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IZA DP No. 15902

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# ABSTRACT

## Train Drain? Access to Foreign Workers and Firms' Provision of Training*

Does better access to foreign workers reduce firms' willingness to provide general skills training to unskilled workers? We analyze how the opening of the Swiss labor market to workers from the European Union affected the number of apprenticeship positions that firms provide. We exploit that the availability of foreign workers increased more in firms close to the border because they gained unrestricted access to cross-border workers from Switzerland's neighboring countries. Our Difference-in-Differences estimates suggest that firm-provided training to unskilled workers and access to foreign workers are not necessarily substitutes: opening the borders did not have a statistically significant effect on apprenticeship provision. Using unique data on firms' costs and motives to train apprentices, we show that the greater availability of foreign workers reduced firms' incentive to train because hiring skilled workers externally became cheaper, among others because new hires became more productive from the start. Positive impacts on firm growth worked in the opposite direction.

**JEL Classification:** J24, J63, M53

**Keywords:** apprenticeships, cross-border workers, firm-provided training, free movement of workers, hiring costs, immigration, immigration policy, labor mobility, vocational education and training

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# 1 Introduction

Does firms' willingness to provide general skills training depend on the supply of workers in the local labor market? A greater local supply makes it easier and cheaper for firms to find external workers with the right skills, which could reduce their incentive to invest in the skills of unskilled (internal) workers. Both the theoretical and empirical literature on firm-sponsored general training highlights a potential trade-off between the supply of skilled workers and firms' willingness to train.[1]

Such a trade-off would have important implications for migration policy. If training and the local supply of skilled workers are substitutes, removing migration restrictions would shift the costs of acquiring labor market skills from firms to native workers as well as firms and governments in immigrants' countries of origin. Such shifts may exacerbate a potential brain drain associated with free labor mobility and may lead residents to oppose open borders.[2]

This paper analyzes whether firms' access to foreign workers has a causal effect on their provision of general skills training, as measured by the number of apprenticeship positions they offer to young adults.[3] We exploit an exogenous increase in the availability of foreign workers that resulted from the implementation of the Agreement on the Free Movement in Switzerland. The "free movement policy" gave EU citizens full and free access to the Swiss labor market starting from 1999 onward. As shown by Beerli et al. (2021), opening the borders had a larger impact on firms near the border because the free movement policy abolished the pre-existing restrictions on employing cross-border workers from Switzerland's neighboring countries. Beerli et al. (2021) show that firms in close vicinity to the border hired substantially more EU workers after the border opening

[1] In the training model of Stevens (1994), firms train workers to save on hiring costs for skilled workers in the external labor market. In the search and matching models of Shintoyo (2008, 2010), the proportion of skilled workers in the unemployment pool is the central determinant of firm training. Empirical papers also support the notion of a trade-off between external recruitment and training (see, e.g., Blatter et al., 2012, 2016).

[2] Denmark, for example, defends its strict stance on immigration with the argument that Danish firms should invest more in the training of native workers (cf. Hermann, 2019). In Switzerland, the country studied in this paper, the citizens voted in favor of a 2014 referendum which intended to restrict free labor mobility with the EU. One argument in the pre-vote debate was that open borders incentivize firms to hire cheap labor instead of giving them incentives to invest in the training of unskilled citizens (Blocher, 2011).

[3] In Switzerland, apprenticeships—also called dual vocational education and training—belong to an upper-secondary level education program combining formal schooling with work-based practical learning in firms. The program provides general skills training since the skills are transferable between firms within an occupation (Mueller and Schweri, 2015). Many studies used the firms' employment of apprentices to study their willingness to provide general skills training, including the influential study by Acemoglu and Pischke (1998). For more information about apprenticeships see Wolter and Ryan (2011) and Backes-Gellner et al. (2020).

than firms farther away. Although the policy opened the labor market for all types of workers, firms' primarily hired skilled workers. Following Beerli et al. (2021), we analyze the effects of the border opening on apprenticeship training using a transparent Differences-in-Differences (DiD) design that compares changes in firms' training near the border with changes in firms' training farther away.

The descriptive patterns in cross-sectional data suggest that cross-border workers may substitute apprentices. Data from the Swiss Business Censuses 1995–2008, which provide the employment and geo-coordinates for the universe of establishments in Switzerland, show that firms located near the border are substantially less likely to train apprentices and more likely to employ cross-border workers than firms farther away. In the cross-section, we also observe a strong negative correlation between a firm's apprentice share and its employment share of cross-border workers. However, these cross-sectional correlations may not be causal. They become substantially smaller once we look at over-time changes in outcomes within firms.

Our DiD estimates indeed consistently suggest that cross-border workers and apprenticeship provision are not necessarily substitutes. As we show, the policy had a substantial positive impact on the employment of foreign workers in firms close to the border, even among firms that train apprentices. In addition, many labor market segments with a high share of apprentices and apprenticeship graduates experienced significant increases in the employment of cross-border workers after the reform. Nevertheless, the policy had only a small, precisely estimated, and statistically insignificant negative effect on the number of apprenticeship positions in these firms and sectors, even in highly treated establishments within 15 minutes of travel time to the border. We also find no significant effect on the probability that a firm trains apprentices—the extensive margin of training.

These conclusions hold in various robustness checks. For instance, the results are similar when we compare changes in outcomes between treated establishments and a matched sample of control establishments that are similar in pre-reform characteristics, including a few characteristics where we observe relevant pre-reform differences between firms near and farther from the border. The results are also similar if we re-estimate our regression models using data aggregated to the municipal and commuting-zone level. These regressions include all establishments in contrast to most of our establishment-level specifications that focus on incumbent and sometimes surviving establishments. Thus, they take into account that the policy may affect the training behavior of entering and exiting establishments or that it may cause a reallocation of apprentices between firms within region.

We then show that two opposing mechanisms contribute to the null effect of the free movement policy on apprenticeship training. A *cost effect* may arise if firms train unskilled

workers to save on hiring costs for skilled workers in the future (Stevens, 1994). Better access to suitable foreign workers may thus lower firms' provision of apprenticeship training because it reduces the future savings in terms of hiring costs associated with training. A counteracting *scale effect*, familiar from standard labor demand theory, may arise if immigrant workers are either imperfect substitutes of (unskilled) apprentices or trained apprenticeship graduates (e.g., Ottaviano and Peri, 2012). In this case, the hiring of additional foreign workers may increase firms' demand for apprentices through a reduction in wage costs.

To evaluate these mechanisms empirically, we exploit rich firm-level surveys in 2000, 2004, and 2009 on the costs to hire skilled workers and the motives to train apprentices in Switzerland (Schweri et al., 2003; Muehlemann et al., 2007b; Strupler and Wolter, 2012). Consistent with the scale effect, the free movement policy had a large positive effect on establishment size in manufacturing and a sizeable but marginally insignificant effect in the private service sector. We find evidence for a displacement of apprenticeship positions in the construction sector, precisely the sector where the policy did not affect firm growth. Consistent with the cost effect, the opening of the borders made hiring skilled workers cheaper, mainly because it became easier to find workers who meet the firm-specific needs. Due to the policy, firms became substantially less likely to experience a shortage of skilled workers and to think that the local supply of skilled workers was insufficiently qualified. The consequence was a meaningful reduction in the costs of hiring skilled workers: the border opening reduced the time it takes until new hires reach full productivity by 12%. Newly hired skilled workers also became 11% more productive during this adaptation period. Firms also spent less on fees for firms that help them find workers. Studying firms' training motives, we find suggestive evidence that firms became less likely to train apprentices to save the costs of external hiring and because it is difficult to find workers on the labor market.

