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Abstract:

Many European countries impose employment bans that prevent asylum seekers from entering the local labor market for a certain waiting period upon arrival. We provide evidence on the long-term effects of such employment bans on the subsequent economic integration of refugees. We leverage a natural experiment in Germany, where a court ruling prompted a reduction in the length of the employment ban. We find that five years after the waiting period was reduced, employment rates were about 20 percentage points lower for refugees who, upon arrival, had to wait an additional seven months before they were allowed to enter the labor market. It took up to ten years for this employment gap to disappear. Our findings suggest that longer employment bans considerably slowed down the economic integration of refugees and reduced their motivation to integrate early on after arrival. A marginal social cost analysis for the study sample suggests that this employment ban cost German taxpayers about 40 million Euro per year on average in terms of welfare expenditures and forgone tax revenues from unemployed refugees.

Summary:

Temporary employment bans for asylum seekers have lasting negative consequences for their economic integration.

Keywords:

asylum policy | refugee migration | economic integration | employment ban | labor market

Introduction

European countries are struggling with the largest refugee crisis since the aftermath of World War II. Following steep increases in the number of people seeking refugee status in Europe, policymakers face a major challenge in determining how best to integrate refugees and asylum seekers into the host country's economy and society (*1*). One of the most important issues involves their access to the host country labor market (*2–4*). Policymakers face a dilemma: On the one hand, given the costs of supporting refugees and asylum seekers after arrival, European countries would benefit from rapidly integrating them into the local labor markets such that they can start to work, become self-sufficient, and contribute to the local economy. On the other hand, European governments are often reluctant to allow new asylum seekers to work given the uncertainty about whether their asylum claims will be approved and political concerns that they might displace native workers (*5, 6*).

Most European governments have opted to require asylum seekers to wait before they are allowed to enter the labor market (*7, 8*). As shown in Figure 1, there is considerable variation in the required wait time across European countries, with most falling between six and twelve months. The United States, Turkey, and other OECD countries outside Europe have imposed similar employment bans on asylum seekers (*1*).

Proponents of employment bans often argue that letting asylum seekers access the labor market effectively integrates them into the host society during the asylum process, making deportation more difficult if their asylum claim is rejected. Work permission, they say, also acts as a pull-factor and encourages even more people to apply for asylum (*5, 6*). In addition, employment bans may be popular with voters who worry that asylum seekers and refugees take away jobs from natives (*9*). Opponents argue that employment bans make it difficult for asylum seekers and refugees to gain a footing in their host country. Forced into unemployment, asylum

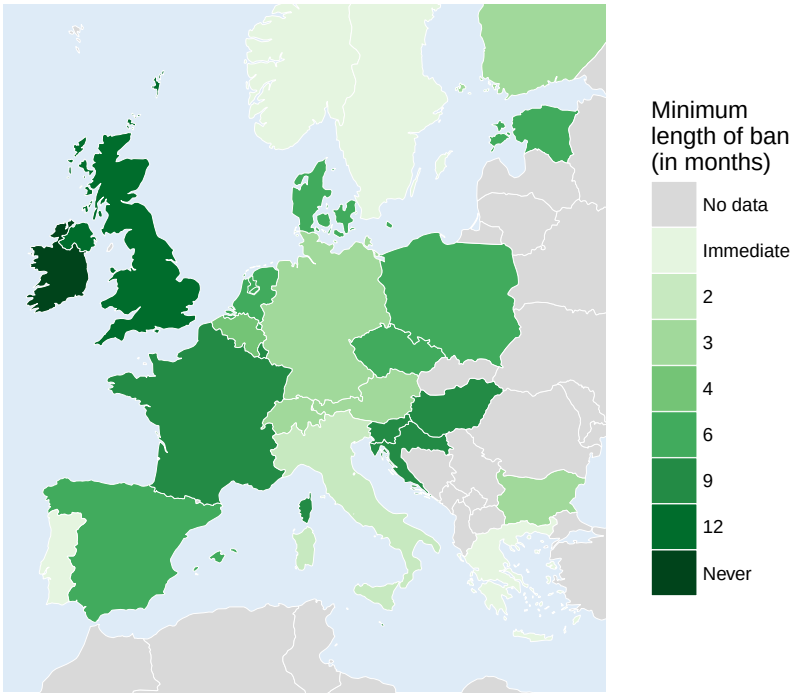

Figure 1: **Minimum length of employment bans for asylum seekers in European countries in 2016.** There is considerable heterogeneity in the length of time asylum seekers have to wait until they can access the labor market across Europe, ranging from the day of arrival (e.g. Sweden) to an indefinite ban (Ireland). The median length across countries is 6 months. [Data source: (*1*, *8*)]

seekers are in limbo until they can seek work. This can lead to lower motivation, depreciation of human capital, and scarring, which might slow labor market integration for many years after the waiting period is completed (*10–12*). Opponents also argue that this is costly for host societies, which face higher welfare expenditures for unemployed asylum seekers and refugees and forgo the tax contributions they would have made if employed.

Despite the importance of this issue, we have very limited evidence about how employment bans affect asylum seekers and refugees. In fact, even though employment bans are in place across Europe, we are not aware of any published study that has provided causal evidence on the effects of waiting periods for accessing the labor market on the short- and long-term

economic integration of refugees. Studying the effects of employment bans for asylum seekers is difficult for at least two reasons. First, there is a measurement problem, because general population surveys often do not measure whether immigrants first entered the country as asylum seekers or under another immigration status. Second, it is challenging to empirically isolate the causal effects of employment bans, because countries that impose waiting periods of different lengths also differ on many other confounding factors that can impact refugee employment. And even if the comparison is limited to refugees who are affected by changes in the length of an employment ban within the same country, one might worry that such changes are endogenous to changes in the local labor market conditions. For example, a country might introduce or extend an employment ban because labor market conditions are deteriorating.

In this study we take a first step toward generating causal evidence on the effects of employment bans on refugee integration. In particular, we examine the short- and long-term effects of such employment bans on the economic integration of refugees. We draw on a case study in Germany, a country that has been a major European destination country for refugees in the past decades, including refugees during the Yugoslavian wars in the nineties and the present refugee crisis stemming from violence in the Middle East and Africa (*13*).

To address the causal identification problem, our study design leverages a natural experiment. On March 22, 2000 a court ruling prompted the German government to change the employment ban for asylum seekers from indefinite to twelve months, thereby creating exogenous variation in the amount of time asylum seekers from different arrival cohorts had to wait before they could enter the German labour market. To address the measurement problem, we draw on the 2000-2014 waves of the German *Mikrozensus*. Each wave is a representative annual survey that covers 1% of the resident population. We focus on the group of immigrants from the former Federal Republic of Yugoslavia (FRY) who arrived in Germany in 1999 and 2000 (*14*). Even though the *Mikrozensus* does not measure asylum-seeker status upon arrival, we can establish

based on register data that the overwhelming majority of immigrants who arrived in Germany during those years from the FRY were asylum seekers who were fleeing because of the Kosovo war.

