

The Labor Market Effects of Restricting Refugees' Employment Opportunities

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Abstract

This paper investigates whether employment restrictions contribute to refugees having poorer labor market outcomes than citizens. Utilizing linked register data from Switzerland and within-canton policy variation between 1999–2015, we find substantial negative effects on employment and earnings when refugees are barred from working upon arrival, excluded from specific sectors or regions, or face resident prioritization. Removing 10% of refugees' outside options reduces job-to-job mobility by 7.5% and wages by 3.0%, widening the wage gap to citizens in similar jobs. The restrictions depress refugees' labor market outcomes even after they apply, but do not spur emigration nor benefit other immigrants.

Keywords: Labor market integration, migration, labor market policies, labor market institutions, monopsony, refugees, employment, wages, outside options, employment opportunities
JEL: J08, J31, J42, J61, J68

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1 Introduction

Immigrants, particularly refugees¹, typically have lower employment rates and wages than otherwise similar native citizens (Brell, Dustmann, and Preston, 2020). 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 run the gamut from restrictions on work permits and visa rules (Kerr, Kerr, and Lincoln, 2015; Naidu, Nyarko, and Wang, 2016; Wang, 2021), incomplete recognition of educational certificates (Brücker et al., 2021), occupational licensing (Cassidy and Dacass, 2021), random regional assignment of refugees (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 policies restricting immigrants’ labor market access reduce employment and earnings while they apply but also result in 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. The empirical analyses leverage the liberalization of restrictions determining the labor market prospects of refugees in Switzerland. We trace the short- and longer-term effects of four of the most widespread policies affecting refugees: (i) prioritization, which grants residents priority over refugees on the labor market; (ii) sector restrictions, which regulate that refugees can only work in specific economic sectors; (iii) geographical restrictions, which prevent refugees from working in neighboring regions; and (iv) temporary employment bans for the initial period after arrival.

Identifying the impact of labor market restrictions is challenging since it requires a causal research design building on exogenous policy variation while accounting for the sorting of immigrants across locations. Two institutional features conspire to facilitate the identification of the policy effects in our setting. First, cantons have considerable discretion in setting policies restricting refugees’ labor market access, which generates substantial policy variation across and within cantons. Exploiting within-cantonal variation in policies allows us to control for time-invariant local labor market conditions co-varying with restrictions. Second, refugees are, conditional on mostly observable information, exogenously assigned to cantons shortly after arrival. Moreover, most refugees are, by law, forced to stay in the canton to which they were initially assigned for at least five years. The combination of dispersal policy and settlement restriction increases the credibility of our identification strategy by ruling out confounding through endogenous sorting of refugees to cantons with favorable policies.

Our empirical analyses combine original data on cantonal labor market policies covering the period 1999–2016 with detailed administrative data linking asylum profiles, census, and social security earnings data. We examine the labor market outcomes of refugees using individual-level

¹We use the term ‘refugee’ to refer to internationally displaced persons who have obtained refugee status according to the Geneva Convention or some form of subsidiary protection that allows them to stay in the host country.

panel data and event study regression models, linking these outcomes to the policies of the cantons where 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 19.2% to 11.5% in the average employment rate of affected refugees. This reduction is more than four times larger than the increase in the unemployment rate during the Great Recession and at least as large (but with an opposite sign) as the employment effects of extensive language programs offered to refugees in Denmark (see Foged, Hasager, and Peri, 2022; Arendt, Dustmann, and Ku, 2022).

We then analyze whether restricting employment opportunities reduces refugees' wages and relative wages compared to native citizens. In the social security data, we find evidence that prioritizing residents over refugees lowers monthly earnings by 7.7%. In addition, refugees' monthly wages are 3.0% lower in cantons where sectoral and regional restrictions remove 10% of their potential jobs. The latter estimate is corroborated by a regression focusing on the wages of refugees relative to native citizens in the Swiss Earnings Structure Surveys (SESS) 2012–2016, which provide employer-reported information on workers' hourly wages and monthly hours. The effect of region and sector restrictions on relative wages remains almost unchanged if we augment the wage regressions with a rich set of worker, firm, and job characteristics. This implies that these restrictions cause wage gaps between observationally similar refugees and between refugees and similar native citizens in similar jobs.

The rich contextual data on workers and their jobs allows us to illuminate why the restrictions decrease refugee wages and to examine 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. The unchanged estimates when we control for firm and job characteristics suggest that the wage effect is not due to sectoral and regional restrictions forcing refugees to work in low-paid 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. The results also counter the notion 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 restrictions decrease refugees' hourly pay because they constrain their outside options. This finding aligns with several influential labor market models such as monopsonistic and search and bargaining models (see Caldwell and Harmon, 2019, for a discussion), which suggest that workers who can work in many jobs benefit from higher wages due to increased employment opportunities and job mobility. In a first step, we corroborate that restrictive policies 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 remove up to two-thirds of refugees' potential jobs. Empirically, sector and region restrictions and prioritization substantially reduce refugees' employment rates and job-to-job mobility. We then illuminate the mechanism through which outside options

affect wages. According to monopsonistic models of the labor market, restrictions increase firms' power to post low wages because workers with fewer potential employers respond less to changes in wages (e.g., Manning, 2003; Card et al., 2018). To test this mechanism, we estimate refugees' wage elasticity of quits, drawing on a recent empirical literature (see Sokolova and Sorensen, 2021, for an overview). We find that quits are less responsive to wages in cantons where policies are restrictive, consistent with greater wage-setting power for firms. In addition, enforcing priority reduces on-the-job wage growth, consistent with models in which fewer outside options hamper workers' bargaining position in wage (re-)negotiations (e.g., Cahuc, Postel-Vinay, and Robin, 2006).

Finally, we take initial steps toward assessing the costs and benefits of labor market restrictions. Three pieces of evidence suggest that restrictions burden 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 in the medium term, thus helping to explain why refugees earn less than comparable resident workers. In line with the literature showing that adverse initial conditions can leave long-term scars (Marbach, Hainmueller, and Hangartner, 2018; Fasani, Frattini, and Minale, 2021; Von Wachter, 2020), the priority and blocking policies reduce refugees' labor market earnings for up to four years after they cease applying. Second, our estimates suggest that labor restrictions have limited effects on the propensity to emigrate. This finding extends to refugees who obtain only subsidiary protection and a temporary residence permit subject to frequent renewal. Third, we find no evidence that restrictive policies measurably improve the earnings and employment of unrestricted refugees and low-paid EU-15 immigrants.

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 (see Brell, Dustmann, and Preston, 2020; Dustmann, Landerso, and Andersen, forthcoming; 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; Martén, Hainmueller, and Hangartner, 2019; Müller, Pannatier, and Viarengo, 2023), the recognition of educational certificates (Brücker et al., 2021; Anger, Bassettoy, and Sandner, 2022), the generosity of social assistance (LoPalo, 2019; Dustmann, Landerso, and Andersen, forthcoming), and temporary employment bans (Marbach, Hainmueller, and Hangartner, 2018; Fasani, Frattini, 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 prevalence across Europe and beyond. One exception is Slotwinski, Stutzer, and Uhlig (2019), who study the relationship between the employment of asylum seekers in Switzerland and a composite index incorporating three of the four policies studied here. Using cross-sectional variation in labor market policies of 24 cantons, they find a strong negative association between the index and asylum seekers' employment rate. Our study extends this work by studying each policy's effect using high-quality individual-level administrative data and newly collected panel data of cantonal labor market policies that permit a quasi-experimental research design based on within-cantonal policy variation. In addition, our

linked register data allow us to explore the policy effects on a range of outcomes unexplored in Slotwinski, Stutzer, and Uhlig (2019) and most other papers on host country policies, including hourly wages, separations and job mobility, 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 show that fewer outside options reduce workers' earnings, wages, and job mobility 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, forthcoming), wage changes in secondary jobs for dual jobholders (Lachowska et al., 2022), and changes in the enforceability of non-compete agreements (Johnson, Lavetti, and Lipsitz, 2020). We advance this literature by exploiting a close-to-ideal natural experiment. Cantons that impose geographic or sector restrictions on refugees' labor markets or enforce priority cause large, observable shifts in workers' outside options that are plausibly unrelated to workers' productivity in their current jobs in the short run. Most of our results provide evidence that the restrictions' effects on outside options matter for wages and job mobility and contribute to explaining why refugees have lower wages than observationally equivalent resident workers.²

Third, we add to the rich and expanding literature on the relevance of monopsonistic competition in modern labor markets. Within this literature, our paper is most closely related to studies that analyze immigrant labor markets (see Manning, 2021, for a discussion). A seminal study is Naidu, Nyarko, and Wang (2016), which analyzes 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.³ 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-

²These findings are consistent with the long-standing hypothesis that outside options explain wage gaps between otherwise exchangeable workers. See, for instance, Black, 1995 and Manning, 2021.

³Depew, 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.

and long-run economic outcomes are shaped by labor market conditions at arrival (Aslund and Rooth, 2007; Azlor, Damm, and Schultz-Nielsen, 2020; Müller, Pannatier, and Viarengo, 2023). Most closely to our study, Marbach, Hainmueller, and Hangartner (2018) and Fasani, Frattini, and Minale (2021) document that temporal employment bans impair refugees’ economic integration for years after they cease 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.

2 Labor market access for refugees

2.1 Labor market restrictions for refugees in Europe

Most 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 prevent employment for asylum seekers and refugees for the first 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 recognized refugees) *vis-à-vis* aforementioned groups. Such prioritization policies are used by several countries, including Austria, Germany, and Switzerland (ECRE, 2020). While the implementation and enforcement of 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. 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 can work in. Such 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 (as is the case in Switzerland) by preventing asylum seekers from moving between localities or working outside of the assigned labor market region. They can 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 geographic restrictions exist in the U.S., too, both for refugee and non-refugee migrants.⁴

2.2 Switzerland—a Laboratory to Study Labor Restrictions

Over the last two decades, Swiss cantons adopted 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. From 1999–2016, a number of policy changes, especially in 2006–2010, eased the labor market access for refugees. However, the timing and degree of liberalizations varied by canton and asylum status. This led to substantial policy variation across cantons, by refugee status, and time. Consequently, 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.

Asylum process and permits. The average length of the asylum process in Switzerland, from submitting an application to obtaining a decision, is about two years during our study period. 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. If the asylum claim is rejected and no subsidiary protection is granted, the person has no right to stay in Switzerland and is excluded from the analyses. Appendix Figure A.2 shows that more than two-thirds of refugees remaining in Switzerland receive a TAF permit. At the same time, only 26.6% are legally recognized as refugees (B permit), and 8.5% eventually receive the TAR status.

Dispersal policy. The identification of the policy effects is 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,

⁴For 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).

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.⁵ We empirically substantiate the exogeneity of assignment in Appendix Figure C.1. The exogenous assignment through placement officers minimizes the ability of refugees to endogenously sort into cantons with favorable labor market policies upon arrival.

Settlement restriction. Refugees’ assignment to cantons is all the more consequential as asylum seekers and refugees under subsidiary protection are prohibited from moving across cantons during the first five years in Switzerland. The settlement restriction leads to very low levels of inter-cantonal mobility among newly arrived refugees. As Appendix Figure A.1 shows, 92% of TAF and TAR permit holders still reside in the initially assigned canton eight years after arrival. Even for holders of a B permit, who are allowed to move between cantons after the asylum decision, inter-cantonal mobility is low. The combination of the dispersal policy and the settlement restriction implies that the policies regulating labor market access in the canton of initial assignment can persistently impact refugees’ economic integration trajectories.

Labor market restrictions. To build a panel dataset on the four labor market policies from 1999 to 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, permit, canton, and year with representatives of the relevant cantonal ministries.⁶

For all four asylum status groups, we coded the following policies from 1999 to 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 1 (a) illustrates that, during our study period, several cantons extended the minimal three-month ban, increasing it to six months (e.g., Glarus, Nidwalden, Uri, Zug) or even fourteen months (Solothurn until 2006). In contrast to other policies discussed below, the minimal three-month ban does not depend on the permit status.

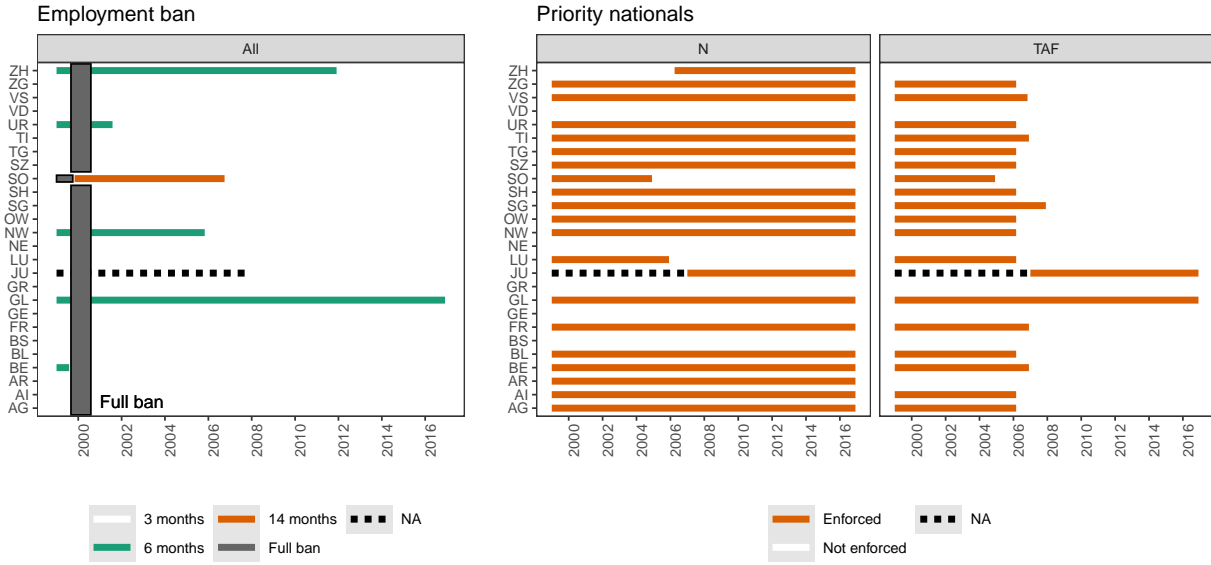
Figure 2 provides descriptive evidence that for the vast majority of refugees, employment bans are binding and determine the integration path beyond the expiration time. We observe only few refugees who work in periods when they are subject to an employment ban. These few cases are likely due to mismeasurement of refugees’ start or end dates of employment spells or arrival dates in Switzerland or non-compliance with the ban in specific cases.

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

⁵These reasons are pre-existing first-degree family networks, health issues requiring treatment in a particular hospital, or the accommodation of unaccompanied minors (the latter group is excluded from our analysis). 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)

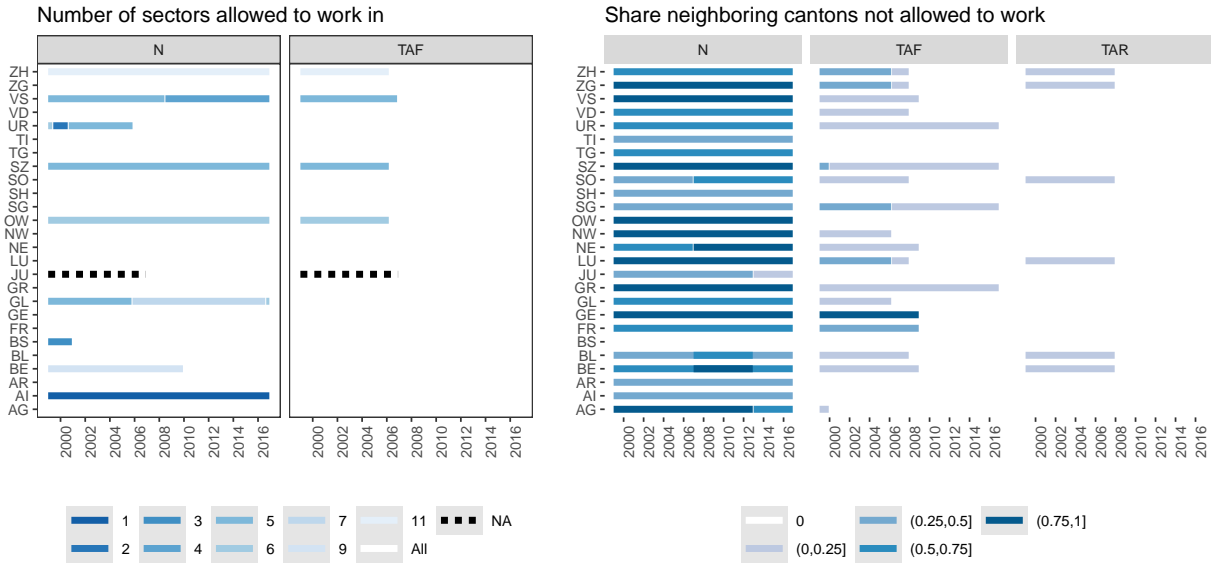
⁶We code a policy as “highly reliable” (43.1% of the sample) if the information is confirmed by a law, public internet resources, or two experts. If the information about a cantonal policy change remained unspecific (e.g., the exact date of the policy change was not confirmed) we classified it with the label “low reliability” (15.6% of the sample). The remaining policy observations were assessed to be of “normal reliability.” Appendix Table C.4 shows that our main results are similar if we allow for separate effects of policy changes coded as low reliability.

