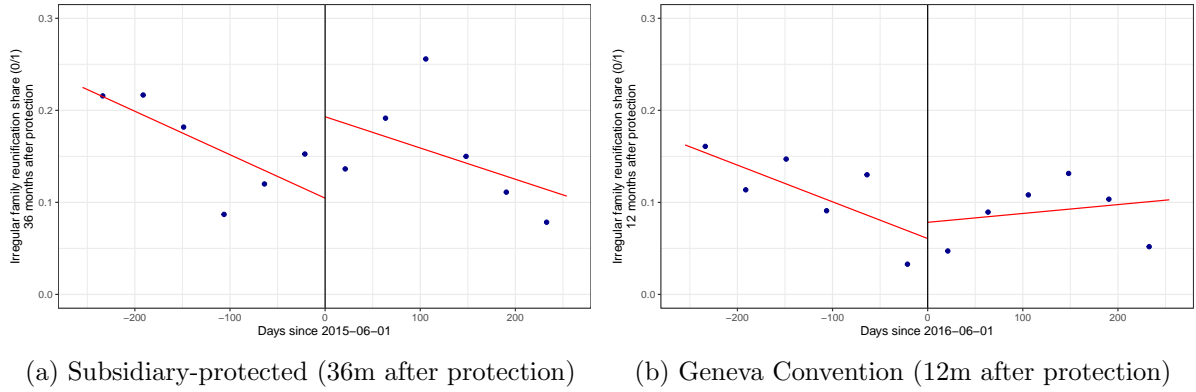
*Notes:* The figures show regression discontinuity plots for welfare benefits receipt shares for both subsidiary-protected and Geneva Convention refugees, using a uniform kernel and a bandwidth of 255 days. The red solid line shows a linear fit with different slopes at both sides of the cutoff. For subsidiary-protected refugees, the sample size includes 299 individuals on the left side of the cutoff and 303 on the right. For Geneva Convention refugees, the sample sizes are 1625 and 1042, respectively. Both models control for the covariates in Table 3 and nationality fixed-effects.

Figure 8 shows the effect on the likelihood to receive any benefits. For subsidiary-protected refugees, we see a change in slope but no jump at the cutoff—mirroring the effects on employment. Table A.5 shows the corresponding regression results also for other years, which are also insignificant.

For convention refugees, we observe a reduction by 3.6 pp. after twelve months (not statistically significant). In the second year, the effect is positive and close to zero thereafter (Table A.11). This pattern is consistent with the idea that refugees require more benefits shortly after their family arrives but this effect dissipates as their labor market integration improves.

**Irregular family reunification.** An alternative to regular family reunification is that family members come to Austria irregularly and then apply for asylum. Family members of subsidiary-protected refugees, who cannot apply in the first 36 months and who have slim chances afterward due to the hard-to-reach income requirements, might be particularly drawn towards this option. To investigate this potential substitution behavior, we study whether refugees are united with a family member where the timing of protection and arrival rules out regular family reunification. Again, this measure is imperfect, and the binned scatter plots look noisy. Thus, we take these results with a pinch of salt.

Figure 9: Irregular family reunification rates



*Notes:* The figures show regression discontinuity plots for irregular family reunification shares of both subsidiary-protected and Geneva Convention refugees, using a uniform kernel and a bandwidth of 255 days. The red solid line shows a linear fit with different slopes at both sides of the cutoff. For subsidiary-protected refugees, the sample size includes 299 individuals on the left side of the cutoff and 303 on the right. For Geneva Convention refugees, the sample sizes are 1587 and 1020, respectively. Both models control for the covariates in Table 3 and nationality fixed-effects.

Figure 9(a) shows the shares of irregular family reunification for subsidiary-protected refugees. This rate increased by about 6.5 pp., or by 40% of the control mean at the cutoff, although not statistically significant at conventional levels. This increase in irregular family reunification nearly offsets the reduction in regular family reunification rates. Table A.6 in the Appendix shows the effect up to five years after protection. Although this increase in irregular family reunification seems persistent, it rises only to 7.5 pp. in years 4–5 after protection. Although only suggestive, given its imprecision, this result hints at a partial substitution between regular and irregular family reunification.

While we do not expect such a substitution effect for convention refugees, we cannot ignore that an irregular way is still an option for some of them. Figure 9(b) shows a small positive but not statistically significant increase in the probability of irregular family reunification of 1.5 pp (see Table A.12 in the Appendix). These coefficients become somewhat larger in later years but remain statistically insignificant.

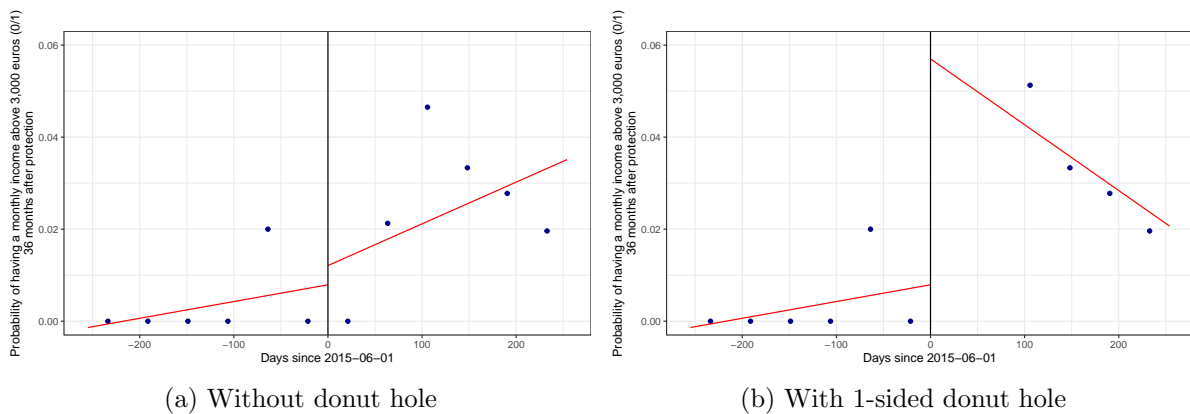


## 5.2 Mechanisms

While we see an effect of the amendment on the probability of having a reunified family member in Austria, we find no statistically significant effects on employment outcomes for the subsidiary-protected refugees. In this section, we want to dig deeper into the possible reasons why employment did not increase, despite the stricter income requirements to apply.

**Subsidiary-protected.** The reform imposed stricter income requirements to apply for family reunification. Reports from the Austrian Red Cross (ÖRK) estimate that a refugee wishing to bring a spouse and three children to Austria needed an average monthly income of 3,000 euros (or 36,000 euros annually), which is in line with the ASVG (General Social Insurance Act) rates for self-sufficiency. Data from Statistik Austria shows that the gross yearly median income for blue-collar workers, where most refugees are employed, was around 25,000 euros.<sup>18</sup>

Figure 10: Monthly income over 3,000 euro for subsidiary-protected refugees 36 months after protection



*Notes:* The figures show regression discontinuity plots for a dummy equal to one if the gross monthly real income (in 2015 euros) exceeds 3,000 euros, using a bandwidth of 255 days. The red solid line shows a linear fit with different slopes at both sides of the cutoff. The sample size left of the cutoff is 299 individuals, and 303 right of the cutoff. Both models control for the covariates in Table 2 and nationality fixed-effects.

Although the data does not differentiate between refugees and native workers, it is likely that for a subsidiary-protected refugee, meeting these new income requirements was nearly impossible. In our sample, only about 3% of subsidiary-protected and 4% of convention refugees achieved a monthly income of 3,000 euros five years after receiving protection (see Figure A.6 in the Appendix). Additionally, Figure A.5 shows that five years after protection both, convention and subsidiary-protected refugees earned on average 1,000 euros per month. These graphs confirm that it is nearly impossible for refugees to reach a monthly income of 3,000 euros even after five years of obtaining protection. The income requirements represent the most significant hurdle for subsidiary-protected refugees to apply for family reunification.

To test whether there was a change in the share of subsidiary-protected refugees who achieved a monthly income of 3,000 euros, we code a dummy equal to one if a subsidiary-protected refugee earned at least 3,000 euros gross income three years (36 months) after protection.

<sup>18</sup><https://www.statistik.at/statistiken/bevoelkerung-und-soziales/einkommen-und-soziale-lage/jaehrliche-personeneinkommen>

Figure 10(a) shows a change in slope but no statically significant jump at the cutoff. Since the observations within 90 days of the cutoff might be problematic, we use a one-sided 90-day donut hole (Figure 10(b)) and find an increase of 4 pp. in the probability of earning more than 3,000 euros monthly (control mean = 0.3%), however, not statistically significant. In any case, the effect of the amendment on the likelihood of fulfilling the requirement remains extremely small, and the overall chance remains low. Thus, regular family reunification is hardly a viable possibility for subsidiary-protected refugees. The inability to meet the income requirement might be a reason why subsidiary-protected refugees substitute between regular and irregular family migration.

### 5.3 Robustness checks

We conduct a series of robustness tests to assess the sensitivity of the results. This section discusses the results of these exercises whose results are shown in Tables A.1–A.15, and Figures A.9–A.12 in the Appendix.

**Without covariates.** Our main specification includes covariates as the composition of immigrants by origin changes seasonally. To test the sensitivity of our results to the inclusion of these variables, we run Equation 1 without them. Column (1) in all Tables A.1–A.12 shows the results of every outcome without covariates. Comparing these estimates to the ones in column (2)—our preferred specification with covariates—we see only small changes in point estimates for some outcomes, with the overall effects remaining largely unchanged. For instance, in Table A.7 (regular family reunification for convention refugees) the point estimate in column (1) is about 1.5 pp. higher than in column (2) when controlling for covariates. In Table 3, the share of married individuals is 2.45 pp. lower in the control group. This could point towards a downward bias of  $Y_0$  and thus an upward bias of the effect in column (1). Overall, our results appear to be insensitive to the inclusion of covariates.

**Different bandwidths.** For our main analysis, we use a bandwidth of 255 days. As a robustness exercise, we estimate equation 1 for various bandwidths from 15 to 300 days. Figure A.9 shows the results for our main outcomes for the subsidiary-protected refugees. All estimates are very imprecise and point estimates volatile for bandwidths below 50 days. For all bandwidth larger than that, point estimates are very stable. Confidence intervals get smaller as the bandwidth increases. For power reasons, we decided on a bandwidth of 255 (red dot in the Figure) to have at least 602 observations in our subsidiary-protected sample.

Figure A.10 shows the results for the convention refugees. Estimates are larger in magnitude but imprecisely estimated for bandwidths below 100. For larger bandwidths they are rather stable. The different estimates for bandwidths below 100 might reflect the effect of the “grace period”.

**Using a triangular kernel.** In our preferred specification, we use a uniform kernel. As a robustness check, column (3) in all Tables A.1–A.12 shows the estimates using a triangular kernel. This kernel relies more on the observations closer to the cutoff—which in our case would be the somewhat problematic observations—, while the uniform kernel gives the same weight

to all observations. The results remain very similar for most outcomes. For the probability of regular family reunification for subsidiary-protected refugees, columns (2) and (3) of Table A.1 have almost identical point estimates for years 2–3 after protection, while for years 4–5, the point estimates when using the triangular kernel are 2.5 pp. smaller. When looking at the results for convention refugees (Table A.7), the estimates for the probability of having a reunified family member are, on average, 1.5 pp. smaller when using the triangular kernel. These small differences remain for most outcomes. If anything, we interpret the estimates with the triangular kernel as a lower bound.

**Donut-RDD.** As mentioned in Subsection 2.2, refugees who got protection within 90 days from the cutoff might have been treated differently due to the goodwill of the authorities (“grace period”). Columns (4)–(6) in all Tables A.1– A.12 show results for a one-sided (90 days after the cutoff) donut hole, a one-sided and recentered donut hole, and a two-sided one.<sup>19</sup> The “grace period” might lead to an attenuation bias for the subsidiary-protected. Suppose the subsidiary-protected had a 90-day “grace period” to apply for family reunification. In that case, we might expect an upward bias in our estimates (towards zero). Figure 5(a) hints that there was some non-compliance within 90 days right of the cutoff, as some subsidiary-protected still reunified with some family members when it was not allowed. We see an increase in the magnitude of the effects by 4.6 pp. when applying the donut hole: from -7.1 pp. in column (2) to -11.7 pp. in column (4) of Table A.1 in the probability of having a reunified family member three years after obtaining protection. Our one-sided recentered donut hole (column 5) shows smaller point estimates for the later years than the standard one-sided donut hole (column 4), and both of them deliver smaller estimates than the two-sided donut hole (column 6) for years 4–5 after protection. Given the small sample size and the possible period of grace, we interpret our main estimates as providing us with lower bounds of the effect—the true effect is likely larger in magnitude. Three years after protection, the probability of having a reunified family member was reduced by 7–11.7 pp. In comparison to the control mean (13.4%) this relates to a reduction of the probability to almost zero, showing that the policy had the intended effect on family reunification rates for the subsidiary-protected refugees.

