The Labor Market Effects of Restricting Refugees' Employment Opportunities

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Abstract

In many countries, newly arrived immigrants are not allowed to work for all firms in the labor market. Sometimes, they are not allowed to work at all. We argue that such restrictions on employment opportunities help explain why immigrants have lower employment rates and wages than similar native citizens. This paper explores this hypothesis in the context of refugee integration in Switzerland. We leverage the exogenous geographic assignment of refugees upon arrival, substantial over-time variation in labor market restrictions in Swiss cantons 1999-2016, and linked asylum process and employer-employee data. We document large negative employment and earnings effects of banning refugees from working in the first months after arrival, of prioritizing residents over refugees, and of restricting refugees' labor markets geographically and sectorally. Moving from the least to the most restrictive policy mix reduces refugees' average employment rate in the first five years after arrival from 23% to 16%. Consistent with an impact of workers' outside options on wages, removing 10 percent of refugees' potential jobs also lowers their hourly wage by 3% and increases the wage gap relative to similar host-country citizens in similar jobs by 2.2%. Finally, we show that the policies leave scars: priority to residents and a long initial work ban reduce refugees' earnings for up to four years after they ceased to apply. We find no evidence that restrictive policies spur refugee emigration or improve labor market outcomes of competing EU immigrants. Together, these results suggest that labor restrictions burden both refugees and host communities with significant costs.

Keywords: Labor market integration, migration, labor market policies, labor market institutions, monopsony, refugees, employment, wages

JEL: J08, J42, J61, J68

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1 Introduction

The 2015/16 refugee crisis—and more recently, the war in Ukraine—caused a substantial increase in the number of asylum seekers and refugees seeking protection in Europe and many other continents. A fundamental challenge posed by these unprecedented numbers is how best to facilitate the integration of refugees into host countries' labor markets. While the economic literature has typically stressed the endowments, motives, and experiences of refugee migrants to explain their worse economic outcomes relative to other migrants (see Brell et al., 2020, for a literature review and some evidence), recent studies have focused on one particular labor market policy—temporary employment bans during the asylum process—on refugees' economic integration. However, most European countries restrict refugees' employment opportunities and outside options along several additional dimensions.[1] Yet, we know very little about how these labor restrictions affect refugees' employment, earnings, and wages in the short- and long run.

This paper uses the case of Switzerland as a laboratory to study the effects of policies that regulate whether, where, and for whom refugees are allowed to work. We exploit that each of the 26 Swiss cantons has far-reaching authority in implementing labor regulations, leading to substantial policy variation across cantons and over-time within cantons. We focus on four policies that, as we document, are common in many European countries, either for refugees or for other groups of migrants: (i) prioritization, which grants resident citizens priority on the labor market, (ii) sector restrictions, which regulate that refugees can only work in certain economic sectors, (iii) geographical restrictions, which in our case prevent refugees from working in some or all neighboring regions, (iv) and temporary employment bans, which prevent refugees from working in the initial period after arrival (the last policy is also the focus of (Fasani et al., 2021) and Marbach et al. (2018)).

In addition to providing evidence on the costs and potential benefits of labor restrictions for refugees and the host society, the restrictions on refugees' labor markets also provide an ideal natural experiment to study the elusive wage effects of outside options. Sector and mobility restrictions generate shifts in workers' outside options between initially identical workers that are both observable and exogenous. This is different from typical changes in outside options, which are unobserved and potentially related to factors that affect refugees' productivity in their current jobs (Caldwell and Harmon, 2019). In addition, the shifts we leverage are quantitatively meaningful. Based on the commuting patterns of non-refugees and the sectoral composition of refugees in unrestricted cantons, we estimate that sector and region restrictions sometimes remove up to two thirds of refugees' potential jobs. As a consequence, the policies provide a rare opportunity to test models of imperfect labor markets where constraining workers' outside options depresses wages (as in monopsony models Manning, 2003; Card

[1] Some of these restrictions have recently been lifted for European refugees displaced by the war in Ukraine.

et al., 2018).[2]

Our empirical analyses draw from original data on cantonal labor market policies covering the period 1999–2016. We complement these policies with linked administrative data on refugees' background and asylum processes as well as their earnings histories, obtained from social security records. We also match employer-employee data that provide high-quality information on workers hourly wages and monthly hours worked. Three institutional features conspire to facilitate the identification of the policies' effects. First, refugees are, conditional on (mostly) observable information, exogenously assigned to cantons shortly after arrival. Second, most refugees are, by law, forced to stay in the canton to which they were initially assigned, depending on their status and timing of the asylum decision for five or even more years. Thus, most refugees do not have the option to move to cantons with less restrictive labor regulations. Third, we can leverage the timing of asylum decision and their implications for labor market access when asylum seekers obtain refugee or subsidiary protection status. To alleviate endogeneity concerns that the timing of status change is not orthogonal to refugee's labor market potential, we focus the comparison on refugees in the same arrival cohort undergoing the same status change (and by showing that the results are similar when not exploiting policy variation associated with status change). Together, these features enables us to trace the policies' short- and long-term effects using individual-level panel regression models that relate refugees' labor market outcomes to the policies of the cantons to which refugees were initially assigned.

We first document that all policies depress refugees' employment rate and total labor earnings while they apply. Together, the cumulative effects of the four policies are substantial in size: our estimates imply that moving from an unrestricted to the most restrictive cantonal policy mix observed in our sample is associated with a reduction from 23% to 16% in refugees' average employment rate. This reduction is roughly four times larger than our model estimates for the increase in the unemployment rate during the Great Recession. Using supervised machine learning methods to fit a prediction model for refugees' employment probability, we show that the policies' negative employment and earnings effects are concentrated among refugees for whom we predict above-median employability. These analyses also lend credibility to our empirical design, as we find virtually no policy effects for refugees who have low chances of finding employment in the first place.

[2]The prediction that shifts in workers' outside job opportunities affect wages is not unique to monopsony models. Outside job opportunities are also a key ingredient in the wage-setting protocol of search and bargaining models. These assume that individual workers negotiate—-and potentially renegotiate—-their wage with their employers (e.g., Postel-Vinay and Robin, 2002). Based on such a framework, Chassamboulli and Peri (2020) analyze different immigration policies. One key prediction of their model is that immigrants, especially those with insecure legal status, may be willing to accept lower wages because they face higher search costs relative to native citizens. The paper shows that increasing immigrants' search costs reduces wages via a decrease in outside options. However, lower wages have a positive impact on firms' incentives to create jobs. This incentive offsets the depressing effect of facing a smaller pool of immigrants for hire.

We then analyze whether restricting employment opportunities lowers refugees' wages. We primarily use matched employer-employee data from the Swiss earnings structure surveys (SESS) 2012–2018, which provide employer-reported information on workers' monthly wages and monthly hours. We find no impact of prioritizing residents over refugees but the policy lowers monthly earnings. Sectoral and regional restrictions, in turn, reduce refugees' pay substantially. Refugees' hourly wages are 3% lower in cantons where the restrictions remove 10% of their potential jobs. The effect remains almost unchanged if we augment the wage regressions with a rich set of worker, firm, and job characteristics, which shows that the restrictions cause wage gaps between observationally similar refugees in similar jobs. A striking consequence is that the policies also help explain why refugees are paid less than similar native citizens. In particular, we find that the refugee-native wage gap is at least 2% larger in cantons where sector and region restrictions remove 10% of refugees' potential jobs.

We use the rich contextual information about workers and their jobs to find out why sector and region restrictions lower refugees' pay. We first show that there is surprisingly little evidence that the wage effects can be explained by a restrictions-induced lowering of refugees' productivity. We find no evidence that the policies force refugees to work in low-paying industries or positions. An increase in mismatch—for example, because refugees have to work in jobs for which they are overqualified—does not seem to explain the findings either. There is also no evidence that the wage effect stems from refugees having fewer opportunities to learn on the job. The wage effect is unaltered if we control for powerful proxies of workers' human capital such as firm tenure, accumulated months of work experience in Switzerland, and educational attainment including informal educational degrees attained on the job.

The preponderance of the evidence suggests that sector and region restrictions reduce refugees' hourly pay because they restrict refugees' outside options. Several leading models of imperfect labor markets predict that differences in outside options can generate differences in pay between equally productive workers (see Caldwell and Harmon, 2019, for discussion). In line with our calculations that the restrictions remove a sizeable share of refugees' potential jobs, we find that regional restrictions increase employer concentration at the cantonal level. We also find that sectoral and regional restrictions—and priority to residents—substantially reduce refugees' chances to change employer. These results suggest that the policies reduce refugees' opportunities to work for many firms.

One mechanism how a reduction in outside options leads to lower wages that is consistent with our evidence is that sector and region restrictions increase firms' power to set wages below workers' marginal product. According to monopsonistic models of the labor market, workers with fewer potential employers are easier to attract and retain because they respond less to changes in firms and market conditions. Firms, therefore, can post lower wages to groups of workers that supply labor more inelastically to individual firms (e.g., Boal and Ransom, 1997; Manning, 2003; Ashenfelter et al., 2010; Card et al., 2018). Following a burgeoning monopsony literature (see Sokolova and Sorensen, 2021, for an overview), we approximate the firm labor supply elasticity by contrasting the wage elasticity of separations into employment for refugees in cantons with and without restrictions. We find that separations into employment are less responsive to wages in cantons where policies are restrictive, suggesting a lower firm labor supply elasticity and greater potential wage setting power for firms.

We find less support for other mechanisms through which outside options affect wages. While dynamic search models posit that workers with fewer outside options have limited opportunities to work their way into well-paying jobs (e.g., Manning, 2003), our analysis finds little support for the hypothesis that sector and region restrictions differentially reduce transitions to jobs with a higher monthly income. We also find no evidence that restrictions affect on-the-job wage growth, which stands in contrast to models which posit that fewer outside options hamper workers' bargaining position in wage renegotiations (e.g., Postel-Vinay and Robin, 2002; Cahuc et al., 2006). However, our data suggests that prioritization reduces on-the-job wage growth.

Finally, we take steps towards exploring the costs and benefits of labor restrictions for refugees and host societies. We present three pieces of evidence that suggest that labor restrictions burden both refugees and host communities with significant costs. First, we document that restrictive labor market policies impair refugees' economic integration not only in the short- but also medium-term. In line with an extensive literature that shows that adverse initial labor market conditions leave long-term scars among unfortunate cohorts (see Von Wachter, 2020, for an overview), the priority and blocking policies reduce refugees' labor market earnings for up to five and six years, respectively, after they cease applying. Second, our estimates suggest that labor restrictions have likely no, or, if anything, a negative, effect on emigration. These results also hold for refugees that obtain only subsidiary protection that comes with a temporary residence permit subject to frequent renewal. These results speak against the view popular among some policy makers that labor restrictions provide incentives for refugees to leave the country, nor that they are particularly effective for refugees, who were granted only temporary permissions to stay (Marbach and Hangartner, 2019). Finally, we provide back-of-the-envelope calculations suggesting that the observed labor market restrictions imply missed total labor earnings among refugees of CHF 72.9 million over the 2006–2015 period. On a per-person basis, this amounts to CHF 1,919 in lost labor earnings per refugee during the first five years in Switzerland.

Our analyses and findings contribute to four strands of literature. First, our paper contributes to the literature evaluating how host country policies shape refugees' economic integration (see Brell et al., 2020; Dustmann et al., 2021; Foged et al., 2022, for recent overviews). Previous studies have analysed the effects of the geographic dispersal of refugees upon arrival (Edin et al., 2004; Damm, 2009; Beaman, 2012; Bansak et al., 2018; Dagnelie et al., 2019; Martén et al., 2019; Hangartner and Schmid, 2021), the speed at which asylum decisions are made (Hainmueller et al., 2016; Hvidtfeldt et al., 2018; Bertoli et al., 2020; Aslund et al., 2022), the recognition of educational certificates (Brücker et al., 2021), the generosity of social assistance (LoPalo, 2019; Dustmann et al., 2021), and temporary employment bans (Marbach et al., 2018; Fasani et al., 2021). Our study adds to last strand of literature in terms of scope, research design, and data quality. First, we provide evidence on the labor market effects of four different labor regulations that constrain refugees' labor market access in distinct ways. Since previous works solely focused on employment bans, we lack evidence on the effects of of the other three policies, despite their popularity across Europe and beyond. Second, by combining the largely exogenous geographic assignment of refugees with within-cantonal variation in labor market policies, we can plausibly identify their causal effect. Finally, our original policy and linked register data, which cover both asylum processes and employer-employee relations, allow us to explore the policies' effects on a range of outcome variables that measure immigrant integration across multiple dimensions. These outcomes include refugees' hourly wages, hours worked, separations and job mobility, educational attainment as well as emigration.

Second, our paper contributes to the literature on the relevance of employment opportunities for wage setting and job mobility. To date, there is still limited empirical evidence on the link between workers' outside options, wages, and job mobility—likely because workers' outside options are typically unobserved and rarely change exogenously.[3] The distinctive feature of our work relative to the earlier papers is that we study the effects of changes in labor market restrictions that generate large, observable, and exogenous shifts in workers' outside options that are plausibly unrelated to workers' productivity in their current job. Consistent with a long-standing hypothesis[4], our results suggest that restricting refugees' employment opportunities helps to explain why refugees are paid less than similar resident workers.

Third, we add to the vast and expanding literature on the relevance of monopsonistic competition in modern labor markets (see Manning, 2021, for an overview). Within this literature, our paper is most closely related to studies that analyze immigrant labor markets.[5]

[3]Caldwell and Harmon (2019) use changes in labor demand at former coworkers' current firms to generate exogenous shocks to workers' information set about outside job offers. They show that improved outside opportunities lead to mobility and wage growth. If outside options improve sufficiently, the wage growth partly stems from job changes. If outside options improve only slightly, some workers renegotiate a better wage at the current firm. Lachowska et al. (2021) examine how the wages and separations of dual jobholders in Washington State respond to wage changes in their secondary job. They find that separation probabilities in the primary job are sensitive to wages in the secondary job. Similarly, for higher wage workers, wage increases in the secondary job lead to wage increases in the primary job. Both results suggest that outside options matter for wages and job mobility. Johnson et al. (2020) study non-compete agreements that contractually limit a worker's ability to enter into a professional position in competition with his or her employer in the event of a job separation. They present evidence that a higher enforceability of such agreements diminishes workers' earnings and job mobility. Finally, Jäger et al. (2021) show that workers wrongly anchor their beliefs about outside options on their current wage.

[4]Important contributions include Black (1995), Chassamboulli and Peri (2020), Hirsch and Jahn (2015), Amior and Manning (2020), and Manning (2021).

[5]Manning (2021) provides several arguments for why immigrants' labor markets might be more monopson-

A seminal paper is by Naidu et al. (2016), who study a reform to visa rules that made it easier for guest workers in the United Arab Emirates to switch employers when the first visa expired. In line with predictions of monopsony models, the paper finds that increasing labor market competition increased migrant earnings and employer retention, primarily because of reduced return migration. Similar findings are presented by Gupta (2022), who finds that increased job-switching frictions for Indian and Chinese immigrants reduced inter-firm job mobility and increased firm value.[6] Our paper adds to this literature by exploiting exogenous variation in workers' job opportunities generated by changing labor restriction policies to test the extent to which modern models of monopsony can explain our findings.

Finally, our long-run analyses also contribute to the literature on scarring effects of bad initial conditions when entering the labor market. Existing studies provide strong evidence that entering the labor market in a recession may have lasting negative consequences for employment and wages (Von Wachter, 2020, provides an overview). One strand of studies within this literature has explored how labor market conditions at arrival affect immigrants' economic integration in the medium- and long-run (Aslund and Rooth, 2007; Azlor et al., 2020). Most importantly, Marbach et al. (2018) and Fasani et al. (2021) document that temporal employment bans impair refugees' economic integration for years after they ceased applying. Our results support these findings of long-term repercussions of temporary employment bans, and complement these studies 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

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

A second, wide-spread restriction consists of the prioritization of other workers, either citizens and foreign nationals with more secured residence permits, or immigrants originating

istic than other labor markets.

[6]Depew et al. (2017), Hunt and Xie (2019), and Wang (2021) study (skilled) temporary visa holders in the US that face legal restrictions to change employers. The papers present evidence suggesting that job mobility of visa holders is depressed but conclude that mobility is too large to support the notion that visa holders are effectively tied to their employers.

from other EU/EFTA countries, over asylum seekers when filling vacant jobs. The EU Receptions Condition Directive (Art. 15) explicitly leaves room for the posteriorization of asylum seekers (but not refugees) vis-a-vis aforementioned groups and such prioritization policies are used by several countries including Austria, Germany and Switzerland (ECRE, 2020). While the implementation and enforcement of such prioritization policies varies, they often require firms to either provide proof that they made an effort to hire among the prioritized groups and/or that they registered the job advertisement with local employment offices. Note that such prioritization of resident or citizen workers is not unique to Europe, nor to refugees. For example, Clemens (forthcoming) documents the effects of the U.S. seasonal employment visa for low-skill farm work (H-2A) that only allows employers to hire immigrants if they can show that they have made significant efforts to fill the position with US workers.[7]

A third dimension spans along sector restrictions, where asylum seekers are only allowed to work in selected economic sectors (or occupations) that experience recruitment difficulties. Such sector 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), see ECRE (2020).[8]

Fourth, many European countries including Denmark, Germany, Norway, Sweden, Switzerland, and the Netherlands, use dispersal policies to allocate asylum seekers to host localities (typically relative to their population size). Often, these regional assignments are enforced, either implicitly by tying the provision of housing to the assigned locality, or explicitly by preventing asylum seekers from moving between localities or working outside of the assigned labor market region. In either case, the combination of dispersal policies with mobility restrictions can have similarly detrimental consequences as regional restrictions for those assigned to thin labor markets with few job opportunities. Evidence for this is provided by Åslund et al. (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. Similar policies exist outside of Europe, too. For 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; Mossaad et al., 2020). Lastly, explicit and implicit mobility restrictions also exist for non-refugee migrants. For example, the firm-sponsored H1-B and L-1 visa programs in the U.S. are frequently described as "effectively tying" workers to their employers (Bound et al.,

[7]Under this scheme, employers can hire immigrant workers only after providing evidence that they advertised the position in two local daily newspapers, contacted former US workers to inform them of the opening, and submit a report explaining why they were not able to fill the position with a US worker.

[8]Closely related are restrictions on self-employment, which prevent asylum seekers from opening their own business. Over the last two decades, countries like Germany, Switzerland, and the U.K. have been banning self-employment for asylum seekers (ECRE, 2020).

2015). The resulting lock-in effects, and their consequences for wages, are further compounded if the worker needs the sponsor to apply for permanent residency (Kerr et al., 2015).

2.2 Switzerland—a Laboratory to Study Labor Restrictions

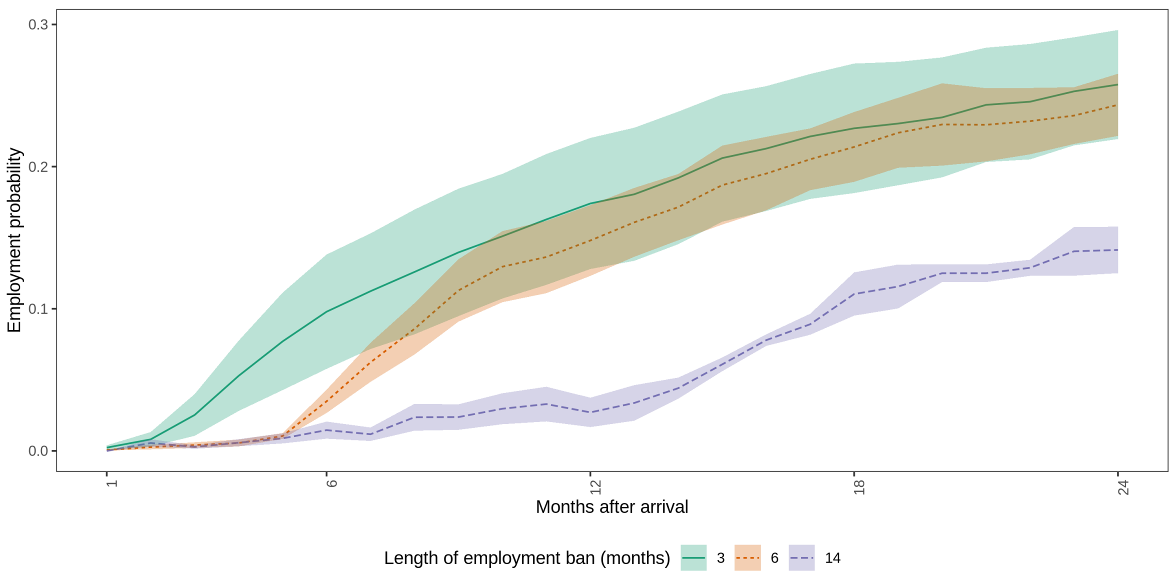
Interestingly, Swiss cantons adopted each and all of the labor market restrictions observed at the European level and discussed above—(i) temporary employment bans, (ii) prioritization of citizens and foreigners with more secured residence permits, (iii) sector restrictions and (iv) restrictions on geographic mobility—over the last two decades.[9] While the national State Secretariat for Migration (SEM) is solely responsible for the processing and decision on asylum applications (more details below), policies on labor market access of asylum seekers and refugees are by and large in the autonomy of each of the 26 cantons. This high degree of federalism leads to substantial policy variation both across cantons and within cantons over time. As a consequence, the subnational policy variation observed in Switzerland closely mirrors the main dimensions of labor restrictions that exist at the European level. As such, 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.

To build a panel dataset on the four main labor market policies, members of the research team coded cantonal policies from publicly available sources (cantonal laws and published guidelines) and then verified the entries for each policy, canton, and year with representatives of the relevant cantonal ministries for the study period 1999–2016. Below, we also discuss how we can leverage register data on refugee mobility and employment to further validate our coding of policies. Most of these cantonal policies also vary by the different protection status that refugees can obtain. We discuss these status in detail in Section 2.3, but briefly preview the main groups here: During the asylum process and until receiving a decision, asylum seekers obtain a 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. For all these groups, we coded the following policies:

(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 08/31/2020, the Swiss Government issued a one-year work ban for

[9]Cantons can also forbid self-employment for asylum seekers and refugees, and several do so. Appendix Figure A.2 shows that the vast majority of cantons does not issue permits for self-employment for asylum seekers and that there is only limited within-cantonal variation during the study period. In contrast, TAF are subject to these restrictions in about half the cantons, and there is considerable within-canton variation, with all but three cantons lifting self-employment restrictions by the end of the study period. TAR and B are not subject to self-employment restrictions.

asylum seekers who arrived after September 1999. Figure 2, panel B, illustrates that several cantons made use of the option to extend the ban's length and set it to a total of 6 months (e.g., Glarus, Nidwalden, Uri, Zug) or even 14 months (Solothurn) for some years (until 2006) during our study period. In contrast to other policies discussed below, the minimal three-month ban does not depend on the refugee status. Figure 1 provides descriptive evidence that for the vast majority of refugees, employment bans were binding and conditioned long term employment rates. As shown in Table 1 below, we observe a few refugees who work in periods when they are subject to an employment ban. Most likely, this is due to mis-measurement of refugees' start or ending dates of employment spells or arrival dates in Switzerland, or non-compliance with the in select cases.



Notes: The underlying model regresses employment status against months-since-arrival fixed effects 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. Standard errors are clustered at the canton $\times$ status group level.

Figure 1: Employment probability since arrival by initial employment ban using 1999-2006 sample

(ii) Prioritisation: Prioritisation of residents is a national law that grants Swiss citizens, foreign nationals with a residence permit, and EU/EFTA residents priority on the labor market. The implementation and enforcement of this law varies across cantons. To take this into account, we use the following coding scheme. We code the policy as not enforced if the canton states that prioritisation is not checked or proactively enforced. We code the policy as enforced if cantons mandate employers to make a 'reasonable effort' to find prioritized jobseekers 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 (RAV) for a minimum of three weeks (such that RAV case workers can encourage prioritized jobseekers to apply). Figure 2, panel A, illustrates that a major change for this policy occurred in April 2006, when the posteriorization of TAF vis-a-vis prioritized jobseekers has been lifted. However, even after April 2006, two cantons (Jura and Glarus) continued to posteriorize TAF. TAR and B refugees are not subject to these posteriorization policies.

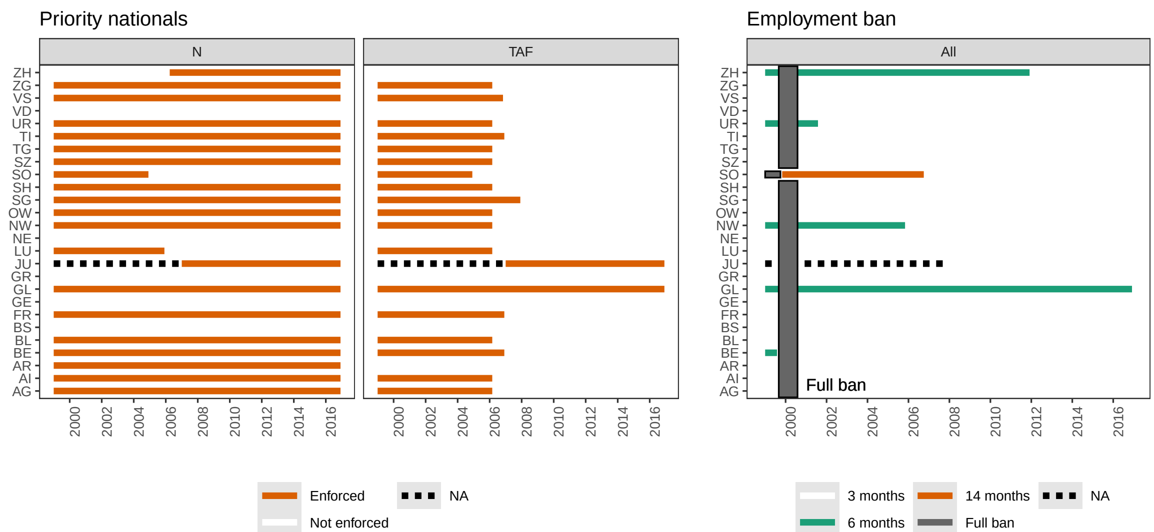
(iii) Sector restrictions: Cantons can restrict work permits for asylum seekers (i.e., before they receive a decision on their asylum claim) to selected economic sectors or occupations. Figure 3, panel A, shows that a total of eleven cantons used that power at some point during the study period. Among cantons with restrictions, the number of sectors that asylum seekers are allowed to work in varies between a single to nine sectors. These often include farm and construction work, care work in hospitals and elderly homes, and waste disposal. Cantons also have the option to impose sector restrictions for TAF. Prior to the national policy change in April 2006, a total of five cantons made use of this option. Refugees with a B/TAR permit are not subject to sector restrictions.

(iv) Mobility restrictions: Cantons have considerable discretion in issuing work permits for asylum seekers, TAF and TAR who are assigned to live in another canton. We count the number of neighboring cantons (to which daily commuting is feasible), which as a general policy do not allow extra-cantonal asylum seekers and refugees with subsidiary protection to work (cantonal authorities might deviate from this general policy on a case by case basis). Figure 3, panel B, shows that overall, asylum seekers face the most severe restrictions. We also observe that labor restrictions gradually decline for TAF and TAR, with the latter group facing no restrictions starting in 2007. Refugees with a B permit are free to settle and work in any canton.

In addition to checking our coding of the policies with experts of the relevant cantonal ministries, we can also leverage our register data to empirically substantiate whether the labor and mobility restrictions are indeed binding. 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.