Our study contributes to the literature on immigration and firm-provided training in three important ways. First, we provide causal evidence on one particular factor that shapes firms' decisions to train unskilled workers instead of hiring skilled external ones. Few empirical studies have examined the determinants of these "make or buy"-decisions. Examples of observational studies that analyze the determinants of "make or buy" decisions are Bellmann et al. (2014) and Blatter et al. (2016). Overview articles on why firms train such as Leuven (2005) and Bassanini et al. (2007) do not touch directly upon these decisions. Our empirical results suggest that firms' cost-benefit considerations and production complementarities shape the effects of the supply of skilled workers on training. Conceptually, our paper relates to the literature that has shown that firms provide less training if there are more firms competing for a fixed number of workers (e.g., Winkel-

mann, 1996; Cappelli, 2004; Mohrenweiser and Zwick, 2009; Muehlemann and Wolter, 2011; Mohrenweiser et al., 2019). Our study, instead, examines how access to more workers affect training in an initially fixed number of firms.

Second, our study contributes to the literature analyzing how firms' willingness to train unskilled workers depends on immigration.[4] The existing empirical studies on this relationship are observational and reach conflicting conclusions. They find that more immigration has no (Baker and Wooden, 1992), a positive (Campo et al., 2018), or a negative effect (Mountford and Wadsworth, 2019; Aepli and Kuhn, 2021) on training provision. Our primary innovation relative to these papers is that we use a DiD design to estimate the causal impact of better access to skilled foreign workers on training and that we use unusually detailed data on the costs to hire skilled workers to explain these results. In doing so, we follow a small number of previous papers that leverage changes in commuting policies to estimate the labor market effects of immigration (Mansour, 2010; Dustmann et al., 2017; Beerli et al., 2021). An attractive feature of this approach is that the increase in immigrant workers and its unequal regional impact are a direct consequence of the exogenous change in immigration policy. The approach thus circumvents the identification problems that arise because immigrants likely move to regions with good employment prospects. Another distinct characteristic is that possible consumption-side effects of immigration are muted because cross-border workers do not relocate to the country they work. While this likely increases the scope for displacement effects in the labor market, it also makes it more likely that our empirical estimates reflect the labor demand effects highlighted by our framework.

Importantly, our main finding that firm-provided training and access to skilled workers are not necessarily substitutes stands in contrast to the results of a concurrent study by Aepli and Kuhn (2021). In part using similar data as we do, they argue that cross-border workers substitute firms' provision of apprenticeships. The differences in results is due to differences in the research designs. Aepli and Kuhn (2021) estimate the effect of cross-border workers on training in cross-sectional regressions that instrument the firms' employment of cross-border workers with an establishment's distance from the border. This approach relies on an exclusion restriction susceptible to biases from unobserved differences between firms correlated with distance to the border.[5] Our DiD approach,

[4]There is a larger literature that analyzes the effects of immigration on the educational choices of residents. Two recent examples are Baechli and Tsankova (2020) and Brunello et al. (2020).

[5]The exclusion restriction of the IV strategy of Aepli and Kuhn (2021) requires that distance to the border affects firms' provision of training only through the greater supply of cross-border workers near the border. However, establishments located close and farther away from the border differ in some important characteristics, some of have been shown to correlate with firms' training behavior (e.g., firms' size and international exposure). Hence, firms located close may train less than firms located farther away from the border because of differences in unobserved dimensions

instead, exploits exogenous variation in the availability of cross-border workers and focuses on over-time changes in outcomes within the same firms. In addition, our paper goes beyond Aepli and Kuhn (2021) by providing empirical evidence on the mechanisms that mediate the impact of skilled workers on firms' training behavior.

Third, by linking our empirical results to the introduction of the free movement policy, our study has important and concrete implications for policy makers. It sheds light on the potential societal costs of open borders for the training opportunities for young adults in the receiving country. Our evidence shows that open borders do not necessarily conflict with the growing importance of apprenticeship and other forms of work-placed training in many countries although they lower firms' costs to hire skilled workers.

This study is organized as follows. Section 2 presents a conceptual framework that guides our empirical analyses. Section 3 provides the institutional background on the reform process of the free movement policy, its effect on apprentices and the apprenticeship system in Switzerland. Section 4 describes the data in the empirical part and the research design. Section 5 presents the empirical results and important robustness checks. Section 6 discusses our main findings and concludes.

# 2 Conceptual framework

We resort to both the literatures on firm-sponsored training (see Picchio and van Ours, 2011, for an overview) and the labor market effects of immigration (see, e.g., Dustmann et al., 2016) to formulate predictions about how open borders affect firms' training decisions. We expect that employment, not wages, is the main margin how firms respond to free movement policy. The reason is that apprentices' wages are inflexible and typically set at the national level.[6]

In the training literature, an important body of papers has highlighted that firms offer apprenticeships to unskilled workers if the benefits outweigh the costs (see, e.g., Wolter and Ryan, 2011). Open borders, by improving firms' access to foreign workers, may influence these cost-benefit considerations. A larger supply of skilled and unskilled workers likely reduces aggregate labor market tightness and improves the matching of workers to jobs (as in, e.g., Chassamboulli and Peri, 2020). Therefore, firms may spend

not accounted for in the regression. In fact, this is what our results in section 5.1 suggest.

[6] We do not expect that the policy had a measurable effect on apprentices' wages because firms are not flexible when setting apprentices' wages. Apprentices' wages are heavily regulated: professional associations publish guidelines how to pay apprentices in each training occupation and year. The wage recommendations are nationwide, i.e. they are not differentiated regionally. As Muehlemann et al. (2007a) write, "wages of apprentices are closely related to wage recommendations of professional associations. Hence, the variation of wages within a profession is relatively small."

less time to find suitable candidates when filling a position or have candidates that better match their needs. If open borders reduce the costs to hire workers on the external labor market, firms' may be less inclined to train unskilled workers, for instance because of reduced future savings in terms of hiring costs associated with training (Blatter et al., 2016; Muehlemann and Strupler Leiser, 2018).

The literature on the labor market effects of immigration has emphasized that the effect of immigration on resident workers depend crucially on the substitutability between immigrant and resident workers. For instance, if immigrant workers are complements to apprenticeships, a greater availability of immigrant workers lowers firms' wage costs, which generates an incentive for firms to expand and to hire more apprentices—the well-known *scale effect* from labor demand theory.

Interestingly, whether we expect a scale effect may depend not only on the skill composition of immigrants, but also on firms' training motives. Firms following an investment motive—which is probably the majority of training firms in Switzerland[7]—incur net cost during the training period. In such firms, apprentices cannot be considered a productive input factor. Instead, these firms train because they can offset the training costs by keeping the apprentice after the training period, thereby saving on costs to hire skilled workers on the external labor market (Oatey, 1970; Lindley, 1975; Merrilees, 1983; Stevens, 1994). Thus, in such firms, the impact of immigration on apprenticeship provision likely depends on the complementarity between immigrants and trained (graduated) apprentices. In contrast, firms that train unskilled workers with a production motive have net benefits during the training period: their training costs are offset by the productive work of the trainee (Lindley, 1975). In such firms, the impact of immigration likely depends on the complementarity between immigrants and apprentices.