To identify the effects of the length of the waiting period we compare the cohorts of FRY asylum seekers who arrived in Germany in 1999 and 2000, respectively. These cohorts faced very different wait times, because the new, 12-month waiting period went into effect on December 15, 2000. Importantly, this new rule was also applied to asylum seekers who had arrived before December 15, 2000. Therefore, all refugees who entered in 2000 had to wait 12 months from their date of arrival before they were allowed to enter the German labor market. By contrast, refugees who entered in 1999 had to wait between 13 and 24 months, depending on when in 1999 they had arrived. For example, a refugee who had arrived in January 1999 had to wait 24 months while a refugee who had arrived in December 1999 only had to wait 13 months. On average, refugees in the 1999 cohort had to wait 7.1 months longer than the 2000 cohort (see the Supplementary Materials for details). As we show below, these two arrival cohorts were otherwise similar across many characteristics, allowing us to isolate the short- and long-term effects of the differences in the length of the waiting period on the economic integration of the refugees. In addition, we use various placebo checks with the FRY refugees and other immigrant groups to rule out alternative explanations. Details about the measures, sample, design, and statistical analysis can be found in the Materials and Methods section.

Results

Figure 2, Panel A shows the estimated employment rates of both cohorts of FRY refugees who arrived in Germany in 1999 and 2000, respectively. We find that the 1999 cohort, which faced on average 7.1 months of additional wait time, experienced much lower employment rates compared to the 2000 cohort for many years following the reduction of the employment ban in

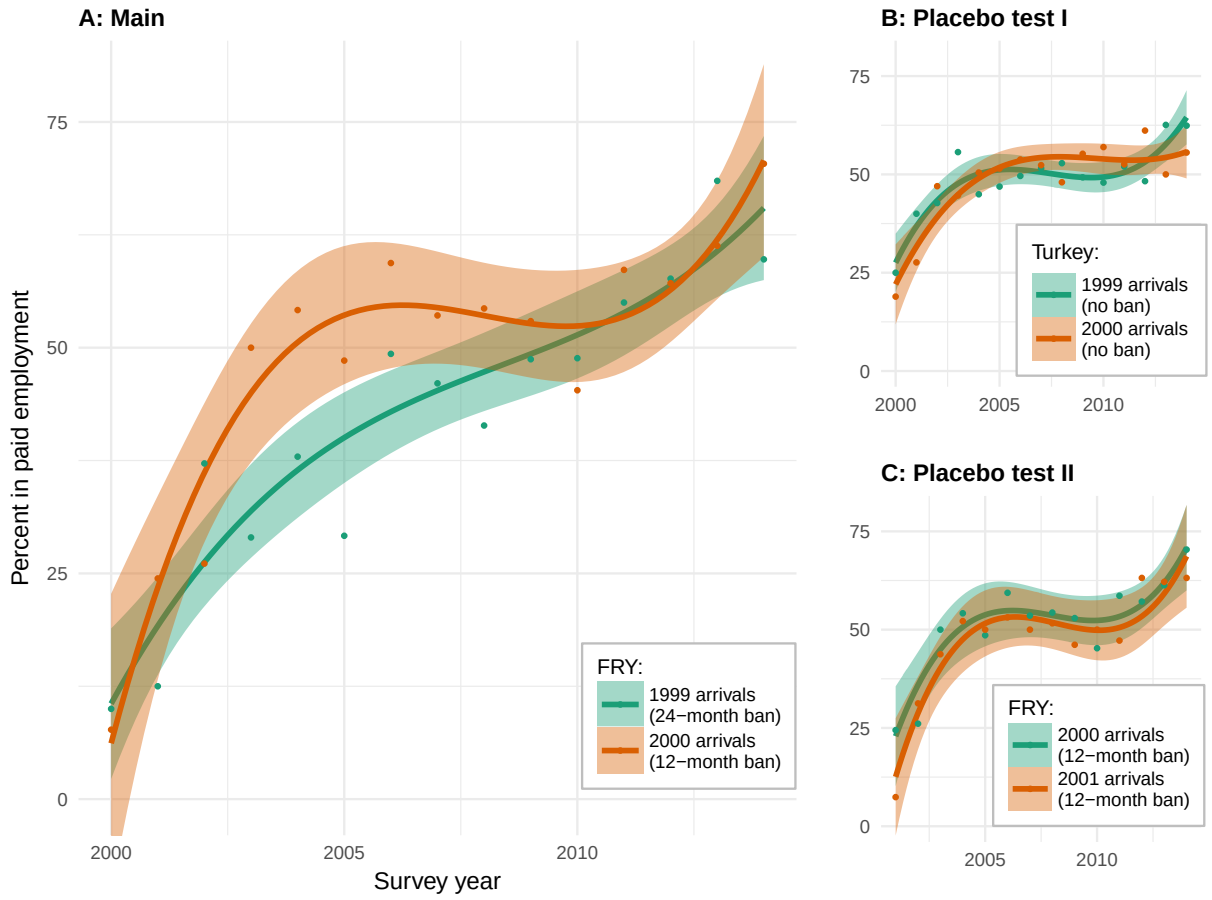

Figure 2: **Longer employment bans worsen employment trajectories of refugees.** Panel A shows the employment trajectories of FRY refugees who arrived in Germany in 1999 (in green) and in 2000 (in red), ($n = 1,748$). The 1999 arrival cohort faced a 13–24-month employment ban (depending on their month of arrival), while the 2000 arrival cohort faced a 12-months employment ban. The average difference in the length of the waiting period between the 1999 and 2000 cohort is 7.1 months. The dots indicate the percentage of respondents who are in paid employment by survey year. The curved regression lines and corresponding 95% confidence intervals are a non-parametric approximation of the employment trajectories using regression B-splines. Panel B shows the results of the first placebo test: Turkish immigrants who arrived in 1999 and 2000 but were not subject to the ban experienced very similar employment trajectories ($n = 3,712$). Panel C shows the results of the second placebo test: FRY refugees who arrived in 2000 and 2001 and were subject to the same 12-month waiting period experienced virtually identical employment trajectories ($n = 1,067$).

2000. While both cohorts start out with similarly low employment rates in 2001, the year they were allowed to enter the German labour market, the 2000 cohort finds work much faster over the following years, while the employment growth among the 1999 cohort considerably lags behind. By 2005, five years after the ban is reduced, we find that the employment rates among the 1999 cohort is only 29% compared to 49% among the the 2000 cohort. This 20 percentage points gap in employment amounts to about a 67% difference compared to the employment rate among the 1990 cohort. After 2005 the gap starts to narrow, but it is not until 2010, about ten years after the ban was reduced, that the 1999 cohort catches up to the employment rate of the 2000 cohort.

