

Ahrens, Achim; Beerli, Andreas; Hangartner, Dominik; Kurer, Selina;
Siegenthaler, Michael

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The Labor Market Effects of Restricting Refugees' Employment Opportunities

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DISCUSSION PAPER SERIES

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Achim Ahrens
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Achim Ahrens

ETH Zurich

Andreas Beerli

ETH Zurich and IZA

Dominik Hangartner

ETH Zurich

Selina Kurer

ETH Zurich

Michael Siegenthaler

ETH Zurich and IZA

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

The Labor Market Effects of Restricting Refugees' Employment Opportunities*

Refugees, and immigrants more generally, often do not have access to all jobs in the labor market. We argue that restrictions on employment opportunities help explain why immigrants have lower employment and wages than native citizens. To test this hypothesis, we leverage refugees' exogenous geographic assignment in Switzerland, within-canton variation in labor market restrictions, and linked register data 1999–2016. We document large negative employment and earnings effects of banning refugees from working in the first months after arrival, from working in certain sectors and regions, and from prioritizing residents over refugees. Consistent with an effect of outside options on wages, removing 10% of jobs reduces refugees' hourly wages by 2.8% and increases the wage gap to similar host-country citizens in similar jobs by 2.2%. Furthermore, we show that restrictions reduce refugees' earnings even after they cease applying. Restrictions do not spur refugee emigration nor improve earnings of non-refugee immigrants.

JEL Classification: J08, J31, J42, J61, J68

Keywords: labor market integration, migration, labor market policies, labor market institutions, monopsony, refugees, employment, wages, outside options, employment opportunities

Corresponding author:

Michael Siegenthaler
KOF Swiss Economic Institute
ETH Zurich
Leonhardstrasse 21
8092 Zurich
Switzerland
E-mail: siegenthaler@kof.ethz.ch

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1 Introduction

Immigrants, and in particular refugees, typically have lower employment rates and wages than otherwise similar native citizens.¹ Reasons for the gap in labor market performance are manifold and include differences in skills, abilities, preferences, and discrimination. Another potential contributing factor is policies that restrict immigrants' employment opportunities. Such policies are running the gamut from restrictions on work permits and visa rules (Kerr, Kerr, and Lincoln, 2015; Naidu, Nyarko, and Wang, 2016; Wang, 2021), prioritisation of nationals, incomplete recognition of educational certificates (Brücker et al., 2021), occupational licensing (Cassidy and Dacass, 2021), random assignment of refugees to thin labor markets (Åslund, Östh, and Zenou, 2010), to employment bans (Marbach, Hainmueller, and Hangartner, 2018; Fasani, Frattini, and Minale, 2021). The impact of these policies is potentially severe for immigrants and costly for host communities. This paper argues—and provides evidence—that these policies reduce employment and earnings while they apply and also have longer-term repercussions due to scarring effects. Furthermore, we show that the removal of potential jobs from immigrants' choice set suppresses wages. In doing so, we highlight and empirically validate the central role of outside options in wage determination as predicted by imperfect labor market models such as monopsonistic and search and bargaining models.

Identifying the impact of these policies is challenging since it requires a causal research design building on exogenous policy variation, while accounting for the sorting of immigrants across locations. Leveraging Switzerland as a laboratory providing subnational policy variation, this paper traces the short- and longer-term effects of four of the most widespread labor market restrictions for refugees: (i) prioritization, which grants residents priority on the labor market, (ii) sector restrictions, which regulate that refugees can only work in certain economic sectors, (iii) geographical restrictions, which prevent refugees from working in some or all neighboring regions, and (iv) temporary employment bans for the initial period after arrival. Three institutional features conspire to facilitate the identification of the policy effects. First, cantons have considerable discretion in setting policies restricting labor market access, which generates substantial policy variation across and within cantons. Second, refugees are, conditional on (mostly) observable information, exogenously assigned to cantons shortly after arrival. Furthermore, most refugees are, by law, forced to stay in the canton to which they were assigned, depending on their status and timing of the asylum decision for five or even more years. Thus, refugees do not have the option to sort into cantons with less restrictive labor regulations. While the exogenous assignment is not a necessary condition for identifying the impact of within-canton policy changes, the dispersal policy facilitates placebo tests that confirm that changes in labor market restrictions are orthogonal to the composition of the local refugee population. Third, we can leverage the timing of asylum decision and

¹See, e.g., Brell, Dustmann, and Preston, 2020.

their implications for labor market access when asylum seekers obtain refugee or subsidiary protection status. To alleviate endogeneity concerns that the timing of status change is not orthogonal to refugees' labor market potential, we compare refugees in the same arrival cohort undergoing the same status change.

Our empirical analyses combine original data on cantonal labor market policies covering the period 1999–2016 with detailed administrative data linking asylum profiles and earnings histories, as well as employer-employee data on workers' wages and hours worked. Using individual-level panel regression models, we relate refugees' labor market outcomes to the policies of the cantons to which they were initially assigned. Our results document that all four policies depress refugees' employment rate and total labor earnings while they apply. Together, the cumulative effects of the policies are substantial in size: our estimates imply that moving from an unrestricted to the most restrictive cantonal policy mix observed in our data is associated with a reduction from 19% to 11% in affected refugees' average employment rate. This reduction is more than four times larger than our model estimates for the increase in the unemployment rate during the Great Recession. Benchmarked against the benefits of language training, the eight percentage point difference is at least as large as the employment effects of extensive language programs offered to refugees in Denmark (see Foged, Hasager, and Peri, 2022 and Arendt, Dustmann, and Ku, 2022).

We then analyze whether restricting employment opportunities reduces refugees' absolute wages and relative wages compared to native citizens. For this, we use matched employer-employee data from the Swiss Earnings Structure Surveys (SESS) covering 2012–2018, which provide employer-reported information on workers' monthly wages and monthly hours. While prioritizing residents over refugees lowers monthly earnings and monthly hours, we find no evidence for an effect on hourly wages. In contrast, sectoral and regional restrictions reduce refugees' pay substantially. Refugees' hourly wages are 2.8% lower in cantons where the restrictions remove 10% of their potential jobs. The effect remains almost unchanged if we augment the wage regressions with a rich set of worker, firm, and job characteristics, implying that restrictions cause wage gaps between observationally similar refugees in similar jobs. We furthermore find that the policies help explain why refugees are paid less than similar native citizens. In particular, the refugee-native wage gap is at least 2% larger in cantons where sector and region restrictions remove 10% of refugees' potential jobs.

We use rich contextual data about workers and their jobs to illuminate why sector and region restrictions decrease refugee wages, and to adjudicate between different labor market models. We find surprisingly little evidence that the policy effects on wages can be explained by a restrictions-induced reduction in refugees' productivity. We find no evidence that sector and region restrictions force refugees to work in low-paying industries, regions, or occupations. An increase in skill mismatch—for example, because refugees have to work in jobs for which they are overqualified—does not seem to explain the findings either. To the contrary, we find

that firms, if anything, dismiss workers when restrictions increase, which is consistent with the idea that they keep the best matches. We can also rule out that the wage effects stem from refugees having fewer opportunities to learn on the job or accumulate work experience in Switzerland.

Instead, the preponderance of the evidence suggests that sector and region restrictions decrease refugees' hourly pay because they reduce outside options. This finding resonates with several leading labor market models (see Caldwell and Harmon, 2019, for a discussion) which postulate that workers who can work in many jobs benefit from higher wages and increased mobility. We begin our investigation by demonstrating that the restrictive policies indeed reduce refugees' opportunities to work in many jobs. Based on the commuting patterns of non-refugees and the sectoral composition of refugees in unrestricted cantons, we estimate that sector and region restrictions can remove up to two-thirds of refugees' potential jobs. Empirically, the reduced outside options manifest across several dimensions: regional restrictions increase employer concentration at the cantonal level; and sectoral and regional restrictions, as well as prioritisation of residents, substantially reduce refugees' employment rates and chances to change employers voluntarily.

We then explore three mechanisms to illuminate how restricting refugees' outside options affects wages. According to monopsonistic models of the labor market, sector and regional restrictions increase firms' power to post low wages to refugees because workers with fewer potential employers respond less to changes in wages (e.g., Manning, 2003; Ashenfelter, Farber, and Ransom, 2010; Card et al., 2018). Following a recent empirical literature (see Sokolova and Sorensen, 2021, for an overview), we approximate how responsive refugees are to wage changes by estimating their wage elasticity of quits. We find that quits are substantially less responsive to wages in cantons where policies are restrictive, consistent with greater wage-setting power for firms. There is less support for other mechanisms through which outside options affect wages. In particular, sector and regional restrictions do not appear to depress wages by reducing the opportunities for employed refugees to climb up the wage ladder (e.g., Manning, 2003), nor do they affect on-the-job wage growth. These results contrast with models that posit that fewer outside options hamper workers' bargaining position in on-the-job wage renegotiations (e.g., Cahuc, Postel-Vinay, and Robin, 2006).

Finally, we take initial steps toward assessing the costs and benefits of labor restrictions and present three pieces of evidence that suggest that labor restrictions burden both refugees and host communities—without measurable benefits. First, we document that restrictive labor market policies impair refugees' economic integration not only in the short but also medium term. In line with an extensive literature showing that adverse initial labor market conditions can leave long-term scars (see Marbach, Hainmueller, and Hangartner, 2018, Fasani, Frattini, and Minale, 2021; for an overview, see Von Wachter, 2020), the priority and blocking policies reduce refugees' labor market earnings for up to three years after they

cease applying. These medium-run effects contribute to explaining why refugees earn less than comparable resident workers. Second, our estimates suggest that labor restrictions likely have no—or, if anything, a negative—effect on the propensity to emigrate. This finding also applies to refugees that obtain only subsidiary protection and a temporary residence permit subject to frequent renewal. Third, analyzing the monthly earnings of low-paid EU-15 immigrants, there is no evidence that restrictive refugee policies measurably improve labor market outcomes for this competing group of workers.

Our findings contribute to four strands of literature. First, our study relates to the rich literature evaluating how host country policies shape refugees’ economic integration in terms of scope and outcome measures (see Brell, Dustmann, and Preston, 2020; Dustmann, Landerso, and Hojsgaard Andersen, 2021; Foged, Hasager, and Peri, 2022, for recent overviews). Previous studies have analyzed the effects of the geographic dispersal of refugees upon arrival (Edin, Fredriksson, and Åslund, 2004; Damm, 2009; Bansak et al., 2018; Dagnelie, Mayda, and Maystadt, 2019; Martén, Hainmueller, and Hangartner, 2019), the recognition of educational certificates (Brücker et al., 2021; Anger, Bassettoy, and Sandner, 2022), the generosity of social assistance (LoPalo, 2019; Dustmann, Landerso, and Hojsgaard Andersen, 2021), and temporary employment bans (Marbach, Hainmueller, and Hangartner, 2018; Fasani, Fratini, and Minale, 2021). We provide evidence on the labor market effects of four different labor regulations that constrain refugees’ labor market access in distinct ways. Three of these policies—sector, region and priority restrictions—have received little attention, despite their popularity across Europe and beyond. Our linked register data allow us to explore the policy effects on a range of often unexplored outcome variables including refugees’ hourly wages, hours worked, separations and job mobility, educational attainment, and emigration.

Second, our paper contributes to the growing empirical literature on the relevance of outside job opportunities for wage setting and job mobility. Identifying the causal effect of outside options is very challenging, both because workers’ outside options are typically unobserved and because factors that change workers’ outside options—such as receiving an MBA or a shift in the demand for their skills—likely affect workers’ productivity in the current jobs. Previous studies analyze the role of outside options by exploiting changes in workers’ information about outside job offers (Caldwell and Harmon, 2019), variation in outside options due to varying industry-specific employment trends (Beaudry, Green, and Sand, 2012; Caldwell and Danieli, 2021), wage changes in secondary jobs for dual jobholders (Lachowska et al., 2021), and changes in the enforceability of non-compete agreements (Johnson, Lavetti, and Lipsitz, 2020). These studies consistently find that fewer outside options reduce workers’ earnings, wages, and job mobility.²

We advance this literature by exploiting a close to ideal natural experiment. Cantons that

²In addition, Jäger et al. (2021) show that workers wrongly anchor their beliefs about outside options on their current wage.

impose geographic or sector restrictions on refugees' labor market cause large, observable, and exogenous shifts in workers' outside options that are plausibly unrelated to workers' productivity in their current job in the short run. Our results provide strong evidence that the restrictions' effects on outside options matter for wages and job mobility, and are also key to explaining why refugees have lower wages than observationally equivalent resident workers. These findings are consistent with the long-standing hypothesis that outside options explain wage gaps between otherwise exchangeable workers.³

Third, we add to the rich and expanding literature on the relevance of monopsonistic competition in modern labor markets (see Manning, 2021, for an overview). Within this literature, our paper is most closely related to studies that analyze immigrant labor markets (see Manning, 2021, for a discussion). A related study is Naidu, Nyarko, and Wang (2016), who analyze a visa reform that made it easier for guest workers in the United Arab Emirates to switch employers when their first visa expired. In line with predictions of monopsony models, the study finds that increasing labor market competition increased guest workers' earnings and employer retention, primarily because of reduced return migration. Similar findings are presented by Gupta (2022), who documents that larger job-switching frictions for Indian and Chinese immigrants reduced inter-firm job mobility and increased firm value.⁴ Our paper adds to this literature by exploiting exogenous variation in workers' job opportunities. In line with key predictions from monopsony models, we find evidence that refugees' wage elasticity of quits is lower if they are assigned to a restrictive canton.

Fourth, our long-run analyses also contribute to the literature on the scarring effects of adverse initial labor market conditions. Existing studies provide evidence that entering the labor market in a recession may have lasting negative consequences for employment and wages (Von Wachter, 2020, provides an overview). These effects are particularly pronounced for immigrants whose medium- and long-run economic outcomes are shaped by labor market conditions at arrival (Aslund and Rooth, 2007; Azlor, Damm, and Schultz-Nielsen, 2020). Most closely to our study, Marbach, Hainmueller, and Hangartner (2018) and Fasani, Fratini, and Minale (2021) document that temporal employment bans impair refugees' economic integration for years after they ceased applying. Our results support the finding of long-term repercussions of temporary employment bans and advance existing research by documenting similar scarring effects from policies that prioritize residents.

³See, for instance, Black, 1995, Hirsch and Jahn, 2015, Amior and Manning, 2020, and Manning, 2021.

⁴Depew, Norlander, and Sørensen (2017) and Wang (2021) study (skilled) temporary visa holders in the US that face legal restrictions to change employers. These studies present evidence suggesting that the job mobility of visa holders is depressed but too large to support the notion that visa holders are effectively tied to their employers.

2 Labor market access for refugees

2.1 Labor market restrictions for refugees in Europe

The vast majority of European countries restrict labor market access for refugees in one way or another. These restrictions can span several dimensions. Particularly popular among European policymakers are temporary employment bans that completely prevent employment for asylum seekers and refugees for the first few months after arrival (Fasani, Frattini, and Minale, 2021). Marbach, Hainmueller, and Hangartner (2018) document a median length of employment bans in Europe of six months, but also considerable heterogeneity, ranging from 1 day in Sweden to an infinite ban in Ireland before 2019.

A second, widespread restriction is the prioritization of other workers, either citizens and foreign nationals with more secured residence permits, or immigrants originating from other EU/EFTA countries, over asylum seekers when filling vacant jobs. The EU Receptions Condition Directive (Art. 15) explicitly leaves room for the posteriorization of asylum seekers (but not refugees) vis-à-vis aforementioned groups and such prioritization policies are used by several countries including Austria, Germany, and Switzerland (ECRE, 2020). While the implementation and enforcement of such prioritization policies vary, they often require firms to either provide proof that they made an effort to hire among the prioritized groups and/or that they registered the job advertisement with local employment offices. Note that such prioritization of resident or citizen workers is not unique to Europe, nor to refugees. For example, Clemens (2022) documents the effects of the US seasonal employment visa for low-skilled farm work (H-2A) which only allows employers to hire immigrants if they prove significant efforts to fill the position with US workers.

A third dimension imposes restrictions on which sectors or occupations asylum seekers and refugees are allowed to work in. Similar restrictions exist for example in Austria, where a 2004 ordinance restricted asylum seekers' labor market access to agriculture, forestry, and tourism; France, where each region has its own list of permissible occupations; or the U.K., which operates a narrow and highly specific list of unrestricted shortage occupations, (ECRE, 2020).

Fourth, many European countries including Denmark, Germany, Norway, Sweden, Switzerland, and the Netherlands, use dispersal policies to allocate asylum seekers and refugees to host localities. These regional assignments are often explicitly enforced by preventing asylum seekers from moving between localities or working outside of the assigned labor market region. They could also be implicitly enforced by tying the provision of housing to the assigned locality. In either case, the combination of dispersal policies with moving restrictions can have the same detrimental consequences as regional restrictions for those assigned to thin labor markets with few job opportunities. Evidence for this is provided by Åslund, Östh, and Zenou (2010), who leverage the Swedish dispersal policy to show that assignment to locations with

poor job access reduces refugee employment and earnings for up to ten years. Explicit or implicit mobility restrictions exist in the US, too, both for refugee and non-refugee migrants.⁵

2.2 Switzerland—a Laboratory to Study Labor Restrictions

Over the last two decades, Swiss cantons adopted each and all of the labor market restrictions observed at the international level discussed above: (i) temporary employment bans, (ii) prioritization of the resident population over refugees, (iii) sector restrictions and (iv) restrictions on geographic mobility combined with a geographic dispersal policy. While national legislation provides a common framework for labor market access for asylum seekers and refugees, the 26 cantons have considerable authority over the four policy dimensions. This high degree of federalism leads to substantial policy variation both across cantons and over time. As a consequence, the subnational policy variation observed in Switzerland can serve as a magnifying glass to study the short and longer-term ramifications of labor market restrictions for refugees in a context where most of the usual heterogeneity plaguing cross-country comparisons is held constant by design.

Dispersal policy. The identification of the policy effects is further aided by the largely exogenous assignment of refugees to cantons. A few weeks into the asylum process, the Swiss State Secretariat of Migration (SEM) conducts the cantonal placement solely based on the information provided in the SEM’s Central Migration and Information System (ZEMIS) and without any personal interaction between the placement officer and the asylum seeker (see Martén, Hainmueller, and Hangartner, 2019, for details). The allocation does not take into account the preferences of the asylum seeker except for a narrow and clearly defined set of reasons—namely pre-existing first-degree family networks, health issues that require treatment in a particular hospital, or the accommodation of unaccompanied minors (who are excluded from our analysis). Since we also have access to the ZEMIS database, we can control the same information observed by the placement officers, except for a free text field containing (sensitive) information about the above-mentioned reasons for deviating from the exogenous assignment. However, these exceptions are relatively rare and, more importantly, are not granted to facilitate or discourage labor market integration for individual cases (Martén, Hainmueller, and Hangartner, 2019).

We can empirically substantiate the exogeneity of assignment by regressing asylum seekers’ sociodemographic characteristics (measured at the time of arrival) on indicators for each canton, controlling for nationality, cohort, and processing center. Note that this is a hard test

⁵For example, the practice of U.S. resettlement organizations to haphazardly assign refugees to localities can have similar consequences as explicit dispersal policies in a context where secondary migration out of suboptimal locations is low (Bansak et al., 2018). For economic migrants, the firm-sponsored H1-B and L-1 visa programs in the U.S., which “tie” workers’ residency to their employers, may have similar lock-in effects with negative consequences for wages (Kerr, Kerr, and Lincoln, 2015).

since the assignment is only assumed to be exogenous *conditional* on additional characteristics provided in ZEMIS such as age and gender that we control for in our main analysis. Figure J.1 shows the results for five key refugee characteristics associated with economic integration: age, gender, religion (Christian and Muslim), and marital status. For all outcomes, we find that most characteristics are fairly balanced across assigned cantons. The only exceptions that stand out are the very small cantons (Glarus, Obwalden, Nidwalden, and Uri). The allocation key assigns less than 1% of asylum seekers to these cantons. While exogenous assignment through placement officers minimizes the ability of refugees to endogenously sort into cantons with favourable labour market policies, it is important to note that this feature is not crucial for identifying the policy effects. Since all regression models control for canton fixed effects, and in the more restrictive specifications even for individual fixed effects, all we need for identification is that the composition of refugees does not endogenously co-vary with within-canton policy changes. We provide empirical support for this assumption in Section 4.

Asylum process and permits. To build a panel dataset on the four labor market policies 1999–2016, members of the research team coded cantonal policies from publicly available sources (cantonal laws and published guidelines). We then verified and validated the entries for each policy, canton, and year with representatives of the relevant cantonal ministries.⁶ Most of these cantonal policies also vary by the different protection statuses that refugees can obtain. Four residence permits are relevant: During the asylum process, asylum seekers hold the residence permit N. If the asylum seeker receives subsidiary protection, they either obtain the status of a temporarily admitted foreigner (TAF) or temporarily admitted refugee (TAR). If they are recognized as refugees according to the Refugee Convention, they obtain a B permit.

The average length of the asylum process from submitting an application to obtaining a decision is about two years during our study period (Hainmueller, Hangartner, and Lawrence, 2016). While holding the N permit, asylum seekers are only allowed to live and find work in the assigned canton. The settlement restriction also applies to TAF and TAR and is only lifted for refugees with a B permit. However, given the length of the process, we might expect that lock-in effects lead to low levels of inter-cantonal movements even after mobility restrictions are lifted. This expectation is empirically substantiated by the analysis presented in Appendix Figure A.1, which shows that eight years after arrival, between 87% (B refugees) and 92% (TAR and TAF) still reside in the initially assigned canton. This suggests that the policies

⁶We coded a change to a certain cantonal policy as “highly reliable” (roughly 50% of all policy changes) if the information was confirmed by a law text, public internet resources, or two experts. If the information about a cantonal policy change remained unspecific (e.g., the exact date of the policy change was not confirmed) we classified it with the label “low reliability” (roughly 25% of all policy changes). The remaining policy changes were assessed to be of “normal reliability” (roughly 25% of all policy changes). Table E.3 shows that our main results are similar if we allow for separate effects of policy changes labeled as low reliability.

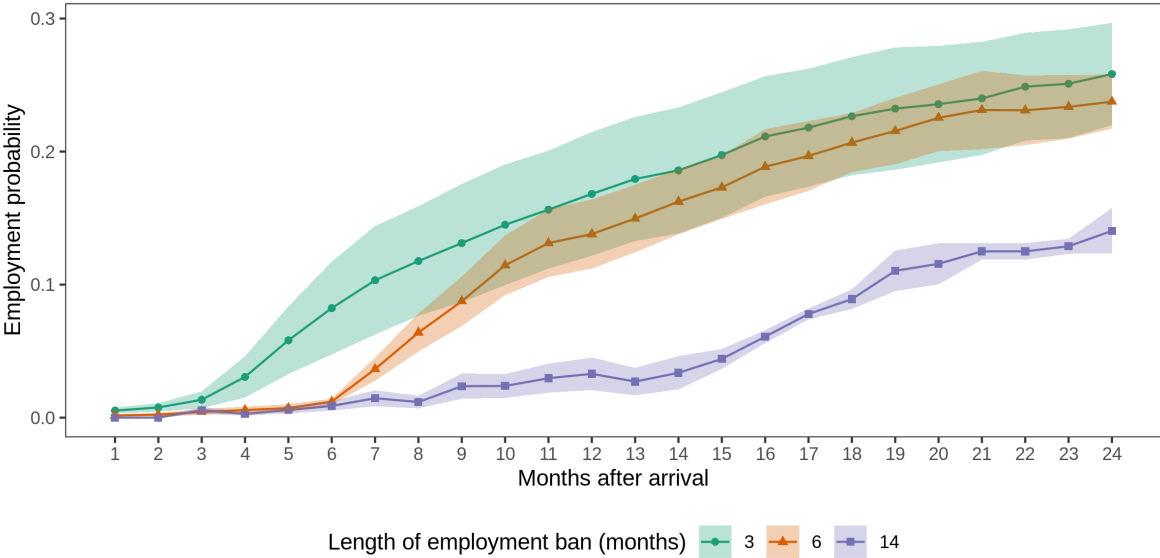
regulating labor market access in the canton of initial assignment can have a persistent impact on refugees’ economic integration trajectories.

Labor market restrictions. For all four asylum status groups, we coded the following policies from 1999–2016:

(i) *Employment ban.* Asylum seekers and refugees are banned from work for the first three months by national law, but cantons can extend this ban unilaterally. Between 09/01/1999 and 08/31/2000, the Swiss government issued a one-year work ban for asylum seekers who arrived after September 1999. Figure 2 (b) illustrates that several cantons extended the minimal three-month ban, increasing it to six months (e.g., Glarus, Nidwalden, Uri, Zug) or even fourteen months (Solothurn) for some years (until 2006) during our study period. In contrast to other policies discussed below, the minimal three-month ban does not depend on the refugee status.