In sum, we expect that opening the borders may affect apprenticeship provision through a cost effect that depends on the extent to which a greater supply reduces firms' hiring costs. A scale effect may work in the opposite direction. The strength of this effect likely depends on the degree of complementarity between immigrant workers and apprenticeship graduates and immigrant workers and apprentices. While our empirical analyses below focus on these mechanisms, opening the borders could affect apprenticeship training also through further channels. Examples include potential effects of open borders on firms' productivity (see Beerli et al., 2021), the local demand for goods and services, or between-firm reallocation (as in Mountford and Wadsworth, 2019).

[7]Estimates regarding apprenticeship training in Switzerland suggest that 60% of training firms follow an investment motive (Muehlemann et al., 2007a).

# 3 Institutional background

## 3.1 Agreement on the free movement of persons

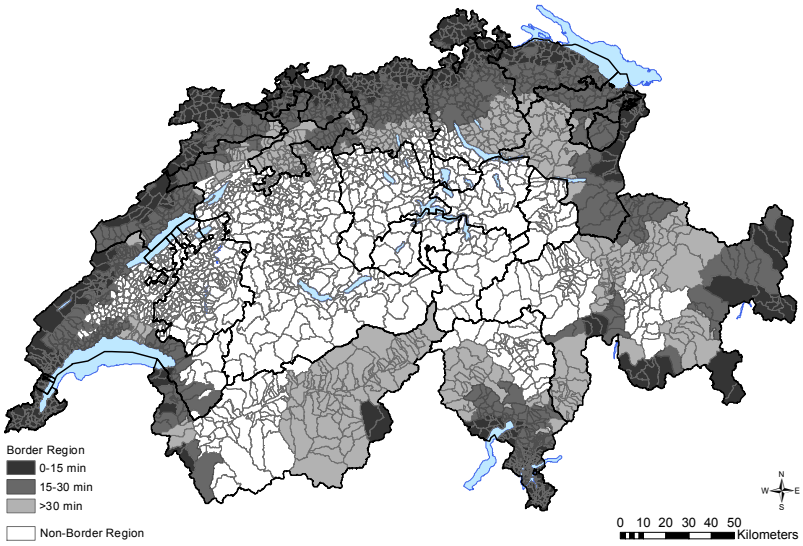
In 1999, Switzerland and the EU signed a bilateral agreement, called the "Agreement on the Free Movement of Person" (hereafter, free movement policy), that introduced free worker mobility for Swiss and EU citizens in Switzerland and the EU. The policy lifted all labor market and immigration restrictions against permanent resident immigrants and cross-border workers from EU countries. In the following, we focus on the liberalizations for cross-border workers because they are the reason why the opening of the labor market had a larger impact on regions close to the border.

There were several hurdles to hiring cross-border workers before the free movement policy came into force. An important restriction was the priority requirement: a firm that wanted to hire a cross-border worker had to provide formal evidence that there was no Swiss worker with skills equivalent to those of the cross-border worker. This priority requirement was enforced through a government-controlled admission process for a cross-border worker permit. Firms had to prepare an application detailing the job requirements, the contract and working conditions, and had to demonstrate that they had searched unsuccessfully in Switzerland for a certain number of weeks. This process took one to three months.

The employment of a cross-border worker was subject to further conditions. A permission to work as a cross-border worker was bound to the specific job, valid for one year only, and granted only to individuals that had lived in a municipality close to the Swiss border for at least six months. Cross-border workers were also required to commute home daily. Moreover, the hiring firm on the Swiss side had to be situated within the so-called "border region", a set of well-defined Swiss municipalities located in vicinity of the Swiss border. Figure 1 shows the border region in Switzerland and illustrates the estimated travel time to the closest border crossing. The border region had been defined bilaterally with each neighboring country between 1928 and 1973 in specific agreements. It is specific to these contracts—it does not follow any cultural, religious, or administrative borders.

The free movement policy abolished these restrictions in a stepwise process. The first liberalization step occurred in June 2002 and removed the geographical and occupational restrictions for cross-border workers. Swiss firms in the border regions could now hire anybody from the neighboring countries—the hiring zone was no longer restricted to the border region on the other side of the border. Similarly, cross-border workers were no longer required to go back home daily and to have lived within the border region for at least six months. Finally, their working permits were now valid for five years at every employer. Hence, both new and incumbent cross-border workers now enjoyed unrestricted

Figure 1: The Swiss border region by travel time to the closest border crossing




Source: Beerli et al. (2021)

*Notes*: This figure shows the municipalities belonging to the Swiss border region by car travel times to the nearest border crossing. The regions belonging to the non-border regions are in white. The cantonal borders are shown with black, municipal border with grey lines. A canton is a sub-regional entity similar to U.S. states.

job mobility within Switzerland.

The second reform step happened in June 2004. It abolished the priority requirement and the bureaucratic admission process that came along with it. The final reform step in 2007 abolished the "border region". From 2007 onward, all firms in Switzerland gained permission to hire cross-border workers. By implication, the liberalizations in 2002 and 2004 only applied to firms in the border region.

Four further characteristics of the free movement policy are important to understand our approach and our results. First, we view the policy as an exogenous increase *in the availability of foreign cross-border workers*. This interpretation encompasses that the policy likely had effects on firms beyond those mediated through the increased supply of foreigners. Following Beerli et al. (2021), we thus do not present instrumental variable estimates that use the policy to instrument for the cross-border worker or immigrant share in a region. Instead, we focus on the (policy-relevant) reduced-form effects of opening the border.[8]

[8]For instance, the policy increased the geographical and occupational mobility of the pre-existing stock of cross-border workers. It also plausibly reduced firms' hiring costs directly by abolishing the priority requirement and by improving firms' chances to find skilled workers. These effects would invalidate the exclusion restriction of an instrumental variable strategy in which the increased supply is instrumented with the policy.

Second, it is possible that the reform caused anticipation effects after the policy's approval by the Swiss parliament. Beerli et al. (2021) provide evidence that certain cantonal offices started to handle cross-border worker applications in a more relaxed way because it became clear that cross-border workers' labor market access would be liberalized eventually.

Third, although the free movement policy allowed *Swiss* cross-border workers to work in Switzerland's neighboring countries, our estimates likely reflect the effects of a greater availability of foreign workers to Swiss firms. The reason is that cross-border commuting out of Switzerland remained almost negligible despite a lack of restrictions, probably because it is financially unattractive.[9] In part for the same reason, we do not expect that the policy affected Swiss firms' training through a greater fear that foreign firms poach the trained workers.

Fourth, the policy, in principle, increased the supply of apprentices as it made it easier for firms to employ cross-border workers as apprentices. However, this phenomenon remained very limited quantitatively in all cantons except the canton of Ticino, anecdotally in part because cantonal employment offices remained reluctant to grant cross-border permits to apprentices even in the free movement period.[10] We thus view it as quite unlikely that the policy change that we study should be primarily interpreted as a shock to the supply of apprentices.