Figure 1: Labor market policies for refugees by canton, year, and permit



(a) Employment ban

(b) Priority enforcement

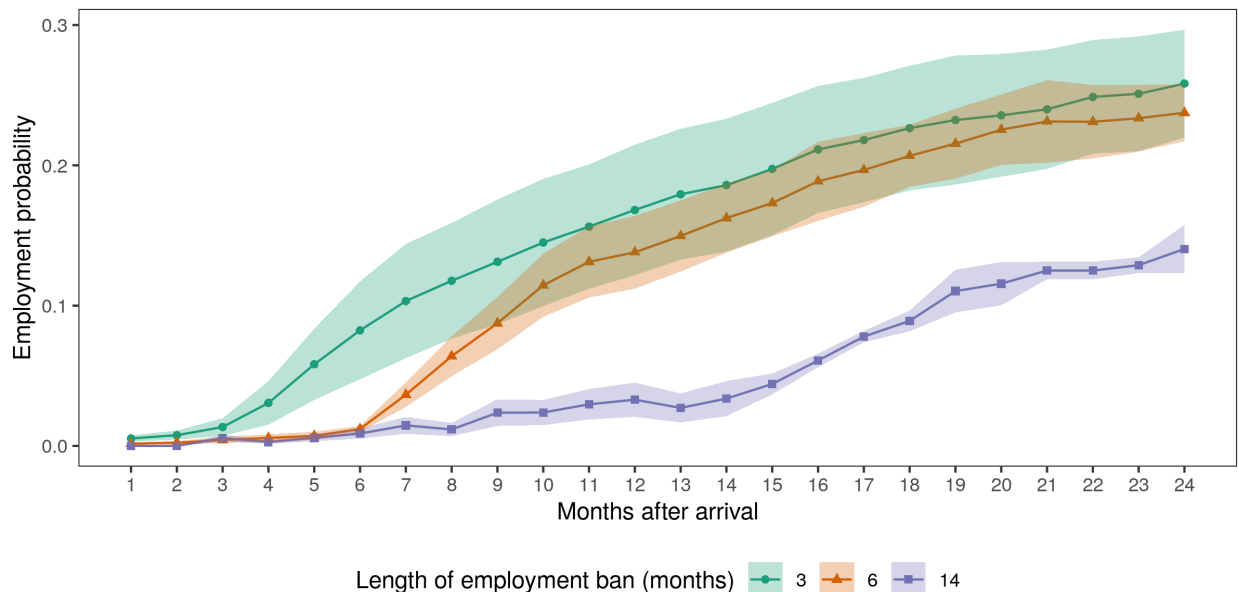


(c) Sector restrictions

(d) Region restrictions

Notes: Figure (a) 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. Figure (b) shows when prioritization was enforced or strictly enforced in Swiss cantons. Figure (c) depicts the number of sectors asylum seekers (N) and temporarily admitted foreigners (TAF) are allowed to work in. Figure (d) shows the share of neighboring cantons that do not issue work permits for refugees assigned to a canton.

Figure 2: 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 \times transition group clustering.

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 caseworkers can encourage prioritized jobseekers to apply). Figure 1 (b) illustrates that a major change for this policy occurred in 2006 when the posteriorization of TAF *vis-à-vis* prioritized job seekers has been lifted in many cantons. However, two cantons (Jura and Glarus) continued to posteriorize TAF until 2016. TAR and B refugees are not subject to these posteriorization policies.

(iii) *Sector restrictions.* Cantons can restrict work permits to selected economic sectors for asylum seekers and TAF permit holders. Figure 1 (c) shows that if any sector restrictions are applied, restricting cantons allow refugees to work in between one and nine out of 58 two-digit sectors. The permissible sectors typically include farm and construction work, care work in hospitals and elderly homes, and waste disposal.

(iv) *Regional restrictions.* Cantons have considerable discretion in issuing work permits for asylum seekers, TAF, and TAR who are assigned to live in another canton. In Figure 1 (d), we calculate the share of neighboring cantons that do not issue work permits for asylum seekers and refugees with subsidiary protection living in a particular canton. The figure shows that, overall,

asylum seekers face the most severe restrictions. We also observe that regional restrictions gradually d 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.

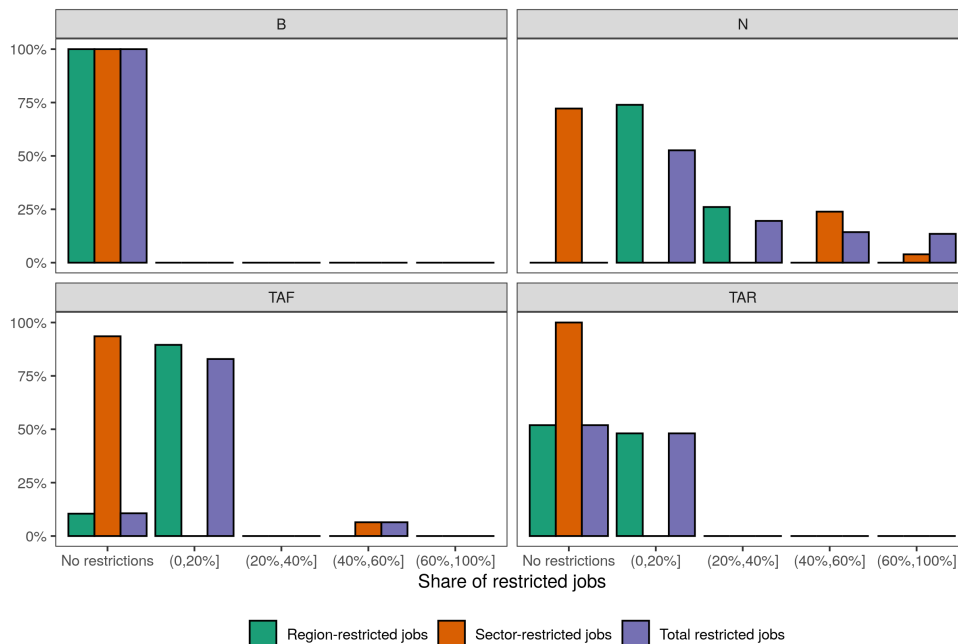
Total share of restricted jobs. For the analysis, we combine the sector and regional restrictions into a joint variable measuring the share of job opportunities *not* available to refugees. To construct this “share of restricted jobs” for each permit category, canton, and month, we combine sectoral employment shares of refugees who have never been exposed to sector restrictions with national commuter data from the Swiss population census in 2000 to proxy ease of commuting. 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 regional restrictions. $r_{ijts\ell}$ 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 and month t either due to region or sectoral restrictions, 0 otherwise (see Appendix B for details). This measure quantifies the total loss of job opportunities 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, 88% of the jobs are unavailable to refugees because they are banned from specific sectors and from working in 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. However, existing studies agree that in Switzerland, undocumented immigrants, not asylum seekers and refugees, 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 market policies in Appendix A.1. Consistent with Figure 2, 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 a sector 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 (State Secretariat for Migration, 1999), and social security data from the Old-Age and Survivors’ Insurance

Figure 3: Distribution of the share of restricted jobs due to geographical and sectoral restrictions, by status.



Notes: This figure shows the share of job opportunities *not* available to refugees (by status) due to sector and regional restrictions. See Appendix B for details on the construction of this variable.

(AHV) database (Swiss Central Compensation Office, 1999). 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 (Swiss Federal Statistical Office, 2010), the register-based census of Switzerland conducted since 2010, to verify which individuals still reside in Switzerland after leaving the asylum system.

We measure refugees' employment and earnings history using AHV records covering 1999–2015. 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 are irrespective of the residency permit or the contract type. The data 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 annual income threshold of CHF 2,300. Labor earnings recorded in the data are uncapped and broadly defined. In the registry, employed and self-employed individuals generate one record per job spell and year with an associated start and end month, a firm identifier, and the total earnings over the employment relationship. 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 (SSES). To study how the labor market policies affect refugees’ hourly wages, we analyze the waves of 2012, 2014, and 2016 of the Swiss earnings structure surveys (Swiss Federal Statistical Office, 2012).⁸ The employer surveys are a large stratified random sample of private and public firms with at least three full-time-equivalent workers. The surveys cover all sectors except the agricultural sector. Depending on the wave, they contain information on 1.58 to 1.98 million workers, which translates to 32 to 39% of total employment in the sectors covered. Since the surveys are mandatory, response rates are high (e.g., 75% in 2014). 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 STATPOP 2012–2016 using the social security number. The STATPOP, a census, provides information on refugees’ residency permits, the place of living, and the canton to which they were assigned upon arrival in Switzerland.

4 Employment and earnings

4.1 Empirical approach

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

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

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 status $s \in \{N, TAF, TAR, B\}$ in month t . Our sample only includes individuals who applied for asylum after January 1999 and who received either a TAF, TAR, or B permit after their asylum decision, i.e., we exclude persons who are required to leave Switzerland. 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 , (ii) an indicator whether the assigned canton enforces priority for residents, and (iii) the share of

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 C.3.

⁸Although the surveys were conducted bi-annually since 1994, 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.

sector- and region-restricted jobs for refugees in the local economy as defined in Section 2.2.⁹ Our base specification uses canton of assignment (μ_c), month fixed effects (δ_t) and months-since-arrival fixed effects interacted with status s , which we denote by $\gamma_{t-T(i),s}$ where $T(i)$ is the arrival month. Finally, we adjust for a few relevant controls, collected in the vector x_{icst} , which we will discuss below.

To understand the rationale for our econometric model, it is worth highlighting that there are two reasons why a refugee i might experience a policy change: either because she changes her status s after a decision on her asylum application or because the canton c changes its policy for a certain status group (permit). As discussed, our analysis benefits from the largely exogenous assignment of refugees to cantons and the settlement restriction prohibiting refugees from endogenously sorting into cantons with favorable labor market policies for the first five years after arrival. However, two threats to identification are related to the two sources of policy changes. First, the timing and outcome of the asylum decision could be related to refugees’ employment potential. Legally, this should not be the case under Swiss asylum law and the refugee convention requiring that asylum decisions are made independently of labor market considerations. Nevertheless, in our conservative baseline specifications, we abstract from status variation and only compare individuals exposed to different policy regimes who hold the same status and have been in Switzerland for the same number of months. We achieve this by including fixed effects for the number of months since arrival in Switzerland interacted with the status ($\gamma_{t-T(i),s}$). In addition, we show that the estimated effects of restrictions are robust when we focus on TAF permit holders only or when we split the samples by “transition groups”. We refer to a transition group as all individuals who enter as asylum seekers and eventually obtain the same protection status. Note that refugees receiving a TAR or B permit have little or no, respectively, within-permit variation in policies as shown in Figure 1. In Section 5, we show in the context of the wage analysis that exploiting between-status variation does not alter the estimates. Based on this result, we exploit between-status variation in the SSES data because there is very little within-canton policy variation in the SSES sample period (2012–2016).

A second concern 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 use canton fixed effects, ensuring that we account for time-invariant correlates of cantonal policies. Second, we generalize the monthly panel regression to an event study to test for pre-trends. These high-frequency event studies show that the outcomes of treated and untreated individuals evolve similarly in the months before and display immediate differences in trends in the months after policy changes. Lastly, we explore in a separate analysis (in Appendix C.2) if local labor market conditions predict policy changes up to three years ahead and find little evidence that cantonal economic conditions determine policies for refugees.¹⁰

⁹Since 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.

¹⁰Table C.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

Finally, our regressions contain a few control variables. In the base specifications, we control for the typical life-cycle earnings profile of refugees through gender-specific age and age-squared. We also always control for changes in the cantonal level of social welfare benefits, which we coded along with the labor market restrictions.¹¹ Changes in welfare benefits may be correlated with changes in labor market restrictions, and benefits potentially affect refugees’ labor market outcomes (see, e.g., Dustmann, Landerso, and Andersen, forthcoming). In an additional control vector, we assess whether our results are sensitive to the inclusion of the contemporaneous cantonal unemployment rate and the cantonal unemployment rate at refugees’ arrival. While the former control variable further addresses concerns that policy changes may be correlated to local economic conditions, the latter accounts for the importance of initial conditions for long-term labor market outcomes (Von Wachter, 2020; Müller, Pannatier, and Viarengo, 2023). Finally, the extended control vector also includes time-invariant personal characteristics that the case workers observe when processing the cantonal assignment decision. These controls are indicators for marriage status, self-reported religion (Christian and Muslim), nationality, and fixed effects for the asylum processing centers. As shown below, including these additional controls does not change any main policy estimates.

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) and total labor earnings, including zero earnings for non-workers (Panel B). The first three columns show separate estimates for three different transition groups: asylum seekers whose claim is rejected but who are temporarily admitted (denoted $N \rightarrow TAF$) and asylum seekers whose claim is granted (denoted as $N \rightarrow TAR$ or $N \rightarrow B$, respectively). Columns 4–6 pool these three transition groups. Column 7 shows the results for refugees with a TAF permit only, thus focusing on the largest group of refugees with the richest policy variation. We cluster standard errors at the canton level in columns 1–3, and 7, as these are the levels at which the policies vary. In the remaining columns, we cluster at the transition group times canton level to allow for within-person over-time dependence.

Employment. Panel A of Table 1 shows the effect of the labor market restrictions on a monthly employment indicator. We estimate the regressions using OLS, but results are similar in a logistic regression (see Table C.2). 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 2, we find that the employment probability is strongly reduced in a month when an employment ban applies. The likelihood of being employed is

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.

¹¹Swiss cantons have the freedom to set the base allowance, determining the monthly financial support refugees receive. To code the allowance level, we reviewed cantonal laws and regulations and cross-checked our coding with cantonal ministries. The base allowance ranges between 180 and 1190 CHF depending on status and canton.

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	-0.1156*** (0.0262)	-0.2228*** (0.0352)	-0.1481*** (0.0264)	-0.1325*** (0.0184)	-0.1317*** (0.0219)	-0.0760*** (0.0113)	-0.1067*** (0.0221)
Priority enforced	-0.0535*** (0.0108)	-0.0553* (0.0274)	-0.0607*** (0.0197)	-0.0530*** (0.0096)	-0.0493*** (0.0102)	-0.0247*** (0.0091)	-0.0592** (0.0258)
Share restricted jobs	-0.0599 (0.0405)	-0.0403 (0.0297)	-0.0447 (0.0283)	-0.0569* (0.0287)	-0.0583* (0.0335)	-0.0358* (0.0191)	-0.0698 (0.0688)
Outcome mean	0.1894	0.1438	0.1452	0.1732	0.1732	0.1732	0.2293
Num. individuals	41,075	6,494	20,059	67,628	67,780	67,780	33,941
Observations	1,764,732	246,365	759,223	2,770,320	2,772,775	2,772,775	1,263,386
<i>Panel B. Total earnings (Poisson estimator)</i>							
Employment ban	-1.105*** (0.1878)	-2.589 (1.621)	-1.591*** (0.4004)	-1.220*** (0.1342)	-1.237*** (0.1521)	-1.597*** (0.1566)	-0.9081*** (0.1165)
Priority enforced	-0.3948*** (0.0629)	-0.7375*** (0.1804)	-0.9841*** (0.1945)	-0.4517*** (0.0606)	-0.4612*** (0.0592)	-0.3738*** (0.0698)	-0.2403** (0.1086)
Share restricted jobs	-0.5771*** (0.2121)	0.4383 (0.5554)	-0.1188 (0.4130)	-0.4757** (0.2020)	-0.5903** (0.2336)	-0.5742*** (0.1465)	-0.3403 (0.2728)
Outcome mean (CHF)	505.7	365.8	328.0	444.3	444.3	950.9	621.6
Num. individuals	41,075	6,494	20,059	67,628	67,780	23,225	33,941
Observations	1,763,557	246,047	759,222	2,770,155	2,772,610	1,295,608	1,263,366
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 x status	Yes	Yes	Yes	Yes	Yes	Yes	
Months-since-arrival FE							Yes
Additional controls	Yes	Yes	Yes	Yes	No	No	Yes

Notes: The table shows the effect of the labor market restrictions on monthly employment indicators (panel A) and total labor earnings of refugees (including non-workers, panel B) in the first five years after arrival. The regressions are based on equation (1). In panel B, we estimate a Poisson fixed effects model to accommodate zero earnings. Columns 1–3 present separate estimates for individuals who enter as asylum seekers and eventually transition 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 transition groups. Column 7 shows the effects for refugees with a TAF permit only. All columns include month and canton of assignment fixed effects and month-since-arrival fixed effects, which are interacted with refugees’ status in columns 1–6. Column 5 omits the additional controls. These are indicators for marriage status, self-reported religion (Christian and Muslim), nationality, and fixed effects for the asylum processing centers. 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: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

more than 13 percentage points (p.p.) lower, consistent with the evidence presented by Marbach, Hainmueller, and Hangartner (2018) and Fasani, Frattini, and Minale (2021) for a range of European countries. These estimates also serve as a validation check for our coding: We observe very few refugees with jobs in the months with an employment ban. Second, refugees allocated to cantons that enforce the priority of permanent residents are 5.3 p.p. less likely to be employed in a given month than refugees in cantons where this policy is not enforced. As the average employment rate of refugees is only 17.3%, this effect represents 30.6% reduction in refugees' overall employment rate. Third, restricting the share of jobs available to refugees, either by only allowing their employment in certain sectors or only in some but not all neighboring cantons, seems to reduce employment further. While the effect is only marginally significant in the pooled specifications, it is economically large in size: 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 the employment rate by roughly 1.7 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 TAF refugees (column 7). When individual fixed effects are included (column 6), the estimated employment effects are a bit smaller. This small reduction is likely driven by three reasons: First, the effects are implicitly identified from a shorter window around the policy changes.¹² Second, as we show below, tightening restrictions does not lead to layoffs of refugees in employment. Rather, non-employed refugees become less likely to find a job. Third, policies reduce workers' outcomes for years beyond they apply, implying that relaxing a restriction has a less immediate impact on an exposed person relative to someone who arrived shortly after restrictions have been lifted. Together, these facts imply that the effects of the policies are larger for two individuals of different cohorts than for the same individual over time.