One concern with the previous results might be that FRY refugees in the 1999 arrival cohort differ from the FRY refugees in the 2000 arrival cohort in important confounding characteristics that could explain the considerable gap in employment. This seems unlikely for various reasons. First, balance checks show that the two cohorts exhibit no discernible differences in terms of many demographic characteristics, such as age at arrival, gender, and education level, as well as in their answers to health-related survey items. The only exception to this are some imbalances that appear in the first *Mikrozensus* wave in 2000. Refugees arrived throughout that year, but the *Mikrozensus* was fielded in May, so the 2000 arrival cohort was only partially covered (for details see SM Section 3.1). We exclude the 2000 wave from the statistical models below and focus on the post 2000 period when both arrival cohorts were covered. There are also no discernible differences in terms of the propensity to leave Germany. If that were the case, we would expect the sample composition to shift over time. However, the relative sampling fraction of each cohort is fairly constant across waves (see SM Section 3.2).

Second, one potentially important difference between the two cohorts is that the 1999 cohort has one additional year of residency in Germany compared to the 2000 cohort. This should, if anything, bias the comparison against finding a negative effect of the longer waiting period, be-

cause much research has shown that years of residency in the host country is one of the strongest predictors of economic integration (*15–17*). Given that the 1999 cohort had one additional year to acquire local knowledge about Germany, learn the language, build networks, and search for opportunities, these refugees should have enjoyed a considerable advantage over those who had just arrived.

Third, if there exists a confounding characteristic that is associated with lower employment for immigrants who arrived in 1999 compared to 2000 (such as long-term consequences of differences in initial economic conditions), we would expect that confounder also to operate on groups who did not enter as refugees and are therefore unaffected by the employment ban. Figure 1, Panel B shows the results of a placebo check that rules out this possibility. Leveraging Turkish immigrants who arrived in Germany in 1999 and 2000, most of whom were not asylum seekers and or refugees and so were unaffected by the policy change, we find no discernible difference in the employment rates of these Turkish arrival cohorts.

In addition, Figure 1, Panel C shows that there are also no discernible differences between the employment trajectories of FRY refugees who arrived in 2000 and 2001, respectively, and were subject to the same 12-month ban. This suggests that in the absence of changes in the length of the waiting period, there are no confounders that independently caused a gap in employment rates between subsequent cohorts, let alone a gap of the magnitude as large as the one we find for the 1999 and 2000 FRY refugee cohort. Taken together, these additional tests suggest that it is unlikely that the long-term employment effects of the ban we find are driven by differences in unobserved confounders.

The blue line in Figure 3 shows the estimated effects of the 7.1-months-longer average waiting period on the probability of employment for the first sixteen years after arrival. These estimates are based on a statistical model where we pool the data of both the 1999 and 2000 FRY refugee arrival cohorts across all survey waves starting in 2001 and regress the employment

outcome on an indicator for the 1999 and 2000 arrival cohort, the years of residency, and the interaction of the two. The model also includes a full set of survey waves fixed effects, as well as the covariates age, gender, and schooling. The 7 months of additional waiting had a considerably negative short- and long-term impact on the employment of refugees. In particular, it takes up to ten years until the 1999 cohort recovers from the longer employment ban and is able to close the gap with the 2000 cohort. The red estimates suggest that these findings are very similar when we relax the linearity assumption on the interaction effect and utilize a binned interaction model (see SM Section 3.3 for details).

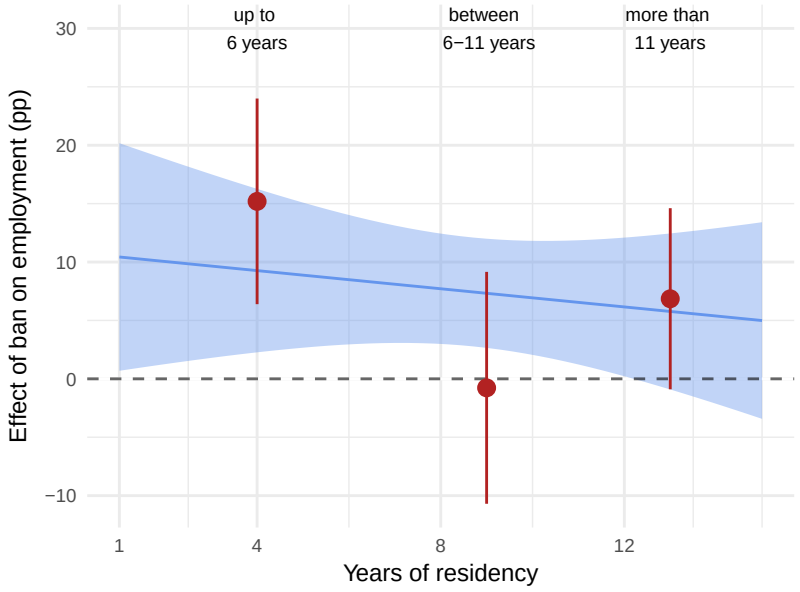

Figure 3: **Short- and long-term effects of on average 7 additional months of employment ban on refugee employment.** The Figure shows the effect of an on average 7-month longer employment ban on the probability that refugees are employed in years one to sixteen after their arrival in Germany. The blue line shows the point estimates from the linear interaction effect model with corresponding 95% confidence interval ($n = 1,645$). Red point estimates and corresponding 95% confidence intervals show the corresponding effect sizes for a binning specification that relaxes the linear interaction effect assumption and estimates the effect at the median of each tercile of the length-of-residency variable.

Discussion

In this study we have provided the first evidence on the short- and long-term effects of employment bans on the subsequent employment rates of refugees, a critical aspect of their economic integration. Leveraging a natural experiment in Germany that provided exogenous variation in the length of the employment ban imposed on refugee cohorts from FRY, we find that longer employment bans had severe negative and long-lasting consequences for subsequent employment. The average seven months of additional waiting reduced employment for up to ten years after the ban expired, considerably delaying the economic integration of refugees. Consistent with this we also find a reduction in personal income (see SM section 3.5 for details).

What mechanism might explain these effects of the longer waiting period? While the sample size limits us in answering this question definitively, additional analysis suggests that the longer waiting period considerably reduced refugees' efforts to find work when the ban finally lifted. Noting that the 1999 cohort exhibits lower employment levels than the 2000 cohort, we would, ceteris paribus, expect them to search more intensively for jobs. However, unemployed respondents who arrived in 1999 were much less likely than the 2000 cohort to search for a job during the 3 weeks before each survey wave (see SM Section 3.4 for details). This difference in search effort is consistent with the idea that the effect of longer waiting periods could be driven by reducing refugees' motivation to find work.

Our study has important implications for theory and policy. For theory, our findings are consistent with and contribute to the literature on the "scar" effects of unemployment (*18–20*). The results show that the long-term negative consequences of forced unemployment are particularly pronounced for refugees, a highly vulnerable population of individuals who often arrive in the host country without any resources and being traumatized from having fled violence and war. Furthermore, the findings point to the existence of an influential early integration win-

dow. In other words, the initial period after arrival is highly consequential for the subsequent integration trajectory of refugees, and early investments yield disproportionate integration returns (*17, 21, 22*).