## 3.2 The exposure of apprentices to cross-border workers

Lifting the restrictions on cross-border workers caused an unprecedented growth in the employment of cross-border workers. As Beerli et al. (2021) show, this growth was substantially larger in regions close to the border. According to their DiD estimates, the number of foreign workers as a share of 1998 employment grew 10 percentage points more in municipalities within 15 minutes travel time to the border compared to municipalities more than 30 minutes away from the border. Beerli et al. (2021) also find that two thirds

[9]Both nominal wages and the cost of living are substantially higher in Switzerland. Therefore, the influx of foreign cross-border workers into Switzerland was nine times higher than the influx of Swiss cross-border workers into its neighbor countries (Beerli et al., 2021).

[10]Legally, the border opening made it easier for firms to hire unskilled cross-border workers as apprentices. However, with the singular exception of the canton of Ticino, where cross-border apprentices represented 8% of apprentices in 2013, the numbers of cross-border apprentices remained very low even in the 2010s. The reasons for the low numbers are likely manifold, and probably include problems to certify Swiss apprenticeship degrees in the country of residence, the lower social status of apprenticeships in some of the Swiss neighboring countries, and a reluctance of many cantonal employment offices to grant cross-border permits to apprentices even in the free movement period. See Gross (2013), "Lehrlinge und Grenzgänger. Unterstützung der betroffenen Kantone", https://www.parlament.ch/en/ratsbetrieb/suche-curia-vista/geschaeft?AffairId=20131073.

of the immigrants who came to Switzerland because of the opening of the borders were tertiary qualified. Consistently, the immigration effect of the free movement policy was particularly large in higher-skilled and better-paid occupations.

Apprentices in regions close to the border were exposed to this inflow for two reasons. First, despite of the fact that many new cross-border workers were tertiary-educated, they often started to work in occupations and sectors with a high share of apprenticeship graduates. This is illustrated in Figure 2 and Appendix Tables B.1 and B.2 using data from the Swiss Earnings Structure Surveys (see Beerli et al., 2021, for details).

Figure 2 compares an occupation's employment share of recent native apprenticeship graduates in 1998 with the change in the employment share of cross-border workers in total employment between 1998 and 2010. Recent native apprenticeship graduates are Swiss workers aged 18–29 with an apprenticeship as highest educational degree. The figure shows that many occupations with a high share of young recent graduates from apprenticeship tracks saw a strong growth in the employment of cross-border workers in the years following the labor market liberalization. Examples include clerical occupations, machine operators, occupations involving medical and social tasks, and occupations in the construction and retail sectors. A similar conclusion can be drawn from the sectoral composition of the immigrant inflow.

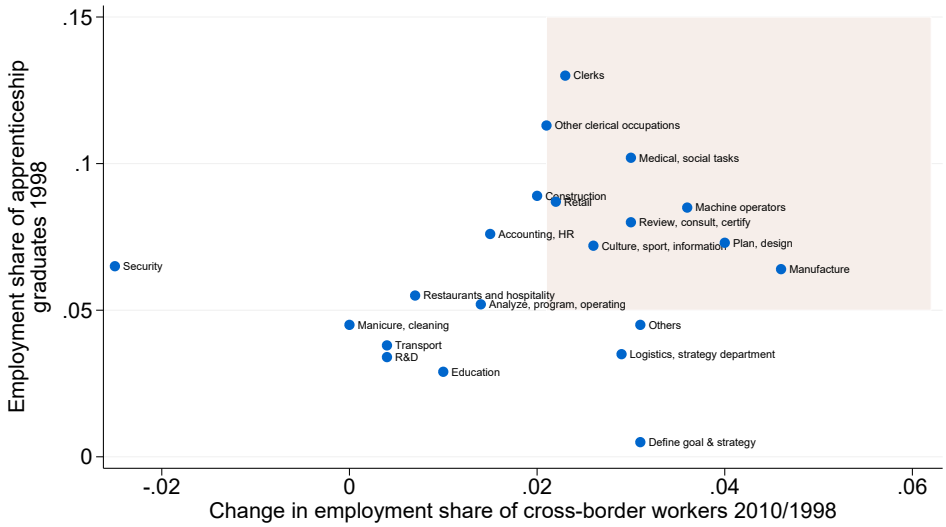
Appendix Tables B.1 and B.2 show that firms in the manufacturing, IT, R&D, business services, real estate, and in the health sector hired many new cross-border workers after the borders were opened. Apprenticeship graduates represent a sizeable share of the workforce in these sectors.

The second reason why apprenticeship graduates were exposed to the border opening is that the skill content of the immigrant inflow is an equilibrium outcome. The policy opened the labor market for all EU workers, so firms in principle could have hired cross-border workers to substitute any apprentice or apprenticeship graduate. Hence, apprentices were exposed to the border opening even in labor market segments where we effectively see little inflows of cross-border workers. The reform provided firms with the trade-off to either hire cross-border workers or train apprentices.

## 3.3 Vocational education and training in Switzerland

The fact that apprenticeship graduates often work in the same occupations as tertiary-educated cross-border workers in part reflects the differences in education systems between Switzerland and some of its neighboring countries, where apprenticeships are much less common. In Switzerland, vocational education and training is the largest program at the upper-secondary education level. Each year, about two-thirds of students who complete

Figure 2: Employment share of recent apprenticeship graduates in 1998 and change in employment share of cross-border workers 1998–2010, by occupation




*Notes*: This figure compares the employment share of recent (native) apprenticeship graduates in 1998 in an occupation with the change in the employment share of cross-border workers in total employment between 1998 and 2010. The figure is based on data from the Swiss Earnings Structure Surveys 1998 and 2010. The employment share is the fraction that the two worker categories make up in the specific occupation in the border region. We focus on workers aged 18–65 in the private sector, with non-missing information for nationality, place of work, education, wages, and full-time equivalents. Cross-border workers are identified based on their residency permit. Recent native apprenticeship graduates are Swiss workers aged 18–29 with an apprenticeship as highest degree. The same data is shown in Table B.2 in tabular form.

compulsory education begin a vocational education and training program. Nine out of ten students take a dual vocational education and training program, which is also referred to as apprenticeship. Apprentices typically spend one to two days at a vocational school, where they obtain formal education, and three to four days at a training firm, where they learn practical skills and acquire work experience.

Apprenticeship programs last three to four years. In this period, students follow a structured national curriculum, which ends with a final external examination that leads to a national diploma. The national diploma seeks to ensure that graduates learned a defined set of skills in each occupation. Since these skills are transferable between firms within an occupation, apprenticeships are considered as general skills training (Mueller and Schweri, 2015). Thus, apprenticeship training in Switzerland should not be confused with an internship, a traineeship, or an active labor market or on-the-job training program. Instead, they should be viewed as a complete, accredited, and regulated training program that is recognized and valued by Swiss employers (ILO, 2019).

Nevertheless, firms decide on their own whether to participate in training apprentices—providing apprenticeships is neither mandatory nor subsidized (Wolter et al., 2006). Training apprentices can generate net costs for firms. Dionisius et al. (2009) estimate that Swiss apprentices, by their final year, reach 75% of the productivity of an average skilled worker

at a firm. Survey estimates suggest that roughly a third of all apprentices are associated with—sometimes substantial—net costs for firms.[11] As hypothesized in section 2, a larger supply of foreign workers may thus affect the balance between training and hiring skilled workers externally.