Figure 1 provides descriptive evidence that for the vast majority of refugees, employment bans were binding and conditioned long-term employment rates. We observe only few refugees who work in periods when they are subject to an employment ban. Most likely, this is due to mismeasurement of refugees’ start or end dates of employment spells or arrival dates in Switzerland, or non-compliance with the ban in select cases.

Figure 1: Employment probability since arrival by initial employment ban



Notes: The underlying model regresses employment status against months-since-arrival dummies interacted with the initial employment ban policy (3, 6 or 14 months). We exclude individuals who arrived during a full employment ban and focus on the 1999-2006 sample, since the 14-month ban was abolished after 2006. 95% confidence interval is robust to canton × status group clustering.

(ii) *Prioritisation.* Prioritisation of residents is a national law that grants Swiss citizens,

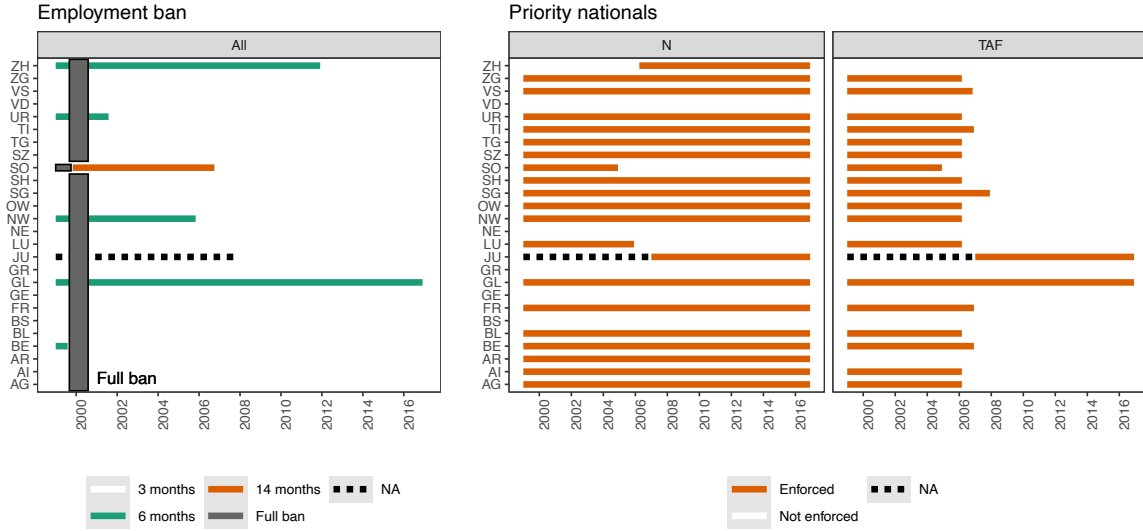
foreign nationals with a residence permit, and EU/EFTA residents priority in the labor market. The implementation and enforcement of this law vary across cantons. We code the policy as not enforced if the canton states that prioritization is not checked or proactively enforced. We code the policy as enforced if cantons mandate employers to make a ‘reasonable effort’ to find prioritized job seekers in combination with employers having to provide evidence of such effort (either upon request or for each vacancy) and/or if the job advertisement needs to be registered with local employment offices for a minimum of three weeks (such that case workers can encourage prioritized jobseekers to apply). Figure 2 (a) illustrates that a major change for this policy occurred in April 2006, when the posteriorization of TAF vis-à-vis prioritized job seekers has been lifted. However, even after April 2006, two cantons (Jura and Glarus) continued to posteriorize TAF. TAR and B refugees are not subject to these posteriorization policies.

(iii) *Sector restrictions.* Cantons can restrict work permits for asylum seekers to selected economic sectors or occupations. Figure 2 (c) shows that eleven cantons used that power at some point during the study period. The most restrictive of these cantons only allowed refugees to work in one two-digit sectors; the most liberal (among those with sector restrictions) in nine two-digit sectors. The permissible sectors typically include farm and construction work, care work in hospitals and elderly homes, and waste disposal. Cantons also have the option to impose sector restrictions for TAF. Prior to the national policy change in April 2006, a total of five cantons made use of this option.

(iv) *Mobility restrictions.* Cantons have considerable discretion in issuing work permits for asylum seekers, TAF, and TAR who are assigned to live in another canton. For each year, we count the number of neighboring cantons that do not issue work permits for asylum seekers and refugees with subsidiary protection living in a particular canton. Figure 2 (d) shows that overall, asylum seekers face the most severe restrictions. We also observe that labor restrictions gradually decline for TAF and TAR, with the latter group facing no restrictions starting in 2007. Refugees with a B permit are free to settle and work in any canton.

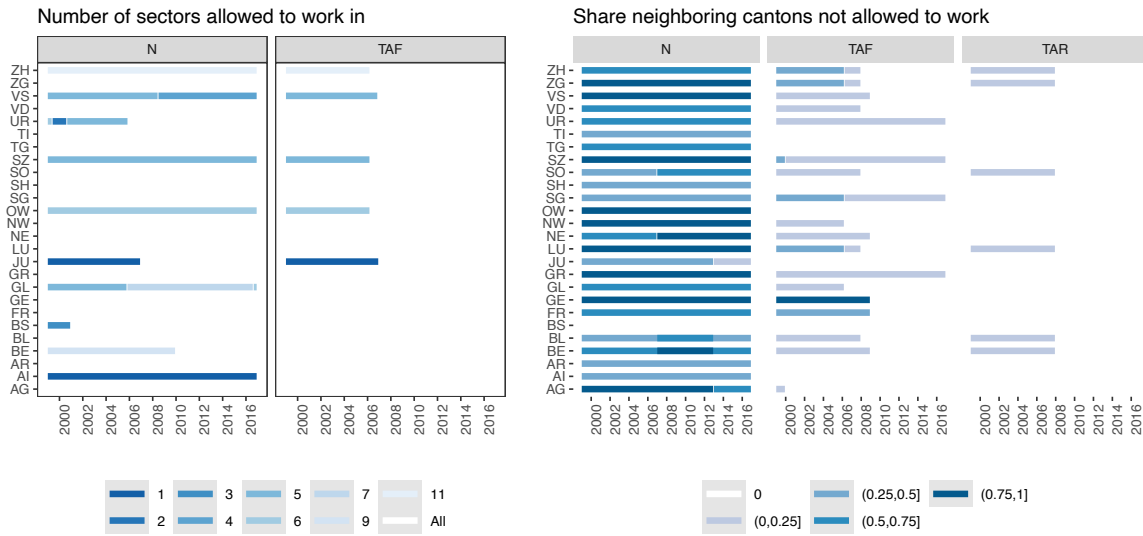
Total share of restricted jobs. For the analysis, we combine both the sector and regional restrictions into a joint variable measuring the share of job opportunities *not* available to refugees. To construct the “share of restricted jobs” for each permit category, canton and month, we combine sectoral employment shares of refugees who have never been exposed to sector restrictions with national commuter data from the Swiss population census in 2000. Specifically, we define the share as $\sum_j \sum_\ell \xi_{i,j,\ell} \times r_{i,j,t,s,\ell}$ where $\xi_{i,j,\ell}$ measures the propensity of refugees in canton i to work in canton j and sector ℓ in the absence of sector or mobility restrictions. $r_{i,j,t,s,\ell}$ is equal to 1 if a refugee of status s residing in canton i is not allowed to work in sector ℓ in canton j either due to region or sectoral restrictions, 0 otherwise (see Appendix B for details). This measure allows us to quantify the total loss of job opportunities

Figure 2: Overview of labor market policies



(a) Priority enforcement

(b) Employment ban



(c) Sector restrictions

(d) Region restrictions

Notes: Panel A shows when prioritization was enforced or strictly enforced in Swiss cantons. Panel B provides an overview of the length of employment bans in Swiss cantons. The default national policy is an employment ban of 3 months. This has been extended to 6 or 14 months in some cantons. In September 1999, all cantons except Solothurn introduced a full employment ban during which newly arriving asylum seekers were not allowed to work. This ban was lifted in August 2000. Panel A depicts the number of sectors asylum seekers (N) and temporarily admitted foreigners (TAF) are allowed to work in. Panel B shows the share of neighboring cantons that issue work permits.

due to the sector and geographic restrictions for refugees. Figure 3 shows that the sector and regional restrictions are most severe for TAF and N holders, but do not apply to holders of B permits. Region restrictions are more frequent but, if they apply, remove a lower share of jobs compared to sector restrictions. Both restrictions together sometimes remove a sizable share of the potential jobs of refugees. In one case, even 88% of the local jobs are unavailable to refugees because they are banned from certain sectors and from working in the neighboring cantons.

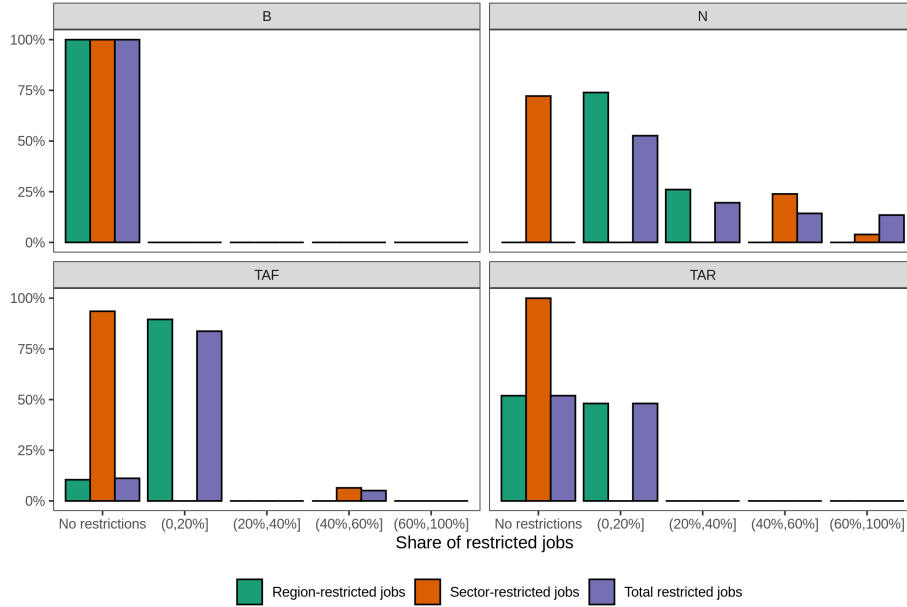
Compliance with labor market policies. The enforcement of these restrictions is the responsibility of the cantonal control bodies, which use a range of tactics, including random checks of firms, to penalize and deter illicit labor. Reliable estimates of the size of the workforce engaged in illicit employment are notoriously hard to come by, but existing studies agree that in Switzerland not asylum seekers and refugees, but undocumented immigrants make up the largest share of workers in illicit employment (Bolliger and Féraud, 2012; Longchamp et al., 2005). We leverage our register data to assess compliance with the labor and mobility restrictions in Appendix A.1. Consistent with Figure 1, Appendix Table A.1 documents very high compliance with the employment ban. With regard to region restrictions, Table A.1 shows that relatively few refugees work in cantons that do not permit extra-cantonal commuters. Compliance is less than perfect since cantons have the discretion to issue extra-cantonal work permits on a case-by-case basis. Lastly, Table A.1 suggests that sector restrictions, too, have a considerable bite, despite cantons’ power to apply sector restrictions with some discretion. This discretion is particularly relevant for refugees who hold a valid work permit in an occupation for which access is only later restricted.

3 Data

AHV-ZEMIS data. For the main part of our analysis, we match the canton-level policies with registry data from ZEMIS, which is maintained by the Swiss State Secretary for Migration, and social security data from the Old-Age and Survivors’ Insurance database (AHV). Descriptive statistics can be found in Table A.2 and A.3. ZEMIS includes records of asylum applications and decisions, the date of entry, and the assigned canton. The social security data holds records of employment spells. The long-run analysis also uses census data from STATPOP, the register-based census of Switzerland conducted since 2010, to verify which individuals are still residing in Switzerland after leaving the asylum system.

We measure refugees’ employment and earnings history using AHV records covering the period 1999–2016. Contributions to the pension scheme are mandatory for all workers starting from the calendar year in which they turn 18 until they reach the legal retirement age—65 years for men and 63 (until 2005) and 64 years (since 2005), respectively, for women. Contributions

Figure 3: Distribution of share of restricted jobs due to geographical and sectoral restrictions.



are irrespective of the residency permit or the contract type. The data thus cover incomes from small-scale employment and irregular working contracts such as internships, apprentices, or short-term seasonal work as long as the annual labor earnings in the job exceed the very low income threshold of CHF 2,300 (about 35% of the median monthly full-time wage in 2016). Labor earnings recorded in the data are uncapped and broadly defined. For instance, it includes variable pay components and in-kind benefits. The data also contain records of non-workers since non-workers are in general also required to contribute to the AHV. The only exception is individuals married to a spouse who contributes at least twice the minimum amount.

In the registry, employed and self-employed individuals generate one record per job and per year that details the starting and ending month of an employment relationship along with a firm identifier and the total earnings over that time period. This information allows us to compile a monthly individual-level panel of employment, monthly labor earnings, job separations, and job-to-job mobility, which we can match with ZEMIS using the social security number.⁷ Unfortunately, the AHV data do not allow us to compute hourly or daily earnings.

Swiss Earnings Structure Surveys. To study how the labor market policies affect refugees' hourly wages and hours worked, we analyze the waves of 2012, 2014, 2016, and 2018 of the

⁷ZEMIS also includes employment records which allows us to validate the AHV employment data. In general, the correlation between the two outcome measures is very high. However, employment spells are not consistently recorded for individuals with a B permit. We thus use AHV employment data for the analysis. We present the main results with the employment indicator recorded in ZEMIS in the appendix, Table E.2.

Swiss Earnings Structure Surveys (SSES), which are conducted by the Swiss Federal Statistical Office.⁸ The employer surveys are a large stratified random sample⁹ of private and public firms with at least three full-time-equivalent workers. The surveys cover all sectors except the agricultural sector. They contain information on 1.58 to 1.98 million workers depending on the wave, which translates to 32 to 39% of total employment in the sectors covered. Since the surveys are mandatory, response rates are high.¹⁰ They contain extensive information on the individual characteristics of workers, the characteristics of their jobs, and detailed salary information by pay components. Moreover, they provide reliable employer-reported information on hours worked per worker, which we use to compute hourly wages. Firms and workers can be linked across surveys with a unique firm and person identifier, respectively.

To identify refugees in the SSES, we link the surveys to the Swiss population registers 2010–2018 using the social security number. The registers provide workers’ residency permits, the place of living, and the canton refugees were initially assigned upon arrival in Switzerland. Since the population registers do not allow us to distinguish TAR from TAF refugees, we estimate the effects of a weighted average of the two permit-specific policies. We use the number of employed refugees in the two statuses in 2012 as the weight.

4 Employment and earnings

4.1 Empirical approach

In this section, we study the policies’ impact on refugees’ employment and earnings in the first five years after arrival. We use monthly individual-level panel data constructed from the matched AHV-ZEMIS data set. We estimate variants of the following panel regression model:

$$y_{icst} = \alpha' p_{icst} + \beta' x_{it} + \pi' w_i + \theta u_{ct} + \gamma_{t-T(i),s} + \mu_c + \delta_t + \varepsilon_{icst} \quad (1)$$

The dependent variable y_{icst} is employment, earnings, or another labor market outcome of refugee i , who is assigned to canton c upon arrival, and has permit s in month t . The policies are collected in the vector p_{icst} , which includes (i) a dummy that is equal to one if a person is banned from employment in month t in the assigned canton c , which depends on refugee’s arrival time $T(i)$, (ii) an indicator whether the assigned canton enforces priority for residents, and (iii) the share of sector- and region-restricted jobs for refugees in the local economy as defined in Section 2.2.¹¹ Our base specification also includes canton (μ_c) and month fixed

⁸Unfortunately, we cannot use data from the surveys before 2012 because they cannot be merged with the population registers and thus do not identify the refugee population.

⁹The complex stratification has about 1600 strata based on firms’ size, industry, and broad locations.

¹⁰The gross response rate to the 2018 survey was 74%.

¹¹Since priority enforcement, sector and region restrictions should have no effect while the employment ban is in force, we set these treatment variables to zero if the employment ban is in force.

effects (δ_t) and a vector of time-varying control variables x_{it} , which adjusts for the typical life-cycle earnings profile of refugees through gender-specific age and age-squared controls.

Intuitively, we identify the effects of the policies by exploiting two sources of variation. The first source is over-time changes in cantonal labor market policies for permit group s . The second source originates from within-person policy changes due to asylum decisions. A transition from asylum seeker (permit N) to the next permit (either B, TAR, or TAF) may relax restrictions by more or less, depending on the canton and the period. In both cases, our analysis benefits from the largely exogenous assignment of refugees to cantons, which prohibits refugees to endogenously sort into cantons with favorable labor market policies for the first five years after arrival. Following the discussion in Section 2.2, we control for the key time-invariant covariates observed by the placement officers when assigning refugees to cantons, w_i .¹² In addition, we also present the results from a more restrictive model with individual fixed effects.

One concern about exploiting cantonal policy variation is that cantonal policy changes may be correlated with local labor market conditions or other cantonal policies. We address this concern in a number of ways. First, we include canton fixed effects, which ensures that we control for all time-invariant correlates of cantonal policies. Second, we control for the contemporaneous unemployment rate in each canton (u_{ct}) to account for regional business cycle effects and for two important cantonal refugee policies that we coded when collecting our policy dataset.¹³ Third, we generalize the panel regression to an event study to test for pre-trends. These event studies show that the outcomes of treated and untreated individuals evolved similarly in the months prior to changes in policies. Lastly, we find little evidence that local labor market conditions in the three years prior to a policy change, as proxied with the cantonal unemployment rate and the cantonal refugee unemployment rate, systematically predict subsequent changes in labor market policies.¹⁴

A central concern with exploiting policy variation from status changes is that the outcome of the asylum decision and its timing may depend on a refugee’s labor market potential. Although this should not be the case under Swiss asylum law and the refugee convention, it is easy to think of ways how unobserved refugee characteristics might influence both. For example, asylum seekers with higher innate abilities and thus greater labor market potential might be more adept at navigating asylum interviews, which might not only increase the likelihood

¹²These are nationality, sex, arrival-center fixed effects, two dummies for self-reported religion (Muslim and Christian), marriage status, as well as the average unemployment rate over the first two years after arrival.

¹³In particular, we control for the level of social assistance for refugees in each canton—the cantonal cash allowance in Swiss Franc—and an indicator whether a canton prohibits refugees to work as self-employed.

¹⁴Table K.1 presents regressions of a dummy for the tightening (or loosening) of a particular restriction on lags of the overall local unemployment rate and the local unemployment rate for refugees. The table shows that most of the coefficients of these regressions are small and not statistically significant. For sectoral or geographic restrictions, there are some significant coefficients for the different lags of the unemployment rate of refugees, but the estimates have alternating signs and are also economically small.

of protection but also shorten the time until a decision is made. To limit concerns about such forms of unobserved heterogeneity, we compare only individuals whose asylum decision turned out to be the same and hence received the same permit and who have been in Switzerland for the same number of months. Econometrically, we achieve this by including fixed effects for the number of months since arrival in Switzerland interacted with the transition group, $\gamma_{t-T(i),s}$. Additional analyses show that the estimated effects of restrictions are robust to specifications that only exploit within-cantonal policy variation. To show this, we focus on refugees with a TAF permit because it is the only permit group where we have a large sample and sufficient within-canton policy variation in all four policies to reliably estimate the policy effects.¹⁵

4.2 Results

We start by documenting the contemporaneous effects of the labor market restrictions on refugees' employment and labor earnings in Table 1. The table provides separate regression results for monthly employment (panel A), total labor earnings including zero earnings for non-workers (panel B), and log labor earnings (panel C, discussed in the next section). The first three columns show separate estimates for three different status transition groups: asylum seekers whose claim is rejected but who are temporarily admitted (denoted N→TAF), and asylum seekers whose claim is granted (denoted as N→TAR or N→B, respectively). The remaining columns 4–6 pool these three group-specific regressions by interacting the month-since-arrival fixed effects with an indicator for the three transition groups. Column 7 shows the results for refugees with a TAF permit only, thus abstracting from policy variation arising when refugees switch status. We cluster standard errors at the canton level in columns 1–3 and 7, and at the group times canton level in the remaining columns.

Employment. Panel A of Table 1 shows the effect of the labor market restrictions on a monthly employment indicator. Our main focus is on our baseline specification in column 4, which averages the effects across all transition groups (columns 1–3). The panel provides several insights. First, in line with the descriptive patterns in Figure 1 we find that the employment probability is strongly reduced in a month in which an employment ban applies. The likelihood to be employed is almost 12 percentage points lower, consistent with the evidence presented by Marbach, Hainmueller, and Hangartner (2018) and Fasani, Frattini, and Minale (2021) for a range of European countries. This estimate also serves as a validation check for our coding: in the months with an employment ban, we observe very few refugees with a job. Second, refugees allocated to cantons that enforce *priority* of permanent residents are 5.6 percentage points less likely to be employed in a given month compared to refugees in cantons where this policy is not enforced. As the average employment rate of refugees is at a

¹⁵Note that refugees receiving a TAR or B permit have little or no, respectively, within-permit variation in policies as shown in Figure 2.

Table 1: Effect of labor market policies on employment and total earnings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A. Employment</i>							
Employment ban	-0.1078*** (0.0245)	-0.2249*** (0.0332)	-0.1466*** (0.0237)	-0.1198*** (0.0160)	-0.1153*** (0.0195)	-0.0673*** (0.0092)	-0.1229*** (0.0281)
Priority enforced	-0.0551*** (0.0138)	-0.0552* (0.0291)	-0.0607*** (0.0204)	-0.0563*** (0.0120)	-0.0555*** (0.0134)	-0.0293*** (0.0110)	-0.0638** (0.0262)
Share restricted jobs	-0.0518 (0.0367)	-0.0393 (0.0302)	-0.0454 (0.0277)	-0.0522* (0.0269)	-0.0486 (0.0303)	-0.0341* (0.0203)	-0.0767 (0.0635)
Outcome mean	0.1889	0.1438	0.1452	0.1728	0.1728	0.1728	0.2294
Num. individuals	41,218	6,494	20,059	67,771	67,771	67,771	33,897
Observations	1,741,073	246,365	759,223	2,746,661	2,746,661	2,746,661	1,239,727
<i>Panel B. Total earnings (Poisson)</i>							
Employment ban	-1.241*** (0.1708)	-2.606 (1.599)	-1.587*** (0.4062)	-1.260*** (0.1046)	-1.260*** (0.1225)	-1.556*** (0.1856)	-1.215*** (0.1478)
Priority enforced	-0.3914*** (0.0685)	-0.7374*** (0.1764)	-0.9848*** (0.2005)	-0.4568*** (0.0672)	-0.4741*** (0.0661)	-0.3895*** (0.0702)	-0.2561** (0.1075)
Share restricted jobs	-0.6302*** (0.2006)	0.4792 (0.5524)	-0.1221 (0.4036)	-0.5054*** (0.1870)	-0.5388*** (0.2060)	-0.5399*** (0.1462)	-0.3239 (0.2738)
Outcome mean (CHF)	504.3	365.8	328.0	442.9	442.9	949.7	621.8
Num. individuals	41,218	6,494	20,059	67,771	67,771	23,050	33,897
Observations	1,739,868	246,047	759,222	2,746,496	2,746,496	1,280,860	1,239,677
<i>Panel C. Monthly earnings (log)</i>							
Priority enforced	-0.0718* (0.0374)	-0.4005** (0.1854)	-0.3913* (0.2135)	-0.1709*** (0.0424)	-0.1670*** (0.0466)	-0.1273** (0.0554)	0.0002 (0.0279)
Share restricted jobs	-0.3218** (0.1351)	0.2851 (0.4647)	-0.1323 (0.3799)	-0.2070 (0.1351)	-0.2084 (0.1340)	-0.1659 (0.1540)	-0.0880 (0.1169)
Outcome mean	2,667.9	2,540.9	2,259.2	2,563.4	2,563.4	2,563.4	2,710.8
Num. individuals	14,536	2,060	6,454	23,050	23,050	23,050	13,938
Observations	328,862	35,426	110,230	474,518	474,518	474,518	284,372
Sample	N->TAF	N->TAR	N->B	All	All	All	TAF
Months-since-arrival FE	Yes	Yes	Yes	Interacted	Interacted	Interacted	Yes
Additional controls	Yes	Yes	Yes	Yes	No	No	Yes
Canton FE	Yes	Yes	Yes	Yes	Yes		Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE						Yes	

Notes: The table shows the effect of the labor market restrictions on monthly employment indicators (panel A), total labor earnings of refugees (including non-workers, panel B), and log monthly labor earnings of the employed (panel C) in the first five years after refugees arrival. The regressions are based on specification (1). In panel B, we estimate a Poisson fixed effects model to accommodate zero earnings. Columns 1–3 present separate estimates for asylum seekers transitioning to temporarily admitted foreigners (denoted N→TAF), to temporarily admitted refugees (N→TAR), and to recognized as refugees (N→B). Columns 4–6 pool these three groups. Column 7 shows effects for refugees with a TAF permit only, thus, disregarding the within-person variation arising from status changes. All columns include month, canton of assignment, and month-since-arrival fixed effects. In columns 4–6 we interact the months-since-arrival fixed effects with dummies for the three transition groups. Column 6 adds individual fixed effects. All regression models include age and age-squared interacted with sex and maximum cash allowance in CHF for refugees. Additional controls are marriage status, two dummies for self-reported religion (Christian and Muslim), nationality, and asylum processing centre fixed effects as well as the contemporaneous unemployment rate and the unemployment rate at arrival. Standard errors are clustered at the canton × transition group level.