Total earnings. Next, we document how the policies affect total labor earnings in Panel B of Table 1. Since the outcome variable includes zero earnings for refugees without employment, we employ the Poisson fixed effects 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 70.5%, enforcing priority by roughly 36.3% and removing a third of refugees' potential jobs by about 14.7%. Table C.5 in the Appendix shows that this negative earnings effect in our main specification stems from both sector and region restrictions.

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 F, our estimates imply that moving from an unrestricted to the most restrictive cantonal

¹²The 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.

policy environment observed in our sample reduces the average employment rate of refugees facing the most restrictions (i.e., N and TAF) from 19.2% to 11.5% during the first five years in Switzerland. Similarly, for the most affected, average monthly earnings are 44.6% lower in the most restrictive policy environment compared to no restrictions. Furthermore, Appendix C.4 shows that the effects are concentrated among refugees with above-median predicted employability, particularly younger and male refugees.

Benchmarking the 7.7 p.p. employment effect on most affected refugees suggests that the labor market restrictions are at least as important determinants of refugees' economic integration as local economic conditions or some of the most 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%.¹³ These effects are also large in comparison to the six p.p. employment increase associated with language training, the most effective integration policy studied by Foged, Hasager, and Peri (2022) and Arendt, Dustmann, and Ku (2022) for refugees in Denmark.

4.3 Robustness

Event studies. As a first step to probe the robustness of our baseline estimates, we assess whether the main outcomes follow parallel trends before the policy changes. To this end, we generalize 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.

We focus on the priority policy and the share of restricted jobs since an employment ban implies that individual-level labor market outcomes prior to lifting it are zero and, hence, parallel by definition. We estimate the following event study model, which extends conventional event studies to two policies (Freyaldenhoven et al., 2021):¹⁴

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

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 $j = -1$), 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-month time period that each refugee is in our sample.

¹³The estimates of the effects of the contemporaneous local unemployment rate on employment are -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.

¹⁴For 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 shows the event study plots for the priority policy and the share of restricted jobs on employment and earnings, respectively. Note that our estimates are mostly identified from the removal of restrictions, while Figure 4 shows the effect of introducing restrictions. The event studies provide little evidence for differential trends in refugees’ employment and earnings trends in cantons and months before restrictions are tightened or loosened. If anything, we observe a slight linear upward trend for some pre-treatment outcomes in some specifications, suggesting that we might underestimate the impact of the policies. Importantly, 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.

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 (Figures (a) and (b)) and total earnings (Figures (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 status dummies. We also add individual-level controls: age and age-squared interacted with sex, maximum cash allowance in CHF, marriage status, two dummies for self-reported religion (Christian and Muslim), nationality, and asylum processing center fixed effects as well as the unemployment rate (measured contemporaneously and at arrival). 95% confidence intervals are robust to canton \times transition group dependence.

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, 2023). 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. Similar in spirit to Wooldridge (2021) and Sun and Abraham (2020), we thus interact the policies with group indicators and aggregate the effects using group shares to avoid issues associated with negative weights. This approach is robust to heterogeneous treatment effects along either months-since-arrival in Switzerland or calendar-year fixed effects (see Appendix C.5 for details). Overall, we find qualitatively similar effects if we account for heterogeneity by time since arrival or calendar year.

5 Wages

We have established that the restrictions adversely affect refugees’ employment and total earnings. In this section, we examine the effect of the labor market policies on refugees’ pay. Specifically, in Sections 5.1, we analyze whether restrictions affect refugees’ monthly earnings using the AHV data. In Section 5.2, we leverage a complementary data source, the SESS waves 2012–2016, to investigate the policies’ impacts on hourly wages and analyze if restrictions contribute to explaining the refugee-native wage gap. Finally, Section 5.3 explores why these wage effects arise.

5.1 Results using social security data (AHV)

Table 2 presents the results of regressing log monthly earnings of refugees on the priority dummy and the share of sector- and region-restricted jobs based on equation (1).¹⁵ Columns 1 and 2 focus on refugees during the first five years after arrival who, after the asylum decision, receive a B or TAR permit, respectively. The columns provide evidence that prioritizing residents reduces refugees’ monthly pay. The effects are imprecisely estimated but economically large, indicating that prioritization reduces monthly wages by about a third. We do not find a statistically significant wage effect of the share of sector- and region-restricted jobs in the first two columns.

One issue with columns 1 and 2 of Table 2 is that prioritization and sector and region restrictions do not apply to holders of a B permit and, as shown in Figure 1, there is virtually no within-cantonal policy variation for TAR permit holders, the smallest of the permit groups. In the first two columns, the policies’ wage effects are primarily identified from within-cantonal policy variation for asylum seekers only (N permit). However, given their low employment rate, we have limited statistical power to identify wage effects for asylum seekers alone.

We thus shift our attention to the large group of refugees who receive a TAF permit after their asylum decision (i.e., $N \rightarrow TAF$) for whom we observe sufficient within-canton variation in the restrictions (see Figure 1). Results are shown in the remaining columns of Table 2. Column 3 shows that restricting refugees’ labor markets sectorally or geographically depresses their wages. The point estimates suggest that removing 10% of potential jobs reduces monthly wages by 3.0%.

¹⁵We control for but do not show the effect of banning refugees from working since it is hard to interpret. After all, we should not observe any wage while the ban applies.

Table 2: Effect of labor market policies on monthly earnings

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Dependent variable: Earnings (log)</i>						
Priority enforced	-0.4130*	-0.4060*	-0.0797*	-0.1102	-0.0800*	-0.0773*
	(0.2227)	(0.1978)	(0.0414)	(0.0665)	(0.0405)	(0.0410)
Share restricted jobs	-0.1385	0.2510	-0.2968**	-0.1928	-0.2964**	-0.2915**
	(0.3767)	(0.4745)	(0.1193)	(0.1761)	(0.1169)	(0.1199)
Sample	N->B	N->TAR	N->TAF	N->TAF	N->TAF	N->TAF
Outcome mean	2,259.2	2,540.9	2,668.8	2,669.2	2,668.8	2,668.8
Num. individuals	6,454	2,060	14,661	14,711	14,661	14,661
Observations	110,230	35,426	334,139	334,539	334,139	334,139
Canton FE	Yes	Yes	Yes		Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE				Yes		
Months-since-arrival FE x status	Yes	Yes	Yes	Yes		
Months-since-arrival FE					Yes	
Months-since-arrival levels + squared						Yes
Additional controls	Yes	Yes	Yes		Yes	Yes

Notes: The table shows the effect of the labor market restrictions on log monthly labor earnings of the employed in the first five years after refugees arrival. The regressions are based on specification (1). Columns 1–3 present separate estimates for individuals who enter as asylum seekers and eventually transition to temporarily admitted foreigners (denoted N→TAF), to temporarily admitted refugees (N→TAR), and to recognized as refugees (N→B). Columns 4–7 pool these three groups. Columns 1–5 use months-since-arrival fixed effects interacted with the asylum status, while column 6 omits the interaction with asylum status. Column 7 controls for months-since-arrival in levels and squared. All columns include month fixed effects. Column 5 uses individual fixed effects, while all other columns control for canton 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: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

The effect size is relatively similar to those reported by Caldwell and Danieli (forthcoming), who find that access to 10% additional outside options increases wages by 1.7%. The estimates in column 3 also suggest that enforcing priority lowers monthly pay by 8.0% (significant at the 10% level). The point estimates are similar in magnitude in the restrictive specification adding individual fixed effects (column 4), but they turn statistically insignificant (p -value for priority: 0.11, p -value for the share restricted jobs: 0.28). Relative to the canton-fixed effects specification in column 3, the effect size for both policies is virtually unchanged when removing status fixed effects (column 5) and when replacing month-since-arrival fixed effects with a continuous measure of length-of-stay in levels and squared (column 6). Furthermore, Appendix Table D.1 shows that the point estimates are virtually unchanged if we drop the additional controls, estimate separate effects for region and sector restrictions, and allow for heterogeneous treatment effects. By omitting the status fixed effects, columns 5 and 6 of Table 2 exploit policy variation arising from the timing—not the outcome¹⁶—of the asylum decisions. In the next section, we will use the similarity in the point estimates in columns 3, 5, and 6 as a motivation to devise a research design that exploits status variation to overcome the limited within-canton and within-status policy variation during the shorter SESS study period 2012–2016.

¹⁶We do not exploit differences in the decisions’ outcomes since we restrict the sample to temporarily admitted foreigners and thus individuals with the same outcome of the asylum process.

5.2 Results using employer survey (SESS)

In this section, we leverage the 2012, 2014, and 2016 waves of the SESS to provide evidence of the restrictions’ effects on absolute and relative hourly wages. While the SESS study period is shorter than the coverage of the AHV data, two properties of the SESS data make it attractive for studying wage effects: First, in contrast to the AHV data, the SESS records employer-reported monthly hours worked, enabling us to estimate effects on hourly pay. Second, the data include rich information on workers and their jobs, including workers’ education, tenure, occupation, and industry association. This allows us to study the reasons for potential wage effects and compare workers with similar observed characteristics in similar jobs.

We use two complementary estimation approaches with the SSES data. The first, presented in Appendix D.2, closely mirrors the AHV-based strategy but accounts for the shorter study period of the SESS data. The second estimation strategy, which delivers similar results and is discussed in the following, allows us to quantify the impact of labor market restrictions on the refugee-native wage gap. Specifically, including refugees and non-refugees in our estimation sample, we estimate conditional wage differentials between refugees, other immigrants, and Swiss citizens and interact the estimated wage gaps with the labor market restrictions. The interaction terms identify the restrictions’ effect on refugees’ wages and reveal whether the wage gap of refugees relative to native citizens is particularly large in cantons with restrictive policies. In addition, the focus on relative wages has the benefit of accounting for unobserved time-varying canton-specific conditions that similarly affect refugee and non-refugee wages.

Our estimation equation can be written as

$$y_{idct} = \psi_d + \psi_F + \alpha'_d p_{idct} + \beta' x_{idct} + \gamma_{d,1} \tau_{it} + \gamma_{d,2} \tau_{it}^2 + \mu_c + \delta_t + \varepsilon_{idct} \quad (3)$$

where c denotes worker i ’s canton of residence and $t \in \{2012, 2014, 2016\}$ is the SESS wave. The sample comprises all employees aged 18–64 surveyed in one of the three SESS waves, including Swiss citizens, foreigners working and living in Switzerland (regular migrants), and employees with an asylum background provided they still have an N, TAR, TAF, or B permit.¹⁷ The outcome variable y_{idct} is the log hourly wage. ψ_d is an indicator denoting refugees’ transition group d and measures the wage penalty relative to Swiss citizens. We distinguish two transition groups d : refugees who eventually receive subsidiary protection (i.e., N→TAR/F) and refugees who receive a B permit (N→B). We pool the refugees with subsidiary protection (TAR and TAF permit) because the SESS data does not distinguish between the two groups. We define the joint TAF/TAR policy variables as the weighted average of TAF and TAR policies using the number of employed refugees in the two statuses in 2012 as the weight, which gives TAR refugees a weight of 14.1%. ψ_F is an indicator equal to one for other foreign workers in the Swiss labor market. Thus, the wage effects in (3) are estimated relative to Swiss nationals.

¹⁷To increase the precision of the estimates, we also include refugees with one of these permits who have stayed in Switzerland for longer than five years. Column 5 of Appendix Table D.2 shows that the wage effects in the SSES are similar but less precise if they focus only on workers within their first five years in Switzerland.

The key parameters α_d , estimated separately for the two transition groups, capture the wage effects of the labor market restrictions p_{idct} . They reveal whether refugees allocated to a canton with restrictive policies have a larger wage gap relative to Swiss citizens than refugees allocated to a liberal canton. The vector x_{idct} includes controls for gender, gender-specific age and age squared, marital status, and the cantonal unemployment rate in the survey month.

Compared to the regressions in Section 5.1 using AHV data, the most significant change in identifying the policies' effects is that we now also exploit between-status policy changes originating from the asylum decision as we do not control for status fixed effects. We leverage status variation because, over 2012–2016, there is *no* within-cantonal variation in prioritization and sector restrictions and virtually no variation in region restrictions in the relevant permit categories (see Figure 1). Our previous strategy based solely on within-status within-canton policy variation is thus not feasible. However, leveraging between-status variation is not innocuous. A key concern 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, one might think of ways through which 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, increasing the likelihood of protection and shortening the time until a decision is made.

We address this concern in various ways. First, empirically, we have already validated in the context of the wage analysis in columns 5 and 6, Table 2, that exploiting between-status variation leaves our key point estimates virtually unchanged in the AHV data where we have sufficient time coverage only to use cantonal policy variation. Second, our focus on variation within transition groups d implies that we only compare individuals whose asylum decisions turned out to be the same. Third, we only compare refugees who spent a similar amount of time in Switzerland. To this end, we control for refugees' years since arrival in Switzerland, $\tau_{id} = t - T(i)$. We include those controls separately per transition group d and increase precision by adding them in levels and squared instead of a full set of fixed effects. In Table 2, the results are virtually unchanged when replacing years-since-arrival fixed effects with a continuous measure in levels and squared. The same is true in Appendix Table 5.2 with the SSES data.

Before exploring the role of the labor restrictions, we present estimates of the unexplained wage gap between refugees and Swiss citizens in columns 1–3, Table 3. The coefficients of interest are indicators for refugee workers who are eventually granted asylum (N→B) and who are temporarily admitted (N→TAF). The first regression, which only includes the baseline controls x_{it} , canton, and year fixed effects, suggests that refugees' average wages are more than a third lower than native citizens. The wage gap amounts to $100 \times (e^{-0.475} - 1) = -38.8\%$ for refugees granted asylum and -39.0% for temporarily admitted refugees. Refugees' wage gaps relative to other foreign workers are smaller but still very large (approximately -28.4%). The second column shows that roughly two-thirds of this gap can be explained by differences regarding proxies of human capital (i.e., firm tenure and educational attainment). Another half of the remaining gap can be attributed to

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
N→B	-0.475*** (0.026)	-0.176*** (0.025)	-0.085*** (0.016)	-0.437*** (0.082)	-0.151* (0.084)	-0.098 (0.085)	-0.090 (0.086)
N→TAR/F	-0.495*** (0.018)	-0.153*** (0.016)	-0.065*** (0.015)	-0.440*** (0.077)	-0.133* (0.073)	-0.028 (0.067)	-0.023 (0.067)
Foreigner	-0.141*** (0.008)	-0.041*** (0.003)	-0.018*** (0.003)	-0.141*** (0.008)	-0.041*** (0.003)	-0.018*** (0.003)	-0.018*** (0.003)
N→B × Priority enforced				0.057 (0.065)	-0.006 (0.029)	0.008 (0.028)	0.014 (0.037)
N→B × Share restricted jobs				-0.207 (0.138)	-0.125 (0.141)	0.006 (0.147)	-0.018 (0.158)
N→TAR/F × Priority enforced				0.000 (0.067)	-0.046 (0.064)	-0.053 (0.061)	-0.036 (0.060)
N→TAR/F × Share restricted jobs				-0.541** (0.208)	-0.460** (0.180)	-0.513*** (0.176)	-0.550*** (0.178)
Observations	4,547,343	4,007,608	3,368,545	4,547,343	4,007,608	3,368,545	3,368,514
R-squared	0.170	0.424	0.529	0.170	0.424	0.529	0.529
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Canton of living FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Tenure FE	No	Yes	Yes	No	Yes	Yes	Yes
Educational attainment FE	No	Yes	Yes	No	Yes	Yes	Yes
Canton of work FE	No	No	Yes	No	No	Yes	Yes
Industry FE	No	No	Yes	No	No	Yes	Yes
Occupation and management level FE	No	No	Yes	No	No	Yes	Yes
Years-since-arrival controls	No	No	No	Yes	Yes	Yes	Yes
Cumulative work experience	No	No	No	No	No	No	Yes

Notes: This table uses SESS data 2012, 2014, and 2016 to analyze the effects of labor market restrictions on the (relative) wages of refugees, other foreign workers, and Swiss nationals based on equation 3. The dependent variable is the hourly wage of workers aged 18–65. N→B is a dummy equal to one for individuals who enter as asylum seekers and eventually transition to B permit. Similarly, N→TAR/F is a dummy equal to one for individuals transitioning from N to TAR or TAF permit. Foreigner is a dummy for all other resident non-Swiss workers. Columns 1 and 4 estimate wage gaps conditional on controls and fixed effects for the year and workers’ canton of living. Columns 2 and 5 add years of tenure and eight levels of highest educational attainment fixed effects. Columns 3 and 6 add fixed effects for canton of work, two-digit NACE industry, ISCO two-digit occupation, and five management levels (from no to highest-level management). Columns 4–7 also show interactions between the status-specific labor market restrictions and the refugee indicators. The policy variables are set to zero for non-refugees. These columns additionally control for refugees’ years since arrival in Switzerland (linear and squared), separately for the two transition groups. Column 7 additionally controls for refugees’ cumulative months employed in Switzerland as measured in AHV data (linear and squared). Standard errors are clustered at the canton of residence times person group (Swiss, foreigner, N→B, and N→TAR/F) level. Observations are weighted using the person weights in the data. Controls are gender, gender-specific age and age squared, marital status, and the cantonal unemployment rate in the survey month. Signif. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

differences in the canton of work, industry, occupation, and the jobs' management levels (column 3). However, even if we only compare similarly qualified refugees and natives working in similar jobs, the unexplained gap amounts to 6.3% and 8.1% for $N \rightarrow F$ and $N \rightarrow B$, respectively.