# 4 Empirical strategy

## 4.1 Data

Our empirical analyses are based on two datasets. The first is the Swiss business censuses from 1995, 1998, 2001, 2005, and 2008. The censuses provide information on the total employment and workforce composition for all private and public establishments in Switzerland in September. Since apprentices are a separate category of workers, we can precisely measure employment of apprentices for all firms in Switzerland. Moreover, the data allow us to precisely compute each establishment's distance to the nearest border crossing because they contain the precise geo-coordinates of each establishment.[12] We assign each establishment to its 1998 location in all sample years to avoid biases from endogenous relocation of businesses.

We impose three sample restrictions: we drop establishments from the agricultural sector because the sector was not covered in all waves, a small number of establishments that we cannot assign to the border or non-border region with certainty, and establishments that did not exist in 1998 when the free movement policy was announced. In our preferred specifications, we additionally focus on a fully balanced sample of firms that existed throughout 1995–2008. Balancing the sample ensures that compositional effects do not drive our results. These effects could arise because the free movement policy led to the entry of new firms in the highly treated region, as shown by Beerli et al. (2021). However, restricting on establishments existing in 1998 may lead to survivorship biases. We provide evidence suggesting that such biases are unlikely in the robustness section.

Our second dataset is three waves of an administrative and representative survey on the costs and benefits of apprenticeship training in Switzerland.[13] The unit of observation in these surveys is a firm. The surveys were conducted in 2000, 2004, and 2009 and sys-

[11] As long as firms comply with the regulations in the federal act on vocational and professional education and training, they may also freely decide in which of the 240 occupations they offer an apprenticeship and to whom. Schweri et al. (2003) present an overview of the factors influencing the costs and benefits of apprenticeship training in Swiss firms.

[12] We use the same data as Beerli et al. (2021) on the location of border crossings in Switzerland to compute the travel duration to the nearest border crossing.

[13] These data have been used extensively to study apprenticeship training. See Backes-Gellner et al. (2020), which contains references to several studies using this data.

tematically cover a firm's recruitment and training behavior. In general, firms answer the questions for one particular occupation in which they train apprentices. Companies that train in different occupations were given one focus occupation. Non-training firms were asked to answer many questions in the surveys, too. They could indicate the occupation in which they would be most likely to train if they were to train.

We impose two sample restrictions in the cost-benefit data. First, we drop companies that responded in four occupations where we have no data from firms that do not train. Second, we drop firms where we are unsure about their assignment to the border or non-border region. We end up with 11'067 observations for non-training firms and 4'604 observations for training firms.

A disadvantage of the cost-benefit data is that the data are cross-sectional. Thus, we cannot estimate firm fixed effects regressions. The advantage is that the data provide direct information on rarely measured outcomes including firms' recruitment and training costs and qualitative survey questions on firms' training motives. For instance, firms were asked to assess whether they train to attract skilled workers, save hiring costs for external workers, or save adjustment costs.[14]

Table 1 provides descriptive statistics for the main variables in the two datasets and compares the pre-reform characteristics of establishments in the four regions that we will compare in our DiD estimations: establishments within the border region within 15 minutes of the border, establishments within the border region 15–30 minutes of the border, and establishments more than 30 minutes of the border within the border region and the non-border region.

Panel A of the table shows that roughly one of four establishments engages in apprenticeship training (24.8%). The average number of apprentices per establishment is 0.65, or 5.5% of total full-time equivalent employment. Among training firms, the apprentice share is 22.3%. As expected, the employment of cross-border workers is concentrated close to the border. A striking fact is that firms close to the border are substantially less likely to train apprentices but more likely to employ foreign workers than firms farther away from the border. Turning to the cost-benefit data (panel C of Table 1), we see that the three most important motives for training apprentices are to qualify junior staff into skilled workers, to attract skilled workers, and to secure skilled workers in the sector/region.

Table 1 also suggests that the four regions are quite comparable in a number of important pre-reform characteristics such as the industry composition. Beerli et al. (2021) additionally show that the regions are also similar in terms of labor market size, a few important worker characteristics, and workers' mean log hourly wages. However, we also

[14] Appendix Table B.3 provides the list of training motives levied in the surveys and an English translation of the exact survey questions.

observe some relevant pre-treatment differences across regions. For instance, establishments within 15 minutes to the border employ more foreigners and are more likely to be exporters and importers. They are also somewhat larger. Given these differences in pre-treatment establishment characteristics, we will probe the robustness of our results if we use covariate matching to generate a control group comparable to establishments close to the border in terms of these and other pre-treatment characteristics.

## 4.2 Regression model

We analyze the effects of the free movement policy by following Beerli et al.'s (2021) transparent Difference-in-Differences approach. The approach leverages the larger impact of lifting restrictions for cross-border workers on establishments near the border. We assign firms in Switzerland to one of three groups: Establishment $i$ is considered to be *highly treated* if it is located within 15 minutes travel time of the border within the "border region", $I(d_i \leq 15)$. It is considered to be *slightly treated* if it is located 15–30 minutes of the border in the border region, $I(15 < d_i \leq 30)$. An establishment serves as a *control* establishment if it is either located within or outside of the border region. We generally pool the two control groups because neither control group is clearly more similar to the highly treated group in terms of observables (see Table 1). However, we also show the most important results for the two control groups separately.

To estimate the effect of the free movement policy, we interact the indicators of the three distance groups with indicators of the year $t$. For the business censuses, which took place in 1995, 1998, 2001, 2005, and 2008, we estimate variants of the following DiD model:

$$\begin{aligned} y_{i,t} = \beta_{high}^{t=2001}[I(d_i \leq 15) * I(t = 2001)] \\ +\beta_{high}^{t\geq 2005}[I(d_i \leq 15) * I(t \geq 2005)] \\ +\beta_{slight}^{t=2001}[I(15 < d_i \leq 30) * I(t = 2001)] \\ +\beta_{slight}^{t\geq 2005}[I(15 < d_i \leq 30) * I(t \geq 2005)] \\ +\gamma Supply_{j(i),t} + \alpha_i + \alpha_t + \epsilon_{i,t} \end{aligned} \tag{1}$$

$y_{i,t}$ represents the training or employment outcome of firm $i$ in year $t$. The $\beta$ coefficients capture the DiD of this outcome for highly treated firms ($\beta_{high}$) and slightly treated ($\beta_{slight}$) firms *relative* to control establishments. For both slightly and highly treated establishments, we estimate separate effects for the year 2001, to capture anticipation effects of the reform, and for the years 2005 and 2008 to capture an average impact one and four years after full liberalization. Note that the estimation does not leverage the 2007

