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ABSTRACT

Lift the Ban? Initial Employment Restrictions and Refugee Labour Market Outcomes*

This article investigates the medium to long-term effects on refugee labour market outcomes of the temporary employment bans being imposed in many countries on recently arrived asylum seekers. Using a newly collected dataset covering almost 30 years of employment restrictions together with individual data for refugees entering European countries between 1985 and 2012, our empirical strategy exploits the geographical and temporal variation in employment bans generated by staggered introduction and removal coupled with frequent changes at the intensive margin. We find that exposure to a ban at arrival reduces refugee employment probability in post-ban years by 15%, an impact driven primarily by lower labour market participation. These effects are not mechanical, since we exclude refugees who may still be subject to employment restrictions, are non-linear in ban length, confirming that the very first months following arrival play a key role in shaping integration prospects, and last up to 10 years post arrival. We further demonstrate that the detrimental effects of employment bans are concentrated among less educated refugees, translate into lower occupational quality, and seem not to be driven by selective migration. Our causal estimates are robust to several identification tests accounting for the potential endogeneity of employment ban policies, including placebo analysis of non-refugee migrants and an instrumental variable strategy. To illustrate the costs of these employment restrictions, we estimate a EUR 37.6 billion output loss from the bans imposed on asylum seekers who arrived in Europe during the so-called 2015 refugee crisis.

JEL Classification: F22, J61, K37

Keywords: asylum seekers, economic assimilation, asylum policies

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"When her £38 ($47) weekly allowance runs out, she sometimes begs for money. Yet, like almost all those on the asylum waiting list, she is banned from working. «I have my hands, I have my skills» she says. «I don't need the government's money. I want to be able to make my own.»"
("Britain's ban on jobs for asylum-seekers pleases nobody", The Economist, Sept 19th 2019)

1. Introduction

The recent inflow of refugees into European countries, commonly termed the European refugee crisis, has resulted in an unprecedented increase in the area's population of asylum seekers, with almost 5 million protection applications filed between 2012 and 2018 and the number of those with recognized refugee status rising from 1.3 to 2.5 million over that 7-year period. Although these formidable numbers necessarily raise the question of how to effectively and smoothly integrate such a large refugee population into host countries' labour markets and societies, the asylum policy design that likely plays a crucial role in shaping this process is as yet little understood. Hence, determining which specific features of asylum legislation can accelerate or hinder refugee integration lies at the very core of the current policy debate.

One feature of asylum legislation many western economies share is to impose temporary employment bans that prevent asylum seekers from working during the application process and, unless duration is statutorily limited, are lifted only after the applicant is granted refugee status.[1] In Europe, although such employment ban policies have become less strict over time, the vast majority of countries still implement some form of temporary ban for all asylum seekers. In 2015, at the peak of the European refugee crisis, only four European countries (Greece, Norway, Portugal and Sweden) allowed asylum seekers immediate access to their labour markets, with most other countries imposing bans of between 2 and 12 months or even an indefinite restriction in the case of Ireland and Lithuania. In the United States, asylum seekers must wait 6 months from the date of application submission before they can receive a work permit, with the current administration putting forward proposals to increase the length to one year.[2]

[1] In middle income and developing countries, bans to formal labour market access are often imposed on the refugee population throughout their entire residence in the host country (Zetter and Ruaudel, 2016; Clemens et al., 2018).
[2] See the Department of Homeland Security (DHS) Proposed Rule on "Asylum Application, Interview, and Employment Authorization for Applicants", Federal Register Vol. 84, No. 220, pp. 62374-62424 (14 November, 2019).

Although such bans may appeal to governments as means to reduce the number of asylum applications, discourage economic migrants' improper exploitation of the humanitarian channel, simplify the removal of rejected asylum seekers, and alleviate natives' concerns about labour market competition; they imply potentially sizeable costs for both affected refugees and hosting societies. First, by automatically shutting asylum seekers out of the formal labour market for the entire ban duration, governments are forfeiting the newcomers' potential contributions to output and tax revenue. Second, and perhaps more important, employment bans can have long-lasting negative consequences on such refugee outcomes as employment status, labour market participation, occupational quality and welfare reliance. Yet despite this policy relevance, there exists very little empirical evidence on employment ban effects on refugee labour market outcomes.

In this article, we assess the medium to long-term effects of employment bans on the labour market outcomes of refugees. To do so, we gathered almost 30 years of data on the presence and length of employment bans across 19 European countries and combined them with repeated cross-sectional information on refugees who arrived from 1985 onwards from the European Labour Force Survey (EULFS). In particular, our empirical approach exploits – in a difference-in-differences framework – the geographical and temporal variation in employment bans generated by the staggered introduction or removal of bans, together with frequent changes at the intensive margin of the bans' duration. These policy changes deliver variation in ban exposure both across refugee entry cohorts within the same destination country and within entry cohorts across destination countries. Using this empirical approach, we derive three major findings: First, being banned (i.e., exposed to an employment ban) at entry reduces refugee employment probability in the medium run by 8.9 p.p. or 15.2%, a negative effect explained primarily by a (9.2 p.p.) lower labour market participation rather than by a higher probability of being unemployed. Given that our sample excludes refugees who may still be subject to employment restriction, this effect, which is quantitatively equivalent to about a 4-year delay in the integration process, is in no way a mechanical one. Second, although exposure to a longer ban (13 months or more) has a slightly larger negative effect than exposure to a shorter one (up to 12 months), the difference is not particularly pronounced, implying that most detrimental effects are likely to materialize during the first months of the ban. Third, the negative effects of employment bans are highly persistent, with

negative coefficients remaining sizeable up to 10 years post arrival despite growing smaller over time.

In addition to including a battery of fixed effects to control for unobserved heterogeneity, we employ several strategies to deal with potential confounding factors and threats to identification in our design. We first address the possibility that changes in bans correlate with factors that directly affect refugees' future integration outcomes, such as economic conditions or other country-specific shocks at the time of entry. We find that a placebo analysis on a sample of non-refugee migrants that closely resemble the refugees but were not subject to the employment ban delivers a precisely estimated zero effect. We then show that when directly controlling for country-specific shocks measured at the time of arrival (economic conditions, refugee-specific shocks and political cycle), the estimated effect of bans remains stable and strongly significant. To investigate the timing of the effect and gather evidence in favour of the parallel trend assumption, we estimate dynamic specifications of our main equation and find no effect of employment bans just *before* their introduction or right *after* their removal, suggesting that the estimated impact on banned refugees is not driven by differential trends with respect to non-banned refugees. Additional estimates from a restricted sample of refugee cohorts arrived just before and just after a change in employment ban legislation further supports the credibility of our findings. We then consider the possibility of refugees sorting into employment bans – by selectively choosing timing and destination of their migration – and find no evidence in this direction. Lastly, we implement an instrumental variable strategy that exploits a 2003 EU Directive limiting the maximum employment ban duration in EU countries and obtain 2SLS estimates that fully confirm our OLS ones. Taken together, the results of these different methodologies strongly support a causal interpretation of our estimates.

To identify which mechanisms may be at work, we first note that the concentration of employment restriction's detrimental effects is among less educated refugees, suggesting that such bans mainly harm migrants whose employability in host countries is already relatively limited. Then, by considering a broader range of outcomes, we observe that banned refugees also experience lower occupational quality (lower likelihood of employment in a high skilled occupation and higher probability to have a temporary job), report lower proficiency in the host country language, and have more health issues and a greater likelihood of receiving benefits. We then assess whether part of the effect can be explained by asylum-related policies potentially introduced or modified simultaneously with the employment bans: we find that, although the overall restrictiveness of the

asylum policy at arrival seems to affect future refugees' outcomes, the estimated impact of bans is robust to the inclusion of these additional controls. Finally, we use aggregate Eurostat data on asylum seekers and refugee to assess whether employment bans influence refugee flows and fail to find evidence for this conjecture. To conclude, in the last part of the paper, we use our estimates to quantify the cost of imposing employment bans on asylum seekers who arrived in Europe during the crisis years of 2015−2016 in terms of both potential output loss for the EU economy and forgone earnings for the asylum seekers who remained as refugees. Abstracting from general equilibrium considerations and focusing only on the non-mechanical effect of employment bans, we show that the ban imposed on over 1 million new refugees may have resulted in an overall output loss of EUR 37.6 billion over a 8-year period, equivalent to about EUR 4,100 per banned refugee per year.

Our analysis and findings contribute to the literature on the determinants of refugee economic integration in host countries (Cortes, 2004; Bratsberg et al., 2014; Bevelander and Pendakur, 2014; Ruiz and Vargas-Silva, 2018; Sarvimäki, 2017; Fasani et al., 2018).[3] In particular, our paper is a valuable addition to the small but growing literature on asylum policies and their effects on refugee labour market integration. Previous studies have analysed the effect of specific features of the asylum system process such as geographic dispersal programs (Edin, 2004; Damm, 2009; Beaman, 2012; Fasani et al., 2018; Dagnelie et al., 2019), asylum application processing speed (Hainmueller et al., 2016; Hvidtfeldt et al., 2018; Bertoli et al., 2020) or generosity of income support for refugees (LoPalo, 2019). Other studies concentrate on the effects of policies specifically designed to improve refugee labour market outcomes, such as job search assistance programs (Battisti et al., 2019) or language training (Clausen et al., 2009; Lochmann et al., 2019). Instead, very little is yet known about the effects of temporary employment bans, despite their diffusion. We fill this gap by exploring the medium and long-run effects of a multiplicity of policy changes – on both the intensive and extensive margins of employment bans – that occurred over almost three decades in a large number of European countries and affected refugees from a highly diverse set of origin countries. We also strive to identify the underlying mechanisms by exploring heterogeneity in the effects and studying the impact on a broad set of outcomes. The detrimental impact of bans on employment we identify confirms the only piece of direct evidence on the matter available so far;

[3] For recent reviews of this literature see Becker and Ferrara (2019) and Brell et al. (2020).

namely Marbach et al.'s (2018) analysis of the impact of a ban length reduction on the employment probability of former Yugoslavian refugees in Germany.[4]

Our analysis also contributes to the literature on the importance of initial conditions at entry in the labour market, one strand of which focuses on how labour market conditions at arrival affect future outcomes for immigrants (Åslund and Rooth, 2007; Azlor et al., 2020). In terms of market entry conditions in general, the existing evidence for natives strongly suggests that graduating in a recession may have lasting negative consequences on earnings and other labour market outcomes (Kahn, 2010; Oreopoulos and von Wachter, 2012; Altonji et al., 2016). Nor are these negative outcomes limited to college graduates: Schwandt and von Wachter (2019) document especially large effects among less advantaged entrants, a population more similar to that studied here. The findings in this literature thus resonate closely with our observations on the persistency of this initial scarring effect and its stronger impact on less educated workers.

The remainder of the paper is structured as follows. Section 2 explains the employment bans imposed on asylum seekers in the European Union and discusses their potential impact on refugee labour market outcomes. Section 3 describes the data used in the empirical analysis, and section 4 outlines the empirical strategy. Section 5 then reports the main estimation results, with additional identification and robustness tests discussed in section 6. Section 7 explores possible channels for and interpretations of the main effect, after which section 8 discusses some concluding remarks.

2. Employment Bans in Europe

Individuals seeking humanitarian protection in the EU typically need to physically arrive in a member country in order to file an application for refugee status.[5] While waiting for case evaluation, asylum seekers remain in a legal limbo whose main element of uncertainty is whether the host country will eventually decide to allow them to stay or not. The regulations governing

[4] Marbach et al. (2018) study the effect of a specific episode of reduction in employment ban length (from indefinite to 12 months) implemented in Germany in 2000. They compare employment trajectories of refugees from the former Federal Republic of Yugoslavia arrived in year 2000 with those of their conational arrived in 1999 and find negative employment effects of size comparable to ours. Indirect evidence in the same direction is provided by Couttenier et al. (2019), who document a lower refugee propensity for crime engagement in Swiss cantons that allow asylum seekers immediate labour market access than in those that impose restrictions. Similarly, Slotwinski et al. (2019), by exploiting variation across Swiss cantons on an index of this labour market openness (which captures employment ban duration among other factors), find an association between a more inclusive regime and higher market participation.

[5] Resettlement policies that first screen potential refugees in source or transit countries and then relocate them in destination countries are the main entry channel for forced migrants in countries such as Australia, Canada and the US but are quantitatively marginal in most European countries.

asylum seeker status and treatment, however, are highly heterogeneous, both across country and over time.[6] One important aspect of this legislative heterogeneity is whether asylum seekers are allowed immediate access to the host country labour market or are subject to employment bans. Although employment bans have been a pervasive feature of asylum policies in European countries for decades, they are not without controversy. The European Union, for instance, has repeatedly requested that member states reduce the statutory length of the bans, issuing Directives in 2003 and 2013 that limited the maximum duration to 12 and then 9 months, respectively (see section 6.2). Nor do bans appear to have wide support among the general public: in a survey conducted in 21 European countries, a majority of respondents (64%) agreed or strongly agreed that asylum seekers should be granted the right to work (see Figure 1),[7] with favourability falling below 50% in only two countries (the United Kingdom and Hungary) and reaching over 70% in seven others (the Netherlands, Switzerland, Finland, Ireland, Denmark, Luxembourg and Sweden). As regards the question of whether this heterogeneity is somehow reflected in the employment bans in place in different European countries, Appendix Figure A1 reveals no correlation between citizen support for asylum seekers working and the presence and duration of employment bans in their countries of residence (measured in the same survey year).

What, then, are the pros and cons of imposing employment bans that prevent asylum seekers from taking formal jobs?[8] Even if informal employment opportunities are available, these bans might confine asylum seekers to low pay, low productivity and highly temporary positions, as the literature on undocumented immigrant suggests (Kossoudji and Cobb-Clark, 2002; Kaushal, 2006; Amuedo-Dorantes et al., 2007; Borjas and Cassidy, 2019). Hence, in the short run, such restrictions imply obvious costs of human capital underutilization, as well as output loss and forgone income tax and social security payments. They also oblige host countries to provide income support for forcedly idle workers in order to prevent the destitution that may lead to social exclusion and deviant behaviour (Bell et al., 2013, Couttenier et al., 2019). Yet despite these initial costs, several

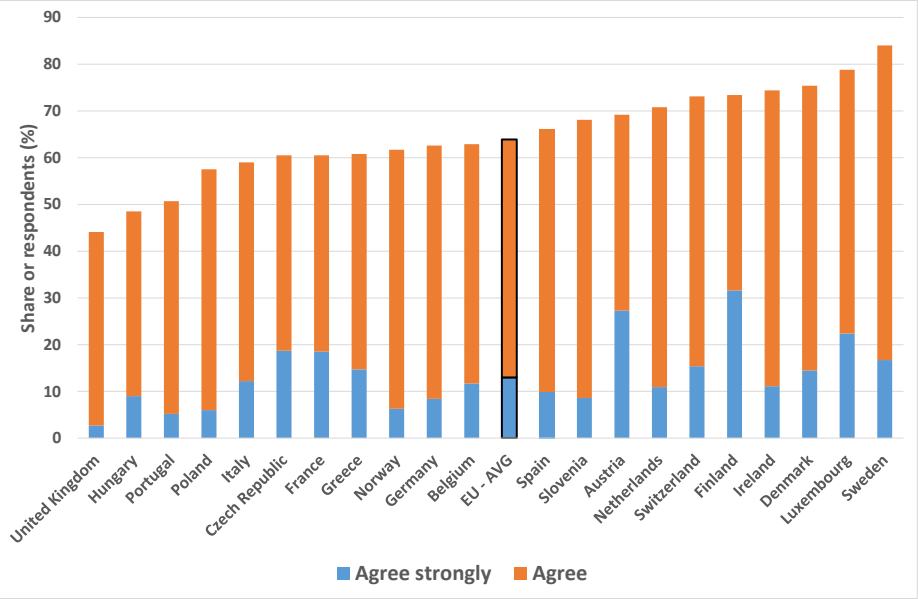
[6] The treatment of individuals with recognized refugee status, in contrast, is fairly homogenous across EU countries: having been granted the right to stay in the host country, their status and legal rights are generally similar to those of long-term legal migrants.