Signif. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

low 17.2%, this effect represents 32% reduction in refugees' overall employment rate. Third, restricting the share of jobs available for refugees, either by only allowing their employment in certain sectors or only in some but not all neighboring cantons, seems to reduce employment further. Even though the effect is not always statistically significant, economically it is: the estimate in panel A, column 4, suggests that removing about 30% of refugees' potential jobs (a move from the median to roughly the 90 percentile in the share of restricted jobs) reduces employment by roughly 1.6 p.p.

Columns 5 to 7 of Table 1 show that the employment effects are similar if we drop the additional controls, include individual fixed effects, or if the effects are only identified from the within-canton but not status-transition variation. When individual fixed effects are included (column 6), the estimated employment effects are a bit smaller. The reasons are that the effects are implicitly identified from a shorter window around the policy changes¹⁶. The results below suggest that the effects of policy changes are larger for two individuals of different cohorts than for the same individual over time. One reason is that stricter policies do not lead to layoffs of refugees in employment. Rather, non-employed refugees become less likely to find a job. Another is that the policies reduce workers' outcomes for years beyond they apply. This means that relaxing a restriction has a less immediate impact on a person who had been exposed to it.

Lastly, we show in Appendix C that the effects of all three restrictions policies are concentrated among refugees with high predicted employability (particularly younger and male refugees). Conversely, the effects are close to zero for refugees with low employability (particularly female and older refugees). The fact that the policies have no impact on the latter group is not surprising as they exhibit a very low likelihood to be employed in the first place. The result can thus be seen as a further validity check to our research design.

Total earnings. Next, we document how the policies affect total labor earnings in Panel B of Table 1. The outcome variable includes zero earnings for refugees without employment. The model is thus estimated using a Poisson fixed effect estimator, capturing both the intensive and extensive margin effects of the restrictions on earnings. Our baseline specification in column 4 of Table 1, panel B, reveals that labor market restrictions have very detrimental effects on refugees' earnings. The presence of an employment ban reduces labor earnings by as much as 126%, enforcing priority by roughly 46%, and removing a third of refugees' potential jobs by about 14%.

¹⁶The individual fixed effects regression implicitly reduces the time window used to identify the policy effects to five years as we only use within-person policy variation in refugees' first five years in Switzerland. The baseline specification compares different cohorts of refugees across a longer time horizon.

Discussion of effect sizes. In sum, the labor market restrictions that we study depress refugees’ employment and earnings substantially while they apply. As we discuss in more detail in Appendix section L, our estimates imply that moving from an unrestricted to the most restrictive cantonal policy environment observed in our sample reduces the average employment rate of refugees from 19% to 13% during the first five years in Switzerland. Similarly, average monthly earnings are 40% lower in the most restrictive policy environment compared to no restrictions. These effects are only driven by refugees for whom the restriction environment can effectively change across cantons, i.e., mostly refugees with either an N or TAF permit. If we consider only these directly affected groups, earnings are 46% lower and employment drops from 19% to 11%.

The magnitude of these effects is also substantial because they suggest that the labor market restrictions are at least as important determinants of refugees’ labor market integration as local economic conditions or very successful integration policies. For instance, in our regressions, we estimate that an increase in the local cantonal unemployment rate by +1.2 p.p. like in the Great Recession reduces refugees’ employment by 1.8 p.p. and their earnings by 13%.¹⁷ Recent work discussing integration policies, e.g., Foged, Hasager, and Peri (2022) or Arendt, Dustmann, and Ku (2022), highlights that language training, the policy with the largest overall employment effects in the first five years, raises employment by roughly 6 percentage points for refugees in Denmark.

4.3 Robustness

Event studies. As a next step to probe the robustness of our baseline estimates, we now assess whether the main outcomes follow parallel trends before the policy changes. We do this by generalizing the regression model to a dynamic difference-in-differences event study. If policy changes were correlated with unobserved cantonal trends in local labor market conditions or political shocks, this would likely be reflected in differential trends in outcomes prior to the policy change.

Since the employment bans start applying at arrival and usually last only for a couple of months, we cannot reliably estimate pre-trends for more than a few months. We thus focus on the pre-trends for the priority policy and the share of restricted jobs, and exclude months with active employment bans. We estimate the following event study model, which extends

¹⁷The estimates of the effects of the contemporaneous, local unemployment rate on employment is -1.496 (standard error: 0.7856) and -10.71 (standard error: 4.429) on earnings, respectively. These estimates are omitted from table 1 for brevity but the local unemployment rate is included in all specifications.

conventional event studies to two policies (Freyaldenhoven et al., 2021)¹⁸:

$$y_{icst} = \sum_{j=-\omega+1}^{\omega-1} \alpha_{p,j} \Delta p_{cs,t-j} + \alpha_{q,j} \Delta q_{cs,t-j} + \beta' x_{it} + \pi' w_i + \theta u_{ct} + \gamma_{t-T(i),s} + \mu_c + \delta_t + \varepsilon'_{icst} \quad (2)$$

where $p_{cs,t-j}$ is a dummy for the priority policy and $q_{cs,t-j}$ represents the share of restricted jobs, respectively, and Δ is the first-difference operator. The coefficients $\alpha_{p,j}$ and $\alpha_{q,j}$ represent the cumulative policy effects on the outcome j months after a change in the priority policy (switch on or off) and a change in the share restricted jobs (relative to the reference $\omega = -1$), respectively. We consider a 30 month window around the policy change (i.e., $\omega = 15$), so that the event window covers approximately half of the 60 months time period that each refugee is in our sample.

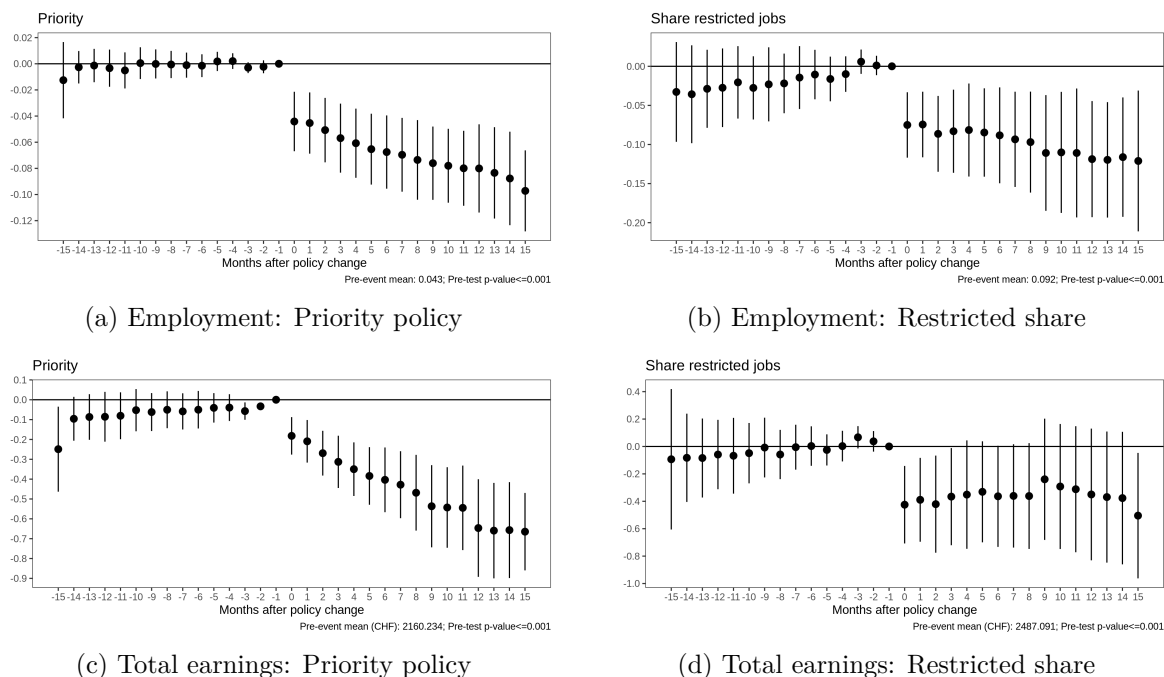
Figure 4 shows the event study plots for the priority policy and the share of restricted jobs on employment (panels A and B) and earnings (panels B and C), respectively. All plots provide no evidence for systematic differences in refugees' employment and earnings trends in cantons and time periods with tightening or loosening restrictions compared to such without. Conversely, all plots show a clear and immediate decrease in employment and earnings starting in the month when either prioritization is enforced or when the share of restricted jobs increases. Over the next few months, the effects of both policies become larger. Note that our estimates are mostly identified from the removal of restrictions, while Figure 4 shows the effect of introducing restrictions.

Potential bias from heterogeneity. As a further robustness check, we assess whether our estimates are affected by the potential bias of fixed effects estimators arising from heterogeneous treatment effects, and variation in treatment timing (for a review see De Chaisemartin and D'Haultfoeuille, 2022). The two-way fixed effects estimator recovers a weighted average of group-specific treatment effects where the weights are generally not inversely proportional to group size and may even be negative. To our knowledge, none of the solutions currently suggested in the literature accommodates our setting with multiple fixed effects, multiple treatments—both continuous and binary—and treatment intensities that may increase or decrease. We thus follow a simple approach that is robust to heterogeneous treatment effects along one pre-specified dimension. Our approach is similar in spirit to Wooldridge (2021) and Sun and Abraham (2020) who focus on settings with common treatment timing and staggered designs.

In the first heterogeneity-robust analysis, we address the concern that treatment effects may vary by duration of stay. This is in line with empirical evidence highlighting the impor-

¹⁸For the sake of brevity, we omit the endpoint variables associated with the long-run effects from the formula, but not from the estimation. Specifically, the full estimation model uses $\varepsilon'_{icst} = \alpha_{p,\omega} p_{cs,t-\omega} + \alpha_{p,-\omega} (-p_{cs,t+\omega-1}) + \alpha_{q,\omega} q_{cs,t-\omega} + \alpha_{q,-\omega} (-q_{cs,t+\omega-1}) + \varepsilon_{icst}$. We also set $\alpha_{p,-1} = \alpha_{q,-1} = 0$.

Figure 4: Event study estimates of effects of prioritization and total restricted share on employment and total earnings.



Notes: The figure shows the dynamic effect of the labor market restriction on employment (panels a and b) and total earnings (panels c and d) using the event study model ((2)). The effects show the cumulative effect of a policy change between 15 months before and 15 months after a policy change. Figures (a) and (c) plot the event path for the priority policy. Figures (b) and (d) show the event path for the share total restricted jobs. The sample excludes months with an employment ban. The model includes month, canton, and month-since-arrival fixed effects interacted with dummies for the three transition groups. We also add individual-level controls.

tance of the policy exposure in the first years after arrival (e.g., Fasani, Frattini, and Minale, 2021), including our long-run results in Section 6. We implement our approach by interacting the policies with months-since-arrival fixed effects and aggregate the effects using group shares to avoid issues associated with negative weights whereby the groups constitute observations with the same number of months since arrival. Second, we allow for treatment heterogeneity by calendar years. That is, we repeat the exercise above by interacting our policy variables with the calendar year of policy exposure, and by then obtaining calendar-year-weighted aggregate estimates. Overall, we find very similar effects for the priority policy if we account for heterogeneity by duration (Table E.5) or for heterogeneity by calendar year (Table E.6). The effect for the restricted share is a bit larger in absolute size but qualitatively similar.

5 Wages

In this section, we show that the sector and regional restrictions, but not prioritization, reduce refugees’ absolute and relative hourly wages. After outlining the estimation strategy (in Section 5.1) and presenting the baseline results (Section 5.2), we exploit the rich contextual information on workers and their job characteristics to investigate why the wage effects arise (Section 5.3).

5.1 Empirical approach

Since hours worked are unobserved in the AHV data, we build our main analysis on the bi-annual SESS surveys. When doing so, we deviate slightly from our estimation model in (1) since we only have overlap between the policy dataset and the biannual SESS data in three years (October 2012, 2014, and 2016). In this data and period, there is little policy variation, both because few cantons change policies and because we observe few status changes from asylum seekers to the next status.¹⁹ Thus, our preferred estimation strategy with this data relates workers’ wages to policies that were in place when refugees started working for their current employer instead of relating wages to the current policy exposure.²⁰ We then compare the wages of refugees of the same transition group and nationality, which participated in the same survey wave, and have the same years of tenure but were exposed to different policies when they started their jobs. Since the policies are autocorrelated, the estimates also capture the effects of subsequent policies on wages. To ensure that we do not measure wages and policies too far apart, we restrict the sample to workers that started to work at their employer in 2005 or later. Our preferred specification is also restricted to observations from the earliest survey if the same refugee is observed in the same firm several times, although we present results without this sample restriction.

This strategy has the advantage that we can leverage the substantial policy variation in the late 2000s. It also allows us to incorporate the data from the 2018 survey if the worker started to work for her current firm in 2016 or earlier. Not surprisingly, the approach leads to more robust and precise results than if we relate wages to contemporaneous policies. A disadvantage is that the policy variation depends to some extent on how long a worker is at her current employer, which is a function of the policies, too (as shown later). While the sign of a possible bias is unclear²¹, we show that the estimation results are similar if we use the

¹⁹This has to do with the sampling of the SSES—the surveys covers roughly a third of all workers per wave—and with the fact that very few asylum seekers have a job. Thus the surveys contain less than 200 wage observations of asylum seekers.

²⁰The approach relies on the tenure variable in the SESS survey, which is measured in years. We merge the policies observed in April of each year because an employee with one year of tenure is expected to have started working at his current firm 1.5 years ago. Using the policies observed in October leads to very similar results.

²¹The estimates could be biased downward if workers with initially low wages due to restrictive initial policies have a greater probability to quit their job than workers with high wages. The estimates could be biased upward

AHV data to estimate equation (1) using monthly earnings as outcome variable, and if we relate hourly wages to contemporaneous policies using analogous regressions with the SSES data.²²

5.2 Baseline results

Table 2 uses our preferred approach to estimate the effects of the policies on log hourly wages (panel A) and log monthly hours (panel B) using the SESS over 2012–2018. All specifications contain our baseline controls (see table notes) and fixed effects for workers’ nationality, canton of living, survey wave, and first year of a spell. The sample in column 1 includes refugees who transition from asylum seeker (N) to B permit, while column 2 uses refugees who transition from N to either TAR or TAF permit. The remaining columns pool the two groups but estimate separate survey year effects by transition group.

The regressions in panel A of Table 2 provide no evidence that enforcing the priority for residents affects hourly wages. However, panel B provides some evidence that prioritizing residents may have negative effects on monthly hours. This effect on hours worked may explain why the policy has a meaningful negative impact on refugees’ monthly earnings if we use the AHV data to estimate the effects on log monthly earnings of employed refugees (Table 1).

Panel A of Table 2 provides clear evidence that restricting the share of jobs available to refugees reduces their hourly pay. According to our baseline specification with pooled transition groups (column 3), removing 10 percent of potential jobs reduces refugees’ hourly wages by 2.8%. As we show in columns 4–6, this effect is robust to the exclusion of control variables, to using all and not only the first observation per spell, and to including full interactions between transition groups and years-since-arrival in Switzerland. Interestingly, the estimate is quite similar to those reported by Caldwell and Danieli (2021), who estimate that access to 10% additional outside options increases wages by 1.7%.

Reassuringly, we find comparable evidence when using contemporaneous policies or when considering monthly earnings based on AHV data. First, using only the 2012–2016 waves in the SESS, we find statistically indistinguishable results for the impact of contemporaneous policies (Table F.1, column 1). If anything, our preferred approach may be conservative

if workers in cantons with restrictive policies remain longer with an employer despite a low wage because they are locked into their jobs.

²²The regression model is similar except for three adjustments. First, we include refugees that are longer in Switzerland than five years, provided they still have a refugee status at the time of the survey. Second, we focus mostly on workers transitioning from N to TAR or TAF, given the little policy variation and few refugees receiving a B permit. Third, we do not control for years-since-immigration fixed effects in our baseline specification. Adding these fixed effects leads to an almost fully saturated model, leaving us with few refugees in unrestricted cantons that could act as a comparison group for restricted refugees. A comparison of columns 2 (baseline), 5 (with years-since-immigration FE), and 7 (only workers within their 5 five years in Switzerland) in Appendix Table F.1, the results are similar but less precise if we do not impose these restrictions.

in that the estimated wage effects are larger in absolute size when using contemporaneous policies. Secondly, our preferred specification in Table 1 (panel C), which pools all three transition groups (column 4), suggests that sector and region restrictions reduce monthly earnings. However, the estimates are marginally statistically insignificant except for the large group of refugees transitioning from N to TAF permit (column 1). The estimates are also slightly less negative (albeit not significantly so). One explanation is that sector and region restrictions, if anything, increase hours worked per month, as shown in Table F.1, panel B, which makes it harder to detect the wage effect with the AHV data.

Table 2: Effect of labor market policies on monthly hours worked and hourly wages

VARIABLES	(1) N→B	(2) N→TAR/F	(3) Both	(4) Both	(5) Both	(6) Both
<i>A. Log hourly wages</i>						
Priority enforced	0.005 (0.055)	0.070 (0.082)	0.058 (0.043)	0.021 (0.042)	0.049 (0.036)	0.061 (0.042)
Share restricted jobs	-0.296 (0.196)	-0.347** (0.153)	-0.281*** (0.102)	-0.374*** (0.102)	-0.192** (0.086)	-0.254** (0.106)
Observations	1,942	4,381	6,342	6,361	9,231	6,340
<i>B. Log monthly hours worked</i>						
Priority enforced	-0.213* (0.122)	-0.056 (0.129)	-0.084 (0.087)	-0.090 (0.088)	-0.041 (0.077)	-0.080 (0.091)
Share restricted jobs	0.248 (0.244)	0.086 (0.242)	0.173 (0.174)	0.527*** (0.185)	0.152 (0.162)	0.170 (0.191)
Observations	1,942	4,381	6,342	6,361	9,231	6,340
Observations per spell	First	First	First	First	All	First
Baseline controls	Yes	Yes	Yes	No	Yes	Yes
Survey wave FE	Yes	Yes	Interacted	Interacted	Interacted	Interacted
First year of tenure FE	Yes	Yes	Yes	Yes	Yes	Yes
Canton FE	Yes	Yes	Yes	Yes	Yes	Yes
Years-since-entry FE	No	No	No	No	No	Interacted

Notes: This table shows the effect of labor restrictions on log hourly wages of refugees (panel A) and log monthly hours worked (panel B) of refugees using SESS data 2012–2018. We relate workers’ hours and wages to the status-specific policies that were in place at the start of an employment spell. In all columns but 5, we only keep the first observation per refugee-spell in the case of multiple appearances. The sample in column 1 is employed refugees aged 18–65 who transition from N to a B permit. Column 2 uses refugees transitioning from N to TAR/TAF permit. The remaining columns pool these groups. In all columns, we focus on refugees who started their current spell in 2005 or later. We aggregate the policies for TAR/TAF refugees, which we cannot distinguish in the data, by giving TAR policies a weight of 14.1%. All specifications control for canton and survey wave fixed effects (interacted with transition group in columns 3–7), and fixed effects for the first spell year. Baseline controls are gender, gender-specific age and age squared, nationality dummies, marital status, the unemployment rate at the start of the spell, social assistance in the canton (in CHF), and dummies equal to 1 if the canton has a blocking period exceeding 3 months and self-employment restrictions at the start of the spell. Standard errors are clustered at the canton \times status-group level.

Signif. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

These results suggest that sectoral and regional restrictions have a sizeable negative impact on refugees’ hourly wages. A possible implication of these findings is that the restrictions

contribute to explaining why refugees are paid less relative to observationally similar native citizens. In Table 3, we test whether policies contribute to the refugee-native wage gap using the SESS. We impose the same sample restrictions as previously but now include non-refugee workers.

The odd-numbered columns in Table 3 present estimates of the unexplained wage gap between refugees and Swiss citizens. The variable of interest is an indicator variable that the worker is a refugee. The first regression result, which only includes our typical set of controls, canton and wave fixed effects, suggests a substantial wage gap relative to Swiss citizens of more than a third ($100 \times (e^{-0.492} - 1) = -38.8\%$). Half of this gap can be explained by differences in terms of firm tenure, canton of work, and industry (column 3). Another two-thirds can be attributed to differences in educational attainment, occupation, and management level (column 5). However, even if we only compare refugees and natives working in similar jobs and in the same firm and year (column 7), the unexplained gap still amounts to 6%.

The even-numbered columns in Table 3 test whether the unexplained refugee-native wage gap relates to refugees' labor market restrictions. To this end, we interact the refugee identifier with the policies. The estimates are consistent with the impact of sectoral and regional restrictions on the unexplained wage gap. The relative wages of refugees allocated to cantons that restrict more potential jobs are substantially lower than the relative wages of refugees allocated to cantons with more liberal policies. This holds even if we condition on a rich set of worker, firm, and job characteristics (columns 4 and 6). The magnitudes of the policy effects are remarkably similar as in Table 2, suggesting that the pay differences among refugees estimated above translate almost one-to-one into a larger unexplained wage gap relative to Swiss citizens. In sum, the results suggest that sector and mobility restrictions help to explain why refugees are paid worse than otherwise comparable resident workers in similar jobs. While interesting as such, this result also shows that the policy effects are unlikely to be driven by unobserved labor market shocks correlating with policy changes, as such shocks would be at least partially controlled for when looking at the relative wages of groups of workers in similar jobs.

5.3 Mechanism

In the following, we investigate explanations for why labor market policies reduce the absolute and relative wages of refugees. To begin with, we can confidently rule out that the policy effects are driven by aggregate labor supply and demand effects. In a stylized labor market equilibrium, restrictions would shift the supply curve inward, leading to wage increases instead of decreases if the labor demand curve is downward-sloping. In fact, we view it as unlikely more generally that the wage effects reflect market-level adjustments caused by aggregate labor supply shifts. Refugees only represent 0.16% of total (refugee and non-refugee) employment

Table 3: Labor market policies and the wage gap between refugees and native citizens

VARIABLES	(1) Log hourly wage	(2) Log hourly wage	(3) Log hourly wage	(4) Log hourly wage	(5) Log hourly wage	(6) Log hourly wage	(7) Log hourly wage	(8) Log hourly wage
Refugee	-0.492*** (0.015)	-0.512*** (0.031)	-0.275*** (0.015)	-0.292*** (0.038)	-0.104*** (0.010)	-0.089*** (0.026)	-0.073*** (0.007)	-0.061*** (0.016)
Foreigner	-0.122*** (0.010)	-0.122*** (0.010)	-0.055*** (0.006)	-0.055*** (0.006)	-0.021*** (0.005)	-0.021*** (0.005)	-0.016*** (0.003)	-0.016*** (0.003)
Refugee × Priority enforced		0.044 (0.038)		0.053 (0.032)		0.037 (0.025)		0.050* (0.029)
Refugee × Share restricted jobs		-0.327*** (0.121)		-0.285** (0.118)		-0.225* (0.131)		-0.226*** (0.083)
Observations	2,305,182	2,305,139	2,305,182	2,305,139	1,707,312	1,707,278	1,686,093	1,686,059
R-squared	0.151	0.151	0.296	0.296	0.493	0.493	0.659	0.659
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Canton of living FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Canton of work FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
First year of tenure FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Educational attainment FE	No	No	No	No	Yes	Yes	Yes	Yes
Occupation and management level FE	No	No	No	No	Yes	Yes	Yes	Yes
Firm-year FE	No	No	No	No	No	No	Yes	Yes

Notes: This table uses SESS data 2012–2018 to analyze the wage differential between refugees, other foreign workers, and Swiss nationals. The dependent variable is the workers’ hourly wage. We focus on workers aged 18–65 that started to work in 2005 or later for their current employer. We only keep the observation from the earliest survey if the same worker is observed within the same firm in several surveys. Columns 1–2 estimate wage gaps conditional on the baseline controls (see Table 2), and fixed effects for the survey year and workers’ canton of living. Columns 3–4 add firm entry-year fixed effects, the canton of work, and two-digit industry codes. Columns 5–6 add fixed effects for eight levels of educational attainment, ISCO two-digit occupation, and five management levels (no, lowest, low, mid-, and highest-level management). Lastly, columns 7–8 add firm-year fixed effects. Columns 2, 4, 6, and 8 also show interactions between the labor market restrictions and the refugee indicator. We use status-specific policies measured at the beginning of a spell. All policy variables are set to zero for non-refugee workers. Standard errors are clustered at the initial canton of living × worker group level. We differentiate three groups: N → B refugees, N → TAR/TAF refugees, and the remaining workers.