Table 1: Firm characteristics before the reform, by region and distance to the border

| *Travel time to border* | **Border region** | | | | | | **Non-border region** | |
|---|---|---|---|---|---|---|---|---|
| | ≤ 15 min | | 15–30 min | | >30 min | | | |
| | Mean | SD | Mean | SD | Mean | SD | Mean | SD |
| ***A. Business census (all establ.)*** | | | | | | | | |
| FTE employment | 11.46 | (57.24) | 12.14 | (78.40) | 9.53 | (43.41) | 9.89 | (38.44) |
| Foreign share (in FTE, 1995) | 0.24 | (0.32) | 0.16 | (0.27) | 0.14 | (0.25) | 0.10 | (0.21) |
| Cross-border worker share (1995) | 0.08 | (0.17) | 0.01 | (0.05) | 0.00 | (0.04) | 0.00 | (0.01) |
| Share of other foreigners (1995) | 0.17 | (0.27) | 0.15 | (0.25) | 0.13 | (0.24) | 0.10 | (0.21) |
| Firm growth 1991–1998 in % | 11.44 | (67.99) | 12.19 | (68.55) | 10.72 | (64.65) | 11.39 | (64.73) |
| Training firm (0/1) | 0.21 | (0.41) | 0.25 | (0.43) | 0.25 | (0.43) | 0.28 | (0.45) |
| Apprentice share | 0.04 | (0.11) | 0.05 | (0.12) | 0.06 | (0.13) | 0.06 | (0.13) |
| Manufacturer (0/1) | 0.12 | (0.32) | 0.11 | (0.32) | 0.12 | (0.33) | 0.13 | (0.33) |
| High-tech manufacturer (0/1) | 0.03 | (0.18) | 0.03 | (0.17) | 0.03 | (0.18) | 0.03 | (0.17) |
| Construction firm (0/1) | 0.09 | (0.28) | 0.09 | (0.29) | 0.10 | (0.30) | 0.11 | (0.31) |
| Publicly owned firm (0/1) | 0.12 | (0.33) | 0.13 | (0.33) | 0.14 | (0.34) | 0.14 | (0.35) |
| Exporter (0/1, 1995) | 0.16 | (0.37) | 0.15 | (0.35) | 0.12 | (0.33) | 0.11 | (0.31) |
| Importer (0/1, 1995) | 0.26 | (0.44) | 0.26 | (0.44) | 0.22 | (0.41) | 0.21 | (0.41) |
| Travel minutes to border | 7.13 | (3.54) | 23.36 | (4.07) | 39.25 | (9.92) | 53.65 | (13.57) |
| Observations | 42623 | | 55500 | | 26905 | | 55947 | |
| ***B. Business census (training establ.)*** | | | | | | | | |
| FTE employment | 27.59 | (110.79) | 27.59 | (150.66) | 21.34 | (80.85) | 20.79 | (68.54) |
| Foreign share (in FTE) | 0.24 | (0.26) | 0.16 | (0.22) | 0.13 | (0.20) | 0.09 | (0.17) |
| Apprentice share | 0.21 | (0.15) | 0.22 | (0.15) | 0.23 | (0.15) | 0.23 | (0.15) |
| Observations | 8907 | | 13718 | | 6774 | | 15468 | |
| ***C. Cost-benefit surveys*** | | | | | | | | |
| Training firm | 0.14 | (0.35) | 0.15 | (0.36) | 0.20 | (0.40) | 0.18 | (0.39) |
| Number of apprentices | 0.35 | (1.57) | 0.42 | (2.17) | 0.46 | (1.64) | 0.44 | (1.43) |
| Attract skilled workers | 3.82 | (1.24) | 3.86 | (1.18) | 3.75 | (1.19) | 3.89 | (1.16) |
| Hiring costs | 2.69 | (1.15) | 2.62 | (1.12) | 2.50 | (1.13) | 2.56 | (1.12) |
| Avoid adjustment costs | 2.84 | (1.25) | 2.79 | (1.22) | 2.74 | (1.19) | 2.78 | (1.15) |
| Insufficient qualifications | 0.26 | (0.44) | 0.24 | (0.43) | 0.17 | (0.38) | 0.18 | (0.39) |
| Skills shortage | 0.40 | (0.49) | 0.38 | (0.49) | 0.32 | (0.47) | 0.35 | (0.48) |
| Work permits (foreigners) | 0.14 | (0.35) | 0.12 | (0.33) | 0.11 | (0.31) | 0.10 | (0.29) |
| Interviews per job | 4.70 | (3.20) | 4.60 | (3.37) | 4.71 | (3.09) | 4.36 | (2.88) |
| Interview time in hours | 5.53 | (6.08) | 6.85 | (7.71) | 5.92 | (5.80) | 6.67 | (7.32) |
| Advertisement costs per job (in CHF) | 1136.27 | (1711.85) | 1432.91 | (2057.26) | 1384.17 | (2609.42) | 1364.10 | (2353.54) |
| Costs of external consultant (in CHF) | 858.59 | (2459.39) | 972.66 | (2854.31) | 712.90 | (2678.17) | 857.38 | (2562.92) |
| Training period in months | 3.90 | (3.48) | 4.13 | (3.26) | 4.11 | (3.20) | 3.86 | (3.39) |
| Shortfall in productivity | 30.10 | (15.03) | 29.65 | (14.62) | 27.32 | (13.64) | 26.95 | (13.71) |
| Costs for further education (in CHF) | 827.17 | (2594.15) | 644.73 | (1876.69) | 811.91 | (1998.66) | 653.07 | (1962.23) |
| Observations | 725 | | 1077 | | 562 | | 949 | |

*Notes:* The table shows descriptive statistics of establishments in the border and non-border region using the business census (BC, panels A and B) in 1998 (unless noted otherwise) and firm characteristics according to the cost-benefit survey in 2000 (panel C). The border region is split into groups depending on firms' travel duration to the nearest border crossing. The data in panel A is restricted to establishments existing throughout 1995–2008. The "share of other foreigners" encompasses all non-Swiss workers that are not cross-border workers. "Firm growth 1991–1998 in %" reflects an establishment's growth in FTE employment between 1991 and 1998, winsorized at the first and 99th percentile. Panel B focuses on establishments that train apprentices. Panel C shows descriptive statistics using the cost-benefit data in 2000. "Attract skilled workers," "hiring costs," and "avoid adjustment costs" are training motives. They are measured on a 5-point Likert scale. The table shows the mean of the ordinal variable. "Insufficient qualifications," "Skills shortage," and "Work permits" are perceived personnel problems that firms either agree to or not. The remaining variables from the cost-benefit dataset are explained in the notes to Table 6. Appendix Table B.3 also provides more details on the variables from the cost-benefit dataset.

abolition of the border region because the 2007 liberalizations affected very few firms.[15]

The further elements in equation (1) are control variables and fixed effects. $Supply_{j(i),t}$, added to some specifications, is a control variable reflecting the potential supply of apprentices in the local labor market. It measures the number of graduates from lower secondary schools (11th grade) in an establishment's commuting zone $j$ (NUTS-III region) in a given census year. The variable controls for possible trend differences in the supply of potential apprentices across regions. In addition, we control for year fixed effects ($\alpha_t$), which capture aggregate macroeconomic shocks common to all firms such as changes in aggregate prices and foreign demand, and establishments fixed effects ($\alpha_i$), which control for the baseline effects of establishments' distance to the border ($d_i$) and other pre-existing differences between establishments and regions.