The remaining columns in Table 3 test whether labor market policies affect refugees' wages and the unexplained refugee-native wage gap. To this end, we interact the transition group with the priority dummy and the share of restricted jobs. The interaction terms on the priority policy are generally close to zero and statistically insignificant. We also do not find significant effects for the sector and region restrictions among $N \rightarrow B$ refugees, although the estimates point in a negative direction in columns 4 and 5. The fourth interaction term suggests that $N \rightarrow \text{TAR}/F$ refugees allocated to cantons where sector and region restrictions remove many potential jobs are paid substantially worse relative to natives than refugees of the same transition group allocated to a less restrictive canton. For example, if the assigned canton prohibits access to 30% of potential jobs, their relative wages are at least 14.2% lower than if the assigned canton imposes no sector or region restrictions. The effect remains very similar and statistically significant even if we condition on a rich set of worker, firm, and job characteristics (columns 5–7), a result to which we return below. The pattern of results is also similar when we restrict the SSES sample to refugees only and consider a specification similar to our AHV-based analysis with absolute rather than relative wages, suggesting that the restrictions exert similar effects on refugees' relative and absolute hourly wages (see Appendix D.2).

Overall, the AHV and SSES results suggest that sector and region restrictions lower refugees' monthly and hourly pay. We also find a sizeable negative effect of enforcing priority on refugees' monthly earnings in the AHV data. The SSES data do not confirm the evidence for negative wage effects of prioritization, but the point estimates for the $N \rightarrow \text{TAR}/F$ group are mostly negative, and standard errors are quite wide, such that the 95% confidence intervals include the negative estimates from the AHV sample.

5.3 Mechanism

In the following, we investigate mechanisms of why sector and region restrictions and possibly prioritization reduce the wages of refugees, focusing on market-level labor supply shifts, reductions in refugees' productivity, and outside options.

5.3.1 Labor supply effects

One possible reason for the wage effects of labor market restrictions is that restrictions depress refugees' wages by shifting the equilibrium labor supply curve inward. Such equilibrium effects are particularly likely if refugees are close substitutes to each other but imperfect substitutes to other workers. However, if the labor demand curve is downward-sloping, this would create upward pressure on wages. Our results thus speak against this mechanism because we established negative

wage effects in the previous subsections.¹⁸ Furthermore, refugees only represent 0.16% of the Swiss workforce. Even in the restaurant sector, where refugees' employment share is highest, they only amount to 1.5% of total employment. Thus, shifts in refugees' labor supply are unlikely to have measurable effects on the wages of all workers.

5.3.2 The role of productivity differences

A possible explanation for the negative wage effects of labor market restrictions is that they lower the productivity of employed refugees. Table 3 (relative wages) and Appendix Figure D.1 (absolute wages) explore three productivity-based explanations of the wage effects. Together, the tables show that the wage effect is unaltered if we control for industry, canton of work, and occupation fixed effects, refugee workers' tenure, accumulated months of work experience in Switzerland (computed using the social security data), and highest educational attainment (which includes a separate indicator for informal on-the-job learning for workers with no secondary degree). These results speak against the possibility that restrictions lower refugees' productivity by forcing them to accept jobs in sectors, regions, or occupations with low productivity and, hence, low pay.¹⁹ They also argue against the possibility that the restrictions lower pay by adversely affecting how much refugees can work throughout their careers, thus preventing on-the-job learning and skill accumulation. Finally, the results are also inconsistent with the hypothesis that the restrictions cause more productive and, thus, better-paid workers to lose their jobs. Controlling for job and worker characteristics would reduce the wage effect if its cause were a change in the composition of workers. Another argument against composition effects is the fact that the estimated effects on monthly earnings do not change statistically significantly if we focus on within-person variation (column 4 of Table 2) only.

Another productivity-based explanation of the observed wage effects is that 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, which in our sample are never restricted, if engineering-related sectors are barred. The skill mismatch hypothesis implies that in more restrictive cantons, restaurants would have to hire more poorly matched refugee workers. However, it is not clear why restaurants would be inclined to hire poorly matched 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 unrestricted sectors and regions. If anything, we find evidence for the opposite (see

¹⁸Sectoral restrictions are a possible exception. They may increase refugees' labor supply in industries where refugees are forced to work. Column 6 of Appendix Table D.2 shows the wage effects of sector and region restrictions in the SSES data if we drop sector-canton cells in which refugees must work if sector restrictions apply. Although sector restrictions only apply to asylum seekers (N permit) in the 2012-2016 period, we also drop refugees with TAR and TAF status working in these cells to ensure that the results are not driven by spillovers among status groups. The wage effects are very similar if we drop these observations, suggesting that positive supply shocks in non-restricted sectors do not explain our wage results.

¹⁹Appendix FigureB.1 corroborates this finding for sector restrictions by showing that the wage distribution of refugees across all sectors is similar to the wage distribution if we only consider sectors that refugees are typically forced to work if sector restrictions apply.

Appendix Section D.3 for a discussion).

Contrasting with our expectations, we find limited evidence that productivity effects contribute to explaining the negative wage effects of labor market restrictions. These results imply that the restrictions cause pay differences among equally productive workers.

5.3.3 The role of outside options

Another plausible explanation for the negative wage effects is that sector and region restrictions and prioritization constrain workers' outside options. Leading models of imperfect labor markets predict that poor outside options reduce a worker's wage. By reducing outside options, restrictions could explain the wage differentials between equally productive workers documented above.

Effects on outside options. To investigate the extent to which outside options can explain the wage penalty of labor market restrictions, we first discuss whether restrictions have a measurable impact on outside options. One implication of fewer outside options is a lower probability of finding a job. Panel A of Table 1 shows that granting priority to residents lowers refugees' employment, consistent with a reduction in outside options. Although we estimate that sector and region restrictions remove a sizeable share of refugees' potential jobs (see Figure 3), the evidence that these restrictions lower employment is somewhat less clear-cut: we find only marginally significant negative employment effects in our baseline specification (Table 1, panel A).

To probe further whether sector and region restrictions reduce refugees' outside options, we now analyze the impact of the restrictions on refugees' job-to-job mobility. If the restrictions reduce workers' outside options, we expect a reduction in the arrival rate of outside job offers and thus (voluntary) job changes. Using the monthly social security data, Table 4 presents regressions based on our baseline specification in (1) where the dependent variables are indicators of job separation and job mobility. Column 1 uses all separations. Columns 2 and 3 distinguish separations into employment and non-employment. The latter is coded as one if the worker found a new job within 2 months. Finally, column 4 considers the closest proxy of job-to-job mobility we can observe in the data: separations into employment without an 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.²⁰ 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.

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. According to column 4, an increase in the share of restricted jobs by ten p.p. reduces monthly job-to-job transition rates by 0.19–0.24 p.p., depending on the panel, or by 6.6–8.4% relative to the

²⁰Employer-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.

mean transition rate. Enforcement of the priority rule reduces refugees’ job-to-job mobility even by 0.48–0.75 p.p. We do not find statistically significant effects on separations into non-employment (column 2), suggesting that the introduction of more restrictive policies do not induce firms to lay off refugees that they already employ. Instead, the negative employment effects of the restrictions primarily stem from lower entry into employment. Since separations to non-employment represent 70% of separations, we do not find a significant effect on separations overall (column 1).

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 $\Delta w > 0$	Job-to-job $\Delta w < 0$	On-the-job $\Delta w > 0$	On-the-job $\Delta w < 0$
<i>Panel A. Canton fixed effects</i>								
Priority	-0.0038 (0.0035)	0.0014 (0.0032)	-0.0052** (0.0024)	-0.0048** (0.0019)	-0.0026** (0.0011)	-0.0022 (0.0013)	-0.0350*** (0.0117)	0.0258** (0.0104)
Share restricted jobs	-0.0177 (0.0108)	0.0050 (0.0096)	-0.0227** (0.0092)	-0.0189*** (0.0071)	-0.0094*** (0.0033)	-0.0093** (0.0043)	-0.0035 (0.0295)	0.0023 (0.0289)
<i>Panel B. Individual fixed effects</i>								
Priority	-0.0016 (0.0055)	0.0052 (0.0050)	-0.0067* (0.0040)	-0.0075** (0.0037)	-0.0043* (0.0022)	-0.0033 (0.0021)	-0.0476** (0.0222)	0.0457** (0.0217)
Share restricted jobs	-0.0392* (0.0211)	-0.0138 (0.0155)	-0.0253** (0.0111)	-0.0241** (0.0093)	-0.0089 (0.0059)	-0.0148*** (0.0047)	0.0141 (0.0707)	0.0072 (0.0702)
Outcome mean	0.1105	0.0772	0.0333	0.0286	0.0153	0.0130	0.7250	0.2455
Num. individuals	22,805	22,805	22,805	22,805	22,805	22,805	10,592	10,592
Observations	399,463	399,463	399,463	399,463	399,463	399,463	19,551	19,551

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 baseline controls, fixed effects for month, and month-since-arrival times status group. Panel A also includes additional controls and canton fixed effects. Panel B controls for individual fixed effects. 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 non-employment, zero otherwise. Column 3 uses an indicator equal to one if the worker separates from the main job in 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 job-to-job indicator from column 4 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 through 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 who 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. Standard errors are clustered at the canton \times transition group level. Signif. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

How outside options affect wages. Taken together, we find that region and sector restrictions and prioritization reduce refugees’ outside options. We now explore the explanatory power of three specific 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. 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 based on 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. To illuminate the first mechanism, we analyze the relationship between the restrictions and the firm’s labor supply elasticity. We follow a large recent literature, summarized in Sokolova and Sorensen (2021), and estimate quit elasticities of refugee workers, separately for refugees in restrictive and less restrictive cantons. In a 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 indicate whether a refugee earns relatively more or less, given their 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 column 4 of Table 4) on the group dummies. The key question is how much job-to-job mobility increases when a firm’s pay is low and whether this reaction relates to the restrictions. While the implied quit elasticities do not have a causal interpretation since we do not have exogenous variation in the wage at the firm level, our interest lies less in the level of the elasticity as such but in the comparison by policy environment. If the estimates of the elasticities are similarly biased across policy environments, then relative comparisons of estimates are valid even if estimates of the level are not (see Langella and Manning, 2021, for a similar argument).

Figure 5, 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 2’000. The figure shows that refugees who are low-paid, given their characteristics, have an approximately 20-30% higher quit rate than the base group. Conversely, comparatively high-paid refugees, shown on the right of the figure, have an approximately 20-30% lower quit rate. 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 -0.35 (0.089). This estimate is close to the main separation elasticities reported by Langella and Manning (2021) for US (-0.31) and UK (-0.37) data.

Figures 5(b)–(d) estimate separate regressions for cantons where the priority rule is enforced or not (subfigure (b)), with or without region restrictions (subfigure (c)), and with or without sector restrictions (subfigure (d)). The evidence supports the hypothesis that the policies reduce the quit

elasticity: the estimated wage elasticities are smaller if the policies apply. For instance, the implied wage elasticity of refugees' quits is 2.6 times higher in cantons that enforce priority compared to cantons that do not. When computing the elasticities, we omit the two groups at the tails to compute the fitted regression line. Note, however, that differences in slopes by policy environment would even be larger with 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.

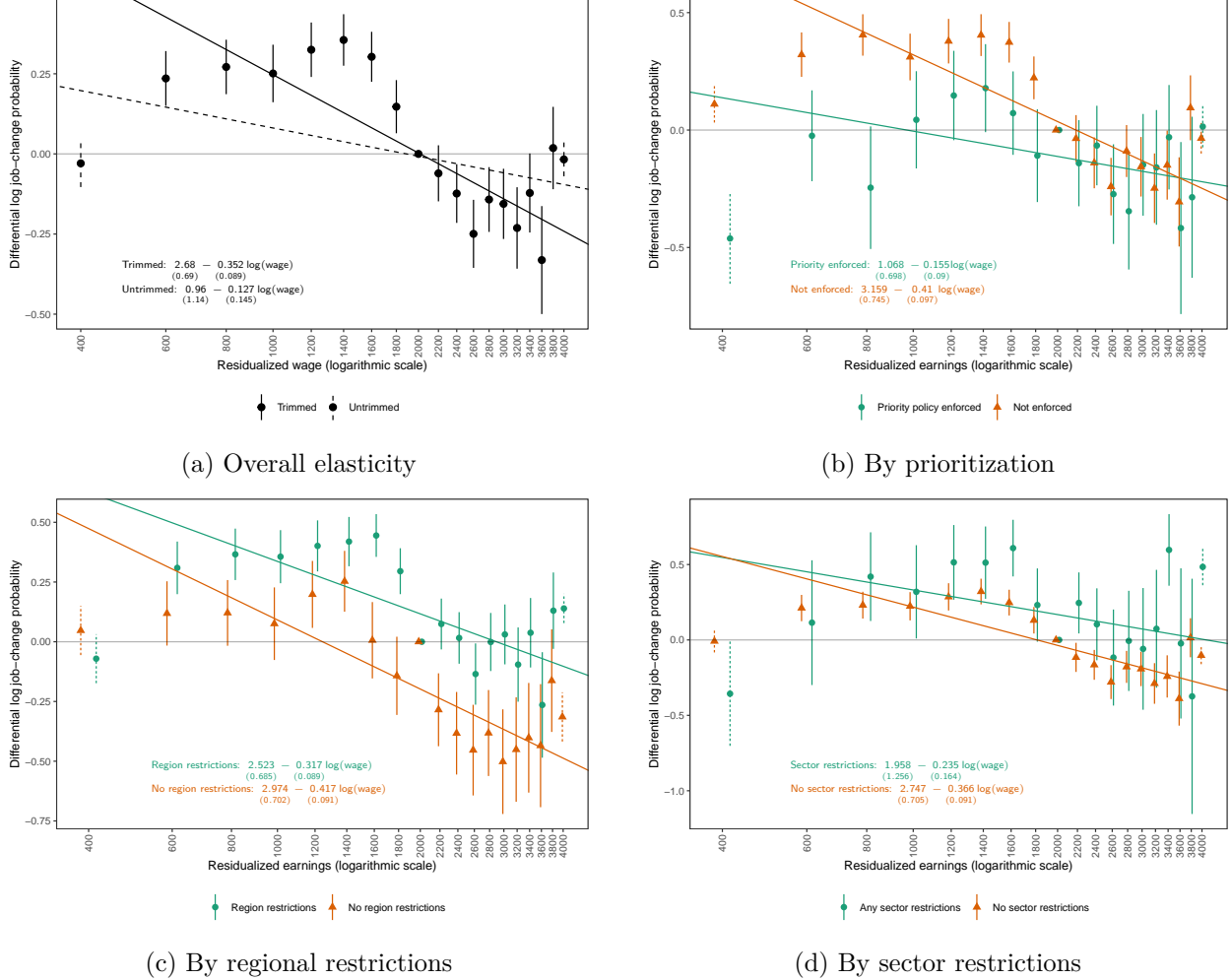
Between-job wage growth. The second mechanism through which fewer outside options reduce pay is that fewer outside options make it more difficult for non-employed refugees to find a well-paid job. This could be reflected in refugees' inability to climb the wage ladder. To investigate this, Table 4 estimates 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 transitions to jobs with a higher and lower monthly income. Hence, refugees' pay penalty in restrictive cantons does not seem to arise because of the greater difficulty of climbing the wage ladder between jobs. Instead, our evidence suggests that the wage effect happens primarily when non-employed refugees start a new job after a phase of non-employment.

On-the-job wage growth. To examine the third 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 who 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.²¹ The analysis reveals that enforcing priority reduces refugees' chances of experiencing an increase (panel A, column 7) and increases the chances of a decrease (panel A, column 8) in monthly earnings in an ongoing employment relationship. While priority to locals appears to reduce on-the-job wage growth, the share of restricted jobs has no impact on this outcome. The evidence that a weaker bargaining position in wage renegotiations contributes to the wage effects of restrictions is thus mixed.

Summary. Taken together, the preponderance of the evidence in this section supports the view that restrictions decrease refugees' hourly pay because they constrain their outside options. While the wage effect of sector and region restrictions may operate through increased firm wage-setting power, enforcing priority also reduces on-the-job wage growth, consistent with models where fewer outside options hamper workers' bargaining position in wage renegotiations.

²¹For 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 5: Labor market restrictions and the wage elasticity of job-to-job transitions.



Notes: The figures 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 month, canton, and month-since-arrival \times status FE and baseline plus 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 4'000 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 to fit the regression lines, 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 firm-level wages.

6 Long-run effects

In the past sections, we focused on the contemporaneous effects of the policies. We now turn to their potential longer-term effects beyond they cease applying. 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., Aslund and Rooth, 2007; Müller, Pannatier, and Viarengo, 2023; Von Wachter, 2020) 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 in the longer run, further contributing to the gap in labor market outcomes between refugees and comparable citizens.

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 over the first 12 months after arrival, measured as the share of months with enforcement of prioritization, the 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 in 2015. The empirical specification reads as follows:

$$Y_{icdt} = a_\tau + b'_\tau P_{cdT} + f_\tau \bar{u}_{cT} + \pi' w_i + \psi_d + \mu_c + \delta_t + \nu_{icdt} \quad (4)$$

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_{cdT} measures initial policy exposure as defined above, which depends on the time of arrival $T(i)$ and the assigned canton $c(i)$. 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 2). The vector b_τ captures deviations from this integration path due to initial policy exposure. We benchmark the policy effects against the scarring effects from initial local labor market conditions by including the initial unemployment rate averaged over the first 12 months after arrival, \bar{u}_{cT} . The vector f_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 and age-squared interacted with gender.