For convention refugees, a “grace period” means that those granted asylum before June 1, 2016, but who did not apply by that date, could still apply until September 1, 2016 and face no income requirements. Hence, we would expect some “late applicants” to the left of the cutoff, who would bias  $Y_0$  upwards. Refugees granted asylum before June 1, 2016 were in a transitional phase, and had more time to apply until September 2016, than someone who was granted asylum in June. Therefore, observations within 90 days to the left of the cutoff could introduce a bias in our estimates. Additionally, those refugees who got protection right after the cutoff would have less time to prepare their documents than those granted asylum some months later who would anticipate that they only had three months. Columns (4)–(6) in Table A.7 show the results for the probability of regular family reunification for the different donut holes. With the one-

<sup>19</sup>When using a donut hole, we usually drop the observations within it but do nothing with the running variable. Recentering the running variable, in our case, subtracting 90 days from the running variable to the right of the cutoff might give us a more insightful estimate since we are not relying only on the projection to the cutoff but on a new value.

sided recentered donut hole (column 5), the point estimates are 3 pp. higher than in our main specification (column 2). The estimate using the one-sided donut hole is only 0.7 pp. larger, while those from the specification with the two-sided donut hole suggests an increase of 18.6 pp. in the likelihood of family reunification, i.e., 11 pp. more than in our baseline specification in column (2). One year after receiving protection, the probability of having a regular reunified family member is more than twice as large in the two-sided donut specification (18.6 pp.) than in our main one (7.5 pp.). Since the “grace period” is only a concern and was not officially stated by the authorities, we take the estimates of the different donut holes with a pinch of salt and choose column (2) in all tables as our preferred results.

**Excluding those affected by “Asyl auf Zeit”.** The amendment not only changed the family reunification requirements but also the initial residence permit length for convention refugees, who applied for asylum after November 15, 2015, as mentioned in Subsection 2.2. Since some of the convention refugees in our sample—namely, those who applied for asylum after November 15, 2015 and got protection from June 1, 2016 onwards—were additionally affected by this change, we exclude this group for a robustness check. We proxy the date of asylum application with date of arrival, and exclude all refugees who arrived after November 15, 2015. Figure A.11 in the Appendix shows the results for regular family reunification and employment. While the sample size reduces by 257 individuals, the results remain virtually unchanged. Without these doubly affected refugees, family reunification shares increased by 7.6 pp. twelve months after protection, significant at the 5% level; and, employment rates by 3.1 pp. although not statistically significant.

**Data-driven bandwidth selector.** In our main specification, we choose a bandwidth of 255 days to ensure a sufficient number of observations (increasing statistical power) and a stable treatment effect. In addition to showing how our treatment effect varies with different bandwidths (see Figures A.9 and A.10), we let the default of the *rdrobust* command choose the mean-squared error (MSE) optimal bandwidth through a data-driven procedure. Panel A of Tables A.13 and A.14 present these results. For subsidiary-protected refugees (Table A.13), the point estimates for regular and irregular family reunification are smaller than in our main results but go in the same direction. Our main results show null effects for employment and benefit receipt, while the results in panel A of Table A.13 show a large and negative employment effect and a large and positive effect for welfare benefit receipt. However, these are imprecisely estimated due to the smaller bandwidths and sample sizes (between 135 and 189 individuals). For convention refugees, the point estimates for all outcomes more than double. For instance, the results show an increase in family reunification rates of 14.7 pp. and in employment rates of 6.3 pp., compared to only 7.5 and 2.8 pp., respectively, in our main specification. However, due to the smaller selected bandwidth, the number of observations is drastically reduced—about five times smaller than in our main specification. In addition, this reduces the observations to the potentially problematic sample within the grace period.

**OLS with same trends.** Given our small sample, we compare our baseline non-parametric specification with a simpler model. We estimate a standard OLS with a joint time trend for

both control and treatment groups. Panel B of Tables A.14 and A.13 present these results. Our results are virtually identical to our preferred specification using local linear regressions with a uniform kernel.

**Inference with a discrete running variable: RDHonest.** Kolesár and Rothe (2018) argue that when the running variable is discrete—as in our case—with few values and gaps near the threshold, conventional heteroskedasticity-robust standard errors would be preferred over clustering by the running variable that became standard practice since Lee and Card (2008). However, Kolesár and Rothe (2018) mention that both methods could have poor coverage rates for confidence intervals (CIs). Hence, the authors propose two methods to produce “honest” CIs. We use the *RDHonest* command in R that relies on the assumption of bounding the second derivative of the CEF to estimate a maximum bias of the estimator. Panel C of Tables A.14 and A.13 present these results. Since this approach is quite conservative, the estimated CIs are very large and contain impossible values for our estimates. For instance, most of our outcomes only take 0-1 values, and the bounds of the honest CIs are above 2. Although our running variable is discrete, it has many values close to each other and also values close to the cutoff (as shown in the histograms above), so we argue that our support is rich enough and that the continuity assumption is still plausible allowing for an estimation as in an RD setting with a continuous running variable. We have also shown that our estimates remain stable for smaller bandwidths.

**Local randomization approach.** This approach considers the RD around the cutoff as a randomized experiment. Within a narrow window around the cutoff, individuals are assumed to be randomly assigned to either side of it. In the first step, it selects a window in which the covariate balance holds. In the second step, it performs a difference in means within this window (Cattaneo et al., 2016). Similar to the previous point, not only inference but also estimation could be problematic with a discrete running variable. The standard continuity-based approach would require extrapolation outside the support of the score if the number of unique values is not large enough (Cattaneo and Titiunik, 2022). As previously mentioned, we argue that this is not a problem in our case, and a continuity-based approach can still be used. Since local randomization methods are more robust to discrete running variables, we conduct estimation and inference using the *rdlocrand* in R for finite samples. Panel D of Tables A.14 and A.13 present these results. The selected windows and sample sizes are almost as small as in Panel A, but they do not vary between outcomes. The point estimates for subsidiary-protected refugees are smaller than in our preferred specification but go in the same direction, except for receipt of welfare benefits. However, all point estimates are not significantly different from zero. For convention refugees, the point estimates for family reunification rates are also slightly smaller than in our preferred specification, while the ones for employment, income, and benefits receipt are larger. As in our main results, only the estimate for monthly income is statistically significant at the 5% level. Thus, we conclude that our results are robust to the choice of estimation method.

**Difference-in-differences.** As an alternative approach, we implement a difference-in-differences strategy, similar to Bratsberg and Raaum (2023). We use the main sample as for our RD estimations, i.e., we consider only male subsidiary-protected and convention refugees who got

protection 255 days before and after the respective cutoff date. Similar to [Bratsberg and Raaum \(2023\)](#), we use convention refugees as a control group for the subsidiary-protected refugees since they were only affected by the amendment after June 1, 2016. The reverse comparison—using subsidiary-protected as a control group for convention refugees—is not possible because of the way regular family reunification is defined in our data. The outcome variable is by construction equal to zero until month 18 after protection, and as our main result for the convention refugees, we look at family reunification rates 12 months after protection. Hence, we only present the estimates for the subsidiary-protected here. Table [A.15](#) shows the effects of a standard 2x2 DiD approach. It shows that in the post-treatment period, subsidiary-protected refugees were 6.8 pp. less likely to have a reunified family member than the control group. This result is almost the same as in our main RD specification in column (2) of Table [A.1](#). Additionally, we find no employment or income effects, which is in line with our main results. However, contrary to our main results, we find a 7 pp. reduction in the probability of receiving benefits in the post-treatment period. We find null effects in our main specification, but it is important to note that the control group differs. In our RD specifications, we compare subsidiary-protected refugees just above and below the cutoff, while the DiD results are relative to convention refugees who received protection during the same months as convention refugees. To test for pre-trends, Figure [A.12](#) shows results for the same outcomes (measured 36 months after protection) using a dynamic difference-in-differences specification. Time since reform is measured bimonthly. Hence, period 1 refers to the treatment effects for subsidiary-protected refugees who received asylum in June-July 2015. Overall, since the number of observations is relatively small per bin, the coefficients are imprecisely estimated and are only marginally significant in some periods. However, when pooling all post-treatment periods, we find statistically significant results at the 5% level. These DiD estimates suggest that our findings for subsidiary-protected refugees hold up under this alternative identification strategy.

## 6 Discussion and Conclusion

This paper studies refugees’ responses to an amendment to the family reunification law in Austria in 2016. While the main debate around this amendment was centered on the reduction of refugee arrivals, an increase in inland security, and better integration chances for refugees already in Austria, the amendment had different goals for subsidiary-protected and for Geneva Convention refugees. On the one hand, stricter requirements apply to the subsidiary-protected refugees, and the option to apply for family reunification was only possible three years after obtaining protection. On the other hand, convention refugees were also subject to stricter rules but indirectly encouraged to apply within three months of receiving protection. Our analysis shows that refugees responded accordingly to their respective restrictions and the incentives created by the amendment. Three years after protection, subsidiary-protected refugees were 7.1 pp. less likely to have regularly reunified family members in Austria (or a decrease of 53% compared to the control mean). One year after receiving protection, convention refugees were 7.5 pp more likely than the non-treated group to have a reunified family member in Austria (or 66% with respect to the control mean). This not only reflects a shift in timing, with applications happening earlier, but a persistent increase in family reunification. Moreover, we



find suggestive evidence that irregular arrivals of family members partially offset the decrease in regular family reunification for subsidiary-protected refugees. This result hints at a potential margin of adjustment that is relevant for policy design, by showing that family members did not stop arriving in Austria. This suggestive result implies a substitution from a regular and secure way to a riskier and dangerous one.

In a second step, we explore the labor market reaction of the affected refugees. While the aim for the subsidiary-protected was to incentivize labor market integration, we find no effects on employment, earnings, or welfare benefits receipt. Potentially because the required income thresholds were almost impossible to achieve. Convention refugees brought their families faster and to a greater extent. Those falling under the new policy were employed at a higher rate than the non-treated group. One year after protection, they were 2.8 pp. more likely to be employed. This effect increases over time reaching a 10 pp. increase four years after protection. Similarly, for real monthly income, they earned, on average, 97 euros more one year after protection and 235 euros more four years after. Hence, we find significant and persistent employment and earnings effects for the convention refugees.

Our study cannot answer the question of whether the policy changed the incentives of refugees, who consider bringing their families later, to come to Austria in the first place.

Overall, our findings underscore the importance of rigorous evaluation of immigration policies. Policy changes might have consequences not intended by policymakers. The shift to irregular migration appears to be the case for family members of subsidiary-protected refugees is such an unintended consequence. An increase in regular family reunification despite stricter rules for convention refugees might be another example.