Intuitively, this DiD model assesses whether highly and slightly treated establishments display different over-time changes in establishment-demeaned outcomes compared to establishments in the control group. Appendix Figure B.1 provides a graphical illustration of the identifying variation in our DiD design. Empirically, the DiD estimates capture all effects of the border opening that affect firms closer to the border more than firms located more than 30 minutes away from the border. While we view it as plausible that the DiD estimates mainly reflect effects operating through labor market channels, they capture all effects of the policy that are stronger closer to the border, including potential consumption-side effects (e.g., additional demand for restaurants serving lunch).

The central identifying assumption of the design is that establishments in the three regions would have, on average, had the same *change* in firm-demeaned outcomes had the border not opened. This common trend assumption is violated, for instance, if there are unobserved third factors that affect the training behavior of firms near the border differently from firms farther away at the time of the border opening. Potential confounders are simultaneous, region-specific policy changes or shocks to prices, demand, or productivity that have region-specific effects because of differences in the sectoral composition between regions. Section 5.4 thus presents several robustness checks that probe the robustness of our results if we control for more restrictive sets of fixed effects such as industry times year or labor market region times year fixed effects.

We also analyze the plausibility of the common trend assumption by assessing pre-treatment trends in outcomes. Therefore, we also estimate an event study version of equation (1) that contains year-specific effects for each census year except 1998, which

[15] Almost all firms in the non-border region experienced negligible increases in employment of cross-border workers even after 2007, most likely because most of them are just located too far away from the border (i.e., more than 30 minutes) to attract them (see panel A of Figure 3 below). We discard the few firms located in the non-border region that are located within less than 30 minutes to the border to avoid any confounding from the 2007 policy change.

serves as the reference year. The event study allows us to examine whether establishments near and farther from the border displayed similar trends in training outcomes before the free movement policy.

Admittedly, however, our possibilities to assess pre-trends are somewhat limited since our micro data do not cover a long pre-treatment period. Against this background, it is reassuring that Beerli et al. (2021), and the various follow-up papers that use the same research design (Ariu, 2022; Baechli and Tsankova, 2020; Cristelli and Lissoni, 2020; Naguib, 2019), present evidence of common pre-trends for a variety of firm outcomes, including firm size, productivity, wages, worker composition, innovation, and patents, in some cases with data that goes back until the 1980s. Moreover, the long-run trends in apprenticeship provision between 1985 and 1995 were similar in regions close and farther away from the border. In particular, Figure B.2 suggests that the long-run trends in apprenticeship provision in a canton are, by and large, unrelated to the canton's travel distance to the border. Indeed, if we use these cantonal data to regress the change in the training share and the number of apprentices on two dummy variables indicating cantons that are within 15–30 minutes and cantons more than 30 minutes away from the border (see Table B.4), we find a small and statistically insignificant correlation between cantonal trends in apprenticeship provision and a canton's distance to the border. Lastly, we also present DiD estimates that use a matched control group similar to treated units in terms of several pre-treatment observables, including industry affiliation, international exposure, and firm size. This approach is robust to nationwide shocks that affect firms differently along these or correlated dimensions.

# 5 Results

This section explores the causal effect of a greater availability of foreign workers on the establishments' willingness to train unskilled workers.

## 5.1 Descriptive evidence

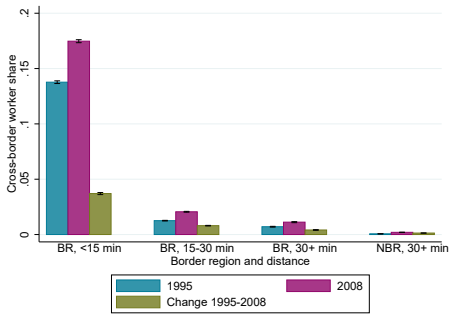
Figure 3 shows the employment of cross-border workers and training provision separately for highly treated establishments, slightly treated establishments, and establishments in the two control groups. Panel (a) uses the censuses in 1995 and 2008, which contain establishments' employment of cross-border workers, to show the share of cross-border workers in total full-time equivalent employment. The figure demonstrates that employment of cross-border workers is highly concentrated near the border, both before and after the free movement of workers. In 2008, cross-border workers made up more than a sixth of the

workforce in establishments within 15 minutes to the border.

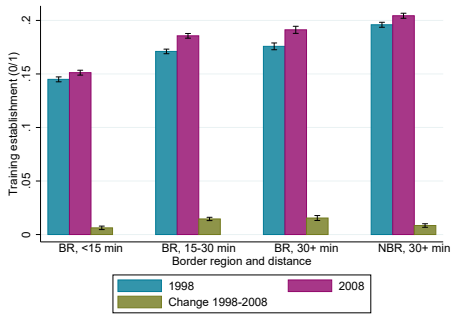
Panel (b) of Figure 3 shows the fraction of establishments engaged in the training of apprentices. Panel (c) shows the percentage of apprentices in total employment. The two figures demonstrate that establishments near the border offer remarkably fewer training positions than establishments farther away. The magnitudes are considerable: the share of apprentices in total employment is 34% lower in establishments within 15 minutes compared to those located at least 30 minutes away from the border. Similarly, the share of firms that train at least one apprentice (panel b) is 24% lower.

Figure 3: Employment of cross-border workers and apprentices by region

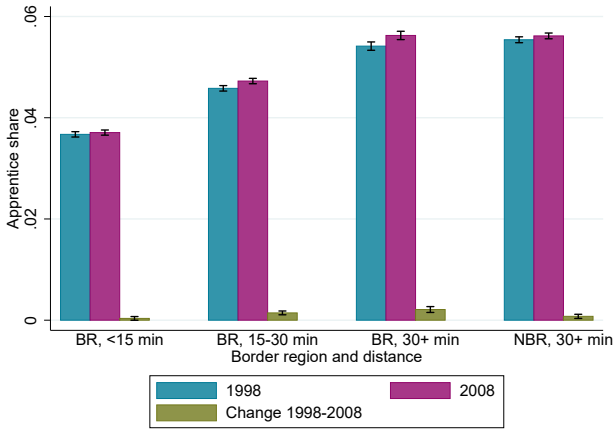
(a) Cross-border worker share




(b) Share of training establishments




(c) Apprentice share




*Notes*: This figure shows establishments' cross-border worker share and training provision by travel distance in minutes to the nearest border crossing. The figure uses data from the business censuses. We differentiate highly treated establishments (establishments within 15 minutes of the border, "BR, <15 min"), slightly treated establishments (establishments within 15–30 minutes of the border, "BR, 15–30 min"), establishments more than 30 minutes of the border within the border region ("BR, 30+ min"), and establishments in the non-border region ("NBR, 30+ min"). Panel (a) shows the employment percentage of cross-border workers. We use 1995 data because the 1998 census provides no information on cross-border workers. Panel (b) shows the fraction of establishments that train apprentices. Panel (c) shows the employment percentage of apprentices. The fractions in panels (a) and (c) are employment-weighted. The black lines show the standard errors of the sample means.

The subfigures also present the longer-term changes in each variable within each region. Panel (a) reveals that the growth in the employment share of cross-border workers between 1995 and 2008 is substantial (about 4 percentage points) and concentrated close to the border, too. Despite of this increase, we observe an increase in the share of firms training apprentices and the apprentice share in employment in highly treated establishments between 1998 and 2008. An increase is observed in establishments further away from the border, too. However, there is no clear indication that the growth in training provision is smaller in the highly treated region than in regions further away from the border.