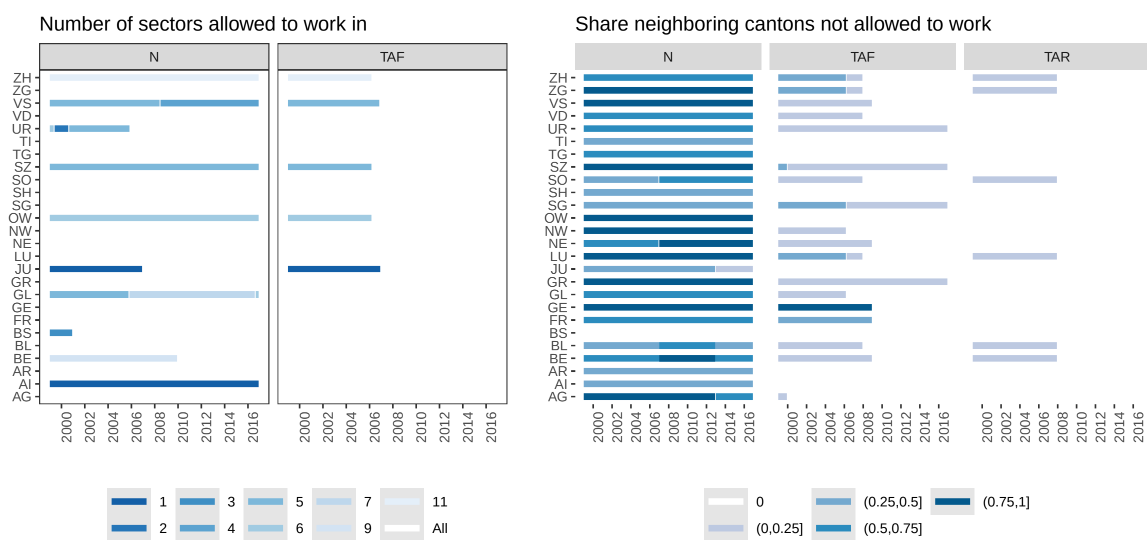
Table 1 shows the results. Panel A and B use employment status from the linked AHV and ZEMIS databases (discussed in detail below) to check for compliance with the initial employment ban. We find that only 0.3% (AHV) and 0.8% (ZEMIS) of refugees are recorded as employed during months in which they are subject to a ban according to our coding. This is in stark contrast to employment rates of 14% and 19%, respectively, for individuals not

Figure 2: Priority policies and employment bans



Notes: Panel A shows when prioritisation was enforced or strictly enforced in Swiss cantons. Panel B provides an overview of the length of employment bans in Swiss cantons. The default national policy is an employment ban of 3 months. This has been extended to 6 or 14 months in some cantons. In September 1999, all cantons except Solothurn introduced a full employment ban during which newly arriving asylum were not allowed to work. This ban was lifted in August 2000.

Figure 3: Sector and geographic restrictions



Notes: Panel A depicts the number of sectors asylum seekers (N) and temporarily admitted foreigners (TAF) are allowed to work in. Panel B shows the share of neighboring cantons which issue work permits.

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

Policy	Yes	No	Share
A. Banned from working	*Employed (AHV)*		
No	442842	2892192	13.28%
Yes	471	233372	0.2%
Missing	1187	16256	6.81%
B. Banned from working	*Employed (ZEMIS)*		
No	478806	2069231	18.79%
Yes	1688	223726	0.75%
Missing	1851	7145	20.58%
C. Extra-cantonal	*Cross-canton commuter*		
Allowed	76167	419725	15.36%
Not allowed	7982	132617	5.68%
Missing	1183	7041	14.38%
D. Sector restriction	*Employed in 'always restricted' sector (ZEMIS)*		
Any restrictions	7554	28152	21.16
No restrictions	74556	145570	33.87
	5741	8412	40.56
E. Sector restriction	*Newly employed in 'always restricted' sector (ZEMIS)*		
Any restrictions	520	1816	22.26
No restrictions	4308	7069	37.87

Notes: Panel A and B show 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.

subject to a ban. Turning to Panel C and focusing on employed refugees, we find that about 5.8% of employed refugees in a canton which does not allow for extra-cantonal commuters are, in fact, cross-cantonal commuters, compared to 15.5% working in cantons that allow for it. As discussed above, this degree of non-compliance is perhaps to be expected given the canton's discretion to decide on such extra-cantonal work permits on a case-by-case basis. Lastly, we turn to sector restrictions. As discussed in the previous section, there is large variation in the number of sectors in which refugees are (not) allowed to work. In Panel D, we focus the analysis on those refugees employed in a sector for which all cantons who implement sector restrictions generally prohibit access (we refer to these sectors as 'always restricted'). We find that about 21.5% of refugees are employed in 'always restricted' sectors in canton-months when access to these sectors is indeed restricted. This share increases to 33.7% for employment in the same sectors in canton-months where there are no sector restrictions. We interpret this pattern as consistent with the idea that cantons apply these sector restrictions with some discretion, in particular to refugees who hold a valid work permit in an occupation for which access is only later restricted. At the same time, the results indicate that these sector restrictions also have some 'bite', as the employment share in these occupation is 12 percentage points higher (a more than 50% increase) if sector restrictions are lifted.

For the analysis, we combine both the sector and regional restrictions into a joint variable measuring the share of job opportunities *not* available to refugees. To construct the "share of restricted jobs", we combine employment shares of refugees who have never been exposed to sector restrictions with national commuter data from the Swiss population census in 2000. Specifically, we define the share as $\sum_j \sum_\ell \xi_{i,j,\ell} \times r_{i,j,t,s,\ell}$ where $\xi_{i,j,\ell}$ measures the propensity of residents in canton i to work in canton j and sector ℓ in the absence of sector or mobility 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 either due to extra-cantonal or sectoral restrictions, 0 otherwise (see Appendix C for details). This joint measure allows us to quantify the loss of job opportunities due to the sector or geographic restrictions for refugees. In one case, 88% of the local jobs are unavailable to refugees because they are banned from certain sectors and from working in the neighboring cantons.

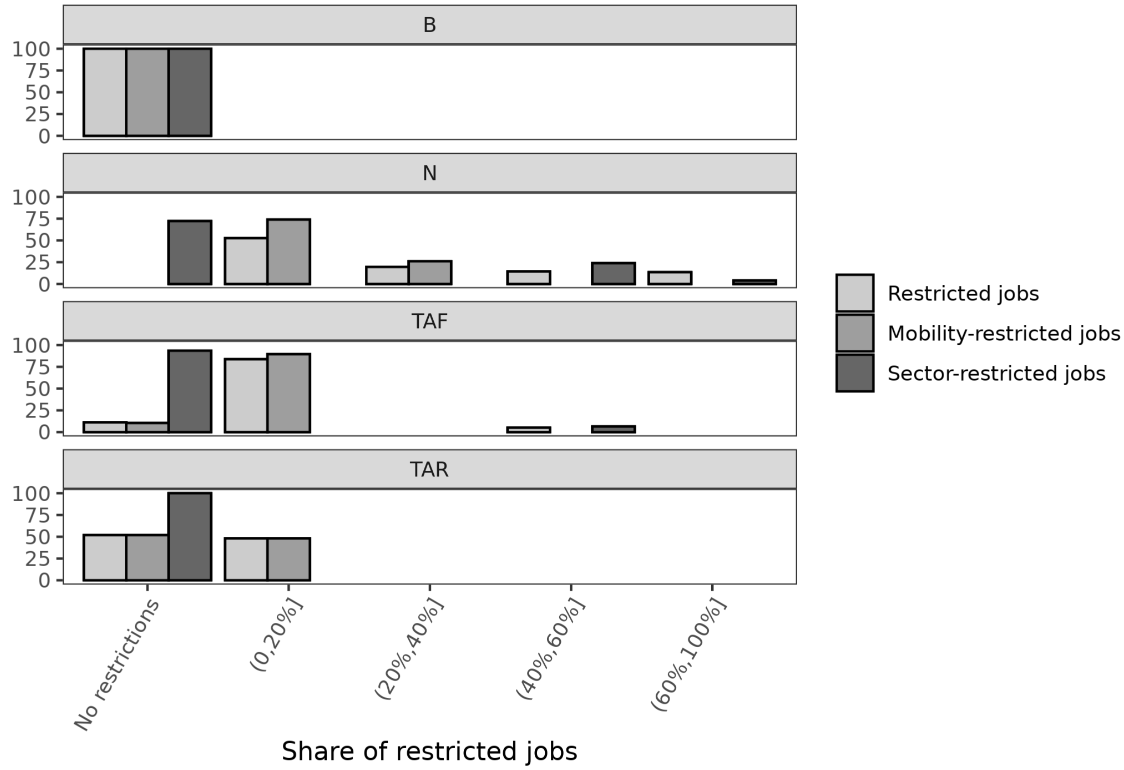


Figure 4: Distribution of share of restricted jobs due to geographical and sectoral restrictions.

The enforcement of these restrictions are the responsibility of the cantonal control bodies, which use a range of tactics, including random checks of firms, to penalize and deter illict labor. Reliable estimates of the size of the workforce engaged in illicit employment is notoriously hard to come by, but existing studies agree that not asylum seekers and refugees, but undocumented immigrants make up the largest share of workers in illict employment (Bolliger and Féraud, 2012; Longchamp et al., 2005).

2.3 Switzerland's asylum system and geographic dispersal policy

As a signatory of the 1951 Refugee Convention and its 1967 Protocol, the Swiss asylum system mirrors those of many other countries in Europe and beyond. Upon arrival, asylum seekers are registered in a few centralized processing centers and then are transferred to one of the 26 cantons within 90 days. While waiting for their asylum decision, asylum seekers obtain an "N" permit and have to reside in the assigned canton. Paralleling other European countries, there are multiple possible outcomes of the asylum process and associated residence permits for refugees seeking protection in Switzerland. If the asylum claim is granted, the asylum seeker receives a "B" permit for refugees who are covered by the refugee convention. If the asylum claim is rejected, the asylum seeker has to leave the country within a short period of time.

If the claim is reject but removal is not feasible or permissible, the asylum seeker receives subsidiary protection and an “F” permit for “temporarily admitted foreigners” (TAF)[10]

If the asylum claim is granted but only because of protection reasons that materialized after the refugee left her origin country[11], the asylum seeker also receives subsidiary protection and an “F” permit, but for “temporarily admitted refugees” (TAR). Our analysis will focus on all asylum seekers that eventually receive (subsidiary) protection and are allowed to stay in Switzerland: TAR, TAF, and B. As we saw above, there are relevant differences in labor restrictions between these groups.

For refugees that eventually receive (subsidiary) protection, the asylum process takes on average about two years (Hainmueller et al., 2016). During this time on a “N” permit, they are only allowed to live and find work in the assigned canton. Exceptions of this rule are granted in very few cases, for example for reunification with first degree family members.[12] These settlement and work restrictions also apply to “F” permit holders and they are only lifted for refugees with a “B” permit. However, given the lengthy asylum process, we expect that lock-in effects lead to low levels of inter-cantonal movements, even after mobility restrictions are lifted. This expectation is empirically substantiated by the analysis presented in Appendix Figure A.9, which shows that eight years after arrival, between 88% (B refugees) and 91% (TAR and TAF) still reside in the initially assigned canton. This implies that the policies regulating labor market access in the canton of initial assignment can have a persistent impact on refugees’ economic integration trajectories.

At the end of the 90-day stay in the processing center, the assignment to a canton is done manually at the headquarter of the the Swiss State Secretariat of Migration (SEM) without any personal interaction between the allocation officer and the asylum seeker (see Martén et al. (2019) for details). By law, the allocation has to be proportional to the population size of the canton and balance the distribution of asylum seekers and refugees’ main origin countries across cantons. Except for a narrow and clearly defined set of reasons—namely pre-existing first degree family networks, health issues that require treatment in a particular hospital, or the accommodation of unaccompanied minors (who are excluded from our analysis)—allocation does not take into account the preferences of the asylum seeker.[13]

[10] Deportation might not be “feasible” if, for example, no commercial airline offer flights to the origin country. Deportation is not “permissible” if, for example, the asylum seeker suffers from mental health issues that cannot be successfully treated in the origin country or the “Dublin” country of first arrival.

[11] Frequent examples for such post-departure protection reasons are political activities that lead to a risk of persecution in the origin country but which the refugee only started after arrival in host country or if the act of deserting the origin country (or its military), itself leads to protection concerns.

[12] Hangartner and Schmid (2021) analyze decisions from the Swiss Federal Administrative Court, which show that the SEM’s very restrictive policies regarding inter-cantonal movements and work permits are binding and extremely rarely overturned.

[13] Hangartner and Schmid (2021) show that asylum seeker who resided in a processing center in French-speaking Switzerland have a higher chance to be assigned to a French-speaking canton (and the reverse holding for German-speaking centers), the reason being that this allows the SEM to complete the asylum process in

When assigning asylum seekers to cantons, allocation officers rely on information provided by the ZEMIS (central migration information system) database, which the SEM uses to process asylum applications and track asylum seekers and refugees. Hangartner and Schmid (2021) provide a detailed overview of the asylum seeker characteristics entered in the ZEMIS. Since allocation officers do not directly interact with asylum seekers, cantonal assignment should be exogenous if we were to condition on the entire information contained in ZEMIS at the time of assignment. Unfortunately, for most of the time period we study, some placement-relevant information has not been stored in ZEMIS, in particular a free text field where SEM employees in the processing center can provide information on placement restrictions (usually sensitive information about family networks and health issues).

Fortunately, this missing data should not pose a threat to identification for the purpose of this study. First, allocation officers are by law not allowed to take the preferences of the asylum seekers into account except for the few exceptions discussed above. Second, all our models flexibly control for canton fixed effects, which renders time-constant self-selection into cantons impotent. Third, we can empirically substantiate the exogeneity of assignment by regressing asylum seekers' sociodemographic characteristics (measured at time of arrival) on indicators for each canton, controlling for nationality, cohort and processing center. Note that this is a hard test since 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 L.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), to each of which the allocation key assigns less than 1% of asylum seekers. Together, these legals constraints on taking into account refugees' preferences, the fact that we observe most information provided in ZEMIS that case officers use for allocation, and these balance tests suggest that self-selection of asylum seekers into cantons is small and unlikely to bias our effect estimates of within-cantonal changes in labor market policies.

In addition to leveraging over-time changes in cantonal labor market policies, (some of our) models also exploit the status change from asylum seeker to refugee (TAR, TAF or B). Under Swiss asylum law (and the refugee convention), the outcome and timing of the asylum process should be independent of the labor market potential of refugees. However, it is easy to think of several ways how (unobserved) refugee characteristics might influence both. For example, asylum seekers with higher innate ability might be more adept at navigating asylum interviews (e.g., by being able to describe the flight in a consistent manner), which might not only increase the likelihood of protection but also shorten the time until a decision is made.

a single Swiss language. The potential endogeneity associated with sorting into a processing center and its downstream effects on cantonal allocation disappears after conditioning on the refugee's origin country.

At the same time, these skills are likely also valuable on the labor market.

To account for such unobserved heterogeneity, our models flexibly control for the time spent in Switzerland since arrival and, in richer specifications, for the time until the (first) asylum decision. These fixed effects should be sufficient to make the timing of the status change conditionally independent of cantonal labor market policies. Additional analysis show that the estimated effects for restrictions are robust to specifications that only exploit within-cantonal policy variation.

3 Labor market data

3.1 AHV-ZEMIS data

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

We measure refugees' employment history using social security annual earnings records covering the period 1999–2016. Contributions to the old age 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 thus cover incomes from small-scale employment and irregular working contracts such as internships, apprentices, or short-term seasonal work as long as the annual labor earnings in the job exceed the income threshold of CHF 2,300 (about 35% of the median monthly full-time wage in 2016). Labor earnings recorded in the data are uncapped and broadly defined. For instance, it includes variable pay components and in-kind benefits. In the registry, employed and self-employed individuals generate one record per job per year that details the starting and ending month of an employment relationship along with the total earnings over that time period. These information allow us to compile a monthly individual-level panel of employment and labor earnings, which we can match with ZEMIS using the social security number.[14] The AHV data also allow us to compute average monthly earnings per year and spell. However,

[14]ZEMIS 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 B.2.

they do not allow us to compute hourly or daily earnings. The data also contain records of non-workers since non-workers are in general also required to contribute to the AHV. The only exception are individuals married to a spouse who contributes at least twice the minimum amount.

3.2 Swiss earnings structure survey

To study how the labor market policies affect hourly wages and hours worked of refugees, we complement the AHV data with an analysis of the waves 2012, 2014, 2016, and 2018 of the Swiss earnings structure surveys, which are conducted by the Swiss Federal Statistical Office. The employer surveys are a large stratified random sample[15] of private and public firms with at least three full-time-equivalent workers. The surveys cover the manufacturing and service sectors but not the agricultural sector. They contain information on 1.58 to 1.98 million workers depending on the wave, which translates to 32 to 39% of total employment in the sectors covered. Since the surveys are mandatory, response rates are high.[16] They contain extensive information on the individual characteristics of workers, on the characteristics of their jobs, and salary information broken down into different pay components. Moreover, they provide reliable employer-reported information on hours worked per worker, which we use to compute hourly wages.

To conduct our analyses, we link the surveys to the Swiss population registers 2010–2018 using the social security number. The registers provide workers' residency permits, the place of living at the time of the survey, and the canton initially assigned to arrival in Switzerland. Unfortunately, we cannot use data from the wage structure surveys before 2012 because they did not contain the social security number and, hence, cannot be merged to the population registers. The employer-reported residency permits in the pre-2012 surveys, which in principle identify the subgroup of workers relevant for this project, turn out to be unreliable.[17] Moreover, the data do not allow us to distinguish TAR from TAF refugees. In this data, we thus estimate the effects of a weighted average of the two permit-specific policies. We use the number of employed refugees in the two statuses in 2012 as the weight.[18]

[15]The complex stratification has about 1600 strata based on firms' size, industry, and broad locations.

[16]The gross response rate to the 2018 survey was 74%.

[17]Prior to 2012, employers were asked to report their workers' residency permits. The relevant category for us is the residual category "other residency permits", which in principle should only contain workers with permits N or F (TAR and TAF). However, in each wave, there are about ten times more individuals in this category than there effectively were in Switzerland according to population registers. Employers apparently assigned too many foreigners to the residual category, possibly because they were unsure about workers' residency status or did not invest the time to find out the correct status.

[18]The weights are the following: 14.1% TAR and 85.9% TAF.

Table 2: Descriptive statistics

	Mean	Sd.	$P_{.01}$	$P_{.50}$	$P_{.99}$	Obs.
Panel A. Merged AHV-ZEMIS data, January 2005						
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
Panel B. Merged AHV-ZEMIS data, January 2015						
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
Panel C. Merged AHV-ZEMIS-STATPOP data (2005)						
Labor income	24591.54	19002.57	262.00	21786.00	68244.27	5152
Employed (AHV)	6.65	5.04	0.00	7.00	12.00	6877
Age	32.20	7.66	19.00	31.00	53.00	13952
Female	0.39	0.49	0.00	0.00	1.00	13952
Panel E. Merged AHV-ZEMIS-STATPOP data (2015)						
Labor income	34007.90	23169.41	323.87	34303.50	88096.59	17888
Employed (AHV)	7.88	5.08	0.00	12.00	12.00	23047
Age	37.98	8.61	23.00	37.00	62.00	34687
Female	0.35	0.48	0.00	0.00	1.00	34687
Panel F. LSE data (October 2016)						
Hourly wage	25.32	7.84	11.58	24.10	52.65	3834
Monthly labor income	3566.55	1519.02	195.00	3899.81	6672.51	3834
Full-time equivalents	0.79	0.30	0.04	1.00	1.00	3834
Monthly hours worked	143.97	55.45	7.00	177.67	199.33	3834
Female	0.27	0.44	0.00	0.00	1.00	3834
Age	35.59	7.60	22.00	35.00	56.00	3834
Primary education	0.78	0.41	0.00	1.00	1.00	3473
Tertiary education	0.02	0.15	0.00	0.00	1.00	3473
Tenure	2.11	2.31	0.00	1.00	9.00	3834
Hospitality sector	0.22	0.42	0.00	0.00	1.00	3834
Trade sector	0.10	0.30	0.00	0.00	1.00	3834
Construction sector	0.02	0.13	0.00	0.00	1.00	3834

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

4 Employment and earnings

4.1 Empirical approach

To study the policies' impact on refugees' labor market outcomes in the first five years after arrival, we use monthly individual-level panel data constructed from the matched AHV-ZEMIS data set. As baseline, we estimate the following panel regression model:

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

The dependent variable y_{icst} is the labor market outcome for refugee i, who was assigned to canton c and holds permit status s in month t. For outcomes, we use a binary employment indicator, log monthly labor earnings of individuals in employment, and the inverse hyperbolic sine (IHS) of labor earnings, which includes workers with zero earnings in a given month and, thus, represents a summary measure of the policies effects on earnings.[19] 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, which depends on the canton's employment ban policy and the asylum seekers' arrival time $T(i)$, (ii) an indicator whether a canton enforces the priority requirement in given month, and (iii) the share of the jobs in the local economy that is banned for refugees due to sectoral and mobility restrictions in a given canton and month.

Our base specification includes canton of assignment (μ_c) and month fixed effects (δ_t). To control for the life-cycle profile of labor supply we add gender-specific age and age-squared controls in vector x_{it}.[20] Intuitively, we identify the effects of the restriction policies by exploiting two sources of variation. The first source of identifying variation comes from *within-canton and within-status changes in policy*; i.e., we compare labor market outcomes of individuals assigned to the same canton and with the same status in more or less restrictive periods. The second source of variation originates from *within-person policy changes due to asylum status transitions*; i.e., depending on the canton and period, transitions from asylum status (permit N) to another status (either B, TAR, or TAF) may relax restrictions to a stronger or lesser degree.

One concern related to the first source of variation is that cantonal policy changes may be correlated with local labor market conditions or other cantonal policies. We address this concern in a number of ways. First, the inclusion of canton fixed effects assures that we identify the policies' effects only from within-canton variation. Second, to account for regional business cycle effects, we control for the contemporaneous unemployment rate in each canton (u_{ct}). In a more demanding specification, we add a few time-invariant and contextual characteristics w_i to

[19] The IHS of outcome y is $IHS(y) = ln(y + \sqrt{1 + y^2})$. We also consider a Poisson fixed effects model to deal with zero earnings.

[20] We also control for an indicator whether a canton prohibits refugees to work as self-employed.

account for the fact that refugees' allocation to cantons may not be completely exogenous (see section 2.3).[21] As expected, it turns out that these controls have little impact on our results, suggesting that the non-exogenous element in the dispersal of refugees is not systematically related to changes in cantonal policies. Third, we control for the cantonal cash allowance for refugees (in Swiss Franc) at time t to control for a possible correlation between labor market policies and the level of social assistance provided to refugees in a canton. Forth, we test for pre-trends below and show that outcomes of treated and untreated individuals evolved similarly in the months prior to policy implementation.

Another concern related to exploiting policy variation from status changes is that the asylum decision and its timing may not be independent of a refugee's labor market potential. Even though receiving a (subsidiary) protection status is legally independent of refugees' labor market integration (as discussed in section 2.3), in practice better integrated refugees may also receive a positive asylum decision earlier. This highlights the importance of flexibly accounting for the status-specific integration path. To alleviate this concern, we report results only for individuals who do the same transition (from permit N to either B, TAR, or TAF) and by permit status (i.e, N, B, TAR or TAF). The latter set of results effectively only exploits within-status-canton variation. Furthermore, to account for status-specific integration trajectories, we also show specifications including fixed effects for the number of months since arrival in Switzerland interacted with permit status, $\gamma_{t-T(i),s}$.[22]

Our most demanding specification replaces canton with individual fixed effects. This specification identifies the policy effects only from the before-after comparison of outcomes of refugees that experience a policy change in their first five years of arrival. Unsurprisingly, these coefficients are, in general, slightly smaller than those from the less demanding specifications because we implicitly reduce the time window used to identify the effects. The individual fixed effect nest the fixed effects for the cantons to which refugees were assigned. Hence, the individual FE also account for any correlation between changes in policies and the initial allocation of refugees across cantons.[23]

4.2 Results

We start by documenting the contemporaneous effects of the labor market restrictions on refugees' employment,total earnings, and monthly earnings in table 3, Panels A to C, respectively.

[21] These are nationality, sex, arrival-center fixed effects, two dummies for self-reported religion (Muslim and Christian), marriage status and the average unemployment rate over first two years after the refugee arrived in Switzerland.

[22] We abstract from differences in the date of arrival and date of application and use the date of application to measure time spent in Switzerland. When observed, the two measures are highly correlated, but date of arrival variable is often missing and more noisy than the date of application variable.

[23] Note that refugees transiting to a B permit can change their location as soon as they receive the B permit.

The table is structured as follows. The first three columns (Columns 1–3) show separate estimates for the three different status transition groups: asylum seekers (with permit status N) whose claim is rejected (denoted N→TAF), and asylum seekers whose claim is granted (denoted as N→TAR or N→B, respectively). The remaining columns 4–6 report results that pool these three group-specific regressions. We do this by simply interacting the month, canton, and month-since-arrival fixed effects with an indicator for the three transition groups. We cluster standard errors at the canton level in columns 1–5, and at the group times canton-level in the remaining columns. Column 4 is our baseline specification that controls for month and canton fixed effects. Columns 5 to 7 show robustness checks. Column 5 shows that effects are similar if we omit the controls that correlate weakly with refugees' dispersal across cantons at entry. Column 6 adds individual fixed effects. Column 7 provides estimates for the sample of refugees who received a TAF permit status. In this column we abstract from individuals' status transitions. Thus, the estimated policy effects are only identified from within-canton changes with more or less restrictive periods.[24]

Employment Panel A of table 3 shows the estimates of the effect of different employment restrictions from a regression specification (1) with an indicator for employment (positive earnings in a calendar month) as dependent variable.

First, we can see in column 4, averaged across all permit transition groups, that refugees allocated to cantons enforcing *prioritization* of permanent residents in a given month are 5.6 percentage points less likely to be employed compared to refugees in cantons where this policy is not enforced. This effect roughly similar for refugees in different transition groups (column 1 to 3). As the average employment rate of refugees is at a low 17.2%, this effect represents 32% reduction in their overall employment.

Second, we see that *employment bans* reduce the likelihood to work by almost 12 percentage points on average. This estimates also serves as a verification check for our coding: in the months with an employment ban refugees' employment is almost fully reduced. This instantaneous negative effect of employment bans is consistent with evidence presented by Frattini et al. (2020) for other European countries and by Hangartner & Marbach (2018) for Germany.

Third, restricting the share of available jobs for refugees, either by only allowing their employment in certain sectors or only in some but not all neighbouring cantons, seems to reduce employment further even though the effect is not always significant. The estimate in panel A, column 5, indicates an increase in the share of restricted jobs from zero to 30% (roughly the 90 percentile in the share of restricted jobs) reduced employment by roughly 1.7

[24]Note we do not provide separate estimates for the sample of refugees with a B permit as those individuals are free to leave the assigned canton after receiving a B permit. We also do not provide a separate estimate for refugees with a TAR status as the sample is too small to produce meaningful estimates.