Our findings also have implications for policymakers struggling with the integration of refugees in Europe. By depressing refugees' employment rates for many years after arrival, employment bans not only adversely affect the well-being of refugees, they also impose significant costs on the host country's economy. Refugees who struggle to find employment require increased public expenditures for welfare and make lower tax contributions. Our simple marginal social cost analysis indicates that reducing the employment ban for all 40,500 FRY refugees who arrived in 1999 by seven months would have led to annual savings of about 40 million Euros in unspent monthly welfare transfers for unemployed refugees and unearned tax and welfare contributions from employed refugees (see SM Section 3.7 for details). This implies total marginal costs of about 370 million Euros over the 2001 to 2009 period for FRY refugees alone. These are substantial costs for a policy with uncertain returns in light of the empirical evidence, which suggests that refugee employment has no consistent effects on depressing the wages or employment rates of natives (*23, 24*).

More generally, the negative effects of the longer employment ban point to a broader paradox in the unintended consequences of the European asylum regime. Governments need to honor their legal commitments under the Geneva Convention to provide effective humanitarian protection for refugees. Still, they tend to prohibit newly arrived asylum seekers from accessing the host country's labor market, to facilitate their removal if their asylum claims are rejected. But at the same time, these initial restrictions severely slow down the economic integration of the asylum seekers who are accepted, many of whom stay in the host country indefinitely (*25*). Therefore, even though the policy of restricting initial access might pay short term political dividends, it backfires in the long run: governments find themselves stuck with the long-term

costs of supporting refugees with low rates of economic integration and punished by the public backlash associated with these integration failures.

Materials and Methods

Our statistical analysis is based on the 2000-2014 waves of the German *Mikrozensus*, a representative annual household survey that is conducted by the Federal Statistical Office of Germany. Each year a random sample of 1% of all private households are surveyed which results in annual samples of about 800,000 individuals. Individuals in collective dwellings, such as asylum seeker shelters, are included in the target population. Selected respondents are required by law to participate in the survey. While the *Mikrozensus* provides no interpreters for the interviews, the respondents can switch to English as well as consult with other household members. The legal duty to respond lies with the respondent and therefore they are required to find the help they need to respond to the survey questions.

Our main study sample consists of immigrants who arrived in Germany from the Federal Republic of Yugoslavia as adults (at least 18 years old) in 1999 and 2000 and that have been surveyed by the *Mikrozensus*. Yugoslavian immigrants are defined as individuals who report holding or having held a citizenship from the Socialist Federal Republic of Yugoslavia, the Federal Republic of Yugoslavia, the State Union of Serbia and Montenegro, Montenegro, the Republic of Kosovo and/or the Republic of Serbia. We selected this group for two reasons. First, the large majority of immigrants who arrived from the Federal Republic of Yugoslavia during this time period entered Germany as asylum seekers. Second, this group has a sufficient number of arrivals such that we can measure the effects of the employment ban using the annual German *Mikrozensus*. While there are other data sources in Germany that contain samples of (former) asylum seekers from our study period (such as the Socio-Economic Panel or Stichprobe der Integrierten Erwerbsbiografien), the *Mikrozensus* is the only data source in Germany that has a

sufficient number of cases and contains information on immigrants' year of arrival.

Our use of the *Mikrozensus* data was governed by a data use agreement with the Federal Statistical Office and did not require informed consent and institutional review board approval, given the nature of the data. We were granted permission to analyze the data, but the data itself was not transferred to us. Instead, we developed code files based on mock data and sent these code files to the staff of the *Mikrozensus* who executed the code in a secure data facility and returns the results to us. The staff returned only those results that met the legal confidential criteria. All our replication code is posted in a dataverse at `doi:10.7910/DVN/TZCJ83`.

To estimate the effect of the difference in the waiting period that was caused by the changes in the employment ban, we compare respondents who arrived in 1999 and 2000. This information is encoded in the respondents' answer to a question about their year of arrival in Germany (the arrival month is not available in the data). We cannot distinguish between those that entered Germany as asylum seekers and entered with, for example, a work visa. Thus, all our estimates are intention-to-treat effects (ITT) which could be interpreted as lower-bound estimates of the local average treatment effects. We measure the employment status of a respondent using a variable that is consistently available across survey years and encodes if a person is employed or (in)voluntary unemployed in the survey week. Our identifying assumption is that, controlling for the covariates, the cohorts do not differ systematically in attributes other than the waiting period that also affect their employment status. We conducted a series of placebo and balance checks that lend credibility to this assumption.

Our baseline specification is a linear OLS regression model where we interact an indicator for the cohort (D_i; 1 if 2000 and 0 if 1999) with a measure of the length of residency (R_i). We additionally include a series of control variables: age (continuous), schooling (binary), gender (binary) and survey-wave fixed effects (collected in a matrix $\mathbf{X}_i$). We cluster standard errors at the household level. The linear interaction specification takes the following form:

$$y_i = \beta_0 + \beta_1 D_i + \beta_2 R_i + \beta_3 R_i \times D_i + \mathbf{X}_i' \boldsymbol{\gamma} + \epsilon_i \tag{1}$$

Our central quantity of interest is β_3 which identifies the differences in the employment rates between the arrival cohorts conditional on a given number of years of residency.

As an additional check we also use a binning specification that adds another interaction with an indicator for the length-of-residency variable's tercile (G_{ji}; short, medium, or long residency). With this specification we can estimate the conditional effect of the cohort indicator for individuals that lived in the country for a short, medium and long period of time. The advantage of this specification is that it doesn't require to assume that the interaction effect between cohort and the length of residency is linear. We again cluster standard errors on the household level for this specification. The specification of the binning estimator takes the following form:

$$y_i = \sum_{j=1}^{3} \Big(\mu_j + \alpha_j D_i + \eta_j (R_i - r_j) + \beta_j (R_i - r_j) D_i \Big) G_{ij} + \mathbf{X}_i' \boldsymbol{\gamma} + \epsilon_i \tag{2}$$

where r_j is the median length of residency in the j^{th} tercile and G_{ij} an indicator if the i^{th} observation is in the j^{th} tercile. Since $(R_i - r_j)$ equals zero when $R_i = r_j$, the coefficients α_1, α_2,α_3 directly measure the conditional marginal effect of D_i on y_i at the median in each tercile.

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Author Contributions: All authors conceived the research, designed the analyses, conducted the analyses, and wrote the manuscript.

Competing Interests: The authors declare that they have no competing interests.

Data and materials availability: Replication code for the statistical analyses, and additional data and materials are available online at `doi:10.7910/DVN/TZCJ83`.

Supplementary Materials and Methods

Background on Kosovo Refugee Crisis

The Kosovo war took place between 1998 and 1999. Before the war, the Kosovo region was part of the Federal Republic of Yugoslavia (FRY). In the mid 1990s, the Kosovo Liberation Army began an insurgency against the central government with a series of attacks against Serbian installations. In early 1998, the conflict escalated to a full scale civil war, civilian casualties increased and Kosovar Albanians and others fled from the Kosovo region. After a diplomatic solution failed, the NATO launched a comprehensive air bombardment campaign on all Yugoslavian military installations in 1999, which led to more people fleeing from the region.