[7] To the best of our knowledge, the 2002 European Social Survey analysed here is the most recent survey asking European citizens about employment bans for asylum seekers.

[8] In general, the prohibition to work is clearly stated on asylum seekers' documents (see, e.g., the UK ID card for asylum seekers in Appendix Figure A 2), immediately informing prospective employers about the status of the job applicant.

considerations may prompt receiving countries to limit access to their labour markets. The first is a belief that such restrictions may reduce the number of asylum applications by both decreasing the county's attractiveness for legitimate refugees and discouraging economic migrants from using the asylum system as a backdoor entry channel. The second is a hope that bans may alleviate native concerns about labour market competition and reduce the electoral backlash against immigration (Marbach and Hangartner, 2019). A final incentive is a host country government's desire to explicitly hold back applicants' socio-economic integration while their cases for international protection are assessed in order to minimize obstacles to deportation if claims are rejected.[9]

Figure 1 – Should asylum seekers be granted the right to work?



Notes. The figure shows, for each country, the share of respondents agreeing or agreeing strongly with the statement that asylum seekers should be granted the right to work. The EU-AVG column reports the mean value across all countries. Source: European Social Survey, 2002.

The conclusions of this short-run cost-benefit analysis, however, may change substantially if the initial phase of idleness produces lasting negative effects on asylum seekers' outcomes and behaviour. Yet at present, not only the direction of these effects but even their very existence remains theoretically unclear. Whereas a period of forced inactivity may help refugees recover

[9] In its proposal to extend the duration of the ban in the US (see footnote 2), for instance, the DHS argues that it *"…seeks to reduce incentives for aliens to file frivolous, fraudulent, or otherwise non-meritorious asylum applications to obtain employment authorization (…) or other non-asylum-based forms of relief such as cancellation of removal, and to discourage illegal entry into the United States. DHS also seeks to reduce incentives for aliens to intentionally delay asylum proceedings in order to extend the period of employment authorization…"* (p. 62375).

from their traumatic experiences, invest in host country-specific human capital and escape pressure to accept just any job right after arrival (avoiding lock-in in low-paying occupations), being barred from working is not the same as being granted the opportunity of not working for some time. Bans may trigger a variety of detrimental mechanisms. For example, by imposing idleness on those who have already been forced into extended periods of inactivity by conflict or dangerous journeys to a safe haven, bans may further depreciate their human capital while also enhancing their perceived uncertainty about a future in the host country. Both could potentially harm refugees' motivation and incentives to make early and effective investment in acquiring host country-specific human capital (Adda et al., 2016). Exclusion from the labour market may also severely reduce interactions with natives, slowing down host country language acquisition (Chiswick and Miller, 2015) - a major determinant of economic integration, while hindering the development of networks that may help assimilation (Dustmann et al., 2016) and increasing the sense of isolation. In fact, as is well documented in the unemployment and mental health literature (Schaller and Stevens, 2015; Farrè et al., 2018), not only does idleness in individuals who have suffered trauma not necessarily aid recovery, it may actually worsen mental well-being by reducing their sense of purpose and self-esteem. At the same time, being placed on welfare immediately upon arrival may generate a culture of welfare reliance (Hansen and Lofstrom, 2003 and 2011), leading to lower motivation to engage in the labour market. The aim of our subsequent analysis is thus to empirically document the existence and quantify the magnitude and persistence of these medium to long-term effects of employment bans.

3. Data and Descriptive Statistics

Our analysis is based primarily on two datasets: the European Labour Force Survey (EULFS), distributed by Eurostat, and the EMPBAN database, which we assembled specifically for this project to map the presence and length of employment bans across European countries since 1985. The EULFS is a large household survey of individuals aged 15 and over in all 28 EU member states, certain candidate countries and three countries of the European Free Trade Association (Iceland, Norway and Switzerland), which routinely collects information on respondent demographics and labour market outcomes.[10] In this paper, we use the two *ad hoc* modules on

[10] The EULFS has only limited information on wages and earnings. No information is available in 2008, whereas the 2014 module includes information on an individual's decile in the national distribution of monthly take-home pay. No income data are available in any year for the Czech Republic, France, Hungary, Norway and Sweden.

migration collected in 2008 and 2014, which provide a rich set of additional variables on migrant experience in the host country. Of particular relevance for our study is the inclusion of a *main reason for migration* question asked of all non-natives who arrived in the country of residence at age 15 or older, with response options being employment, study, international protection or family reunification. By defining all foreign-born individuals as non-natives and designating all those selecting 'international protection' (other options) as *refugees* (*other migrants*), we are able to clearly differentiate asylum seekers from other arrivals.[11]

To assemble the EMPBAN data set, we first used legislative references and policy documents to construct a precise history of employment ban presence and duration in all EU countries and Norway for every year since 1985 (whenever possible) and then had several country experts validate the result while also complementing the information as needed. We then merged the EMPBAN and EULFS datasets using information on the year of arrival in the host country to assign data on ban presence and its duration to each refugee at the time of entering the country to seek humanitarian protection.[12] Being interested in the medium to long-run effects of ban exposure rather than its mechanical incapacitation, we exclude individuals for whom the employment bans may still be active by restricting the sample to refugees who have been in the host country for at least 2 years. Further, we focus on the 25−64 age group in order to exclude individuals who may not yet have completed their education and those not of working age. Our final estimating sample contains 4,242 refugees, interviewed in 19 European countries,[13] who arrived in Europe between 1985 and 2012 and for whom data on key outcomes, control variables and employment ban at arrival are available. In addition, we construct a secondary dataset of approximately 49 thousand non-refugee migrants from the same origin areas as the refugees that we use to perform placebo tests.

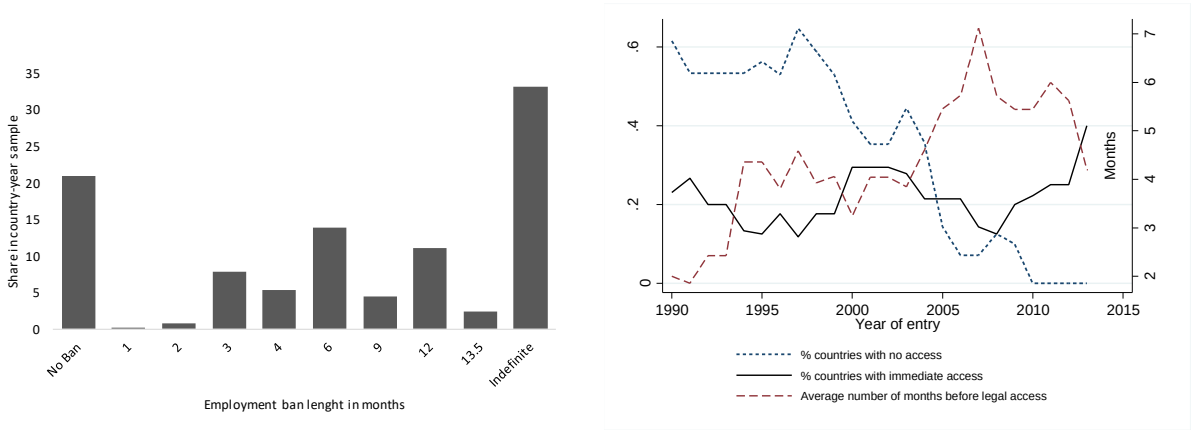
As Appendix Table A 1 shows, almost 60% of the refugees in our sample are male, with an age breakdown of 37% aged 25-39, 50% aged 40-54 and the remaining 13% aged 55-64. Approximately 24% have tertiary education, with upper and lower education accounting for 37 and 39%, respectively. As regards entry cohort, 36.5% entered the country between 1985 and 1994,

[11] Except for Germany, where non-natives are defined as "foreign nationals". When birth country information is missing, we use the parents' country of origin to determine the individual's non-native status.
[12] Further details about each dataset and the merging procedure are provided in Appendix A1.1.
[13] The countries are: Austria, Belgium, Croatia, Cyprus, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Luxembourg, Netherlands, Norway, Portugal, Slovenia, Spain, Sweden and United Kingdom.

48% between 1995 and 2003, and 15% between 2004 and 2012. The largest group of refugees originates from EU New Member States and other non-EU countries (35.5%), followed by North Africa and the Middle East (27%), other African countries (18.9%), South-East Asia (16.2%) and Latin America (2.4%). As regards the primary outcomes for our empirical analysis, the average employment rate is 58.5% versus an unemployment rate of 17.5%, with a labour market participation rate of 71%.[14]

Figure 2 – Temporary employment bans for asylum seekers in Europe

A. Distribution of bans in country-year sample *B. Evolution of bans over time*



Notes. In Figure A, shows the distribution of the ban length variable (measured in months) in the country-year dataset used for the empirical analysis. The category "no ban" indicates immediate access to labour market granted at arrival, while "indefinite" indicates bans lasting until the individual is granted refugee status. Figure B reports for each year since 1990 the share of countries with an indefinite employment ban (thin dotted line, left axis), the share of countries without an employment ban (black solid line, left axis) and the average duration of the ban for countries in which ban was not unlimited (red scattered line, right axis).

Figure 2, Panel A shows the distribution of employment ban lengths (measured in months) for our country-entry cohort dataset, revealing that asylum seekers in 21% of the country-year observations are allowed immediate access to the labour market, while those in 33% are subject to indefinite bans. The remainder are limited by bans of statutory durations that vary from 1 to 13.5 months but cluster around 6 and 12 months. Panel B of the figure then graphs the variation in labour market restrictions in European countries over the study period, revealing that whereas around 20% of the countries grant asylum seekers the immediate right to work (solid line) in 1990, this number fluctuates between 12% and 29% up until 2008 when it begins rising to an eventual 40% in 2013. At the other extreme, the share of countries implementing an indefinite employment

[14] For reference, the corresponding values for other migrants in the EULFS survey are 69, 80 and 15% (see Fasani et al., 2018, for a detailed discussion of the labour market gap between refugees and other migrants).

ban (dotted line) starts at almost 60% in 1990 then steadily decreases over time, reaching zero from 2010 onwards. The average number of months an asylum seeker must wait before accessing the labour market (dashed line), which inherently excludes countries with indefinite bans, fluctuates between a minimum of 2 and a maximum of 7.[15]

Taken together, the above numbers highlight a clear trend in EU countries from indefinite to time-limited bans with a slight increase in these latter's duration. One important driver of this trend is past attempts to homogenize the treatment of asylum seekers at the EU level through specific Directives, as mentioned above. On the other hand, a crucial feature for our research design is the considerable within-country variation over time as countries switch ban policies on and off and vary their intensity (see Appendix Figure A 3 for details).

4. Empirical Strategy

To identify the effect of employment bans on refugee labour market outcomes, we exploit both across- and within-country variation in the ban exposure of different refugee cohorts arriving in Europe between 1985 and 2012. More specifically, we estimate difference-in-differences (DiD) regressions comparing the outcomes of refugees who migrated to destination countries having a (longer) ban in place at the time of arrival with those of refugees exposed to no (a shorter) restriction. With respect to a standard DiD, our setting includes both staggered ban introductions and staggered ban removals together with changes at the intensive margin in both directions (i.e. extensions and shortening of ban duration), all of which our empirical analysis exploits. We estimate the following specification:

$$y_{idTt} = \alpha_0 + \alpha_1 BAN_{dT} + \mu_{dt} + \lambda_T + \gamma X_{it} + \epsilon_{idTt} \qquad \text{(eq. 1)}$$

where y_{idTt} are labour market outcomes (e.g. employment, participation, unemployment) of refugee i, residing in country d, who migrated in time T and was interviewed in survey year t. We regress these outcomes on alternative measures of ban exposure in destination country d at arrival time T (BAN_{dT}). In particular, we use dummy variables for ban presence or ban duration above (below) certain thresholds and continuous variables such as ban duration in months in order to

[15] Because employment ban duration is definable in months only for countries with no indefinite ban, countries enter the sample used to calculate average ban length as they switch from the indefinite to some definite category. As a result, the reduction in the share of countries imposing an indefinite ban coincides with an increase in average ban length across European countries.

explore both the extensive and the intensive margin of employment restrictions. Our regressions further include a set of destination country fixed effects interacted with interview year dummies (μ_{dt}) that capture any unobserved country-specific conditions (e.g. business cycle, attitudes toward refugees) that may affect the labour market outcomes of all refugees who reside in a given country – and were interviewed in the same survey year – and may also correlate with employment ban presence, timing of ban introduction or ban duration. We also control for arrival cohort fixed effects (λ_T) to absorb unobservable factors common to all asylum seekers arriving in the EU during the same period, capturing, for instance, the lower socio-economic integration of more recently arrived cohorts. Finally, we condition on a vector X_{it} of individual characteristics that includes dummy variables for gender, 5-year age groups and three educational levels. This vector also includes indicators for origin areas, which are allowed to vary by arrival cohort in our most complete specification, to capture any common feature (e.g. culture, ethnicity, discrimination in European labour markets) shared by individuals born in the same area and who arrived in Europe at the same time. Lastly, we designate ϵ_{idTt} as an idiosyncratic shock. We cluster standard errors at the destination country level and show that results are robust to the use of wild cluster bootstrapping.