Table 3: Effect of labor market policies on employment and total earnings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Employment							
Employment ban	-0.1078***	-0.2249***	-0.1466***	-0.1198***	-0.1153***	-0.0673***	-0.1229***
	(0.0245)	(0.0332)	(0.0237)	(0.0160)	(0.0195)	(0.0092)	(0.0281)
Priority enforced	-0.0551***	-0.0552*	-0.0607***	-0.0563***	-0.0555***	-0.0293***	-0.0638**
	(0.0138)	(0.0291)	(0.0204)	(0.0120)	(0.0134)	(0.0110)	(0.0262)
Share restricted jobs	-0.0518	-0.0393	-0.0454	-0.0522*	-0.0486	-0.0341*	-0.0767
	(0.0367)	(0.0302)	(0.0277)	(0.0269)	(0.0303)	(0.0203)	(0.0635)
Outcome mean	0.1889	0.1438	0.1452	0.1728	0.1728	0.1728	0.2294
Num. individuals	39,795	6,369	19,152	65,381	65,381	65,381	33,258
Observations	1,741,073	246,365	759,223	2,746,661	2,746,661	2,746,661	1,239,727
Panel B. Total earnings (Poisson)							
Employment ban	-1.241***	-2.606	-1.587***	-1.260***	-1.260***	-1.556***	-1.215***
	(0.1708)	(1.599)	(0.4062)	(0.1046)	(0.1225)	(0.1856)	(0.1478)
Priority enforced	-0.3914***	-0.7374***	-0.9848***	-0.4568***	-0.4741***	-0.3895***	-0.2561**
	(0.0685)	(0.1764)	(0.2005)	(0.0672)	(0.0661)	(0.0702)	(0.1075)
Share restricted jobs	-0.6302***	0.4792	-0.1221	-0.5054***	-0.5388***	-0.5399***	-0.3239
	(0.2006)	(0.5524)	(0.4036)	(0.1870)	(0.2060)	(0.1462)	(0.2738)
Outcome mean	504.3	365.8	328.0	442.9	442.9	949.7	621.8
Num. individuals	38,488	6,117	18,256	62,891	62,891	18,726	32,620
Observations	1,739,868	246,047	759,222	2,746,496	2,746,496	1,280,860	1,239,677
Panel C. Monthly earnings (log)							
Priority enforced	-0.0718*	-0.4005**	-0.3913*	-0.1709***	-0.1670***	-0.1273**	0.0002
	(0.0374)	(0.1854)	(0.2135)	(0.0424)	(0.0466)	(0.0554)	(0.0279)
Share restricted jobs	-0.3218**	0.2851	-0.1323	-0.2070	-0.2084	-0.1659	-0.0880
	(0.1351)	(0.4647)	(0.3799)	(0.1351)	(0.1340)	(0.1540)	(0.1169)
Outcome mean	7.580	7.476	7.296	7.506	7.506	7.506	7.605
Num. individuals	7,661	989	2,823	11,502	11,502	11,502	7,497
Observations	328,862	35,426	110,230	474,518	474,518	474,518	284,372
Sample	N->TAF	N->TAR	N->B	All	All	All	TAF
Canton FE	Yes	Yes	Yes	Yes	Yes		Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Months-since-arrival FE	Yes	Yes	Yes	Interacted	Interacted	Interacted	Yes
Individual FE						Yes	
Additional controls	Yes	Yes	Yes	Yes	No	No	Yes

Notes: The tables (a) and (b) show the effect of the labor market restrictions on monthly employment and total earnings of refugees in the first five years after their arrival based on specification (1). In table (b), the IHS transformation has been applied to the dependent variable to accommodate zero earnings. The tables also show the effects of the contemporaneous, cantonal unemployment rate and the average cantonal unemployment rate during the first two years after arrival in Switzerland. Columns 1–5 present separate estimates for asylum seekers (N) and the three permit groups: asylum seekers who are recognized as refugees (denoted as N→B), temporarily admitted foreigners (N→TAF), and temporarily admitted refugees (denoted as N→TAR). Columns 6–8 pool these three permit groups. All columns include month, canton, month-since-arrival, and months-to-decision fixed effects. In column 6–8 these fixed effects are interacted with dummies for the three transition groups. Columns 1–6 include the controls that correlate weakly with refugees' dispersal across cantons at entry. Column 8 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 contemporaneous unemployment rate and unemployment rate at arrival. Standard errors are clustered at the canton × status-group level.

p.p.

Fourth, the next three columns (5 to 7) show the estimated effects of these restriction policies (employment ban, priority, share total restricted jobs) are qualitatively and quantitatively similar if we drop additional controls, include individual fixed effects, or if the effects are only identified from the within-canton but not status-transition variation. When individual fixed effects are included (column 6) the effect of the restriction policies are a bit smaller while precision increases. This is expected as the effects from the restrictions are only identified from within-person-changes in the policy environment. Thus, this specification ignores effects coming from different cohorts entering in more and less restricted periods in the same canton. The effects in the within-person comparison are also smaller since they lower refugees employment integration path even beyond the period when they apply as we will show in sections 6.1.

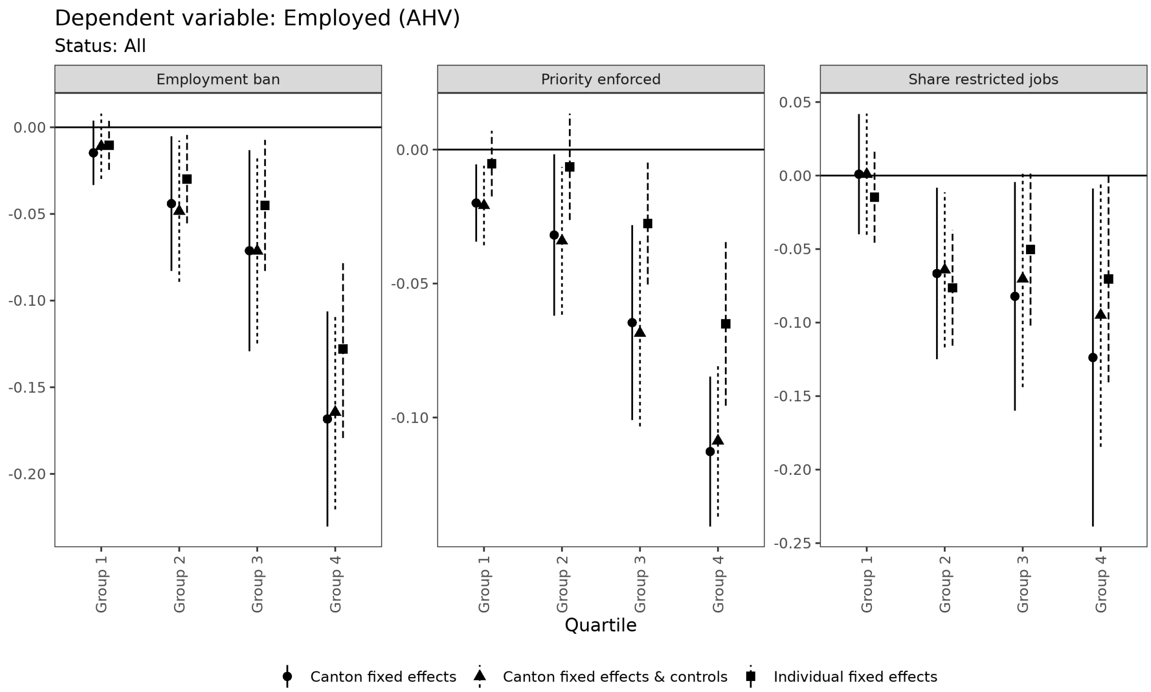
Last, we investigate the heterogeneity of these effects in two different ways. First, figure G.1 shows that the harmful effects of the three policies are strongest for younger and male refugees while the effect is closer to zero for female and older refugees. As labor force participation among male and younger refugees is substantially higher in general, this suggests that the restrictions reduce employment among those with the highest employment potential. Figure 5 corroborates this by plotting the policies' effects separately by quartile of potential employment. We measure potential employment as the predicted likelihood to be employed in the fifth year after arrival given predetermined characteristics such as age sex, nationality, religion, and language.[25] Average predicted employment in these quartiles is 3%, 13%, 26%, and 45%, respectively. The figure reveals that the deleterious effects of employment bans and prioritization increase with potential employment score. While standard errors do not allow to rule 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 more broadly in all quartiles but the bottom quartile. Generally, any of the restriction policies have no impact on the bottom quartile of potential employment. This is not surprising as refugees in this quartiles exhibit a very low likelihood to be employed even in the absence of effects from the policies.

Total earnings Next, we document the estimated effects on labor earnings in Panel B of table 3. The outcome variable includes zero earnings for refugees without employment or self-employment and the model is estimated using a poisson fixed effect estimator.[26] We thus capture both the intensive and extensive margin responses to these policies. Note that we investigate the effects of these policies on wages separately in section 5.

[25] See Appendix D for more details.

[26] An alternative approach using OLS and the inverse hyperbolic sine transformation of the dependent variable is shown in table G.3.

Figure 5: Heterogeneity in the effect of policies on employment, by employment score



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 Appendix D). The figure also shows the effects of the average cantonal unemployment rate during the first two years after arrival in Switzerland. The regression pools refugees from the three permit transition groups (N→B, N→TAF, and N→TAR) and includes interactions of these three groups with month, canton, month-since-arrival, and months-to-decision fixed effects. All regression models include age and age-squared interacted with sex and maximum cash allowance in CHF for refugees. Additional controls are marriage status, two dummies for self-reported religion (christian and muslim), nationality and asylum processing centre fixed effects as well as contemporaneous unemployment rate and unemployment rate at arrival. Standard errors are clustered at the canton × status-group level.

Our baseline specification in column 6 of table 3 reveals that the presence of an employment ban reduces earnings by 102% and prioritization by roughly 46%. Increasing the share of restricted jobs to the 90 percentile (roughly 30% restricted) lowers earnings by about 14%.

In sum, comparing the differences in effect sizes shows that the most pronounced effects on both employment and earnings come from the policy environment newly arriving refugees face, particularly from employment bans and prioritization. The effects from local economic conditions are substantially smaller in comparison even though these are similarly depressing compared to effects found in the literature.

4.3 Robustness checks

In the baseline specification above, we directly control for local economic conditions and other policies that could be correlated with changes in labor market restrictions. An alternative way of addressing the concern of endogenous policy changes, is to exploit differences in the timing of tightening and removal of labor market restrictions for refugees across cantons (see figures 2 and 3) to shed light at the parallel trend assumption embedded in our identification strategy. This assumption stipulates that in the absence of a change in labor market restrictions the outcomes of refugees would have developed similarly.

To this end, we extend the static model in (1) by allowing for anticipation and dynamic policy effects in an event study design. To reduce the complexity of the model, we focus first on the priority policy and the total restricted share.[27] We thus omit months with an active employment ban from the analysis (i.e., the first 3 to 14 months after arrival) and provide a separate event study for the employment ban below. For the first part of the analysis, we estimate the following event study model:

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

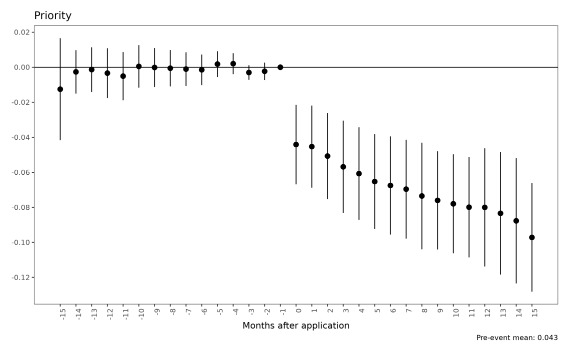
where $p^1_{cs,t-j}$ and $p^2_{cs,t-j}$ are the priority policy and total restricted share, respectively, and Δ is the first-difference operator. Note that the largest share of variation comes from a reduction in restrictions both when individuals transit to permits with less restrictions and over time as restrictions are abolished. We estimate the event path $\{\alpha_{1,j}\}$ and $\{\alpha_{2,j}\}$ for $j = -\omega, \ldots, -1, 0, 1, \ldots, \omega$. The coefficient $\alpha_{q,j}$ represents the cumulative effect of the policy on the outcome after j months for each policy (relative to the reference $\omega = -1$). We set $\omega = 15$, so that the event window covers approximately half of the 60 months time period that each refugee is in our sample. The model extends conventional event studies to two policies

[27] We show a separate event-style analysis for lifting employment bans in appendix G.3.

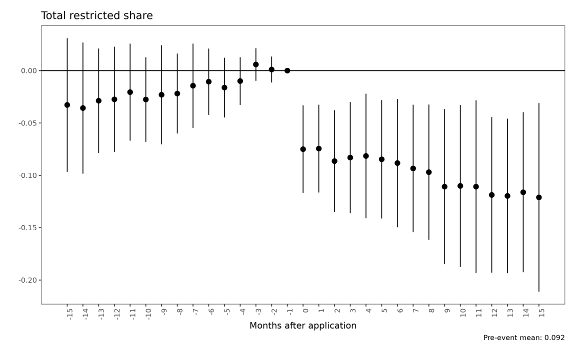
(Freyaldenhoven et al., 2021; Schmidheiny and Siegloch, 2020).

Panel A and B of Figure 6 shows the event plots for the priority policy and total restricted share, respectively. The dependent variable is employment. Both plots provide no evidence for systematic differences in employment trends of refugees in cantons where restriction policies get tightened compared to cantons where this is not the case. On the other hand, both plots also show a clear and immediate reduction in employment starting in the month when either prioritization is enforced or when the share of restricted jobs increases. Over time, the effects of both policies increase.

Figure 6: Event study: Cumulative effects of prioritisation and total restricted share on employment.



(a) Priority policy



(b) Restricted share

Notes: Figures (a) and (b) are based on the event study model in (2). Effects show the cumulative effect of a policy change between 15 months before and 15 months after a policy change. Figure (a) plots the event path for the priority policy. Figure (b) show the event path for the total restricted share. The sample excludes months where an employment ban is in force. The model includes month, canton, month-since-arrival, and months-to-decision fixed effects. These fixed effects are interacted with dummies for the three transition groups. We furthermore add additional individual-level controls.

5 Wages

In this section, we study whether restrictive labor market policies affect refugees' pay. We first establish the baseline wage effects of prioritization and sectoral and regional restrictions. We then exploit the rich contextual information on workers and their jobs in our data to investigate why the wage effect arises. Finally, we show that the policies help to explain why refugees are paid less than similar native citizens.

5.1 Empirical approaches

Our first approach to study the policies' effects on wages is to use the regression models and data used in the previous section but to focus on monthly earnings of employed refugees.

We then refine these analyses by distinguishing between effects on monthly hours and hourly wages using the Swiss earnings structure surveys 2012–2018.

This data has an important shortcoming for our purposes: it provides refugees' hourly wages only in three months that are also covered by our policy dataset (October 2012, 2014, and 2016). Few cantons change their policies during this period. There is also little policy variation that comes from refugees changing status simply because very few asylum seekers have a job. Specifically, the dataset contains less than 200 wage observations of asylum seekers.

To increase the inferential power to estimate the wage effects of the policies with these data, we relate workers' wages as observed in the earnings structure surveys 2012–2018 to the policies that were in place when refugees started working for their current employer.[28] We then compare wages of refugees of the same nationality, participated in the same survey wave, and have the same years of tenure but that were exposed to different policies when they started their jobs. Since the policies are autocorrelated, the estimates also capture the effects of subsequent policies on wages. To ensure that we do not measure wages and policies too far apart, we restrict the sample to workers that started to work at their employer in 2005 or later. Our preferred specification is also restricted to observations from the earliest survey if the same refugee is observed in the same firm several times.[29]

This strategy has the advantage that we can leverage the substantial policy variation in the late 2000s. It also allows us to incorporate the data from the 2018 survey if the worker started to work for her current firm in 2016 or earlier. Not surprisingly, the approach leads to more robust and precise results than if we apply the strategy from the last section. A disadvantage of the approach is that the policy variation depends to some extent on how long a worker is at her current employer, which is a function of the policies, too (as shown later). While the sign of a possible bias is unclear[30], we thus also present the results of relating wages to contemporaneous policies as we did so far, slightly adapting the estimation strategy to deal with the limited policy variation in our sample.[31] The results from the two approaches

[28]The approach exploits that we know each workers' tenure at her current employer. Since the variable is in (discrete) years, we merge the policies in place in April of each year because an employee with one year of tenure is expected to have started working at his current firm 1.5 years ago. Using the policies in place in October of the years leads to very similar results.

[29]Our main wage table provides the results if we do not impose this sample restriction.

[30]The estimates could be biased downward if workers with initially low wages due to restrictive initial policies have a greater probability to quit their job than workers with high wages. The estimates could be biased upward if workers in cantons with restrictive policies remain longer with an employer despite a low wage because they are locked into their jobs.

[31]We estimate specifications very similar to those in the previous section (equation 1), but we relax two constraints. First, we generally incorporate wage observations of refugees that are longer in Switzerland than five years, provided they still have a N, TAR, TAF, or B permit at the time of the survey. Due to the limited policy variation for holders of a B permit—they do not face any labor market restrictions once they have a B permit and we observe very few of them before that—, we generally focus on workers transitioning from N to a TAR or TAF permit. Second, we do not control for years-since-immigration fixed effects in our baseline

are in most cases statistically indistinguishable. If anything, our preferred approach may be conservative: the estimated wage effects are less negative compared to many estimates using contemporaneous policies.

5.2 Baseline results

Panel C of Table 3 uses monthly social security data to estimate the effects of enforcing priority to residents and of mobility and sector restrictions on log monthly earnings of employed refugees in the first five years after their arrival. We do not report but control for the effect of the employment ban at arrival because the ban affects the probability to be employed but should have no effect on earnings if employed. Our preferred specification, which pools all three transition groups (column 4), suggests that enforcing priority reduces monthly earnings by 15.5%. The point estimates in the table also suggest that sector and region restrictions meaningfully reduce monthly earnings. However, the estimates are marginally statistically insignificant except for the large group of refugees transitioning from a N to a TAF permit (column 1).[32]

Table 4 uses the Swiss earnings structure surveys 2012–2018 to estimate the effects of the policies on log hourly wages (panel A) and log monthly hours (panel B). For the reasons explained in the previous subsection, we regress these outcomes on the policies in place when the worker joined the firm. All specifications contain our baseline controls (see table notes) and fixed effects for workers' nationality, canton of living, survey wave, and first year of spell. The sample in column 1 is refugees that transition from asylum seeker (N) to a B permit. The sample in column 2 is refugees that transition from N to either a TAR or TAF permit. The remaining columns pool the two samples.

Panel A of Table 4 provides clear evidence that restricting the share of jobs available to refugees reduces their hourly pay. According to our baseline specification that pools groups (column 3), removing 10 percent of refugees' potential jobs lowers refugees' hourly wage by 3%. As we show in columns 4–6, this effect is robust to excluding the control variables, to using all and not just the first observation per job, and to including full interactions between transition groups and years since arrival in Switzerland. Reassuringly, we find comparable evidence if we regress hourly wages in 2012, 2014, and 2016 on the contemporaneous policies (see Appendix Table H.1). In this case, the estimated effects are also negative—in fact often more negative than in Table 4—and generally statistically significantly so, but the effect sizes

specification. Adding these fixed effects leads to a very saturated model, leaving us with few refugees in unrestricted cantons that could act as a comparison group for restricted refugees. As one can see by comparing columns 2 (baseline estimates), 5 (with years-since-immigration FE), and 7 (focusing on workers within their 5 five years in Switzerland) in Appendix Table H.1, the results are similar but less precise if we do not impose these two adjustments.

[32] Appendix Figure H.1) suggests that the effect of mobility and sector restrictions on monthly earnings is similar for different groups of workers.

vary quite strongly across specifications.

The regressions in panel A of Table 4 provide no evidence that enforcing the priority for residents affects hourly wages. Instead, the negative impact of the policy on monthly earnings (Panel C of Table 3) may be due to negative effects on monthly hours (panel B of Table 4).

Overall, we find no impact of prioritization on hourly wages, but the policy reduces workers' monthly earnings. In contrast, sectoral and regional restrictions meaningfully reduce refugees' hourly wages.

These findings are not in line with a textbook labor supply and demand framework. In such a framework, more restrictions can be seen as an inward shift of the supply curve, which should lead to a movement up a market-level labor demand curve. As a consequence, wages should increase rather than decrease unless labor demand is infinitely elastic. More generally, it is unlikely that the policies affect wages through market-level adjustments. Even in sectors where refugees typically work, they represent at most 1.5% of total employment. Thus, the policies are unlikely to shift aggregate labor supply sufficiently strongly to meaningfully affect market outcomes.[33]

5.3 Mechanism

5.3.1 Theoretical considerations

This section analyzes why shifts in sector and region restrictions reduce refugees' hourly wages. We focus on sector and region restrictions because firms are not free to choose wages if priority is enforced: prioritization requires firms to disclose refugees' work contracts and wages to the cantonal authorities.[34] It is possible that this disclosure explains why prioritization has no effects on hourly pay. While this is an interesting result in itself, it is not clear how much we can learn about wage determination in general from these results.

There are two plausible explanations why stricter sector and region restrictions lower wages. The first relates to the fact that the restrictions generate large shifts in workers' outside options—in some cases, the restriction remove two thirds of all potential jobs available to identical refugees in unrestrictive cantons. Several leading models of imperfect labor markets predict that worse outside options reduce wages and may generate wage differentials even among equally productive workers. In monopsonistic models[35], this wage effect arises because

[33]According to the SSES data, refugees represent 0.16% of total employment in Switzerland. They are only 1.5% of total employment even in the sector where they are most represented (restaurants). In this sector, the largest shift in sector and mobility restrictions (removing two thirds of jobs) reduces employment by $2/3 * 0.052\% = 3.5\%$ (see panel A, Table 3). Thus, increases in the share of restricted jobs shift total employment in the sector by at most 0.05 percentage points ($3.5\% * 1.5\%$).

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

[35]In "classical" models of monopsony, employers face an upward-sloping labor supply curve because there are few employers competing for certain groups of workers. In more recent models of monopsony, employers

Table 4: Effect of labor market policies on monthly hours worked and hourly wages

VARIABLES	(1) N→B	(2) N→TAR/F	(3) Both	(4) Both	(5) Both	(6) Both	(7) Both
A. Log hourly wages							
Priority enforced	0.005	0.070	0.050	0.017	0.043	0.057	0.059
	(0.055)	(0.082)	(0.042)	(0.041)	(0.036)	(0.041)	(0.041)
Share restricted jobs	-0.296	-0.347**	-0.301***	-0.375***	-0.200**	-0.261**	-0.324***
	(0.196)	(0.153)	(0.104)	(0.106)	(0.087)	(0.112)	(0.100)
Observations	1,942	4,381	6,342	6,361	9,231	6,340	6,334
B. Log monthly hours worked							
Priority enforced	-0.213*	-0.056	-0.086	-0.089	-0.042	-0.082	-0.093
	(0.122)	(0.129)	(0.087)	(0.088)	(0.077)	(0.091)	(0.071)
Share restricted jobs	0.248	0.086	0.174	0.530***	0.156	0.169	0.287*
	(0.244)	(0.242)	(0.169)	(0.185)	(0.157)	(0.192)	(0.151)
Observations	1,942	4,381	6,342	6,361	9,231	6,340	6,334
Observations per firm	First	First	First	First	All	First	First
Baseline controls	Yes	Yes	Yes	No	Yes	Yes	Yes
First year of tenure FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Canton FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Years-since-entry FE	No	No	No	No	No	Interacted	No
Industry FE	No	No	No	No	No	No	Yes
Canton of work FE	No	No	No	No	No	No	Yes

Notes: This table shows the effect of labor restrictions on log hourly wages of employed refugees (panel A) and log monthly hours worked (panel B) using data from the Swiss earnings structure surveys 2012–2018. We relate workers' hours and wages—those observed in the survey waves 2012–2018—to the status-specific policies that were in place when the refugees started to work at their current employer, exploiting information on workers' firm tenure. In all columns but column 5, we only keep the observation from the earliest survey if the same refugee is observed in the same firm in several surveys. The sample in column 1 is employed refugees aged 18–65 that transition from asylum seeker (N) to a B permit. The sample in column 2 is employed refugees aged 18–65 that transition from N to either a TAR or TAF permit, which we cannot distinguish in the data. The remaining columns pool the two samples. In all columns, we focus on refugees that started to work in 2005 or later for their current employer. We aggregate the policies for TAR and TAF refugees by giving TAR policies a weight of 14.1%. All specifications control for (initial) canton and survey wave fixed effects, and fixed effects for the year in which the worker joined the firm. Baseline controls, dropped in column 4, are gender, gender-specific age and age squared, nationality dummies, marital status, the unemployment rate at the start of the spell, the level of social assistance in the canton (in CHF), and dummies equal to 1 if the canton has a blocking period exceeding 3 months and self-employment restrictions at the start of the spell. Column 6 adds status-specific years-since entry fixed effects to our baseline specification. Column 7 adds two-digit industry fixed effects and fixed effects for the canton that refugees work to our main specification. Standard errors are clustered at the canton × status-group level.

firms post lower wages to groups of workers that supply labor more inelastically to individual firms, for instance because the group faces larger search frictions. Workers with fewer potential employers are easier to attract and retain because they respond less to changes in firms and market conditions (e.g., Manning, 2003; Card et al., 2018). In dynamic monopsony models and some search and bargaining models, workers with fewer potential employers also have lower wages because they have less options to work their way into well-paying jobs (e.g., Manning, 2003). Finally, in a large class of search and bargaining models, workers negotiate—and possibly renegotiate—wages on the basis of outside job opportunities (Postel-Vinay and Robin, 2002; Cahuc et al., 2006). In such models, wages increase in the arrival rate of outside job offers. Below, we evaluate each of these mechanisms.

The second explanation why sector and region restrictions may reduce wages is that they may lower the marginal product of refugee workers. For instance, the policies may force refugees to work in sectors or regions with comparatively low productivity and wages. Moreover, restricted labor market access may gradually lower refugees' earnings potential because refugees may miss out opportunities to learn on the job. A lack of work-related skills and on-the-job training may eventually result in lower wage rates.[36] Finally, the policies may increase the mismatch between refugees' skills and the skill requirements of their jobs.

In what follows, we first exploit our rich data on workers and their jobs to present evidence that the decline in wages is unlikely to result from productivity effects. We then explore whether the results are consistent with the outside options story.

5.3.2 Productivity and wages

Figure 7 analyzes whether a lack of human capital accumulation or the selection of refugees into types of jobs can explain why sector and mobility restrictions lower refugees' hourly pay. The figure compares our baseline wage estimates (column 3 of Table 4) with specifications that control for more and more worker and job characteristics.[37] Specifically, we add fixed effects for workers' two-digit industry, canton of work, and highest educational attainment, control for accumulated work experience in Switzerland (total months in employment since arriving in Switzerland) as well as fixed effects for two-digit (ISCO) occupation codes and a worker's management (hierarchy) level within the firm. The comparison of the first two coefficients also shows what happens if we do not control for workers' tenure. Appendix Figure H.2 shows the results if we relate hourly wages to contemporaneous policies. The findings are very similar.

have wage-setting power either because workers face search frictions (Manning, 2003), or because they have idiosyncratic tastes for amenities offered by firms (Card et al., 2018, e.g.,).

[36]In richer models of skill accumulation such as Gibbons and Waldman (2006), these effects may also arise through fewer promotions or because refugees accumulate skills that are not easily transferable to better-paying jobs.

[37]The samples of the regressions vary somewhat due to missing values in certain covariates. The data on educational attainment is missing for 9% of the refugees. The occupation code is missing for 12% of the refugees.

Figure 7 shows that differences in pay levels between industry, occupations, and cantons of work cannot explain the negative wage effects of the share of restricted jobs. Hence, the wage effect does not simply reflect that the policies force refugees to work in low-paying sector or regions. A mirror image of this finding is that refugees' wage distribution across all sectors is remarkably similar to the wage distribution in sectors to which refugees are typically restricted when sectoral restrictions apply (see Appendix Figure C.3).

A lack of human capital accumulation is also unlikely to explain the wage effect. Controlling for education—which includes a separate fixed effects for informal educational degrees attained on the job—, tenure, and accumulated work experience does not alter the estimated wage effect. Finally, the figure also suggests that the wage effect is not due to restrictions causing better-paid workers to lose their jobs (selection effect). Controlling for job and worker characteristics would reduce the wage effect if its cause were a change in the composition of workers. The same conclusion can be drawn from the fact that the estimated wage and monthly earnings effects do not change much if we control for individual fixed effects and thus focus on within-person variation only.[38]

Lastly, we examine whether increased skill mismatch can explain why regional and sectoral restrictions lower refugees' wages. This is not implausible: the restrictions might force the well-educated engineer to look for a job as a waitress.