The base specification with transition group, canton, and outcome year fixed effects (labeled as “additive” specification) identifies the average change in labor market outcomes due to initial restrictions, including the usual evolution of restrictions experienced afterward. The reason is that the initial policy environment is likely correlated with the policy environment after a refugee’s entry (see, e.g. Oreopoulos, Von Wachter, and Heisz, 2012). We also present a fully interacted specification with canton \times outcome year \times transition group fixed effects, which absorb the effects

of the later policy environment and local economic conditions (labeled as “multiplicative”). This specification thus measures the effect of the initial restrictions *net* of effects from policy exposure in subsequent years. This net effect can also be illustrated by evaluating long-term effects only for refugees transiting from permit N to B. Since they face no restrictions after they receive refugee status recognition, there are no contemporaneous employment restrictions whose effects could add to the effects of the initial policy environment.

6.2 Results

Figure 6 plots the effects of the labor market policies on employment and earnings in each year after arrival using our base specification in (4) (“All additive”), the specification with the canton-year \times transition group interactions (“All multiplicative”), and the base specification for the transition group N \rightarrow B only (“N \rightarrow B additive”).²²

Focusing on the base specification, we observe the following long-run policy effects. First, enforcing prioritization in all instead of the zero months of the arrival year reduces employment by around 17 p.p. in the first two years after arrival. This represents a sizable 94.6% or 51.4% reduction of average employment in years one and two, respectively. In these two years, Figure 6(b) also shows a strong decline in earnings of around 67.2% and 52.7%, respectively. The long-run effects on employment and total earnings slowly fade and become insignificant after years 4-5.

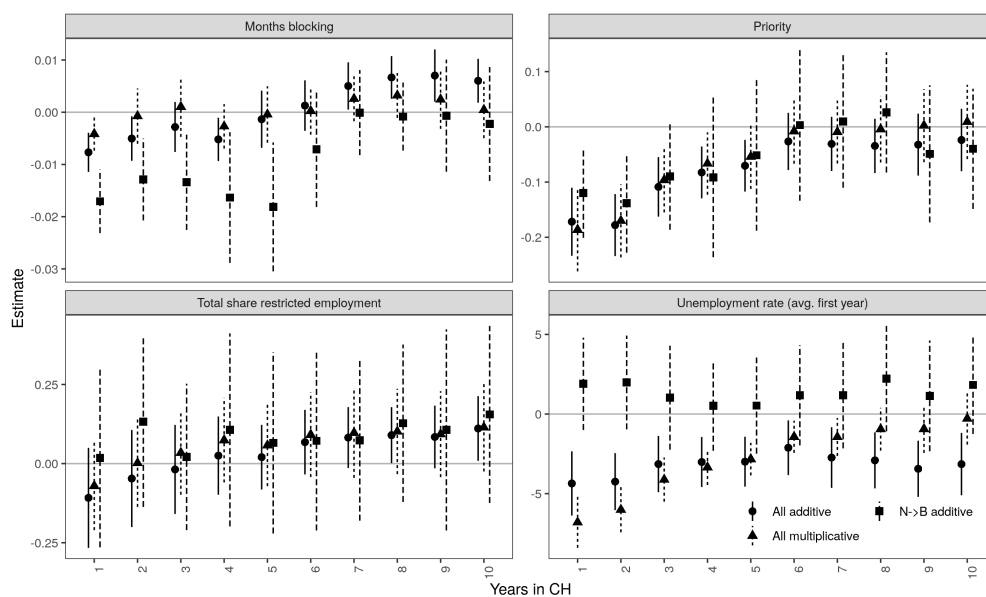
Second, the number of months banned from working has the largest effect on employment in the first year after arrival (when the ban is effective). A one-year employment ban lowers employment by 9.6 p.p. (12×0.8 p.p.) in the first year and about half of that magnitude in years 2–4. The effect on earnings is also concentrated in the arrival year (approximately a 10.5% reduction per month blocking or 71.6% for a twelve-month ban duration) and zero after that. The dynamics of these effects parallel those documented by Fasani, Frattini, and Minale, 2021. In Appendix E.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 years 7–9. In addition, the Appendix section shows 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 significantly reduces earnings in the first three years, whereas the effects on employment are sizeable but statistically insignificant. In the first year, removing a third of available jobs reduces earnings by roughly 31.1%. In years two and three, the point estimates for the effect on earnings are still negative and significant, but their magnitude reduces by about half yearly. The effects fade out from year four onward.

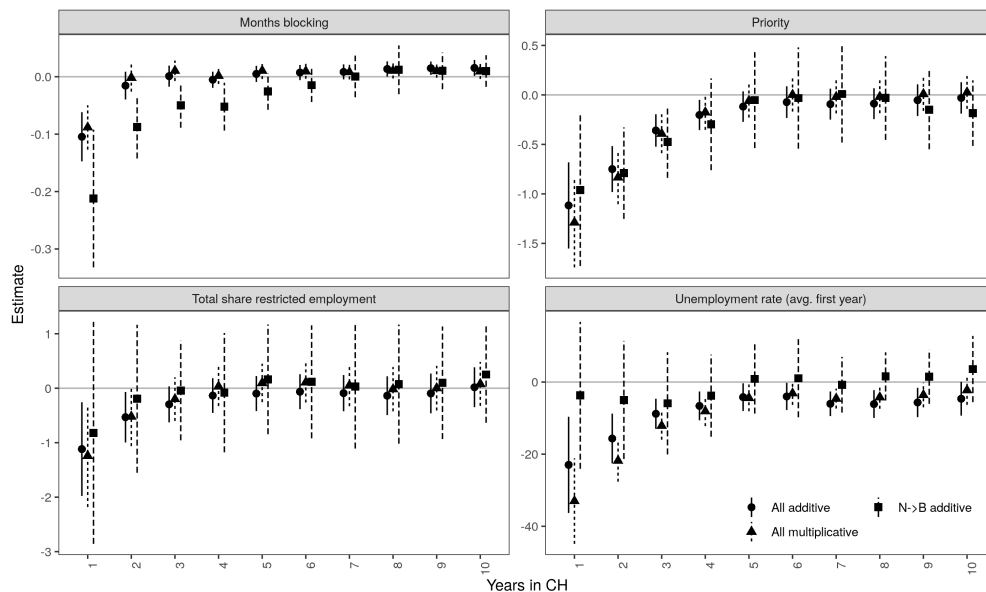
The estimates showing the *net impact* of the initial policy environment on later outcomes based on the “multiplicative” specification in Figure 6 generally follow a similar dynamic and have

²²Note 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.

Figure 6: 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 estimator)

Notes: The figures show the effects of the first-year labor market restrictions and the first-year average cantonal unemployment rate on employment (Figure (a)) and total earnings (Figure (b)) of refugees in years 1–10 after their arrival in Switzerland based on the specification (4). The specifications include transition group, canton, and year fixed effects (labeled with “All additive”) or fixed effects for canton \times year \times (“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 labeled “N→B additive” only show effects for refugees transitioning to a B permit based on specifications with canton and year fixed effects. We control for age and age-squared interacted with gender. The estimations in Figure (b) use the Poisson fixed effects estimator.

mostly similar effect sizes in the first years after arrival as those from the “additive” specification. 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 (“N→B additive”). 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, in the case of employment, statistically significantly negative five years after arrival (or four years after they cease applying). Also, the effects of prioritization on earnings and employment are slightly larger in this specification, with negative estimates until the fifth year after arrival.

In sum, our analysis reveals sizeable detrimental impacts of the restrictions on refugees’ employment and earnings during their initial year after arrival, aligning with the evidence presented in Table 1. Moreover, the specifications that uncover the policies’ net effects on subsequent outcomes suggest that an employment ban and prioritization in the first year leave detectable scars for some groups up to five years after arrival. Benchmarking these results against unfavorable economic conditions suggests that the medium-run effects of the policies studied here are at least as, or even more harmful, than adverse economic conditions.²³

7 Potential benefits of restrictions

Lastly, we explore two potential motivations why policymakers might put in place policies that restrict access to the labor market for refugees: potential positive spillovers for similar workers who compete with restricted refugees for jobs and encouraging the emigration of refugees.

7.1 Spillover effects on other immigrants

One argument for implementing labor market restrictions for refugees put forward by policymakers is shielding other vulnerable groups from the competition of refugees. Clemens, Lewis, and Postel (2018) point out that there has been relatively little research on the effectiveness of restrictive policies that aim to improve resident workers’ labor market outcomes. We focus on other immigrants to investigate whether restrictive policies have positive spillover effects on competing workers. Table 3 shows large unconditional wage gaps between refugees and Swiss workers, suggesting that the average Swiss worker is unlikely to compete with refugees.

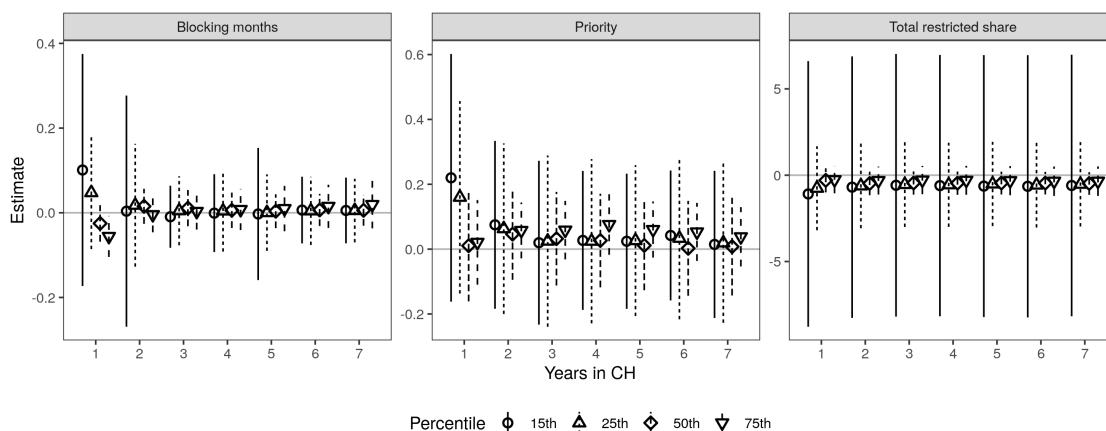
First, we investigate the effects on refugees receiving asylum protection (i.e., B permit holders). These refugees arguably compete for similar jobs as refugees receiving subsidiary protection and thus constitute the group that should benefit the most when other refugees are not allowed to work. To test for spillovers, we estimate the baseline specification (Table 1, column (3)) for the

²³Weak local economic performance can also severely reduce the number of available job options, particularly for vulnerable populations (Von Wachter, 2020; Aslund and Rooth, 2007). We proxy economic conditions with the cantonal unemployment rate during an individual’s first year in Switzerland. Figure 6 shows that an average increase in the local unemployment rate of +1.2 p.p. (comparable to the Great Recession) in the arrival year lowers employment by about 5.2 p.p in the same year and earnings by 31.7%. These results confirm the findings of Müller, Pannatier, and Viarengo (2023), which also find a strong impact of the cantonal unemployment rate at arrival on the employment rate of refugees in Switzerland in later years.

sample of B permit holders augmented with the three TAF policies—employment ban, enforcement of prioritization, and the restricted share. To measure the intensity of the employment ban, we take the share of TAF refugees banned in a canton in a specific month. Appendix Table C.9 shows that none of the three restrictions for TAF has beneficial effects on the employment or earnings of B permit refugees. The estimated effects of the TAF policies are small and not statistically significant, while effects from the policies for the own group are similar to Table 1.

Second, we analyze spillover effects of restrictive policies on the earnings of low- and high-paid immigrants from EU-15 countries. We apply unconditional quantile regressions based on the long-run model in equation (4) to the linked AHV-STATPOP data.²⁴ This approach allows us to interpret potential effects in the lower part of the wage distribution as evidence for spillover effects to EU-15 immigrants competing for similar jobs. Effects in the upper quantiles can be interpreted as placebo tests since highly paid EU immigrants are unlikely to compete directly with refugees. Any policy effects on the latter group would thus raise concerns about bias due to unobserved time-varying confounders. Figure 7 shows no evidence for significant positive spillover, nor placebo, effects from the three policies on earnings for EU-15 migrants. This pattern holds across all percentiles considered, including the 15th percentile, which corresponds to low-paid EU-15 migrants who are most likely to compete with refugees. Except for the lowest wage group, the estimation of these effects is sufficiently precise to rule out all but very small effects.

Figure 7: Spillover and placebo effects on earnings for EU-15 immigrants not subject to restrictions



Notes: The figure reports the restrictions’ effects at different percentiles on total earnings (IHS transformed) of EU-15 immigrants using unconditional quantile regression. The sample includes all EU-15 immigrants arriving in 2005-2008 and who still reside in Switzerland in 2015. The estimation model includes canton and year fixed effects, and we control for the unemployment rate at arrival, and age group interacted with gender.

When interpreted as a placebo test, these null results corroborate that our main estimates of the impact of restrictions are unlikely biased by time-varying confounders correlated with policies.

²⁴Compared to our baseline specification, there are three differences: First, we can only consider the years after 2005 since the canton of residence is not recorded in STATPOP for earlier years. Second, since we do not observe the month of arrival for EU-15 immigrants, we use the policy environment in January. Third, we only observe age groups for EU immigrants rather than the precise age. We thus control for age group interacted with gender.

When interpreted as a test for positive spillover effects, these estimates cast doubts on claims that restrictive policies, while hurting refugees, will benefit other competing workers.

7.2 Effects on emigration

Another argument put forward by policymakers in favor of labor market restrictions is that they might encourage refugees to emigrate, particularly those who hold only a temporary protection status (TAF or TAR permit). Since the timing of emigration is not consistently recorded in the ZEMIS register, we draw on two complementary approaches to measure emigration.

Our first approach leverages the STATPOP register, which allows us to measure emigration directly, but only for 2011–2015. We resort to the long-run specification (4) and estimate the restrictions’ effects on emigration for years 5–10 after arrival in Switzerland. On average, roughly 3.4% of the refugees in the sample leave Switzerland each year. The second approach uses AHV data over the entire sample period and approximates a person’s emigration with an indicator if she does not appear in the register for at least five years. Since there are few exemptions from the obligation to contribute to the old age insurance, this variable captures emigration quite precisely.²⁵

The estimates presented in Appendix Figures E.1 provide little evidence that restricting refugees’ initial labor market access leads to their emigration in the medium- to long-run. With both approaches, we find suggestive evidence that prioritization, if anything, reduces the likelihood of emigration. In the case of employment bans, the evidence is mixed. The AHV data suggests that there may even be an emigration-reducing effect in the short-run. However, both approaches suggest a small increase in emigration after 5–8 years due to the initial employment ban. We conclude that emigration shows limited response to labor restrictions, making these policies potentially very costly for host societies: refugees with reduced labor income will remain in the country, necessitating support through social assistance.

8 Conclusion

This paper analyzes the employment, wage, job mobility, and emigration 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 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 the significant discretion of Swiss cantons in applying these labor market restrictions. Covering 1999–2016, we combine data on changes in cantonal policies with administrative records on refugees’ asylum processes, earnings records, and linked employer-employee data that provide information on hours worked and hourly wages.

²⁵Aside 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.

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 the most affected refugees from 19.2% to 11.5%. We also find that sectoral and geographical restrictions reduce refugees' monthly and hourly wage rate when employed,. We find evidence that policies prioritizing the resident workforce reduce employment, wage growth on the job, job mobility, and monthly wages. We also assess the persistence of these effects. In line with an extensive literature showing 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 reduces refugees' labor market earnings for up to four years after they cease applying. Together, our findings suggest that these labor market policies are an important reason why refugees have lower employment rates and wages than similar citizens. For theory, our results on the wage effects of restrictions suggest an important role for outside options in determining the level of workers' pay.

Turning to potential benefits, we find little evidence that restrictive refugee policies measurably increase the earnings of other immigrant workers who likely compete with refugees for jobs. These results cast doubt on whether labor restrictions help shield other vulnerable groups from competition with refugees. Finally, we find that labor restrictions have limited effects on the probability that refugees emigrate, even for refugees who 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. Switzerland's exogenous dispersal policy prohibits us from analyzing whether cantons with more restrictive policies would attract fewer refugees if they 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 context, the overwhelming 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 Switzerland. First, the composition of Switzerland's 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. Nevertheless, given the many cross-country differences that may moderate the impact of restrictive labor market policies, more research from other contexts is needed to understand the policies' impacts on immigrants' economic and social integration.

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Online Appendix

The Labor Market Effects of Restricting Refugees' Employment Opportunities

A Descriptives

A.1 Policies

Table A.1 assesses compliance with the labor and mobility restrictions. First, Panel A and B of the table use employment status from the linked AHV and ZEMIS databases to check for compliance with the initial employment ban. We find that only 0.2% (AHV) and 0.8% (ZEMIS) of refugees are recorded as employed during months in which they are subject to a ban according to our coding (compared to 13.3% and 18.8%, respectively, for individuals that are not subject to a ban). Second, in Panel C, we find that about 5.7% of employed refugees in cantons that do not allow for extra-cantonal commuters are, in fact, cross-cantonal commuters, compared to 15.4% 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 in Panel D. Note that if cantons decide to implement sector restrictions, they define a few exempt sectors where refugees are 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 21.2% of refugees are employed in ‘always restricted’ sectors in canton-months when access to these sectors is indeed restricted. This share increases to 33.8% 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 a sector 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 12.7 p.p. higher (a more than 50% increase) if sector restrictions are lifted. Panel E, which only focuses on newly employed, confirms these results.