The main identifying assumption in our empirical design is that − conditional on our set of fixed effects, interactions and other controls – changes in employment bans are orthogonal to the error term. In other words, we assume that had the policy not been in place, the outcomes of treated refugees would have been the same as those of non-treated refugees. This is a reasonable assumption insofar as refugees arriving in a country before and after the introduction (or removal) of a ban are exposed to similar conditions – other than the change in employment restrictions – and do not differ in any systematic way. One obvious threat to identification in equation (1) is the possibility that changes in bans correlate with factors that directly affect refugee integration outcomes. Our double fixed effects specification takes care of these factors only as long as they are country specific but time invariant or time specific but common to all countries in our sample. However, if employment bans are modified in response to a country-specific shock − for instance, if bans are introduced (or lengthened) when an economy enters recession − we may observe that refugees facing more stringent labour market restrictions at their arrival subsequently perform worse than those who did not. This spurious negative correlation between the two variables might be generated by the scarring effects of migration during an economic downturn even in the absence

of a causal relation between bans and refugee outcomes.[16] To tackle concerns related to the potential endogeneity of ban policies to country-specific shocks, we first perform a falsification exercise estimating equation (1) for a sample of non-refugee migrants matched with placebo employment bans which were in place for asylum seekers at the time of their arrival in Europe. In the absence of spurious correlations, this falsification test should indicate no ban effect on the non-refugee migrants who, although comparable to refugees, were not subject to the employment restrictions under study. In fact, the ban effect for non-refugee migrants is a precisely estimated zero across the board.[17] In a second set of tests, we return to our main refugee sample and control directly for country-specific shocks (economic, political and refugee-related ones) measured at the time of arrival. The inclusion of these additional controls does not affect the estimated employment ban impact, lending further support to the credibility of our identification strategy.

We then turn to the evidence in favour of the parallel trends assumption in our DiD regressions. We exploit the heterogeneity in introductions and removals of employment bans to study whether the timing of the effects we estimate reflects closely that of the underlying changes in bans. Estimates from dynamic specifications of equation (1) show no evidence of any effect of employment bans just *before* their introduction or right *after* their removal, suggesting that the estimated impact on *banned* refugees is unlikely to be driven by differential trends in labour market outcomes with respect to *non-banned* refugees. Further, estimates obtained from a restricted sample of refugee cohorts who entered just before and just after a change in employment bans show that the effect materializes (vanishes) right after the introduction (removal) of the ban. An additional empirical issue we directly consider in our analysis is the possibility that unobserved individual characteristics affecting refugee labour market integration might also correlate with employment restrictions at arrival, which would be the case if asylum-seekers self-selected into destination countries and time of migration based on the presence and duration of these bans. Although this concern may be of limited relevance in light of the forced nature of their migration

[16] Spurious correlations may be similarly generated by unobserved political or refugee migration shocks. For instance, an increase in hostility against migrants may lead to both tightening of labour market restrictions and worse labour market performance in both migrants and refugees (who face increased discrimination). Alternatively, a sizeable increase in the number of those seeking humanitarian protection may induce host countries to impose more restrictions and, at the same time, negatively affect future cohorts of asylum seekers, who may receive less resources and face more competition in the labour market from previous (large) cohorts.

[17] To further investigate this aspect we also estimate triple DiD regressions on the pooled sample of refugees and other migrants.

and of the numerous hurdles refugees face in selecting their destinations, we empirically assess the extent of sorting in our data. Using information on mostly pre-determined refugees' characteristics, among which educational level, we observe no conditional correlation between those characteristics and employment restrictions faced at arrival, leaving little role for sorting to explain our findings. Finally, we develop an instrumental variable strategy that exploits the exogenous variation in restrictions generated by an EU Directive that in 2003 imposed a 12 month cap to employment ban duration. The 2SLS estimates we obtain fully confirm the detrimental effect of employment bans on refugees' outcomes, providing further evidence in favour of a causal interpretation of our estimates.

After discussing our main results (section 5) and presenting our findings for each of the above identification issues (section 6), we extend our empirical analysis by exploring mechanisms and heterogeneity in the effects, impacts on refugee outcomes outside the labour market, the role of asylum policies other than employment ban and the elasticity of refugee flows to bans (section 7).

5. Main Results

5.1. Baseline Estimates and Placebo Tests

Baseline results. Table 1 reports the results for the employment ban effect on refugees obtained by estimating equation (1) for three different labour market outcomes: employment (Panel A1), participation (Panel B1) and unemployment (Panel C1). The main covariate of interest (*Employment Ban*) is a dummy equal to one if a work restriction was in place at the time of arrival in the destination country. Column 1 reports the estimates from our baseline DiD specification (which interacts destination country fixed effects with interview year dummies and arrival cohort fixed effects), after which each column stepwise adds in the following controls: individual characteristics (column 2), origin area fixed effects (column 3) and origin area-arrival cohort fixed effects (column 4). The estimates in Panel A1 indicate a considerable and statistically significant negative effect of employment bans on refugee employment probability, with the most restrictive specification (column 4) associating exposure to an employment ban at entry with an 8.9 p.p. lower employment probability in subsequent years, corresponding to a 15.2% decrease relative to the mean (58.5%). These effects are equivalent to slowing down the integration process by about four

years.[18] The negative impact on employment is primarily explained by the lower labour market participation of *banned* refugees rather than by a higher unemployment probability. Indeed, the estimated coefficients in the participation equation (Panel B1, column 4) imply a 9.2 p.p.(12.9%) reduction in participation probability for affected refugees, while the estimated effect on unemployment (Panel C1), albeit positive, is small and imprecisely estimated. These results strongly suggest that the primary effect of bans is to discourage refugees from actively searching for jobs, which, through a variety of mechanisms explored in section 7, results in a lower probability of being employed. Note that since the sample excludes asylum seekers still subject to the ban, these latter cannot be mechanically driving our estimates (cf. section 3).

Placebo test on non-refugee migrants. As discussed in section 4, country-specific shocks may simultaneously affect refugee outcomes and employment bans, making the latter endogenous. To address this concern, we run placebo regressions on non-refugee migrants exposed to the same host country shocks as refugees but not subject to the bans.[19] That is, we match non-refugee migrants from the same set of origin areas and entry cohorts as our main refugee sample with the asylum seeker employment bans in place at entry into the country. Results from estimation of our equation (1) (reported in Table 1, Panels A2, B2 and C2), indicate that the employment ban effect on non-refugee migrants for all outcomes and in all specifications is a precisely estimated zero.[20] These results from placebo regressions suggest the absence of spurious correlations and provide further support for causally interpreting our main estimates (section 6 presents further evidence in favour of the plausibility of our identifying assumptions).

[18] Fasani et al. (2018) find that the refugee-immigrant gap in employment probability decreases by about 2 p.p. for every extra year spent in the country.

[19] If refugees and other immigrants are substitutes in production, employment bans imposed on the former may improve (at least in the short term) the labour market outcomes of the latter by shielding them from competition. The likelihood of this effect, however, although theoretically plausible, is empirically negligible given the size of the asylum seeker population relative to other migrants.

[20] Our main findings are similarly confirmed by estimating a triple DiD that uses non-refugee migrants as the control group (see Appendix Table A 2).

Table 1 – Effect of ban on labour market outcomes: baseline and placebo estimates

	(1)	(2)	(3)	(4)
		Employment		
Panel A1: Refugees				
Employment Ban	-0.125***	-0.113***	-0.124***	-0.089***
	(0.035)	(0.035)	(0.037)	(0.031)
Observations		4,242		
Panel A2: Placebo on other migrants				
Employment Ban	-0.011	-0.018	-0.009	0.003
	(0.030)	(0.028)	(0.023)	(0.024)
Observations		49,100		
		Participation		
Panel B1: Refugees				
Employment Ban	-0.128**	-0.106***	-0.114**	-0.092***
	(0.045)	(0.036)	(0.041)	(0.026)
Observations		4,242		
Panel B2: Placebo on other migrants				
Employment Ban	-0.013	-0.019	-0.011	0.003
	(0.027)	(0.026)	(0.021)	(0.022)
Observations		49,100		
		Unemployment		
Panel C1: Refugees				
Employment Ban	0.010	0.022	0.027	0.009
	(0.042)	(0.042)	(0.044)	(0.056)
Observations		3,112		
Panel C2: Placebo on other migrants				
Employment Ban	0.002	0.004	0.001	-0.001
	(0.010)	(0.009)	(0.008)	(0.007)
Observations		38,634		
Host country x Year FE	X	X	X	X
Entry cohort FE	X	X	X	
Individual characteristics		X	X	X
Origin area FE			X	
Entry cohort x Origin area FE				X

Notes. The table reports estimates of the effects of employment ban at the time of arrival on refugees' (A1, B1, C1) and other non-EU migrants (A2, B2, C2) labour market outcomes. Estimates are from linear probability models with employment (panel A), participation (Panel B) or unemployment (Panel C) dummies as outcome variables. Employment Ban is a dummy equal to one for an employment ban (the length of employment ban larger than zero) being in place in the destination country at the refugee's time of arrival. Individual characteristics are dummies for male, five–year age groups and three educational levels. The sample comprises of refugees and other non-EU migrants aged 25-64, arrived between 1985 and 2012, who spent at least two years in the host country. In Panel C the sample is constrained to refugees who are active in the labour market. Standard errors (in parentheses) are clustered by destination country: *** p<0.01, ** p<0.05, * p<0.1

5.2. Intensive Margin, Persistency and Robustness Checks

Intensive margin and non-linearity. To determine whether an effect exists at the intensive margin of bans − that is, whether their duration has any impact on outcomes − we first split our employment ban dummy into two indicators, one short ban (up to 12 months) and one long ban (13 months or more, including indefinite). The results for our last specification (Table 2, column 4) suggest that exposure to a longer ban has a slightly greater negative effect: 8% and 26% larger for employment (Panel A) and participation (Panel B), respectively. However, the null hypothesis that the coefficients of the two treatment indicators are the same is never rejected across specifications (see last row in each panel).[21] As an alternative, we use a continuous measure for the employment bans, defining BAN_{dT} as the ban duration in months and coding indefinite bans as equal to 30 months. In Appendix Table A 3, we first include only the number of months variable (column 1) and then add in number of months squared to allow for non-linearities.[22] For both employment (panel A) and participation (panel B), our estimates reveal a U-shaped relation between labour market outcomes and ban duration, with each additional month of employment ban reducing employment and participation probabilities by approximately 1 p.p.. Implicit in the positive coefficient estimated on the squared term is that this detrimental effect peaks after 1-1.5 years and declines afterwards.

Taken together, these results suggest that although employment bans' detrimental effects increase with ban duration in the first months post arrival, once asylum seekers have been kept out of the labour market for one year, additional months do not induce further negative consequences. These findings in turn imply not only that most detrimental effects materialize during the first months of the ban, but that the most relevant margin at which a ban policy operates is the extensive rather than the intensive. This observation is consistent with the idea that immigrants' experience upon arrival is a key determinant of future integration paths, affecting their (under)investment in host

[21] Employment bans are also frequently accompanied by further restrictions that limit asylum seekers' labour market access even after ban expiration, thereby reducing the difference between short and long bans. For instance, the UK only allows their employment in so-called shortage occupations, Germany conditions the issuance of a work permit on several checks on remuneration and working conditions and the Netherlands only allows asylum seekers to work for a maximum of 24 weeks per year.

[22] Appendix Table A 3 reports our main results for indefinite bans equal to 30 months (columns 3-4 and 7-9) while also showing the robustness of the estimates to alternative choices (24 months in columns 1-2; 36 months in columns 5-6). According to data on waiting times in the EU, asylum seekers rarely wait more than 2-3 years for application assessment, suggesting this interval as a reasonable choice for our conversion.

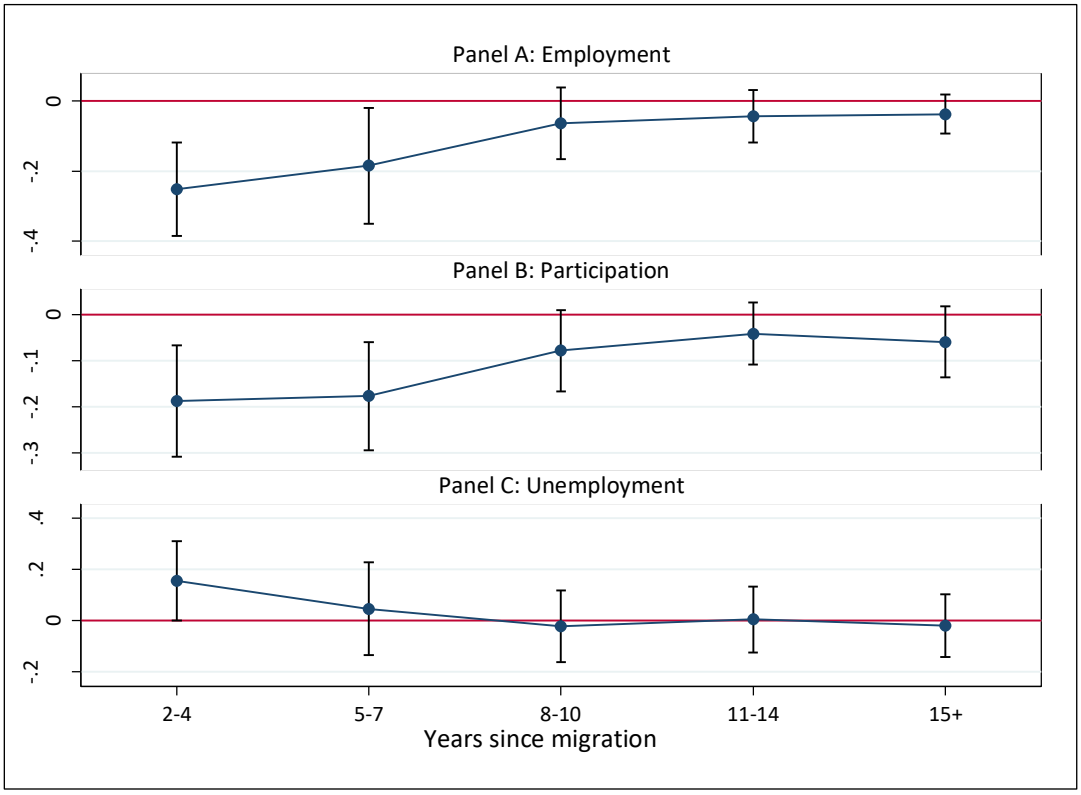
country-specific human capital, motivation to engage in the labour market and welfare reliance (see section 2).