## References

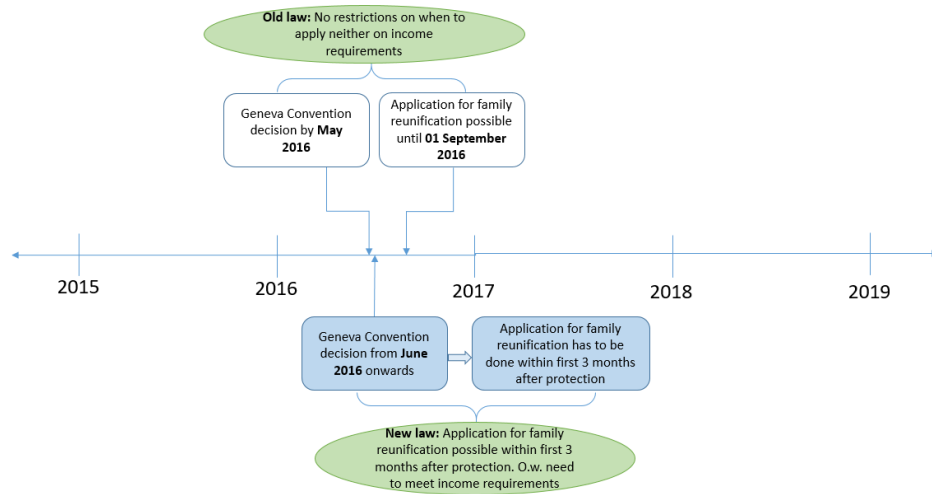
- ADDA, J., C. DUSTMANN, AND J.-S. GÖRLACH (2022): “The Dynamics of Return Migration, Human Capital Accumulation, and Wage Assimilation,” *The Review of Economic Studies*, 89, 2841–2871.
- AGERSNAP, O., A. JENSEN, AND H. KLEVEN (2020): “The Welfare Magnet Hypothesis: Evidence from an Immigrant Welfare Scheme in Denmark,” *American Economic Review: Insights*, 2, 527–542.
- AHRENS, A., A. BEERLI, D. HANGARTNER, S. KURER, AND M. SIEGENTHALER (2024): “The Labor Market Effects of Restricting Refugees’ Employment Opportunities.”
- ASYLKOORDINATION ÖSTERREICH (2016): “AsylKOORDINATEN - Infoblatt der asylkoordination österreich Nr. 4,” <https://archiv2022.asyl.at/de/info/infoblaetter/index.html>.
- BRATSBERG, B., AND O. RAAUM (2023): “Income requirements, family immigration, and refugee labor market integration,” Technical report, mimeo.
- BUNDESMINISTERIUM FÜR INNERES (2020): “Asyl-Jahresstatistik 2020,” [https://www.bmi.gv.at/301/Statistiken/files/Jahresstatistiken/Asyl-Jahresstatistik\\_2020.pdf](https://www.bmi.gv.at/301/Statistiken/files/Jahresstatistiken/Asyl-Jahresstatistik_2020.pdf).
- CALONICO, S., M. D. CATTANEO, M. H. FARRELL, AND R. TITIUNIK (2017): “rdrobust: Software for regression-discontinuity designs,” *The Stata Journal*, 17, 372–404.
- CALONICO, S., M. D. CATTANEO, AND R. TITIUNIK (2014): “Robust nonparametric confidence intervals for regression-discontinuity designs,” *Econometrica*, 82, 2295–2326.
- CATTANEO, M. D., M. JANSSON, AND X. MA (2018): “Manipulation testing based on density discontinuity,” *The Stata Journal*, 18, 234–261.
- CATTANEO, M. D., AND R. TITIUNIK (2022): “Regression discontinuity designs,” *Annual Review of Economics*, 14, 821–851.
- CATTANEO, M. D., R. TITIUNIK, AND G. VAZQUEZ-BARE (2016): “Inference in regression discontinuity designs under local randomization,” *The Stata Journal*, 16, 331–367.
- COUNCIL OF EUROPE (2017): “Realising the Right to Family Reunification of Refugees in Europe,” [https://migrant-integration.ec.europa.eu/library-document/realising-right-family-reunification-refugees-europe\\_en](https://migrant-integration.ec.europa.eu/library-document/realising-right-family-reunification-refugees-europe_en), Accessed: 2022-06-29.
- COUNCIL OF THE EUROPEAN UNION (2003): “Council Directive 2003/86/EC of 22 September 2003 on the right to family reunification,” <https://eur-lex.europa.eu/legal-content/DE/TXT/?uri=CELEX:32003L0086>, Accessed: 2024-09-14.
- CUNNINGHAM, S. (2021): *Causal inference: The mixtape*: Yale university press.
- CZECH, P. (2016): “Die Neuerungen des Asylrechtspakets 2016 - Einschränkungen des Familiennachzugs,” *FABL*, 1, 15, <https://rdb.manz.at/document/rdb.tso.LIfabl20160103>, Accessed: 2024-09-12.

- DUSTMANN, C., R. LANDERSØ, AND L. H. ANDERSEN (2024): “Refugee benefit cuts,” *American Economic Journal: Economic Policy*, 16, 406–441.
- EURONEWS (2024): “Half of Europeans disapprove of EU migration policy and demand stronger border controls,” <https://www.euronews.com/my-europe/2024/03/26/half-of-europeans-disapprove-of-eu-migration-policy-and-demand-stronger-border-controls-po>, Accessed: 2024-06-01.
- EUROPEAN COMMISSION (2014): “Communication from the Commission to the European Parliament and the Council on guidelines for the application of Directive 2004/38/EC on the right of citizens of the Union and their family members to move and reside freely within the territory of the Member States,” <https://eur-lex.europa.eu/legal-content/EN/TXT/?uri=CELEX:52014DC0210&qid=1726319388681>, Accessed: 2024-09-14.
- EUROSTAT (2023): “Asylum applicants by type of applicant, citizenship, age and sex - annual aggregated data,” [https://ec.europa.eu/eurostat/databrowser/product/page/MIGR\\_ASYAPPCTZA\\_custom\\_7568272](https://ec.europa.eu/eurostat/databrowser/product/page/MIGR_ASYAPPCTZA_custom_7568272), Accessed: 2023-09-21.
- FASANI, F., T. FRATTINI, AND L. MINALE (2021): “Lift the Ban? Initial Employment Restrictions and Refugee Labour Market Outcomes,” *Journal of the European Economic Association*, 19, 2803–2854, [10.1093/jeea/jvab021](https://doi.org/10.1093/jeea/jvab021).
- (2022): “(The Struggle for) Refugee integration into the labour market: evidence from Europe,” *Journal of Economic Geography*, 22, 351–393, [10.1093/jeg/lbab011](https://doi.org/10.1093/jeg/lbab011).
- FOGED, M., L. HASAGER, G. PERI, J. N. ARENDT, AND I. BOLVIG (2024): “Language training and refugees’ integration,” *Review of Economics and Statistics*, 106, 1157–1166.
- KNAUS, G. (2024): “Stellungnahme zu Migrationsabkommen mit Sicheren Drittstaaten,” <https://www.esiweb.org/rumeliobserver/wp-content/uploads/2024/06/Knaus-BMI-Sichere-Drittstaaten-17-Juni-2024.pdf>, Accessed: 2024-06-28.
- KOLESÁR, M., AND C. ROTHE (2018): “Inference in regression discontinuity designs with a discrete running variable,” *American Economic Review*, 108, 2277–2304.
- LEE, D. S., AND D. CARD (2008): “Regression discontinuity inference with specification error,” *Journal of Econometrics*, 142, 655–674.
- LEE, D. S., AND T. LEMIEUX (2010): “Regression discontinuity designs in economics,” *Journal of economic literature*, 48, 281–355.
- LÖBEL, L.-M. (2020): “Family separation and refugee mental health—A network perspective,” *Social Networks*, 61, 20–33.
- LÖBEL, L.-M., AND J. JACOBSEN (2021): “Waiting for kin: a longitudinal study of family reunification and refugee mental health in Germany,” *Journal of Ethnic and Migration Studies*, 47, 2916–2937.

- OECD (2017): “International Migration Outlook 2017 - Chapter 3: Recent developments in international migration movements and policies,” <https://www.oecd.org/els/mig/IMO-2017-chap3.pdf>, Accessed: 2024-06-29.
- POESCHEL, F. (2020): “Out there on your own: Absence of the spouse and migrants’ integration outcomes,” *Robert Schuman Centre for Advanced Studies Research Paper*.
- REPUBLIC OF AUSTRIA (2016a): “Stenographisches Protokoll 111. Sitzung des Nationalrates der Republik Österreich XXV. Gesetzgebungsperiode Mittwoch, 27., und Donnerstag 28. Jänner 2016,” <https://www.parlament.gv.at/researchieren/protokolle/>, Accessed: 2024-06-07.
- (2016b): “Stenographisches Protokoll 123. Sitzung des Nationalrates der Republik Österreich XXV. Gesetzgebungsperiode Mittwoch, 27. April 2016,” <https://www.parlament.gv.at/researchieren/protokolle/>, Accessed: 2024-06-07.
- UNHCR ÖSTERREICH (2017): “Familienzusammenführung: Hürden und Herausforderungen,” <https://www.asyl.at/files/uploads/307/690-familienzusammenfuehrung-web.pdf>, Accessed: 2022-06-29.
- ZWEIMÜLLER, J., R. WINTER-EBMER, R. LALIVE, A. KUHN, J.-P. WUELLRICH, O. RUF, AND S. BÜCHI (2009): “Austrian Social Security Database,” *Available at SSRN 1399350*.

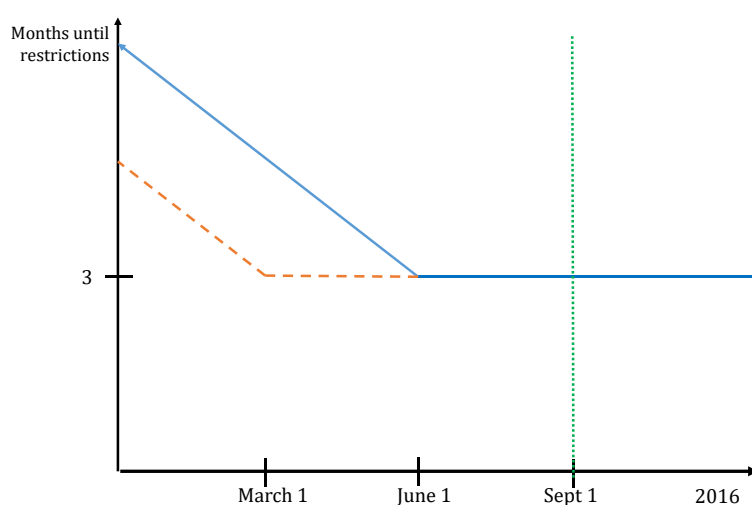
## A Additional Figures and Tables

Figure A.1: Timeline for Geneva Convention refugees



*Notes:* The figure shows the time required to apply for family reunification before and after the reform for Geneva Convention refugees. The top panels describe the rules in place before the amendment, while the bottom panels show what was valid for convention refugees who received protection from June 1, 2016, onwards.

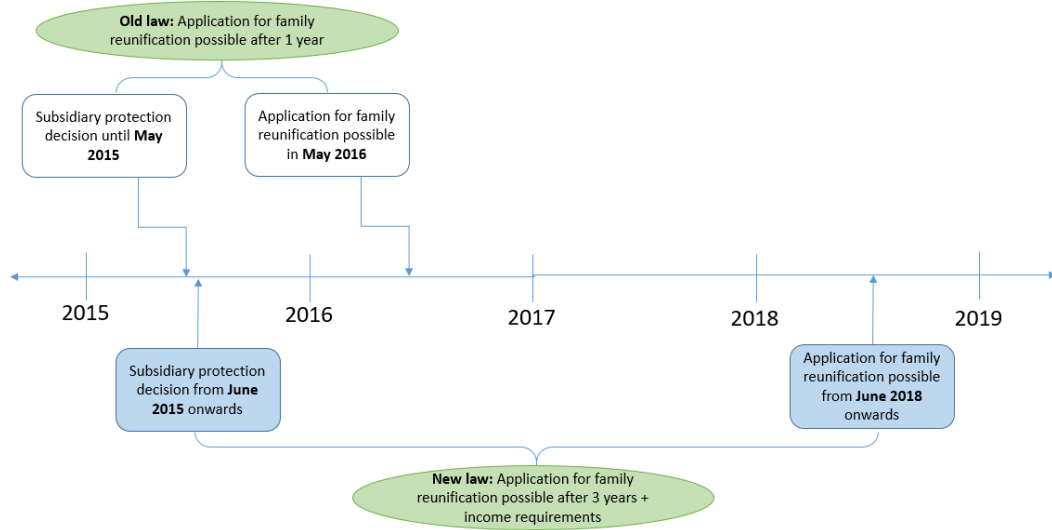
Figure A.2: Timeline for Geneva Convention refugees, including grace period and months until restrictions



*Notes:* The figure shows the time required to apply for family reunification after the reform for Geneva Convention refugees. The x-axis shows the months in 2016. The y-axis shows the number of months the family members could apply without being subject to the stricter income requirements. The blue solid line shows the deadline for those who got protection from June 1, 2016, onwards, and whose families could only apply after that date. They needed to apply within three months of protection. This solid blue line already includes the grace period, as those convention refugees who were granted protection before June 1, 2016, and thus, whose families could have applied immediately, would now still have time to apply until September 1, 2016, without being subject to the stricter income requirements. The dashed orange line shows the situation without the grace period. In this case, all refugees who were granted asylum before June 1, 2016, would have had to apply until then.