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. SSES data (October 2016)</i>						
Hourly wage	24.50	9.03	8.52	23.35	55.93	1530
Monthly labor income	3232.86	1514.71	133.00	3689.85	6273.76	1530
Full-time equivalents	0.75	0.32	0.04	1.00	1.00	1530
Monthly hours worked	136.51	58.81	5.00	168.50	199.33	1530
Female	0.28	0.45	0.00	0.00	1.00	1530
Age	34.61	7.82	20.00	34.00	55.00	1530
Primary education	0.81	0.39	0.00	1.00	1.00	1386
Tertiary education	0.02	0.14	0.00	0.00	1.00	1386
Tenure (in years)	1.23	1.76	0.00	1.00	7.00	1530
Hospitality sector	0.26	0.44	0.00	0.00	1.00	1530
Trade sector	0.08	0.27	0.00	0.00	1.00	1530
Construction sector	0.01	0.10	0.00	0.00	1.00	1530

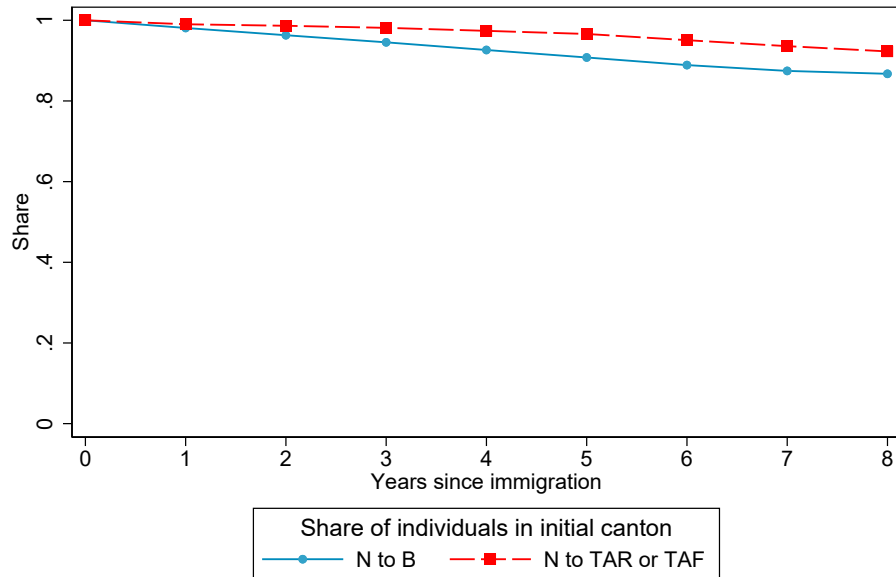
Notes: Panel A and B show summary statistics for the monthly merged AHV-ZEMIS data set covering 1999-2015. This data set includes the first 5 years after arrival and only asylum seekers or refugees who receive either subsidiary protection (TAF/TAR) or are recognized as refugees (B permit). Panel C shows summary statistics for all refugees in transition groups N→B, N→TAF and N→TAR in the 2016 Swiss earnings structure surveys, conducted in October.

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	23842.19	19094.66	173.12	20106.86	68050.91	5697
Employed (AHV)	3.49	4.88	0.00	0.00	12.00	13952
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	18422.65	17139.21	216.70	13062.79	69191.33	16818
Employed (AHV)	2.65	3.60	0.00	0.00	12.00	34687
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 still in Switzerland in 2015. Panel A provides cross-section 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.

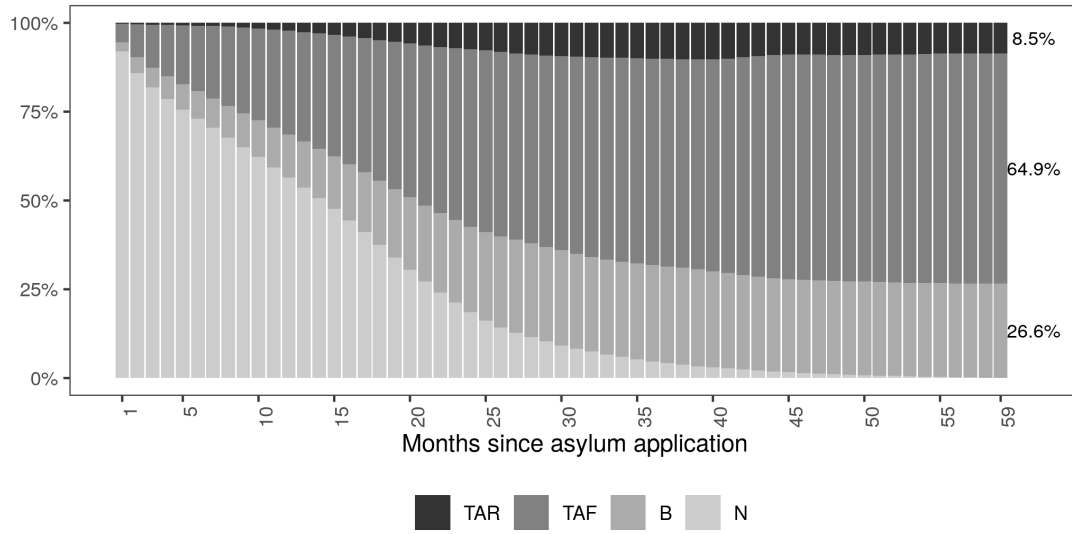
A.3 Between-canton mobility



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 (i.e., transition group $N \rightarrow B$) and for refugees that were temporally admitted (transition groups $N \rightarrow TAF$ and $N \rightarrow TAR$). We focus on refugees who have arrived in Switzerland in the years 2005–2010.

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

A.4 Asylum status over months since asylum application



Notes: The figure shows the share of refugees by permit status over months since asylum application over 1999–2015. The data basis is the merged ZEMIS-AHV data. We only include individuals that receive either subsidiary protection (TAF/TAR) or are recognized as refugees (B permit). Asylum seekers (i.e., individuals who have not yet received their asylum decision) hold an N permit.

Figure A.2: Asylum status over months since asylum application

B Measures of cantonal and sectoral labor market restrictions

This section explains how we construct the measure that summarizes 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

$$share\ region\text{-}restricted\ jobs_{its} = \sum_{j \neq i} commuter\text{-}share_{i \rightarrow j} \times region\text{-}restricted_{jts} \quad (5)$$

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) likely 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

$$share\ sector\text{-}restricted\ jobs_{its} = \sum_{\ell} sector\text{-}share_{\ell} \times sector\text{-}policy_{its\ell} \quad (6)$$

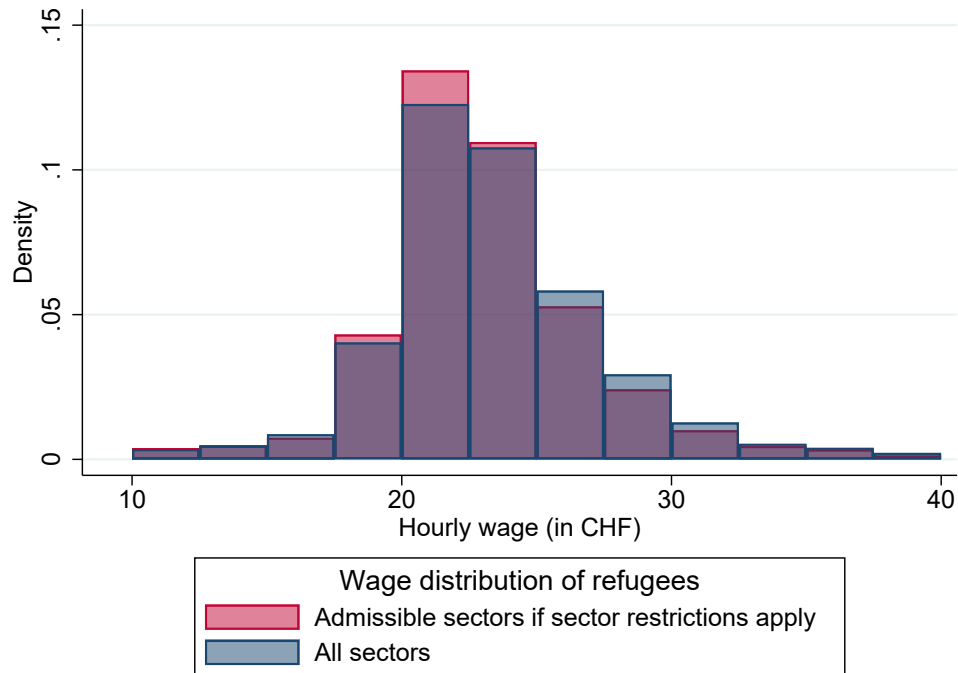
where $sector\text{-}policy_{its\ell}$ is 1 if refugees of status s residing in canton i are banned from working in sector ℓ in the same canton in month t , 0 otherwise. $sector\text{-}share_{\ell}$ is the share of refugees working in sector ℓ . To avoid that the sector shares are distorted by sector 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,

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



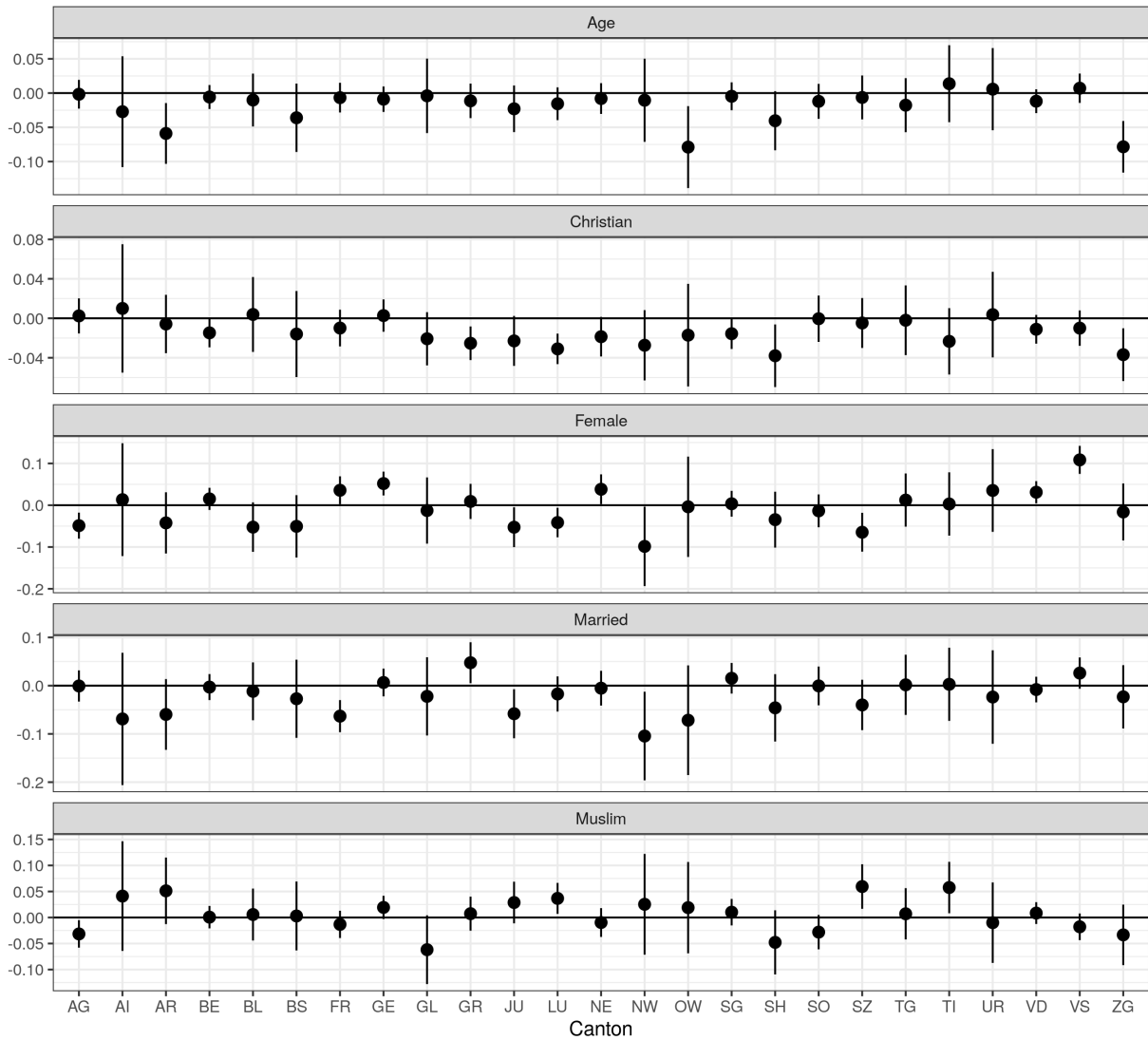
Notes: This figure shows the hourly wage distribution of refugees across all sectors (blue) and in the sectors in which they are typically forced to work if sector restrictions apply (red). The data is from the Swiss Earnings Structure Surveys in 2012, 2014, and 2016. The sample is all refugees with TAR, TAF or B permit that do not face any sector restrictions. The blue histogram shows the unconditional wage distribution. The red histogram focuses on the two-digit industries (NACE rev. 2) to which refugees are restricted at least once. 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 restricted when sectoral restrictions apply.

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

C Further short-run results

C.1 Exogenous allocation check

We 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 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 C.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).



Notes: The figure shows point estimates and associated 95% confidence intervals of OLS regressions of asylum seeker characteristics on indicators for assigned cantons. Since the 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 C.1: Balance tests of various refugee characteristics measured at arrival across assigned cantons

C.2 Exogenous policy check

Table C.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)	-0.0074 (0.0201)	0.0709 (0.0730)	-0.0867 (0.0900)	-0.0621 (0.1858)	-0.0068 (0.0121)
Overall unemployment previous year	-0.1408 (0.1661)	0.1630 (0.1642)	-0.1146 (0.1093)	0.0150 (0.0931)	0.3777 (0.3393)	-0.0209 (0.0260)
Overall unemployment three years ago	0.0878 (0.1324)	-0.1059 (0.1071)	-0.0041 (0.1043)	0.1086 (0.0918)	0.0924 (0.3058)	0.0852 (0.0824)
Refugee unemployment last year	0.0005 (0.0019)	0.0008 (0.0007)	-0.0135* (0.0078)	0.0088* (0.0047)	-0.0191 (0.0317)	-0.0004 (0.0004)
Refugee unemployment previous year	0.0068* (0.0037)	-0.0015 (0.0014)	-0.0162* (0.0086)	0.0014 (0.0036)	0.0195 (0.0236)	-0.0004 (0.0005)
Refugee unemployment three years ago	-0.0023 (0.0029)	0.0004 (0.0004)	0.0194** (0.0084)	-0.0104*** (0.0034)	-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	16,633	16,622	16,684	16,584	4,560	12,164
Joint F -test (p -value)	0.468	0.967	0.002	0.006	0.36	0.967

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, and are set to zero if there is no change. 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.

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

C.3 Further employment and earnings results

Table C.2: Effect of labor market policies on employment with logistic regression

	(1)	(2)	(3)	(4)
Employment ban	-0.133*** (0.018)	-0.132*** (0.022)	-0.115*** (0.032)	-0.117*** (0.024)
Priority enforced	-0.053*** (0.010)	-0.049*** (0.010)	-0.058*** (0.015)	-0.057*** (0.012)
Share restricted jobs	-0.057* (0.029)	-0.058* (0.034)	-0.068 (0.042)	-0.078* (0.046)
Estimator	OLS	OLS	Logit	Logit
Sample	All	All	All	All
Canton FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Month-since-arrival x status FE	Yes	Yes	Yes	Yes
Additional controls		Yes		Yes
Observations	2770320	2770320	2770155	2770155
R ²	0.176	0.150	0.226	0.194

Notes: Columns (1)-(2) use linear regression, while columns (3)-(4) use logistic regression. Columns (1)-(2) in this table reproduce columns (4)-(5) in Table 1, Panel A. The coefficients reported in the last two columns are average marginal effects. See notes in Table 1 for more information. Signif. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table C.3: Effect of labor market policies on employment (ZEMIS data)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Employment indicator (ZEMIS).</i>							
Employment ban	-0.1108*** (0.0278)	-0.2173*** (0.0328)	-0.0853*** (0.0293)	-0.1096*** (0.0209)	-0.1085*** (0.0237)	-0.0762*** (0.0184)	-0.1132*** (0.0230)
Priority enforced	-0.0573*** (0.0083)	-0.0608** (0.0264)	-0.0757*** (0.0213)	-0.0547*** (0.0095)	-0.0538*** (0.0103)	-0.0297*** (0.0096)	-0.0597** (0.0229)
Share restricted jobs	-0.0575 (0.0373)	-0.0341 (0.0302)	0.0295 (0.0447)	-0.0225 (0.0294)	-0.0179 (0.0312)	-0.0126 (0.0200)	-0.0800 (0.0589)
Outcome mean	0.1351	0.1308	0.1043	0.1260	0.1259	0.1259	0.1619
Num. individuals	42,873	6,792	21,160	70,825	70,977	70,977	38,198
Observations	1,989,501	300,258	904,325	3,194,084	3,196,539	3,196,539	1,414,710
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
Months-since-arrival FE x status	Yes	Yes	Yes	Yes	Yes	Yes	
Additional controls	Yes	Yes	Yes	Yes	No	No	Yes