Data from the UNHCR suggests that most of those who fled from the Kosovo war sought asylum in Western European countries, and in particular in Germany. Table 1 shows the number of asylum applications from the FRY in Western Europe and Germany as well as the share of Kosovo Albanians, respectively. These numbers exclude applications that re-filed after an initial rejection. Note that the data for the share of Kosovo Albanians is based on self-reports by the asylum seekers and therefore might be affected by strategic misreporting of ethnicity. In addition, it appears that the ethnicity classification was revised in 1999 which might compromise the comparability across years.

Table 2 shows the number of asylum seekers arriving in Germany from the Federal Republic of Yugoslavia as measured by German registry data (*Ausländerzentralregister*). These data have been requested by the authors from the Federal Statistical Office of Germany via its public information service. Note that these numbers do not exactly match those from the UNHCR in Table 1 since they refer to individuals and not to applications. We see that the large majority of persons from the Federal Republic of Yugoslavia who arrived during 1998 to 2001 were asylum seekers and refugees. An asylum seeker is an individual who filed an application for asylum.

	Asylum Applications from Federal Republic of Yugoslavia			
	Number of Applications		% of Applications	
Year	West Europe	Germany	Germany	Kosovo Albanians
1998	88,236	34,979	42%	88%
1999	119,060	31,450	28%	66%
2000	46,495	11,121	25%	34%
2001	29,497	7,758	28%	40%

Table 1: **Asylum Applications from the Federal Republic of Yugoslavia in Europe and Germany**. Table shows the number of initial asylum-application submissions in Europe and West Germany. The second to last column shows the share of applications filed in Germany and the last column shows the share of applications filed by Kosovo Albanians in Germany. Source: (*26–29*)

A refugee is an individual whose application for asylum has been successful and who receives some form of (temporary) protection.

Year	All Arrivals	Asylum seekers and Refugees	in %
1998	37,047	32,871	89
1999	48,506	40,501	83
2000	14,639	11,249	77
2001	10,640	6,421	60

Table 2: **Arrivals from the Federal Republic of Yugoslavia in Germany**. Table shows the annual number of arrivals from the Federal Republic of Yugoslavia as well as the number of asylum seekers and refugees (based on the legal status (*Aufenthaltsstatus*) as of December 31 of the arrival year; asylum seekers and refugees are individuals who held a *Aufenthaltsgestattung* or a *Duldung*). Source: Ausländerzentralregister Deutschland, 2015.

Labor Market Access of Asylum Seekers in Germany

Before January 1, 2001, access to the German labor market was severely restricted for asylum seekers and refugees. Asylum seekers, refugees with subsidiary protection, and most refugees with temporary asylum had to apply for a job-specific work permit (*Allgemeine Arbeitserlaubnis*) with the German labor market agencies. By law a work permit could be granted only if no

German with the same qualifications was available to fill the vacancy (*Vorrangprüfung*). The small group of refugees who were granted full asylum under the Geneva convention had full access to the labor market via a special work permit (*Besondere Arbeitserlaubnis*), but this is not relevant for our sample of FRY refugees who were not granted full asylum.

In May 1993, the Minister for Work, Norbert Blüm, issued a set of rules that stipulated that employers had to justify in great detail why they rejected an application by a German and instead preferred to hire a foreigner. In June 6, 1997 these rules were further tightened by the head of the labor market regulation unit in the Ministry for Work, Peter Clever, who instructed all local branches to not issue any work permits to asylum-applicants or anyone eligible for temporary protection who arrived in Germany after May 15, 1997. Clever's directive came to be known as the *Clever Erlass* (*30*).

In March 22, 2000, a court effectively declared the *Clever Erlass* illegal (Sozialgericht Lübeck, 2. Kammer, Az.: S 2 AL 8/99) and the government issued a new legislative regulation in September 2000, the so-called *Riester Verordnung* named after the new Minister of Labour and Social Affairs Walter Riester. The *Riester Verordnung* came into effect on December 15, 2000. Under this new regulation asylum seekers could apply for a work permit 12 months after their arrival date and those who had been granted asylum could enter the labor immediately. Importantly, the new rule was retroactively applied to asylum seekers who had arrived before December 15, 2000 (*31, 32*).

Figure 4 illustrates the effect of the regulatory switch from the *Clever Erlass* to the *Riester Verordnung* on the mandatory waiting period as a function of the arrival months between 1999 and 2000. As is evident in Figure 4, the asylum seekers who arrived in Germany in 1999 and 2000 faced very different wait times, because of the new, 12-month waiting period that went into effect on December 15, 2000. All those who entered in 2000 had to wait 12 months from their date of arrival before they were allowed to enter the German labor market. By contrast,

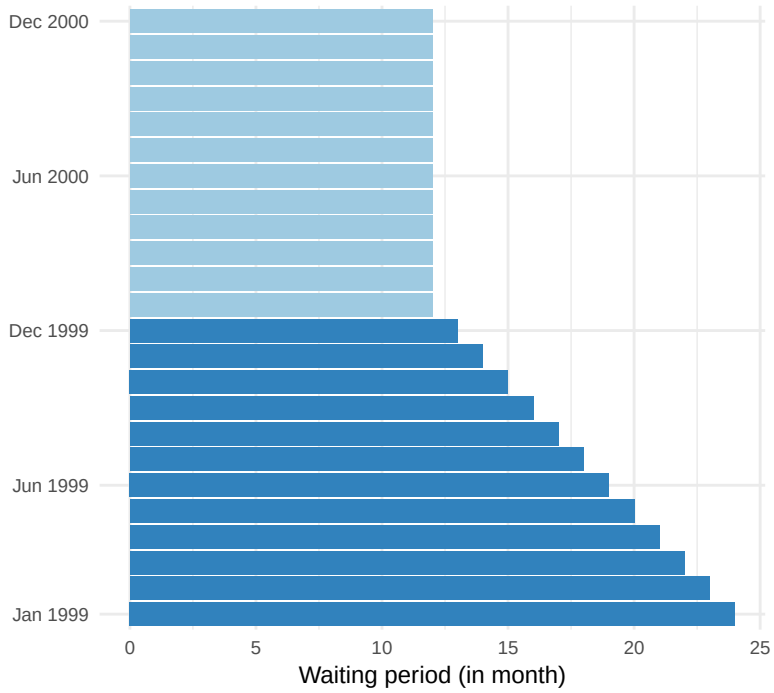

Figure 4: Illustration of the waiting period as a function of the arrival month.

those who entered in 1999 had to wait between 13 and 24 months, depending on when in 1999 they had arrived. For example, somebody who had arrived in January 1999 had to wait 24 months while somebody who had arrived in December 1999 only had to wait 13 months.

Based on monthly data about the number of asylum applications from the Federal Republic of Yugoslavia (*33,34*), we estimated that the average waiting time for the 1999 arrival cohort was about 19 months while it was 12 month for the 2000 arrival cohort. The average difference in the length of the waiting period between the 1999 and 2000 cohort was 7.1 months. Since there is a lag between the month of arrival and the month of applying for asylum (due to bureaucratic backlog), this estimate is approximate.