Table 2 – Effect of ban on labour market outcomes: shorter vs longer bans

	(1)	(2)	(3)	(4)
	Panel A: Employment			
Empl. Ban: up to 12 months	-0.118***	-0.115***	-0.130***	-0.084**
	(0.037)	(0.034)	(0.039)	(0.035)
Empl. Ban: 13+ months	-0.128***	-0.113***	-0.121***	-0.091***
	(0.035)	(0.036)	(0.037)	(0.031)
Observations	4,242	4,242	4,242	4,242
Equality of coeff. (p-value)	0.62	0.92	0.53	0.69
	Panel B: Participation			
Empl. Ban: up to 12 months	-0.117**	-0.103**	-0.110**	-0.078**
	(0.052)	(0.041)	(0.046)	(0.029)
Empl. Ban: 13+ months	-0.132***	-0.107***	-0.115***	-0.098***
	(0.044)	(0.034)	(0.040)	(0.025)
Observations	4,242	4,242	4,242	4,242
Equality of coeff. (p-value)	0.59	0.84	0.82	0.40
	Panel C: Unemployment			
Empl. Ban: up to 12 months	0.010	0.021	0.036	0.015
	(0.044)	(0.044)	(0.046)	(0.058)
Empl. Ban: 13+ months	0.010	0.023	0.024	0.007
	(0.043)	(0.043)	(0.044)	(0.057)
Observations	3,112	3,112	3,112	3,112
Equality of coeff. (p-value)	0.984	0.958	0.680	0.765
Host country x Year FE	X	X	X	X
Entry cohort FE	X	X	X	
Individual characteristics		X	X	X
Origin area FE			X	
Entry cohort x Origin area FE				X

Notes. The table reports estimates of the effects of employment bans of different duration at the time of arrival on refugees' labour market outcomes. "*Empl. Ban: up to 12 months*" and "*Empl. Ban: 13+ months*" are dummies equal to one if the employment bans (that were in place at the time of arrival in the host country) lasted at most 12 months or at least 13 months, respectively. The excluded category is "no employment ban". Estimates are from linear probability models with employment (panel A), participation (Panel B) and unemployment (panel C) dummies as outcome variables. In each panel, we report the p-value from testing the equality of the two estimated coefficients on the employment ban dummies. Individual characteristics are dummies for male, five–year age groups and three educational levels. The sample comprises of refugees aged 25-64, arrived between 1985 and 2012, who spent at least two years in the host country. Standard errors (in parentheses) are clustered by destination country: *** p<0.01, ** p<0.05, * p<0.1

Figure 3 – Effect on employment, participation and unemployment probability by years since migration



Notes. The figure plots estimates and 95% confidence intervals by years since migration from a linear regression of employment (panel A), participation (panel B) and unemployment (panel C) dummies on the Employment Ban indicator and other controls. Other controls are: destination country by observation year fixed effects, cohort of entry by source area fixed effects and dummies for individual characteristics (male, five–year age groups and educational level). Standard errors are clustered by destination country.

Effect persistency. Because our estimates point to sizeable detrimental ban effects on the labour market performance of refugees already in the host country for at least 2 years (for whom bans have already expired), we now examine how lasting these effects are. According to Figure 3, which graphs the estimates and 95% confidence intervals for the ban effect on employment, participation and unemployment probability by years since arrival, the ban has a 24 p.p. negative effect on employment probability in the first 2-4 years post entry, which decreases to 19 p.p. after 5-7 years and 8 p.p after 8-10 years in the country (Panel A). This latter coefficient, like those for refugees with more than 10 years in the host country, is not statistically different from zero. The effect on participation (panel B) follows a very similar pattern (becoming insignificant after 8-10 years), whereas the effect on unemployment (Panel C) is short-lived: it is relatively large and marginally significant for refugees with the shortest duration of residence (2-4 years) but quickly converges

to zero for other groups. The results for employment and participation underscore the high persistency of the effects of even relatively short employment bans, which, consistent with Marbach et al. (2018) and the literature on labour market entrance during a recession (Oreopoulos and von Wachter, 2012; Altonji et al., 2016), may leave scars that last for up to a decade.

Robustness checks. Prior to more extensive testing of research design plausibility (see section 6), we perform a set of robustness checks of our main findings. The first test augments our final specification with a full set of interactions between individual characteristics (gender, age and education) and area of origin fixed effects to allow the characteristics of refugees from different areas to have differential returns in European labour markets. The estimates, reported in Appendix Table A 4 where we measure employment bans with an indicator dummy in odd columns and two dummies for bans above and below 12 months in even columns, are remarkably similar to those reported in column 4 of Table 1 and Table 2, respectively. We then confirm that our findings are not driven by any specific country in our sample, by re-estimating the ban effect on employment (panel A) and participation (panel B) while dropping one host country at a time (Appendix Figure A 4). Finally, we address the concern that our clustering of standard errors (by 19 host countries) may lead to incorrect inference due to a small number of cluster (Cameron et al. 2008). Appendix Table A 5 reports p-values obtained with wild cluster bootstrap and shows that the level of statistical significance of our main estimates – for both employment (columns 1-4) and participation (columns 5-8) – is fully preserved.

6. Further Empirical Issues

6.1. Evidence on Identifying Assumptions

Correlation with country-specific shocks at arrival. The combined evidence from our main regressions for refugees and the placebo regressions for other migrants suggests that our estimates of the employment ban effect are not biased by other conditions at arrival. Nevertheless, a concern remains that certain initial shocks (e.g. entering during a recession or a peak in refugee inflows) may have a differential − and more negative − impact on the asylum seeker population *vis-a-vis* other migrants, a difference not accounted for by our placebo regression. To tackle this issue directly, we re-estimate equation (1) for our main sample of refugees while controlling for specific economic, political and refugee-related shocks at time of arrival. As Table 3 shows, not only are

our baseline estimates (column 1) robust to the inclusion of additional controls for economic conditions (log GDP), a left-right ideology index for the government in power[23] and the number of asylum applications and stock of refugees (columns 2, 3 and 4, respectively), but our estimated employment ban effect on refugee employment and participation remains negative and statistically significant even when all are simultaneously controlled for (columns 5 and 6).[24]

Table 3– Controlling for country-specific shocks at arrival

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Employment			
Employment Ban	-0.117***	-0.125***	-0.117***	-0.120**	-0.127***	-0.091**
	(0.038)	(0.036)	(0.040)	(0.042)	(0.043)	(0.042)
			Panel B: Participation			
Employment Ban	-0.112**	-0.121***	-0.112**	-0.121**	-0.125***	-0.101***
	(0.043)	(0.041)	(0.043)	(0.046)	(0.043)	(0.027)
Observations	4081	4,081	4,081	4,081	4,081	4,081
Economic conditions (_T)		X			X	X
Government ideology (_T)			X		X	X
Asylum appl. & refugee pop. (_T)				X	X	X
Individual charact. and FE	X	X	X	X	X	X
Entry cohort x Origin area FE						X

Notes. The table reports estimates of the effects of employment bans at the time of arrival on refugees' probability of employment (panel A) and participation (panel B), after controlling for a set of initial conditions. In column 2, we control for economic conditions at the time of arrival (T), measured with log GDP. In column 3, we control for government ideology at time T, measured with a left-right scale (going from 0 to 10) elaborated from data provided by ParlGov (http://www.parlgov.org//). In column 4 we control for asylum applications and stock of refugees (both in logs) at time T, taken from Eurostat. In columns 5 and 6, we simultaneously control for all the previous variables. The sample includes all refugees in the main estimating sample for whom we have information on all these variables at the time of arrival. For comparison, we report in column 1 results from our baseline specification on this restricted sample. All specifications include fixed effects for destination country by survey year, cohort of entry and host country and dummies for individual characteristics (male, five–year age groups and educational level) whereas entry cohort by area of origin dummies are included in the last specification only. Standard errors (in parentheses) are clustered by destination country: *** p<0.01, ** p<0.05, * p<0.1

[23] Our government left-right ideology index is derived from the ParlGov (http://www.parlgov.org//) political science data set, which uses information on parties, elections and cabinets from 1990 onwards for all EU countries to construct a time-specific, 0 to 10, left-right ideology index for each party in Europe. To construct our index, we average the indexes of the parties forming the government in each country and year, weighting the indexes by the each party's relative power in government (proxied by its number of parliamentary seats).

[24] An alternative approach would be to test whether current economic, political and/or refugee flow shocks predict future changes in employment bans. Employing this strategy in unreported regressions, however, indicates no predictive power for any of these variables, an outcome that complements the results reported in Table 3.

Timing of the effect and parallel trends. In our setting, employment bans are imposed as well as lifted at different points in time in different countries (see Appendix Figure A 3). To investigate the timing of the effect and to assess the evidence in favour of the parallel trend assumption, we estimate dynamic specifications of equation (1) and test whether we observe any effect of the bans on refugee cohorts that arrived immediately before the introduction or immediately after the removal of an employment ban. Since these cohorts were not treated, finding no effect of the policy would support the credibility of our identification strategy and dispel potential concerns about differences in pre-trends between *banned* and *non-banned* refugees. In order to carry out this empirical test, we include in our main equation dummies that identify placebo cohorts which either preceded the first *banned* cohort or followed the last *banned* cohort (in the case of a ban introduction or of a ban removal, respectively). In particular, for each employment ban introduced (removed) in time T, we focus on the first (T +/- 1) and second (T +/- 2) cohorts arrived before (after) the introduction (removal) of the ban. Estimation results for employment (Panel A) and participation (Panel B) probabilities are reported in Table 4. As the first row of each panel shows, the inclusion of these additional dummies does not affect the estimated effect of employment ban on treated refugees, which remains large and precisely estimated. On the other hand, the estimates on the placebo employment ban dummies in the following rows are centered around zero and never statistically significant at conventional levels, clearly indicating that the effect of employment restrictions does not occur *before* their introduction or *after* their removal and is therefore not driven by differential trends between treatment and control groups.

We further look into the timing of our effect of interest by zooming on refugee cohorts who arrived in European countries in the years around (just before and just after) the introduction or the removal of an employment ban. Results in Appendix Table A 6 show that our estimates of the employment ban impact preserve both their magnitude and level of significance – despite a substantial decrease in sample size – when we focus just on three cohorts before/after each policy change (columns 1-3), suggesting that the effect occurs precisely in coincidence with the implementation of such policy. Remarkably, further restricting our sample to two (column 4) and one cohort(s) (column 5) before/after policy changes still delivers (mostly) significant estimates.

Table 4 – Timing of the effect and parallel trends

	(1)	(2)	(3)	(4)	(5)
			Panel A: Employment		
Empl. Ban (T)	-0.125***	-0.123***	-0.137***	-0.114***	-0.121***
	(0.035)	(0.032)	(0.034)	(0.034)	(0.040)
Empl. Ban (T +/- 1)		0.004	-0.024		
		(0.045)	(0.047)		
Empl Ban (T +/- 1 and 2)				0.017	0.004
				(0.038)	(0.052)
			Panel B: Participation		
Empl. Ban (T)	-0.128**	-0.120***	-0.119***	-0.116***	-0.104*
	(0.045)	(0.033)	(0.040)	(0.036)	(0.051)
Empl. Ban (T +/- 1)		0.013	-0.009		
		(0.065)	(0.058)		
Empl Ban (T +/- 1 and 2)				0.018	0.015
				(0.054)	(0.057)
Observations	4,242	4,242	4,242	4,242	4,242
Host country by Year FE	X	X	X	X	X
Entry cohort FE	X	X	X	X	X
Individ. charact. + Origin area FE			X		X

Notes. The table explores the timing of the effect of employment ban on refugees' employment (Panel A) and participation (Panel B) probabilities. "*Empl. Ban (T)*" is our usual dummy equal to one for an employment ban being in place in the destination country at the refugee's time of arrival (T) while "*Empl. Ban (T +/- 1)*" is an indicator variable that identifies the first cohort arrived before (after) the introduction (removal) of an employment ban. Finally, "*Empl Ban (T +/- 1 and 2)*" is a dummy variable taking value one for those cohorts arrived 1 or 2 periods before (after) the introduction (removal) of the employment ban. Individual characteristics and fixed effects are defined as in previous tables. Standard errors (in parentheses) are clustered by destination country: *** p<0.01, ** p<0.05, * p<0.1.

Sorting. Our estimates of employment ban impact are potentially biased if asylum-seekers self-select into countries with different levels of employment restrictions based on individual unobservable characteristics that may determine future labour market integration. To address this issue, we use information about refugees' individual characteristics and test the sorting of asylum-seekers across countries and time of migration, which determined the degree of employment restrictions they faced at arrival.

Table 5 – A test of sorting into employment bans

Sample:	All refugees				Refugees with education adquired before migration			
Outcome variable:	Empl Ban		Indefinite Ban		Empl Ban		Indefinite Ban	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Male	-0.030	-0.033	-0.027	-0.023	-0.031	-0.032	-0.019	-0.011
	(0.020)	(0.022)	(0.018)	(0.016)	(0.019)	(0.021)	(0.015)	(0.012)
Age at arrival	-0.001	-0.001	-0.009	-0.009	-0.000	0.000	-0.002	-0.001
	(0.003)	(0.003)	(0.010)	(0.009)	(0.002)	(0.002)	(0.005)	(0.004)
Age at arrival_squared	0.000	0.000	0.000	0.000	0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Upper secondary education	-0.020	-0.020	-0.015	0.003	-0.016	-0.012	-0.010	0.007
	(0.017)	(0.015)	(0.040)	(0.035)	(0.021)	(0.019)	(0.063)	(0.058)
Tertiary education	0.007	0.004	-0.019	-0.012	0.008	0.007	0.006	0.009
	(0.022)	(0.019)	(0.028)	(0.026)	(0.023)	(0.020)	(0.043)	(0.039)
New EU member states		-0.016		-0.077		0.001		-0.060
		(0.029)		(0.106)		(0.031)		(0.117)
North Africa and Middle East		-0.010		0.026		-0.009		-0.009
		(0.022)		(0.070)		(0.024)		(0.068)
Other Africa		-0.058		-0.070		-0.083		-0.063
		(0.054)		(0.104)		(0.059)		(0.096)
South-East Asia		-0.006		-0.133		0.003		-0.145*
		(0.037)		(0.080)		(0.030)		(0.079)
Latin America		-0.147		0.106		-0.117		0.110
		(0.129)		(0.174)		(0.126)		(0.179)
Observations	4,242	4,242	4,242	4,242	3,392	3,392	3,392	3,392
First 5 coefficients=0 (p-value)	0.584	0.567	0.265	0.073	0.556	0.541	0.746	0.815

Notes. The table tests whether refugees sort into employment bans based on pre-determined characteristics. Estimates are obtained from LPM. "Empl Ban" is the usual indicator for some ban being in place at the time of arrival in the host country; "Indefinite Ban" is an indicator for the employment ban lasting for the entire duration of the asylum application process. The excluded education category is "lower than upper secondary education". Columns 1-4 report results from the entire sample of refugees used in the main analysis, while columns 5-8 report those from the restricted sample of refugees who had acquired their highest level of formal education before migration. Standard errors (in parentheses) are clustered by destination country: *** p<0.01, ** p<0.05, * p<0.1

We regress two alternative measures of employment restrictions presence at the time of arrival – an indicator for any ban and another for an indefinite ban – on gender, age at arrival (and its squared term), indicators for upper secondary and for tertiary education, and dummies for areas of origin. Table 5 (columns 1-4) shows that, regardless of controlling or not for origin area dummies, the coefficients on individual characteristics are small and not statistically significant. All individual characteristics used in this test refer to the time of migration except for education, which refers to the time of the survey. However, we can use information about the year when the highest level of formal education was acquired for each respondent to focus on those refugees whose education

pre-dates migration. Even in this sub-sample of refugees (columns 5-8 of the table) the level of education *before* migration does not seem to correlate with employment bans and more generally a test of joint significance of coefficients consistently fails to reject the null. These results suggest that – as long as the absence of selection on observable characteristics is informative about the degree of selection on unobservable ones – our results are not significantly affected by sorting bias. This seeming lack of self-selection into our treatment variable – further confirmed by evidence from aggregate data that we discuss in section 7.2 – is straightforward to rationalize once we consider that asylum seekers may have only imperfect control over their country of destination and only partial information about specific aspects of the asylum policy such as the employment bans. Additionally, the saliency of employment restrictions (relative to other factors) for their decision-making process may be limited.