Notes: The table shows the effect of the labor restrictions on employment. The table mirrors Table 1, Panel A, but uses the employment outcome recorded in ZEMIS. Signif. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table C.4: 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	-0.1343*** (0.0164)	-0.0807*** (0.0092)	-0.1317*** (0.0166)	-0.0779*** (0.0108)
Priority enforced	-0.0526*** (0.0099)	-0.0220** (0.0084)	-0.0419*** (0.0104)	-0.0175** (0.0086)
Share sector restricted jobs	-0.0395 (0.0279)	-0.0206 (0.0176)	-0.0572 (0.0347)	-0.0420** (0.0187)
Share region restricted jobs	-0.1431** (0.0602)	-0.2051*** (0.0388)	-0.1506*** (0.0489)	-0.2695*** (0.0409)
Employment ban × Low reliability			-0.0111 (0.0107)	-0.0174** (0.0075)
Priority enforced × Low reliability			-0.0150 (0.0115)	-0.0011 (0.0094)
Share sector restricted jobs × Low reliab.			0.0875** (0.0436)	0.0582 (0.0453)
Share region restricted jobs × Low reliability			0.1993 (0.1237)	0.3100*** (0.0735)
Sample	All	All	All	All
Months-since-arrival x Status FE	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes
Canton FE	Yes		Yes	
Month FE	Yes	Yes	Yes	Yes
Individual FE		Yes		Yes
Observations	2,770,320	2,772,775	2,770,320	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). 43.1% of the sample were classified as highly reliable and 15.6% 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 C.5 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. Signif. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

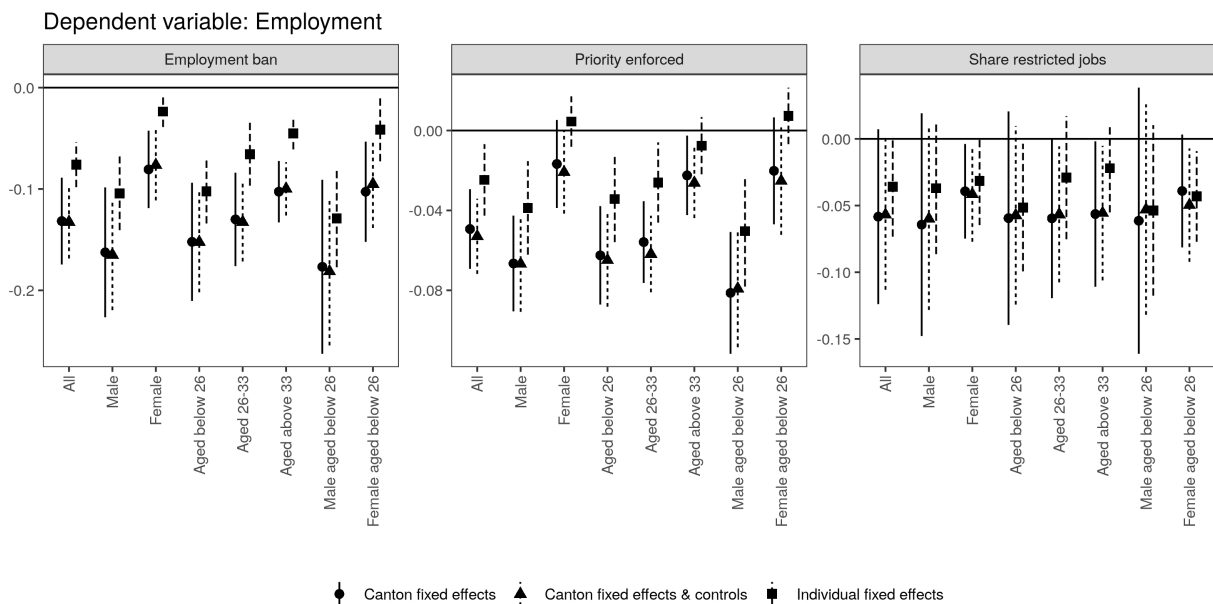
Table C.5: Effect of labor market policies on employment and total earnings with separate effects for job restrictions by region and sector restrictions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A. Employment</i>							
Employment ban	-0.1134*** (0.0244)	-0.2291*** (0.0352)	-0.1575*** (0.0257)	-0.1343*** (0.0164)	-0.1320*** (0.0199)	-0.0807*** (0.0092)	-0.0791*** (0.0264)
Priority enforced	-0.0539*** (0.0114)	-0.0544** (0.0260)	-0.0569*** (0.0171)	-0.0526*** (0.0099)	-0.0493*** (0.0106)	-0.0220** (0.0084)	-0.0489* (0.0260)
Share sector restricted jobs	-0.0477 (0.0394)	-0.0134 (0.0238)	-0.0183 (0.0268)	-0.0395 (0.0279)	-0.0444 (0.0326)	-0.0206 (0.0176)	-0.0660 (0.0506)
Share region restricted jobs	-0.0684 (0.0646)	-0.2680*** (0.0744)	-0.3022*** (0.0885)	-0.1431** (0.0602)	-0.1064 (0.0679)	-0.2051*** (0.0388)	0.9109** (0.4102)
Outcome mean	0.1894	0.1438	0.1452	0.1732	0.1732	0.1732	0.2293
Num. individuals	41,075	6,494	20,059	67,628	67,780	67,780	33,941
Observations	1,764,732	246,365	759,223	2,770,320	2,772,775	2,772,775	1,263,386
<i>Panel B. Total earnings (Poisson estimator)</i>							
Employment ban	-1.106*** (0.1829)	-2.548* (1.442)	-1.584*** (0.3307)	-1.246*** (0.1284)	-1.258*** (0.1452)	-1.629*** (0.1573)	-0.7427*** (0.1496)
Priority enforced	-0.3906*** (0.0640)	-0.6674*** (0.1762)	-0.9154*** (0.1735)	-0.4383*** (0.0601)	-0.4503*** (0.0589)	-0.3433*** (0.0647)	-0.2083* (0.1081)
Share sector restricted jobs	-0.4360** (0.2042)	0.4589 (0.4828)	0.0356 (0.3648)	-0.3314* (0.1902)	-0.4448** (0.2269)	-0.3695** (0.1537)	-0.2951 (0.2232)
Share region restricted jobs	-1.143* (0.6819)	-1.044 (1.365)	-1.777 (1.154)	-1.425** (0.5947)	-1.428** (0.6047)	-1.853*** (0.5664)	3.557** (1.769)
Outcome mean (CHF)	505.7	365.8	328.0	444.3	444.3	950.9	621.6
Num. individuals	41,075	6,494	20,059	67,628	67,780	23,225	33,941
Observations	1,763,557	246,047	759,222	2,770,155	2,772,610	1,295,608	1,263,366
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
Months-since-arrival FE x status	Yes	Yes	Yes	Yes	Yes	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. Signif. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

C.4 Heterogeneity and construction of employment score

We investigate the heterogeneity of the policy effects on employment in two different ways. First, Figure C.2 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 ($N \rightarrow B$, $N \rightarrow TAF$, and $N \rightarrow TAR$) 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 \times transition group level.

Figure C.2: Heterogeneity in the effect of policies on employment, by age and gender

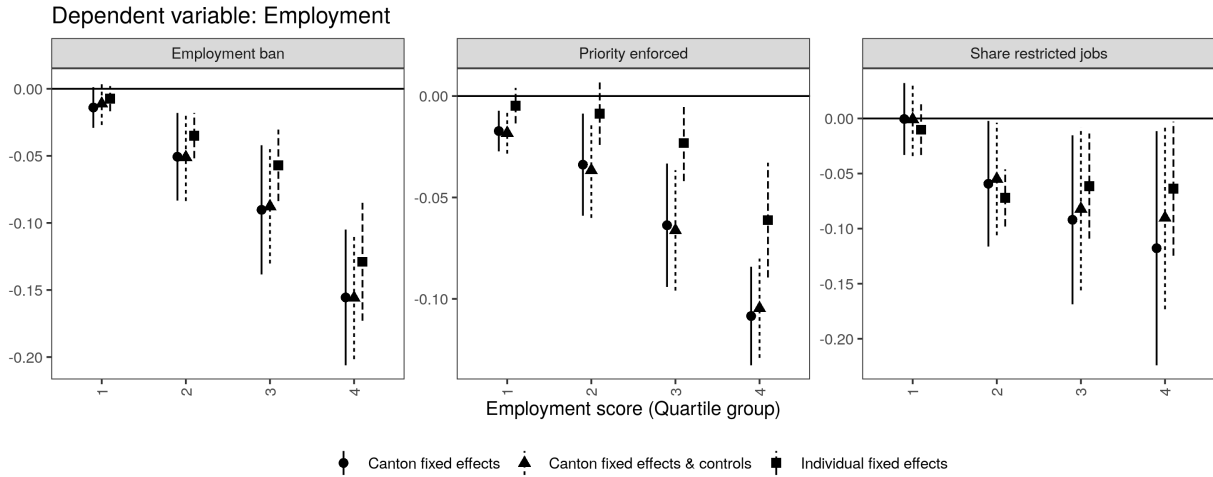
Second, we explore heterogeneity by dividing the refugee sample in four groups from low to high employability. To this end, we predict employment status in the 5th year after asylum application while exclusively relying on individual-level time-invariant characteristics (i.e., age, sex, age at arrival, 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 that estimation errors from the classification stage carry over to the estimation of treatment effects. Table C.6 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.6: 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.3 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 we cannot rule out 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.2 for more information.

Figure C.3: Heterogeneity in the effect of policies on employment, by employment score

C.5 Potential bias from heterogeneity

We further 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, 2023). Generally, 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) which 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 importance 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 aggregating 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 C.7) or for heterogeneity by calendar year (Table C.8). The effect for the restricted share is a bit larger in absolute size but qualitatively similar.

Table C.7: Effect of labor market policies on employment and total earnings

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Employment</i>						
Priority enforced	-0.0563*** (0.0118)	-0.0497* (0.0247)	-0.0585** (0.0213)	-0.0517*** (0.0104)	-0.0480*** (0.0108)	-0.0150* (0.0075)
Share restricted jobs	-0.0990* (0.0534)	-0.0899*** (0.0305)	-0.0590* (0.0312)	-0.0938** (0.0389)	-0.0971** (0.0457)	-0.0640*** (0.0215)
Observations	1,624,259	226,377	694,305	2,544,941	2,547,361	2,547,361
<i>Panel B. Total earnings (Poisson estimator)</i>						
Priority enforced	-0.5629*** (0.1205)	-0.8865*** (0.3236)	-0.9552*** (0.2704)	-0.6726*** (0.1286)	-0.6847*** (0.1312)	-0.4627*** (0.0993)
Share restricted jobs	-0.5522** (0.2730)	-0.1171 (0.9053)	-0.8825 (0.6138)	-0.4714* (0.2717)	-0.5521* (0.2880)	-0.4005*** (0.1475)
Observations	1,624,253	226,309	694,290	2,544,934	2,547,327	1,211,714
Sample	N->TAF	N->TAR	N->B	All	All	All
Canton FE	Yes	Yes	Yes	Yes	Yes	
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE						Yes
Month-since-arrival x status FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-since-arrival FE						
Additional controls	Yes	Yes	Yes	Yes	No	No

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.

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

Table C.8: Effect of labor market policies on employment and total earnings

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Employment</i>						
Priority enforced	-0.0619*** (0.0137)	-0.0422* (0.0218)	-0.0537** (0.0207)	-0.0501*** (0.0108)	-0.0486*** (0.0113)	-0.0305*** (0.0095)
Share restricted jobs	-0.0857* (0.0495)	-0.0695* (0.0358)	-0.0675** (0.0315)	-0.0870** (0.0331)	-0.0870** (0.0386)	-0.0696*** (0.0220)
Observations	1,624,259	226,377	694,305	2,544,941	2,547,361	2,547,361
<i>Panel B. Total earnings (Poisson estimator)</i>						
Priority enforced	-0.4565*** (0.1007)	-1.199*** (0.2394)	-0.9963*** (0.2471)	-0.5977*** (0.0911)	-0.6223*** (0.0927)	-0.4919*** (0.0959)
Share restricted jobs	-0.7745* (0.4482)	0.1577 (0.5531)	-1.151* (0.6566)	-0.8509** (0.3431)	-0.9560** (0.3936)	-0.5655*** (0.1785)
Observations	1,624,253	226,309	694,290	2,544,934	2,547,327	1,211,714
Sample	N->TAF	N->TAR	N->B	All	All	All
Canton FE	Yes	Yes	Yes	Yes	Yes	
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE						Yes
Month-since-arrival x status FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-since-arrival FE						
Additional controls	Yes	Yes	Yes	Yes	No	No

Notes: We interact policies with calendar year fixed effects and report the aggregated estimates. See notes in Table 1 for more information. Signif. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

C.6 Spillover effects from restricted to unrestricted refugees

Table C.9: The effects of restricting TAF refugees on unrestricted B-permit refugees

Dependent Variables:	Employed	Total earnings	Log earnings	Employed	Total earnings	Log earnings
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Employment ban	-0.1423*** (0.0277)	-1.561*** (0.3423)	-0.0977 (0.3993)	-0.0724 (0.2207)	-0.6190 (1.199)	-0.5046 (0.5236)
Priority enforced	-0.0585*** (0.0200)	-0.9466*** (0.2115)	-0.4180* (0.2384)			
Share restricted jobs	-0.0439 (0.0283)	-0.1760 (0.4385)	-0.1335 (0.3813)			
Share currently blocked TAF	-0.0730 (0.0525)	-0.1819 (0.7173)	0.9067 (0.6692)	-0.1128** (0.0529)	-0.5893 (0.5993)	0.2895 (0.5695)
Priority enforced TAF	-0.0178 (0.0258)	-0.2031 (0.1799)	-0.0563 (0.0801)	-0.0066 (0.0267)	-0.1855 (0.1401)	-0.1487* (0.0746)
Share restricted jobs TAF	0.0383 (0.0910)	0.0541 (0.5603)	-0.2174 (0.1852)	0.0601 (0.1170)	0.3308 (0.5633)	0.0989 (0.1372)
Sample	N→B	N→B	N→B	B	B	B
Estimator	OLS	Poisson	OLS	OLS	Poisson	OLS
Canton FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Months-since-arrival FE × status	Yes	Yes	Yes			
Months-since-arrival FE				Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	759,198	759,197	110,213	488,279	488,259	94,681

Notes: The table reports the effects of restricting labor market access for TAF refugees on the labor market outcomes of other refugee groups. The sample in columns (1)-(3) only includes refugees in the N→B transition group (i.e., including pre-decision observations). Column (4)-(6) only include B-permit holders (i.e., only post-decision observations). We estimate the effect of each individual's policy exposure and spill-over effects from TAF policies. The variable 'share currently blocked TAF' measures the share of TAF refugees currently not allowed to work due to the employmentban. All specifications include baseline and additional controls, as well as canton, month and month-since-arrival FE, which are in columns (1)-(3) interacted with the status. Signif. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

D Further wage results

D.1 Using AHV

Table D.1: Effect of labor market policies on monthly earnings: Robustness checks

	(1)	(2)	(3)	(4)	(5)
<i>Dependent variable: Earnings (log)</i>					
Priority enforced	-0.0797*	-0.0736	-0.0778*	-0.0752*	-0.0543
	(0.0414)	(0.0446)	(0.0453)	(0.0433)	(0.0341)
Share restricted jobs	-0.2968**	-0.2499*		-0.2677**	-0.1533
	(0.1193)	(0.1306)		(0.0972)	(0.1444)
Share sector restricted jobs			-0.2389*		
			(0.1232)		
Share region restricted jobs			-0.6169		
			(0.3983)		
Sample	N->TAF	N->TAF	N->TAF	N->TAF	N->TAF
Outcome mean	2,668.8	2,669.2	2,668.8	2,668.8	2,668.8
Num. individuals	14,661	14,711	14,661	14,661	14,661
Canton FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Months-since-arrival FE x status	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes		Yes	Yes	Yes
Heterogeneity-robust				Arrival month	Calendar year

Notes: The table shows the effect of the labor market restrictions on log monthly labor earnings of the employed during the first five years after refugees arrival. The sample only includes the transition group N→TAF. The regressions are based on specification (1). Column (1) uses canton and month fixed effect, as well as months-since-arrival fixed effects interacted with the asylum status. Column (1) also includes the base controls and additional controls. The base set of controls is 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. Column (2) omits additional controls. Column (3) separates the effect of sector and region restrictions. Column (4) and (5) are based on column (1), but employ the heterogeneity-robust approach outlined in Section C.5. Standard errors are clustered at the canton level. Signif. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

D.2 Using SESS

In this section, we leverage the 2012, 2014, and 2016 waves of the SSES to examine the policies' impacts on wages and monthly hours. In contrast to the regressions in section 5.2, which focus on refugees' wages relative to wages of native citizens, we restrict the sample to refugees and analyze the pay differences in refugees' absolute wages depending on the labor market policies.

Our regression approach mimics our approach with the AHV data (Table 2) and adapts it to take into account the smaller sample size and the limited within-status within-canton policy variation over 2012–2016. In particular, as in section 5.2, we exploit within-person policy changes originating from the asylum decision as a second source of variation in policies. As discussed in the main text, the results in the AHV data are very similar if we exploit this plausibly exogenous variation in policies (cf. columns 3 and 6 of Table 2). To gain additional precision, our baseline approach also adjusts for the lengths of stay in Switzerland by adding months-since-arrival in levels and squared as in column 7 of Table 2. Moreover, we include refugees who have been in Switzerland than five years, provided they still have one of the relevant permits at the time of the SSES. We

show that the results are similar if we exclude refugees who stay in Switzerland for more than five years and if we use more demanding fixed effects to control for length of stay in Switzerland.