Variable Dictionary

Table 3 lists all the variables used in the analysis.

Variable	Description
Cohort	Year of arrival ($1999 = 0, 2000 = 1$)
Length of residency	Survey year - year of arrival
Employed	1 if employed (*Erwerbstätiger*); 0 otherwise
Age	Age
Gender	Gender
Schooling	1 if at least lower tier education (*Hauptschulabschluss*); 0 otherwise

Table 3: List of Variables

Descriptive Statistics

Table 4 shows the descriptive statistics for the main study sample and Table 5 the descriptive statistics for the male study sample.

Variable	Mean	SD
Cohort	0.37	0.48
Length of residency	8.80	4.09
Employed	0.49	0.50
Age	38.98	10.83
Gender	0.54	0.50
Schooling	0.76	0.43

Table 4: Descriptive statistics for the main study sample ($n = 1,645$).

Variable	Mean	SD
Cohort	0.38	0.49
Length of residency	8.89	4.10
Employed	0.66	0.47
Age	38.73	10.21
Schooling	0.82	0.38

Table 5: Descriptive statistics for the male study sample ($n = 749$).

Further Results

Balance Tests

Figure 5 reports covariate-by-covariate balance checks comparing the background characteristics of the 1999 and 2000 FRY refugee arrival cohorts. We find that the two cohorts are statistically indistinguishable on all background covariates. The only exception to this are some imbalances that occur in the first survey year in 2000. In this survey year, respondents arriving in 2000 tend to be younger and are less likely to have primary school education. This imbalance is most likely due to the fact that the 2000 arrival cohort is only partially covered in the 2000 *Mikrozensus* wave. This is because refugees were arriving throughout that year, but the field period for the 2000 *Mikrozensus* survey was only between May 8th, 2000 and May 14th, 2000. Therefore the 2000 *Mikrozensus* missed all of the refugees who arrived after May 14, 2000. In contrast, by 2001, all refugees of the 2000 cohort had arrived and were likely included in the target population of the *Mikrozensus* and accordingly the balance in the covariates is restored when we compare the 1999 and 2000 arrival cohorts in the 2001 survey and all following survey waves.

The implication of this incomplete coverage in 2000 for the 2000 arrival cohort is that for this particular survey year, the comparison might well be confounded, but this issue should not affect our inference for the following survey years which are the core focus of our study. In fact, all analyses except Figure 1 (the raw means) and the balance checks exclude the survey year 2000 given that our inferences about the effects of the employment ban focus on the post 2000 period.

In addition, we checked whether the cohorts differ in terms of reported health problems. Across all survey waves, none of the 2000 arrivals reported severe health problems and of the 1999 arrivals only six respondents (1%) reported severe health problems.

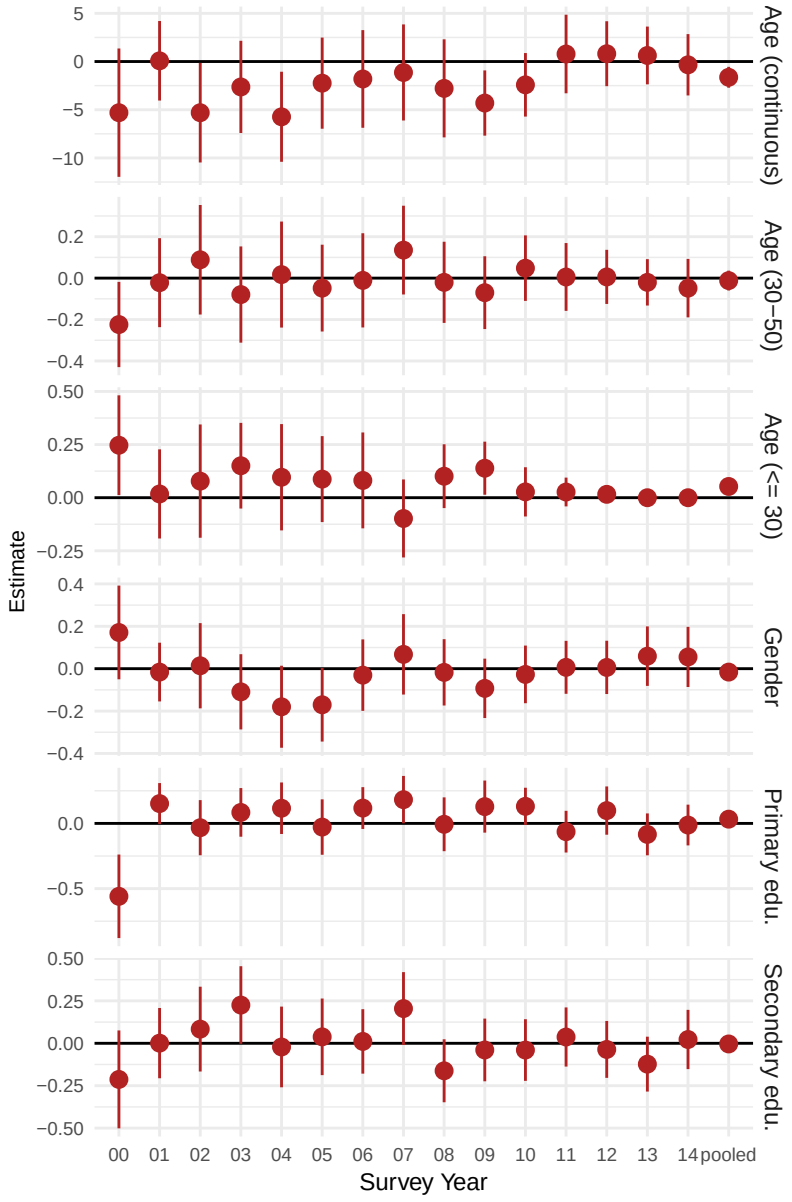

Figure 5: **Balance Checks for 1999 versus 2000 FRY Arrival Cohort**. Each panel shows the difference-in-means estimate for a specific covariate with a 95% confidence interval (based on standard errors clustered at the household) for each survey-year for the main study sample. The last estimate to the right is the difference-in-means estimate pooled across all survey waves estimated with a regression of the covariate on a cohort indicator and survey-year fixed-effects. Covariate definitions: `Age` is a continuous variable measuring the age of a respondent in a survey year, `Age (30-50)` indicates if a respondent is between 30-50 years old, `Age (<=30)` indicates if a respondents is at most 30 years old, `Primary edu.` indicates if a respondent has at least a lower tier eduction (*Haupt(Volks)schulabschluss*) and `Secondary edu` indicates if a respondents has at least a middle tier education (*Realschulabschluss*).