6.2. Instrumental Variable Strategy

As a final test on the validity of our findings, we implement an instrumental variable strategy that exploits the 2003 EU directive capping EU employment bans at 12 months.[25] Although member countries can use legislative discretion to achieve directive goals, these dictums are legally binding, so the EU Commission may launch a formal infringement procedure against countries failing to implement measures that fully transpose directive provisions. Our instrumental variable strategy thus exploits pre-directive cross-sectional variation in ban duration to exogenously predict changes in ban duration over the post-2003 period. The combination of this variation and the timing of the 2003 EU directive generates a natural experiment in asylum seeker exposure to employment bans, an approach similar to the one used to study the effects of gender quotas introduction (Stevenson, 2010; Ahern and Dittmar, 2012; Bertrand et al., forthcoming). We graph our first stage in Appendix Figure A 5 by plotting the observed post-directive changes in employment ban duration across EU countries between 2001 and 2013 (vertical axis) against their pre-EU directive durations as measured in 2000 (horizontal axis). The negative slope suggests that countries with longer (or indefinite) bans before 2003 subsequently experienced the strongest

[25] Here, we refer to the "Council Directive 2003/9/EC of 27 January 2003 laying down minimum standards for the reception of asylum seekers". *Article 11 – Employment. 1. Member States shall determine a period of time, starting from the date on which an application for asylum was lodged, during which an applicant shall not have access to the labour market. 2. If a decision at first instance has not been taken within one year of the presentation of an application for asylum and this delay cannot be attributed to the applicant, Member States shall decide the conditions for granting access to the labour market for the applicant".* In 2013, Reception Conditions Directive; 2013/33/EU further reduced the maximum waiting time for legal access to the labour market from 12 to 9 months.

reductions in ban duration. In our context, the exclusion restriction implies that pre-2003 restrictions affect the integration of refugees arriving post 2003 only through the induced variation in the way employment restrictions evolved in the post-2003 period. Although it is important to acknowledge that the pre-2003 restrictions were not randomly assigned to countries, we support our identifying assumption in two ways. First, the inclusion of fixed effects in all our estimates captures any time-invariant country characteristics that may correlate with both pre-directive bans and refugee integration. Second, in Appendix Table A 7, we test whether EU countries with stricter pre-2003 restrictions were systematically different with respect to other countries in population size, economic performance (GDP per capita and unemployment rate) and size of refugee population (refugee stock and asylum seeker inflow, per 10,000 population). To do so, we regress employment restrictions in 2000 (i.e. our proposed instrument) on contemporaneous country characteristics – adding them separately and then jointly – and find no significant correlation.[26]

Two limitations of using this approach in our particular context are worth discussing. First, less than 14 percent of our refugee sample arrived after 2003, leaving a relatively small population of potential compliers (i.e. individuals whose exposure to ban was exogenously changed by the enactment of the EU directive). Second, although the 2003 EU directive generates exogenous variation on the intensive margin of the bans – mainly inducing countries that formerly gave asylum seekers no legal access to their labour market to reduce their bans to 12 months – it does not generate variation on the extensive margin or in other points of the distribution of bans.[27] Empirically, this latter implies the need for a continuous measure of employment bans, which we achieve by again regressing labour market outcomes on ban duration in months and months squared (cf. Appendix Table A 3). In our first stage regressions, we instrument these two variables with a full set of interactions between post-2003 entry cohort dummies and pre-directive restrictions measured in 2000 (see Appendix Table A 8).[28] Except for the first two cohorts (2004

[26] The outcome variables in Appendix Table A 7 are a dummy equal to one if an EU country had an indefinite ban (columns 1-4) or the ban duration (in months; columns 5-8), both measured before the 2003 EU directive. To minimize concerns about endogenous anticipation effects of the policy change, we use as our pre-2003 directive bans the restrictions in place in 2000. Using alternative measurements – for example, those in 1998 or 2002 – delivers very similar findings in both the first and second stage IV regressions.

[27] This feature is not uncommon in IV settings, where there may be more treatment levels than available instruments (Lochner and Moretti, 2015).

[28] It should also be noted that the sample excludes refugees in UK and Ireland, two countries not subject to the EU directive, which leaves 3,665 observations. Estimates obtained using the full sample are qualitatively similar to those reported in Table 6.

and 2005), all other estimated coefficients are negative and most are significantly different from zero. These estimates suggest that – within a couple of years of directive enactment – EU countries started updating their legislation and reducing their bans at or below the 12 month threshold. At the same time, the strong F-stat reported at the bottom of the table shows that pre-directive restrictions are strong predictors of future changes. 2SLS estimates are reported in Table 6: we gradually add controls and fixed effects to our baseline DiD specification to assess the ban effect on employment (panel A) and participation (panel B), using alternative maximum ban durations of 24, 30 and 36 months. These estimates (although less precisely estimated) not only confirm all our previous findings but suggest that the ban effect may be even larger than estimated with the OLS regressions (cf. Appendix Table A 3).

Table 6 – Effect of ban on labour market outcomes: 2SLS estimates

	(1)	(2)	(3)	(4)	(5)
			Panel A: Employment		
Empl. Ban: # Months	-0.036*	-0.037*	-0.038*	-0.042**	-0.039
	(0.017)	(0.018)	(0.019)	(0.018)	(0.049)
Empl. Ban: # Months Sq.	0.001*	0.001*	0.001*	0.001*	0.001
	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)
			Panel B: Participation		
Empl. Ban: # Months	-0.040*	-0.036	-0.035	-0.039	-0.033
	(0.023)	(0.023)	(0.023)	(0.023)	(0.026)
Empl. Ban: # Months Sq.	0.002*	0.001*	0.001*	0.001*	0.001
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Observations	3,665	3,665	3,665	3,665	3,665
F-stat: Ban - # Months	52.58	183.7	489.8	14.31	162.4
F-stat: Ban - # Months Sq.	333.0	435.1	239.6	53.86	182.5
Max ban: # months	24	30	36	30	30
Host country x Year FE	X	X	X	X	X
Entry cohort FE	X	X	X	X	X
Individual characteristics				X	X
Entry cohort x Origin area FE					X

Notes. The table reports 2SLS estimates of the effects of employment ban at the time of arrival on refugees' employment (panel A), and participation (panel B) probabilities. The "Empl. Ban: # Months" and "Empl. Ban: # Months Sq." variables measure the duration of the ban (if any) in months and in months squared, respectively. These two employment ban variables are instrumented with a full set of interactions between post-2003 entry cohort dummies and pre-directive restrictions measured in 2000 (see Appendix Table A 8). Indefinite bans are set equal to 24 (column 1), 30 (columns 2, 4 and 5) or 36 months (column 3). No bans are coded as bans with zero months duration. Individual controls and FEs are defined as in previous tables. The sample exclude observations from Ireland and the UK. Standard errors (in parentheses) are clustered by destination country: *** p<0.01, ** p<0.05, * p<0.1

7. Mechanisms and Further Results

7.1. Heterogeneous Effects and Other Outcomes

We first investigate whether employment bans have heterogeneous effects by educational levels and report the results for both employment (columns 1) and participation (columns 2) in Panel B of Table 7.[29] We uncover a clear effect gradient that is concentrated among refugees with lower or upper secondary education, albeit stronger among the former than the latter. Refugees with tertiary education, in contrast, seem not to be penalized by the forced initial period of idleness in the host country. Hence, a human capital depreciation mechanism – which would affect highly educated workers more strongly – does not seem a likely means by which employment bans affect economic integration. To the extent that higher education reflects higher underlying ability, our results suggest that capability differences might translate into a differential capacity to recover from initial setbacks in the labour market. At the same time, our findings might be explained by complementarities between extant educational level and speed of acquiring additional human capital, such as host country language. Yet remarkably, this heterogeneous pattern of results closely resembles that in the literature on the effects of entering the labour market during a recession. For example, both Oreopoulos and von Wachter (2012) and Altonji et al. (2016) find that college graduates with lower predicted earnings are those that suffer the largest and most persistent negative consequences, while Schwandt and von Wachter (2019) show that the adverse effects of recessions are concentrated among less educated workers and minorities.

We thus extend our analysis to a broader set of outcomes; namely, occupational quality, host country language proficiency, health and welfare dependency.[30] For each outcome, Table 7 reports the overall effect of bans (Panel A) and the heterogeneous effects by educational level (Panel B). Our estimates for occupational quality – measured by being in a high-skill occupation, having a temporary job and feeling overqualified (columns 3-5) − imply that *banned* refugees are less likely to be employed in a highly skilled occupation and more likely to be employed in a temporary job,

[29] In unreported regressions, we study heterogeneity by gender. Our estimates imply that the detrimental ban effect may be stronger for women than men, although the difference is not statistically significant at conventional levels.
[30] It should be emphasized that all these alternative outcomes (defined in detail in the notes to Table 7) are measured at the same time as labour market status, making a clear causal interpretation difficult. That is, although worse outcomes on other dimensions of *banned* refugees' lives may explain their weaker labour market performance, the opposite effect may be also in place, as well as a feedback mechanism from one dimension to the other.

whereas no significant effect is found for being overqualified. Educational heterogeneity displays an interesting pattern: similar to the main effect on employment, the detrimental effects seem mostly concentrated among individuals with low and intermediate education. On the other hand, the probability of feeling overqualified is significantly higher for *banned* refugees with tertiary education. Taken together with the estimates reported in columns 1 and 2, these findings suggest that *banned* refugees with relatively low education are more likely to drop out of the labour market or to hold low quality jobs if employed. Highly educated refugees, in contrast, seem better able to overcome the potential damage of being banned, possibly by accepting substantial downgrading in the labour market. As regards proficiency in the host country language, refugees exposed to employment bans are significantly more likely to self-report a low proficiency level, with the effect driven mostly by low-skilled individuals (Table 7, column 6). This observation strongly implies that the damage done to the acquisition of host country-specific human capital by restricted labour market access may be the most intensive for individuals with lower initial human capital. In terms of health-related outcomes, it is well-documented that refugees suffer disproportionately from health and mental health issues because of the traumatic experiences leading to their displacement (Burnett and Peel, 2001; Phillimore, 2011). Not only may forced idleness exacerbate these issues, lowering labour market participation, but initial obstacles to employment may increase the likelihood of refugees claiming disability to obtain subsidies. Likewise, in regard to welfare state dependency, the typical coupling of employment bans with welfare assistance (to prevent asylum seekers from falling into poverty and destitution) may also result in higher welfare reliance among banned refugees, especially if such an early encounter with social assistance encourages a culture of welfare dependency that negatively affects future labour market participation. Hence, our estimates on health questions (columns 7-9), although not statistically significant, are qualitatively in line with our intuitive assumptions, as is our finding that *banned* refugees are substantially more likely to report receiving benefits (column 10), an effect driven by low-skilled individuals.

Table 7 – Heterogeneous effects and other outcomes

	Employment status		***Occupational quality***			***Human capital***		***Health status***		***Welfare***
	Employment	**Participation**	**High skill occupation**	**Temporary Job**	**Over-qualified**	**Low language proficiency**	**Lab mkt status: disabled**	**Not FT b/c health or disability**	**Left job b/c health or disability**	**Receiving benefits**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
					Panel A - Overall effect					
Employment Ban	-0.089***	-0.092***	-0.064**	0.101***	0.044	0.125*	0.020	0.104*	0.047	0.038***
	(0.031)	(0.026)	(0.026)	(0.027)	(0.079)	(0.065)	(0.022)	(0.059)	(0.067)	(0.011)
					Panel B - Heterogenous effects by education					
Empl. Ban * Lower Sec. Educ.	-0.163*	-0.129***	-0.048	0.149***	-0.086*	0.208**	0.015	-0.025	0.042	0.082**
	(0.079)	(0.028)	(0.056)	(0.029)	(0.040)	(0.078)	(0.044)	(0.035)	(0.106)	(0.034)
Empl. Ban * Upper Sec. Educ.	-0.103**	-0.091	-0.110**	0.106***	0.111	0.083	0.031	0.286**	-0.143	0.008
	(0.047)	(0.058)	(0.050)	(0.019)	(0.157)	(0.051)	(0.028)	(0.120)	(0.103)	(0.030)
Empl. Ban * Tertiary Educ.	0.095	-0.016	0.003	-0.030	0.142**	0.026	0.012	0.042	0.164**	-0.005
	(0.061)	(0.053)	(0.130)	(0.146)	(0.059)	(0.069)	(0.029)	(0.074)	(0.071)	(0.040)
Mean of outcome	0.58	0.71	0.26	0.19	0.321	0.17	0.06	0.12	0.18	0.07
Observations	4,242	4,242	2,631	2,285	832	1,458	3,365	572	616	3,547
Ind. characteristics and FE	X	X	X	X	X	X	X	X	X	X

Notes. The table reports estimates of the effects of employment ban on different refugees' outcomes (all measured with dummies): employment and participation probabilities (col. 1-2); employed in a high skill occupation (col. 3) or in a temporary job (col. 4); feeling overqualified for the occupation currently held (col. 5); dummy equal one if respondent answers that she is not proficient in host language (only 2014; col. 6); dummy for respondents who are inactive due to permanent disability (col. 7); not FT b/c health of disability (col. 8): dummy for respondents who claim not being able to work full time due to health issues or disability (col. 9); dummy for receiving benefits and/or other forms of assistance (col. 10). All specifications include destination country by observation year fixed effects, cohort of entry by host country fixed effects and dummies for individual characteristics (male, five–year age groups and educational level). Standard errors (in parentheses) are clustered by destination country: *** p<0.01, ** p<0.05, * p<0.1

7.2. Asylum Policies and Refugee Migration Flows

In this section, we investigate two interrelated – although distinct – issues: we first analyse the potential role played by other asylum-related policies and we then estimate the impact of employment bans on refugee flows.