We present the policies' impacts on log hourly wages and log monthly hours based on this specification in Table D.2. We make the following observations. First, as in the social security data, we find no effect of sector- and region restrictions on the wages of refugees whose asylum claim is granted (N→B, column 1, panel A). However, sector and region restrictions reduce the hourly pay of refugees receiving a TAR or TAF permit (columns 2–5, panel A), in line with the evidence in Tables 2 and Table 3. While the estimated effects are even more negative than those in these two other tables, the point estimates are not statistically significantly different from them. Second, we find little evidence that sector and region restrictions affect refugees' hours worked per month (panel B). Indeed, if we take the point estimates at face value, the restrictions increase monthly hours, implying that the estimates for monthly earnings in Table 2 may understate the restrictions' negative effects on refugees' hourly pay. Third, we find no effect of prioritizing residents on refugees' hourly wages and monthly hours in the 2012–2016 period. Columns 3–5 show that the results are similar if we focus on refugee workers within the five years after arrival in Switzerland (column 3), if we control for industry and canton of work fixed effects (column 4), or years-since-entry fixed effects (column 5).

Figure D.1 uses our baseline specification from Table D.2 (column 2) to examine whether worker, job, and firm characteristics can explain the wage effects. As in Table 3, we find that the negative wage effect of sector and region restrictions is unaltered if we control for fixed effects per industry and canton of work, each year of tenure at the firm, indicators of workers' educational attainment, work experience (measured in terms of months employed in Switzerland according to the AHV data), and fixed effects for the occupation and the management level of the job.

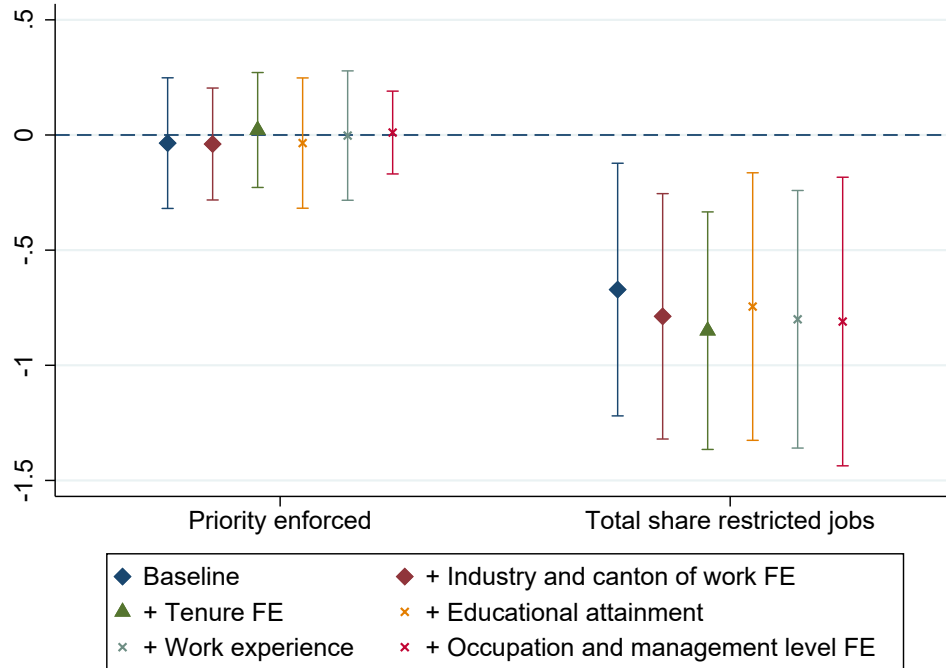
Overall, the results in Table D.2 and Figure D.1 confirm our baseline results in Table 2 and Table 3. In particular, they confirm that sectoral and regional restrictions exert a sizeable negative pressure on the wages of refugees transitioning from N to TAR or TAF permit. This wage effect is present even if we compare refugees with similar observed characteristics and human capital who work in similar jobs.

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

	(1)	(2)	(3)	(4)	(5)	(6)
	N→B	N→TAR/F	N→TAR/F First 5 years	N→TAR/F	N→TAR/F	N→TAR/F w/o sector- restricted
VARIABLES						
<i>A. Log hourly wages</i>						
Priority enforced	0.026 (0.085)	-0.035 (0.138)	0.044 (0.110)	-0.039 (0.118)	0.004 (0.114)	-0.070 (0.150)
Share restricted jobs	-0.032 (0.219)	-0.671** (0.266)	-0.717* (0.366)	-0.787*** (0.259)	-0.704** (0.297)	-0.770** (0.283)
Observations	1,470	2,049	1,008	2,034	2,034	1,701
<i>B. Log monthly hours worked</i>						
Priority enforced	0.038 (0.094)	-0.018 (0.161)	0.012 (0.228)	0.020 (0.181)	-0.143 (0.234)	-0.064 (0.146)
Share restricted jobs	0.047 (0.167)	0.486 (0.461)	0.628 (0.667)	0.888* (0.468)	0.887 (0.551)	0.655* (0.374)
Observations	1,470	2,049	1,008	2,034	2,034	1,701
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Years-since-entry controls	Yes	Yes	Yes	Yes	Yes	Yes
Canton of living FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	No	No	No	Yes	Yes	No
Canton of work FE	No	No	No	Yes	Yes	No
Years-since-entry FE	No	No	No	No	Yes	No

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 2012–2016. The sample in column 1 is employed refugees aged 18–65 who transition from N to a B permit. Columns 2–6 use refugees transitioning from N to TAR or TAF permit, which we cannot distinguish in the data. We aggregate the policies for TAR/TAF refugees by giving TAR policies a weight of 14.1%. We generally use all refugees in the SSES data in the two transition groups independent of their year of arrival. Column 3 is restricted to refugees within their first five years after arrival in Switzerland. Column 6 drops wage observations from canton-industry cells where there exist sector restrictions for asylum seekers in the respective year. All specifications control for canton and year (survey wave) fixed effects. Controls variables are gender, gender-specific age and age squared, fixed effects for nationality and marital status, the cantonal unemployment rate at the time of the survey and at arrival in Switzerland, and social assistance in the canton (in CHF). “Years-since-entry controls” are the number of years since the person arrived in Switzerland (linear and squared). Standard errors are clustered at the canton \times transition group level. Observations are weighted using the person weights in the data. Signif. Codes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Figure D.1: Can worker, job, and firm characteristics explain the wage effects?



Notes: This figure sequentially adds control variables to the wage regression for N→TAR/F refugees with the SSES data (column 2 of Table D.2, panel A). The coefficients show wage effects if we add fixed effects for NACE (rev. 2) two-digit industries and canton of work, years of firm tenure, 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 cantonal level. Controls are gender, gender-specific age and age squared, nationality dummies, marital status, the cantonal unemployment rate at the time of the survey and at arrival in Switzerland, and social assistance in the canton (in CHF).

D.3 Firm analysis

In this section, 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 D.3 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. Columns 1 and 2 (columns 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 regional restrictions.

Table D.3: Effect of labor market policies on employment by firm

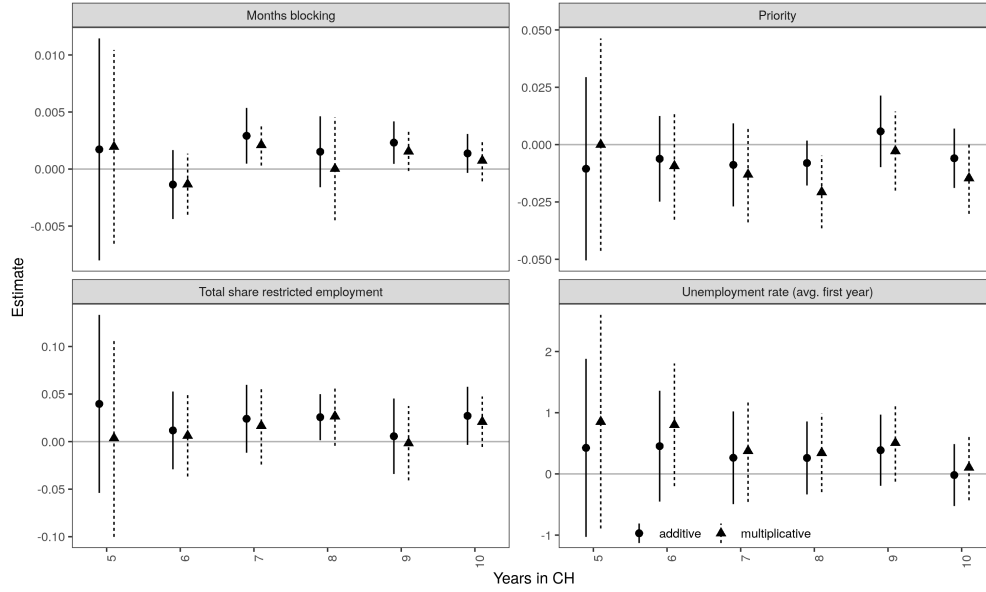
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Dependent variable:</i>	<i>Increase in employment</i>					
Priority	-0.0232** (0.0094)	-0.0197 (0.0167)	-0.0554* (0.0293)	-0.0345** (0.0143)	-0.0274 (0.0219)	-0.0718** (0.0310)
Share restricted job	-0.1052** (0.0434)	0.0037 (0.0439)	-0.1039 (0.0914)	-0.0973* (0.0569)	0.0134 (0.0572)	-0.1333 (0.1091)
Outcome mean	0.1189	0.1397	0.1649	0.1437	0.1880	0.2290
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	210,906	252,377	46,608	167,952	198,697

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

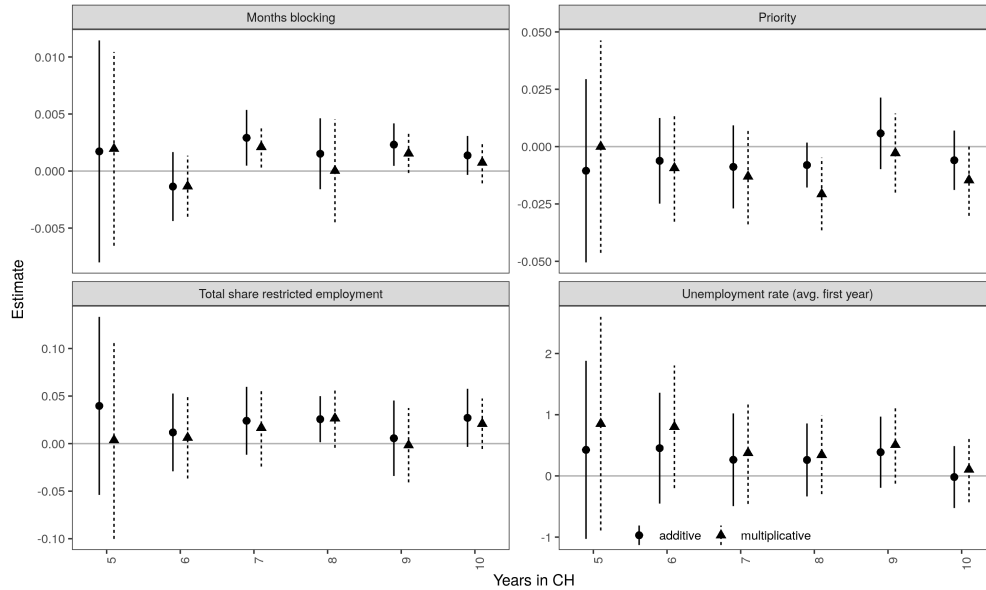
E Complementary Long-run analysis

E.1 Long-run effects on emigration

Figure E.1: Short- and long-run effects of initial policies and unemployment on emigration



(a) Statpop emigration measure



(b) AHV emigration measure

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 Figure (a), 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. In Figure (b), we use AHV-data and define emigration if a person does not appear in the register for at least five years. The estimation model is given by (4). We control for age and age-squared interacted with gender.

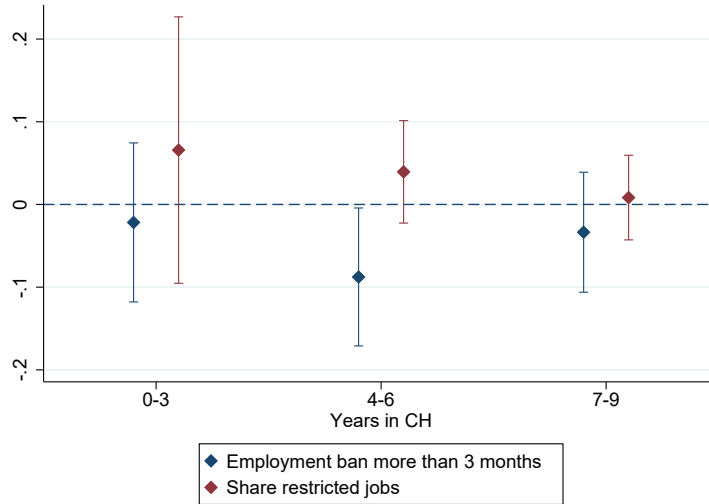
E.2 Long-run effects on wages and education

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 4), 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 transition group fixed effects (i.e., $N \rightarrow B$ and $N \rightarrow \text{TAR/F}$), initially assigned canton, arrival cohort, and survey wave fixed effects interacted with dummies for the two status groups.²⁶

As in Section 6.2, Figure E.2 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 regional restrictions have no long-run impact on wages.

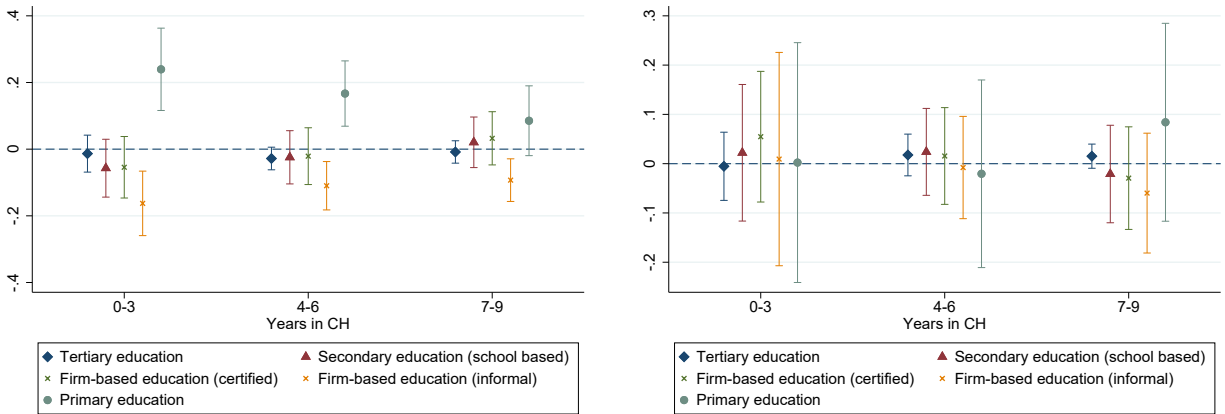
Figure E.3 highlights that lower human capital accumulation could be one potential mechanism explaining how longer employment bans reduce wages. In particular, longer employment bans reduce the probability that refugee workers have acquired firm-based informal secondary education (i.e., on-the-job training) 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.

²⁶Additional 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 transition group level.



Notes: This figure shows the long-run effect of initial labor restrictions on refugees' hourly wages using SESS data 2012–2018. 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 or TAR/TAF permits. Outcomes and policies are measured in October of each year. We control for transition 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 transition groups. Baseline controls are gender, gender-specific age and age squared, refugees' marital status, the unemployment rate in the year of arrival, and the initial level of social assistance in the assigned canton. We also control for the three indicators of years in Switzerland. Standard errors are clustered at the canton \times transition-group level.

Figure E.2: 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 E.2.

Figure E.3: Long-run effects of labor market policies on educational attainment of workers

F 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 labor restrictions 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. (In addition, self-employment was prohibited for N and TAF status refugees.) 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 the 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 \times status fixed effects (Table 1, column 4). As in the main analysis, we only consider asylum seekers and refugees who receive subsidiary protection (i.e., TAF/TAR permit) or are recognized as refugees (B permit) during the first five years after arrival.

Table F.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. Employment</i>				
Status quo	17.32	16.52	10.39	763.29
No restrictions	19.40	19.19	11.64	854.97
Most restrictive	13.39	11.49	8.03	590.03
Difference: no restrictions vs status quo	-2.08	-2.67	-1.25	-91.67
Difference: no restrictions vs most restrictive	-6.01	-7.70	-3.61	-264.94
<i>Panel B. Total earnings (CHF)</i>				
Status quo	444.30	437.03	26658.18	1957.91
No restrictions	507.59	518.36	30455.32	2236.79
Most restrictive	327.11	287.15	19626.72	1441.48
Difference: no restrictions vs status quo	-63.29	-81.33	-3797.15	-278.88
Difference: no restrictions vs most restrictive	-180.48	-231.21	-10828.60	-795.31
<i>Panel C. Social costs (CHF)</i>				
Status quo	440.20	281.34	26411.80	1939.81
No restrictions	401.24	233.24	24074.18	1768.13
Most restrictive	725.29	635.64	43517.12	3196.11
Difference: no restrictions vs status quo	38.96	48.10	2337.62	171.69
Difference: no restrictions vs most restrictive	324.05	402.40	19442.94	1427.99

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

Table F.1 shows the predicted employment months, total earnings, and social aid expenditures under the three scenarios. Comparing the status quo to the most liberal scenario without any restrictions, we find that the labor market restrictions reduced total earnings by CHF 278.9 million over the years 1999-2015 at the aggregate level. On a per-person basis, this amounts to CHF

3'797.2 in lost labor earnings per refugee during the first five years in Switzerland. In terms of employment, refugees lost on average 1.25 employment months due to labor restrictions over the 60-month period. The reduction in labor activity also implies an increase in welfare transfers (for social aid) of at least CHF 2'337.6 in direct cash payments per person (not including non-cash benefits) or 17.7 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 795.3 million in total foregone earnings (CHF 10'828.6 per capita) and 3.6 employment months, which implies an increase in social aid cash transfers by CHF 1.43 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 transfers for the many asylum seekers whose asylum claim is rejected and who 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 7.1 and 7.2, 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.