However, two pieces of evidence speak against this channel. First, we find no evidence that sector and region restrictions force well-educated refugees to work in a job for which they are overqualified (see Appendix I). We examine this by looking at the impact of the restrictions on refugees' years of schooling compared to residents in similar jobs.[39] Second, employment changes in firms also speak against the mismatch story. Firm-level employment trends are relevant because the mismatch explanation requires that firms add badly-matched refugee workers if refugees are forced to work in their sectors or regions. A priori, we see few reasons why firms would hire certain badly-matched refugees under restrictive policies that they would not hire under nonrestrictive policies. Indeed, we find no evidence that an increase in the share of restricted jobs increases employment of refugees.[40] We restrict the 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 (see appendix J).

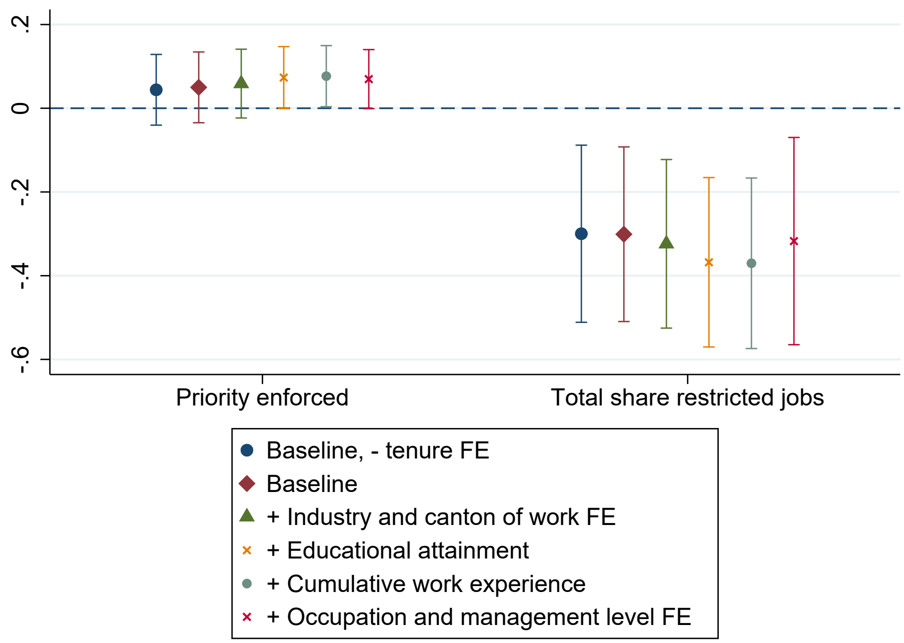
In sum, the negative wage effects do not appear to be the result of lack of skill accumula-

[38]The results using monthly earnings in the AHV data are presented in column 6 of panel C of Table 3. The corresponding specification the Swiss Earnings structure surveys using hourly wages, shown in column 6 of Table H.1 is also similar to our baseline estimates but lacks precision because of the few refugees observed in several surveys.

[39]Prioritization, in contrast, appears to affect overqualification in occupations with higher educational demands. In these occupations, enforcing prioritization reduces the gap that we see between refugee and native education in cantons that do not enforce prioritization.

[40]In fact, the evidence points in the opposite direction, in line with the worker-level evidence in section 4.

Figure 7: Wage effects and the composition of jobs



Notes: The figure shows the effect of enforcing the priority requirement and of sector and mobility restrictions on log hourly wages of refugees aged 18–65 using data from the Swiss earnings structure surveys 2012–2018. The baseline specification (second coefficient from the left) illustrates the results from the wage regression presented in column 3 of Table 4, panel B. The first coefficient shows the wage effects if we do not control for workers' tenure. Econometrically, we implement this by omitting the start year of spell fixed effects. The remaining coefficients show wage effects if we add fixed effects for NACE (rev. 2) two-digit industries and canton of work, for eight levels of educational attainment, accumulated work experience (measured as months employed since arrival in Switzerland, added in levels and squared), and fixed effects for ISCO two-digit occupation and five management levels (no, lowest, low, mid- and highest-level management) (yellow). The underlying samples vary somewhat between regressions due to missing values in certain covariates. Vertical lines show confidence intervals clustered at the canton × status-group level.

tion, of the sorting of workers into different types of jobs, or of increased mismatch. Hence, the results do not appear to be consistent with a competitive labor market where wages reflect workers' marginal product. Instead, the evidence suggests that sector and mobility restrictions cause wage gaps between equally productive refugees in similar jobs assigned to different cantons.

5.3.3 Outside options and wages

The results in the last section suggest that the sector and region restrictions produce wage differences between equally productive workers, consistent with the relevance of outside options in wage determination. In this section, we first provide evidence that the restrictions indeed reduce refugees' outside options. We then evaluate the extent to which our results are consistent with the theoretical mechanisms through which outside options may influence wages.

Effects on outside options As a first step to analyze the relevance of outside options in explaining our results, we now provide evidence that the policies (i) reduce the number of potential employers and (ii) job-to-job mobility, consistent with a reduction in outside employment opportunities.

Appendix section K analyzes whether priority and sector and region restrictions implicitly restrict the number of firms that employ refugees. Using the social security earnings data, we count the number of distinct firms that employ the refugees allocated to a particular canton in a given year. At this level, we then compute different measures of employer concentration separately by permit (N, TAR, TAF, and B), among others the familiar Hirsch-Herfindahl index (HHI). If more restrictive policies reduce the number of potential employers, we would expect that they increase employer concentration. To study this hypothesis, we simply regress the permit-specific concentration measures on the permit-specific policies at the canton-year level. The results suggest that refugees in cantons with a larger share of region-restricted jobs face greater employer concentration, consistent with a reduction in outside options.[41] Priority, in contrast, *reduces* employer concentration. A possible explanation is that enforcing priority makes it harder for a single firm to hire many refugees. This result may help to explain why the policy does not depress wages.

[41]These results relate to a large number of recent empirical studies that document considerable employer concentration in certain segments of modern labor markets. These studies also analyze the relationship between changes in wages and changes in employer market power. These studies find that higher employer concentration has a robust negative association with wages (Azar et al., 2020a,b; Benmelech et al., 2020; Marinescu et al., 2020; Rinz, 2018; Schubert et al., 2020; Stansbury and Summers, 2020). Using exogenous variation in employer concentration induced by hospital mergers in the US, Prager and Schmitt (2021) find evidence that concentration reduces wage growth, but only if the increase in concentration is large and workers' skills are industry-specific.

An alternative way to assess whether the restrictions reduce outside job opportunities is to study how the policies affect refugees' job-to-job mobility. We do this in Table 5 by looking at four different outcomes in the monthly social security data. The dependent variable in column 1 is equal to one in the month the worker separates from her current employer and zero in all other months that the worker is employed. The following columns split this outcome into separations into non-employment (column 2) and employment (column 3). The latter outcome is one if a worker separates from an employer and finds a job at another employer within at most two months. The outcome in column 4, termed job-to-job change, approximates voluntary separations. In particular, we count separations into employment but set the outcome to zero if the individual draws unemployment benefit payments in month t, $t+1$, or $t+2$. In all columns, we focus on main jobs[42] and disregard the data from November and December because of breaks in the firm identifier between two yearly waves of data.[43] Furthermore, we estimate specification 1 with OLS using the preferred model that pools all three transition groups. We present models with canton fixed effects in panel A and ore restrictive models with person fixed effects in panel B. The person fixed effects account for potential effects of the policies on the composition of workers.

The estimations in columns 3 and 4 of Table 5 suggest that prioritization and sectoral and regional restrictions reduce separations into employment and job-to-job mobility. The effects are large in magnitude. An increase in the share of restricted jobs by, say, 20% reduces monthly job-to-job transition rates by 0.4–0.5 percentage points, or by approximately 15% relative to the mean transition rate. Enforcing the priority requirement lowers refugees' job mobility by another 0.5–0.75 percentage points ($\approx -20\%$). We do not find effects on separations into non-employment (column 2), which also explains the absence of effects on all separations (column 1). In sum, the tables provides strong support for the idea that the restrictions reduce refugees' job-to-job mobility, consistent with a reduction in the arrival rate of outside job offers.

Separation elasticities A key prediction of monopsonistic models of the labor market is that firms post lower wages to groups of workers with fewer potential employers (see, e.g., Boal and Ransom, 1997; Manning, 2003; Ashenfelter et al., 2010). The central parameter governing firms' wage setting power is the labor supply elasticity that it faces. These considerations

[42]Workers can have several job at once. The main job in any given month is the job with the highest monthly earnings.

[43]Employer-to-employer transitions that occur between December and January in the following year largely reflect changes in the identifier of continuing firms instead of true job changes. As in other similar datasets, these changes in firm identifiers happen for many reasons, including relocation, restructuring, and relabeling of firms. The problem can, in principle, be reduced by using worker flows to correct firm identifiers (see Hethey-Maier and Schmieder, 2013). However, we lack the data for non-refugee workers to implement these corrections. Since firm identifiers are homogeneous within the yearly waves of data, a simple fix is to disregard job changes that occur over the turn of the year.

Table 5: Effect of labor market policies on separations, job mobility, and on-the-job wage growth

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

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: This table shows the effect of the labor market restrictions on separations, job mobility (columns 2–5) and within-job wage growth (columns 6 and 7) of employed refugees in the first five years after their arrival based on specification (1). We estimate the specification that pools the effects for all three transition groups using OLS. All columns include month, canton, month-since-arrival, and months-to-decision fixed effects, interacted with dummies for the three transition groups.

In columns 1 to 6, we compare the labor market status in t with $t+2$. We disregard November and December in each year because of breaks in the firm identifier between two yearly waves of data. The dependent variable in column 1 is an indicator equal to one if the worker separates from his employer between t and $t+2$ by either transiting to unemployment or moving to a new employer. The dependent variable in column 2 is an indicator equal to one if the worker transitions to unemployment, zero otherwise. Column 3 uses job change as the outcome. A job change is defined as the exit from the main job between month t and $t+2$, where the main job is the highest paying job at time t. The dependent variable in column 4 is identical but disregards observations with intervening unemployment spells. In column 5 (column 6), the indicator is one only if the change of employers leads to a wage increase (decrease) in the main job.

Finally, column 6 and 7 focus on on-the-job wage changes. The sample is restricted to workers that worked for the same employer already in the year before. We focus on the monthly earnings in December and discard the remaining months because the spell data in the AHV does not reveal month-on-month wage changes within a given employer-year. The dependent variable in column 6 (column 7) is a dummy variable equal to one if workers' monthly earnings in December are smaller (larger) compared to his or her (nominal) monthly earnings in the same job in December the year before.

raise the question whether the restrictions influence the labor supply elasticity facing the firm.

To evaluate this question, we follow a large recent literature, summarized in Sokolova and Sorensen (2021), and estimate the sensitivity of separations into employment with respect to workers' wages. In a steady state, this elasticity is directly proportional to firms' labor supply elasticity (Manning, 2003).

Our approach to estimating this elasticity uses the monthly social security data. The basis of our approach is the model of employer-to-employer transition presented in column 4 of Table 5. We enrich this regressions with workers' monthly earnings. The covariates and fixed effects are the same. To account for non-linearities, we add 9 wage dummies that order monthly earnings in any given month from low to high.

Panel A of Figure 8 shows the coefficients on these dummies. The figure shows the difference in the estimated effect in each decile relative to the effect in the 5th group. The panel shows that low-paid refugees have a higher separation rate than high-paid refugees. The figure also plots two regression lines fitted through the data points. The first is fitted over the entire distribution (dashed line), the second disregards the two estimates in the tail (solid line). Due to the normalization of the separation rates, the slopes are directly related to the implied firm labor supply elasticity: The flatter they are, the lower the elasticity. Since the slopes are not vertical, firms' labor supply elasticity is finite and the firms possess monopsony power vis-á-vis refugees. Monopsony power is larger—the regression slope flatter—if we consider the two data points at the tails (dashed line).

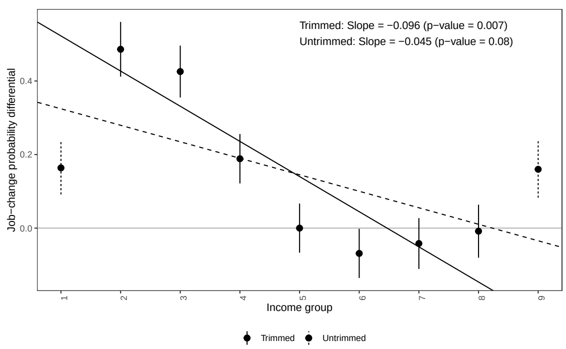
Panels B–D of Figure 8 examine whether the relative separation rates relate to the labor market restrictions. To examine this question, we simply split the sample according to whether priority is enforced or not (panel B), whether there are region restrictions or not (panel C), and whether there are sector restrictions or not (panel D). The hypothesis is that the policies reduce the separation elasticity by lowering the arrival rate of outside job offers. The evidence in each panel is consistent with this hypothesis: the slope of the fitted regression line is flatter if the policies apply, implying a smaller firm labor supply elasticity. With the exception of panel C, this conclusion does not depend on including the two estimates at the extremes of the distribution of monthly earnings.[44]

Taken together, regional restrictions, sectoral restrictions, and prioritization are associated with a lower wage elasticity of separations to employment, consistent with the monopsony prediction. However, these estimates have to be interpreted cautiously: they may be biased because we use monthly earnings instead of hourly wages and because of the potential endogeneity of earnings in the regression.[45]

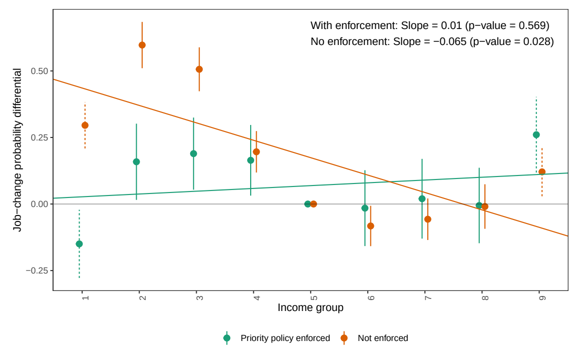
[44]If we fit the regression lines in Figure 8 without the two most extreme data points, we do not find evidence that regional restrictions are associated with a lower firm elasticity of labor supply.

[45]Ideally, we would have exogenous variation in the wage and use this to estimate the separation elasticity. This is hard because one needs exogenous variation in wages at the firm level. As pointed out by Langella and Manning (2021), the interest here is less in the estimate of the level of the separation elasticity but in how it

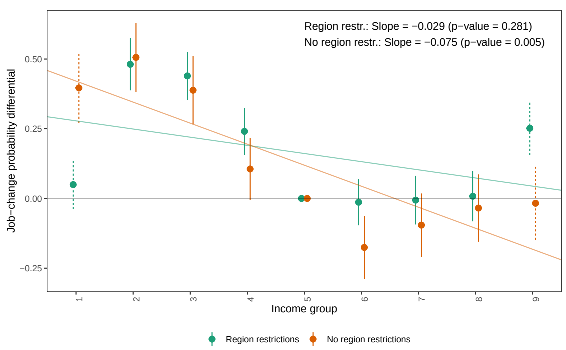
Figure 8: Labor market restrictions and the elasticity of separations into employment.



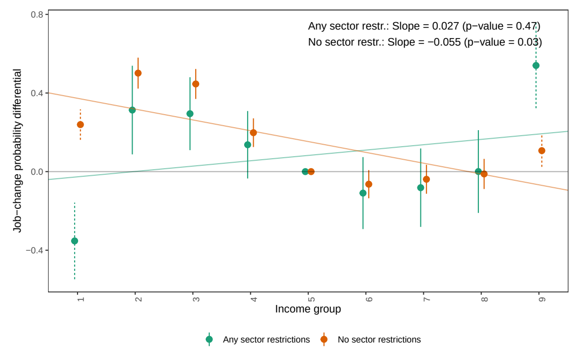
(a) Overall elasticity



(b) By prioritization



(c) By regional restrictions



(d) By sectoral restrictions

Notes: The figures plot the probability to separate into employment across income groups relative to the base group (group 5). The basis is the model of employer-to-employer transition presented in column 4 of Table 5. We enrich this regressions with workers' monthly earnings. The covariates and fixed effects are the same. To account for non-linearities, we add 9 wage dummies that order monthly earnings in any given month from low to high. The job change indicator is equal to one if the employee exits from the main job between month t and $t+2$ while remaining in employment. We disregard November and December in each year because of breaks in the firm identifier between two yearly waves of data. In Figure (a), we trim the bottom and top end of the income distribution (as in Bassier et al. 2021). Figures (b) to (d) show the job-change differentials depending on the policy status. The slope of the curve is a measure of how elastic labor supply reacts to changes in wages.

Career effects Dynamic models of monopsony (e.g., Manning, 2003) and certain search and bargaining models (e.g., Postel-Vinay and Robin, 2002) also predict that sector and mobility restrictions may lower wages by making it less likely that workers can transition into better-paying and more demanding jobs. In bargaining models with on-the-job wage bargaining, fewer outside options also lower wage growth by hampering workers' bargaining position in wage negotiations.

To examine the first mechanism, we return to column 4 of Table 5. We then estimate separate policy effects job-to-job mobility for job changes that are accompanied by an increase in monthly earnings (column 5) and by a decrease (column 6). The regressions suggest that prioritization and sector and region restrictions reduce both, transitions to jobs with a higher and lower monthly income. The results thus do not support the prediction that the pay penalty of refugees arises because they find it harder to land a well-paying job. These results are consistent with those in Figure 7, which also suggest that the sector and mobility restrictions do not lower wages by preventing switches to better-paying jobs higher up in the firm hierarchy or in better-paying occupations.

To examine the second mechanism, columns 7 and 8 of Table 5 study whether the policies impact the rate at which workers receive higher salaries in ongoing jobs. The dependent variable in column 6 (column 7) 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 December the year before. The sample is restricted to workers that worked for the same employer already in the year before. 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.[46]

The comparison of monthly earnings across calendar years shows that priority to residents lowers refugees' chances to experience an increase in monthly pay in an ongoing employment relationship (panel A, column 6). In turn, it increases the chances for a wage decrease (panel A, column 7). These results are consistent with search and matching models where fewer outside options hurt the bargaining position of workers in wage renegotiations. However, we do not find evidence that the share of restricted jobs affects on-the-job wage growth. This finding suggests that the wage penalty of sector and region restrictions affects starting wages. The result is also consistent with wage posting by employers.

varies over the wage distribution and depending on the policy environment. If the biases are constant, then the conclusions about this variation will be valid even if the level is not.

[46]For each job spell and calendar year, our data contain 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.

5.4 Refugee-native wage gap

The weight of the evidence presented in this section suggests that restricting refugees' labor market opportunities lowers their wages because it limits their options to work for many firms. A possible implication is that the policies contribute to explain why refugees are paid less than observationally equivalent natives, which have an unrestricted access to jobs. In Table 6, we test this prediction by relating the policies to the refugee-native wage gap using the Swiss earnings structure surveys. We impose the same sample restrictions as in Table 4. The main difference is that we now include non-refugee workers.

Columns 1, 3, 5, and 7 of Table 6 present estimates of the unexplained wage gap between refugees and Swiss citizens. The variable of interest is an indicator variable that the worker is a refugee. Since the model also contains an intercept for other foreign workers, the coefficient on the refugee dummy reveals the wage gap between Swiss and refugee workers. The first column, which controls for the baseline controls and fixed effects for the survey year and workers' canton of living, suggests a very large wage gap of refugees of more than a third ($100 \times (e^{-0.492} - 1)$ = -38.8%). Half of this gap can be explained by differences between refugees and native citizens in terms of firm tenure, canton of work, and industry (column 3). Two thirds of the remaining gap can be explained by differences in educational attainment, occupations, and management levels (column 5). Column 7 adds firm-year fixed effects. Even if we only compare refugees and natives in similar jobs that work in the same firm and year, the unexplained gap still amounts to 6%. In all specifications, refugees are also paid substantially worse than other foreign workers.

The even columns of Table 6 test whether the unexplained refugee-native wage gap relates to refugees' labor market restrictions. We do this by interacting the refugee dummy with the policies. As in Table 4, we assign the permit-specific policy that applied when the refugee started working for the current company.

The estimates are consistent with an impact of sectoral and regional restrictions on the unexplained wage gap. Refugees allocated to cantons that restrict many potential jobs are paid substantially worse relative to natives compared to refugees allocated to a less restrictive canton. The magnitudes are remarkably similar as in Table 4. The negative wage effect established in section 5.2, which reflected pay differences between refugees in different cantons, translates almost one to one in a larger unexplained wage gap relative to Swiss citizens. The interaction term is negative and statistically significant even if we only compare refugees and native citizens that do similar jobs within the same firm and year (column 8).

Overall, the results suggest that sector and mobility restrictions help to explain why refugees are paid worse than otherwise comparable resident workers in similar jobs.

Table 6: Labor market restrictions and the unexplained wage gap between refugees and native citizens

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

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table uses data from the Swiss earnings structure surveys 2012–2018 to analyze the wage differential between refugees, other foreign workers, and Swiss nationals. The dependent variable is workers' hourly wage. We focus on workers aged 18–65 that started to work in 2005 or later for their current employer. We only keep the observation from the earliest survey if the same worker is observed within the same firm in several surveys. Columns 1 and 2 estimate wage gaps conditional on the baseline controls (gender, gender-specific age and age squared, marital status, the unemployment rate at the start of the spell, and dummies equal to 1 if the canton has a blocking period exceeding 3 months and self-employment restrictions at the start of the spell), and fixed effects for the survey year and workers' canton of living. Columns 3 and 4, additionally, contain fixed effects for the year in which the worker joined the firm, the canton of work, and two-digit industry codes. Columns 5 and 6 additionally contain fixed effects for eight levels of educational attainment, ISCO two-digit occupation, and five management levels (no, lowest, low, mid- and highest-level management). Columns 2, 4 and 6 also show interactions between the labor market restrictions and the refugee indicator. We use the status-specific policies that were in place when the workers started to work at their current employer. All policy variables are set to zero for non-refugee workers. Standard errors are clustered at the initial canton of living × worker group level. We differentiate three groups: N → B refugees, N → TAR/TAF refugees, and the remaining workers.

6 Costs and benefits

6.1 Long-run effects

One important question is whether restricting the access to the labor market for refugees has effects beyond the period when these restrictions actually apply and, thus, may reduce refugees' long-term labor market integration. To capture the policies short-, medium-, and long-run effects, we estimate the effects over a period of ten years after arrival in Switzerland. We measure initial exposure as the policy environment in the first year after arrival, i.e., (i) the presence of prioritization in the first year, (ii) the duration of each individuals' employment ban (in months), and (iii) the share of jobs restricted by sectoral employment bans or the prohibition to work in neighbouring cantons. To ensure that we can follow individuals at least ten years, we restrict the sample to asylum seekers with entry years 1999 to 2005 who we are still in Switzerland and, thus, in our data in 2015. We use the following specification to relate refugees' labor market outcomes to initial labor market policies:

$$y_{it} = a_\tau + b_\tau' \mathrm{P}_{cT(i)} + d_\tau \bar{u}_{cT(i)} + \pi' w_i + \mu_c + \delta_t + \nu_{it} \tag{3}$$

where y_{it} is annual employment or annual labor income observed in year t for refugee i. The index $\tau = \tau(i,t)$ denotes the number of years since arrival in Switzerland for $\tau \in \{1, \ldots, 10\}$. The vector $\mathrm{P}_{cT(i)}$ measures initial policy exposure, which depend on the time of arrival $T(i)$ and the assigned canton c. Note that initial conditions do not depend on the asylum decision, but are the same for all asylum applicants arriving at the same time and assigned to the same canton. We benchmark the effects from the initial policy environment with the scarring effects from initial labor market conditions by including $\bar{u}_{cT(i)}$, which is the initial unemployment rate averaged over the first 12 months after arrival. In section 6.2, we also explore the effect of initial conditions on emigration.

The fixed effects a_τ describe the path of labor market integration as a function of years spent in Switzerland in the absence of labor market restrictions. The vector b_τ reflects deviations from the typical integration path due to initial policy exposure. Similarly, d_τ measures deviations from the integration path due to initial labor market conditions as in Von Wachter (2020). The individual characteristics w_i are age, age-squared as well as month-to-decision and squared months-to-decision. We control for year and canton fixed effects. We also present a more restrictive specification where we interact year and canton fixed effects to capture contemporaneous labor market conditions and policy restrictions. As in the short-run models, we pool permit transition groups (N→B, N→TAR, N→TAF) by interacting canton, cohort, and year fixed effects with permit status. We completion, we present separate estimates by permit transition groups in appendix F.[47]

[47]Due to the small sample, we do not present results for the N→TAR group.

6.1.1 Long-run effects on employment and earnings

Figure 9 plots the effects of the labor market policies on employment and earnings as the deviation from the typical, concave labor market integration profile in each year after arrival. Note that due to different samples, the effects reported here in the first five years can slightly deviate from the effects reported in section 4. Alongside our baseline specification with canton and year fixed effects (shown as "additive" circles), we also present estimates that include canton-year-interactions (shown as "multiplicative" coefficients). The latter absorb effects of contemporaneous labor market restrictions or contemporaneous economic conditions (similarly as in Oreopoulos et al., 2012). We make the following observations:

First, *prioritization* has no effect in the first year after arrival when average employment levels are typically very low, around 5%. In year 2 and 3, prioritization reduces employment by about 8-11 p.p. Since average employment in these years is usually around 18%, this represents a 50% decline in employment. In these two years, panel B also shows a strong reduction in earnings around 80-150%. The effects on both outcomes peters out after year 3 and are not statistically different from zero.

Second, *employment bans* affect employment and earnings most strongly in year 2 and, to lesser extent, also in year 3-5. The effect is zero in later years. Evaluated at the median duration of 3 months, bans lower employment by roughly 3 p.p. (3×1 p.p.) and earnings by roughly 30% (3×10%) in year 2 and about half to a third of that magnitude in year 3-5. The dynamics of these effects compare well with those reported in Fasani et al.(2020) for European countries which also have the strongest negative impact in the first years after arrival and fade out after year 7.[48]

Third, also the initial *share of restricted jobs* lowers earnings in the first two years and employment in the second year. Unlike the other two policies, restricting employment to sector or by residency already creates a negative effect in the first year on earnings. Although these point estimates are clearly negative, the large standard errors do not allow to rule out a zero effect in these initial years. The detrimental effects of restricting the the share of available jobs fades out in year 3.

Fourth, we can benchmark the effects of the policies with effects from a weak local economic performance at an individual's time of arrival which could also severely reduce the number of available job options, particularly for vulnerable populations (Von Wachter, 2021; Schwandt & Von Wachter, 2019; Aslund & Rooth 2007; Barsbai et al., 2022). We proxy economic conditions with the cantonal unemployment rate during an individual's first year in

[48] In terms of magnitude, Fasani et al. (2020) find that employment bans reduce employment by 24 p.p. in year 2-4 post entry, declining to a reduction of 19 p.p in year 5 to 7 and fading out thereafter. Since the median ban duration in their sample is around 10 months, this would be consistent with a 2.4 p.p. reduction per month ban duration in year 2 to 4 or roughly double the effect we find. Effect sizes are hard to compare very precisely, however, since about in a third of their county-year observations employment bans have an indefinite length.

Switzerland. Figure 9 shows that the economic situation at arrival already impacts employment and earnings in the first year after arrival and peaks in year 2 at an effect of 4.8 p.p lower employment and 72% lower earnings for a rise in unemployment (of +1.2 p.p.) like in the Great Recession.[49] In the years 3-6 after arrival the effect of initial economic conditions weakens a little bit and fades out if we control for effects from contemporaneous economic conditions but shows a permanent reduction if we do not control for such effects.