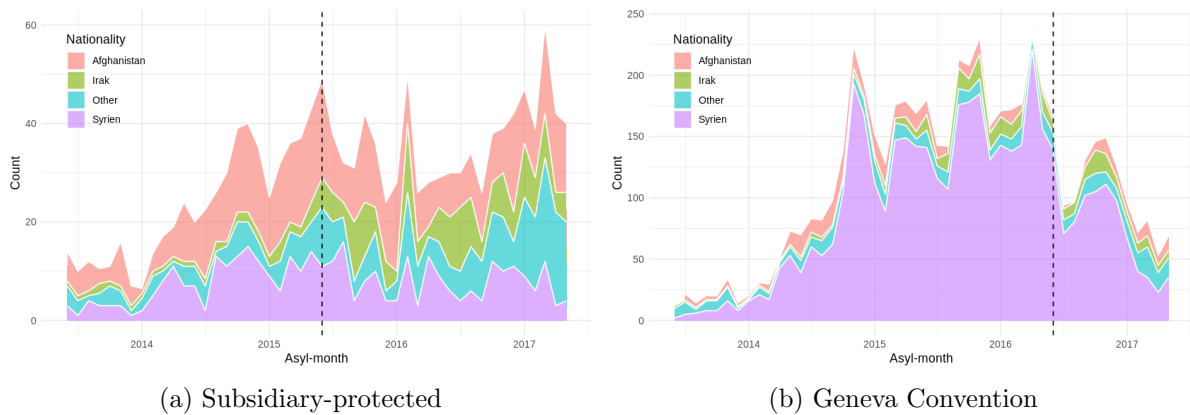


Figure A.3: Timeline for subsidiary-protected



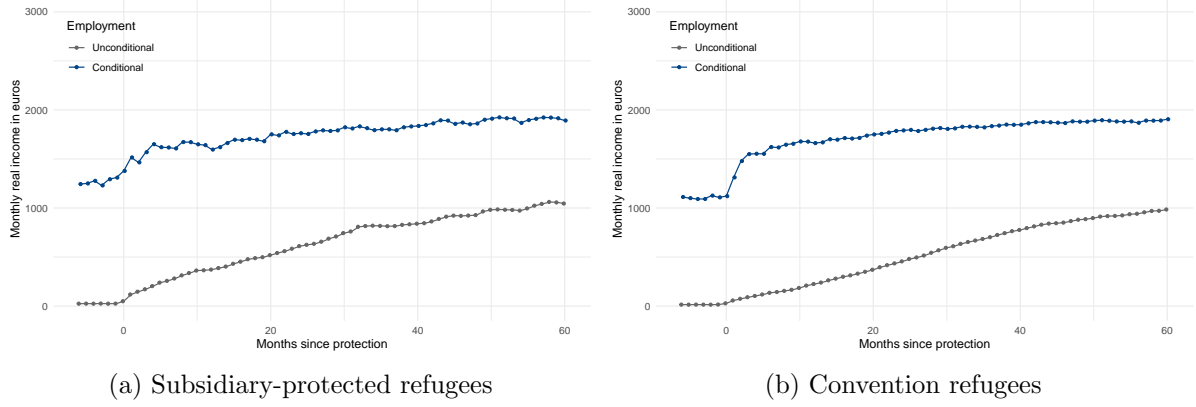
*Notes:* The figure shows the time required to apply for family reunification before and after the reform for subsidiary-protected refugees. The top panels describe the rules in place before the amendment, while the bottom panels show what was valid for subsidiary-protected refugees who received protection from June 1, 2015, onwards.

Figure A.4: Counts of Refugees by Nationality, Status, and Asylum month



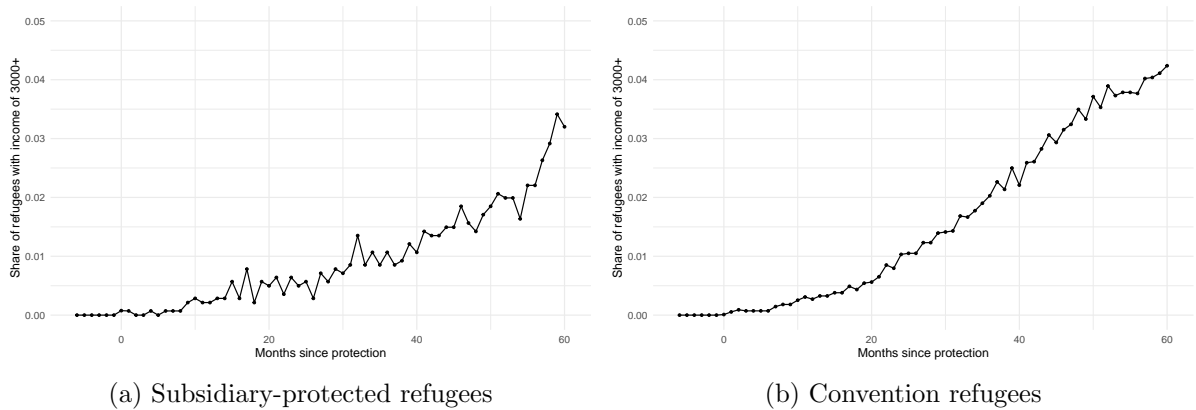
*Notes:* The figures show the monthly counts of refugees by status granted, nationality, and month of asylum. The vertical dashed lines reflect the respective cutoffs: June 1, 2015 and 2016. Additionally, the figures show monthly seasonality that could confound our results. Hence, we will always control for nationality fixed-effects in our estimations.

Figure A.5: Evolution of monthly real income for refugees



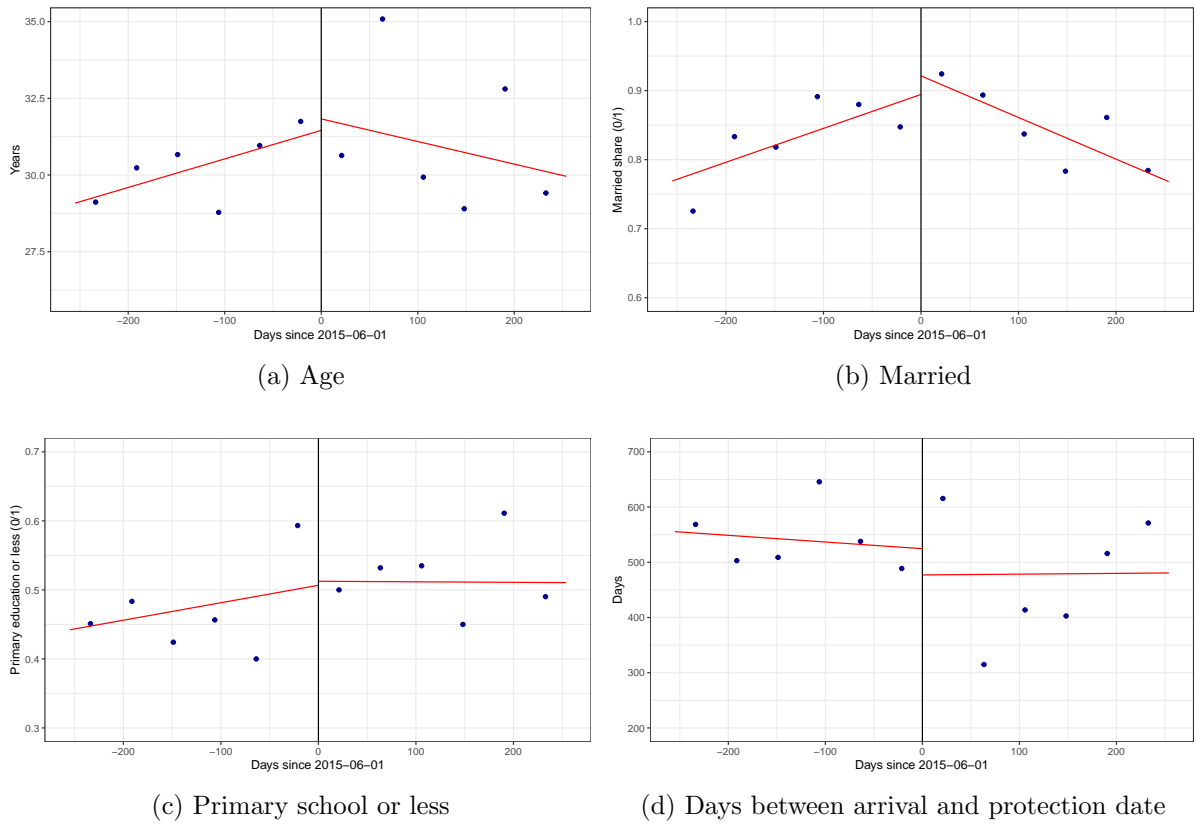
*Notes:* The graphs show the evolution of monthly real labor income for subsidiary-protected and convention refugees by months since protection. The graphs show both conditional and unconditional monthly labor income. We condition on having any employment subject to social security contributions as of the 1st of each month. The blue lines (upper ones) show the conditional means. For the months before zero, the sample size is very small: below 20 for subsidiary-protected refugees and below 50 for convention refugees. The sample contains only men who got asylum between 2013 and 2017, aged 18–50, and declared not to be single at arrival.

Figure A.6: Evolution of the share of refugees with an income above 3000 euros



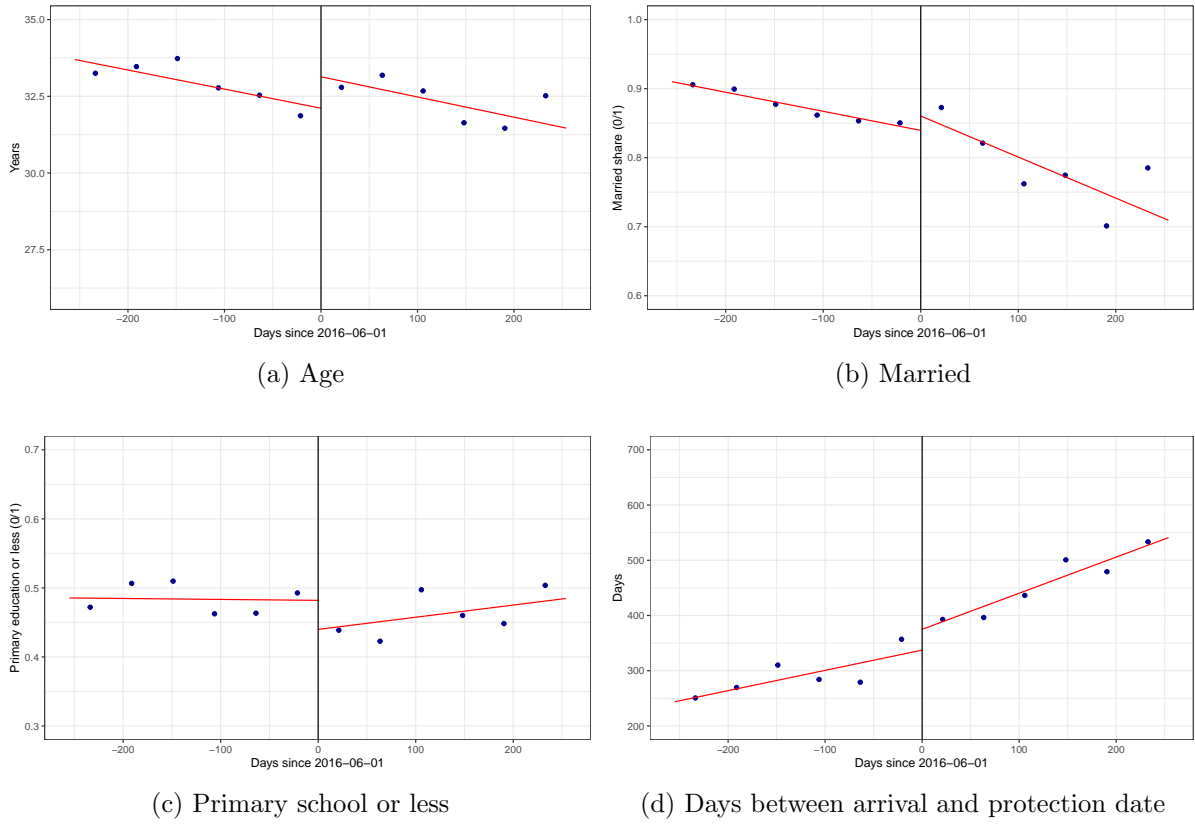
*Notes:* The graphs show the evolution of the share of refugees with a monthly real income above 3,000 euros, by refugee status and months since protection. The sample contains only men who got asylum between 2013 and 2017 and declared not to be single at arrival.