Simultaneous asylum-related policies. Legislative changes in migration and asylum policy are often introduced as a bundle rather than as individual elements. To ensure our estimates are not picking up the effect of other policies, simultaneously introduced with employment ban modifications, we add in controls for immigration and asylum policies in place at time of arrival. This analysis employs two indices developed by the Immigration Policies in Comparison (IMPIC) Project to measure different migration policy dimensions, each with a separate version for asylum seekers and economic migrants, and each increasing in the restrictiveness of the policy measured.[31] The first index (access) addresses policies that govern entry to the country, such as the eligibility requirements for asylum seekers or immigrants to enter the country, while the second index (inside) captures policies that regulate status security once inside the country and asylum-seeker/immigrant rights to labour market access and other privileges. In Table 8, we report the results for refugee employment (columns 1-6) and participation probability (columns 7-12), restricting our sample to refugees who arrived before or during 2010, the last year for which IMPIC data are available. In each panel, we report baseline equation (1) estimates in the first column (for purposes of comparison), then show estimates for each policy index separately and conclude with jointly conditioning on all indices. Although only suggestive, the coefficients on the index for asylum policies inside the country (from which everything related to employment bans is excluded) associate more restrictive regulation with lower employment and participation probabilities, whereas the estimates for the other indices are fairly inconclusive. More important, the estimated coefficients on employment ban are robust to controlling for other migration and asylum policy dimensions; strong evidence that our employment ban measure is capturing a distinctive and specific feature of host country asylum policies.

[31] The Immigration Policies in Comparison (IMPIC) Database, a project of the German Research Foundation, provides a set of quantitative indices to measure immigration policies in all OECD countries for the time period 1980-2010 (for more details, see Helbling et al., 2017, and http://www.impic-project.eu/data/).

Table 8 – Simultaneous asylum and immigration-related policies

	Employment						Participation					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Employment Ban	-0.072**	-0.078**	-0.082***	-0.076***	-0.076***	-0.073**	-0.075***	-0.060***	-0.082**	-0.075***	-0.075***	-0.052***
	(0.030)	(0.036)	(0.018)	(0.023)	(0.021)	(0.027)	(0.022)	(0.013)	(0.031)	(0.022)	(0.022)	(0.017)
Restrictivness of policies for:												
Asylum-seekers & refugees (access to the country)		-0.017				0.034		0.039				0.107*
		(0.037)				(0.060)		(0.034)				(0.051)
Asylum-seekers & refugees (inside the country)			-0.058**			-0.070			-0.040**			-0.088***
			(0.021)			(0.042)			(0.014)			(0.029)
Economic migrants (access to the country)				0.018**		-0.015				-0.002		0.035
				(0.007)		(0.035)				(0.003)		(0.028)
Economic migrants (inside the country)					0.038**	0.063					-0.001	-0.059
					(0.013)	(0.055)					(0.008)	(0.047)
Observations	4,008	4,008	4,008	4,008	4,008	4,008	4,008	4,008	4,008	4,008	4,008	4,008
Individual controls and FE	X	X	X	X	X	X	X	X	X	X	X	X

Notes. The table reports estimates of the effect of employment bans at the time of arrival on refugees' probability of employment and participation. In columns 2-6 and 8-12, we control for other measures of asylum and migration policies at arrival in host countries, derived from the IMPIC database and available from year 1985 to 2010. Specifically, in columns 2 and 8 we control for an index of restrictiveness of access to the country for asylum seekers, in columns 3 and 9 we control for an index of restrictiveness of policies toward asylum seekers and refugees in the host country, whereas in columns 4 and 10 and in columns 5 and 11 we control for the equivalent measures for economic migrants; in columns 6 and 12 we include all four measures of migration policy jointly. The sample includes all refugees in the main estimating sample for whom we have information on all these variables at the time of arrival. For comparison, we report in columns 1 and 7 results from our baseline specification on this restricted sample. All specifications include destination country by observation year fixed effects, individual characteristics (dummies for male, five–year age groups and educational level) and cohort of entry by host country fixed effects. Standard errors (in parentheses) are clustered by destination country: *** p<0.01, ** p<0.05, * p<0.1.

Effect on flows. Although the evidence presented in section 6.1 suggests the absence of sorting of asylum-seekers into employment bans on individual characteristics, one may still wonder whether these restrictions are effective in discouraging refugee migration.[32] To assess the employment ban effect on refugee flows, we thus run country-level regressions using Eurostat data on the number of asylum applications and refugee populations recorded in our sample countries since 1985. More specifically, we regress either the logarithm of asylum applications received (Table A 9, columns 1-3) or the percent annual change in refugee stock (columns 4-6) on our main employment ban indicator and country and year fixed effects. Further, we include time variant controls (per capita GDP, unemployment rate, and a left-right index of government ideology) in columns 2 and 5, and their one-year lags in columns 3 and 6. For the log of asylum applications as outcome, the coefficient is negative, around -0.17; however, the standard errors are large and the estimates marginally significant only in the first specification. For the annual change in refugee stock, however − a variable available for a slightly larger sample of country-years − the estimates are close to zero (0.007) and flip sign when controls are included. These estimates thus provide little evidence for a deterrence effect of employment bans on asylum seeker flows. The result is quite remarkable given that policy makers often consider employment bans an effective means to discourage inflows, thereby overlooking their potential detrimental effects on the labour market performance of new arrivals.

In columns 7- 9 of Table A 9, we regress the asylum applications received by each EU country, disaggregated by gender and age group (as detailed in the Eurostat dataset from 2008 onwards), which reveals that employment bans have no effect on either the share of males or the probability of falling into any of the following age categories: 14-34, 35-64, 65+. The policy thus does not appear to be associated with changes in the composition of arriving asylum seekers in terms of gender or age profile, confirming our results on the absence of sorting that we obtained from the EULFS data and reported in Table 5. Further, the comparison of findings from data on inflows (i.e. Eurostat) and current stocks (i.e. EULFS) – both pointing at no evidence of sorting into

[32] Moreover, insofar as bans affect refugee economic integration, they may also alter their return decisions. If, as Borjas and Bratsberg (1996) suggest, immigrants who initially had a bad draw from the income distribution at destination were more likely to return to their home countries, this selective return migration (if present in our sample) would make it harder to observe the detrimental effect of employment bans.

employment restrictions on individual characteristics – suggests that outflows are also not systematically responding to bans.

7.3. Quantifying Potential Output Losses

Our analysis clearly demonstrates that temporary employment restrictions have long-lasting negative consequences for asylum seeker employment prospects that extend well beyond the initial incapacitation effect inherent in the ban. In this section we use our causal estimates to calculate the aggregate direct cost of employment bans in terms of output loss for the European economy during the peak of the so-called refugee crisis.

During the two years 2015-2016 approximately 3.7 million asylum applications were filed in the 28 EU countries plus Norway and Switzerland, and we estimate that about 1.3 million applicants were granted some form of international protection in the first instance. However, only 140 thousand (11% of the total) of these new refugees arrived in one of the four countries that granted immediate access to the labour market (Greece, Norway, Portugal and Sweden). The remaining 1.15 million (89%) were hosted by countries that banned asylum seekers from working for a period ranging between 2 months (Italy) and the granting of refugee status (Ireland and Lithuania). We can use our estimates of the employment bans effect by years since migration (see Figure 3) to estimate: 1) the economic output generated by these new refugees during their first eight years in the country (i.e. over the time period over which our analysis shows the bans exert a negative effect); 2) the counterfactual economic output that refugees could have contributed if no country had a ban in place. The difference between (2) and (1) represents the output loss due to the presence of employment bans on asylum seekers. Our calculations – which we discuss in detail in Appendix A1.2 – are based on very conservative assumptions, potentially leading to an underestimation of the actual losses. Additionally, by focusing only on the economic contribution of refugees, we abstract from general equilibrium considerations such as the potential impact of employment bans on wages and employment prospects of other workers.[33]

[33] The evidence on the impact of refugee waves in Europe on natives' labour market outcomes is mixed. Looking at the flow of forced migrants from the 1990s' Balkan wars to European countries, Angrist and Kugler (2003) find sizeable detrimental effects on natives' employment probability. Recent reappraisals of their evidence, however, have questioned both the magnitude of their estimates (Borjas and Monras, 2017) and their statistical significance (Clemens and Hunt, 2019). Positive employment effects on natives are instead found in Foged and Peri (2016) for Denmark.

According to our estimates, under the employment ban policies in place at the time they entered the country, the 1.3 million individuals who arrived in 2015-2016 and were granted refugee status in first instance may contribute, during their first eight years of residence, to increase the size of the European economy by almost EUR 58 billion, raising the counterfactual question of what would have happened if all EU countries had lifted their ban and allowed asylum seekers to look for a job immediately upon arrival in the host country? Our analysis suggests that this policy change would have resulted in a 65% higher (EUR 37.6 billion) economic contribution than the baseline, bringing the economic balance of the *refugee crisis* to a EUR 95 billion larger output for Europe as a whole. In other words, our estimates suggest that the employment ban policies in place across European countries generated a EUR 37.6 billion output loss. These figures imply that, on average, the 1.15 million new refugees who were subject to the ban lost about EUR 32.8 thousand per capita, over the period studied, or about EUR 4,100 per year. By way of comparison, the EU Asylum, Migration and Integration Fund (AMIF) scheme − designed to support EU countries' management of migration flows, and the implementation, strengthening and development of a common EU approach to asylum and immigration for the 2014-2020 period – was financed with EUR 3.1 billion over the 7 years.

8. Conclusions

Given our empirical evidence that imposing temporary employment bans on asylum seekers has large negative consequences for their subsequent labour market integration − an effect that may remain sizable for up to 10 years − host country governments should carefully weigh the (alleged) benefits of such bans against their longer term costs for both refugees and the host country economy. For example, according to our estimates, such attempts to reduce the filing of unfounded asylum applications and discourage illegal entry during the European refugee crisis resulted in an average yearly loss of EUR 4,100 in forgone output per banned refugee over a 8-year period, totalling a massive loss of EUR 37.6 billion for the EU economy. Not surprisingly, then, employment bans for asylum seekers have become an increasingly contentious issue over recent years as activists and policymakers become more aware of their potential negative effects, which may be little affected by the EU imposed 12-month and 9-month caps (Directives 2003/9/EC and 2013/33/EU, respectively). That is, according to our findings, although the length of the ban matters, the most important factor is whether or not asylum seekers are granted immediate access

to the host country labour market. Not only do most detrimental ban effects develop in the first few post-arrival months, when refugees must make important decisions about investment in host country-specific human capital, but simply shortening the ban duration may not be effective in increasing refugee labour market integration if their right to work is not actively enforced after ban expiration. Yet several countries still restrict the job type, employment sector, and contract duration available to asylum seekers even beyond ban expiry, making their labour market integration extremely unlikely.

The recent spike in asylum applications lodged in European countries has also reignited the debate about the best practices to foster refugee's social and economic integration, raising questions about barring them from legal access to the labour market. For instance, Germany, the EU country with the highest number of asylum applicants, has acknowledged the costs of employment bans but, not wanting to lift them completely, has opted for a legal system that modulates them based on the probability of obtaining refugee status. Hence, asylum applicants from countries considered safe (whose applications are very likely to be rejected) face a complete employment ban, while those from countries more highly recognized as problematic are allowed quick access to the German labour market. In this way, the system does nothing to hinder the integration of asylum seekers likely to remain as refugees but raises the application costs for those likely to be rejected. Nonetheless, given our evidence that the mere existence of a ban has no positive impact on the size of asylum seeker inflows, the case for its deterrence effect may well be overstated.

Evidence from past refugee waves further suggests that the socio-economic integration of forced migrants is often difficult and slow, in part because of difficulties inherent to the compulsory, unplanned and often traumatic origin of refugee migration. Yet whereas allowing asylum seekers some months to recover physically and psychologically may be beneficial for their long-term integration, forcing them out of the labour market seems an ill-conceived approach. That is, to be effective, a period of initial idleness should be coupled with well-designed and well-funded programs of counselling and training, a complex and expensive task for host countries. As a more pragmatic alternative, our findings point to the need for a zero-cost policy that effectively accelerates refugee integration by lifting barriers to their labour market access, which, while requiring only relatively straightforward legislative intervention and no additional funding, would generate substantial gains for both refugees and hosting societies.

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A1. Appendix

A1.1. Data Appendix

i. EULFS

Our analysis is based on the two EULFS ad hoc modules on migration collected in 2008 and 2014, which contain a rich set of additional variables on migrant experience in the host country. These modules are available for both 2008 and 2014 for: Austria, Belgium, Cyprus, France, Greece, Italy, Lithuania, Luxembourg, Norway, Portugal, Spain, Sweden, and the UK. Data for Germany, Ireland and the Netherlands are available only for 2008 and those for Bulgaria, Croatia, the Czech Republic, Estonia, Finland, Hungary, Latvia, Malta, Poland, Romania, Slovakia, Slovenia and Switzerland only for 2014.

These two modules allow us to distinguish refugees from other migrants thanks to the presence, in both years, of a question on main reason for migration (MIGREAS). The reason-for-migration question was asked to all non-native individuals who arrived in the country of residence when they were 15 years of age or older. The question slightly changed between 2008 and 2014 but it allows us to construct a consistent measure of refugee migrants. In 2008, respondents were asked to choose among eight alternative reasons for migration: (1) employment, intra-corporate transfer; (2) employment, job found before migrating; (3) employment, no job found before migrating; (4) study; (5) international protection; (6) accompanying family/family reunification; (7) family formation, and (8) other. In 2014, the categories were reduced to six: (1) employment, job found before migrating; (2) employment, no job found before migrating; (3) family reasons; (4) study; (5) international protection or asylum, and (6) other. We define "refugees" all the migrants who responded "international protection" as main reason for migrating. Our main sample consist of refugees who have been in the host country for at least two years. Additionally, we construct a second sample which includes only non-refugee migrants from outside the European Union, which we use for some falsification tests.

Apart from labour market outcomes such as employment, unemployment, participation status and the type of occupation, the EULFS also includes, for persons not born in the country, information on years of residence in this country (YEARESID). We use this variable to match immigrants to conditions faced at arrival, in particular on the presence and length of any employment ban included in EMPBAN. We keep in our sample only refugees who have been in the host country

for at least 2 years, who are in the 25−64 age group, and drop countries where we observe less than 25 such refugees.

ii. EMPBAN

EMPBAN is a dataset which we collected that provides information on the presence and duration of employment bans for asylum seekers in EU countries and in Norway. We have excluded Switzerland because of the substantial cantonal heterogeneity. We have gathered information on the evolution of legislation since 1985 (whenever possible; see Appendix Figure A 3). The dataset provides, for every country and year, a dummy indicating whether a ban is in place or not, and a continuous variable measuring the duration in months of the ban (if a ban is in place). When policy changes occur during the year we use for the whole year the median of the monthly values (for instance, if in country C a ban is lifted on March 1, 2010 the median value of the monthly "ban" dummy will be 0).

iii. Merging EULFS and EMPBAN

We merge EULFS and EMPBAN to attribute to every refugee information on the presence and potential duration of the employment ban they faced in the year they arrived in the country. For each refugee in EULFS we reconstruct the year of entry in the host country, based on the variable YEARESID (years of residence) and on the year of the interview. We then match each individual with the values of the employment ban variables in the year they entered the host country, YEARESID is provided at the yearly level for values up to ten (included) and then aggregated in 5-year age bands. In these latter cases we construct a five-year interval of entry in the host country and we match individuals to the modal value of the employment ban variables for the five-year interval. All our results are robust to using median (or mean) values instead.