Signif. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

and less than 1.5% of total employment even in the sector where they are most represented (i.e., restaurants).

We thus concentrate on the role of productivity effects and outside options. We focus on sector and region restrictions because firms are not free to choose wages if priority is enforced. Prioritization requires firms to disclose refugees’ work contracts and wages to the cantonal authorities.²³ It is possible that this disclosure explains why prioritization has no, or if anything, even a positive effect on refugees’ hourly pay. While this is an interesting result in itself, it is not clear how much we can learn about wage determination in general from these results.

²³In the process of hiring refugees, firms have to disclose wages and the work contract to the cantonal authorities. The authorities check whether wages and other working conditions are in line with sectoral and regional standards, including wage standards defined in collective bargaining agreements.

5.3.1 The role of productivity

Stricter sector and region restrictions could depress wages by reducing the marginal product of refugees in employment. In particular, the restrictions might suppress productivity by forcing refugees to work in low-paying sectors, impeding human capital accumulation, changing the composition of the workforce, or through a reduction in the quality of worker-firm matches. However, the evidence presented below suggests that these mechanisms play a surprisingly limited role in explaining the wage effect. We summarize the main evidence in Figure 5, which is based on our base specification in Table 2, column 3.

First, restrictions may force refugees to accept jobs in sectors, regions, firms, or occupations with low productivity and hence low pay. In Figure 5, we successively add fixed effects for workers' two-digit industry, canton of work, and occupation to our base specification, but find that the effect sizes are unaltered. Furthermore, since restrictions also explain part of the unexplained wage gap within firms, we can rule out that the results are driven by fine-grained within-industry displacement effects (Table 3, column 8). Second, restrictions could reduce productivity by adversely affecting how much refugees are able to work throughout their careers, which in turn suppresses on-the-job learning and skill accumulation. However, as shown in Figure 5, the wage effect does not change if we control for refugee workers' tenure, accumulated months of work experience in Switzerland, and highest educational attainment (which includes a separate indicator for informal on-the-job learning for workers with no secondary degree).

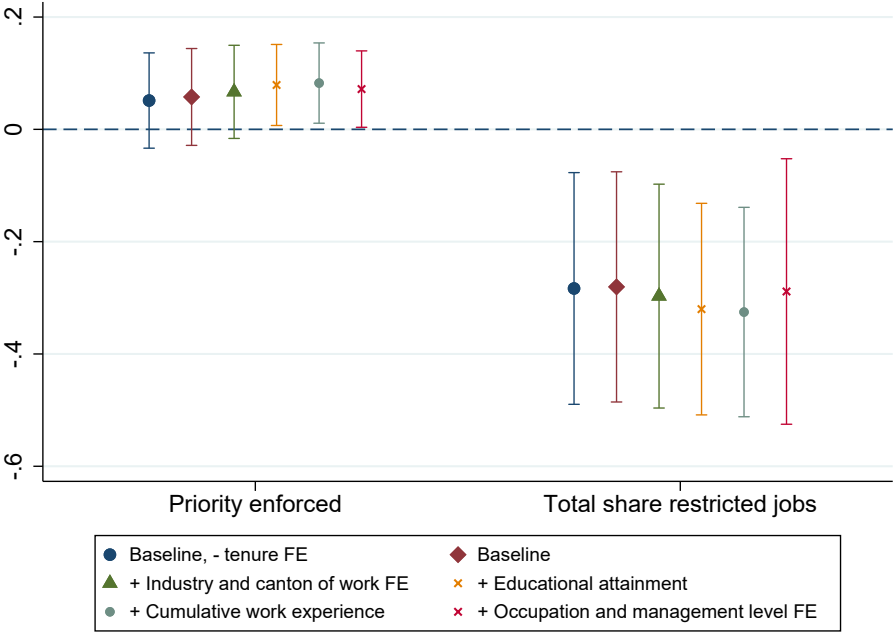
The results in Figure 5 also indicate that the wage effect is not due to restrictions causing better-paid workers to lose their jobs. Controlling for the job and worker characteristics would reduce the wage effect if its cause were a change in the composition of workers. Furthermore, if the composition effect would play a major role, the wage and monthly earnings effects would also change if we focus on within-person variation only. This is not the case in the specifications where we have sufficient variation to add individual fixed effects.²⁴

Lastly, policies could reduce the match quality between refugees' skills and the skill requirements of their jobs, thereby suppressing worker productivity and wages. For example, the restrictions might force high-skilled engineers to work in restaurants if engineering-related sectors are barred. An implication of the skill mismatch hypothesis is that, in more restrictive cantons, restaurants (which in our sample are never restricted) would have to hire more badly matched workers. However, it is not clear why restaurants would be inclined to hire more badly trained staff when restrictions in other sectors are more stringent. Indeed, using firm-level data we find that sector and region restrictions do not increase firm employment in

²⁴The results using monthly earnings in the AHV data are presented in column 6 of panel C of Table 1. The corresponding specification using the Swiss Earnings Structure Surveys and hourly wages, shown in column 6 of Table F.1 is also similar to our baseline estimates but lacks precision because of the few refugees observed in several surveys.

unrestricted sectors and regions. If anything, the opposite happens. This evidence suggests that firms in these sectors do not employ additional, possibly badly matched refugee workers with low productivity (see Appendix Section H for a discussion). In a separate analysis, we relate observed qualifications of refugees to qualifications of resident citizens in similar jobs, and find no evidence that sector and region restrictions force refugees to take up work for which they are overqualified (see Appendix G). We thus find no support for a skill mismatch explanation.

Figure 5: Can worker, job, and firm characteristics explain the wage effects?



Notes: This figure illustrates and extends the wage regression presented in column 3 of Table 2, panel B. The first coefficient shows the wage effects if we do not control for workers’ tenure. The remaining coefficients show wage effects if we add fixed effects for NACE (rev. 2) two-digit industries and canton of work, for eight levels of educational attainment, accumulated work experience (months employed since arrival in Switzerland, in levels and squared), and fixed effects for ISCO two-digit occupation and five management levels. The underlying samples vary slightly due to missing observations in some covariates. Vertical lines show 95%-confidence intervals clustered at the canton \times status-group level.

5.3.2 The role of outside options.

Taken together, the results presented in the last section suggest that productivity is unlikely to explain why sector and mobility restrictions depress hourly wages. One plausible alternative explanation for the wage effect is that sector and region restrictions reduce workers’ outside options. Leading models of imperfect labor markets predict that poor outside options reduce wages and may generate wage differentials—even among equally productive workers. In the

following, we evaluate the extent to which outside options are able to explain the wage penalty of sector and region restrictions.

Effects on outside options. Our analysis of non-refugee commuting patterns and the sectoral composition of refugees in unrestricted cantons suggests that sector and region restrictions remove up to two-thirds of refugees' potential jobs (see Figure 3). The policies also lower refugees' employment rate (Table 1). To further corroborate the policy effects on refugees' outside options, we now show that sectoral and regional restrictions reduce the number of firms employing refugees and diminish job-to-job mobility.

Appendix I analyzes whether the restrictions reduce the number of firms employing refugees. Using the AHV data, we calculate multiple concentration measures of refugee-hiring employers separately by permit, canton, and year. The results from regressing these concentration measures against cantonal policies suggest that refugees in cantons with a larger share of region-restricted jobs face greater employer concentration, consistent with a reduction in outside options. Sector restrictions, on the other hand, do not significantly influence employer concentration. Interestingly, prioritization *reduces* employer concentration, possibly because the policy makes it harder for a single firm to hire many refugees.

Next, we study refugees' job-to-job mobility. If the restrictions reduce workers' outside options, we would also expect a reduction in the arrival rate of outside job offers and thus (voluntary) job changes. Using monthly AHV data, columns 1–4 of Table 4 present regressions of our baseline specification in (1) where the dependent variables are indicators of job separation, separation into non-employment, separation into employment, and job-to-job transition. The fourth outcome variable, for instance, is an indicator equal to one if the worker separates from her current employer and finds a job at another employer within two months without intervening unemployment spell. In all columns, we disregard the data from November and December because of breaks in the firm identifier between two yearly waves of data.²⁵ We present models with canton fixed effects in panel A and more restrictive models with person fixed effects in panel B. The person fixed effects account for the potential effects of the policies on the composition of refugees in employment.

The estimations in columns 3 and 4 of Table 4 suggest that prioritization and sectoral and regional restrictions reduce separations into employment and job-to-job mobility. The effects are large in magnitude. An increase in the share of restricted jobs by, say, 20 p.p. reduces monthly job-to-job transition rates by 0.4–0.5 percentage points, or by approximately 15% relative to the mean transition rate. Enforcing the priority requirement lowers refugees'

²⁵Employer-to-employer transitions occurring between December and January could reflect changes in the firm identifier instead of actual job changes. As in other similar datasets, these changes in firm identifiers happen for many reasons, including relocation, restructuring, and relabeling of firms. Since we lack the data for non-refugee workers to implement corrections based on worker flows, we simply disregard job changes that occur over the turn of the year. This works because firm identifiers do not change within a year.

job mobility by another 0.5–0.75 percentage points ($\approx -20\%$). We do not find effects on separations into non-employment (column 2), consistent with the notion that the policies primarily reduce voluntary job changes but do not directly induce firms to lay off refugees.

Table 4: Effect of labor market policies on job mobility and on-the-job wage growth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Sepa- rations	Separation non-emp.	Separation employment	Job-to-job change	Job-to-job $\Delta e > 0$	Job-to-job $\Delta e < 0$	On-the-job $\Delta e > 0$	On-the-job $\Delta e < 0$
<i>A. Canton fixed effects</i>								
Priority	-0.0038 (0.0037)	0.0018 (0.0032)	-0.0056** (0.0024)	-0.0050*** (0.0019)	-0.0029*** (0.0011)	-0.0020 (0.0012)	-0.0368*** (0.0117)	0.0257** (0.0108)
Share restricted jobs	-0.0146 (0.0110)	0.0076 (0.0091)	-0.0223** (0.0094)	-0.0187** (0.0073)	-0.0085** (0.0033)	-0.0100** (0.0043)	-0.0020 (0.0291)	0.0048 (0.0278)
<i>B. Individual fixed effects</i>								
Priority	-0.0021 (0.0067)	0.0051 (0.0058)	-0.0072* (0.0043)	-0.0074* (0.0040)	-0.0042* (0.0025)	-0.0033 (0.0021)	-0.0408 (0.0246)	0.0338 (0.0233)
Share restricted jobs	-0.0387* (0.0226)	-0.0152 (0.0166)	-0.0234** (0.0109)	-0.0219** (0.0088)	-0.0081 (0.0058)	-0.0133*** (0.0044)	0.0015 (0.0705)	0.0190 (0.0722)
Outcome mean	0.1108	0.0774	0.0333	0.0286	0.0153	0.0130	0.7248	0.2458
Num. individuals	11,515	11,515	11,515	11,515	11,515	11,515	259	259
Observations	394,779	394,779	394,779	394,779	394,779	394,779	19,273	19,273

Notes: This table shows the effect of the labor market restrictions on job separations, job mobility, and within-job wage growth of employed refugees based on specification (1). We pool all transition groups. All columns include month, canton, month-since-arrival, and months-to-decision fixed effects, interacted with dummies for the three transition groups. The outcome in column 1 is equal to one if the worker separates from his employer between t and $t + 2$. Column 2 uses an indicator equal to one if the worker transitions to unemployment, zero otherwise. Column 3 uses an indicator equal to one if the worker separates from the main job between month t and finds a job at another employer until month $t + 2$. The outcome in column 4 is identical but disregards observations with intervening unemployment spells. In column 5 (column 6), the indicator is one only if the change of employers leads to a wage increase (decrease). In each case, we focus on workers' main jobs, defined as the highest paying job at time t . We disregard November–December in each year due to breaks in the firm identifier at the turn of the calendar year (see text for information). Column 7 and 8 focus on on-the-job wage changes. The sample is restricted to workers that worked for the same employer already in the year before. The outcome in column 7 (column 8) is a dummy variable equal to one if a worker's monthly earnings in December are smaller (larger) compared to her (nominal) monthly earnings in the same job in December the year before. Column 7 and 8 only use December data because the spell data in the AHV does not reveal month-on-month wage changes within a given employer-year. Signif. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Channels. Having established that region and sector restrictions reduce outside options, we now explore the explanatory power of three mechanisms describing how outside options suppress refugee wages in models of imperfect labor markets. First, in dynamic models of monopsony, the wage effect arises because firms post lower wages to groups of workers supplying labor more inelastically to individual firms, for instance, because the group faces larger search frictions. Workers with fewer potential employers are easier to attract and retain because they respond less to changes in firm and market conditions (see Ashenfelter, Farber, and Ransom, 2010; Manning, 2021, for overviews). Second, in dynamic monopsony models and some search and bargaining models, workers with fewer potential employers also

have lower wages because they have fewer options to work their way into well-paying jobs (e.g., Manning, 2003). Third, in a large class of search and bargaining models, workers negotiate—and possibly renegotiate—wages on the basis of outside job opportunities (e.g., Cahuc, Postel-Vinay, and Robin, 2006). In such models, wages increase with the arrival rate of outside job offers.

Separation elasticities. In monopsony models, the labor supply elasticity is the central parameter determining firms’ wage-setting power. To illuminate the role of monopsonistic forces in explaining the wage effect of labor market restrictions, we follow a large recent literature, summarized in Sokolova and Sorensen (2021), and approximate firms’ labor supply elasticity by estimating quit elasticities of refugee workers, separately for refugees in restrictive and less restrictive cantons. In steady state, the quit elasticity is directly proportional to firms’ labor supply elasticity (Manning, 2003).

Our approach closely follows Langella and Manning (2021) and uses the monthly AHV data. We first residualize refugees’ monthly earnings using our baseline specification with the full set of controls, month, canton, and month-since-arrival \times status FE. The residualized earnings reveal whether a refugee earns relatively much or little given her observed characteristics and circumstances. We then assign workers into 20 bins of log residualized monthly earnings and regress job-to-job transitions (as defined in Table 4) on the group dummies. The key question is how much job-to-job mobility increases when pay is bad, and whether this reaction relates to the restrictions.

Figure 6, Panel A, shows the relative job-to-job transition rate by income groups. We express the estimated coefficients in terms of log differences to the base group of CHF 2000. The figure shows that refugees who are low-paid given their characteristics (shown on the left of the graph) have an approximately 20-30% higher quit rate than the base group. Conversely, comparatively high-paid refugees (shown on the right of the graph) have an approximately 20-30% lower quit rate than the base group. The figure also plots two regression lines, fitted either through all coefficients (dashed line) or disregarding the two outlier coefficients in the tails (solid line). Since we use logarithmic scales for both axes, the slopes of the lines represent estimates of the wage elasticity of quits. A flatter line corresponds to a lower elasticity. Since the slopes are not vertical, firms’ labor supply elasticity is finite and the firms possess monopsony power vis-à-vis refugees. Our preferred estimate of the wage elasticity of quits, which disregards the two outlier coefficients at the tails, is -0.36 . This estimate is very close to the main separation elasticities reported by Langella and Manning (2021) for the comparable US (-0.31) and UK (-0.37) data.

Panels B–D in Figure 6 examine whether labor market restrictions relate to the quit elasticity. We estimate separate regressions for cantons where the priority rule is enforced or not (panel B), with or without region restrictions (panel C), and with or without sector

restrictions (panel D). The evidence supports the hypothesis that the policies reduce the quit elasticity: the estimated wage elasticities are smaller if the policies apply. In each case, we omit the two groups at the tails to compute the fitted regression line although the evidence that restrictive policies reduce quit elasticities would be even stronger if we included these outliers.

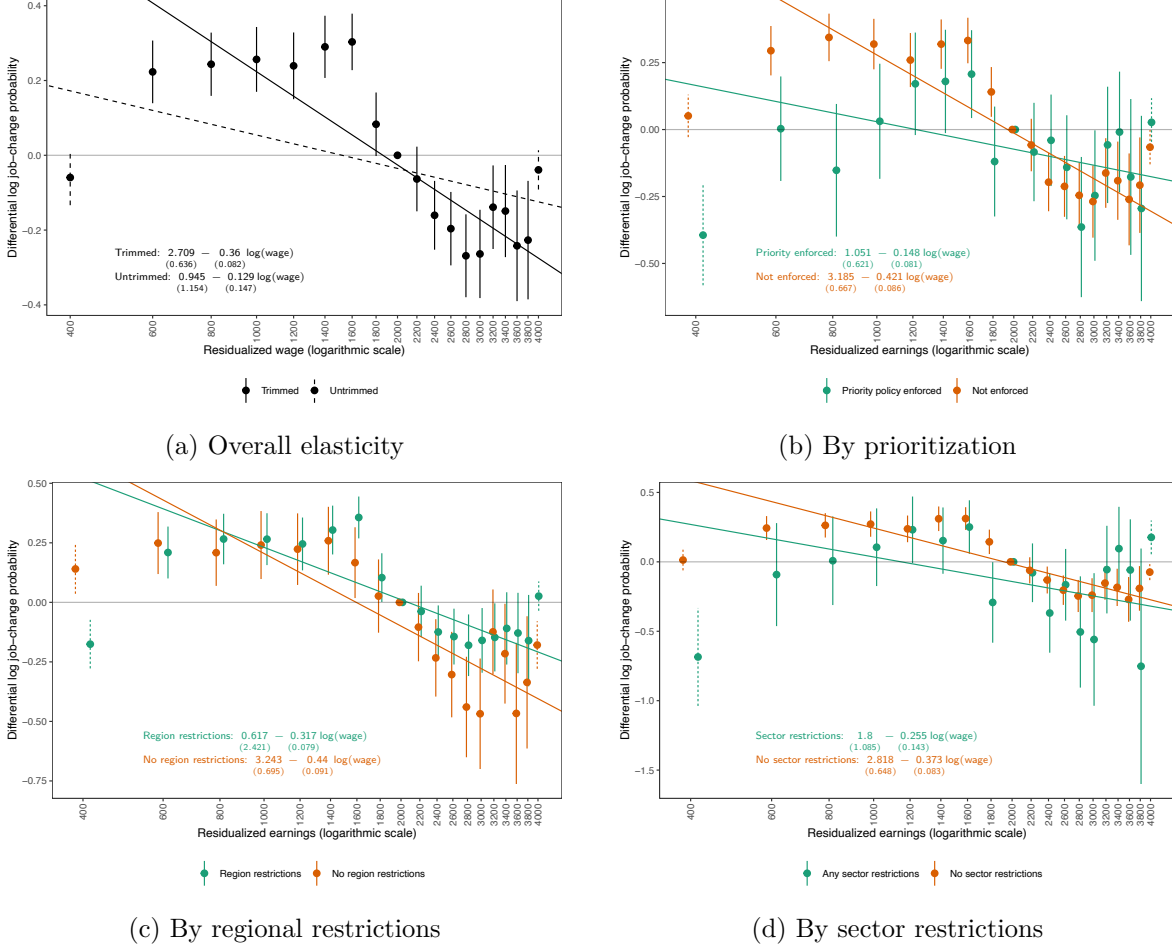
Taken together, we conclude that regional and sectoral restrictions and prioritization are associated with a lower wage elasticity of quits, consistent with the prediction from monopsonistic models. While the estimated elasticities do not have a causal interpretation since we do not have exogenous variation in the wage at the firm level, as in Langella and Manning (2021), our interest lies less in the estimate of the level of the elasticity as such but in the comparison by policy environment. If the biases are constant, then the conclusions about this variation could be valid even if the level is not.

Between-job wage growth. A plausible mechanism consistent with our previous results is that it is more difficult for non-employed refugees to find a well-paid job. Table 4 examines whether, in addition, it is more difficult for employed refugees to climb the wage ladder. To this end, we simply estimate separate policy effects on job-to-job mobility for job changes accompanied by an increase (column 5) or decrease (column 6) in monthly earnings. The regressions suggest that prioritization and sector and region restrictions reduce job transitions irrespective of whether they are associated with a higher or lower monthly income. Hence, refugees' pay penalty in restrictive cantons does not seem to arise because of the greater difficulty to climb the wage ladder. These results are consistent with Figure 5, which also suggests that the sector and mobility restrictions do not lower wages by preventing employed refugees to switch to better-paying occupations or jobs higher up in the firm hierarchy. Instead, our evidence suggests that the wage effect happens primarily when non-employed refugees enter the labor market.

On-the-job wage growth. To examine the bargaining power mechanism, we study the impact of the policies on salary increases in ongoing jobs. The dependent variable in column 7 (column 8) of Table 4 is a dummy variable equal to one if a worker's nominal monthly earnings in December are smaller (larger) compared to his or her earnings in the same job in the same month of the previous year. The sample is restricted to workers that do not change employers. We focus on the monthly earnings in December and discard the remaining months because the spell-level earnings records do not reveal within-job month-on-month wage changes within a calendar year.²⁶ The analysis reveals that enforcing priority to resi-

²⁶For each job spell and calendar year, our data contain the start and end date plus the total earnings over the duration of the spell within the calendar year. Therefore, we do not observe within-job variation in monthly earnings in a calendar year.

Figure 6: Labor market restrictions and the elasticity of separations into employment.



Notes: The figure plot the differential log job-change probability across 19 income groups relative to the base group (group 9). The construction of the plot follows Langella and Manning (2021). To define the income groups, we first regress log monthly earnings on our base specification with month, canton, and month-since-arrival \times status FE and additional controls. The income groups are then obtained by rounding the exponentiated residuals (plus the outcome mean) to the nearest multiple of 200 CHF (from 400 to 4000 CHF). The probability differentials shown are calculated by regressing job-to-job changes against income groups and calculating the log difference to the base group. The job-to-job indicator is equal to one if the employee exits from the main job between month t and $t + 2$ while remaining in employment without intermittent unemployment spells. We disregard November and December in each year because of breaks in the firm identifier between two yearly waves of data. Figures (b) to (d) show the job-change differentials depending on the policy status. The plotted regression curve is obtained by regressing the differential log job-change probability against groups' log earnings. We exclude the bottom and top end of the income distribution, except in Figure (a) where compare the trimmed and untrimmed regression curves. The slope of the curve measures how elastic quits respond to changes in wages.

dents reduces refugees’ chances to experience an increase in monthly earnings in an ongoing employment relationship (panel A, column 7), but increases the chances of a decrease (panel A, column 8). Prioritization could thus lead refugees to be paid comparatively well at the beginning of an employment relationship at the cost of reduced wage growth thereafter. The regressions do not provide evidence that the share of restricted jobs affects on-the-job wage growth. This finding does not suggest that refugee wages are lower in restrictive cantons because of a weaker bargaining position in wage renegotiations. While these results are consistent with the (a priori plausible) assumption of wage posting by employers in refugee labor markets, they do not rule out that the negotiation channel plays a role. It could be that the worse bargaining position matters in negotiations about starting wages.

6 Long-run effects

Existing studies on the effects of employment bans (e.g. Fasani, Frattini, and Minale, 2021; Marbach, Hainmueller, and Hangartner, 2018) or initial labor market conditions (e.g. Von Wachter, 2020; Aslund and Rooth, 2007) suggest that labor market entrants starting their careers in bad conditions may bear—in some cases long-lasting—scars. The scarring effects of restrictive policies may depress refugees’ earnings and employment and thus contribute to the gap in labor market outcomes between refugees and observationally similar native citizens. Thus, we now investigate whether restrictive policies leave longer-term scars.