Attrition Check

Figure 6 examines the sampling probabilities for the 1999 and 2000 arrival cohorts in each survey wave by showing the fraction of the pooled sample that is from the 1999 arrival cohort. The sample composition remains fairly constant over time which indicates that there are no discernible differences between the 1999 and the 2000 cohort in terms of the propensity to leave Germany. If one cohort were more likely to emigrate the fraction of that cohort would be expected to decline over time as the *Mikrozensus* is based on a representative sample of the resident population.

Note again that the exception to the pattern is the 2000 survey wave, where the 2000 arrival cohort has a relatively lower sampling rate due to the incomplete coverage of asylum seekers who arrived in 2000 but after the *Mikrozensus* fieldwork for that year had been completed.

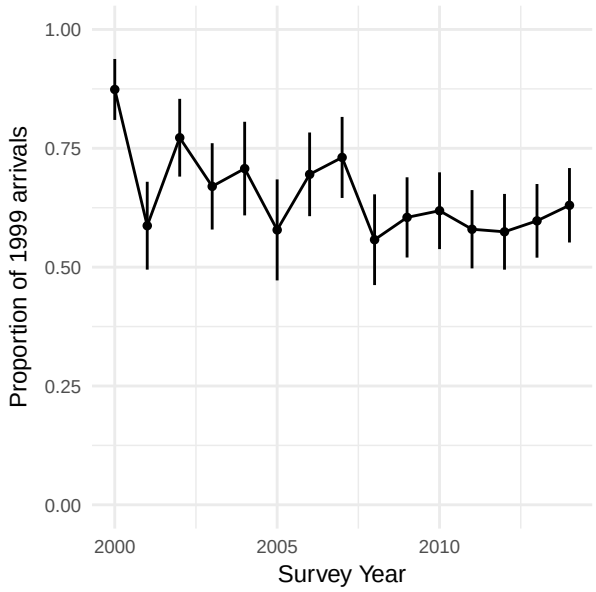

Figure 6: **Sampling Probabilities for Arrival Cohorts by Year**. Shows the proportion of 1999 arrivals in the pooled 1999/2000 arrivals *Mikrozensus* sample in each survey year for respondents from the Federal Republic of Yugoslavia.

Effect Estimates from Linear Interaction and Binning Specification

Table 6 shows the effects of the longer employment ban on refugee employment based on the linear interaction specifications. These estimates correspond to the marginal effect line shown in Figure 1 in the main text. Table 7 shows the corresponding effects from the binning specification. These estimates correspond to the dots shown in Figure 1 in the main text.

Years of Residency	Marginal Effect	Standard Error
1	0.10	0.050
3	0.10	0.040
5	0.09	0.032
7	0.08	0.026
9	0.07	0.024
11	0.07	0.027
13	0.06	0.034
15	0.05	0.043

Table 6: **Estimated Difference in Employment Rates between 1999 and 2000 cohort by Years of Residency**. Shows the estimated marginal effects of the cohort indicator for various levels of length of residency based on the linear interaction specifications as described in equation 1 ($n = 1,645$).

Years of Residency	Marginal Effect	Standard Error
4	0.15	0.045
9	-0.01	0.051
13	0.07	0.040

Table 7: **Estimated Difference in Employment Rates between 1999 and 2000 cohort by Years of Residency**. Shows the estimated marginal effects of the cohort indicator for three length of residencies based on the binning specifications as described in equation 2 ($n = 1,645$).

Effect of Employment Ban on Search Effort

Table 8 shows the effects of the employment ban on the search effort as measured by a question about whether unemployed respondents are reporting that they are actively searching for a job or not in the last 3 weeks before the survey. We regress this indicator for active search on the cohort indicator plus survey-wave fixed effects and the baseline model covariates (gender, age, schooling).

We find that unemployed respondents from the 2000 cohort that faced a shorter waiting period had about a 7 to 9 percentage point higher probability of searching for a job according to the pooled estimates. The estimates broken down by two-year intervals are more noisy and generally positive but insignificant. The sample sizes are also smaller since the models are only fitted to unemployed respondents.

	2001-03	2004-06	2007-09	2001-09	2001-09
Cohort	0.06	0.14	0.09	0.09	0.08
	(0.076)	(0.089)	(0.079)	(0.046)	(0.043)
Constant	0.23	0.14	0.18	0.22	0.74
	(0.058)	(0.055)	(0.067)	(0.056)	(0.106)
Survey FE	Yes	Yes	Yes	Yes	Yes
Covariates	No	No	No	No	Yes
N	222	149	173	544	544

Table 8: **Effect of Employment Ban on Search Effort**. Shows coefficients and household-clustered standard error in parentheses. The outcome variable is an indicator for whether unemployed respondents are reporting that they are actively searching for a job or not in the last 3 weeks before the survey. Covariates include gender, age and schooling.

Subsample Analysis: Male Respondents

Figure 7 shows the replication of Figure 3 in the main text for male respondents only. The results are fairly similar to the overall sample but less precisely estimated given the lower sample size. Table 9 shows the corresponding effect estimates from the linear interaction specification and Table 10 shows the corresponding effect estimates from the binning specification.

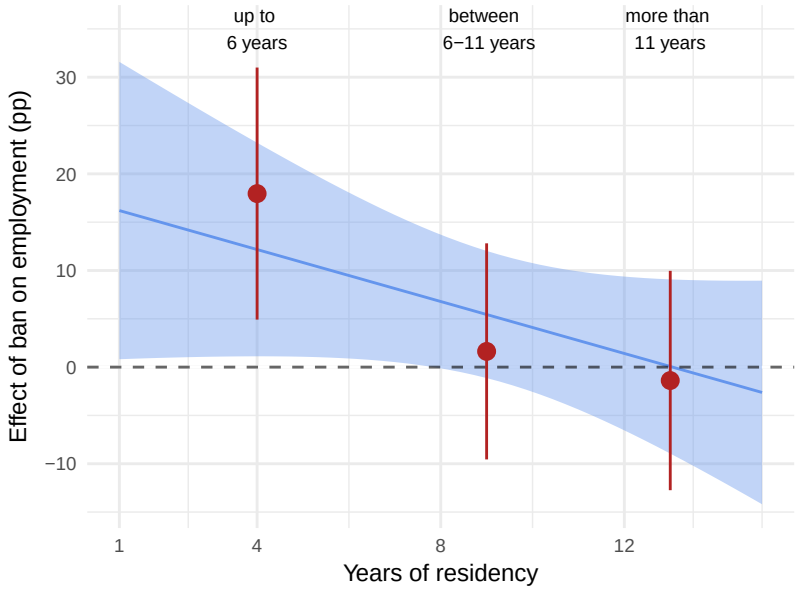

Figure 7: **Short- and long-term effects of on average 7 additional months of employment ban on refugee employment. (Male Respondents)**. The Figure shows the effect of an on average 7-month longer employment ban on the probability that refugees are employed in years one to sixteen after their arrival in Germany. The blue line shows the point estimates from the linear interaction effect model with corresponding 95% confidence interval ($n = 749$) . Red point estimates and corresponding 95% confidence intervals show the corresponding effect sizes for a binning specification that relaxes the linear interaction effect assumption and estimates the effect at the median of each tercile of the length-of-residency variable.