Figure A.7: Subsidiary-protected refugees: Covariate discontinuities



*Notes:* The figure shows the RD estimates for receiving protection after June 1, 2015 (i.e., falling under the new law) on the covariates, using local linear regressions with a uniform kernel and controlling for nationality fixed-effects.

Figure A.8: Geneva Convention refugees: Covariate discontinuities



*Notes:* The figure shows the RD estimates for receiving protection after June 1, 2016 (i.e., falling under the new law) on the covariates, using local linear regressions with a uniform kernel and controlling for nationality fixed-effects.

Table A.1: Robustness: Regular family reunification for subsidiary-protected refugees

	(1)	(2)	(3)	(4)	(5)	(6)
Years since protection	RD estimates					
1	-	-	-	-	-	-
[Mean = 0.000]	-	-	-	-	-	-
2	-0.018	-0.022	-0.031	-0.029	-0.032	0.012
[Mean = 0.030]	(0.027)	(0.027)	(0.031)	(0.024)	(0.023)	(0.048)
3	-0.072	-0.071	-0.070	-0.117	-0.111	-0.089
[Mean = 0.134]	(0.047)	(0.046)	(0.051)	(0.049)	(0.042)	(0.104)
4	-0.133	-0.136	-0.113	-0.188	-0.153	-0.272
[Mean = 0.197]	(0.056)	(0.055)	(0.059)	(0.082)	(0.057)	(0.137)
5	-0.125	-0.125	-0.097	-0.194	-0.151	-0.326
[Mean = 0.224]	(0.059)	(0.058)	(0.063)	(0.083)	(0.059)	(0.140)
N below/above	299 / 303	299 / 303	299 / 303	299 / 186	299 / 186	184 / 186
Kernel	Uni	Uni	Tri	Uni	Uni	Uni
Donut hole	No	No	No	1-sided	1-sided (rec)	2-sided
Bandwidth	255	255	255	255	255	255
Covariates	No	Yes	Yes	Yes	Yes	Yes

*Notes:* Regular family reunification is defined as a dummy for having at least one coinsured family member who arrived eighteen months or more after the sponsor got protection. The rows show the RD estimates for receiving protection after June 1, 2015 (i.e., falling under the new law) on the outcome for 1 to 5 years after protection, respectively, using local linear regressions. Our outcome variable, regular family reunification, is by construction equal to zero for year one. Every column shows a different specification: column (1) does not include controls, column (2) includes controls for being married, age at immigration, having completed primary schooling or less, and nationality dummies, column (3) uses a triangular instead of a uniform kernel, column (4) implements a one-sided donut hole of 90 days to the right of the cutoff, column (5) applies the same donut hole and recenters the running variable by subtracting 90 days, column (6) shows the results of a two-sided 90 days donut hole. Additionally, we show the control mean for every year. We calculate the estimates using the *rdrobust* command in R. Robust standard errors in parentheses.

Table A.2: Robustness: Employment for subsidiary-protected refugees

	(1)	(2)	(3)	(4)	(5)	(6)
Years since protection	RD estimates					
1	-0.022	-0.001	0.002	-0.103	-0.030	-0.011
[Mean = 0.204]	(0.058)	(0.052)	(0.056)	(0.101)	(0.059)	(0.138)
2	-0.004	0.015	0.012	-0.020	0.032	0.020
[Mean = 0.258]	(0.066)	(0.058)	(0.061)	(0.112)	(0.069)	(0.156)
3	-0.019	0.006	0.004	0.019	0.080	0.170
[Mean = 0.358]	(0.077)	(0.071)	(0.077)	(0.123)	(0.080)	(0.172)
4	-0.005	0.015	0.021	0.009	0.022	0.118
[Mean = 0.472]	(0.077)	(0.071)	(0.077)	(0.142)	(0.088)	(0.193)
5	-0.088	-0.066	-0.067	-0.112	-0.037	-0.228
[Mean = 0.512]	(0.076)	(0.072)	(0.078)	(0.135)	(0.086)	(0.184)
N below/above	299 / 303	299 / 303	299 / 303	299 / 186	299 / 186	184 / 186
Kernel	Uni	Uni	Tri	Uni	Uni	Uni
Donut hole	No	No	No	1-sided	1-sided (rec)	2-sided
Bandwidth	255	255	255	255	255	255
Covariates	No	Yes	Yes	Yes	Yes	Yes

*Notes:* Employment is defined as a dummy for having any employment spell subject to social insurance contributions. The rows show the RD estimates for receiving protection after June 1, 2015 (i.e., falling under the new law) on the outcome for 1 to 5 years after protection, respectively, using local linear regressions. Every column shows a different specification: column (1) does not include controls, column (2) includes controls for being married, age at immigration, having completed primary schooling or less, and nationality dummies, column (3) uses a triangular instead of a uniform kernel, column (4) implements a one-sided donut hole of 90 days to the right of the cutoff, column (5) applies the same donut hole and recenters the running variable by subtracting 90 days, column (6) shows the results of a two-sided 90 days donut hole. Additionally, we show the control mean for every year. We calculate the estimates using the *rdrobust* command in R. Robust standard errors in parentheses.

Table A.3: Robustness: Monthly real income for subsidiary-protected refugees

	(1)	(2)	(3)	(4)	(5)	(6)
Years since protection	RD estimates					
1	-87.171	-51.258	-4.676	-265.236	-107.552	-237.620
[Mean = 342]	(110.577)	(101.703)	(110.933)	(190.750)	(111.931)	(249.900)
2	-60.848	-11.144	37.643	-124.997	-6.719	-138.332
[Mean = 477]	(122.777)	(107.930)	(113.321)	(212.592)	(129.460)	(285.007)
3	-92.266	-50.568	-19.820	-63.167	67.617	47.782
[Mean = 687]	(146.768)	(134.130)	(146.432)	(256.578)	(162.726)	(347.292)
4	-110.740	-71.624	-49.565	-47.784	-15.481	-24.058
[Mean = 893]	(152.265)	(135.924)	(143.347)	(300.603)	(181.934)	(391.303)
5	-93.984	-53.663	-56.086	-71.589	9.843	-200.997
[Mean = 989]	(157.326)	(149.979)	(160.674)	(307.432)	(188.231)	(399.421)
N below/above	299 / 303	299 / 303	299 / 303	299 / 186	299 / 186	184 / 186
Kernel	Uni	Uni	Tri	Uni	Uni	Uni
Donut hole	No	No	No	1-sided	1-sided (rec)	2-sided
Bandwidth	255	255	255	255	255	255
Covariates	No	Yes	Yes	Yes	Yes	Yes

*Notes:* The outcome is gross monthly real income (in 2015 euros). The rows show the RD estimates for receiving protection after June 1, 2015 (i.e., falling under the new law) on the outcome for 1 to 5 years after protection, respectively, using local linear regressions. Every column shows a different specification: column (1) does not include controls, column (2) includes controls for being married, age at immigration, having completed primary schooling or less, and nationality dummies, column (3) uses a triangular instead of a uniform kernel, column (4) implements a one-sided donut hole of 90 days to the right of the cutoff, column (5) applies the same donut hole and recenters the running variable by subtracting 90 days, column (6) shows the results of a two-sided 90 days donut hole. Additionally, we show the control mean for every year. We calculate the estimates using the *rdrobust* command in R. Robust standard errors in parentheses.



Table A.4: Robustness: Monthly income above 3,000 euros for subsidiary-protected refugees

	(1)	(2)	(3)	(4)	(5)	(6)
Years since protection	RD estimates					
1	0.003	0.004	0.007	-0.005	-0.006	0.000
[Mean = 0.003]	(0.014)	(0.014)	(0.018)	(0.008)	(0.008)	(0.000)
2	0.008	0.008	0.007	0.036	0.018	0.039
[Mean = 0.007]	(0.013)	(0.013)	(0.012)	(0.037)	(0.022)	(0.037)
3	0.004	0.001	-0.006	0.040	0.029	0.049
[Mean = 0.003]	(0.013)	(0.014)	(0.012)	(0.047)	(0.027)	(0.046)
4	0.005	0.003	0.005	0.024	0.022	-0.008
[Mean = 0.003]	(0.013)	(0.013)	(0.010)	(0.051)	(0.027)	(0.064)
5	0.034	0.035	0.025	0.073	0.054	0.109
[Mean = 0.017]	(0.020)	(0.020)	(0.017)	(0.057)	(0.032)	(0.062)
N below/above	299 / 303	299 / 303	299 / 303	299 / 186	299 / 186	184 / 186
Kernel	Uni	Uni	Tri	Uni	Uni	Uni
Donut hole	No	No	No	1-sided	1-sided (rec)	2-sided
Bandwidth	255	255	255	255	255	255
Covariates	No	Yes	Yes	Yes	Yes	Yes

*Notes:* The outcome is a dummy equal to one if the gross monthly real income (in 2015 euros) exceed 3000 euros. The rows show the RD estimates for receiving protection after June 1, 2015 (i.e., falling under the new law) on the outcome for 1 to 5 years after protection, respectively, using local linear regressions. Every column shows a different specification: column (1) does not include controls, column (2) includes controls for being married, age at immigration, having completed primary schooling or less, and nationality dummies, column (3) uses a triangular instead of a uniform kernel, column (4) implements a one-sided donut hole of 90 days to the right of the cutoff, column (5) applies the same donut hole and recenters the running variable by subtracting 90 days, column (6) shows the results of a two-sided 90 days donut hole. Additionally, we show the control mean for every year. We calculate the estimates using the *rdrobust* command in R. Robust standard errors in parentheses.

Table A.5: Robustness: Welfare benefits receipt for subsidiary-protected refugees

	(1)	(2)	(3)	(4)	(5)	(6)
Years since protection	RD estimates					
1	0.060	0.040	0.024	0.156	0.084	0.201
[Mean = 0.803]	(0.061)	(0.057)	(0.062)	(0.111)	(0.068)	(0.151)
2	0.043	0.026	-0.004	0.088	0.036	0.082
[Mean = 0.719]	(0.069)	(0.062)	(0.069)	(0.127)	(0.077)	(0.162)
3	0.016	-0.009	0.001	0.050	-0.034	-0.135
[Mean = 0.559]	(0.077)	(0.069)	(0.076)	(0.131)	(0.083)	(0.176)
4	0.010	-0.002	-0.029	0.155	0.035	0.146
[Mean = 0.445]	(0.077)	(0.068)	(0.074)	(0.136)	(0.086)	(0.180)
5	0.029	0.020	0.041	-0.001	-0.029	-0.038
[Mean = 0.318]	(0.074)	(0.065)	(0.071)	(0.119)	(0.075)	(0.164)
N below/above	299 / 303	299 / 303	299 / 303	299 / 186	299 / 186	184 / 186
Kernel	Uni	Uni	Tri	Uni	Uni	Uni
Donut hole	No	No	No	1-sided	1-sided (rec)	2-sided
Bandwidth	255	255	255	255	255	255
Covariates	No	Yes	Yes	Yes	Yes	Yes

*Notes:* The outcome is a dummy equal to one if a refugee still receives welfare benefits. The rows show the RD estimates for receiving protection after June 1, 2015 (i.e., falling under the new law) on the outcome for 1 to 5 years after protection, respectively, using local linear regressions. Every column shows a different specification: column (1) does not include controls, column (2) includes controls for being married, age at immigration, having completed primary schooling or less, and nationality dummies, column (3) uses a triangular instead of a uniform kernel, column (4) implements a one-sided donut hole of 90 days to the right of the cutoff, column (5) applies the same donut hole and recenters the running variable by subtracting 90 days, column (6) shows the results of a two-sided 90 days donut hole. Additionally, we show the control mean for every year. We calculate the estimates using the *rdrobust* command in R. Robust standard errors in parentheses.