6.1 Empirical approach

To explore the medium- and long-run policy effects, we relate outcomes up to ten years after arrival in Switzerland to refugees’ initial policy exposure. We measure the initial policy exposure over the first 12 months after arrival as the share of months with enforcement of prioritization, the average duration of the employment ban (in months), and the average share of the sector- and region-restricted jobs. To ensure that we can follow individuals for at least ten years, we restrict the sample to asylum seekers with entry years 1999 to 2005 who are still in Switzerland and in 2015. We use the following specification to relate refugees’ labor market outcomes to initial labor market policies:

$$Y_{it} = a_\tau + b'_\tau P_{cT} + d_\tau \bar{u}_{cT} + \pi' w_i + \mu_c + \delta_t + \nu_{it} \quad (3)$$

where Y_{it} is annual employment (defined as at least one month in employment in a given year) or annual labor earnings observed in year t for refugee i . The index $\tau(i, t)$ denotes the number of years since arrival in Switzerland for $\tau \in \{1, \dots, 10\}$. The vector P_{cT} measures initial policy exposure as defined above, which depends on the time of arrival $T(i)$ and the assigned canton

$c(i)$.²⁷ The fixed effects a_τ describe the typically concave path of labor market integration as a function of years in Switzerland in the absence of labor market restrictions (akin to Figure 1). The vector b_τ captures deviations from this integration path due to initial policy exposure. We benchmark the policy effects with the scarring effects from initial labor market conditions by including the initial unemployment rate averaged over the first 12 months after arrival, \bar{u}_{cT} . The vector d_τ measures deviations from the integration path due to initial labor market conditions as in, e.g., Von Wachter (2020). We furthermore control for the vector of individual characteristics w_i , which includes age, age-squared as well as month-to-decision, and squared months-to-decision. As in the short-run models, we pool permit transition groups (i.e., N→B, N→TAR, N→TAF) by interacting canton, cohort, and year fixed effects with permit status.²⁸

In the base specification in (3), we include canton and outcome-year fixed effects. Since the initial policy environment is likely correlated with the policy environment in years after a refugee’s entry, the base specification identifies the average change in labor market outcomes due to those initial restrictions, including the usual evolution of restrictions experienced afterward (see e.g. Oreopoulos, Von Wachter, and Heisz, 2012). Another interesting policy parameter is the effect of those initial restrictions *net* of effects from policy exposure in subsequent years. Thus, alongside our baseline specification with canton and year fixed effects (labeled as “additive” specification), we also present estimates including canton-year-interactions absorbing effects of the later policy environment and local economic conditions (labeled as “multiplicative”). The net effect of initial restrictions can also be illustrated by evaluating long-term effects only for refugees for whom essentially all restrictions fall away when they receive refugee status recognition: those transiting from permit N to permit B. For this group, there are no contemporaneous employment restrictions whose effects could add to the effects of the initial policy environment.

6.2 Long-run effects on labor market outcomes

Figure 7 plots the effects of the labor market policies on employment and earnings in each year after arrival using our base specification (3) (“All additive”), the specification with the canton-year-interactions (“All multiplicative”), and the base specification for the status transition group N→B only (“N→B additive”).²⁹ We make the following observations. First, the enforcement of prioritization in all months in the first year (vs. in no month) reduces employment by about 12 p.p. and earnings by around in the first two years after arrival. This represents a sizable 66% or 36% reduction of average employment in years one and two,

²⁷Note that these initial conditions do not depend on the asylum decision, but are the same for all asylum applicants arriving at the same time and assigned to the same canton.

²⁸For completeness, we present separate estimates by permit transition groups in Appendix D. Due to the small sample, we do not present results for the N→TAR group.

²⁹Note that due to different samples and levels of aggregation (annual vs monthly), the yearly effects reported here in the first five years can deviate from the 5-year average effect reported above in section 4.

respectively. In these two years, panel B also shows a strong decline in earnings around 92% and 56%, respectively. In year three, the point estimates are still negative for both outcomes but not statistically significant and fade out thereafter.

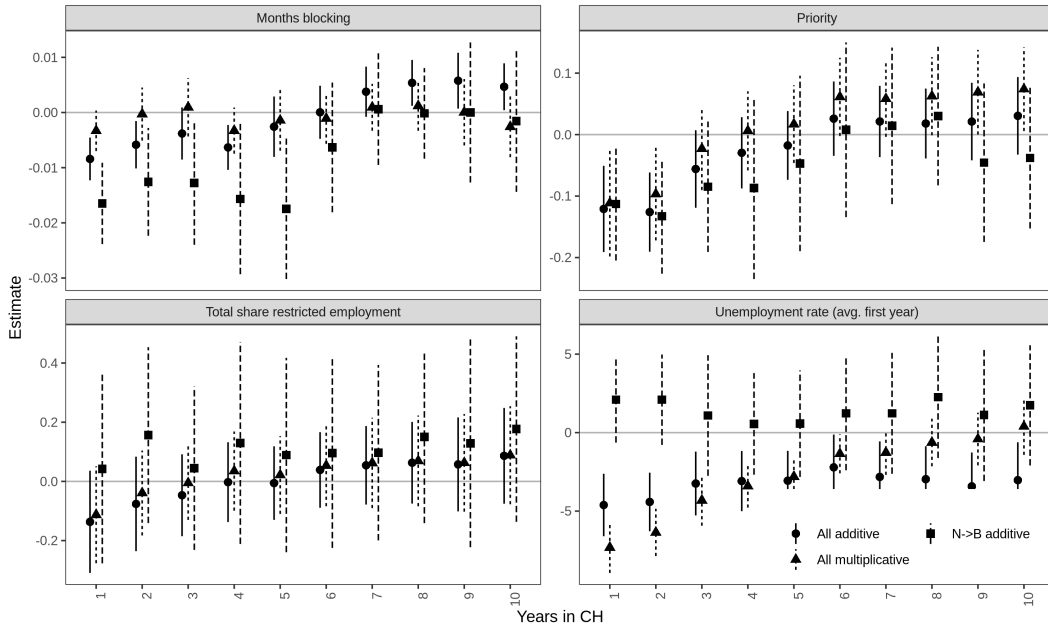
Second, the number of months banned from working has the largest effect on employment in the first year after arrival (when the ban is effective). The effects are small in absolute size in years 2–4 and close to zero thereafter. Evaluated at the median duration of 3 months, bans lower employment by roughly 2.4 p.p. (3×0.8 p.p.) in the first year and about half to a third of that magnitude in years 2–4. The effect on earnings is also concentrated in the first year after arrival (roughly an 11% reduction per month blocking or around 33% for an average ban duration) and zero thereafter. The dynamics of these effects parallel those documented by Fasani, Frattini, and Minale, 2021. In Appendix D.2, we use SESS data to document that employment bans of more than three months during the first year of arrival also reduce refugees’ hourly wages 4–6 years after arrival in Switzerland. This effect fades out in year 7–9. In addition, we find that lower human capital accumulation could be one potential mechanism explaining how longer employment bans reduce wages. In particular, such bans reduce the probability that refugee workers have acquired firm-based informal secondary education (i.e., on-the-job training).

Third, the initial share of restricted jobs also reduces earnings in the first year significantly, whereas the effects on employment are sizeable but statistically insignificant. In the first year, removing a third of available jobs reduces earnings by roughly 28%. In years two and three, the point estimates for the effect on earnings are still negative and sizable but the confidence intervals do not allow us to rule out a zero effect. The effects fade out from year four onward.

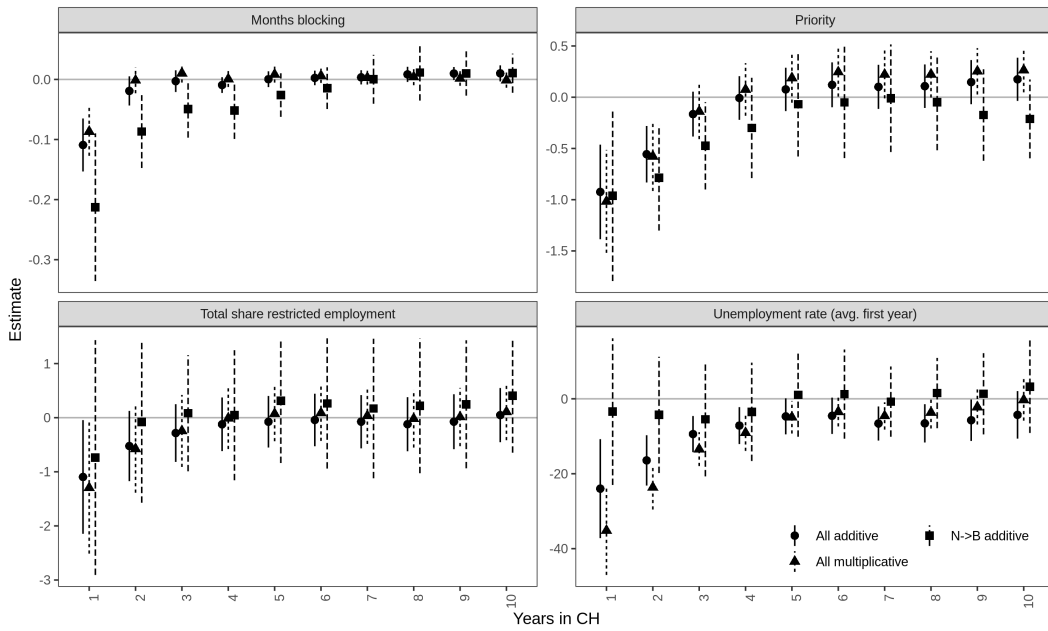
The estimates showing the *net impact* of the initial policy environment based on the specification with the canton-year-interactions (from the “multiplicative” specification in Figure 7) generally follow a similar dynamic and have mostly similar effect sizes in the first years after arrival as the baseline estimates with additive canton and year fixed effects. The effects also follow a similar qualitative pattern when we focus on the subsample of refugees receiving a B permit who face no restrictions after asylum recognition. While the confidence intervals are generally larger due to the smaller sample, the estimated medium- and long-run effects of employment bans are larger in absolute size for both outcomes and also slightly more long-lasting. Also, the effects on prioritization show more persistently negative effects on employment, lasting until the 5th year after arrival.

In summary, we observe strong harmful effects on both employment and earnings during the first years after arrival for both employment and earnings of refugees, particularly of employment bans and prioritization, but also for sectoral and residency-based restrictions. Although some of the policies are only imposed prior to the asylum decision, their effects are detectable for some groups during the first five years after arrival. Benchmarking these results against the effects of unfavorable economic conditions suggests that the medium-run effects

Figure 7: Short- and long-run effects of initial policies and unemployment on annual labor earnings and employment



(a) Effects on employment



(b) Effects on earnings (Poisson)

Notes: This figure shows the effects of the labor market restrictions and the average cantonal unemployment rate on employment (panel A) and total earnings (panel B) of refugees in years 1–10 after their arrival in Switzerland based on the specification (3). Estimates are based on specifications including canton and year fixed effects (labelled with “All additive”) and canton-year interactions (“All multiplicative”), respectively, pooling refugees from all permit groups (N→B, N→TAF, and N→TAR) arriving between 1999 and 2005, and still residing in Switzerland in 2015. Estimates labelled “N→B additive” only show effects for refugees transiting to a B permit based on specifications with canton and year fixed effects. We control for age, age-squared, wait months until decision (in months) and squared.

of the policies studied here are at least as, or even more harmful, than adverse economic conditions.³⁰ A comparison with policies that boost refugees’ labor force attachment, such as language training or placing refugees in regions with strong economic performance (Bansak et al., 2018; Foged, Hasager, and Peri, 2022; Foged et al., 2022; Arendt, Dustmann, and Ku, 2022), suggests that removing the restrictions discussed here is more effective, particularly in the first years after arrival.

6.3 Potential benefits motivating restrictions for refugees

Potential benefits to other immigrants. One motivation for implementing labor market restrictions for refugees put forward by policymakers is the protection of other vulnerable groups from the competition of refugees. Since it would be hard to detect spillover effects on Swiss residents³¹, we investigate whether restrictive policies have positive effects on a sample that is more likely to compete with refugees: low-paid immigrants from EU-15 countries. To document the effects on low-paid immigrants, we estimate the policies’ earnings effects using unconditional quantile regression. We can interpret the effects on the upper quantiles as placebo tests because highly paid EU immigrants are unlikely to compete directly with refugees. No effects in the upper quartiles would further reduce concerns that labor restrictions for refugees correlate with unobserved changes in labor market conditions, thereby confounding the policy effects.

For this analysis, we use the linked AHV-STATPOP data which covers all EU-15 immigrants residing in Switzerland. The design resembles the long-run model in equation (3) for refugees. However, we can only consider the years after 2005 since the canton of residence is not contained in STATPOP for earlier years.³² Figure 8 shows that we find no evidence for significant positive spillover (nor placebo) effects on earnings for EU-15 migrants for all three policies—months of blocking, prioritization, and total share of restricted employment. This pattern holds across all quartiles, including the first which consists of low-paid EU-15 migrants that are most likely to compete with refugees in the labor market. With the exception of the first quartile, the estimation of these effects is sufficiently precise to rule out all but very small effects. Figure D.1 shows a similar pattern for employment. Among the three

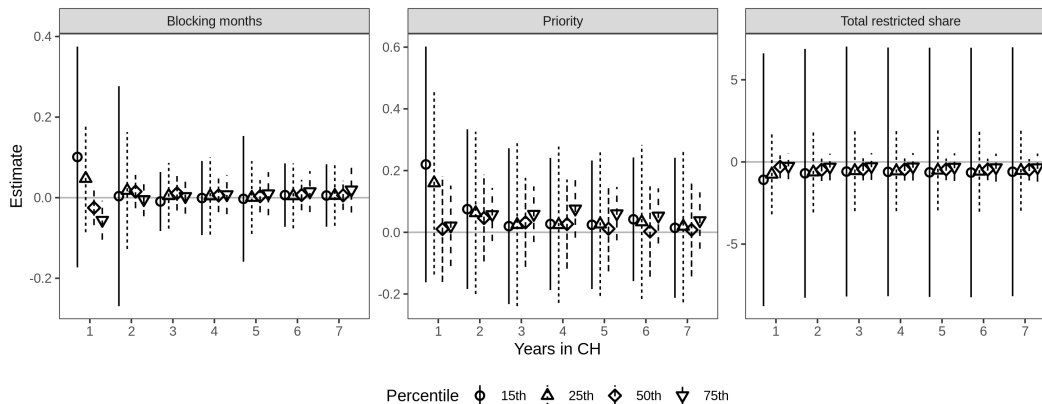
³⁰Weak local economic performance can also severely reduce the number of available job options, particularly for vulnerable populations (Von Wachter, 2020; Schwandt and Von Wachter, 2019; Aslund and Rooth, 2007). We proxy economic conditions with the cantonal unemployment rate during an individual’s first year in Switzerland. Figure 7 shows that an average increase in the local unemployment rate (of +1.2 p.p.) as during the Great Recession in the first year of arrival lowers employment by about 5.4 p.p in the first two years after arrival and earnings by 30% and 20%, respectively.

³¹As shown by the very large unconditional wage gap between Swiss residents and refugees in column 1 of Table 3, Swiss citizens and refugees do not typically work in the same labor market segments. The unconditional and conditional wage gap is much smaller compared to other immigrant workers.

³²There are two more small differences. First, since we do not observe the month of arrival for EU-15 immigrants, we use the policy environment in January. Second, we only observe age groups for EU immigrants rather than the precise age. We thus control for age group interacted with gender.

policies considered, we again find no significant placebo or spillover effects.

Figure 8: Spillover and placebo effects on earnings for EU-15 immigrants not subject to restrictions



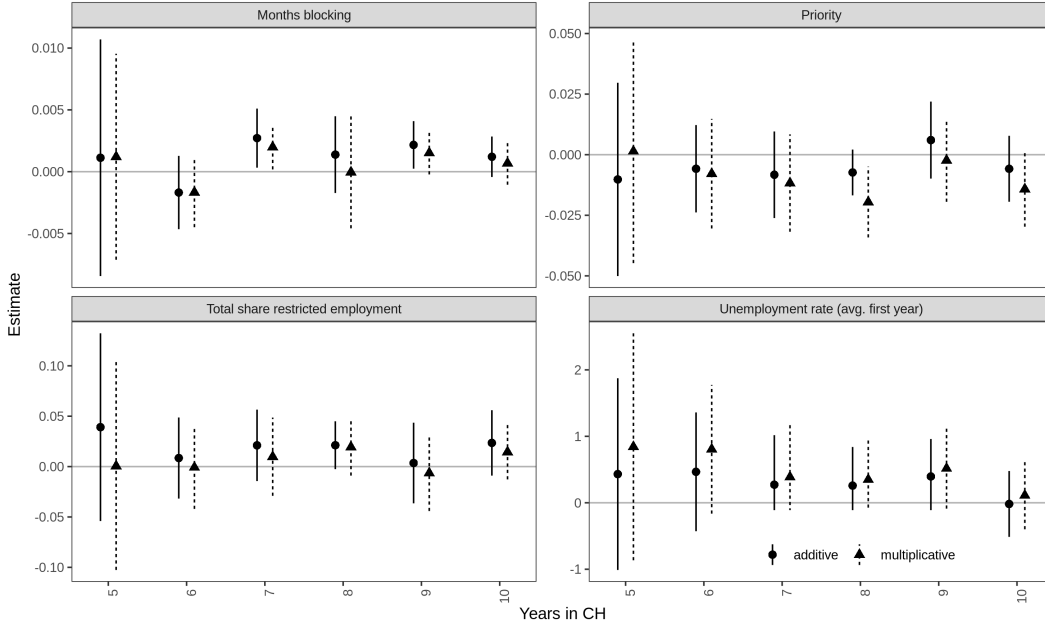
Notes: The sample includes all EU-15 immigrants arriving in 2005-2008 and who still reside in Switzerland in 2015. The estimation model is given by (3). We control for age group interacted with gender. Figure (a) shows the effects on employment (recorded in AHV) using a linear fixed effect model. Figure (b) reports the effects at different percentiles on total earnings (IHS transformed) using unconditional quantile regression.

These results corroborate, when interpreted as a placebo test, that our estimates of the impact of restrictions on refugees' labor outcomes have a causal interpretation and are unlikely confounded by differential trends in labor market conditions across cantons. When interpreted as a test for positive spillover effects on refugees' likely competitors in the labor market, these estimates cast doubts on claims that restrictive policies, while hurting refugees, will benefit other residents with similar characteristics.

Effects on emigration. Another motivation for why policymakers might want to restrict refugees' access to the labor market is to encourage their emigration, particularly for those with subsidiary protection (TAF or TAR permit). Since the emigration is not consistently recorded in the ZEMIS register, we draw from the STATPOP register which allows us to measure emigration directly, but only for the years 2011–2015. In order to estimate the long-run effects on emigration for years 5–10 after arrival in Switzerland, we also include asylum seekers arriving in years 2006 and 2007 (in addition to entry years 1999–2005 considered above). On average, roughly 3.4% of the refugees in the sample leave Switzerland each year.

Figure 9, based on the long-run specification (3), provides little evidence that restricting refugees' initial labor market access leads to their emigration in the medium- to long-run. Prioritization, if anything, reduces the likelihood of emigration. These effects, however, are only significant in year 8 in the specification showing the net impact (with canton-year interactions). In the case of employment bans, there is a small positive and marginally significant effect towards the end of the observation period. Yet, even evaluated at the median ban

Figure 9: Short- and long-run effects of initial policies and unemployment on emigration



Notes: This figure shows the effect of initial labor market restrictions or the effect of initial local economic conditions on the likelihood that refugees emigrate from Switzerland. We use STATPOP data where we can measure emigration directly for the years 2011–2015. The sample of refugees includes all cohorts arriving in 2007 or earlier and individuals who reside in Switzerland in 2015. The estimation model is given by (3). We control for age, age-squared, wait months until decision (in months) and squared.

duration of 3 months, employment bans increase the likelihood of emigration by less than half of a percentage point. Lastly, the figure shows that there is no evidence that sector or geographic restrictions induce emigration. These effects are generally supported when we use an alternative emigration measure derived from AHV data.³³ The absence of an emigration response renders the labor restrictions potentially very costly to host societies since refugees with little labor income will stay in the country and will have to be supported with social assistance.

7 Conclusion

This paper analyzes the employment, wage, and job mobility effects of labor market policies regulating whether, where, and for whom refugees are allowed to work. We focus on four policies that enjoy widespread popularity across Europe and beyond: temporary employment

³³Using AHV data, we approximate a person’s emigration with an indicator if she does not appear in the register for at least five years. Aside from emigration, a person can only leave the AHV register because of death or due to an exemption: married spouses without labor earnings and unemployment benefits are exempt from contributing to the AHV if their partner contributes twice the minimum amount. These events are relatively rare, allowing us to approximate emigration. Results are provided in Figure D.2.

bans that prevent refugees from working in the first months after arrival, prioritization of citizens and foreigners with more permanent residence permits, sector restrictions, and restrictions on geographic mobility. The analyses leverage that Swiss cantons enjoy significant discretion in applying these labor market restrictions. Covering 1999–2016, we combine these policies with administrative data on refugees’ asylum processes, earnings records, and linked employer-employee data that provide employer-reported hours worked and hourly wages. Using a range of individual-level panel regression specifications, we find that all policies have adverse employment and earnings effects. The estimated effects are substantial in size: moving from the least to the most restrictive policy mix reduces the employment rate of affected refugees from 19% to 11%. We also find that when employed, sectoral and geographical reduce refugees’ hourly wage rate. For policies prioritizing the resident workforce, we find no depressing effects on refugees’ hourly wages, but at the cost of reduced hours worked per month, lower employment, and lower wage growth on the job. Together, our findings suggest that these labor market policies are an important reason for why refugees have lower employment rates and wages than similar native citizens. For theory, our results on the wage effects of sector and region restrictions suggest a crucial role of outside options in determining the level of workers’ pay.

We also take initial steps toward estimating labor restrictions’ costs and potential benefits for refugees and host countries. First, we find that restrictive labor market policies impair refugees’ economic integration even in the medium run. In line with a large literature that shows that adverse initial labor market conditions leave long-term scars among unlucky cohorts (see, e.g. Von Wachter, 2020), initial exposure to prioritization and employment bans reduce refugees’ labor market earnings for up to three and five years, respectively, after they cease applying. Second, we find little evidence that restrictive refugee policies measurably increase the earnings of low-paid EU-15 workers that likely compete with refugees for jobs. These results cast doubt on whether labor restrictions help protect other vulnerable groups from the competition of refugees. Finally, we find that labor restrictions have no—and, if anything, a negative—effect on the probability that refugees emigrate, even for refugees that are only temporarily admitted. This finding contrasts the view popular among some policymakers that labor restrictions provide incentives for asylum seekers and refugees to leave the country (Marbach and Hangartner, 2019). Switzerland’s exogenous dispersal policy prohibits us from analyzing whether cantons with more restrictive policies would attract fewer refugees if refugees were free to choose their residential location. Thus, whether liberal access to the labor market is a magnet in attracting more refugees is an important question for future research. However, for the many outcomes analyzed in this study, the evidence suggests that labor restrictions burden refugees with high costs yet provide little benefits for other immigrants and host societies.

How should we consider the external validity of our findings? While it is critical to

abstain from over-generalizing our estimates, a few factors suggest that our findings have relevance beyond refugees in Switzerland. First, the composition of Switzerland’s refugee and asylum-seeking population is similar to that of other important receiving countries in Europe (Hainmueller, Hangartner, and Lawrence, 2016). Second, and as discussed above, the policies analyzed here are far from unique. Similar policies—including restrictions on work permits, employment bans, and priority and visa regulations—limit labor market access for both refugees and labor migrants in many other countries. Moreover, our results for initial employment bans are consistent with studies that have analyzed the impact of similar bans in other European countries (Marbach, Hainmueller, and Hangartner, 2018; Fasani, Frattini, and Minale, 2021). However, given the many cross-country differences that may moderate effect estimates, more research with similar quasi-experimental designs and register data from other contexts is needed to evaluate better the impact of restrictive labor market policies in other countries.

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Online Appendix

The Labor Market Effects of Restricting Refugees' Employment Opportunities

A Descriptives

A.1 Policies

To assess compliance with the labor and mobility restrictions, we leverage our register data (see Table A.1 in the Appendix). First, according to two distinct databases, AHV and ZEMIS (which we discuss in detail below), only 0.3% or 0.8% of refugees are recorded as employed during months in which they are subject to a ban according to our coding compared to 14% and 19%, respectively, for individuals that are not subject to a ban. This suggests a high compliance rate with the employment ban. Second, with regard to the region restrictions, we find that about 5.8% of employed refugees in a canton that does not allow for extra-cantonal commuters are, in fact, cross-cantonal commuters, compared to 15.5% working in cantons that allow for it. When interpreting the results, it is important to note even if the governmental register data were to contain no entry errors, we would not necessarily expect perfect compliance with cantonal labor restrictions, since some asylum seekers and refugees successfully petition the canton for a ‘hardship clause’ to take up work. Cantons have the discretion to decide on such extra-cantonal work permits on a case-by-case basis. Lastly, we turn to sector restrictions. As discussed in the previous section, there is a large variation in the number of sectors in which refugees are (not) allowed to work. For our compliance analysis, we consider the sectors for which all cantons that implement sector restrictions generally prohibit access. We refer to these sectors as ‘always restricted’ sectors. We find that about 21.5% of refugees are employed in ‘always restricted’ sectors in canton-months when access to these sectors is indeed restricted. This share increases to 33.7% for employment in the same sectors in canton-months where there are no sector restrictions. These results suggest that cantons apply these sector restrictions with some discretion, in particular to refugees who hold a valid work permit in an occupation for which access is only later restricted. At the same time, the results indicate that sector restrictions also have some ‘bite’, as the employment share in these occupations is 12 percentage points higher (a more than 50% increase) if sector restrictions are lifted.