Fifth, the estimates based on the specifications that including canton and year interactions generally follow a similar dynamic and have roughly a similar effect sizes as the baseline estimates (based on additive canton and year fixed effects). This alleviates concerns that the baseline estimates compound the effect of the initial policy environments with potentially auto-correlated effects from more contemporaneous policies. An alternative way of testing the evidence for this concern is to evaluate effects only for refugees transiting from asylum seekers (permit N) to recognized refugees (permit B). For this group, essentially all restrictions fall away when they receive refugee status recognition. In other words, there are no contemporaneous employment restrictions whose effects could compound the effects from the initial policy environment. Figure F.1 present estimates for this group. While confidence intervals are larger due to the smaller sample of this group, the effects of the policies follow a similar qualitative pattern as those in figure 9 for all transition group. For this group (N→B), the effects of employment bans are generally larger (more harmful) for both employment and earnings and last until year six. This is remarkable given that most employment bans only last 3 months. Also, prioritization shows more persistent negative effects on employment that last until year 5 after arrival.

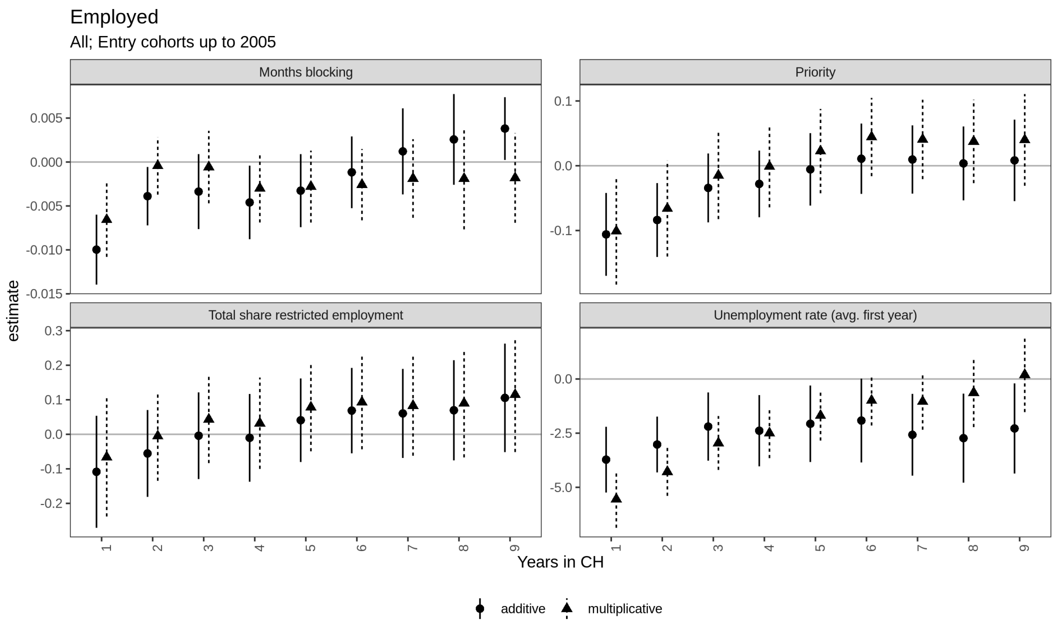
In sum, we observe strong harmful effects on both employment and earnings during the first years after arrival for both employment and earnings of refugees, particularly of employment bans and prioritization but also further sectoral and residency based restrictions. Although some of the policies only restrict employment in the very first period after arrival, their effects are detectable during the first 5 to 7 years after arrival. Even though the effects from the initial employment restriction fade out after this period, they leave considerable gaps in cumulative earnings. We quantify these gaps in potential earnings in section 6.3.

6.1.2 Long-run effects on wages and education

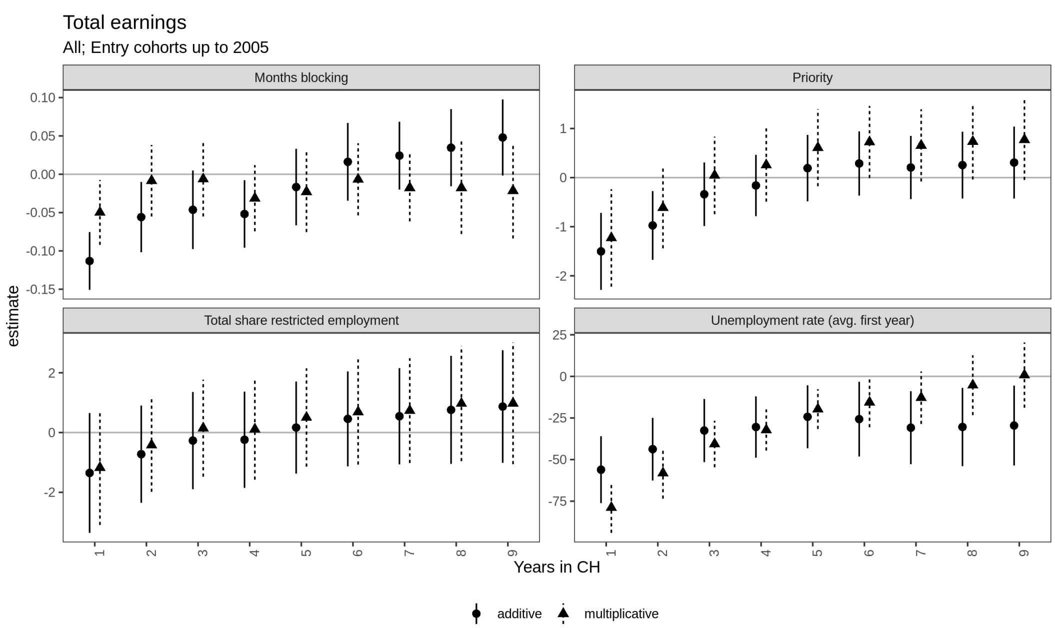
Next, we explore the dynamic long-run effects on refugees' hourly wages using data from the Swiss Earnings Structure Surveys (SESS) 2012, 2014, 2016, and 2018. Mirroring the long-run regression specification for the social security data (equation 3), we relate workers' wages as

[49] Note that this effect is considerably larger than those reported in e.g. Von Wachter (2021) who found that a typical 3-4 p.p. rise in the unemployment rate in a recession, decreases earnings by 10-15 percent in the first year of labor market entry. We believe that such large effects on refugees' employment and earnings are expected, given this is a particularly vulnerable group of potential workers whom employers might be particularly hesitant to hire during times of high economic uncertainty.

Figure 9: Short- and long-run effects of initial policies and unemployment on annual labor earnings and employment



(a) Effects on employment



(b) Effects on earnings

Notes: This figure shows the effects of the labor market restrictions on employment (panel A) and earnings (panel B) of refugees in year 1 to 10 after their arrival in Switzerland based on the specification (3). The figure also shows the effects of the average cantonal unemployment rate during the first years after arrival. The sample pools refugees from all permit groups (N→B, N→TAF, and N→TAR) arriving between 1999 and 2005 and still residing in Switzerland in 2015. We control for age, age-squared, wait months until decision (in months) and squared.

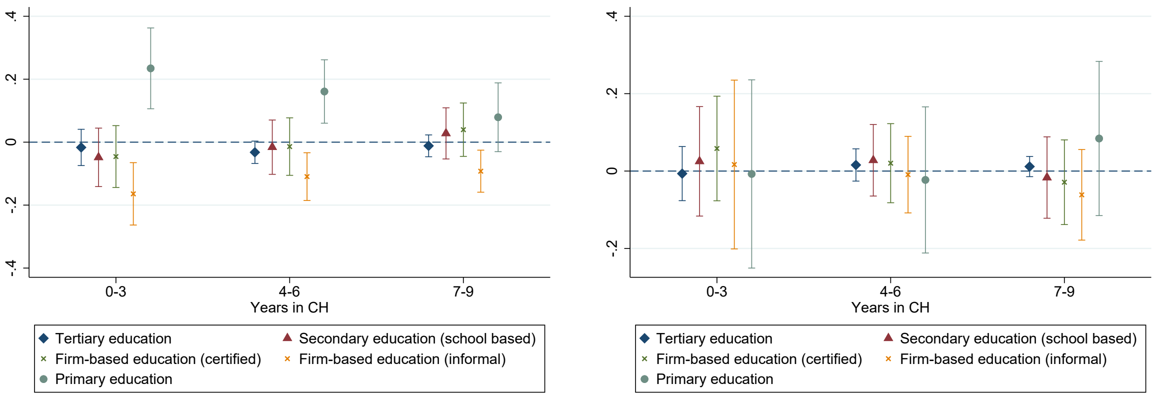
observed in the survey waves 2012–2018 to the policies for asylum seekers (status N) in place in the canton the refugees were initially assigned after arrival in the years 2002-2008. The sample is refugees that transition from asylum seeker (permit N) to a B permit or from N to either a TAR or TAF permit. Outcomes and policies are measured in October of each year. We control for status group fixed effects (N to B and N to TAR/TAF, respectively), initially assigned canton, arrival cohort, and survey wave fixed effects interacted with dummies for the two status groups.[50]

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

Figure 10 highlights that lower human capital accumulation could be one potential mechanism explaining how longer initial employment bans reduce wages. In particular, longer employment bans reduce the probability that refugee workers have acquired informal, firm-based informal secondary education (i.e., on-the-job training) by a bit less than 20 p.p. in the years 0–3 after arrival. Conversely, individuals are about 20 p.p. more likely to have only a primary education. This effect reduces a little bit in the years 4–6 and remains significant also thereafter. Also, the effect on having a tertiary education is significant and negative in the years 3-5 after arrival, even though the point estimate is quite small. It is important to keep in mind that the SESS data used for this analysis only allow to track workers (but not the entire population). Thus, part of the effect that we see in this figure could come from effects of employment bans on the composition of refugee employment.

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

Figure 10: Effect of labor market policies on educational attainment of workers



(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 data from the Swiss labor market surveys 2012–2018. Mirroring the long-run regression specification for the social security data (equation 3), we relate workers' degrees as observed in the survey waves 2012–2018 to the policies for asylum seekers (status N) in place in the canton the refugees were assigned to when arriving in Switzerland. 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. The two panels show the interaction terms between the initial policies and three indicators of the years after arrival in Switzerland (0–3, 4–6, and 7–9 years in Switzerland). Panel a) shows the effects of initial employment bans, panel b) those of the share restricted jobs. The sample is refugees that transition from asylum seeker (N) to a B permit or from N to either a TAR or TAF permit. Outcomes and policies are measured in October of each year. We control for status group fixed effects (N to B and N to TAR/TAF, respectively), (initial) canton, cohort, and survey wave fixed effects interacted with dummies for the two status groups. Baseline controls are gender, gender-specific age and age squared, refugees' marital status, the unemployment rate in the year of arrival, the initial level of social assistance in the assigned canton, and a dummy equal to 1 if the canton has self-employment restrictions at arrival. We also control for the three indicators of years in Switzerland. Standard errors are clustered at the canton $\times$ status-group level.

6.1.3 Placebo Tests with Immigrants from EU countries

In this section, we conduct a series of placebo tests to validate our long-run estimates provided in the preceding section. One concern with a causal interpretation of our results is that labor restrictions for refugees might coincide with hidden labor market conditions, thereby confounding our estimates of the policies' effects. To test for this, we leverage a sample of immigrants from EU-15 countries. EU-15 citizens are allowed to locate in Switzerland since 2002 under the Agreement on the free movement of persons. However, EU-15 citizens are not subject to the labor restrictions for refugees. Thus, would expect these policies to have no, or at least no negative, effect on EU immigrants. As a by-product, this analysis also allows us to shed initial light on the question whether non-refugee immigrants benefit from labor restrictions for their refugee competitors.

One potential issue with using EU immigrants for a placebo test is that they likely differ from refugees with regard to relevant labor market characteristics. We address this concern by analyzing the (placebo) effects of the policies using unconditional quantile regression, which allows to detect effects at the lower end of the EU immigrants' wage distribution—if they exist.

For this analysis, we employ the linked AHV-STATPOP dataset which covers all EU-15 immigrants residing in Switzerland. The design of the placebo test resembles the long-run model in equation (3) for refugees as closely as possible. However, the placebo test can only consider the study period after 2005 since the canton of residence is not contained in STATPOP for earlier years.[51]

Figure 11 shows the results from the quantile regression the first, second and third quartile for employment (Panel a) and total earnings (Panel b). Numerical estimates are provided in Appendix Table E.1. In line with the long-run model for refugees, separate regressions are run for residency years 1–7. Panel (a) shows that for all three policies—months blocking, prioritization, and total share restricted employment—restricting refugees' labor market access, we find no evidence for significant placebo (nor positive spillover) effects on employment for EU-15 migrants. This pattern holds across all quartiles, including the first which consists of EU-15 migrants that are arguably more similar to refugees in labor market characteristics. With partial exception of the first employment quartile, the estimation of these effects is sufficiently precise d to rule out all but very small effects. For comparison, the final subpanel in Figure 11 (a) shows the long-run effects of the initial labor market conditions. In contrast to the null effects for refugees' labor market policies, we see that higher unemployment rates at arrival durably reduce employment for EU-15 immigrants in the lower two quartiles. Panel (b) shows a similar pattern for total earnings. Among the three policies considered, we again find no significant placebo or spillover effects for any of the three earnings quartiles, but durable repercussions of bad conditions at arrival (again for the first and second quartile).

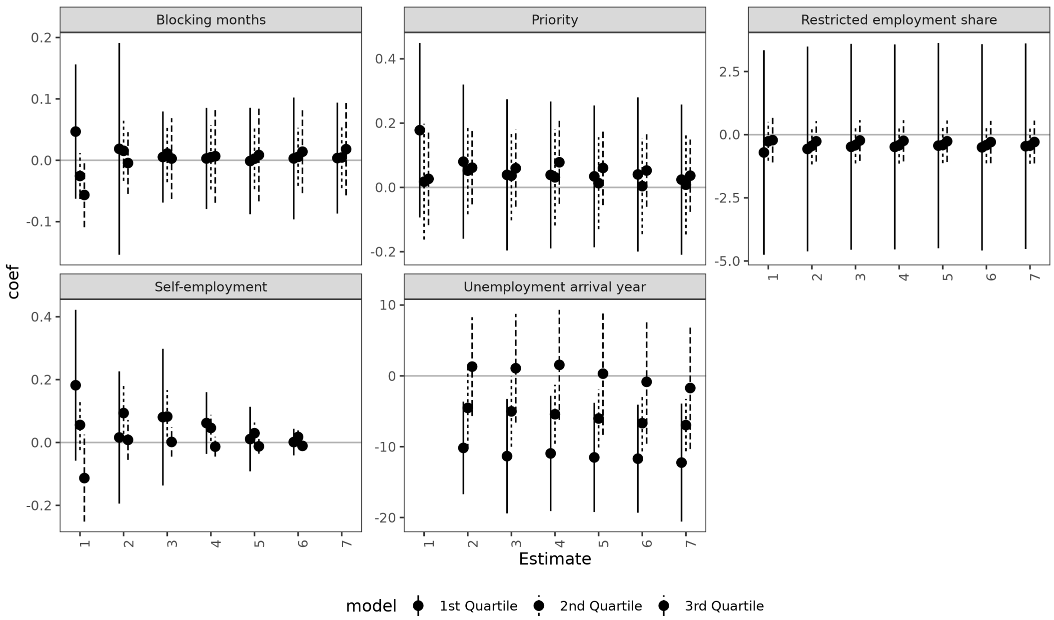
Together, these results provide two implications. First, when interpreted as a placebo test, they corroborate that our estimates of the impact of restrictions for refugees' labor outcomes have a causal interpretation and are unlikely confounded by hidden trends in labor market conditions. Second, when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

6.2 Effects on emigration

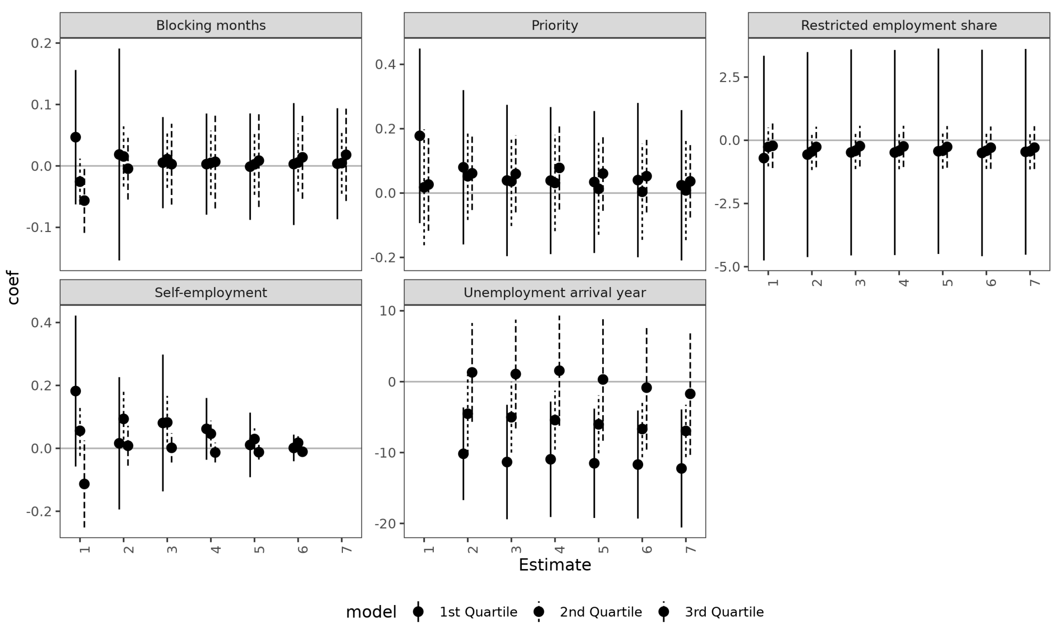
In this section, we investigate whether labor market policies at arrival increase the probability of leaving Switzerland, both in the short- and long-run. A more restrictive labor market and

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

Figure 11: Long-run placebo using EU-15 immigrants who are not affected by labor restrictions



(a) Effects on employment



(b) Effects on total earnings

Notes: The sample includes all EU-15 immigrants arriving in 2005 or later and who still reside in Switzerland in 2015. The estimation model is given by (3). We control for age group interacted with gender.

difficulty at finding employment could increase the probability of returning to the origin country or move to another country even years later.

Since emigration of refugees is not consistently recorded in ZEMIS, we draw from both the AHV and the STATPOP register to measure emigration. First, based on the AHV register, we approximate a person's emigration with an indicator if she does not appear in the register for at least five years.[52] Secondly, in the STATPOP register we are able to measure emigration directly but only for the years 2010–2015. Thus, for this data set we include an indicator for whether a person emigrated in these years.

Figure 12 the shows the long-run effects of labor restrictions on these measures for emigration based on AHV data (panel A) and STATPOP data (panel B), respectively. Generally, these is little evidence that labor restrictions lead to emigration. Prioritization seems to reduce the likelihood of emigration, if anything. These effects, however, are only significant in year 7 (panel A) (or in year 8 in panel B, respectively) in the specification multiplicative specification. In the case of employment bans, there is a small positive and marginally significant effect towards the end of the observation period (in both panels). Yet, even evaluated at the median ban duration of 3 months, employment bans increase the likelihood of emigration by less than halve of a percentage points. Lastly, the figure shows that there is not evidence that sector or geographic restrictions induce emigration.

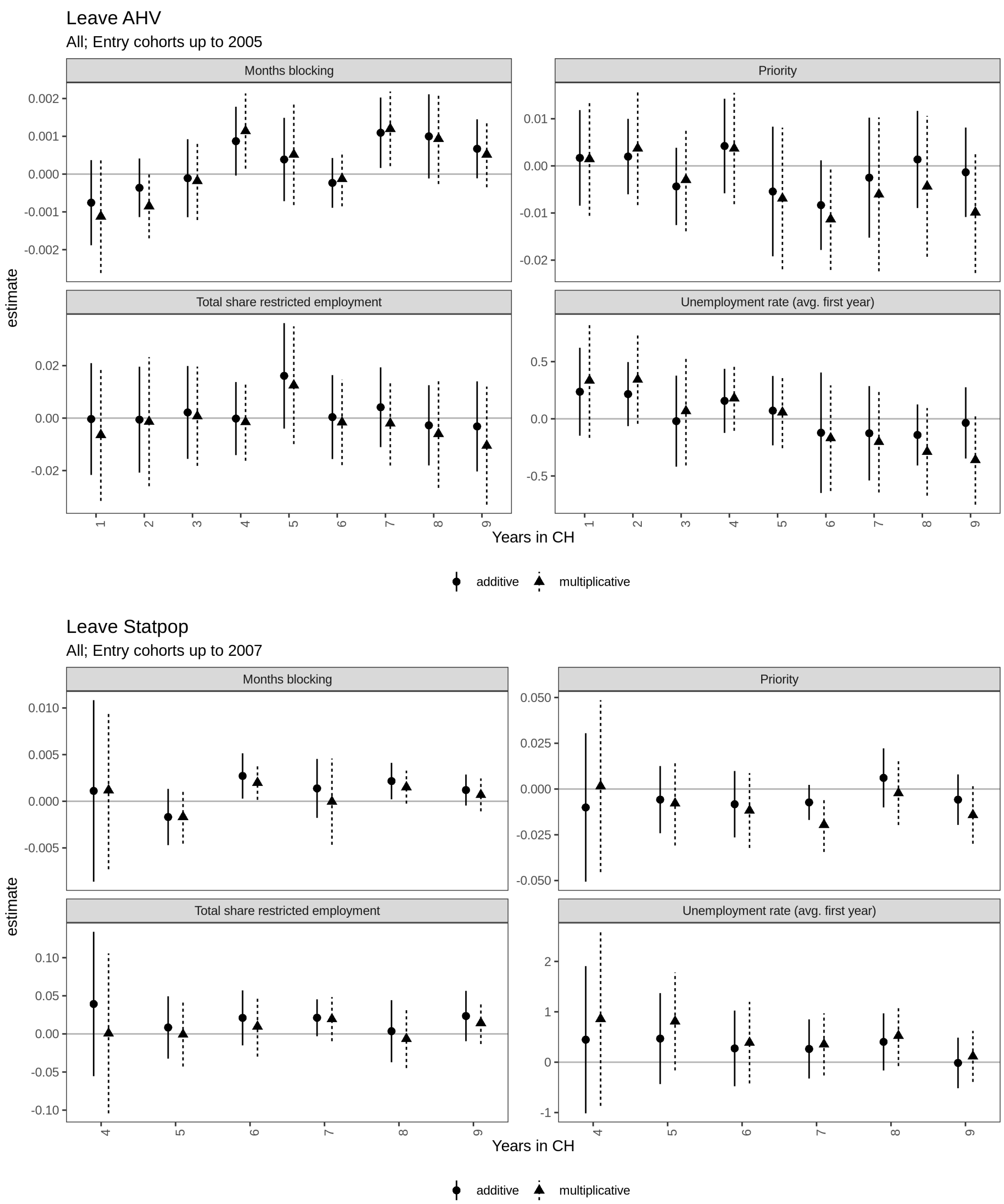
Taken together, our estimates suggest that restricting refugees labor market access does not induce them to leave the country. The absence of an emigration response renders the labor restrictions potentially very costly to host societies, since refugees with little labor income will stay in the country and support themselves with transfer payments and social assistance. We now explore the aggregate costs to host societies of restricting refugees labor market access in greater detail.

6.3 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: a liberal scenario without any labor restrictions, the "status quo" scenario with the actual labor restrictions as observed during our study period, and 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 Solothurn between 1999 and 2004. In this period, Solothurn enforced the priority requirement, had a 14-month employment ban, and restricted 18% of

[52] Aside from emigration, there are only two reasons for a removal from the AHV register. The first is death. The second can arise because of 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.

Figure 12: Short- and long-run effects of initial policies and unemployment on emigration



Notes: The sample includes all cohorts arriving in 2005 or later and individuals who reside in Switzerland in 2015. The estimation model is given by (3). We control for age, age-squared, wait months until decision (in months) and squared.

Panel A. Total earnings (CHF)	*Mean*	*Total (Mio)*
Status quo	16202.00	1189.95
No restrictions	20269.80	1488.71
Most restrictive	7965.30	585.01
Difference: no restrictions vs status quo	4067.80	298.76
Difference: no restrictions vs most restrictive	12304.50	903.70
Panel B. Social costs (CHF)	*Mean*	*Total (Mio)*
Status quo	15419.30	1132.47
No restrictions	13994.60	1027.84
Most restrictive	23912.00	1756.22
Difference: no restrictions vs status quo	-1424.70	-104.63
Difference: no restrictions vs most restrictive	-9917.40	-728.38
Panel C. Employment months	*Mean*	*Total ('000)*
Status quo	6.30	465.26
No restrictions	7.60	555.87
Most restrictive	3.60	263.27
Difference: no restrictions vs status quo	1.20	90.61
Difference: no restrictions vs most restrictive	4.00	292.59

Table 7: Predicted employment, welfare costs, and earnings under three policy scenarios

jobs for refugees.[53] The scenario is intended to provide a plausible upper bound on the aggregate effect that labor restrictions could have if all cantons had followed this highly restrictive set of policies. We calculate predicted employment rates under these three scenarios based on the baseline specifications with canton, month, month-since-arrival, and months-to-decision fixed effects (Table 3, col. 5).

Table 7 shows the predicted employment months, social aid expenditures and total earnings under the liberal, status quo, and restrictive scenario. On aggregate, we find that the observed (status quo) labor market restrictions have reduced total earnings by CHF 289.76 Mio over the years 1999-2015. For this calculation, we only consider refugees receiving an F or B permit and the first five years after arrival. On a per-person basis, this amounts to CHF 4,067.80 in lost labor earnings per refugee during the first five years in Switzerland compared to a scenario without any labor restrictions. In terms of employment, refugees lost on average 1.2 employment months due to labor restrictions over the 60 months period (mean=11 months). The reduction in labor activity also implies an increase in welfare transfers (for social aid) of at least CHF 1424.7 in direct cash payments per person (this figure does not include non-cash benefits).

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 903.7 Mio in

[53]In addition, self-employment was prohibited for N and TAF status refugees.

total foregone earnings (CHF 12,304 per capita) and 4 employment months, which implies an increase social aid cash transfers by CHF 728.4 Mio.

These back-of-the-envelope calculations come with limitations: First, the social aid costs only include a small amount of total fiscal costs. They exclude non-cash payments such as housing costs, health insurance, and integration support. Similarly, unemployment benefits for refugees who have already accumulated more than twelve employment months since arrival are not considered for this analysis. Furthermore, social aid transfer for the many asylum seekers whose asylum claim is rejected and do no receive a F or B status are also not included. Second, we do not take potential crowding out effects into account that may arise if reduced employment for refugees leads to higher employment and earnings for non-refugee workers that may compete with refugees for jobs. Third, we do not explicitly incorporate emigration. However, as shown in the previous section, the labor restrictions considered here do not seem to trigger emigration. Fourth, the analysis above focuses on refugees and does not consider their tax contributions when employed.

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.

7 Conclusion

This paper analyzes the employment, wage, and job mobility effects of labor market policies regulating whether, where, and for whom refugees are allowed to work. The analyses are based on newly collected data on labor restriction policies in Swiss cantons, covering the period 1999–2016. We analyze four distinct policies: temporary employment bans that prevent refugees from working in the first months after arrival, prioritization of citizens and foreigners with more secured residence permits, sector restrictions, and restrictions on geographic mobility. We merge these policies to high-quality administrative data on refugees' asylum processes linked with earnings records, which provide refugees' full earnings histories, as well as linked employer-employee data.