Table A.6: Robustness: Irregular family reunification for subsidiary-protected refugees

	(1)	(2)	(3)	(4)	(5)	(6)
Years since protection	RD estimates					
1	0.012	0.008	0.019	0.017	0.016	0.004
[Mean = 0.020]	(0.025)	(0.024)	(0.030)	(0.040)	(0.026)	(0.044)
2	0.077	0.062	0.051	0.135	0.093	0.153
[Mean = 0.120]	(0.044)	(0.041)	(0.044)	(0.077)	(0.049)	(0.103)
3	0.088	0.065	0.037	0.273	0.172	0.294
[Mean = 0.164]	(0.052)	(0.046)	(0.049)	(0.099)	(0.062)	(0.121)
4	0.100	0.075	0.047	0.299	0.180	0.314
[Mean = 0.174]	(0.055)	(0.049)	(0.052)	(0.104)	(0.065)	(0.128)
5	0.104	0.076	0.043	0.310	0.180	0.347
[Mean = 0.181]	(0.057)	(0.050)	(0.053)	(0.107)	(0.067)	(0.130)
N below/above	299 / 303	299 / 303	299 / 303	299 / 186	299 / 186	184 / 186
Kernel	Uni	Uni	Tri	Uni	Uni	Uni
Donut hole	No	No	No	1-sided	1-sided (rec)	2-sided
Bandwidth	255	255	255	255	255	255
Covariates	No	Yes	Yes	Yes	Yes	Yes

*Notes:* Irregular family reunification is defined as a dummy for having at least one coinsured family member who arrived earlier than eighteen months after the sponsor got protection. The rows show the RD estimates for receiving protection after June 1, 2015 (i.e., falling under the new law) on the outcome for 1 to 5 years after protection, respectively, using local linear regressions. Every column shows a different specification: column (1) does not include controls, column (2) includes controls for being married, age at immigration, having completed primary schooling or less, and nationality dummies, column (3) uses a triangular instead of a uniform kernel, column (4) implements a one-sided donut hole of 90 days to the right of the cutoff, column (5) applies the same donut hole and recenters the running variable by subtracting 90 days, column (6) shows the results of a two-sided 90 days donut hole. Additionally, we show the control mean for every year. We calculate the estimates using the *rdrobust* command in R. Robust standard errors in parentheses.

Table A.7: Robustness: Regular family reunification for convention refugees

	(1)	(2)	(3)	(4)	(5)	(6)
Years since protection	RD estimates					
1	0.080	0.075	0.060	0.082	0.106	0.186
[Mean = 0.111]	(0.030)	(0.029)	(0.032)	(0.041)	(0.030)	(0.067)
2	0.099	0.084	0.069	0.114	0.140	0.152
[Mean = 0.418]	(0.039)	(0.035)	(0.038)	(0.064)	(0.042)	(0.087)
3	0.074	0.059	0.049	0.094	0.111	0.157
[Mean = 0.505]	(0.039)	(0.035)	(0.038)	(0.068)	(0.041)	(0.087)
4	0.069	0.054	0.043	0.089	0.102	0.154
[Mean = 0.529]	(0.039)	(0.035)	(0.038)	(0.065)	(0.042)	(0.087)
5	0.079	0.065	0.051	0.114	0.120	0.190
[Mean = 0.542]	(0.039)	(0.035)	(0.038)	(0.065)	(0.042)	(0.087)
N below/above	1625 / 1042	1625 / 1042	1625 / 1042	1033 / 1042	1033 / 1042	1033 / 687
Kernel	Uni	Uni	Tri	Uni	Uni	Uni
Donut hole	No	No	No	1-sided	1-sided (rec)	2-sided
Bandwidth	255	255	255	255	255	255
Covariates	No	Yes	Yes	Yes	Yes	Yes

*Notes:* Regular family reunification is defined as having at least one coinsured family member who arrived six months or more after the sponsor got protection. The rows show the RD estimates for receiving protection after June 1, 2016 (i.e., falling under the new law) on the outcome for 1 to 3 years after protection, respectively, using local linear regressions. Every column shows a different specification: column (1) does not include controls, column (2) includes controls for being married, age at immigration, having completed primary schooling or less, and nationality dummies, column (3) uses a triangular instead of a uniform kernel, column (4) implements a one-sided donut hole of 90 days to the left of the cutoff, column (5) applies the same donut hole and recenters the running variable by subtracting 90 days, column (6) shows the results of a two-sided 90 days donut hole. Additionally, we show the control mean for every year. We calculate the estimates using the *rdrobust* command in R. Robust standard errors in parentheses.

Table A.8: Robustness: Employment for convention refugees

	(1)	(2)	(3)	(4)	(5)	(6)
Years since protection	RD estimates					
1	0.030	0.028	0.034	-0.005	0.011	0.012
[Mean = 0.123]	(0.029)	(0.028)	(0.031)	(0.044)	(0.031)	(0.069)
2	-0.016	-0.013	0.009	-0.047	-0.010	-0.057
[Mean = 0.258]	(0.035)	(0.034)	(0.038)	(0.053)	(0.037)	(0.079)
3	0.060	0.069	0.056	0.149	0.126	0.206
[Mean = 0.370]	(0.039)	(0.037)	(0.041)	(0.059)	(0.040)	(0.087)
4	0.085	0.095	0.096	0.057	0.032	0.039
[Mean = 0.426]	(0.039)	(0.038)	(0.041)	(0.062)	(0.042)	(0.090)
5	0.091	0.100	0.078	0.178	0.140	0.222
[Mean = 0.450]	(0.038)	(0.037)	(0.041)	(0.063)	(0.042)	(0.091)
N below/above	1625 / 1042	1625 / 1042	1625 / 1042	1033 / 1042	1033 / 1042	1033 / 687
Kernel	Uni	Uni	Tri	Uni	Uni	Uni
Donut hole	No	No	No	1-sided	1-sided (rec)	2-sided
Bandwidth	255	255	255	255	255	255
Covariates	No	Yes	Yes	Yes	Yes	Yes

*Notes:* Employment is defined as a dummy for having any employment spell subject to social insurance contributions. The rows show the RD estimates for receiving protection after June 1, 2016 (i.e., falling under the new law) on the outcome for 1 to 5 years after protection, respectively, using local linear regressions. Every column shows a different specification: column (1) does not include controls, column (2) includes controls for being married, age at immigration, having completed primary schooling or less, and nationality dummies, column (3) uses a triangular instead of a uniform kernel, column (4) implements a one-sided donut hole of 90 days to the left of the cutoff, column (5) applies the same donut hole and recenters the running variable by subtracting 90 days, column (6) shows the results of a two-sided 90 days donut hole. Additionally, we show the control mean for every year. We calculate the estimates using the *rdrobust* command in R. Robust standard errors in parentheses.

Table A.9: Robustness: Monthly real income for convention refugees

	(1)	(2)	(3)	(4)	(5)	(6)
Years since protection	RD estimates					
1	103.630	96.965	122.388	53.314	67.158	17.420
[Mean = 224]	(57.264)	(54.818)	(61.413)	(78.740)	(58.203)	(125.703)
2	44.764	49.246	124.252	-0.094	48.448	-116.634
[Mean = 487]	(74.230)	(71.074)	(79.448)	(106.480)	(76.244)	(158.700)
3	200.686	220.376	220.179	225.937	237.247	328.452
[Mean = 740]	(86.068)	(82.246)	(91.202)	(131.285)	(91.080)	(185.990)
4	212.794	234.879	267.620	116.497	126.432	114.362
[Mean = 862]	(89.634)	(86.556)	(94.393)	(140.035)	(96.264)	(203.089)
5	123.111	146.375	185.976	276.880	244.866	134.667
[Mean = 922]	(88.747)	(86.412)	(94.068)	(140.658)	(95.485)	(204.421)
N below/above	1625 / 1042	1625 / 1042	1625 / 1042	1033 / 1042	1033 / 1042	1033 / 687
Kernel	Uni	Uni	Tri	Uni	Uni	Uni
Donut hole?	No	No	No	1-sided	1-sided (rec)	2-sided
Bandwidth	255	255	255	255	255	255
Covariates	No	Yes	Yes	Yes	Yes	Yes

*Notes:* The outcome is gross monthly real income (in 2015 euros). The rows show the RD estimates for receiving protection after June 1, 2016 (i.e., falling under the new law) on the outcome for 1 to 5 years after protection, respectively, using local linear regressions. Every column shows a different specification: column (1) does not include controls, column (2) includes controls for being married, age at immigration, having completed primary schooling or less, and nationality dummies, column (3) uses a triangular instead of a uniform kernel, column (4) implements a one-sided donut hole of 90 days to the left of the cutoff, column (5) applies the same donut hole and recenters the running variable by subtracting 90 days, column (6) shows the results of a two-sided 90 days donut hole. Additionally, we show the control mean for every year. We calculate the estimates using the *rdrobust* command in R. Robust standard errors in parentheses.

Table A.10: Robustness: Monthly real income above 3000 euros for convention refugees

	(1)	(2)	(3)	(4)	(5)	(6)
Years since protection	RD estimates					
1	0.003	0.002	0.007	0.000	-0.000	-0.014
[Mean = 0.003]	(0.006)	(0.006)	(0.007)	(0.008)	(0.006)	(0.012)
2	-0.004	-0.004	-0.002	-0.005	-0.000	-0.006
[Mean = 0.012]	(0.009)	(0.009)	(0.010)	(0.012)	(0.009)	(0.018)
3	0.000	-0.000	-0.001	0.007	0.009	0.003
[Mean = 0.024]	(0.013)	(0.013)	(0.015)	(0.018)	(0.013)	(0.024)
4	-0.009	-0.009	0.002	-0.032	-0.017	-0.049
[Mean = 0.041]	(0.015)	(0.015)	(0.017)	(0.026)	(0.018)	(0.033)
5	-0.013	-0.013	-0.004	-0.001	-0.002	-0.060
[Mean = 0.040]	(0.016)	(0.016)	(0.017)	(0.025)	(0.017)	(0.038)
N below/above	1625 / 1042	1625 / 1042	1625 / 1042	1033 / 1042	1033 / 1042	1033 / 687
Kernel	Uni	Uni	Tri	Uni	Uni	Uni
Donut hole?	No	No	No	1-sided	1-sided (rec)	2-sided
Bandwidth	255	255	255	255	255	255
Covariates	No	Yes	Yes	Yes	Yes	Yes

*Notes:* The outcome is a dummy equal to one if the gross monthly real income (in 2015 euros) exceed 3000 euros. The rows show the RD estimates for receiving protection after June 1, 2016 (i.e., falling under the new law) on the outcome for 1 to 5 years after protection, respectively, using local linear regressions. Every column shows a different specification: column (1) does not include controls, column (2) includes controls for being married, age at immigration, having completed primary schooling or less, and nationality dummies, column (3) uses a triangular instead of a uniform kernel, column (4) implements a one-sided donut hole of 90 days to the left of the cutoff, column (5) applies the same donut hole and recenters the running variable by subtracting 90 days, column (6) shows the results of a two-sided 90 days donut hole. Additionally, we show the control mean for every year. We calculate the estimates using the *rdrobust* command in R. Robust standard errors in parentheses.

Table A.11: Robustness: Welfare benefits receipt for convention refugees

	(1)	(2)	(3)	(4)	(5)	(6)
Years since protection	RD estimates					
1	-0.037	-0.036	-0.024	-0.036	-0.034	-0.094
[Mean = 0.875]	(0.030)	(0.028)	(0.031)	(0.045)	(0.031)	(0.072)
2	0.057	0.051	0.037	0.142	0.067	0.052
[Mean = 0.711]	(0.036)	(0.034)	(0.037)	(0.056)	(0.038)	(0.083)
3	0.015	0.004	-0.003	0.013	-0.027	-0.110
[Mean = 0.586]	(0.039)	(0.037)	(0.040)	(0.059)	(0.040)	(0.087)
4	-0.012	-0.023	-0.025	-0.001	-0.016	-0.037
[Mean = 0.479]	(0.039)	(0.037)	(0.041)	(0.062)	(0.041)	(0.087)
5	-0.005	-0.016	-0.010	-0.056	-0.051	-0.115
[Mean = 0.455]	(0.038)	(0.036)	(0.040)	(0.061)	(0.041)	(0.084)
N below/above	1625 / 1042	1625 / 1042	1625 / 1042	1033 / 1042	1033 / 1042	1033 / 687
Kernel	Uni	Uni	Tri	Uni	Uni	Uni
Donut hole?	No	No	No	1-sided	1-sided (rec)	2-sided
Bandwidth	255	255	255	255	255	255
Covariates	No	Yes	Yes	Yes	Yes	Yes

*Notes:* The outcome is a dummy equal to one if a refugee still receives welfare benefits. The rows show the RD estimates for receiving protection after June 1, 2016 (i.e., falling under the new law) on the outcome for 1 to 5 years after protection, respectively, using local linear regressions. Every column shows a different specification: column (1) does not include controls, column (2) includes controls for being married, age at immigration, having completed primary schooling or less, and nationality dummies, column (3) uses a triangular instead of a uniform kernel, column (4) implements a one-sided donut hole of 90 days to the left of the cutoff, column (5) applies the same donut hole and recenters the running variable by subtracting 90 days, column (6) shows the results of a two-sided 90 days donut hole. Additionally, we show the control mean for every year. We calculate the estimates using the *rdrobust* command in R. Robust standard errors in parentheses.