Table A.1: Contingency table of labor restriction policies against employment or type of employment

Policy	Yes	No	Share
<i>A. Banned from working</i>			
	<i>Employed (AHV)</i>		
No	442838	2891312	13.28%
Yes	471	233120	0.2%
Missing	1191	17388	6.41%
<i>B. Banned from working</i>			
	<i>Employed (ZEMIS)</i>		
No	478806	2069231	18.79%
Yes	1688	223726	0.75%
Missing	1851	7145	20.58%
<i>C. Region restriction</i>			
	<i>Cross-canton commuter</i>		
Allowed	76167	419725	15.36%
Not allowed	7982	132617	5.68%
Missing	1183	7041	14.38%
<i>D. Sector restriction</i>			
	<i>Employed in 'always restricted' sector (ZEMIS)</i>		
Any restrictions	7551	28146	21.15
No restrictions	74102	144920	33.83
	6198	9068	40.60
<i>E. Sector restriction</i>			
	<i>Newly employed in 'always restricted' sector (ZEMIS)</i>		
Any restrictions	520	1816	22.26
No restrictions	4308	7069	37.87

Notes: Panel A and B show the number of person-months observations by employment ban policy and employment status using AHV and ZEMIS data, respectively. Panel C shows the number of person-months in employment by cross-canton commuter status and by whether the canton of work allows refugees from other cantons to be employed. Panel D distinguishes between two types of sectors: sectors that are always restricted when any sector policies are imposed and sectors that may be exempt from restrictions. The table in Panel D shows employment months for ‘always restricted’ and other sectors by whether any sector restrictions are currently in place. (Panel D uses the employment indicator from ZEMIS since sector association is not recorded in the AHV data.) Panel E uses the same approach as Panel D, but only focuses on newly employed.

A.2 Merged short and long-run data

Table A.2: Descriptive statistics

	Mean	Sd.	P. ₀₁	P. ₅₀	P. ₉₉	Obs.
<i>Panel A. Merged AHV-ZEMIS data, January 2005</i>						
Labor income	2747.51	1965.50	41.31	3173.61	6209.87	2562
Employed (AHV)	0.24	0.43	0.00	0.00	1.00	10657
Employed (ZEMIS)	0.16	0.36	0.00	0.00	1.00	10657
Age	30.89	8.58	18.00	30.00	59.00	10657
Female	0.38	0.49	0.00	0.00	1.00	10657
Months to decision	18.24	22.08	1.00	12.00	125.00	10657
<i>Panel B. Merged AHV-ZEMIS data, January 2015</i>						
Labor income	2290.39	1654.50	50.00	2098.04	5443.74	2382
Employed (AHV)	0.09	0.28	0.00	0.00	1.00	27416
Employed (ZEMIS)	0.08	0.27	0.00	0.00	1.00	27416
Age	30.86	9.30	18.00	29.00	60.00	27416
Female	0.37	0.48	0.00	0.00	1.00	27416
Months to decision	17.20	11.68	1.00	16.00	51.00	27416
<i>Panel C. Merged AHV-ZEMIS-STATPOP data (2005)</i>						
Labor income	24591.54	19002.57	262.00	21786.00	68244.27	5152
Employed (AHV)	6.65	5.04	0.00	7.00	12.00	6877
Age	32.20	7.66	19.00	31.00	53.00	13952
Female	0.39	0.49	0.00	0.00	1.00	13952
<i>Panel E. Merged AHV-ZEMIS-STATPOP data (2015)</i>						
Labor income	34007.90	23169.41	323.87	34303.50	88096.59	17888
Employed (AHV)	7.88	5.08	0.00	12.00	12.00	23047
Age	37.98	8.61	23.00	37.00	62.00	34687
Female	0.35	0.48	0.00	0.00	1.00	34687
<i>Panel F. SSES data (October 2016)</i>						
Hourly wage	25.32	7.84	11.58	24.10	52.65	3834
Monthly labor income	3566.55	1519.02	195.00	3899.81	6672.51	3834
Full-time equivalents	0.79	0.30	0.04	1.00	1.00	3834
Monthly hours worked	143.97	55.45	7.00	177.67	199.33	3834
Female	0.27	0.44	0.00	0.00	1.00	3834
Age	35.59	7.60	22.00	35.00	56.00	3834
Primary education	0.78	0.41	0.00	1.00	1.00	3473
Tertiary education	0.02	0.15	0.00	0.00	1.00	3473
Tenure	2.11	2.31	0.00	1.00	9.00	3834
Hospitality sector	0.22	0.42	0.00	0.00	1.00	3834
Trade sector	0.10	0.30	0.00	0.00	1.00	3834
Construction sector	0.02	0.13	0.00	0.00	1.00	3834

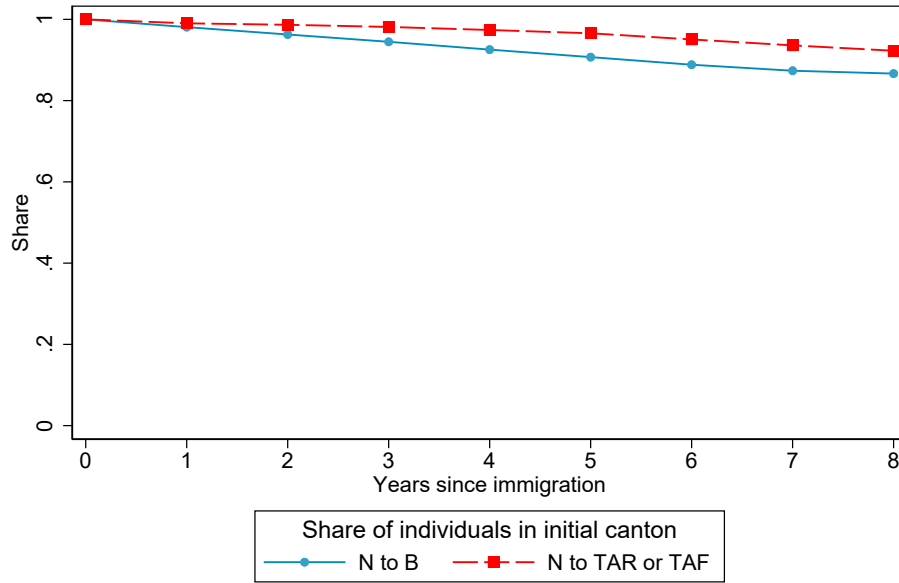
Notes: Panel A and B show summary statistics for the monthly merged AHV-ZEMIS data set covering 1999-2015. This data set focuses on the first 5 years after arrival. Panel C and E refer to an annual long-run panel which allows us to track labor market outcomes beyond the 5-year-window. Panel F shows summary statistics for a complementary data set, the Swiss earnings structure surveys.

Table A.3: Descriptive statistics of refugee and placebo long-run data set.

	Mean	Sd.	P. ₀₁	P. ₅₀	P. ₉₉	Obs.
<i>Panel A. Long-run refugee data set (2005)</i>						
Labor income	24591.54	19002.57	262.00	21786.00	68244.27	5152
Employed (AHV)	6.65	5.04	0.00	7.00	12.00	6877
Age	32.20	7.66	19.00	31.00	53.00	13952
Female	0.39	0.49	0.00	0.00	1.00	13952
<i>Panel B. Long-run refugee data set (2015)</i>						
Labor income	34007.90	23169.41	323.87	34303.50	88096.59	17888
Employed (AHV)	7.88	5.08	0.00	12.00	12.00	23047
Age	37.98	8.61	23.00	37.00	62.00	34687
Female	0.35	0.48	0.00	0.00	1.00	34687
<i>Panel C. Long-run placebo data set (2005)</i>						
Labor income	62524.25	78034.01	775.78	47133.00	323866.71	28479
Employed (AHV)	0.95	0.23	0.00	1.00	1.00	30106
Age	35.07	9.27	21.00	34.00	61.00	30106
Female	0.47	0.50	0.00	0.00	1.00	30106
<i>Panel D. Long-run placebo data set (2015)</i>						
Labor income	74713.29	118216.88	960.00	57200.00	399046.94	316536
Employed (AHV)	0.96	0.19	0.00	1.00	1.00	328398
Age	37.18	10.01	21.00	35.00	63.00	328398
Female	0.44	0.50	0.00	0.00	1.00	328398

Notes: Panel A and B show summary statistics for the annual long-run data set, which includes all refugees in the combined AHV-ZEMIS short-run data set (i.e., individuals applying for asylum in Switzerland between 1999-2015 and aged 16-65 at arrival) who are also in Switzerland in 2015. The data set covers the years 1999-2015. Panel A provides snapshot summary statistics for the year 2005; Panel B for the year 2015. Panel C and D show summary statistics for the placebo samples of EU-15 immigrants.

A.3 Between-canton mobility



Notes: The figure uses data from the Swiss population registers 2010–2018 to show the share of refugees that live in the canton they were assigned to upon arrival, separately for refugees whose asylum claim was granted (transition from N to B permit) and for refugees that were temporally admitted (TAR or TAF). We focus on refugees that arrived in Switzerland in the years 2005–2010.

Figure A.1: Share of refugees living in canton initially assigned to, by years in Switzerland

B Measures of cantonal and sectoral labor market restrictions

This section explains how we construct the measures that summarize the extent to which sector and region restrictions reduce job opportunities for refugees. We construct three measures that quantify the share of all jobs that are unavailable to refugees due to such restrictions.

Share of jobs restricted by region policy. We calculate the share of jobs that are banned for refugees due to region restrictions as

$$share\ region\text{-}restricted\ jobs_{its} = \sum_{j \neq i} commuter\text{-}share_{i \rightarrow j} \times region\text{-}restricted_{jts} \quad (4)$$

where $region\text{-}restricted_{jts}$ is equal to 1 if a refugee with status s residing in other cantons are banned from working in canton j in month t due to region restrictions, 0 otherwise. $commuter\text{-}share_{i \rightarrow j}$ measures the share of residents in canton i who work in canton j . The commuter shares are calculated from the Census 2000 and refer to the total population, i.e., not only to refugees. We employ commuter weights to reflect that region restrictions in cantons that are common work locations for residents (e.g. due to geographic proximity or public transport connections) may have a stronger effect on employment opportunities of refugees.

Share of jobs restricted by sector policy. In a similar fashion, we define the share of jobs restricted due to the sectoral restrictions as

$$share\ sector\text{-}restricted\ jobs_{its} = \sum_{\ell} sector\text{-}share_{\ell} \times sector\text{-}policy_{its\ell} \quad (5)$$

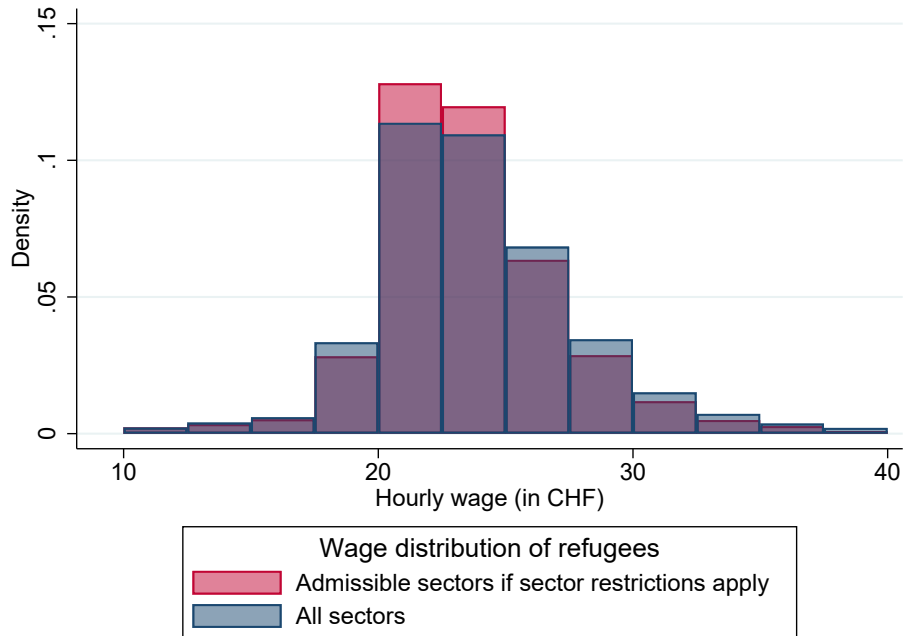
where $sector\text{-}policy_{its\ell}$ is 1 if refugees of status s residing in canton i are banned from working in sector ℓ in the same canton in month t , 0 otherwise. $sector\text{-}share_{\ell}$ is the share of refugees working in sector ℓ . To avoid that the sector shares are distorted by sector restrictions, we calculated the sector shares only using B/TAR refugees that have never been exposed to sector restrictions.

Total share of restricted jobs (main measure). In the main specifications, we employ a joint restriction measure that quantifies the share of jobs that are restricted for refugees due to either region or sectoral restrictions:

$$total\ share\ restricted\ jobs_{its} = \sum_j \sum_{\ell} share_{i \rightarrow j, \ell} \times restriction_{ijts\ell}$$

where $restriction_{ijts\ell}$ is 1 if a refugee of status s residing in canton i is not allowed to work in sector ℓ in canton j in month t either due to extra-cantonal or sectoral restrictions, 0 otherwise. Specifically,

$$restriction_{ijts\ell} = \begin{cases} sector\text{-}restriction_{jts\ell} & \text{if } i = j, \\ \max(extra\text{-}cantonal_{jts}, sector\text{-}restriction_{jts\ell}) & \text{if } i \neq j. \end{cases}$$

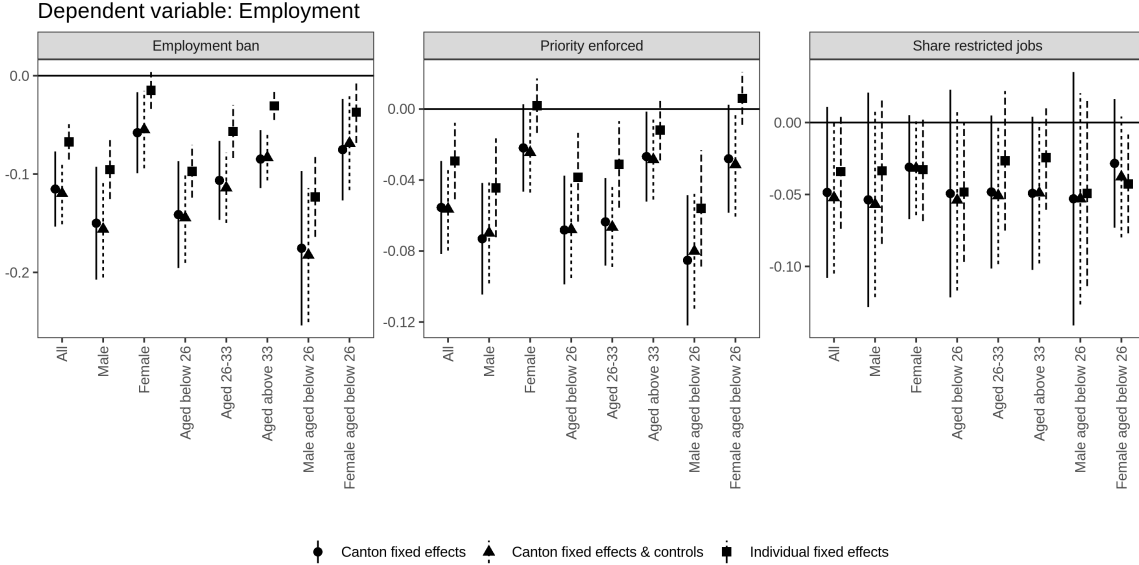


Notes: This figure shows the hourly wage distribution of refugees across all sectors (blue) and in the sectors in which they are typically forced to work if sector restrictions apply (red). The data is from the Swiss Earnings Structure Surveys in 2012, 2014, and 2016. The sample is all refugees with TAR, TAF or B permit, which, in this time period, do not face any sector restrictions. The blue histogram shows the unconditional wage distribution. The red histogram focuses on the following two-digit industries (NACE rev. 2): manufacture of beverages, tobacco products, textiles, wearing apparel, and leather and related products, construction of buildings and civil engineering, accommodation and food and beverages, service activities, human health and residential care activities, and activities of households as employers of domestic personnel. These industries are still admissible for refugees in at least one canton even if that canton applies sector restrictions. Observations with wages above 40 CHF and below 10 CHF are dropped. The figure suggests that the wage distribution across all sectors is similar to the wage distribution of those sectors to which refugees are typically restricted when sectoral restrictions apply.

Figure B.1: Wage distribution of refugees during unrestricted periods in all sectors and typically restricted sectors.

C Heterogeneity and construction of employment score

We investigate the heterogeneity of the policy effects on employment in two different ways. First, Figure C.1 shows that the harmful policy effects are larger for younger and male refugees while the effect is closer to zero for female and older refugees. As labor force participation among male and younger refugees is substantially higher in general, this suggests that the restrictions reduce employment among those with the highest employment potential.



Notes: This figure shows the heterogeneous effects of the labor market restrictions on the employment of different groups of refugees in the first five years after their arrival based on the specification (1). The regression pools refugees from the three permit transition groups (N→B, N→TAF, and N→TAR) and includes interactions of these three groups with month, canton, month-since-arrival, and months-to-decision fixed effects. All regression models include age and age-squared interacted with sex and maximum cash allowance in CHF for refugees. Additional controls are marriage status, two dummies for self-reported religion (Christian and Muslim), nationality, and asylum processing centre fixed effects as well as the contemporaneous unemployment rate and the unemployment rate at arrival. Standard errors are clustered at the canton × transition group level.

Figure C.1: Heterogeneity in the effect of policies on employment, by age and gender

Second, we explore heterogeneity by dividing the refugee sample in four groups from low to high employability. To this end, we predict employment status in the 5th year after asylum application while exclusively relying on individual-level time-invariant characteristics (i.e., age, sex, age at arrival, nationality, religion, native language, marriage status at arrival, ethnicity, family size). The classification model is applied to two randomly split samples and out-of-sample predicted probabilities are used to classify individuals into four groups from low to high employability. We employ logistic elastic net regression with interactions and second-order polynomials, which showed the best classification performance (measured using AUC) among the machine learners considered (extreme gradient boosting, random forests, support vector machines). The use of sample splitting avoids using the same data for both classification into employability groups and for the final estimation. Table C.1 shows summary statistics for each group. For instance, the average predicted employment probability in the

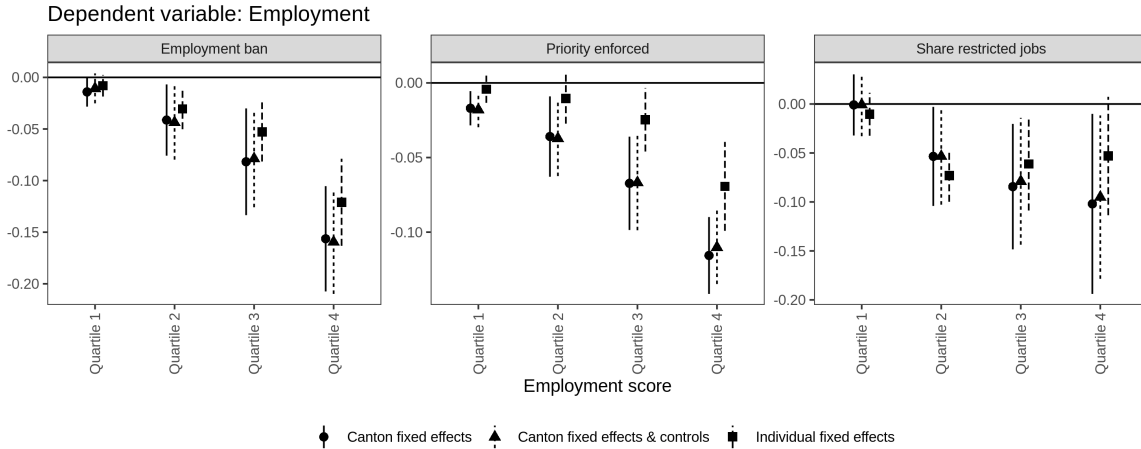
quartiles is 3.5%, 14.9%, 28.7%, and 53.0%, respectively.

Table C.1: Descriptive statistics by employment score group.

Variable	Employment score group			
	1	2	3	4
Employment (%)	3.54	14.92	28.74	53.02
Predicted employment (%)	5.95	15.98	28.48	49.81
Female (%)	57.98	47.44	33.98	7.23
Married (%)	31.22	38.43	48.32	70.74
Muslim (%)	42.76	30.34	32.73	28.73
Christian (%)	8.01	8.06	10.35	8.50
Family size	2.77	2.73	2.54	2.25
Age at application (mean)	31.42	29.88	27.41	26.00
Age at application (p25)	24.00	23.00	21.00	21.00
Age at application (p50)	30.00	29.00	26.00	25.00
Age at application (p75)	37.00	35.00	32.00	30.00

Notes: Descriptive statistics by group ordered from low (group 1) to high (group 4) employment probability. ‘Employment (%)’ indicates the observed employment share in the 5th year after arrival. ‘Predicted employment (%)’ is the out-of-sample predicted employment probability which is used to assign individuals to group 1 to 4.

Figure C.2 reports regression coefficients from separate regressions by employment score groups. The figure reveals that the detrimental effects of employment bans and prioritization increase with the employment score. While standard errors do not allow ruling out zero effects for the bottom quartile, the effects are substantially stronger for the highest two quartiles. Restricting the share of available jobs seems to reduce employment in all quartiles but the bottom quartile.

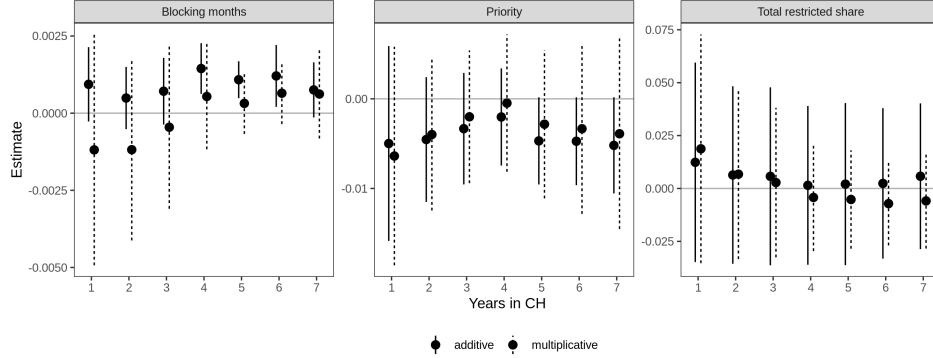


Notes: This figure shows the heterogeneous effects of the labor market restrictions on the employment of groups of refugees in different quartiles of the employment score during the first five years after their arrival based on the specification (1). The employment score measures refugees’ employability and is the predicted likelihood to be employed in the 5th year after arrival given predetermined characteristics such as age sex, nationality, religion, and language. See notes in Figure C.1 for more information.