Years of Residency	Marginal Effect	Standard Error
1	0.16	0.078
3	0.14	0.063
5	0.11	0.050
7	0.08	0.039
9	0.05	0.034
11	0.03	0.036
13	0.00	0.046
15	-0.03	0.059

Table 9: **Estimated Difference in Employment Rates between 1999 and 2000 cohort by Years of Residency**. Shows the estimated marginal effects of the cohort indicator for various levels of length of residency based on the linear interaction specifications as described in equation 1 (male respondents only, $n = 749$).

Years of Residency	Marginal Effect	Standard Error
4	0.18	0.067
9	0.02	0.057
13	-0.01	0.058

Table 10: **Estimated Difference in Employment Rates between 1999 and 2000 cohort by Years of Residency**. Shows the estimated marginal effects of the cohort indicator for three length of residencies based on the binning specifications as described in equation 2 (male respondents only, $n = 749$).

Effect on Reported Income

Figure 8 shows the replication of Figure 3 in the main text but using reported monthly net personal income (earned income and welfare transfers as an outcome variable. Unfortunately, the *Mikrozensus* does not include a variable that allows us to consistently separate between monthly earned income and welfare transfers. Note that unemployed asylum seekers eventually become eligible for welfare benefits and therefore they will report income from these transfers which will reduce income difference between employed and unemployed respondents.

The results suggest that about four years after arrival, the 2000 arrival cohort that faced a shorter ban has on average a higher reported monthly income (about 200 Euros) than the arrival 1999 cohort. This is a 27 percentage increase over the average income for the 1999 arrivals in their fourth year after arrival. The effect is about 100 Euros larger for the men-only sample.

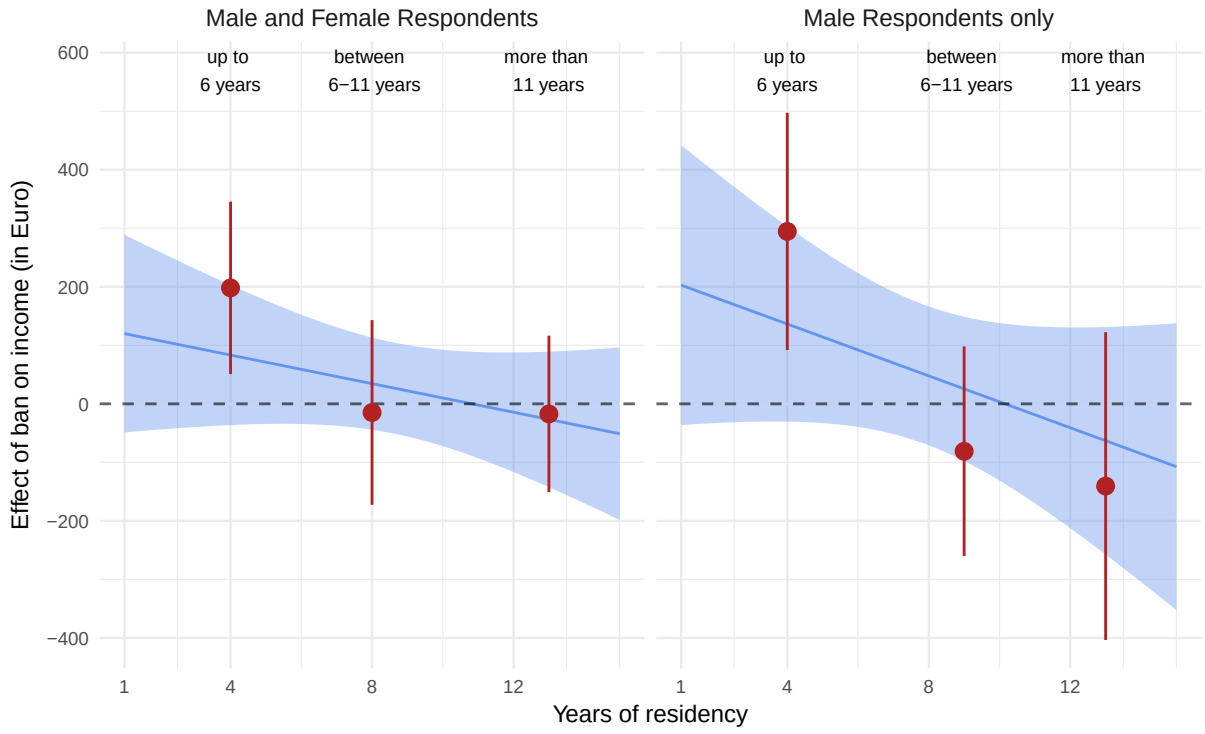

Figure 8: **Short- and long-term effects of on average 7 additional months of employment ban on monthly net personal income.** The Figure shows the effect of an on average 7-month longer employment ban on the reported monthly personal income (in Euro) in years one to sixteen after their arrival in Germany (full sample, $n = 1,645$, male respondents only, $n = 749$). The blue line shows the point estimates from the linear interaction effect model with corresponding 95% confidence interval. Red point estimates and corresponding 95% confidence intervals show the corresponding effect sizes for a binning specification that relaxes the linear interaction effect assumption and estimates the effect at the median of each tercile of the length-of-residency variable.

Social Cost Analysis

Our approach is as follows: Using the estimated effect of the employment ban, we estimate the total number of additional employed asylum seekers in the absence of the employment ban among the 40,500 Yugoslavians who arrived in Germany in 1999. Each of these additional jobs saves the tax payer welfare benefits and creates revenue in the form of additional tax contributions on a monthly basis. We estimate the size of the saved welfare benefits using the reported average monthly personal income for the group of unemployed Yugoslavians in each Mikrozensus wave. We use the the reported average monthly personal income for those that are employed and the annual standard gross tax rate for a family with 2 children to estimate the additional tax contributions. The reported personal income is the only variable that is consistently available in the Mikrozensus to make these calculation.

The calculation for the tax payer's savings in a calendar year t can be expressed as a formula as follows:

$$12 \times \underbrace{(\text{ban effect}_t \times \text{cohort size})}_{\text{additional employed}} \big(\underbrace{(\text{tax rate}_t \times \text{income}_t)}_{\text{tax contribution}_t} + \text{unspent welfare}_t \big).$$

The 40,8 Million Euro we report in the main text is the average of the estimates for each calendar year between 2001 and 2009.

To illustrate the calculation, we use the data for 2006. We estimate that in 2006, a ban reduced the chances for employment by about 10 percentage points. This implies that in the absence of a ban we would expect about an 4,000 additional employed. The average reported monthly personal income for unemployed respondents is about 450 Euros while the reported personal income (after taxes) for an employed responded is about 1,100 Euros. The gross annual tax rate for a family with 2 children is about 25%.

When we plug in these values into the formula, we obtain an estimate about how much the German tax payer could have saved in 2006:

$$12 \times (0.10 \times 40500)(((1100/(1 - 0.25)) - 1100) + 450) = 39.7 \text{ Million Euro.}$$

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