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A1.2. Quantification of Output Losses

We assess the economic losses for receiving economies implied by temporary employment bans of asylum seekers, in terms of potential output loss for the EU economy in the wake of the recent so-called European refugee crisis. In particular, we estimate the potential employment ban-induced loss for asylum seekers entering Europe at the very peak of the crisis in 2015−2016. As explained in the text (section 7.3), we use our estimates of the employment bans effect by years since migration (see Figure 3) to perform a simple back-of-the envelope exercise that compares the potential output from refugees with and without bans over their first eight years since arrival. Our calculations are based on three conservative assumptions. First, we disregard the potential short

term losses implied by bans, assuming that no refugees work during their first twenty-four months in the host country regardless of whether banned or not (meaning that by construction, employment bans become damaging only from the second year of residence onwards). Second, we assume that all employed refugees, regardless of individual characteristics, work in an elementary occupation (ISCO code 9) whose mean gross annual earnings represent therefore the average contribution of employed refugees to the host country GDP. Third, we concentrate exclusively on asylum seekers whose refugee status was recognized on first application (even though bans may also have persistent detrimental effects on those initially rejected). Then, for each country *c*, we compute the output loss from bans (ΔY_c) as the difference between the output refugees would have generated in their first N years in the absence of a ban (Y_c^{NOBAN}) and the potential output generated in the presence of a ban (Y_c^{BAN}):

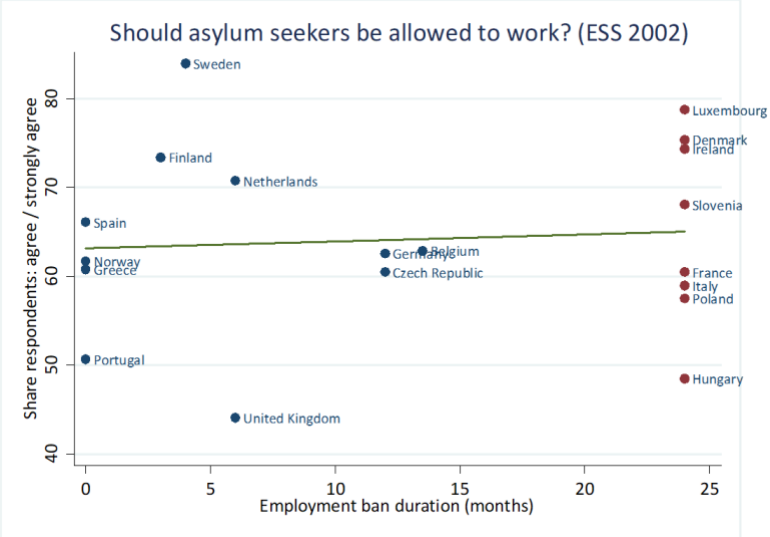
$$\Delta Y_c = Y_c^{NOBAN} - Y_c^{BAN} = \sum_{y=0}^{7} a_c \times s_c \times w_c \times (e_y - b_c p_y)$$

where: a_c indicates the number of first instance asylum applications received by EU country *c* in 2015 and 2016 (from Eurostat), and s_c indicates the share of successful first instance applications over the same 2 years (from UNHCR); w_c represents the mean gross annual earnings in elementary occupations expressed in 2018 euros (from Eurostat, dataset *earn_ses14_28*[34]); e_y is the refugee employment probability after *y* years since migration (assumed to be constant across all countries; we set this by hypothesis to 0 for years 0−1 since migration, and estimate it from EULFS data to 0.44 [0.51] for years 2−4 [5−7] since migration), and p_y is the percentage point gap in employment probability suffered by refugees subject to a temporary employment ban after *y* years since migration (see Figure 3). Lastly, b_c is a dummy variable equal to one if country *c* imposes an employment ban for asylum seekers in 2015/2016 and zero otherwise. We consider years 0 to 7 since arrival to ensure that the output loss is computed for the period during which employment bans exert the largest effect on refugee employment probabilities. Appendix Table A 10 reports for the variable values in each EU country, and the results of our estimates country-by-country as well as for the EU28+Switzerland and Norway as a whole.

[34] Note that the latest available year is 2014. Therefore, we use 2014 figures and discount them by the HICP (from Eurostat dataset *prc_hicp_aind*) to obtain 2018 euros.

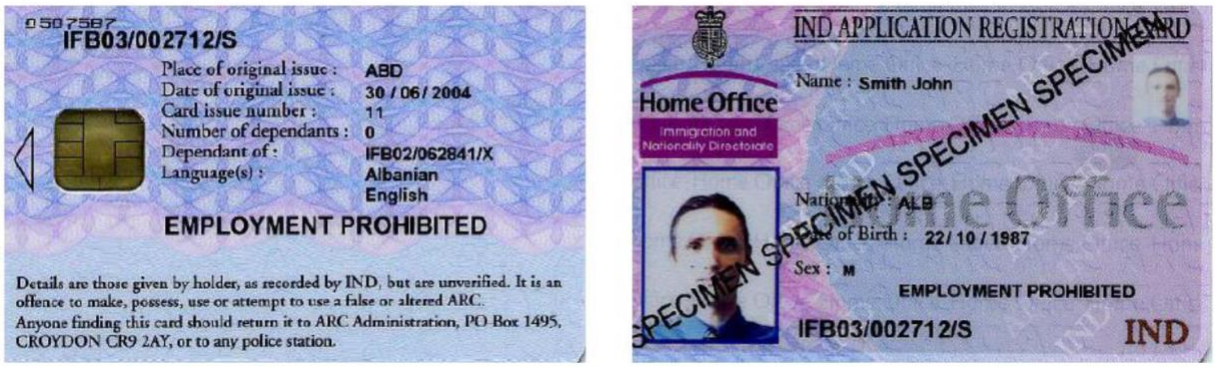
A2. Appendix Figures

Figure A1 - Employment bans and attitudes toward right to work of asylum seekers



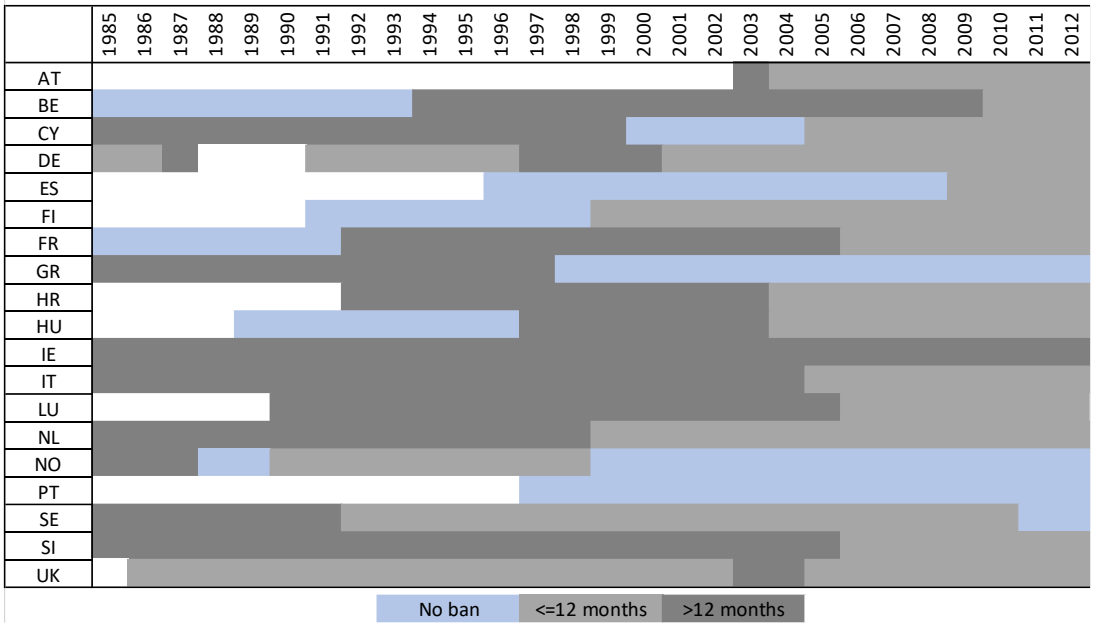
Notes. The figure plots the share of respondents agreeing or agreeing strongly with the statement that asylum seekers should be granted the right to work (vertical axis; European Social Survey 2002) against the duration (in months) of existing bans in 2002 (horizontal axis) in European countries. Red dots identify EU countries that in 2002 had indefinite employment bans for asylum seekers. Indefinite and no bans are coded as having duration of 24 and 0 months, respectively.

Figure A 2 – Specimen of identity card of asylum seekers in the UK



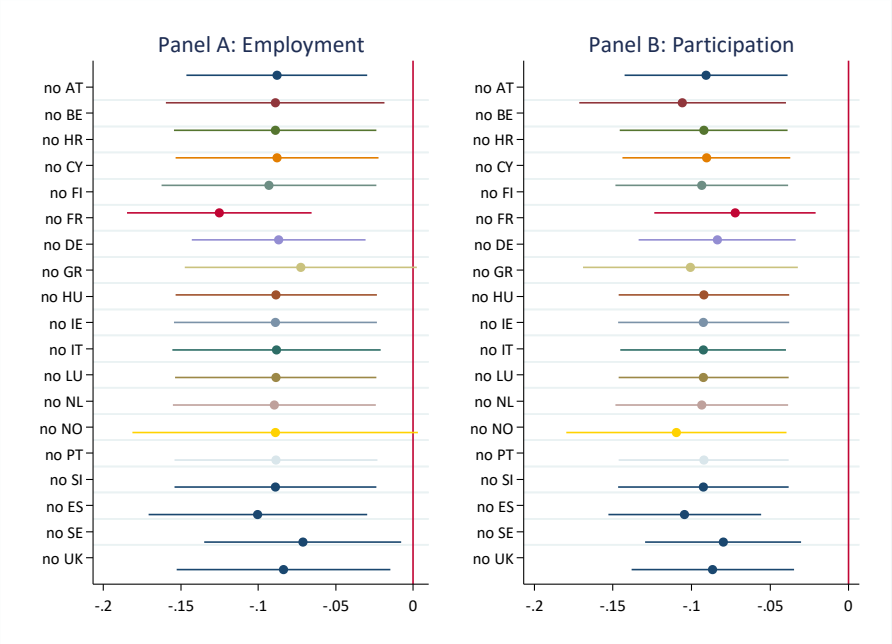
Source: UK Home Office.

Figure A 3 – Employment bans in EU countries (1985-2012)



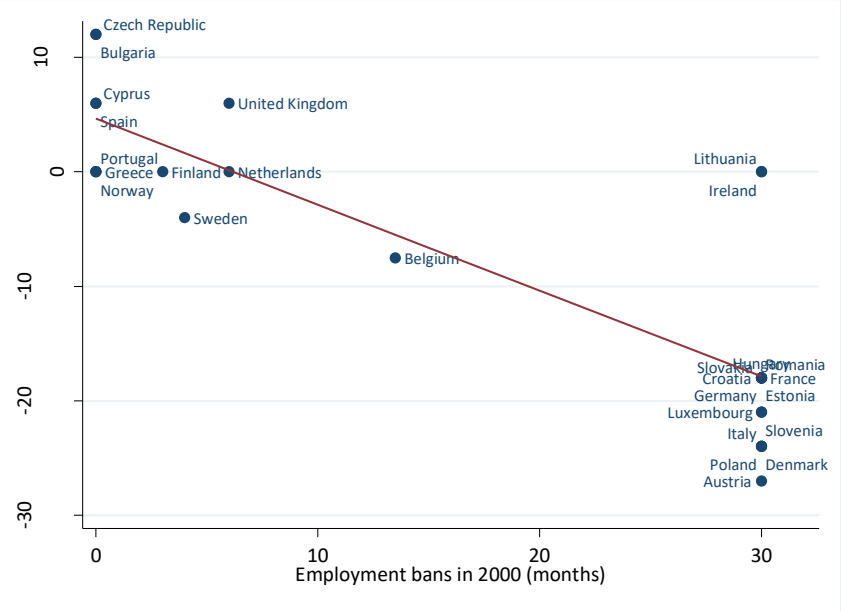
Notes. The figure shows the variation in temporary restriction to employment for asylum seekers for the countries in the estimating sample from 1985 to 2012. Country-year observations with no ban in place (asylum seekers allowed to take up employment right away) are in blue; those with employment ban shorter or equal than 12 months are in light grey; those with employment ban longer than 12 months are in dark grey. White cells indicate no data available.

Figure A 4 – Employment bans and labour market outcomes – Leave out one country



Notes. The Figure reports estimated coefficients (and 95% confidence intervals) of the effect of employment bans at the time of arrival on refugees' probability of employment (panel A) and participation (panel B). On each line, we exclude one host country at a time. All specifications include destination country by observation year fixed effects, individual characteristics (dummies for male, five–year age groups and educational level) and cohort of entry by host country fixed effects. Standard errors are clustered at the host country level.

Figure A 5 - Pre-"EU 2003 Directive" duration of employment bans (in 2000) and post-2003 Directive (2013-2001) change in employment bans – All EU countries



Notes. The graph reports a scatter plot of the change in employment ban durations between 2001 and 2013 (vertical axis) against the duration of employment bans in 2000 (horizontal axis) in European countries. We code indefinite employment bans as having a duration of 30 months.

A3. Appendix Tables

Table A 1 – Summary statistics on refugees in the EULFS sample

Male	59.8
Age	
25-39	36.6
40-54	50.4
55-64	13.0
Education	
Lower secondary	39.1
Upper secondary	37.0
Tertiary	23.9
Entry cohorts	
1985-1994	36.5
1995-2003	48.1
2004-2012	15.4
Area of origin	
New Member States & other Europe	35.5
North Africa and Middle East	27.0
Other Africa	18.9
South-East Asia	16.2
Latin America	2.4
Labour market outcomes	
Employment rate	58.5
Participation rate	70.9
Unemployment rate	17.5
Observations	4,242

Notes. For all refugees in our sample, the table reports: share of males; distribution by three age groups; educational distribution; distribution by broad entry cohorts; distribution by area of origin; employment, participation and unemployment rates. The sample comprises of refugees aged 25-64, arrived between 1985 and 2012, who spent at least two years in the host country and were surveyed in 2008 or 2014 in any of the 19 countries included in the analysis.