Figure C.2: Heterogeneity in the effect of policies on employment, by employment score

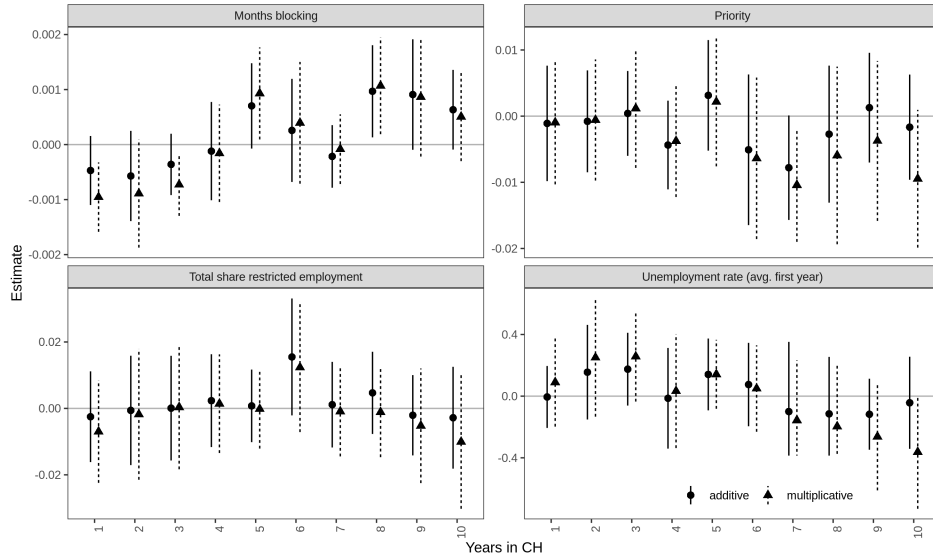
D Long-run analysis

D.1 Complementary long-run results



Notes: The sample includes all EU-15 immigrants arriving in 2005-2008 and who still reside in Switzerland in 2015. The estimation model is given by (3). We control for age group interacted with gender. Figure (a) shows the effects on employment (recorded in AHV) using a linear fixed effect model. Figure (b) reports the effects at different percentiles on total earnings (IHS transformed) using unconditional quantile regression.

Figure D.1: Long-run spillover and placebo effect on employment of EU-15 immigrants not subject to restrictions



Notes: This figure shows the effect of initial labor market restrictions or the effect of initial local economic conditions on the likelihood that refugees emigrate from Switzerland. In Panel A, we use AHV-data and define emigration if a person does not appear in the register for at least five years. Panel B, we use STATPOP data where we can measure emigration directly for the years 2010–2015. The sample of refugees includes all cohorts arriving in 2005 or earlier and individuals who reside in Switzerland in 2015. The estimation model is given by (3). We control for age, age-squared, wait months until decision (in months) and squared.

Figure D.2: Short- and long-run effects of initial policies and emigration (AHV)

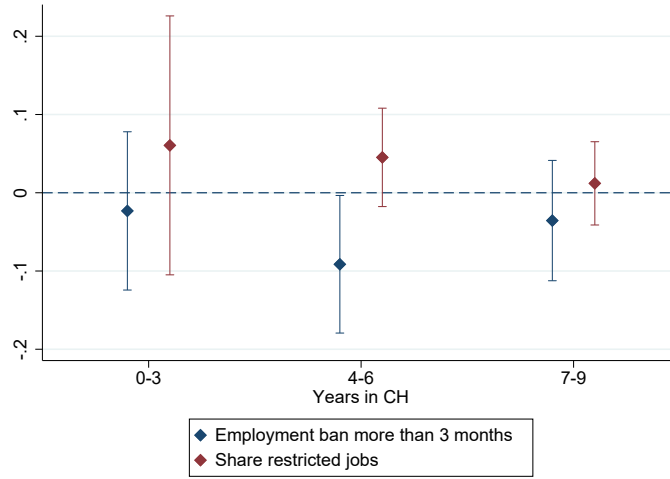
D.2 Long-run effects on wages and education

We explore the dynamic long-run effects on refugees' hourly wages using data from the Swiss Earnings Structure Surveys (SESS) 2012, 2014, 2016, and 2018. Akin to the long-run regression specification for the social security data (equation 3), we relate workers' wages as observed in the survey waves 2012–2018 to the policies for asylum seekers (status N) in place in the canton the refugees were initially assigned after arrival in the years 2002–2008. The sample is refugees transiting from asylum seeker (permit N) to a B, TAR, or TAF permit. Outcomes and policies are measured in October each year. We control for status group fixed effects (N to B and N to TAR/TAF, respectively), initially assigned canton, arrival cohort, and survey wave fixed effects interacted with dummies for the two status groups.³⁴

As in section 6.2, Figure D.3 plots the interaction terms between the initial policies (employment bans and the share restricted jobs) and three indicators of the years after arrival in Switzerland (0–3, 4–6, and 7–9 years in Switzerland). The figure highlights that employment bans of more than 3 months during the first year of arrival reduce refugees' hourly wage rate 4–6 years after arrival in Switzerland. This effect fades out in the years 7–9. Conversely, initial sector and mobility restrictions have no long-run impact on wages.

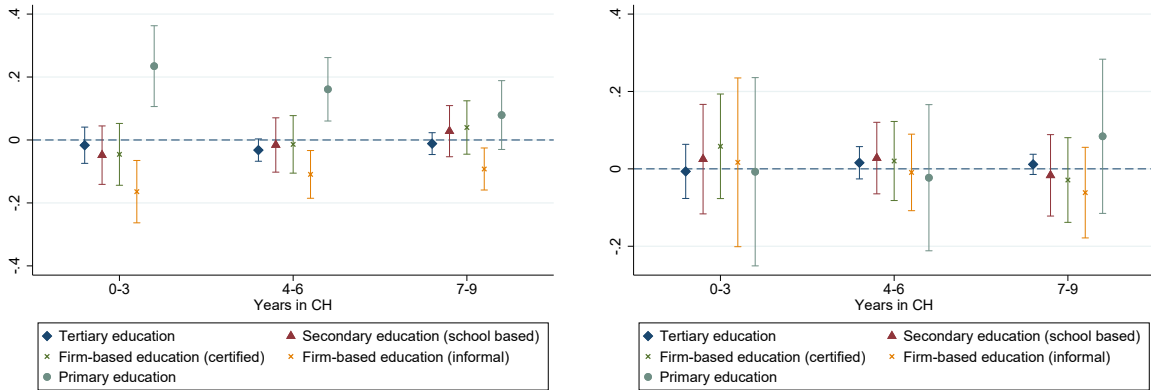
Figure D.4 highlights that lower human capital accumulation could be one potential mechanism explaining how longer employment bans reduce wages. In particular, longer employment bans reduce the probability that refugee workers have acquired firm-based informal secondary education (i.e., on-the-job training) by about 20 p.p. in the years 0–3 after arrival. This effect declines a bit in the years 4–6 but remains significant and economically large. Conversely, individuals are about 20 p.p. more likely to have only primary education. Also, longer employment bans reduce the likelihood of having a tertiary education in the years 3–5 after arrival. The point estimate is small, reflecting that very few refugees have a tertiary degree. It is important to keep in mind that the SESS data used for this analysis only allows observing effects on workers (but not the entire population). Thus, part of the effect that we see in this figure could come from the effects of employment bans on the composition of refugee employment.

³⁴Additional controls are gender, gender-specific age and age squared, refugees' marital status, the unemployment rate in the year of arrival, the initial level of social assistance in the assigned canton, and a dummy equal to one if the canton has self-employment restrictions at arrival. We also control for three indicators for the number of years in Switzerland. Standard errors are clustered at the canton \times status-group level.



Notes: This figure shows the long-run effect of initial labor restrictions on refugees' hourly wages using SESS data. We relate workers' wages as observed in the survey waves 2012–2018 to the policies for asylum seekers (status N) in place in the assigned canton at the time of arrival. The figure shows the interaction terms between the initial policies and indicators for 0–3, 4–6, and 7–9 years after arrival in Switzerland. The sample is refugees that transition from N to B permit or TAR/TAF. Outcomes and policies are measured in October of each year. We control for status group fixed effects (N to B and N to TAR/TAF, respectively), (initial) canton, cohort, and survey wave fixed effects interacted with dummies for the two status groups. Baseline controls are gender, gender-specific age and age squared, refugees' marital status, the unemployment rate in the year of arrival, the initial level of social assistance in the assigned canton, and a dummy equal to 1 if the canton has self-employment restrictions at arrival. We also control for the three indicators of years in Switzerland. Standard errors are clustered at the canton \times status-group level.

Figure D.3: Long-run effects of initial labor market policies on hourly wages



(a) Employment ban

(b) Share restricted jobs

Notes: This figure shows the effect of initial labor restrictions on refugees' highest educational attainment in the long run, using SESS over 2012–2018. The dependent variables of the five separate regressions are indicators whether the refugee has a tertiary degree, a school-based secondary degree, a firm-based certified (secondary) degree (usually an apprenticeship), a firm-based informal further education, or a primary degree. See notes in Figure D.3.

Figure D.4: Effect of labor market policies on educational attainment of workers

E Short-run analysis

E.1 Further employment and earnings results.

Table E.1: Effect of labor market policies on employment with logistic regression

	(1)	(2)	(3)	(4)	(5)	(6)
Employment ban	-0.120*** (0.016)	-0.115*** (0.019)	-0.067*** (0.009)	-0.115*** (0.026)	-0.114*** (0.026)	-0.215*** (0.039)
Priority enforced	-0.056*** (0.012)	-0.055*** (0.013)	-0.029*** (0.011)	-0.058*** (0.016)	-0.059*** (0.017)	-0.060*** (0.018)
Share restricted jobs	-0.052* (0.027)	-0.049 (0.030)	-0.034* (0.020)	-0.070* (0.036)	-0.069* (0.040)	-0.121*** (0.045)
Estimator	OLS	OLS	OLS	Logit	Logit	Logit
Sample	All	All	All	All	All	All
Canton FE	Yes	Yes		Yes	Yes	
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-since-arrival FE	Interacted	Interacted	Interacted	Interacted	Interacted	Interacted
Individual FE			Yes			Yes
Additional controls		Yes			Yes	
Observations	2746661	2746661	2746661	2746496	2746496	1277665

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Notes: Columns (1)-(3) use linear regression, while columns (4)-(6) use logistic regression. Columns (1)-(3) in this table reproduce columns (4)-(6) in Table 1, Panel A. The coefficients reported in the last three columns are average marginal effects. See notes in Table 1 for more information.

Table E.2: Effect of labor market policies on employment (ZEMIS data)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Employment indicator (ZEMIS).</i>							
Employment ban	-0.1076*** (0.0269)	-0.2200*** (0.0317)	-0.0906*** (0.0279)	-0.0976*** (0.0207)	-0.0934*** (0.0237)	-0.0666*** (0.0190)	-0.1320*** (0.0262)
Priority enforced	-0.0583*** (0.0094)	-0.0606** (0.0294)	-0.0734*** (0.0241)	-0.0587*** (0.0114)	-0.0587*** (0.0131)	-0.0335*** (0.0115)	-0.0641** (0.0235)
Share restricted jobs	-0.0526 (0.0345)	-0.0329 (0.0305)	0.0313 (0.0447)	-0.0181 (0.0286)	-0.0118 (0.0309)	-0.0124 (0.0209)	-0.0941 (0.0577)
Outcome mean	0.1347	0.1308	0.1043	0.1257	0.1257	0.1257	0.1619
Num. individuals	43,016	6,792	21,160	70,968	70,968	70,968	38,154
Observations	1,965,842	300,258	904,325	3,170,425	3,170,425	3,170,425	1,391,051
Sample	N->TAF	N->TAR	N->B	All	All	All	TAF
Months-since-arrival FE	Yes	Yes	Yes	Interacted	Interacted	Interacted	Yes
Additional controls	Yes	Yes	Yes	Yes	No	No	Yes
Canton FE	Yes	Yes	Yes	Yes	Yes		Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE						Yes	

Notes: See notes in Table 1. Sign. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table E.3: Effect of labor market policies on employment only using policies coded with high reliability.

	(1)	(2)	(3)	(4)
<i>Employment indicator (AHV).</i>				
Employment ban	-0.1216*** (0.0152)	-0.0737*** (0.0074)	-0.1153*** (0.0149)	-0.0671*** (0.0086)
Priority enforced	-0.0557*** (0.0124)	-0.0256** (0.0104)	-0.0458*** (0.0131)	-0.0225** (0.0105)
Share sector restricted jobs	-0.0351 (0.0262)	-0.0195 (0.0183)	-0.0506 (0.0350)	-0.0402* (0.0205)
Share region restricted jobs	-0.1331** (0.0596)	-0.1951*** (0.0409)	-0.1277** (0.0492)	-0.2526*** (0.0417)
Employment ban × Low reliability			-0.0085 (0.0114)	-0.0154* (0.0079)
Priority enforced × Low reliability			-0.0165 (0.0122)	-0.0002 (0.0096)
Share sector restricted jobs × Low reliab.			0.0875* (0.0470)	0.0583 (0.0473)
Share region restricted jobs × Low reliability			0.1746 (0.1186)	0.3140*** (0.0711)
Sample	All	All	All	All
Canton FE	Yes		Yes	
Month FE	Yes	Yes	Yes	Yes
Months-since-arrival FE	Interacted	Interacted	Interacted	Interacted
Individual FE		Yes		Yes
Additional controls	Yes	Yes	Yes	Yes
Observations	2,772,775	2,772,775	2,772,775	2,772,775

Notes: The policy coders classified observations as highly reliable if the information was confirmed by a law text, public internet resources (typically, cantonal website) or two contacts (by email or telephone). 50% of the observations were classified as highly reliable. 25% of the data points were assessed to be of low reliability. This is usually the case when information provided through a contact (via email or telephone) was unspecific and lacked detail. The remaining observations were assessed to be of normal reliability. Column 1 and 2 in the table above correspond to columns 4 and 6 in Table E.4 which shows the results of Table 1 in the main text but separates sector and region restrictions. Column 3 and 4 interact the policy variables with an indicator for low reliability. See Table 1 for more information.

Sign. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table E.4: Effect of labor market policies on employment and total earnings with separate effects for job restrictions by region and sector restrictions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A. Employment</i>							
Employment ban	-0.1032*** (0.0246)	-0.2382*** (0.0362)	-0.1592*** (0.0224)	-0.1216*** (0.0152)	-0.1161*** (0.0190)	-0.0737*** (0.0074)	-0.0764*** (0.0261)
Priority enforced	-0.0554*** (0.0146)	-0.0510* (0.0284)	-0.0551*** (0.0183)	-0.0557*** (0.0124)	-0.0551*** (0.0139)	-0.0256** (0.0104)	-0.0511* (0.0267)
Share sector restricted jobs	-0.0405 (0.0357)	-0.0110 (0.0236)	-0.0181 (0.0267)	-0.0351 (0.0262)	-0.0349 (0.0291)	-0.0195 (0.0183)	-0.0738* (0.0419)
Share region restricted jobs	-0.0517 (0.0658)	-0.2808*** (0.0808)	-0.3053*** (0.0900)	-0.1331** (0.0596)	-0.1007 (0.0633)	-0.1951*** (0.0409)	0.9399** (0.4154)
Outcome mean	0.1893	0.1438	0.1452	0.1732	0.1732	0.1732	0.2292
Num. individuals	41,227	6,494	20,059	67,780	67,780	67,780	34,093
Observations	1,767,187	246,365	759,223	2,772,775	2,772,775	2,772,775	1,265,841
<i>Panel B. Total earnings (Poisson)</i>							
Employment ban	-1.139*** (0.1787)	-2.568* (1.372)	-1.583*** (0.3237)	-1.251*** (0.1131)	-1.250*** (0.1286)	-1.664*** (0.1556)	-0.7065*** (0.1622)
Priority enforced	-0.3806*** (0.0704)	-0.6472*** (0.1715)	-0.9117*** (0.1796)	-0.4370*** (0.0666)	-0.4573*** (0.0656)	-0.3396*** (0.0644)	-0.2116* (0.1083)
Share sector restricted jobs	-0.4704** (0.1946)	0.5149 (0.4866)	0.0370 (0.3595)	-0.3409* (0.1797)	-0.3830** (0.1952)	-0.3283** (0.1453)	-0.2496 (0.2047)
Share region restricted jobs	-1.228* (0.6942)	-1.322 (1.446)	-1.787 (1.150)	-1.486** (0.5937)	-1.441** (0.5837)	-1.878*** (0.5634)	3.580** (1.824)
Outcome mean (CHF)	505.6	365.8	328.0	444.3	444.3	950.9	621.3
Num. individuals	41,227	6,494	20,059	67,780	67,780	23,225	34,093
Observations	1,765,982	246,047	759,222	2,772,610	2,772,610	1,295,608	1,265,791
<i>Panel C. Monthly earnings (log)</i>							
Priority enforced	-0.0695* (0.0395)	-0.4395** (0.2064)	-0.4082 (0.2406)	-0.1652*** (0.0456)	-0.1608*** (0.0500)	-0.1429** (0.0609)	-0.0149 (0.0283)
Share sector restricted jobs	-0.2626* (0.1382)	0.1823 (0.4043)	-0.1829 (0.3828)	-0.1706 (0.1368)	-0.1725 (0.1385)	-0.1798 (0.1606)	-0.0721 (0.0824)
Share region restricted jobs	-0.6928* (0.3741)	1.153 (1.274)	0.4611 (1.330)	-0.5412 (0.4551)	-0.5499 (0.4717)	0.4582 (0.4030)	-1.537 (1.026)
Outcome mean	2,669.1	2,540.9	2,259.2	2,565.5	2,565.5	2,565.5	2,711.4
Num. individuals	14,711	2,060	6,454	23,225	23,225	23,225	14,148
Observations	334,539	35,426	110,230	480,195	480,195	480,195	290,049
Sample	N->TAF	N->TAR	N->B	All	All	All	TAF
Canton FE	Yes	Yes	Yes	Yes	Yes		Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE						Yes	
Months-since-arrival FE	Yes	Yes	Yes	Interacted	Interacted	Interacted	Yes
Additional controls	Yes	Yes	Yes	Yes	No	No	Yes

Notes: See notes in Table 1 in the main text. The results in this table split the restricted share in jobs restricted by sector and by regional restrictions.

Sign. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

E.2 Robustness

Table E.5: Effect of labor market policies on employment and total earnings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A. Employment</i>							
Priority enforced	-0.0552*** (0.0141)	-0.0480* (0.0258)	-0.0579** (0.0217)	-0.0533*** (0.0122)	-0.0516*** (0.0136)	-0.0170** (0.0084)	-0.0677** (0.0265)
Share restricted jobs	-0.0905* (0.0454)	-0.0891*** (0.0308)	-0.0598* (0.0305)	-0.0882** (0.0344)	-0.0867** (0.0403)	-0.0612*** (0.0207)	-0.0930 (0.0701)
Observations	1,600,565	226,377	694,305	2,521,247	2,521,247	2,521,247	1,221,291
<i>Panel B. Total earnings (Poisson)</i>							
Priority enforced	-0.5592*** (0.1292)	-0.8780*** (0.3173)	-0.9543*** (0.2749)	-0.6801*** (0.1383)	-0.6950*** (0.1405)	-0.4757*** (0.0985)	-0.2909** (0.1299)
Share restricted jobs	-0.6080** (0.2705)	-0.1012 (0.8979)	-0.8838 (0.6045)	-0.5022* (0.2622)	-0.5236* (0.2787)	-0.3958*** (0.1460)	-0.2215 (0.2746)
Observations	1,600,532	226,309	694,290	2,521,213	2,521,213	1,198,760	1,221,258
<i>Panel C. Monthly earnings (log)</i>							
Priority enforced	-0.0706* (0.0382)	-0.4750 (135.0)	-0.3646* (0.2025)	-0.1732*** (0.0445)	-0.1698*** (0.0488)	-0.1105** (0.0481)	0.0011 (0.0274)
Share restricted jobs	-0.3005** (0.1315)	0.1064 (0.3725)	-0.1315 (0.3646)	-0.1598 (0.1245)	-0.1539 (0.1207)	-0.0924 (0.0948)	-0.1104 (0.1107)
Outcome mean (CHF)	2,671.9	2,542.7	2,261.1	2,566.7	2,566.7	2,566.7	2,712.1
Sample	N->TAF	N->TAR	N->B	All	All	All	TAF
Months-since-arrival FE	Yes	Yes	Yes	Interacted	Interacted	Interacted	Yes
Additional controls	Yes	Yes	Yes	Yes	No	No	Yes
Canton FE	Yes	Yes	Yes	Yes	Yes		Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE						Yes	

Notes: We interact policies with month-since-arrival fixed effects and report the aggregated estimates, as described in the main text. See notes in Table 1 for more information.

Sign. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table E.6: Effect of labor market policies on employment and total earnings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A. Employment</i>							
Priority enforced	-0.0609*** (0.0142)	-0.0391* (0.0224)	-0.0520** (0.0210)	-0.0521*** (0.0120)	-0.0513*** (0.0130)	-0.0319*** (0.0103)	-0.0619** (0.0237)
Share restricted jobs	-0.0881* (0.0457)	-0.0700* (0.0363)	-0.0679** (0.0310)	-0.0843*** (0.0314)	-0.0806** (0.0354)	-0.0682*** (0.0220)	5.355** (2.065)
Observations	1,600,565	226,377	694,305	2,521,247	2,521,247	2,521,247	1,221,291
<i>Panel B. Total earnings (Poisson)</i>							
Priority enforced	-0.4556*** (0.1038)	-1.193*** (0.2319)	-0.9954*** (0.2604)	-0.6116*** (0.0969)	-0.6352*** (0.1001)	-0.4976*** (0.0977)	-0.2204** (0.1013)
Share restricted jobs	-0.8172* (0.4540)	0.1599 (0.5548)	-1.153* (0.6560)	-0.8651** (0.3466)	-0.9256** (0.3891)	-0.5596*** (0.1746)	26.20** (11.16)
Observations	1,600,532	226,309	694,290	2,521,213	2,521,213	1,198,760	1,221,258
<i>Panel C. Monthly earnings (log)</i>							
Priority enforced	-0.0499 (0.0326)	-0.3346** (0.1461)	-0.3171* (0.1550)	-0.1485*** (0.0434)	-0.1448*** (0.0459)	-0.1767** (0.0750)	0.0033 (0.0307)
Share restricted jobs	-0.1755 (0.1500)	-0.0834 (0.2437)	-0.2043 (0.3075)	-0.1045 (0.1603)	-0.1011 (0.1549)	-0.0320 (0.1420)	-1.156 (3.337)
Outcome mean (CHF)	2,671.9	2,542.7	2,261.1	2,566.7	2,566.7	2,566.7	2,712.1
Sample	N->TAF	N->TAR	N->B	All	All	All	TAF
Months-since-arrival FE	Yes	Yes	Yes	Interacted	Interacted	Interacted	Yes
Additional controls	Yes	Yes	Yes	Yes	No	No	Yes
Canton FE	Yes	Yes	Yes	Yes	Yes		Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE						Yes	

Notes: We interact policies with calendar year fixed effects and report the aggregated estimates. See notes in Table 1 for more information.

Sign. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

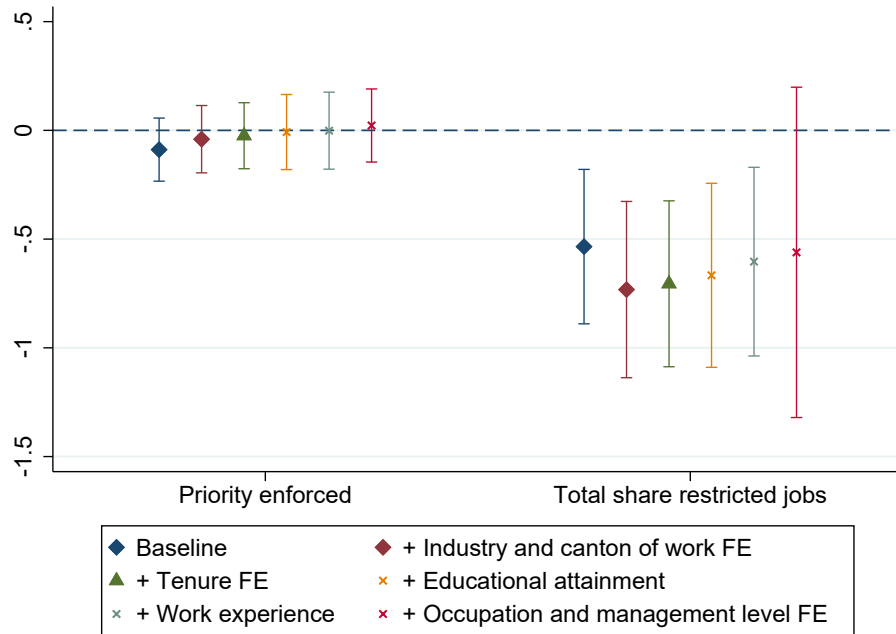
F Further evidence on wage effects

Table F.1: Effect of labor market policies on hourly wages (contemporaneous policies)

VARIABLES	(1) Log hourly wage	(2) Log hourly wage	(3) Log hourly wage	(4) Log hourly wage	(5) Log hourly wage	(6) Log hourly wage first 5 years only
Priority enforced	-0.089 (0.070)	-0.073 (0.050)	-0.041 (0.075)	-0.032 (0.081)	-0.167 (0.135)	-0.053 (0.098)
Share restricted jobs	-0.535*** (0.172)	-0.884*** (0.111)	-0.732*** (0.197)	-0.425 (0.277)	-0.284 (0.523)	-0.569** (0.220)
Observations	4,453	4,465	4,447	4,447	2,172	1,123
R-squared	0.102	0.032	0.166	0.178	0.696	0.161
Sample	N→TAR/F	N→TAR/F	N→TAR/F	N→TAR/F	N→TAR/F	N→TAR/F
Additional controls	Yes	No	Yes	Yes	No	Yes
Canton FE	Yes	Yes	Yes	Yes	No	Yes
Survey wave FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes	No	No
Canton of work FE	No	No	Yes	Yes	No	No

Notes: This table shows the effects of enforcing the priority restriction and of mobility and sector restrictions on hourly wages (in logarithmic terms) of employed refugees based on specification (1) using data from the SESS waves 2012, 2014, and 2016. The sample in column 1 is refugees that transition from asylum seeker (N) to a B permit. The sample in the remaining columns is refugees that transition from N to either a TAR or TAF permit. Outcomes and status-specific policies are measured in October of each year. All specifications control for (initial) canton and survey wave fixed effects. Column 4 additionally controls for years-since-entry and years-since decision fixed effects. Column 6 controls for individual FE. Baseline controls are gender, gender-specific age and age squared, nationality dummies, marital status, the unemployment rate, the level of social assistance in the canton, and dummies equal to 1 if the canton has a blocking period exceeding 3 months and self-employment restrictions. Column 7 is restricted to workers that are within their first five years in Switzerland (a baseline restriction in the social security data). The regressions are weighted using the person (sampling) weights of the survey. Standard errors are clustered at the cantonal level.