Using a range of rich, individual-level panel regression specifications with multiple fixed effects, we find evidence that all policies have negative employment and earnings effects. The estimated effects are substantial in size: moving from the least to the most restrictive policy mix reduces refugees' average employment rate from 23% to 16%. The effects are concentrated among refugees that we predict to have comparatively high chances to be employed. We also find that sectoral and geographical restrictions lower refugees monthly earnings when employed. Using a complementary dataset that provides high-quality information on wages and hours, we find that this effect stems from a reduction in the hourly wage rate, not a reduction in hours worked per month. For policies prioritizing the resident workforce over asylum seekers and refugees, we find no depressing effects on hourly wages, but at the cost of reduced hours worked per month, lower employment, and lower wage growth on the job. Together, our findings suggest that these widespread, restrictive policies are an important reason for why refugees are paid less than similar native citizens.

We also explore several monopsonistic explanations for the effects we document. Our evidence is generally consistent with dynamic models of monopsony Manning (2003). The negative wage effects of the priority requirement seem to arise because the policy reduces refugees' chances to switch to better-paid jobs and to experience within-job wage growth. The negative wage effects of restricting the share of jobs available to refugees seem to arise because these restrictions reduce refugees' opportunities to work their way into more demanding occupations and into positions with supervisory duties. We also find evidence that sectoral and geographical restrictions increase the concentration among employers of refugees, consistent with static models of monopsony.

Finally, we take initial steps towards exploring the costs and benefits of labor restrictions for refugees and host societies. We present three pieces of evidence suggesting that labor restrictions burden both refugees and host communities with significant costs. First, we find that restrictive labor market policies impair refugees' economic integration even in the medium-run. In line with a large literature that shows that adverse initial labor market conditions leave long-term scars among unlucky cohorts (Von Wachter, 2020), the priority and blocking policies reduce refugees' labor market earnings up for to five and six years, respectively, after they cease applying. Second, we find that labor restrictions have no—and if anything even a negative—effect on the probability that refugees emigrate. These results hold also for refugees that are only temporally admitted. This finding stands in stark contrast to the view popular among some policy makers that labor restrictions provide incentives for refugees to leave the country, and that they are particularly effective for refugees that were granted only temporary permissions to stay (Marbach and Hangartner, 2019). Finally, we provide back-of-the-envelope calculations suggesting that the observed labor market restrictions imply missed total labor earnings among refugees of 72.9 million Swiss franc over the 2006–2015 period.

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monopsony.0319-10111R1. URL http://jhr.uwpress.org/content/early/2021/04/05/jhr.monopsony.0319-10111R1.abstract.

Supplementary Material

Contents

A Descriptives

A.1 Policies

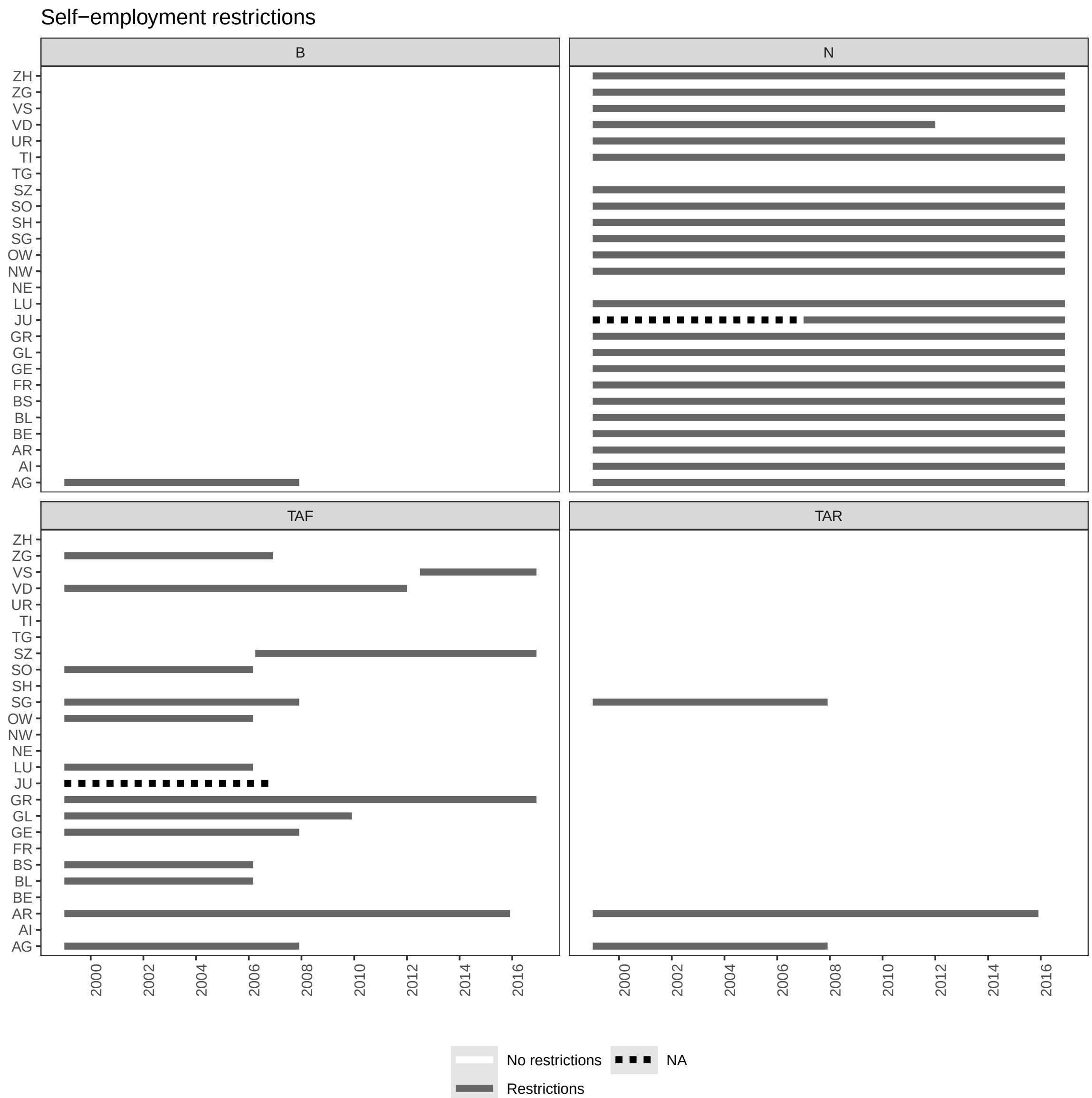


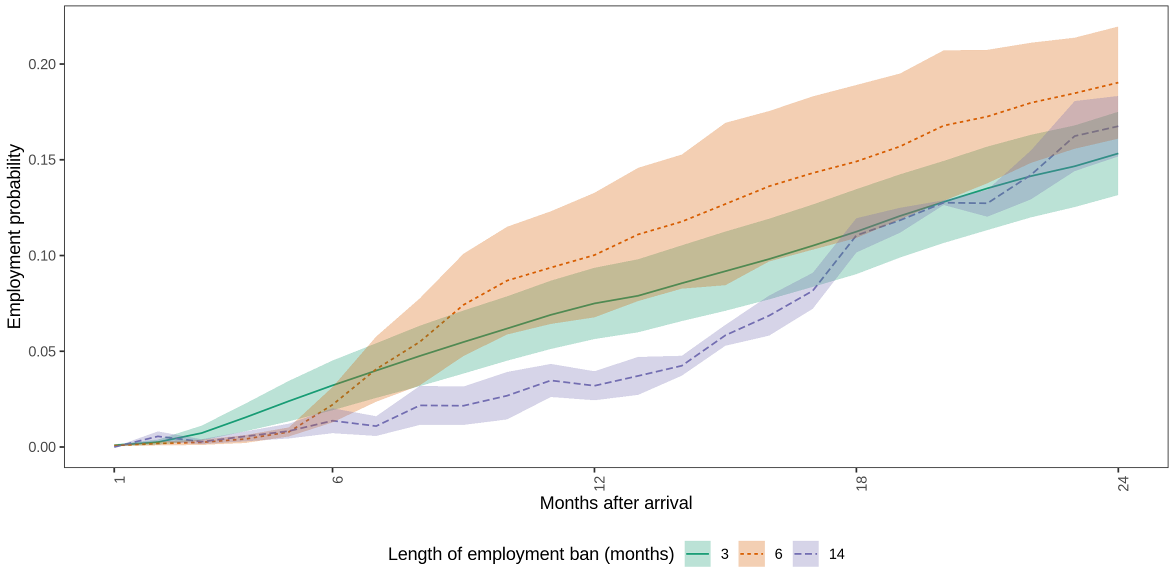
Figure A.1: Self-employment restrictions by canton and status

	Change without transition		Change with transition		Overall	
Status group	Less restrictive	More restrictive	Less restrictive	More restrictive	restrictive	restrictive
Panel A. Prioritization						
N->B	0.06	0.16	57.65	0.00	57.71	0.16
N->TAF	8.15	0.29	38.07	0.03	46.22	0.32
N->TAR	0.12	0.27	62.39	0.00	62.50	0.27
Panel B. Restricted share						
N->B	14.84	8.25	83.44	0.00	83.45	8.25
N->TAF	38.77	5.56	81.38	0.00	82.57	5.56
N->TAR	27.10	7.58	87.69	0.00	87.77	7.58
Panel C. Sector-restricted share						
N->B	1.19	0.23	21.66	0.00	22.84	0.23
N->TAF	4.74	0.26	19.21	0.00	23.84	0.26
N->TAR	1.10	0.09	22.45	0.00	23.55	0.09
Panel D. Region-restricted share						
N->B	12.05	2.59	83.48	0.00	83.49	2.59
N->TAF	34.48	1.71	83.27	0.00	84.19	1.71
N->TAR	25.58	2.98	87.71	0.00	87.78	2.98

Notes: Panel A to D show the share of refugees (in %) that experience a change in policies when transitioning to B, TAR or TAF (i.e. after asylum decision) and that experience a policy change without status transition change.

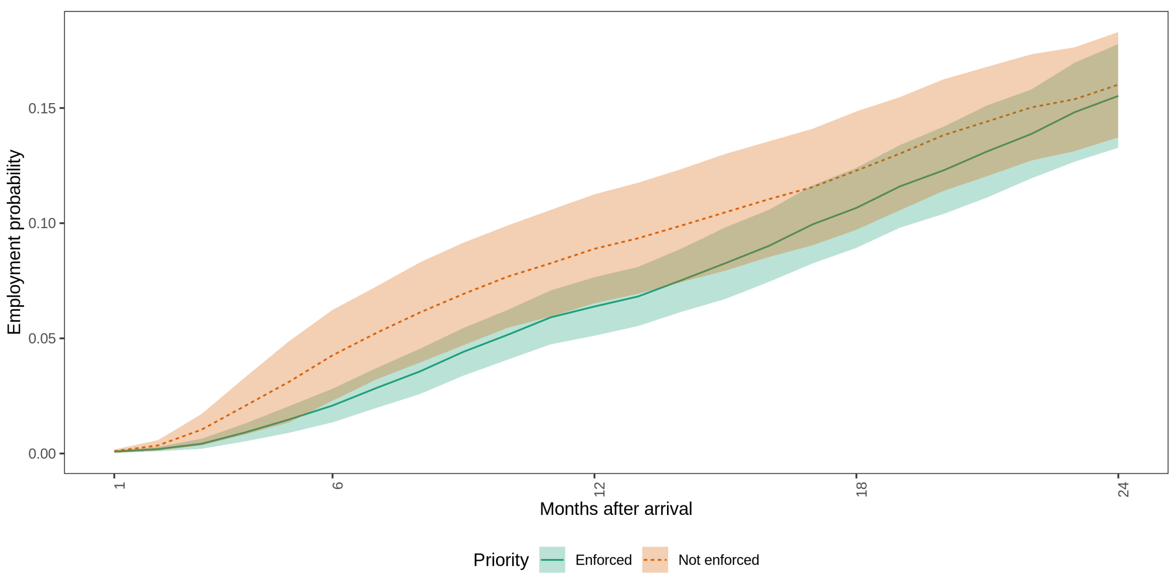
Table A.1: Policy variation: Share (in %) of refugees experiencing a policy change with and without status transition

A.2 Employment paths



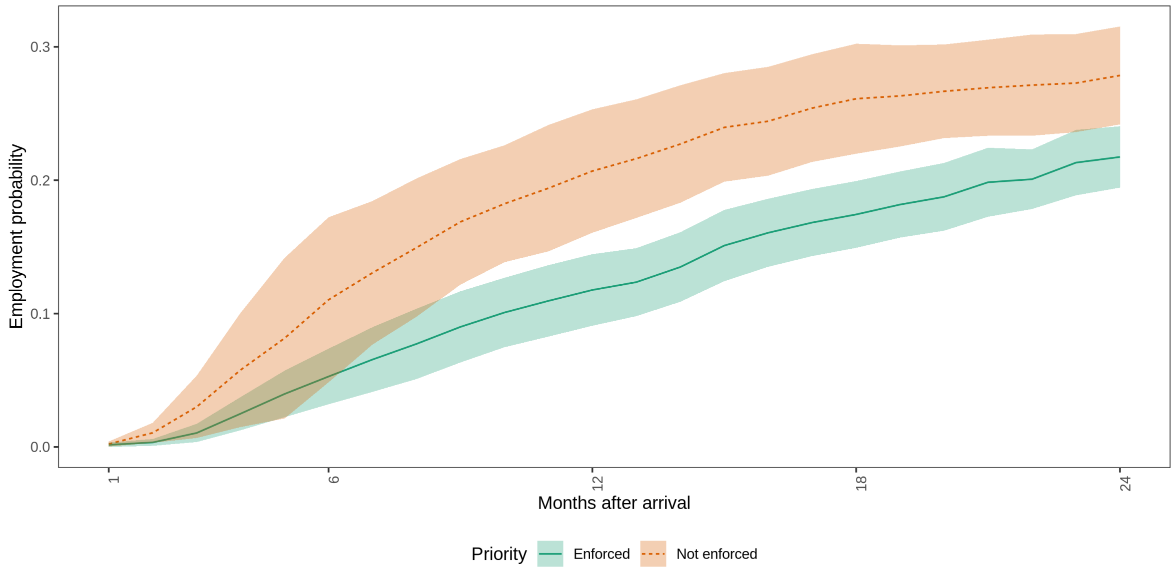
Notes: The underlying model regresses employment status against months-since-arrival fixed effects interacted with the initial employment ban policy (3, 6 or 14 months). We exclude individuals who arrived during a full employment ban. Standard errors are clustered at the canton × status group level.

Figure A.2: Employment probability since arrival by initial employment ban



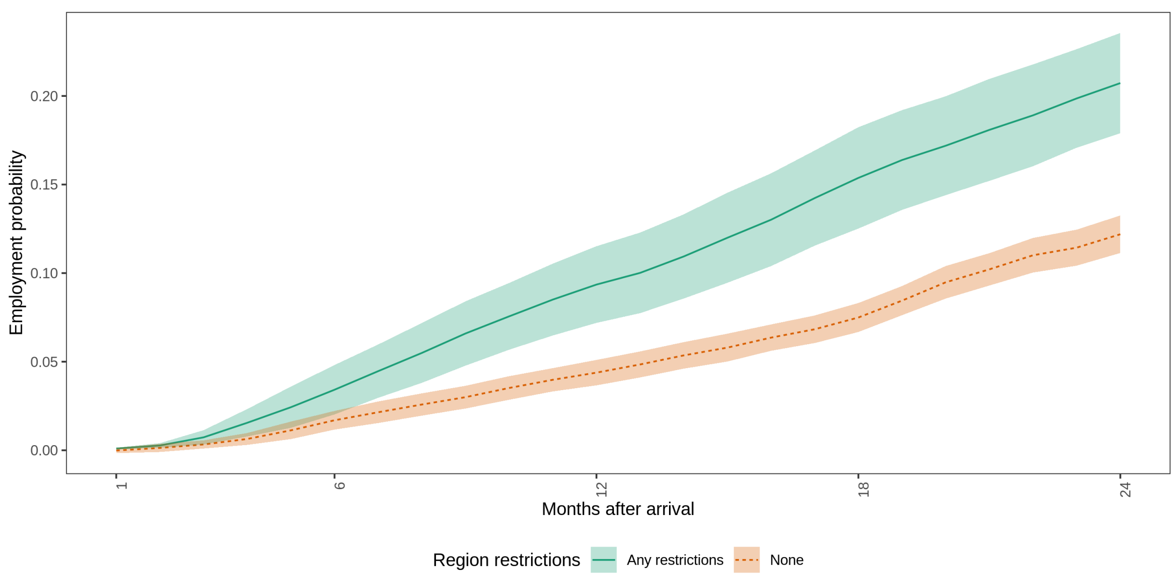
Notes: The underlying model regresses employment status against months-since-arrival fixed effects interacted with a binary priority policy indicator. Standard errors are clustered at the canton × status group level.

Figure A.3: Employment probability since arrival by priority policy



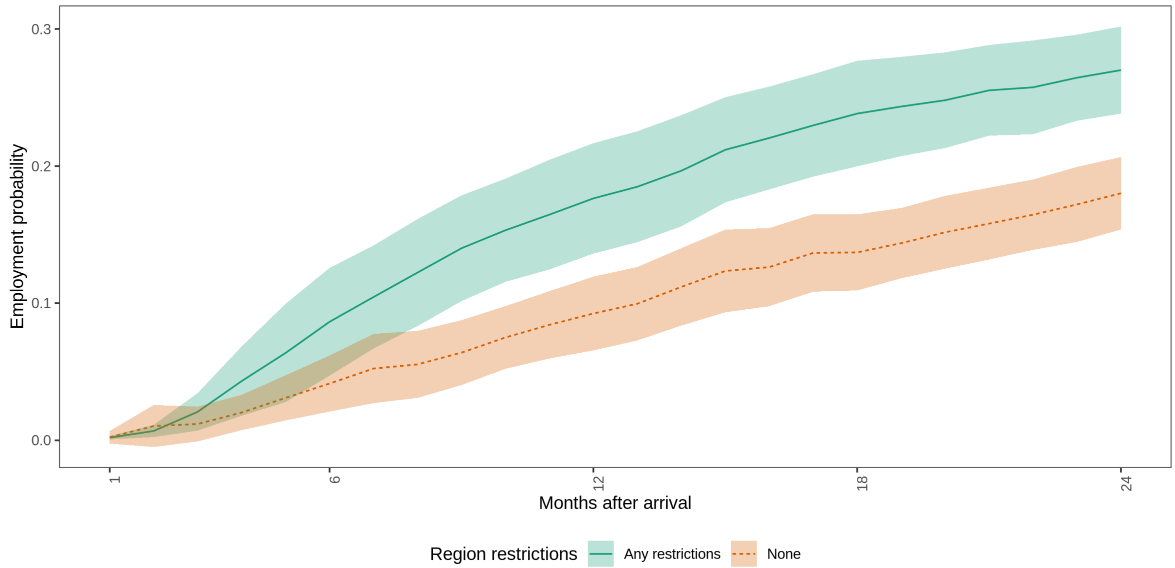
Notes: See previous figure. Only using sample 1999-2006.

Figure A.4: Employment probability since arrival by priority policy using sample 1999-2006



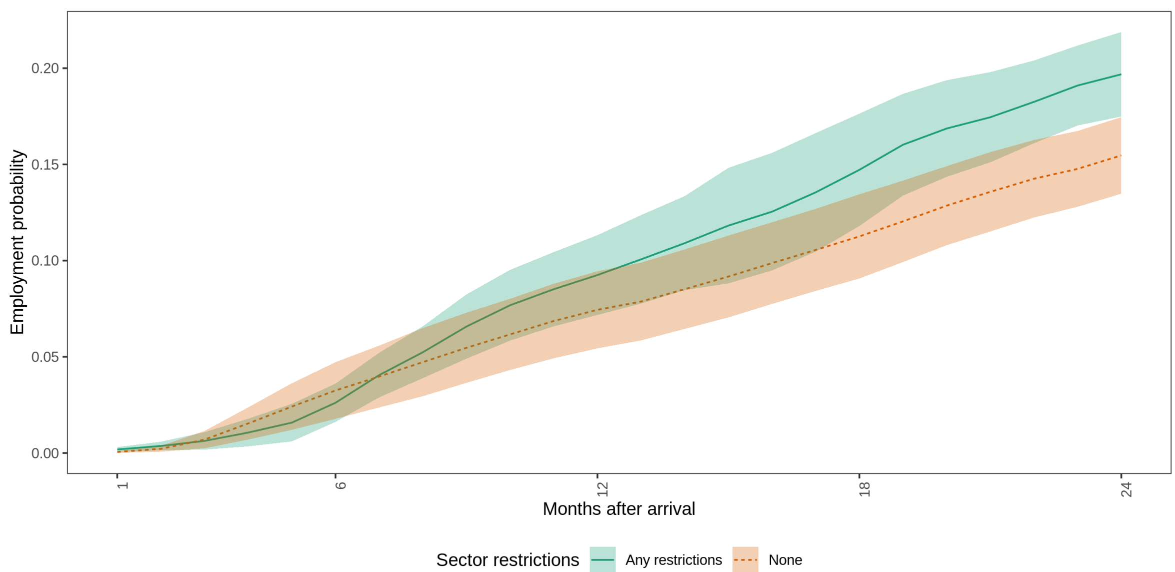
Notes: The underlying model regresses employment status against months-since-arrival fixed effects interacted with a binary indicator that equals 1 if the refugee is restricted from working in at least one neighboring canton, 0 otherwise. Standard errors are clustered at the canton $\times$ status group level.

Figure A.5: Employment probability since arrival by region policy



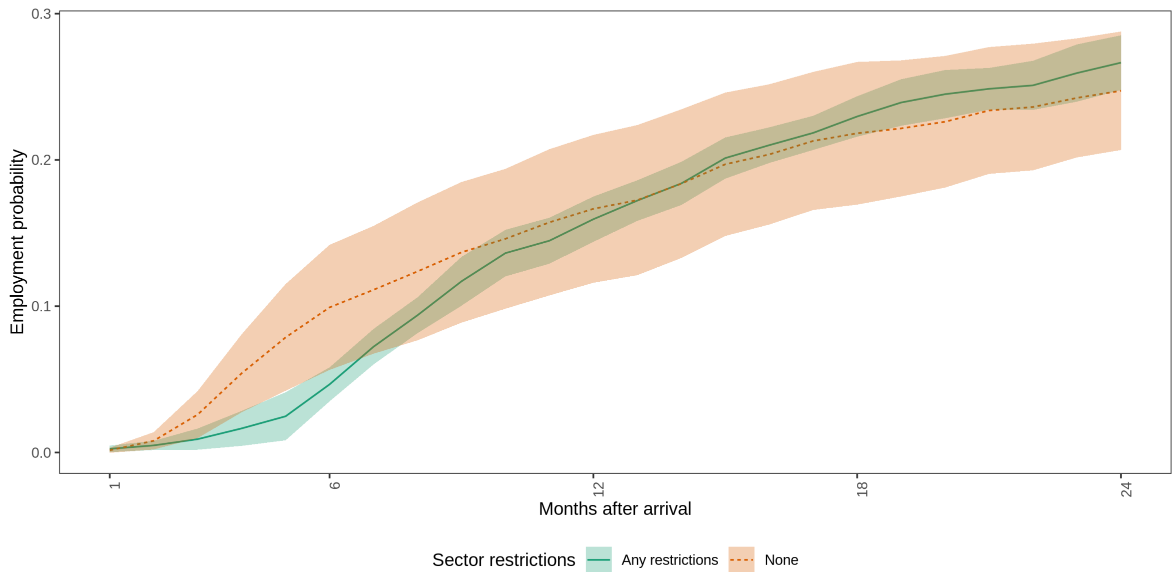
Notes: See previous figure. Only using sample 1999-2006.

Figure A.6: Employment probability since arrival by region policy using sample 1999-2006



Notes: The underlying model regresses employment status against months-since-arrival fixed effects interacted with a binary indicator that equals 1 if the refugee is exposed to any sector restrictions, 0 otherwise. We include fixed effects for the allocated canton × outcome months. Standard errors are clustered at the canton × status group level.

Figure A.7: Employment probability by months after arrival



Notes: See previous figure. Only using sample 1999-2006.

Figure A.8: Employment probability by months after arrival using sample 1999-2006

A.3 Merged short and long-run data

Table A.2: Descriptive statistics of refugee and placebo long-run data set.

	Mean	Sd.	$P_{.01}$	$P_{.50}$	$P_{.99}$	Obs.
Panel A. Long-run refugee data set (2005)						
Labor income	24591.54	19002.57	262.00	21786.00	68244.27	5152
Employed (AHV)	6.65	5.04	0.00	7.00	12.00	6877
Age	32.20	7.66	19.00	31.00	53.00	13952
Female	0.39	0.49	0.00	0.00	1.00	13952
Panel B. Long-run refugee data set (2015)						
Labor income	34007.90	23169.41	323.87	34303.50	88096.59	17888
Employed (AHV)	7.88	5.08	0.00	12.00	12.00	23047
Age	37.98	8.61	23.00	37.00	62.00	34687
Female	0.35	0.48	0.00	0.00	1.00	34687
Panel C. Long-run placebo data set (2005)						
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
Panel D. Long-run placebo data set (2015)						
Labor income	74713.29	118216.88	960.00	57200.00	399046.94	316536
Employed (AHV)	0.96	0.19	0.00	1.00	1.00	328398
Age	37.18	10.01	21.00	35.00	63.00	328398
Female	0.44	0.50	0.00	0.00	1.00	328398

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

A.4 Between-canton mobility

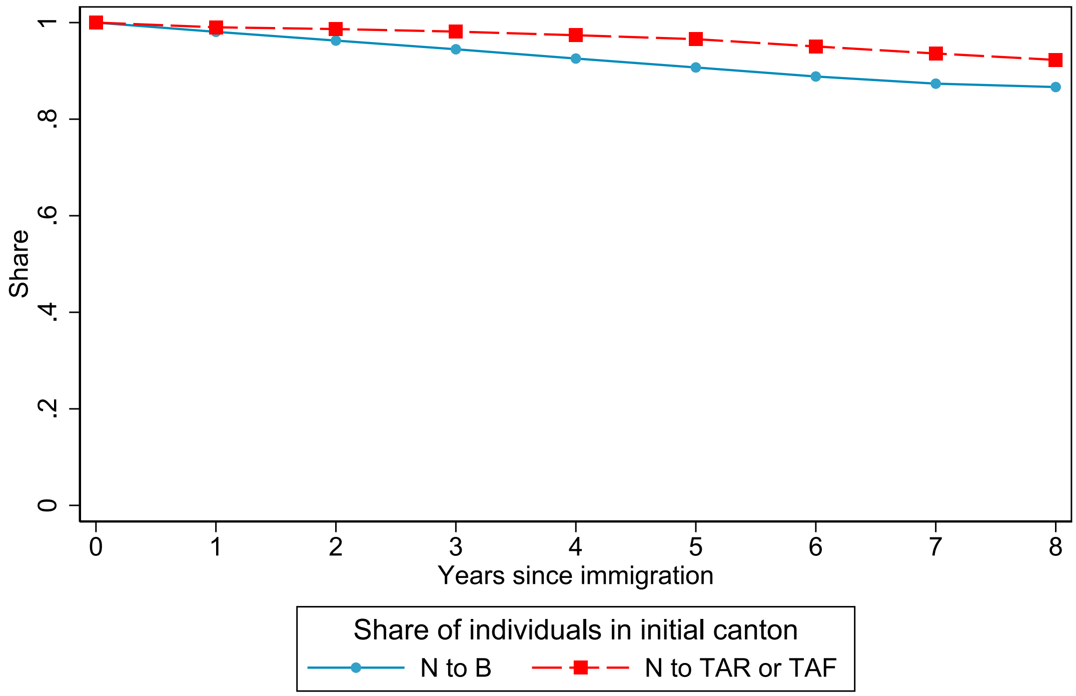


Figure A.9: Share of refugees living in canton initially assigned to, by years in Switzerland
Notes: The figure uses data from the Swiss population registers 2010–2018 to show the share of refugees that live in the canton they were assigned to upon arrival, separately for refugees whose asylum claim was granted (transition from N to B permit) and for refugees that were temporally admitted (TAR or TAF). We focus on refugees that arrived in Switzerland in the years 2005–2010.