Table A 2 – Employment bans and labour market outcomes: triple DiD estimates

	Employment		**Participation**		**Unemployment**	
	(1)	(2)	(3)	(4)	(5)	(6)
Employment ban * Refugee	-0.062*	-0.081**	-0.074*	-0.089*	-0.006	0.007
	(0.034)	(0.034)	(0.040)	(0.043)	(0.041)	(0.038)
Observations	51,524	51,524	51,524	51,524	40,451	40,451
Host country x Year FE	X	X	X	X	X	X
Individual characteristics	X	X	X	X	X	X
Entry cohort x Origin area FE	X	X	X	X	X	X
Entry cohort x Refugee	X	X	X	X	X	X
Host country x Refugee	X	X	X	X	X	X
Indiv. Charact. x Refugee		X		X		X

Notes. The table reports estimates of the effects of employment ban at the time of arrival on refugees' employment (columns 1-2), participation (columns 3-4) or unemployment (columns 5-6) probabilities. Individual controls and FEs are defined as in all main tables. Standard errors (in parentheses) are clustered at the host country level: *** p<0.01, ** p<0.05, * p<0.1.

Table A 3 - Employment bans and labour market outcomes: continuous measure of ban (# months)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Panel A: Employment					
Empl. Ban: # Months	-0.003***	-0.018***	-0.002**	-0.016***	-0.002**	-0.014***	-0.014**	-0.014**	-0.010
	(0.001)	(0.005)	(0.001)	(0.005)	(0.001)	(0.004)	(0.005)	(0.006)	(0.007)
Empl. Ban: # Months Sq.		0.001***		0.000***		0.000***	0.000**	0.000**	0.000
		(0.000)		(0.000)		(0.000)	(0.000)	(0.000)	(0.000)
				Panel B: Participation					
Empl. Ban: # Months	-0.003**	-0.016***	-0.002**	-0.014***	-0.002*	-0.013***	-0.011***	-0.012**	-0.010*
	(0.001)	(0.006)	(0.001)	(0.005)	(0.001)	(0.004)	(0.004)	(0.005)	(0.005)
Empl. Ban: # Months Sq.		0.001**		0.000**		0.000**	0.000**	0.000**	0.000
		(0.000)		(0.000)		(0.000)	(0.000)	(0.000)	(0.000)
Observations	4,242	4,242	4,242	4,242	4,242	4,242	4,242	4,242	4,242
Max ban: # months	24	24	30	30	36	36	30	30	30
Host country x Year FE	X	X	X	X	X	X	X	X	X
Entry cohort FE	X	X	X	X	X	X	X	X	X
Individual characteristics							X	X	X
Origin area FE								X	
Entry cohort x Origin area FE									X

Notes. The table reports estimates of the effects of employment ban at the time of arrival on refugees' employment (panel A), and participation (panel B) probabilities. The "Empl. Ban: # Months" and "Empl. Ban: # Months Sq." variables measure the duration of the ban (if any) in months and in months squared, respectively. Indefinite bans are set equal to 24 (columns 1 and 2), 30 (columns, 3, 4, 7, 8 and 9) or 36 (columns 5 and 6) moths. No bans are coded as bans with zero month duration. Individual controls and FEs are defined as in all main tables. Standard errors (in parentheses) are clustered at the host country level: *** p<0.01, ** p<0.05, * p<0.1

Table A 4 – Robustness checks: additional FEs

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment		***Participation***		***Unemployment***	
Employment Ban	-0.088**		-0.088***		0.017	
	(0.034)		(0.023)		(0.055)	
Empl. ban: up to 12 months		-0.091**		-0.081***		0.029
		(0.037)		(0.027)		(0.057)
Empl. Ban: 13+ months		-0.087**		-0.091***		0.013
		(0.033)		(0.024)		(0.056)
Observations	4,242	4,242	4,242	4,242	3,110	3,110
Host country x Year FE	X	X	X	X	X	X
Individual characteristics	X	X	X	X	X	X
Entry cohort x Origin area FE	X	X	X	X	X	X
Individ.charact. x Origin area FE	X	X	X	X	X	X

Notes. The table reports estimates of the effects of employment ban at the time of arrival on refugees' employment (columns 1-2), participation (columns 3-4) or unemployment (columns 5-6) probabilities. In odd columns, "Employment Ban" is a dummy equal to one for an employment ban being in place in the destination country. In even columns, "Empl. Ban: up to 12 months" and "Empl. Ban: 13+ months" are dummies equal to one if the employment bans lasted at most 12 months or at least 13 months, respectively; the excluded category is "no employment ban". Individual controls and FEs are defined as in all main tables. Standard errors (in parentheses) are clustered at the host country level: *** p<0.01, ** p<0.05, * p<0.1

Table A 5 – Robustness checks: Wild Cluster Bootstrap

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employment				**Participation**			
Employment Ban	-0.125***	-0.113***	-0.124***	-0.089***	-0.128**	-0.106***	-0.114**	-0.092***
	(0.035)	(0.035)	(0.037)	(0.031)	(0.045)	(0.036)	(0.041)	(0.026)
	Wild cluster bootstrap p-values (null imposed):							
Weights (# replications):								
Webb (999)	0.04	0.04	0.02	0.05	0.04	0.05	0.05	0.04
Webb (9999)	0.04	0.03	0.03	0.05	0.05	0.04	0.04	0.03
Rademacher (999)	0.05	0.04	0.03	0.07	0.04	0.04	0.04	0.03
Rademacher (9999)	0.05	0.04	0.02	0.06	0.05	0.04	0.04	0.03
Observations	4,242	4,242	4,242	4,242	4,242	4,242	4,242	4,242
Host country x Year FE	X	X	X	X	X	X	X	X
Entry cohort FE	X	X	X		X	X	X	
Individual characteristics		X	X	X		X	X	X
Origin area FE			X				X	
Entry cohort x Origin area FE				X				X

Notes. The table reports our main estimates for the effect of employment ban on employment (columns 1-4) and participation (columns 5-8) with standard errors clustered at the host country level (19 clusters; see Table 1) and p-values obtained with wild cluster bootstrap (with null imposed and alternative weights and number of replications). The p-values are estimated using the STATA package *boottest* developed by Roodman et al. (2019). Individual controls and FEs are defined as in all main tables.

Table A 6 – Restricting sample to cohorts arrived just before/after policy changes

	(1)	(2)	(3)	(4)	(5)
			Panel A: Employment		
Employment Ban	-0.083**	-0.084**	-0.094**	-0.109***	-0.048
	(0.033)	(0.025)	(0.035)	(0.017)	(0.027)
			Panel B: Participation		
Employment Ban	-0.081*	-0.080**	-0.091**	-0.061*	-0.080**
	(0.043)	(0.031)	(0.037)	(0.031)	(0.031)
# Cohorts before/after policy change	3	3	3	2	1
Observation	1,123	1,123	1,123	967	561
Host country x Year FE	X	X	X	X	X
Entry cohort FE	X	X	X	X	X
Individual characteristics		X	X	X	X
Origin area FE			X	X	X

Notes. The table reports estimates of the effects of employment bans at the time of arrival on refugees' probability of employment (panel A) and participation (panel B). We restrict the sample to 3 (columns 1-3), 2 (column 4) and 1 (column 5) cohort(s) before and after each policy change (i.e. introduction or removal of an employment ban) in our sample. Individual characteristics and fixed effects are defined as in previous tables. Standard errors (in parentheses) are clustered at the host country level: *** p<0.01, ** p<0.05, * p<0.1

Table A 7 – Instrumental variable: balancing test

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Indefinite ban in 2000				**Ban duration in 2000 (months)**			
ln(population)	-0.013			-0.009	-0.031			0.453
	(0.089)			(0.115)	(2.467)			(3.218)
Unemployment rate		0.005		0.014		0.044		0.470
		(0.037)		(0.045)		(1.085)		(1.259)
ln(GDP pc)		-0.145		-0.104		-3.452		-2.632
		(0.245)		(0.286)		(7.092)		(8.364)
Asylum seeekers (per 10,000 pop.)			0.003	0.005			0.202	0.306
			(0.009)	(0.011)			(0.211)	(0.250)
Refugees (per 10,000 pop.)			-0.002	-0.002			-0.058	-0.047
			(0.002)	(0.003)			(0.060)	(0.067)
Observations	19	19	19	19	19	19	19	19

Notes. The table reports estimates from regressing employment restrictions in 2000 (pre-EU directive) on contemporaneous country characteristics. The outcome variable is a dummy equal to one if an EU country had an indefinite ban (columns 1-4) or the ban duration (in months; column 5-8). Sample: 19 countries in our main EULFS sample. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table A 8 - First stage regressions

	(1)	(2)	(3)	(4)
	Empl. Ban: # Months		**Empl. Ban: # Months Sq.**	
Months ban_2000* entry cohort 2004	0.291	0.188	6.491	2.821
	(0.289)	(0.314)	(9.807)	(10.296)
Months ban_2000* entry cohort 2005	0.314	0.180	8.195	4.083
	(0.261)	(0.246)	(9.021)	(8.135)
Months ban_2000* entry cohort 2006	-0.203	-0.296**	-11.739***	-14.679***
	(0.119)	(0.125)	(2.826)	(3.082)
Months ban_2000* entry cohort 2007	-0.330**	-0.441***	-16.449***	-19.470***
	(0.126)	(0.144)	(2.860)	(3.584)
Months ban_2000* entry cohort 2008	-0.214	-0.233	-11.822	-12.039
	(0.283)	(0.276)	(11.089)	(10.576)
Months ban_2000* entry cohort 2009	-0.386*	-0.389**	-19.982**	-19.708***
	(0.194)	(0.149)	(7.470)	(5.461)
Months ban_2000* entry cohort 2010	-0.539***	-0.426***	-18.900***	-18.883***
	(0.171)	(0.118)	(2.807)	(3.346)
Months ban_2000* entry cohort 2011	-0.177	-0.309**	-15.184***	-17.503***
	(0.115)	(0.119)	(2.433)	(3.367)
Months ban_2000* entry cohort 2012	-0.383**	-0.418**	-17.748***	-18.812***
	(0.156)	(0.153)	(3.016)	(3.024)
Observations	3,665	3,665	3,665	3,665
Partial F-stat	183.7	162.4	435.1	182.5
Max ban: # months	30	30	30	30
Host country x Year FE	X	X	X	X
Entry cohort FE	X	X	X	X
Individual characteristics		X		X
Entry cohort x Origin area FE		X		X

Notes. The table reports First Stage estimates of employment ban measures on a full set of interactions between post-2003 entry cohort dummies and pre-directive restrictions measured in 2000. The "Empl. Ban: # Months" (column 1-2) and "Empl. Ban: # Months Sq." (columns 3-4) variables measure the duration of the ban (if any) in months and in months squared, respectively. Indefinite bans are set equal to 30 months while no bans are coded as bans with zero months duration. Partial F-statitics for the excluded instruments are reported below the number of observations. Individual controls and FEs are defined as in previous tables. The sample exclude observations from Ireland and the UK. Standard errors (in parentheses) are clustered at the host country level: *** p<0.01, ** p<0.05, * p<0.1

Table A 9 – Effect on refugee migration flows

	Asylum applications (ln)			Yearly change in refugee stock (ln)			Share of males	Share of aged 14-34	Share of aged 35-64
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Employment Ban	-0.176*	-0.172	-0.173	-0.007	0.008	-0.006	0.006	0.001	0.000
	(0.094)	(0.111)	(0.118)	(0.042)	(0.050)	(0.043)	(0.013)	(0.018)	(0.016)
Observations	505	496	495	543	517	515	184	184	184
Country & year FE	X	X	X	X	X	X	X	X	X
Country controls		X			X		X	X	X
Country controls (1-year lag)			X			X			

Notes. This table explores the potential effects of employment bans on migration flows and their composition. Employment Ban is a dummy equal one for an employment ban (the length of employment ban larger than zero) being in place in the destination country at time *T*. In columns 1-6 the country-year sample is formed by the 19 countries from the main analysis, from 1985 until 2017. Country controls include unemployment rate, log GDP, and a left-right index (going from 0 to 10) of government ideology. Standard errors are clustered at the country level. Regressions in columns 7-9 are estimated on a country-year dataset formed by the 19 countries from the main analysis, from 2008 (first year in which Eurostat data on gender and age group specific asylum applications are available) until 2017. Country controls include unemployment rate, log GDP, and a left-right index (going from 0 to 10) of government ideology. Standard errors are clustered at the country level: *** p<0.01, ** p<0.05, * p<0.1

Table A 10 - Quantifying the effect of employment bans during the refugee crisis – Years 0 to 7

Country	New refugees $(a_c \times s_c)$	Earnings w_c	Ban b_c	Output Y_c^{BAN}	Potential Output Y_c^{NOBAN}	Loss DY_c
Austria	65,848	28,912	1	2,891	5,387	-2,496
Belgium	39,642	34,702	1	2,089	3,893	-1,803
Bulgaria	15,965	3,245	1	79	147	-68
Croatia	182	7,558	1	2	4	-2
Cyprus	2,235	14,323	1	49	91	-42
Czech Republic	989	7,304	1	11	20	-9
Denmark	22,296	39,872	1	1,350	2,516	-1,165
Estonia	210	8,562	1	3	5	-2
Finland	8,299	31,899	1	402	749	-347
France	49,124	23,106	1	1,724	3,212	-1,488
Germany	733,638	23,571	1	26,261	48,932	-22,671
Greece	8,878	15,043	0	378	378	0
Hungary	800	6,435	1	8	15	-7
Ireland	612	31,968	1	30	55	-26
Italy	83,082	23,680	1	2,988	5,567	-2,579
Latvia	148	6,762	1	2	3	-1
Lithuania	278	5,258	1	2	4	-2
Luxembourg	1,521	33,335	1	77	143	-66
Malta	3,159	14,702	1	71	131	-61
Netherlands	44,039	26,218	1	1,753	3,267	-1,514
Norway	18,842	47,128	0	2,513	2,513	0
Poland	784	6,825	1	8	15	-7
Portugal	728	10,070	0	21	21	0
Romania	1,480	3,722	1	8	16	-7
Slovakia	157	6,908	1	2	3	-1
Slovenia	246	14,541	1	5	10	-5
Spain	18,715	18,049	1	513	956	-443
Sweden	111,880	35,657	0	11,288	11,288	0
Switzerland	28,229	53,290	1	2,284	4,257	-1,972
United Kingdom	25,248	23,638	1	906	1,689	-782
Europe	1,287,255			57,717	95,285	-37,568

Notes. Each column of this table reports: *New refugees*, the estimated number of new refugees arrived in 2015-2016 (obtained as the product of the number of asylum applications and the probability of success in first-instance); *Earnings*, the average earnings of employed refugees in the country (assumed to be the average earnings of employees in elementary occupations); *Ban*, a dummy for presence of a ban on employment of asylum seekers in 2015-2016; *Output*, the estimated output produced by employed new refugees over their first eight years in the host country (in million euros); *Potential output*, the output we estimate new refugees would have produced in the absence of the ban (in million euros); *Loss*, the output loss due to the ban, computed as the difference between *Potential output* and *Output* (in million euros).