Sign. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.



Notes: The figure shows the effect of enforcing the priority requirement and of sector and mobility restrictions on log hourly wages of employed refugees aged 18–65 using data from the SESS waves 2012–2016. The baseline specification (blue coefficient) illustrates the results from the short-run wage regression, presented in column 3 of Table F.1 and focuses on refugees transitioning from N to either a TAR or TAF permit. We then add fixed effects for each year of tenure (red coefficient), for NACE (rev. 2) two-digit industries and canton of work (green), for eight levels of educational attainment (yellow), for the accumulated work experience since arrival in Switzerland (measured as months employed in Switzerland, entered both in levels and squared) (in grey), and fixed effects for ISCO two-digit occupation and five management levels (no, lowest, low, mid- and highest-level management) (in light red). The underlying samples vary somewhat between the regressions due to missing values in certain covariates. Vertical lines show confidence intervals clustered at the cantonal level.

Figure F.1: Wage effects and the composition of jobs (contemporaneous policies)

G Overeducation

Table G.1 analyzes whether enforcing priority for residents and sector and region restrictions force well-educated refugees to work in a job for which they are overqualified. We examine this by looking at the impact of the restrictions on refugees' years of schooling relative to natives in similar jobs. The years of schooling are predicted. They reflect the years of schooling that would typically be needed in Switzerland to complete workers' highest educational degree as collected in the Swiss Structure of Earnings Surveys. The dependent variable in the regression is a particular refugee's years of schooling *relative* to the average years of schooling for Swiss citizens in the same ISCO two-digit occupation and survey year. As we can see from the mean of this outcome variable, shown at the bottom of Table G.1, refugees' lack on average more than a year of schooling compared to Swiss citizens in the same occupation.

The table provides no evidence that sector and mobility reduce this educational gap. This result holds although we estimate a specification that holds many observed worker and job characteristics constant. Thus, we only compare refugees educational attainment across similar jobs. Similarly, column 1 provides no evidence that enforcing priority for residents affects refugees' educational attainment relative to citizens. This is not true, however, if we restrict the sample to occupations with somewhat higher educational requirements (occupations where Swiss citizens have at least 12 years of schooling), as we do in column 2. In these occupations, prioritization reduces the gap that we see between refugee and native education in cantons that do not enforce priority to residents.

Table G.1: Effect of labor market policies on overqualification

VARIABLES	(1) Years of schooling	(2) Years of schooling higher requirements	(3) Years of schooling low requirements
Priority enforced	0.084 (0.196)	1.583* (0.811)	-0.048 (0.169)
Share restricted jobs	-0.303 (0.521)	-1.088 (2.333)	-0.439 (0.398)
Observations	5,102	986	4,081
R-squared	0.297	0.484	0.173
Mean of outcome	-1.792	-2.745	-1.561
Observations per spell	First	First	First
Additional controls	Yes	Yes	Yes
Survey wave FE	Interacted	Interacted	Interacted
First year of tenure FE	Yes	Yes	Yes
Canton FE	Yes	Yes	Yes
Industry and canton of work FE	Yes	Yes	Yes
Occupation and management level FE	Yes	Yes	Yes

Notes: This table shows the effect of labor restrictions on refugees' years of schooling expressed relative to the average years of schooling of Swiss citizens. The dependent variable in the regression is a particular refugee's years of schooling *relative* to the average years of schooling for Swiss citizens in the same ISCO two-digit occupation and survey year. We relate this gap to the status-specific policies that were in place when the refugees started to work at their current employer. We only keep the observation from the earliest survey if the same refugee is observed in the same firm in several surveys. The sample is employed refugees aged 18–65 that transition from asylum seeker (N) to either a B a TAR or TAF permit. Column 2 (3) restricts the sample to occupation-year cells where Swiss citizens have at least (less than) 12 years of predicted schooling. In all columns, we focus on refugees that started to work in 2005 or later for their current employer. We aggregate the policies for TAR and TAF refugees by giving TAR policies a weight of 14.1%. All specifications control for (initial) canton and survey wave fixed effects, fixed effects for the year in which the worker joined the firm, two-digit industry fixed effects, fixed effects for the canton of work, ISCO two-digit occupation, and hierarchy level fixed effects. Baseline controls are gender, gender-specific age and age squared, nationality dummies, marital status, the unemployment rate at the start of the spell, the level of social assistance in the canton (in CHF), and dummies equal to 1 if the canton has a blocking period exceeding 3 months and self-employment restrictions at the start of the spell.

Sign. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

H Firm analysis

We test whether an increased skill mismatch can explain the negative wage effects of sector and region restrictions. This would require that those firms, which are still legally allowed to, employ more refugees when policies become more restrictive. If employment increases, it may reflect firms' move along a downward-sloping marginal productivity curve. To test whether these employ more refugees if restrictions increase, we regress the growth in refugee employment in firms that have non-zero employment of a particular group of refugees in two subsequent periods on the policies in the initial period. More specifically, the outcome variable in columns 1–3 in Table H.1 is one if a firm employs more refugees in calendar month t than in calendar month $t - 12$ ($t - 24$ in columns 4–6). It is zero if the firm still employs refugees in the second period but not more than in the first period. We opt for a specification with a dummy variable because of the very low number of refugee workers in most firms. Column 1 and 2 (column 4 and 5) focus on firms' employment of the two main permit categories—N and TAF—which, in contrast to refugees with a B permit, face the restrictions we want to analyze. Column 3 (column 6) pools employment of N, TAR, and TAF refugees. In this case, the policies are measured as a weighted average of the N, TAR, and TAF refugees, where the weights reflect the relative frequencies in the sample. We restrict this analysis to firms that employ a particular refugee group in two subsequent periods to ensure that we focus on firms that can hire them both before and after a change in restrictions.

Overall, the table suggests that enforcing priority for residents and a larger share of sector- and region-restricted jobs do not lead to an increase in employment of refugees in firms that continue to employ them. If anything, we rather find evidence for a reduced probability of increasing employment of refugees, consistent with the worker-level results presented in Table 1. Overall, if the share of restricted jobs is higher, firms that are legally allowed to employ refugees do not seem to hire additional, possibly less productive workers. These results speak against the hypothesis that an increase in skill mismatch explains the negative wage effects of sector and mobility restrictions.

Table H.1: Effect of labor market policies on employment by firm

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Dependent variable:</i>	<i>Increase in employment</i>					
Priority	-0.0232** (0.0094)	-0.0159 (0.0164)	-0.0502 (0.0309)	-0.0345** (0.0143)	-0.0238 (0.0219)	-0.0664** (0.0321)
Share restricted job	-0.1052** (0.0434)	0.0068 (0.0440)	-0.0973 (0.0922)	-0.0973* (0.0570)	0.0185 (0.0568)	-0.1303 (0.1096)
Outcome mean	0.1189	0.1406	0.1661	0.1437	0.1889	0.2302
Reference period	$t - 12$	$t - 12$	$t - 12$	$t - 24$	$t - 24$	$t - 24$
Sample	N	TAF	All	N	TAF	All
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Canton FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm ID FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	55,211	206,644	247,350	46,608	164,861	195,121

Notes: See text. Standard error clustered at canton level. Sign. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

I Employer concentration

An important side effect of labor market restrictions is that they restrict the number of employers that can hire refugees. Static models of monopsony predict that the extent to which employers set wages below the marginal revenue product of workers is inversely related to the number of employers that compete for a group of workers (Manning, 2003; Card et al., 2018). Therefore, sector and region restrictions may decrease refugees’ wages by increasing employer concentration.

Therefore, we now use the social security earnings data to study how the policies affect the number of distinct firms that employ the refugees allocated to a particular canton. The analysis is based on the firm identifier in the data. Employer concentration is measured annually among the employed refugees of each canton, separately by permit category (N, B, TAF, TAR). We focus on refugees that are within their first five years in the country.

We build four different measures of employer concentration from the resulting employer counts, all of which increase employer concentration. The first is the familiar Herfindahl-Hirschman index (HHI). The HHI ranges from $H = 1$ (perfect monopsony) to $H = 1/n$ in the case of many firms of equal employment. A conceptual problem of the HHI in our setting is that more restrictive policies lower the number of refugees with a job (see section 4). If a certain policy reduces the participation of refugees, it is partly natural that they are employed by fewer employers. This “mechanical” reduction in the number of employers provides little insight into the underlying number of employers willing to hire refugees. Therefore, we also present three alternative measures of employer concentration that are invariant to the number of employers and instead measure the *dispersion* of refugees across firms: the Gini index, the Theil index, and the logarithm of the ratio between the number of employed refugees and the number of distinct employers (“log ratio”). This ratio is minimized if every refugee in a given permit category works for a separate employer.

Table I.1 uses annual data at the level of canton times permit category (N, TAR, TAF, B) to regress the four measures of employer concentration on the labor market restrictions. We control for canton, month, and permit group fixed as well as the cantonal unemployment rate in each canton, the average duration of stay in Switzerland (linear and squared), and the share of refugees banned from employment among the refugees in a canton and year. The regressions are weighted with the number of refugees in a given canton-category cell.

Table I.1 provides some evidence that the labor market policies affect employer concentration. The signs of the effects depend on the policy. As expected, restricting the labor market of refugees geographically and sectorally increases employer concentration, but the coefficient is not statistically significant if we use the total share of restricted jobs, which combines both the sector and region restrictions (columns 1–4). If we estimate separate effects for the shares of region- and sector-restricted jobs (columns 5–8), the effect of the region restrictions becomes statistically significantly positive at the 10% level for three of four concentration measures. In contrast, enforcing priority for resident workers lowers employer concentration measured with the Gini index, the log ratio, and the Theil index. These results may reflect the process that the cantons implement to enforce prioritisation. This process is supposed to ensure that the refugees eventually hired are hard to find elsewhere. Enforcing priority may thus make it harder for a single firm to hire many refugees.

Overall, if employers exercise the wage-setting power arising from employer concentration, Table I.1 provides a possible explanation for why mobility restrictions reduce refugees’ hourly

wages. The results also provide a possible explanation for why prioritizing residents does not depress refugees' wages although it clearly limits their employment opportunities.

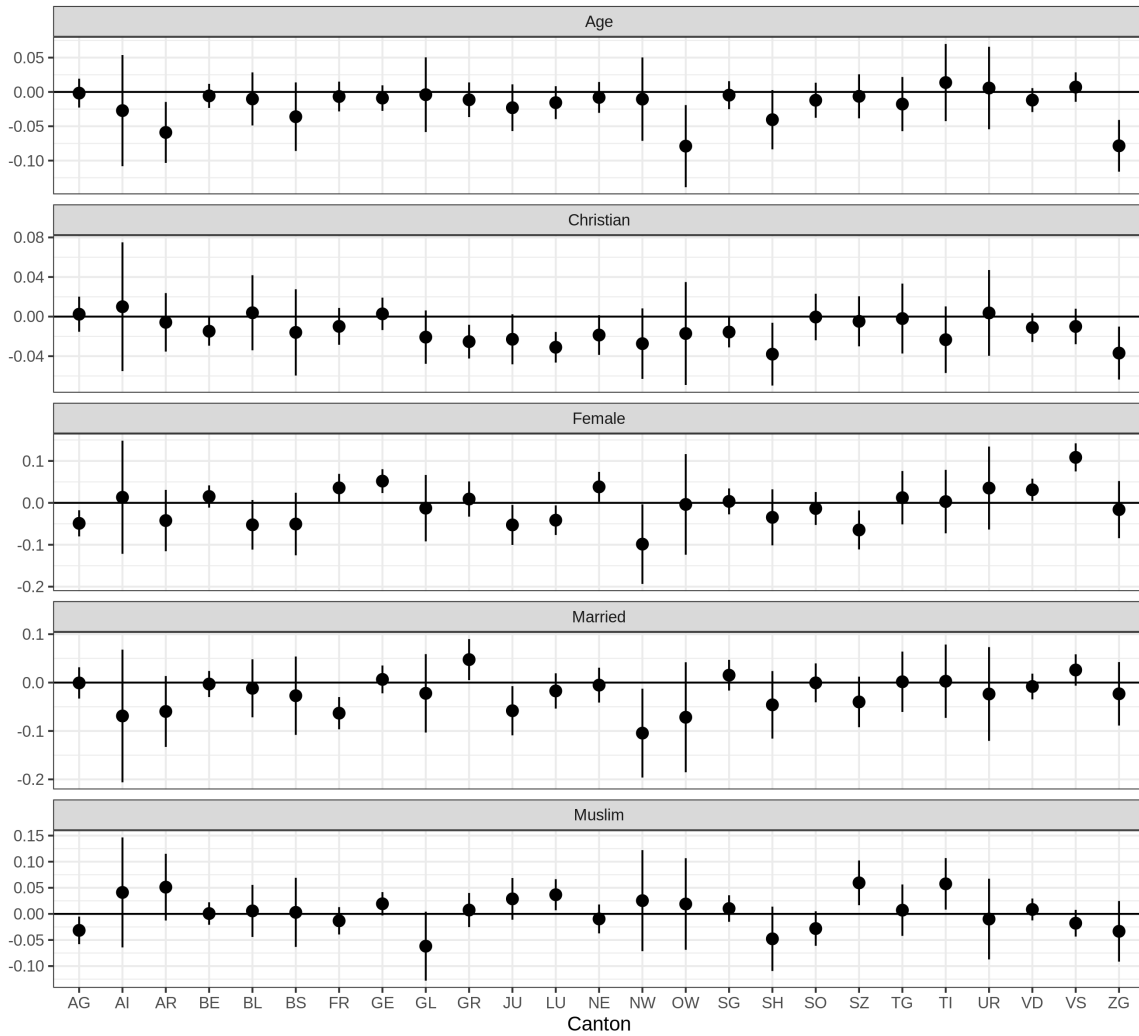
Table I.1: Effect of labor market policies on employer concentration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share banned	-0.263 (0.204)	-0.051 (0.054)	-0.112 (0.104)	-0.130 (0.126)	-0.201 (0.160)	-0.056 (0.046)	-0.108 (0.086)	-0.133 (0.106)
Priority enforced	0.045 (0.043)	-0.070*** (0.024)	-0.127*** (0.042)	-0.074* (0.043)	0.034 (0.046)	-0.074*** (0.025)	-0.135*** (0.045)	-0.079* (0.047)
Share total restricted jobs	0.092 (0.135)	0.051 (0.036)	0.104 (0.066)	0.067 (0.075)				
Share commuter-restricted jobs					0.998* (0.568)	0.316 (0.192)	0.683* (0.348)	0.473 (0.382)
Share sector-restricted jobs					0.070 (0.114)	0.043 (0.033)	0.084 (0.063)	0.059 (0.062)
Measure	HHI	Gini	Log(Ratio)	Theil	HHI	Gini	Log(Ratio)	Theil
Num. obs.	1474	1474	1474	1474	1495	1495	1495	1495
N Clusters	104	104	104	104	104	104	104	104

Sign. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Notes: This table shows the effect of the labor market restrictions on employer concentration. We use annual data at the level of canton times permit category (N, TAR, TAF, B). Employer concentration is also measured at the canton times category level by counting employers among all employed refugees in each cell. We compute four measures of employer concentration (as indicated in the table header): The Herfindahl-Hirschman index (HHI), the Gini index, the Theil index, and the logarithm of the ratio between the number of employed refugees and the number of distinct employers (“Log(ratio)”). We control for the cantonal unemployment rate, the average duration of stay in Switzerland (linear and squared), and the share of refugees banned from employment among the refugees in a canton and year. All models include canton and month fixed effects which were interacted with permit group. The regressions are weighted with the number of employed refugees in a given canton-category cell.

J Exogenous allocation check



Notes: Point estimates and 95% confidence intervals of OLS regressions of asylum seeker characteristics on indicators for assigned cantons. Since allocation is only exogenous conditional on asylum seeker characteristics observable in the ZEMIS database, these models include cohort interacted with processing center as well as nationality fixed effects.

Figure J.1: Balance tests of various refugee characteristics measured at arrival across assigned cantons

K Exogenous policy check

Table K.1: Exogenous policy check

	<i>Employment ban months</i>		<i>Restricted share</i>		<i>Priority</i>	
	<i>Reduction</i>	<i>Increase</i>	<i>Reduction</i>	<i>Increase</i>	<i>Turn off</i>	<i>Turn on</i>
	(1)	(2)	(3)	(4)	(5)	(6)
Overall unemployment last year	-0.0234 (0.1183)	-0.0075 (0.0201)	0.0798 (0.0702)	-0.0861 (0.0896)	-0.0621 (0.1858)	-0.0068 (0.0121)
Overall unemployment previous year	-0.1414 (0.1664)	0.1635 (0.1645)	-0.1064 (0.1045)	0.0077 (0.0937)	0.3777 (0.3393)	-0.0209 (0.0260)
Overall unemployment three years ago	0.0882 (0.1325)	-0.1060 (0.1072)	0.0056 (0.1047)	0.1086 (0.0924)	0.0924 (0.3058)	0.0852 (0.0824)
Refugee unemployment last year	0.0005 (0.0019)	0.0008 (0.0007)	-0.0128 (0.0077)	0.0086* (0.0046)	-0.0191 (0.0317)	-0.0004 (0.0004)
Refugee unemployment previous year	0.0068* (0.0037)	-0.0016 (0.0014)	-0.0167* (0.0085)	0.0014 (0.0035)	0.0195 (0.0236)	-0.0004 (0.0005)
Refugee unemployment three years ago	-0.0023 (0.0029)	0.0004 (0.0004)	0.0194** (0.0083)	-0.0103*** (0.0033)	-0.0116 (0.0144)	-0.0001 (0.0002)
Status FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Canton FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,636	16,636	16,543	16,543	4,560	12,164
Joint F -test (p -value)	0.468	0.966	0.003	0.006	0.36	0.967

Clustered (Canton FE) standard-errors in parentheses
 Sign. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Notes: The table shows results from a regression of policy changes against overall and refugee unemployment rates. Unemployment rates in the ‘last year’ refer to the rolling average over months $t - 1$ to $t - 12$. Unemployment rates in the ‘previous year’ refer to the rolling average over months $t - 13$ to $t - 24$. The dependent variable in columns (1) and (2) is set to 1 if the number of months of employment ban in a given canton decreases or increases, respectively, and 0 if there is no change relative to the previous time period for a given status. Similarly, the outcome in columns (3) and (4) code the reduction or increase in the restricted share. Finally, columns (5) and (6) use a binary indicator equal to 1 if the priority policy switches off and on, respectively, 0 for no change. All regression models include status, canton and month fixed effects. Standard errors are clustered at the canton level. The last row reports the p -value from an F -test of joint significance.

L Costs for refugees and host society

Building on the short-run impact analysis of individual labor restrictions in Section 4, we can provide back-of-the-envelope calculations of the total effect of the policies on refugees' aggregate employment and earnings.

We consider three scenarios: (i) a liberal scenario without any labor restrictions, (ii) the “status quo” scenario with the actual labor restrictions as observed during our study period, and (iii) a restrictive scenario with the most restrictive policy mix observed during our study period applied to all cantons and years. The latter corresponds to the set of policies implemented by the canton Solothurn between 1999 and 2004. In this period, Solothurn enforced the priority requirement, had a 14-month employment ban, and restricted 18% of potentially available jobs for refugees.³⁵ This scenario is intended to provide a plausible upper bound on the aggregate effect that labor restrictions could have if all cantons had followed this highly restrictive set of policies. We calculate predicted number of months in employment and average earnings during the first five years in Switzerland after arrival under these three scenarios based on the baseline specifications with canton, month, and month-since-arrival fixed effects (Table 1, col. 4). For this calculation, we only consider refugees receiving an F or B permit and the first five years after arrival.

Table L.1: Predicted employment, welfare costs, and earnings under three policy scenarios

	<i>Per person-month</i>	<i>Per person-month (TAF/N)</i>	<i>Per person</i>	<i>Total (M)</i>
<i>Panel A. Total earnings (CHF)</i>				
Status quo	442.89	435.12	26573.38	1951.68
No restrictions	513.91	526.40	30834.44	2264.64
Most restrictive	323.27	282.23	19396.00	1424.54
Difference: no restrictions vs status quo	-71.02	-91.28	-4261.06	-312.95
Difference: no restrictions vs most restrictive	-190.64	-244.17	-11438.44	-840.10
<i>Panel B. Social costs (CHF)</i>				
Status quo	440.46	279.96	26427.39	1940.96
No restrictions	397.96	229.41	23877.47	1753.68
Most restrictive	734.92	647.96	44095.28	3238.58
Difference: no restrictions vs status quo	42.50	50.55	2549.92	187.28
Difference: no restrictions vs most restrictive	336.96	418.55	20217.81	1484.90
<i>Panel C. Employment</i>				
Status quo	17.28	16.45	10.37	761.40
No restrictions	19.43	19.22	11.66	856.07
Most restrictive	13.36	11.45	8.01	588.55
Difference: no restrictions vs status quo	-2.15	-2.77	-1.29	-94.67
Difference: no restrictions vs most restrictive	-6.07	-7.77	-3.64	-267.52

Notes: The column ‘Per person-month’ shows the average outcome per person month. ‘Per person’ shows the average outcome aggregated over a 60-months-period, i.e., the length of our main sample. The column labelled ‘Total’ reports to the total over the whole sample population.

Table L.1 shows the predicted employment months, social aid expenditures and total earnings under (i) the liberal, (ii) the status quo, and (iii) the restrictive scenario.

³⁵In addition, self-employment was prohibited for N and TAF status refugees.

Comparing the status quo to the most liberal scenario without any restrictions, we find that the labor market restrictions reduced total earnings by CHF 313 million over the years 1999-2015 at the aggregate level. On a per-person basis, this amounts to CHF 4'261 in lost labor earnings per refugee during the first five years in Switzerland. In terms of employment, refugees lost on average 1.3 employment months due to labor restrictions over the 60 months period. The reduction in labor activity also implies an increase in welfare transfers (for social aid) of at least CHF 2'550 in direct cash payments per person (not including non-cash benefits) or 291 million in total.

A comparison of the most restrictive with the most liberal scenario highlights the substantial impact that labor market access restrictions can have on economic activity among refugees. The gap in total earnings between the two scenarios amounts to CHF 840 million in total foregone earnings (CHF 11'438 per capita) and 3.6 employment months, which implies an increase social aid cash transfers by CHF 1.48 billion.

These back-of-the-envelope calculations come with limitations: First, the social aid costs only include a small amount of total fiscal costs. They exclude non-cash payments such as housing costs, health insurance, and integration support. Similarly, unemployment benefits for refugees who have already accumulated more than twelve employment months since arrival are not considered for this analysis. Furthermore, social aid transfer for the many asylum seekers whose asylum claim is rejected and do not receive an F or B status are also not included. Second, we do not consider the refugees' tax contributions when employed. Third, we do not consider potential effects on either emigration of refugees or crowding out effects on non-refugee workers with similar skills who might compete for the same jobs as refugees. As we show in section 6.3 and 6.3, however, there is no evidence that the restriction policies affect either emigration or other immigrant workers with similar skills.

With the exception of potential crowding-out effects, all the limitations discussed above suggest that our estimates from our analysis sample are a lower bound of the actual costs of labor restrictions for the entire population of asylum seekers and refugees in Switzerland. In sum, the back-of-the-envelope calculation reveals that labor restrictions not only hurt refugees' earnings but also come with considerable costs for welfare transfers that have to be shouldered by the host country's taxpayers.