B Verifying results using ZEMIS employment data

	ZEMIS			
	Count		Share (%)	
AHV	No	Yes	No	Yes
Panel A. All stati				
No	5446515	73640	98.70	1.30
Yes	1147513	631937	64.50	35.50
Panel B. N status only.				
No	1188370	6181	99.50	0.50
Yes	16021	55478	22.40	77.60
Panel C. B status only.				
No	1031719	14800	98.60	1.40
Yes	260503	72870	78.10	21.90

Table B.1: Employment status in AHV and ZEMIS by person-months. Relative to AHV, ZEMIS under-reports employment. 64% of employment-months in AHV are not reported as employed in ZEMIS (Panel A). This under-reporting is less pronounced for asylum applicants (N status; Panel B) and more pronounced for B status individuals (Panel C), who are not exposed to employment bans, sector restrictions and cantonal restrictions and for which employment is not tracked systematically.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Employment indicator (ZEMIS).							
Employment ban	-0.1076***	-0.2200***	-0.0906***	-0.0976***	-0.0934***	-0.0666***	-0.1320***
	(0.0269)	(0.0317)	(0.0279)	(0.0207)	(0.0237)	(0.0190)	(0.0262)
Priority enforced	-0.0583***	-0.0606**	-0.0734***	-0.0587***	-0.0587***	-0.0335***	-0.0641**
	(0.0094)	(0.0294)	(0.0241)	(0.0114)	(0.0131)	(0.0115)	(0.0235)
Share restricted jobs	-0.0526	-0.0329	0.0313	-0.0181	-0.0118	-0.0124	-0.0941
	(0.0345)	(0.0305)	(0.0447)	(0.0286)	(0.0309)	(0.0209)	(0.0577)
Outcome mean	0.1347	0.1308	0.1043	0.1257	0.1257	0.1257	0.1619
Num. individuals	42,747	6,762	20,956	70,504	70,504	70,504	37,331
Observations	1,965,842	300,258	904,325	3,170,425	3,170,425	3,170,425	1,391,051
Sample	N->TAF	N->TAR	N->B	All	All	All	TAF
Canton FE	Yes	Yes	Yes	Yes	Yes		Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Months-since-arrival FE	Yes	Yes	Yes	Interacted	Interacted	Interacted	Yes
Individual FE						Yes	
Additional controls	Yes	Yes	Yes	Yes	No	No	Yes

Notes: See notes in Table 3.

C Measures of cantonal and sectoral labor market restrictions

This section explains how we construct measures that summarize the extent to which sector and mobility 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 extra-cantonal policy. We calculate the share of jobs that are banned for refugees due to mobility restrictions as

$$\textit{share restricted extra-cantonal jobs}_{its} = \sum_{j \neq i} \textit{commuter-share}_{i \to j} \times \textit{extra-cantonal}_{jts} \tag{4}$$

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

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

$$\textit{share sector-restricted jobs}_{its} = \sum_{\ell} \textit{sector-share}_{\ell} \times \textit{sector-restriction}_{its\ell} \tag{5}$$

where $\textit{sector-policy}_{its\ell}$ 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. $\textit{sector-share}_{\ell}$ 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 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 which quantifies the share of jobs that are restricted for refugees either due to either cantonal or sectoral restrictions.

$$\textit{total share restricted jobs}_{its} = \sum_{j} \sum_{\ell} \textit{share}_{i \to j, \ell} \times \textit{restriction}_{ijts\ell}$$

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,

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

Status	Restriction	% Zero	p50	p75	p90	Max
All	Share mobility-restricted jobs	40.60	0.05	4.00	17.10	34.87
	Share restricted jobs	41.42	0.04	5.44	34.31	66.53
	Share sector-restricted jobs	91.51	0.00	0.00	0.00	60.72
B	Share mobility-restricted jobs	100.00	0.00	0.00	0.00	0.00
	Share restricted jobs	100.00	0.00	0.00	0.00	0.00
	Share sector-restricted jobs	100.00	0.00	0.00	0.00	0.00
N	Share mobility-restricted jobs	0.00	13.24	20.39	31.39	34.87
	Share restricted jobs	0.00	18.62	44.79	60.76	66.53
	Share sector-restricted jobs	72.19	0.00	43.05	51.39	60.72
TAF	Share mobility-restricted jobs	10.47	0.14	1.65	4.09	7.47
	Share restricted jobs	11.17	0.22	3.15	7.60	52.77
	Share sector-restricted jobs	93.55	0.00	0.00	0.00	51.39
TAR	Share mobility-restricted jobs	51.92	0.00	0.16	1.26	6.83
	Share restricted jobs	51.92	0.00	0.16	1.26	6.83
	Share sector-restricted jobs	100.00	0.00	0.00	0.00	0.00

Notes: '% Zero' indicates the share of observations where the variables are equal to zero, i.e., no restriction. 'p50', 'p75' and 'p90' indicate the 50th, 75th and 90th percentile. 'Max' is the maximum value.

Table C.1: Share restricted jobs due to extra-cantonal and sectoral restrictions by status. Descriptive statistics over canton-months by status.

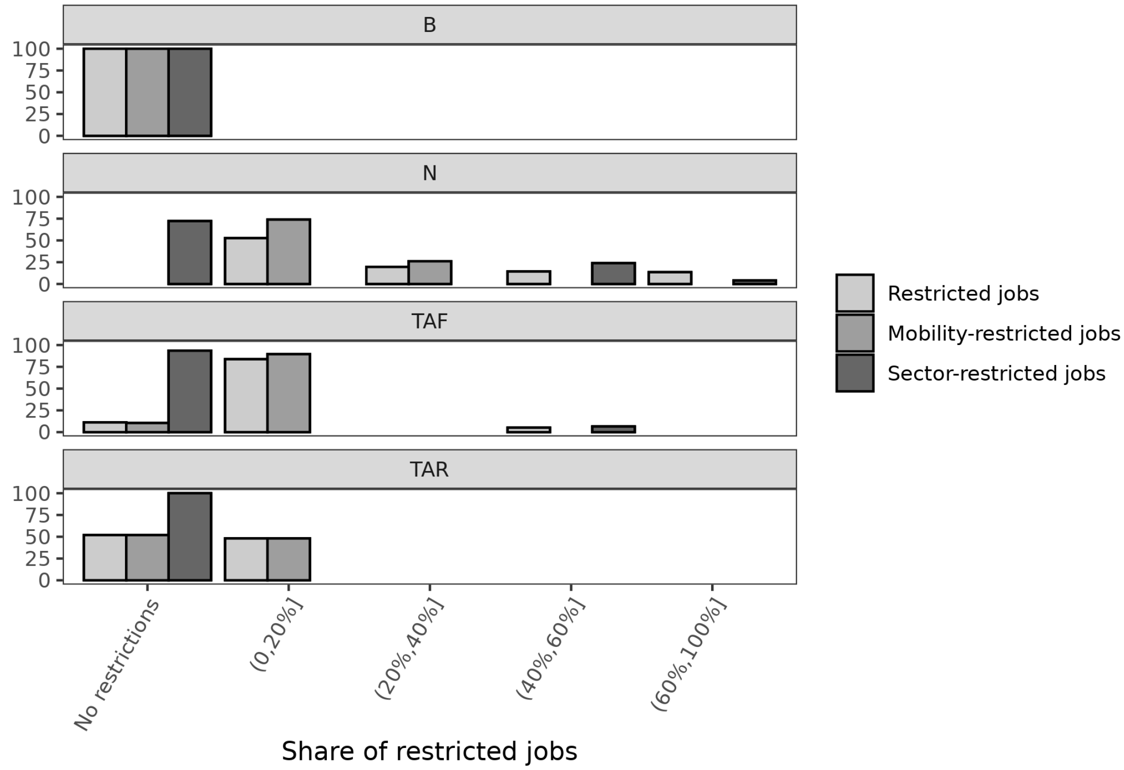


Figure C.1: Distribution of share of restricted jobs due to geographical and sectoral restrictions.

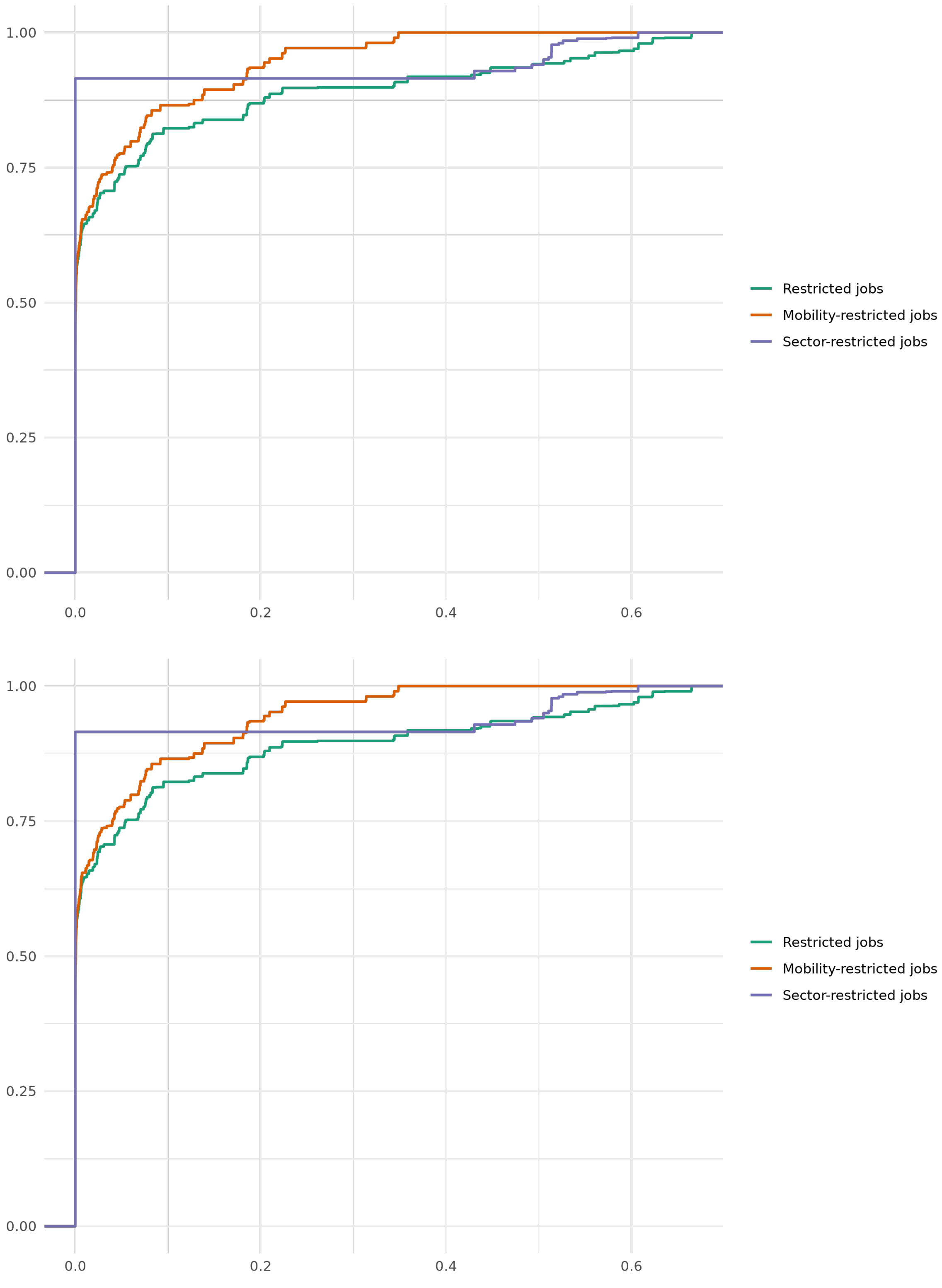


Figure C.2: Distribution of share of restricted jobs due to geographical and sectoral restrictions.

Figure C.3: Wage distribution of refugees during unrestricted periods in all sectors and typically restricted sectors.
Notes: This figures shows the hourly wage distribution of refugees across all sectors (blue) and in the sectors in which they are typically forced to work if sector restrictions apply (red). The data is from the Swiss earnings structure surveys in 2012, 2014, and 2016. The sample is all refugees with TAR, TAF or B permit, which, in this time period, do not face any sector restrictions. The blue histogram shows the unconditional wage distribution. The red histogram focuses on the following two-digit industries (NACE rev. 2): manufacture of beverages, tobacco products, textiles, wearing apparel, and leather and related products, construction of buildings and civil engineering, accommodation and food and beverages, service activities, human health and residential care activities, and activities of households as employers of domestic personnel. These industries are still admissible for refugees in at least one canton even if that canton applies sector restrictions. Observations with wages above 40 CHF and below 10 CHF are dropped. The figure suggests that the wage distribution across all sectors is similar to the wage distribution of those sectors to which refugees are typically restricted when sectoral restrictions apply.

D Construction of employment score

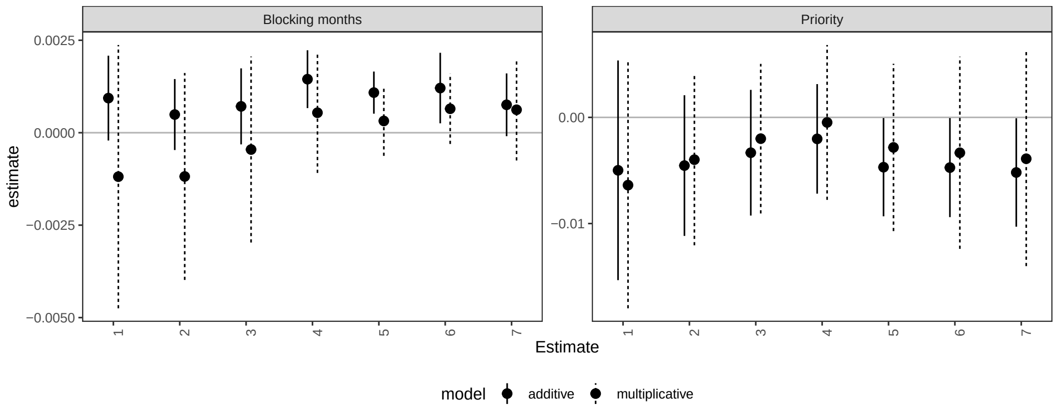
We explore heterogeneity by dividing the refugee sample in four groups from low to high employability. To this end, we predict employment status in the 5th year after asylum application while exclusively relying on time-invariant individual characteristics (age, sex, age at arrival, nationality, religion, native language). The prediction model is applied to two randomly split samples and out-of-sample predicted probabilities are used to classify individuals into four groups. We employ gradient boosting, which yielded larger classification performance compared to random forest and logistic lasso. The use of sample splitting avoids using the same data for both classification into employability groups and for the final estimation which would lead to too small standard errors. The final model is estimated again separately on both samples. The reported point estimates are the average over the two random samples.

	Employment score group			
Variable	1	2	3	4
Employment (%)	2.48	13.33	25.54	45.06
Predicted employment (%)	2.63	14.01	25.88	43.78
Female (%)	50.71	68.26	21.06	6.06
Married (%)	36.05	50.17	49.05	59.44
Muslim (%)	37.24	35.32	35.82	24.08
Christian (%)	7.65	11.77	9.13	6.71
Family size	2.62	3.22	2.41	2.15
Age at application (mean)	31.67	29.86	27.43	25.46
Age at application (p25)	23.00	24.00	22.00	21.00
Age at application (p50)	30.00	29.00	26.00	25.00
Age at application (p75)	38.00	35.00	32.00	29.00

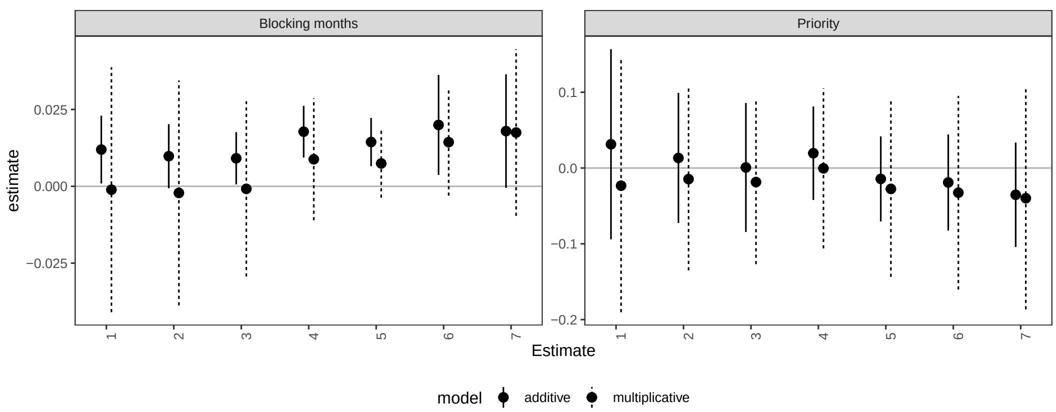
Table D.1: Employment score. Descriptive statistics by group ordered from low (group 1) to high (group 4) employment probability. ‘Employment (%)’ indicates the observed employment share in the 5th year after arrival. ‘Predicted employment (%)’ is the out-of-sample predicted employment probability which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

E Placebos

Figure E.1: Long-run placebo using sample of EU-15 immigrants which are not affected by labor market restrictions



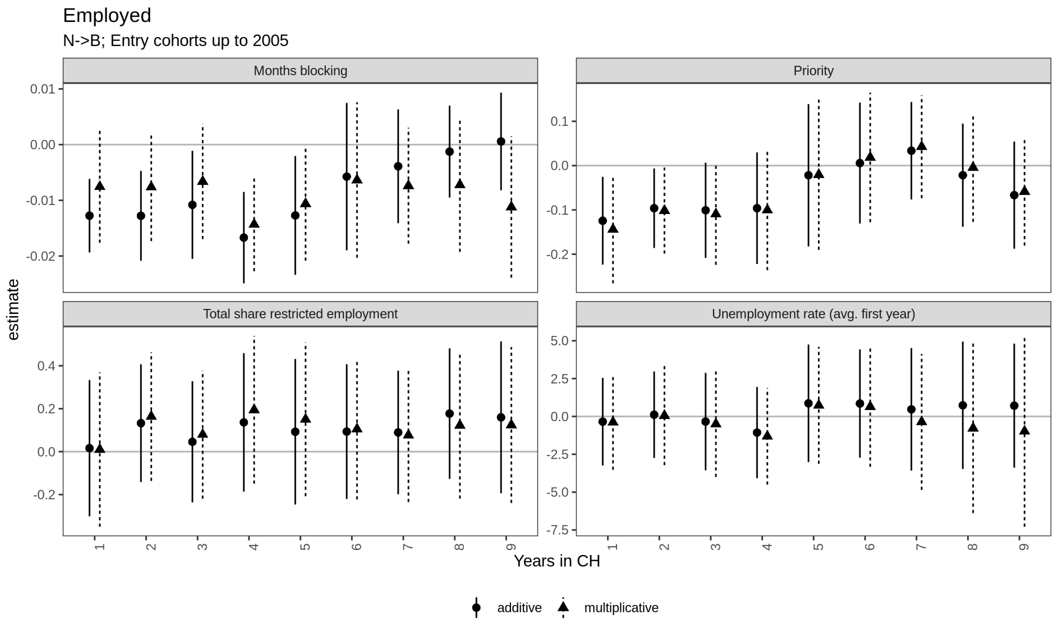
(a) Effects on employment



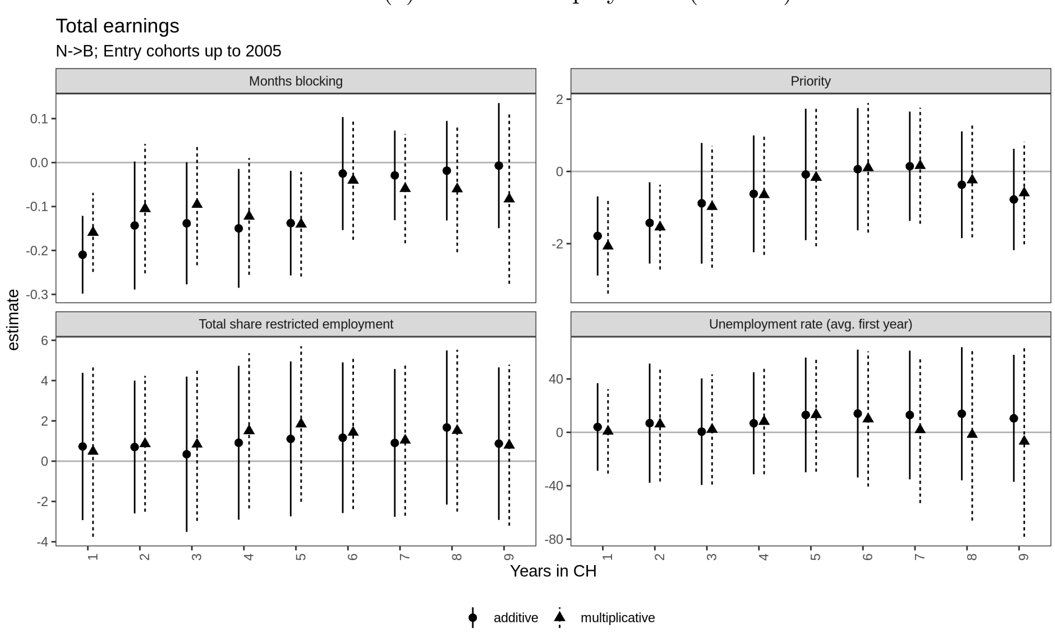
(b) Effects on total earnings

Notes: The sample includes all cohorts arriving in 2005 or later and individuals who reside in Switzerland in 2015. The estimation model is given by (3). We control for age, age-squared, wait months until decision (in months) and squared.

F Long-run analysis



(a) Effect on employment (months)



(b) Effect on earnings

Notes: This figure shows the effects of the labor market restrictions on annual employment (panel A) and annual earnings (panel B) of refugees transiting from permit N to permit B in the first ten years after their arrival based on the specification (3). The figure also shows the effects of the average cantonal unemployment rate during the first years after arrival in Switzerland. The sample includes all cohorts arriving in 2005 or later and individuals who reside in Switzerland in 2015. We control for age, age-squared, wait months until decision (in months) and squared.

Figure F.1: The effect of initial policies and unemployment at arrival on annual total labor earnings (IHS transformed) and annual employment.

G Short-run analysis

G.1 Further employment and earnings results.

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

	(1)	(2)	(3)	(4)	(5)	(6)
Priority enforced	-0.054^{***}	-0.056^{***}	-0.028^{**}	-0.072^{***}	-0.071^{***}	-0.053^{**}
	(0.013)	(0.012)	(0.011)	(0.018)	(0.015)	(0.022)
Share restricted jobs	-0.053	-0.053^{*}	-0.036^{*}	-0.086	-0.081	-0.139^{***}
	(0.033)	(0.028)	(0.020)	(0.057)	(0.054)	(0.048)
Sample	All	All	All	All	All	All
Months FE	Yes	Yes	Yes	Yes	Yes	Yes
Canton FE	Yes	Yes	–	Yes	Yes	–
Individual FE	–	–	Yes	–	–	Yes
Months-since-entry FE	Interacted	Interacted	Interacted	Interacted	Interacted	Interacted
Additional controls	–	Yes	–	–	Yes	–
Outcome mean	0.173	0.173	0.173	0.173	0.173	0.173
Estimator	OLS	OLS	OLS	Logit	Logit	Logit
Num. obs.	2746661	2744238	2746661	2386235	2386235	1248505

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: See notes in Table 3.

Table G.2: Effect of labor market policies on employment only using policies coded with high reliability.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Employment indicator (AHV).							
Employment ban	-0.1286***	-0.2093***	-0.1522***	-0.1402***	-0.1351***	-0.0825***	-0.0950***
	(0.0238)	(0.0284)	(0.0194)	(0.0185)	(0.0212)	(0.0093)	(0.0292)
Priority enforced	-0.0598***	-0.0207*	-0.0440***	-0.0493***	-0.0515***	-0.0211**	-0.0576**
	(0.0208)	(0.0120)	(0.0134)	(0.0150)	(0.0163)	(0.0086)	(0.0266)
Share sector restricted jobs	-0.1064**	0.0810	-0.0474	-0.0721**	-0.0792**	-0.0113	-0.1488***
	(0.0454)	(0.1372)	(0.0476)	(0.0353)	(0.0396)	(0.0265)	(0.0430)
Share region restricted jobs	-0.0959	-0.3086***	-0.3402***	-0.2011***	-0.1801**	-0.2330***	0.8640**
	(0.0772)	(0.0873)	(0.0766)	(0.0662)	(0.0712)	(0.0473)	(0.3656)
Employment ban × Low reliability 4	0.0317	-0.0169	-0.0071	0.0149	0.0141	0.0088	0.0287
	(0.0220)	(0.0217)	(0.0126)	(0.0136)	(0.0148)	(0.0094)	(0.0195)
Employment ban × High reliability 4	0.0643**	0.0458*	0.0551***	0.0595***	0.0571***	0.0477***	0.0276
	(0.0247)	(0.0232)	(0.0134)	(0.0147)	(0.0158)	(0.0098)	(0.0294)
Priority enforced × Low reliability 3	0.0096	-0.0158	-0.0202	-0.0067	-0.0076	-0.0044	0.0893**
	(0.0230)	(0.0184)	(0.0215)	(0.0158)	(0.0169)	(0.0134)	(0.0345)
Priority enforced × High reliability 3	0.0399	-0.0437	-0.0051	0.0168	0.0190	-0.0153	0.0738*
	(0.0350)	(0.0706)	(0.0249)	(0.0219)	(0.0255)	(0.0187)	(0.0393)
Share sector restricted jobs × Low reliability 1	0.0919	0.0149	0.1688**	0.1381***	0.1436***	0.0535	0.2561**
	(0.0664)	(0.1373)	(0.0644)	(0.0474)	(0.0511)	(0.0464)	(0.1181)
Share region restricted jobs × High reliability 2	0.0640	0.0398	-0.1127	-0.0056	0.0043	-0.0082	
	(0.1381)	(0.2248)	(0.1117)	(0.0875)	(0.0818)	(0.1121)	
Share region restricted jobs × Low reliability 2	0.6775*			0.7386**	0.8191**	0.3443*	0.0579
	(0.3452)			(0.3071)	(0.3106)	(0.1747)	(0.4753)
Outcome mean	0.1893	0.1438	0.1452	0.1732	0.1732	0.1732	0.2292
Num. individuals	38,384	6,101	18,253	62,723	62,723	62,723	32,684
Observations	1,767,187	246,365	759,223	2,772,775	2,772,775	2,772,775	1,265,841

Notes: See notes in Table 3. 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 (email or telephone). 50% of the observations were classified as highly reliable. 25% of the data points were assessed to be of low reliability. This is usually the case when information provided through a contact (via email or telephone) was unspecific and lacked detail. The remaining observations were assessed to be of normal reliability.

Table G.3: Effect of labor market policies on inverse hyperbolic sine (IHS) of total earnings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel B'. Total earnings (IHS)							
Employment ban	-0.9695***	-1.917***	-1.175***	-1.029***	-0.9924***	-0.5622***	-1.116***
	(0.2274)	(0.2688)	(0.1990)	(0.1510)	(0.1801)	(0.0920)	(0.2371)
Priority enforced	-0.4706***	-0.4735*	-0.5221***	-0.4916***	-0.4831***	-0.2563***	-0.5257**
	(0.1097)	(0.2549)	(0.1720)	(0.0965)	(0.1082)	(0.0901)	(0.2104)
Share restricted jobs	-0.4744	-0.3546	-0.3822	-0.4479**	-0.4156*	-0.2578	-0.6537
	(0.2954)	(0.2785)	(0.2563)	(0.2152)	(0.2409)	(0.1624)	(0.5065)
Outcome mean	1.563	1.175	1.160	1.417	1.417	1.417	1.904
Num. individuals	39,795	6,369	19,152	65,381	65,381	65,381	33,258
Observations	1,741,073	246,365	759,223	2,746,661	2,746,661	2,746,661	1,239,727
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
Months-since-arrival FE	Yes	Yes	Yes	Interacted	Interacted	Interacted	Yes
Individual FE						Yes	
Additional controls	Yes	Yes	Yes	Yes	No	No	Yes

Notes: See notes in Table 3.