Table A.12: Robustness: Irregular family reunification for convention refugees

	(1)	(2)	(3)	(4)	(5)	(6)
Years since protection	RD estimates					
1	0.018	0.015	-0.002	0.004	-0.025	0.124
[Mean = 0.112]	(0.020)	(0.019)	(0.019)	(0.040)	(0.025)	(0.056)
2	0.032	0.032	0.015	0.028	-0.044	0.135
[Mean = 0.168]	(0.021)	(0.021)	(0.021)	(0.048)	(0.028)	(0.064)
3	0.034	0.035	0.020	0.036	-0.044	0.141
[Mean = 0.180]	(0.022)	(0.022)	(0.022)	(0.048)	(0.029)	(0.064)
4	0.032	0.033	0.015	0.046	-0.041	0.150
[Mean = 0.183]	(0.022)	(0.022)	(0.022)	(0.049)	(0.029)	(0.064)
5	0.033	0.034	0.016	0.048	-0.041	0.151
[Mean = 0.185]	(0.022)	(0.022)	(0.022)	(0.049)	(0.029)	(0.065)
N below/above	1625 / 1042	1625 / 1042	1625 / 1042	1033 / 1042	1033 / 1042	1033 / 687
Kernel	Uni	Uni	Tri	Uni	Uni	Uni
Donut hole?	No	No	No	1-sided	1-sided (rec)	2-sided
Bandwidth	250	250	250	250	250	250
Covariates	No	Yes	Yes	Yes	Yes	Yes

*Notes:* Irregular family reunification is defined as a dummy for having at least one coinsured family member who arrived earlier than six months after the sponsor got protection. The rows show the RD estimates for receiving protection after June 1, 2016 (i.e., falling under the new law) on the outcome for 1 to 5 years after protection, respectively, using local linear regressions. Every column shows a different specification: column (1) does not include controls, column (2) includes controls for being married, age at immigration, having completed primary schooling or less, and nationality dummies, column (3) uses a triangular instead of a uniform kernel, column (4) implements a one-sided donut hole of 90 days to the left of the cutoff, column (5) applies the same donut hole and recenters the running variable by subtracting 90 days, column (6) shows the results of a two-sided 90 days donut hole. Additionally, we show the control mean for every year. We calculate the estimates using the *rdrobust* command in R. Robust standard errors in parentheses.

Table A.13: Robustness: Other RD specification choices for subsidiary-protected refugees, 36 months after protection

	(1) Reg. fam. ref.	(2) Irreg. fam. ref.	(3) Employment	(4) Monthly Income	(5) Benefits receipt
<i>Panel A. rdrobust (mse)</i>					
Estimate	-0.059	0.032	-0.103	-68.942	0.136
SE	(0.120)	(0.094)	(0.130)	(244.185)	(0.159)
N below/above	59 / 76	70 / 80	90 / 97	90 / 99	63 / 78
Bandwidth	45.855	53.473	71.568	72.832	48.726
<i>Panel B. OLS</i>					
Estimate	-0.070	0.064	0.003	-55.339	-0.007
SE	(0.045)	(0.048)	(0.069)	(134.791)	(0.068)
N	602	602	602	602	602
<i>Panel C. RDHonest</i>					
Estimate	-0.072	0.088	-0.019	-92.266	0.016
CI	[-3.083, 2.939]	[-1.612, 1.789]	[-2.062, 2.025]	[-3380.611, 3196.080]	[-3.808, 3.840]
N	602	602	602	602	602
Bandwidth	255	255	255	255	255
<i>Panel D. Local Randomization</i>					
Estimate	-0.069	0.022	0.001	-3.691	0.011
CI	[-0.16,0.02]	[-0.08,0.1]	[-0.14,0.1]	[-290,282]	[0,0.02]
N below/above	80 / 88	80 / 88	80 / 88	80 / 88	80 / 88
Window	[-61;61]	[-61;61]	[-61;61]	[-61;61]	[-61;61]

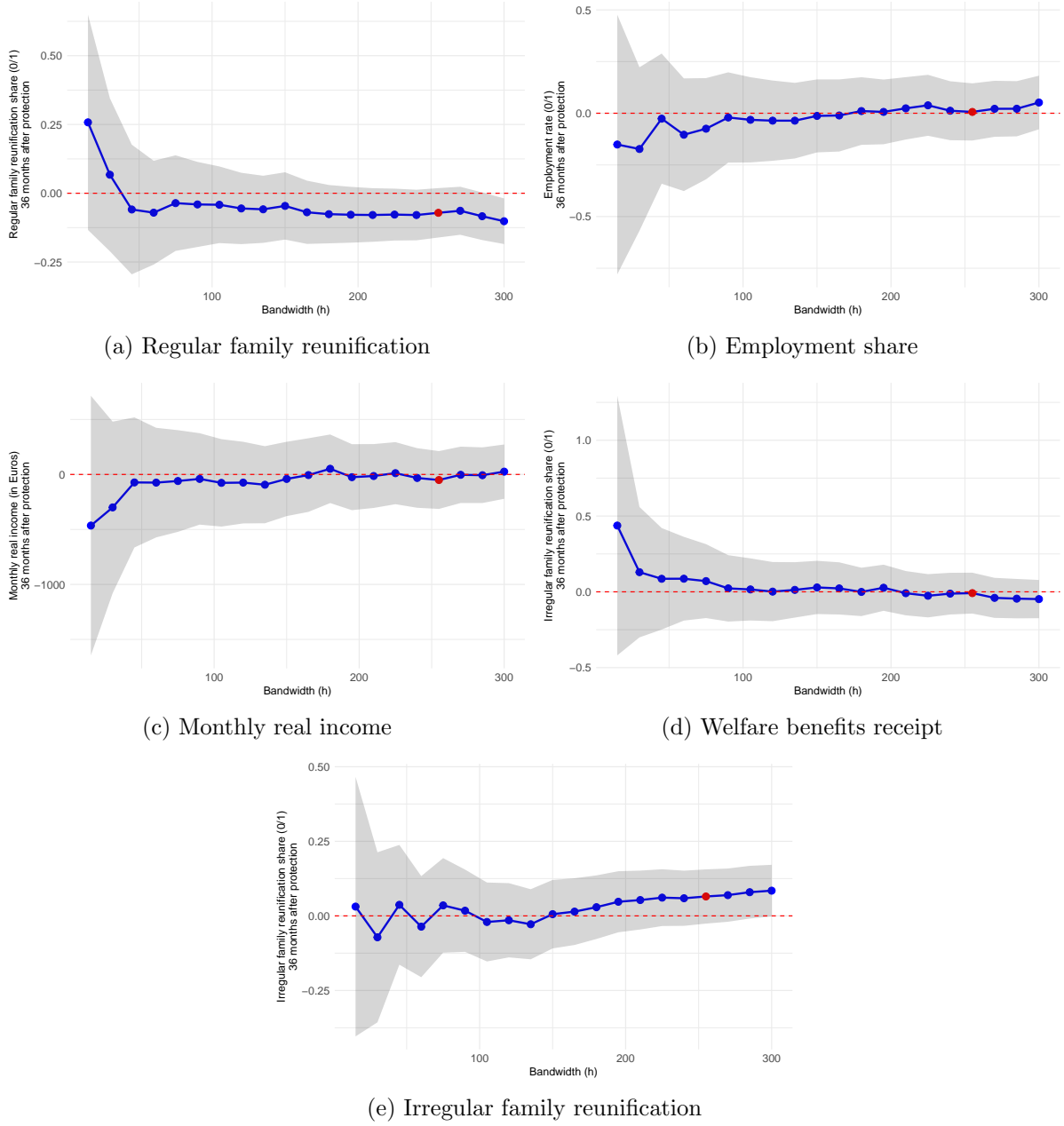
*Notes:* The table presents the main results of selected outcomes for convention refugees using different RD specifications. Every column shows the results for a different outcome measured 36 months after protection. Panel A shows the results using *rdrobust* but instead of using a bandwidth equal to 255 as in the main specification, it uses the MSE-optimal bandwidth selector (data driven). Thus, a different bandwidth is selected by outcome. Panel B shows the results of a standard OLS regression with equal trends at both sides of the cutoff and robust standard errors. Panel C uses the command *RDHonest* (Kolesár and Rothe, 2018) which takes a more conservative approach to inference. Panel D uses a local randomization approach implemented by *rdrandinf* in R, as a complement to the the continuity based approach (Cattaneo et al., 2016).

Table A.14: Robustness: Other RD specification choices for convention refugees, 12 months after protection

	(1) Reg. fam. ref.	(2) Irreg. fam. ref.	(3) Employment	(4) Monthly Income	(5) Benefits receipt
<i>Panel A. rdrobust (mse)</i>					
Estimate	0.147	0.036	0.063	238.383	-0.068
SE	(0.072)	(0.034)	(0.067)	(137.062)	(0.062)
N below/above	253 / 203	426 / 260	309 / 225	300 / 221	363 / 241
Bandwidth	40.232	58.831	47.025	46.762	51.119
<i>Panel B. OLS</i>					
Estimate	0.060	0.033	0.020	77.102	-0.037
SE	(0.028)	(0.020)	(0.027)	(52.093)	(0.027)
N	2667	2667	2667	2667	2667
<i>Panel C. RDHonest</i>					
Estimate	0.080	0.018	0.030	103.630	-0.037
CI	[-2.179, 2.339]	[-0.502, 0.537]	[-2.767, 2.827]	[-5860.091, 6067.351]	[-2.140, 2.066]
N	2667	2667	2667	2667	2667
Bandwidth	255	255	255	255	255
<i>Panel D. Local Randomization</i>					
Estimate	0.067	0.011	0.040	139.389	0.010
CI	[0,0.1]	[-0.02,0.05]	[-0.02,0.1]	[2,279]	[0,0.02]
N below/above	217 / 191	217 / 191	217 / 191	217 / 191	217 / 191
Window	[-35;35]	[-35;35]	[-35;35]	[-35;35]	[-35;35]