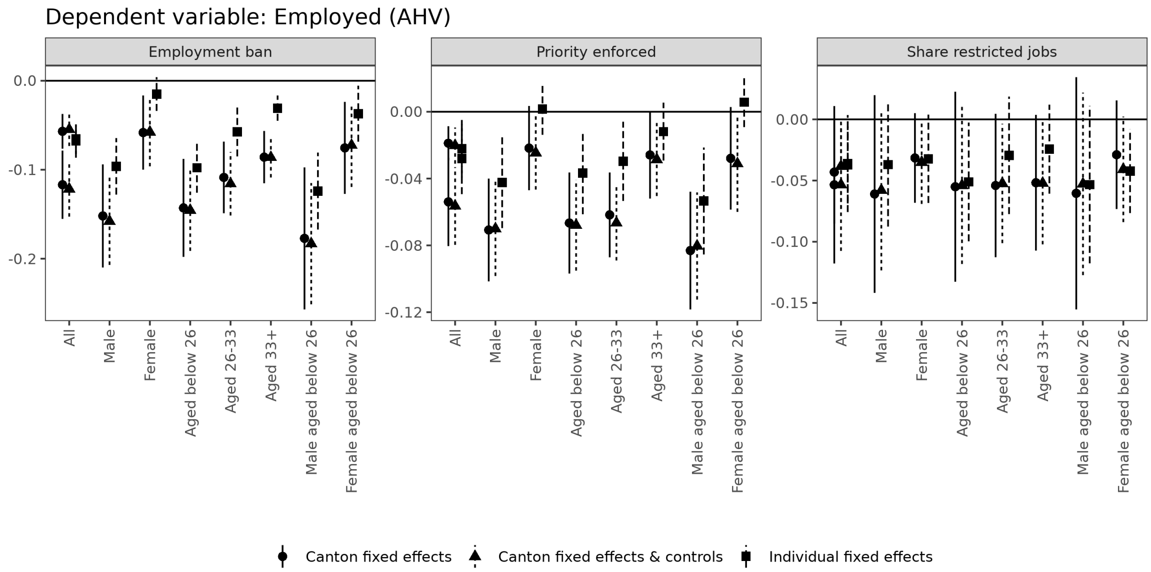
Table G.4: Effect of labor market policies on employment and total earnings with separate effects for job restrictions by region and sector restrictions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Employment							
Employment ban	-0.1032***	-0.2382***	-0.1592***	-0.1216***	-0.1161***	-0.0737***	-0.0764***
	(0.0246)	(0.0362)	(0.0224)	(0.0152)	(0.0190)	(0.0074)	(0.0261)
Priority enforced	-0.0554***	-0.0510*	-0.0551***	-0.0557***	-0.0551***	-0.0256**	-0.0511*
	(0.0146)	(0.0284)	(0.0183)	(0.0124)	(0.0139)	(0.0104)	(0.0267)
Share sector restricted jobs	-0.0405	-0.0110	-0.0181	-0.0351	-0.0349	-0.0195	-0.0738*
	(0.0357)	(0.0236)	(0.0267)	(0.0262)	(0.0291)	(0.0183)	(0.0419)
Share region restricted jobs	-0.0517	-0.2808***	-0.3053***	-0.1331**	-0.1007	-0.1951***	0.9399**
	(0.0658)	(0.0808)	(0.0900)	(0.0596)	(0.0633)	(0.0409)	(0.4154)
Outcome mean	0.1893	0.1438	0.1452	0.1732	0.1732	0.1732	0.2292
Num. individuals	39,798	6,369	19,152	65,388	65,388	65,388	33,447
Observations	1,767,187	246,365	759,223	2,772,775	2,772,775	2,772,775	1,265,841
Panel B. Total earnings (Poisson)							
Employment ban	-1.139***	-2.568*	-1.583***	-1.251***	-1.250***	-1.664***	-0.7065***
	(0.1787)	(1.372)	(0.3237)	(0.1131)	(0.1286)	(0.1556)	(0.1622)
Priority enforced	-0.3806***	-0.6472***	-0.9117***	-0.4370***	-0.4573***	-0.3396***	-0.2116*
	(0.0704)	(0.1715)	(0.1796)	(0.0666)	(0.0656)	(0.0644)	(0.1083)
Share sector restricted jobs	-0.4704**	0.5149	0.0370	-0.3409*	-0.3830**	-0.3283**	-0.2496
	(0.1946)	(0.4866)	(0.3595)	(0.1797)	(0.1952)	(0.1453)	(0.2047)
Share region restricted jobs	-1.228*	-1.322	-1.787	-1.486**	-1.441**	-1.878***	3.580**
	(0.6942)	(1.446)	(1.150)	(0.5937)	(0.5837)	(0.5634)	(1.824)
Outcome mean	505.6	365.8	328.0	444.3	444.3	950.9	621.3
Num. individuals	38,386	6,117	18,256	62,799	62,799	18,956	32,692
Observations	1,765,982	246,047	759,222	2,772,610	2,772,610	1,295,608	1,265,791
Panel C. Monthly earnings (log)							
Employment ban	-0.6604***	1.468**	0.2150	-0.5243***	-0.5570***	-0.3147***	-0.6331***
	(0.0809)	(0.6777)	(0.5010)	(0.0838)	(0.0769)	(0.0787)	(0.0991)
Priority enforced	-0.0695*	-0.4395**	-0.4082	-0.1652***	-0.1608***	-0.1429**	-0.0149
	(0.0395)	(0.2064)	(0.2406)	(0.0456)	(0.0500)	(0.0609)	(0.0283)
Share sector restricted jobs	-0.2626*	0.1823	-0.1829	-0.1706	-0.1725	-0.1798	-0.0721
	(0.1382)	(0.4043)	(0.3828)	(0.1368)	(0.1385)	(0.1606)	(0.0824)
Share region restricted jobs	-0.6928*	1.153	0.4611	-0.5412	-0.5499	0.4582	-1.537
	(0.3741)	(1.274)	(1.330)	(0.4551)	(0.4717)	(0.4030)	(1.026)
Outcome mean	7.581	7.476	7.296	7.508	7.508	7.508	7.606
Num. individuals	7,782	989	2,823	11,572	11,572	11,572	7,638
Observations	334,539	35,426	110,230	480,195	480,195	480,195	290,049
Sample	N->TAF	N->TAR	N->B	All	All	All	TAF
Canton FE	Yes	Yes	Yes	Yes	Yes		Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Months-since-arrival FE	Yes	Yes	Yes	Interacted	Interacted	Interacted	Yes
Individual FE						Yes	
Additional controls	Yes	Yes	Yes	Yes	No	No	Yes

Notes: See notes in Table 3 in the main text. The results in this table split the restricted share in jobs restricted by sector and by regional restrictions.

G.2 Heterogeneity

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



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 figure also shows the effects of the average cantonal unemployment rate during the first two years after arrival in Switzerland. The regression pools refugees from the three permit transition groups (N→B, N→TAF, and N→TAR) and includes interactions of these three groups with month, canton, month-since-arrival, and months-to-decision fixed effects. All regression models include age and age-squared interacted with sex and maximum cash allowance in CHF for refugees. Additional controls are marriage status, two dummies for self-reported religion (christian and muslim), nationality and asylum processing centre fixed effects as well as contemporaneous unemployment rate and unemployment rate at arrival. Standard errors are clustered at the canton × status-group level.

G.3 Event study

For completeness, Figure G.2 analyses the dynamic effects on employment when employment bans are lifted. To this end, we restrict the sample to the first 20 months after application and only consider pre-decision months. The coefficients in Figure G.2 are fixed effects for the number of months to and after the expiration of the employment ban. We see strong evidence of a jump in employment probability after the expiration.

Figure G.2: Event study: Instantaneous effect of prioritisation and total restricted share on employment.

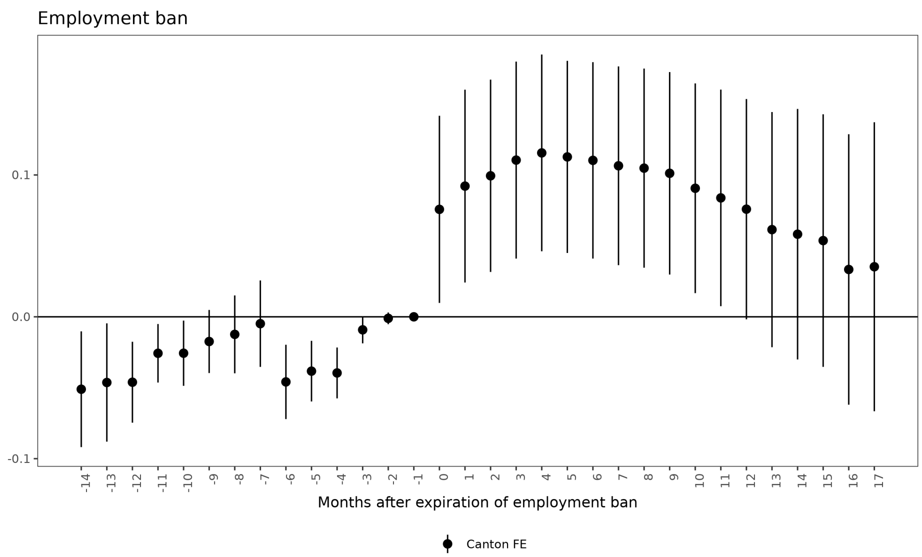


Figure G.3: Employment ban

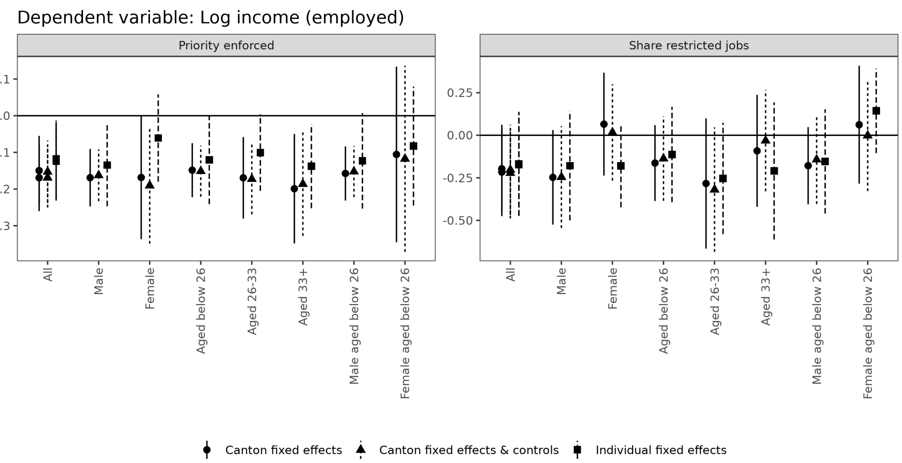
Notes: The figure shows fixed effects for the number of months to and after the expiration of the employment ban. The sample only includes the first 20 months after the arrival in Switzerland. See text for more information.

H Further evidence on wage effects

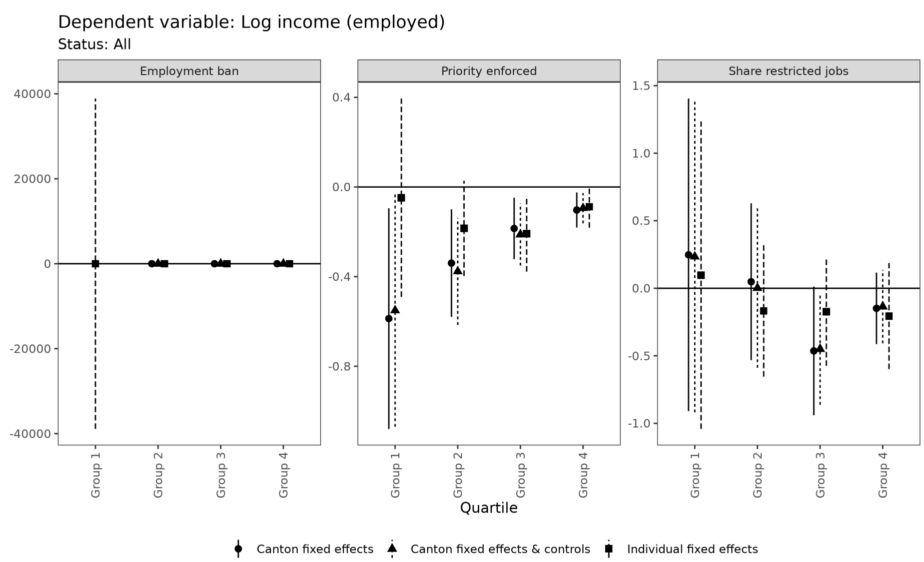
H.1 Wage effects in short-run specification

Figure H.1: Heterogeneity in the effect of labor market policies on refugees' monthly earnings

(a) A. By age and gender



(b) B. By employment score



Notes: This figure shows the heterogeneous effects of enforcing the priority restriction and of mobility and sector restrictions on monthly earnings of the employed by age and gender (panel (a)) and by different quartiles of the employment score (panel (b)). The employment score 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 Appendix D). The regression pools refugees from the three permit transition groups (N→B, N→TAF, and N→TAR). It controls for month and canton fixed effects and interactions between transition group indicators and, month-since-arrival. 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 contemporaneous unemployment rate and unemployment rate at arrival. Standard errors are clustered at the canton × status-group level.

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

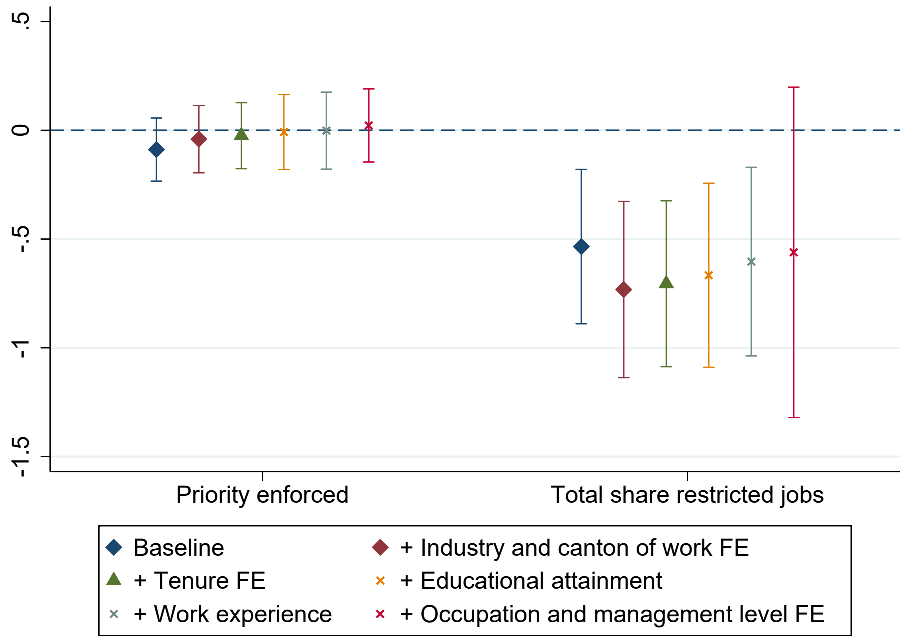
VARIABLES	(1) Log hourly wage	(2) Log hourly wage	(3) Log hourly wage	(4) Log hourly wage	(5) Log hourly wage	(6) Log hourly wage	(7) Log hourly wage first 5 years only
Priority enforced	-0.067	-0.089	-0.073	-0.041	-0.032	-0.167	-0.053
	(0.167)	(0.070)	(0.050)	(0.075)	(0.081)	(0.135)	(0.098)
Share restricted jobs	0.089	-0.535***	-0.884***	-0.732***	-0.425	-0.284	-0.569**
	(0.323)	(0.172)	(0.111)	(0.197)	(0.277)	(0.523)	(0.220)
Observations	1,439	4,453	4,465	4,447	4,447	2,172	1,123
R-squared	0.130	0.102	0.032	0.166	0.178	0.696	0.161
Sample	N→B	N→TAR/F	N→TAR/F	N→TAR/F	N→TAR/F	N→TAR/F	N→TAR/F
Additional controls	Yes	Yes	No	Yes	Yes	No	Yes
Canton FE	Yes	Yes	Yes	Yes	Yes	No	Yes
Survey wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	No	No	No	Yes	Yes	No	No
Canton of work FE	No	No	No	Yes	Yes	No	No
Years-since-entry FE	No	No	No	No	Yes	No	No
Individual FE	No	No	No	No	No	Yes	No

Robust standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

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

Figure H.2: Wage effects and the composition of jobs (contemporaneous policies)



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

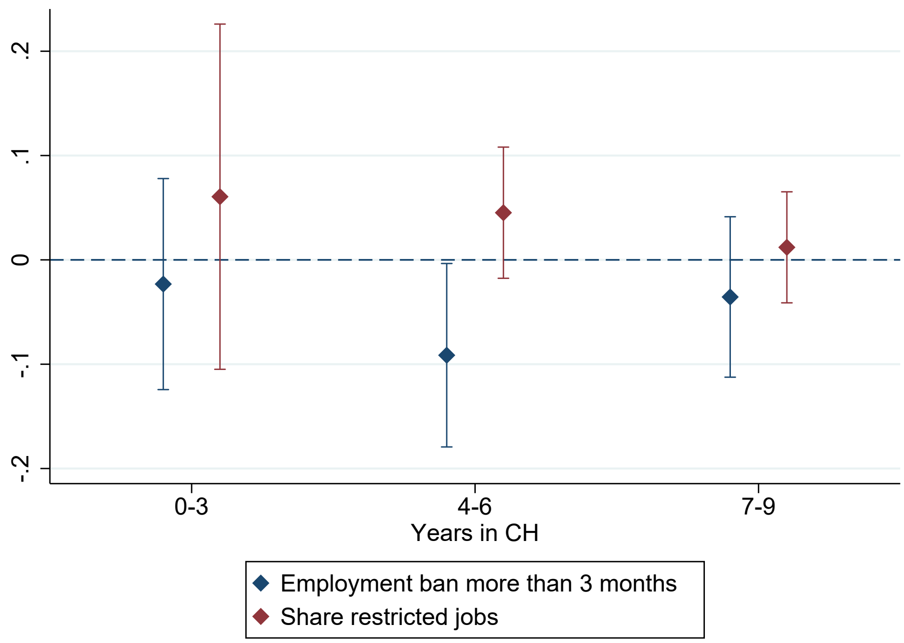
VARIABLES	(1) Log monthly hours	(2) Log monthly hours	(3) Log hourly wage	(4) Log hourly wage
Priority weakly enforced	0.046	-0.032	0.038	0.049
	(0.068)	(0.070)	(0.056)	(0.056)
Priority strictly enforced	-0.221**	-0.147*	0.036	0.039
	(0.099)	(0.084)	(0.041)	(0.041)
Share sector-restricted jobs	0.225	0.359**	-0.262*	-0.307**
	(0.160)	(0.142)	(0.132)	(0.135)
Share commuter-restricted jobs	0.012	0.013	-0.036	-0.025
	(0.053)	(0.058)	(0.026)	(0.026)
Observations	6,385	6,377	6,385	6,377
R-squared	0.157	0.352	0.053	0.116
Observations per firm	First	First	First	First
Additional controls	Yes	Yes	Yes	Yes
First year of tenure FE	Yes	Yes	Yes	Yes
Canton FE	Yes	Yes	Yes	Yes
Survey wave FE	Yes	Yes	Yes	Yes
Industry and canton of work FE	No	Yes	No	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table H.2: Effect of labor market policies on log hourly wages: More detailed policy variables.

H.2 Long-run effects on education and wages

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



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

I Overeducation

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

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

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

VARIABLES	(1) Years of schooling	(2) Years of schooling higher requirements	(3) Years of schooling low requirements
Priority enforced	0.086	1.566*	-0.039
	(0.195)	(0.799)	(0.173)
Share restricted jobs	-0.292	-1.006	-0.400
	(0.534)	(2.262)	(0.409)
Observations	5,102	986	4,081
R-squared	0.297	0.484	0.171
Mean of outcome	-1.792	-2.745	-1.561
Observations per firm	First	First	First
Additional controls	Yes	Yes	Yes
First year of tenure FE	Yes	Yes	Yes
Canton FE	Yes	Yes	Yes
Survey wave FE	Yes	Yes	Yes
Industry and canton of work FE	Yes	Yes	Yes
Occupation and management level FE	Yes	Yes	Yes

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

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

J Firm analysis

In this section, we test whether an increased skill mismatch can explain the negative wage effects of sector and region restrictions. As explained in the main text, this mechanism requires that firms that are still legally allowed to do so 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 firms, which are allowed to, 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 J.1 is one if a firm employs more refugees in calendar month t than in calendar month $t-12$ ($t-24$ in columns 4–6). It is zero if the firm still employs refugees in the second period but not more than in the first period. We opt for a specification with a dummy variable because of the very low number of refugee workers in most firms. Column 1 and 2 (column 4 and 5) focus on firms' employment of the two main permit categories—N and TAF—which, in contrast to refugees with a B permit, face the restrictions we want to analyze. Column 3 (column 6) pools employment of N, TAR, and TAF refugees. In this case, the policies are measured as a weighted average of the N, TAR, and TAF refugees, where the weights reflect the relative frequencies in the sample.

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 3. Overall, if the share of restricted jobs is higher, firms that are legally allowed to employ refugees do not seem to hire additional, possibly less productive workers. These results speak against the hypothesis that an increase in skill mismatch explains the negative wage effects of sector and mobility restrictions.

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

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: Increase in employment						
Priority	-0.0232**	-0.0159	-0.0502	-0.0345**	-0.0238	-0.0664**
	(0.0094)	(0.0164)	(0.0309)	(0.0143)	(0.0219)	(0.0321)
Share restricted job	-0.1052**	0.0068	-0.0973	-0.0973*	0.0185	-0.1303
	(0.0434)	(0.0440)	(0.0922)	(0.0570)	(0.0568)	(0.1096)
Observations	55,211	206,644	247,350	46,608	164,861	195,121
Outcome mean	0.1189	0.1406	0.1661	0.1437	0.1889	0.2302

Notes: This table tests whether firms, which are allowed to, employ more refugees if restrictions increase. To this end, 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 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. Column 1 and 2 (column 4 and 5) focus on firms' employment of the two main permit categories—N and TAF—which, in contrast to refugees with a B permit, face the restrictions we want to analyze. Column 3 (column 6) pools employment of N, TAR, and TAF refugees. In this case, the policies are measured as a weighted average of the N, TAR, and TAF refugees, where the weights reflect the relative frequencies in the sample. All regression models include firm, canton and month fixed effects.

K Employer concentration

An important side effect of the labor market restrictions is that they restrict the number of employers that can hire refugees. Static models of monopsony predict that the extent to which employers set wages below the marginal revenue product of workers is inversely related to the number of employers that compete for a group of workers (Boal and Ransom, 1997; Card et al., 2018). Therefore, sector and region restrictions may decrease refugees' wages by increasing employer concentration.

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

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

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

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

Overall, if employers exercise the wage-setting power arising from employer concentration, Table K.1 provides a possible explanation why mobility restrictions reduce refugees' hourly wages. The results also provide a possible explanation why prioritizing residents does not depress refugees' wages although it clearly limits their employment opportunities.

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

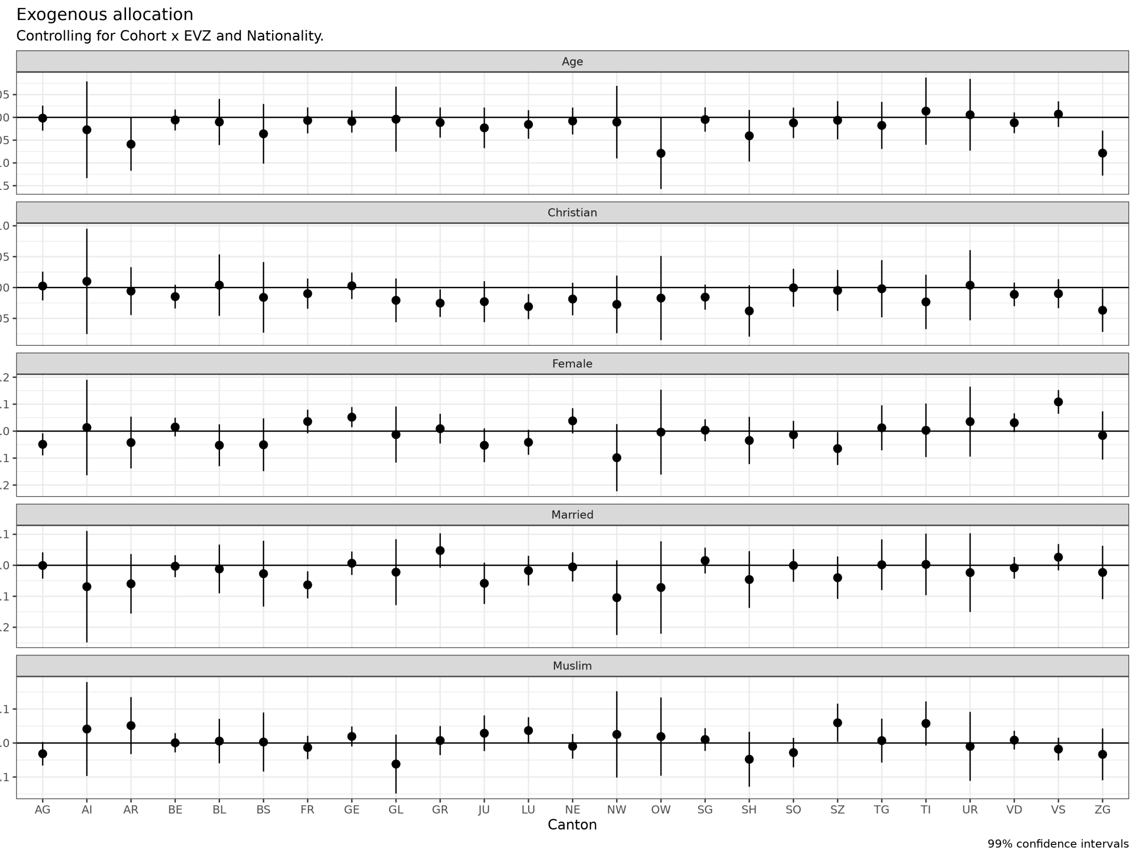
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share banned	-0.262	-0.052	-0.113	-0.130	-0.217	-0.037	-0.082	-0.109
	(0.201)	(0.054)	(0.104)	(0.126)	(0.203)	(0.058)	(0.110)	(0.136)
Priority enforced	0.046	-0.070^{***}	-0.126^{***}	-0.073^{*}	0.032	-0.074^{***}	-0.135^{***}	-0.079^{*}
	(0.043)	(0.024)	(0.041)	(0.043)	(0.046)	(0.025)	(0.045)	(0.046)
Share total restricted jobs	0.027	0.024	0.047	0.030				
	(0.072)	(0.021)	(0.040)	(0.043)				
Share commuter-restricted jobs					1.000^{*}	0.334^{*}	0.711^{**}	0.495
					(0.587)	(0.198)	(0.358)	(0.398)
Share sector-restricted jobs					0.033	0.025	0.049	0.035
					(0.062)	(0.019)	(0.037)	(0.035)
Measure	HHI	Gini	Log(Ratio)	Theil	HHI	Gini	Log(Ratio)	Theil
Num. obs.	1474	1474	1474	1474	1474	1474	1474	1474
N Clusters	104	104	104	104	104	104	104	104

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

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

L Exogenous allocation check

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



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