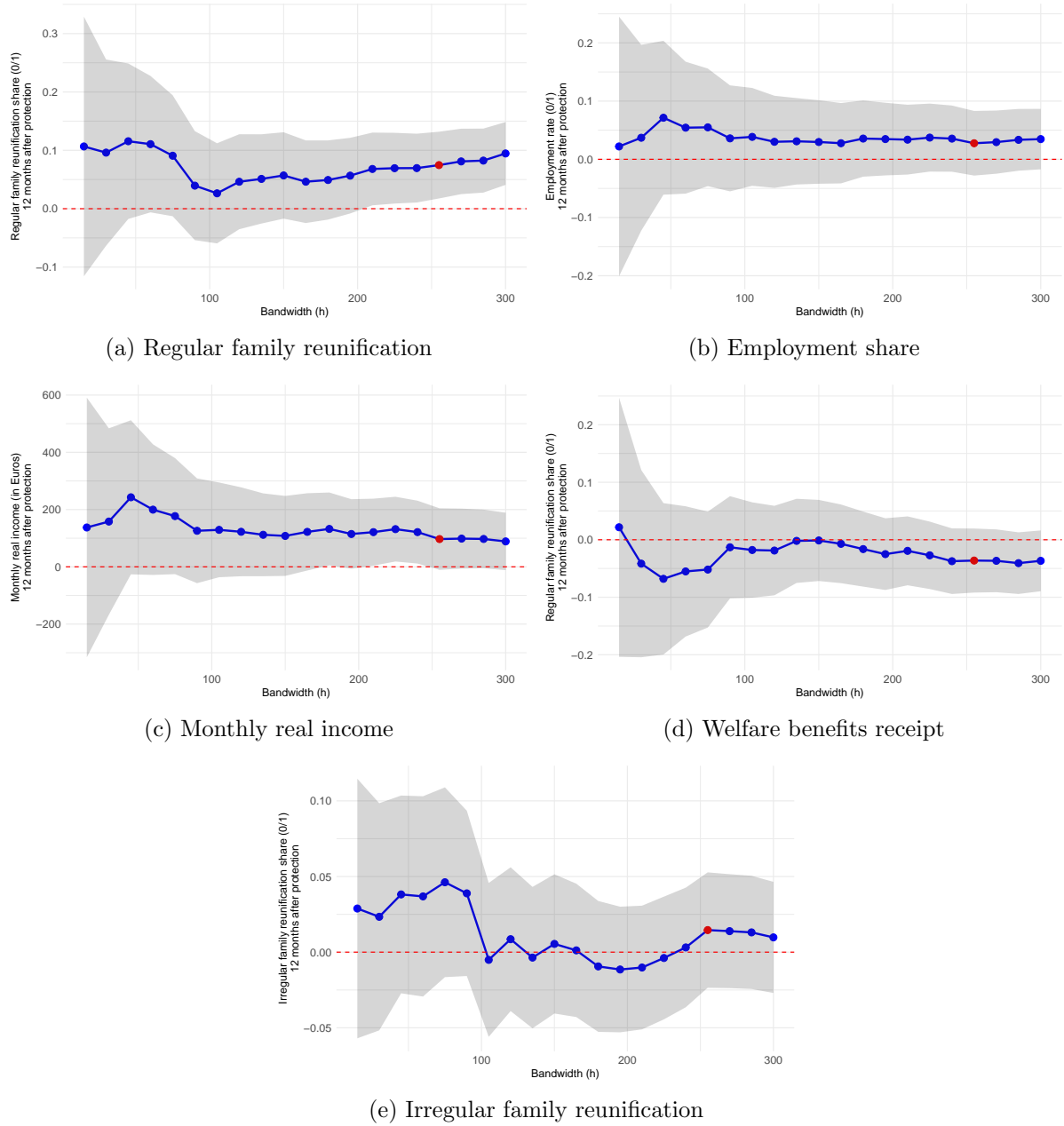
*Notes:* The table presents the main results of selected outcomes for convention refugees using different RD specifications. Every column shows the results for a different outcome measured 12 months after protection. Panel A shows the results using *rdrubust* but instead of using a bandwidth equal to 255 as in the main specification, it uses the MSE-optimal bandwidth selector (data driven). Thus, a different bandwidth is selected by outcome. Panel B shows the results of a standard OLS regression with equal trends at both sides of the cutoff and robust standard errors. Panel C uses the command *RDHonest* (Kolesár and Rothe, 2018) which takes a more conservative approach to inference. Panel D uses a local randomization approach implemented by *rdrandinf* in R, as a complement to the the continuity based approach (Cattaneo et al., 2016).

Figure A.9: The effects of the reform on our outcomes three years after protection using different bandwidths around the reform for subsidiary-protected



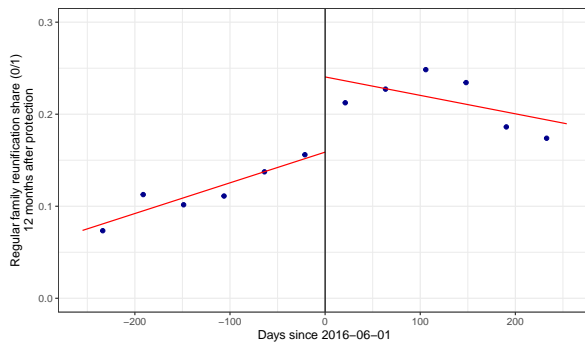
*Notes:* The figure shows the RD estimates for receiving protection after June 1, 2015 (i.e., falling under the new law) on the outcomes three years after protection, using local linear regressions with a uniform kernel and covariates. The figure shows estimates for different bandwidths ranging from 15 to 300 days around the reform. The red dot shows the estimate from our preferred specification (at 255 days). The grey areas show the 95% confidence intervals based on robust standard errors.

Figure A.10: The effects of the reform on our outcomes one year after protection using different bandwidths around the reform for convention refugees

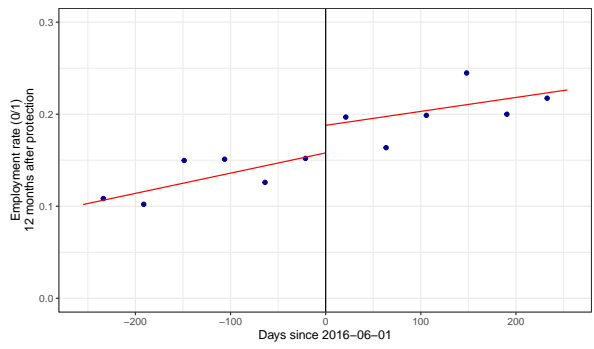


*Notes:* The figure shows the RD estimates for receiving protection after June 1, 2016 (i.e., falling under the new law) on the outcomes one years after protection, using local linear regressions with a uniform kernel and covariates. The figure shows estimates for different bandwidths ranging from 15 to 300 days around the reform. The red dot shows the estimate from our preferred specification (at 255 days). The grey areas show the 95% confidence intervals based on robust standard errors.

Figure A.11: Family reunification rates and employment for Geneva Convention refugees without “Asyl auf Zeit”



(a) Family reunification



(b) Employment rate

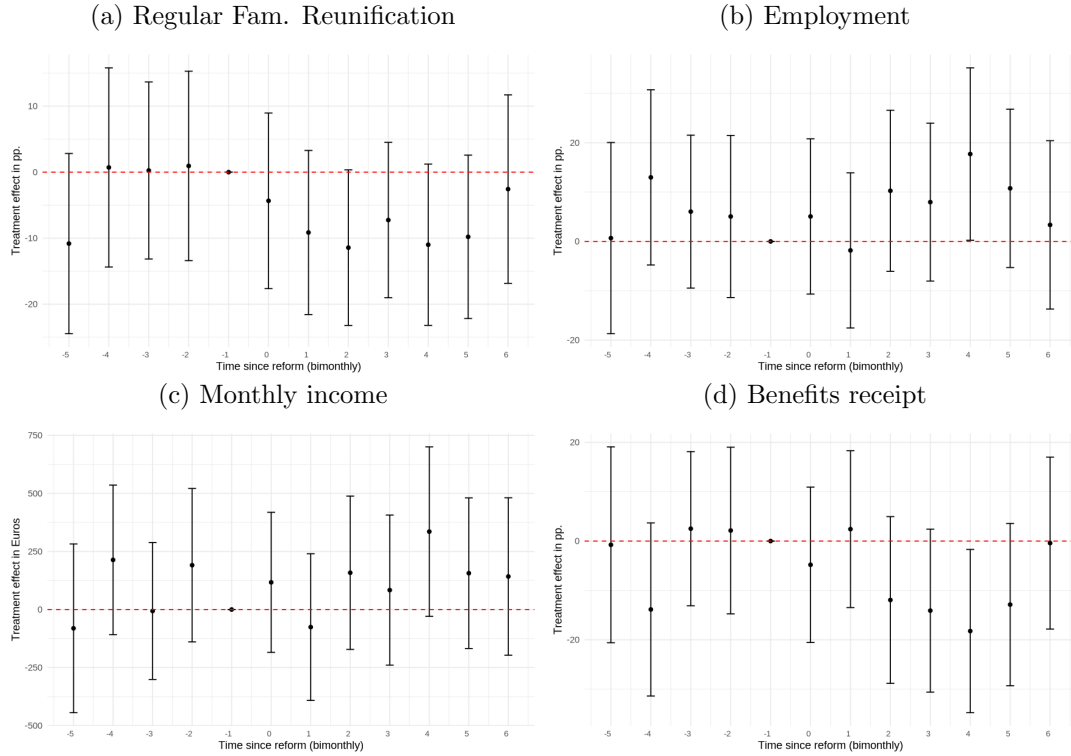
*Notes:* The figures show regression discontinuity plots for employment rates and monthly labor real income of the subsidiary-protected, using a bandwidth of 255 days. The red solid line shows a linear fit with different slopes at both sides the cutoff. The sample size left of the cutoff is 295 individuals, and 294 right of the cutoff. Both models control for the covariates in Table 3.

Table A.15: Estimated DiD effects for subsidiary-protected refugees

	Reg. Fam. Ref.	Employment	Monthly Income	Benefits receipt
Treatment (SUB=1)	-0.282*** (0.022)	-0.028 (0.026)	-64.716 (48.628)	0.001 (0.026)
Post (after June 1, 2015)	0.016 (0.016)	0.080*** (0.015)	187.057*** (30.130)	-0.071*** (0.015)
Treatment $\times$ Post	-0.068** (0.027)	0.025 (0.035)	48.244 (68.532)	-0.070** (0.035)
Num.Obs.	4703	4703	4703	4703
Covariates	Yes	Yes	Yes	Yes

*Notes:* The table shows the results for a standard difference-in-differences approach estimates on the sample of refugees who obtained protection between July 2014, and May 2016. Treated is a dummy equal one for subsidiary-protected refugees; and zero for convention refugees. Post indicates having received protection after June 1, 2015. Treatment  $\times$  Post indicates the treatment effect. Each column shows the effects for a different outcome.

Figure A.12: Dynamic DiD for subsidiary-protected refugees



*Notes:* The panel show the treatment effects of a dynamic difference-in-difference specification. Each panel shows a different outcome. The x-axis shows bimonthly averages for groups that received protection before and after the reform (June 1, 2015). For example, the point estimate for period 1 shows the treatment effect for those subsidiary-protected refugee who got asylum during June-July 2015.



## B Data Appendix

First, we keep all asylum-seekers who arrived to Austria since January 1, 2010. This leaves us with 268,549 uniquely identified asylum-seekers. We have information on the asylum status for 29% of those. Table B.1 compares our data with official statistics from the BMI. Column (1) compares the yearly arrivals from the ASSD with the registered asylum applications by the BMI. At the peak of the asylum-seekers' arrivals in 2015, our data captures around 92% of the official applications. Since asylum-seekers do not submit their application upon arrival but only with some lag, we see larger numbers for the years 2016–2020 in the BMI applications than in our data. For 2015 our data captures almost 73% of refugees with a Geneva Convention status and 100% of subsidiary-protected.

Table B.1: Comparison between our data (AMS) and official statistics (BMI)

Year	(1) Arrivals ASSD	(2) Asylum Applications	(3) (1)/(2)	(4) Gen. Con. (AMS)	(5) Gen. Con. (BMI)	(6) (4)/(5)	(7) Sub. (AMS)	(8) Sub. (BMI)	(9) (6)/(7)
2010	7,804	11,012	70.9 %	1,424	2,977	47.8 %	932	1,749	53.3 %
2011	11,270	14,416	78.2 %	1,858	3,572	52.0 %	1,143	2,023	56.5 %
2012	14,295	17,413	82.1 %	1,865	3,680	50.7 %	1,319	2,050	64.3 %
2013	14,390	17,503	82.2 %	2,283	4,133	55.2 %	1,236	1,819	67.9 %
2014	25,085	28,064	89.4 %	5,901	8,734	67.6 %	1,789	2,617	68.4 %
2015	82,255	88,340	93.1 %	10,502	14,413	72.9 %	2,482	2,478	100.2 %
2016	39,564	42,285	93.6 %	14,360	22,307	64.4 %	3,114	3,699	84.2 %
2017	20,424	24,735	82.6 %	10,893	21,767	50.0 %	4,234	7,081	59.8 %
2018	9,366	13,746	68.1 %	6,483	14,696	44.1 %	2,488	4,191	59.4 %
2019	1,911	12,886	14.8 %	3,793	9,723	39.0 %	1,210	2,246	53.9 %
2020	3,753	14,775	25.4 %	3,422	8,069	42.4 %	1,400	2,524	55.5 %

*Notes:* The table compared the headcounts on yearly arrivals (columns 1, 4, 7), and type of status granted in our data ASSD/AMS with those reported in the official statistics from BMI (columns 2, 5, 8). Columns 3, 6, and 9 show the shares of how many observations do we have in our data with respect to the official numbers. *Sources:* ASSD (Zweimüller et al., 2009), AMS, BMI 2013-2020 (Bundesministerium für Inneres, 2020).