Table 2 contains four regressions that reveal the cross-sectional and within-establishment correlation between the employment shares of apprentices and cross-border workers in the business census. We focus on the census years 1995, 2005, and 2008 because these are the only waves that provide data on both types of workers. Column 1 shows that firms with more cross-border workers employ less apprentices. The correlation between the two employment shares does not change much if we control for a few observable establishment characteristics (column 2). However, columns 3 and 4 demonstrate that the negative correlation becomes three to four times smaller and closer to zero once we add establishment fixed effects and thus focus on changes in employment shares within the same establishment. The comparison shows that unobserved but time-invariant differences between firms may explain a sizeable part of the negative cross-sectional correlation between the two shares.

Taken together, we find that employment of cross-border workers and apprenticeship training are inversely related in the cross-section, suggesting that the two are substitutes. In fact, Aepli and Kuhn (2021) argue based on regressions and a sample similar to the ones in column 3 of Table 2 that cross-border workers displace apprentices. However, analyses that focus on changes over time indicate that other differences between firms near and farther from the border may explain why the former train less[16], an issue that the IV strategy of Aepli and Kuhn (2021) may not overcome. In the next section, we thus present DiD estimates that account for time-invariant differences between firms close and farther from the border, and we isolate the changes in firms' propensity to train in response to a large, exogenous increase in the availability of cross-border workers.

[16]For example, firms close to the border are more likely to be importers and exporters than firms farther away (see Table 1). Swiss firms with international exposure have a lower propensity to train apprentices (Muehlemann, 2014).

Table 2: Correlation between the apprentice and cross-border worker share across and within firms

| VARIABLES | (1) OLS | (2) OLS | (3) FE | (4) FE |
|---|---|---|---|---|
| Cross-border worker share | -0.070*** | -0.067*** | -0.017*** | -0.026*** |
| | (0.001) | (0.001) | (0.002) | (0.002) |
| Observations | 637,152 | 637,151 | 492,308 | 492,307 |
| R-squared | 0.005 | 0.128 | 0.784 | 0.788 |
| Year FE | No | Yes | No | Yes |
| Industry FE | No | Yes | No | Yes |
| Canton FE | No | Yes | No | Yes |
| Establishment size controls | No | Yes | No | Yes |
| Establishment FE | No | No | Yes | Yes |

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

*Notes:* This table shows the correlation between apprentice and cross-border worker share in regressions with and without establishment fixed effects. The regressions are based on the business censuses 1995, 2005, and 2008. The dependent variable is an establishment's number of apprentices relative to total employment. Industry fixed effects are fixed effects by two-digit NACE code (rev. 1.1). Following Aepli and Kuhn (2021), the sample is restricted to establishments with at least three workers. Standard errors are clustered on the establishment level. ***, **, *, denote statistical significance at the 1%, 5%, and 10% level, respectively.

## 5.2 Effect on the employment of foreign workers

Using data from the business census 1995–2008, Figure 4 shows estimates of the effect of the free movement policy on the employment of foreign workers in highly and slightly treated establishments. The figure shows the results of an event study version of equation (1) that separately estimates the policy effects for each census year. The dependent variable is full-time equivalent employment of foreign workers—cross-border workers plus resident workers without a Swiss passport—relative to establishments' total full-time equivalent employment in 1998, when the policy was announced. This outcome retains firms without foreign workers. Because we hold the denominator fix, it also separates an effect on foreign employment from a possible effect on firm size. We winsorize the outcome at the top 0.01% value to reduce the influence of very few extreme outliers. We use our preferred sample: all establishments that exist throughout 1995–2008 (see section 4.1). To allow for arbitrary dependence between units within the same commuting zone (both cross-sectional dependence and over time), we cluster standard errors at the level of commuting zones (NUTS-III regions), which is arguably quite conservative with the full-population census data. Finally, we weight the regression by firm size prior to the policy change.[17]

[17]Initial firm size is a firm's average full-time equivalent employment in the waves 1995 and 1998 of the census. In estimations that incorporate firms founded after 1998, we use a firm's size in the first census wave that the firm appears.

We discuss how these specification choices affect our results in section 5.4.

Figure 4 shows that full-time equivalent employment of foreign workers as a percentage of 1998 employment grows consistently more in highly treated establishments after 1998 than in establishments in the control regions. By 2008, the excess increase amounts to approximately six percentage points. Since the average highly treated establishment employed 11.46 FTE workers in 1998 (see Table 1), foreign employment grew by roughly $6\% * 11.46 * 42'623 = 29'100$ foreigners more in the 42'623 highly treated establishments compared to the establishments in the two control groups. As expected, we find a smaller impact on slightly treated establishments—those located between 15–30 minutes to the border. Importantly, none of the placebo effects for the 1995–1998 period is significantly different from zero, suggesting that the trends in foreign workers' employment in the treatment and control groups are similar in this period.

Panel A of Table 3 uses the same outcome and the main DiD model (equation (1)) to provide two additional insights. The effect of the free movement policy on highly treated establishment is captured by the interaction term $I(t \geq 2005) \cdot I(d_{\text{i}} \leq 15)$. The table, first, shows that the impact on highly treated establishments is very similar in establishments that employ at least one apprentice throughout 1995–2008 (column 4). Second, the estimated effects are quantitatively similar if we use the two control groups separately (columns 5 and 6).

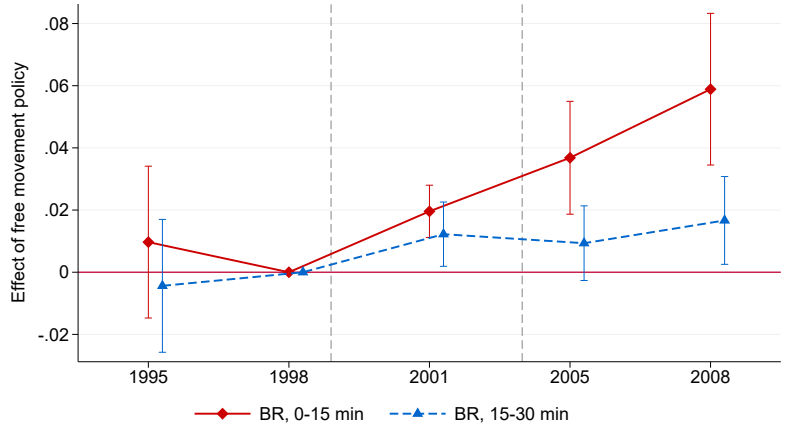
Overall, these findings confirm that the opening of the Swiss labor market to cross-border workers had a large positive impact on the employment of foreign workers in firms in the border region. The section extends similar results by Beerli et al. (2021) by showing that this effect pertains to establishments that train apprentices.

## 5.3 Effect on the firms' provision of training

Did the free movement policy, and the growth in employment of skilled foreign workers in training firms to which the policy led, affect the number of apprenticeships that establishments offer? We present our main DiD estimates based on the censuses 1995–2008 in Panel B of Table 3. Figure 5 shows the corresponding event study. The outcome variable is the number of apprentices trained in an establishment relative to total employment in 1998, consistent with the outcome in the previous section. As before, we winsorize the outcome and weight observations using establishments' pre-1999 employment.

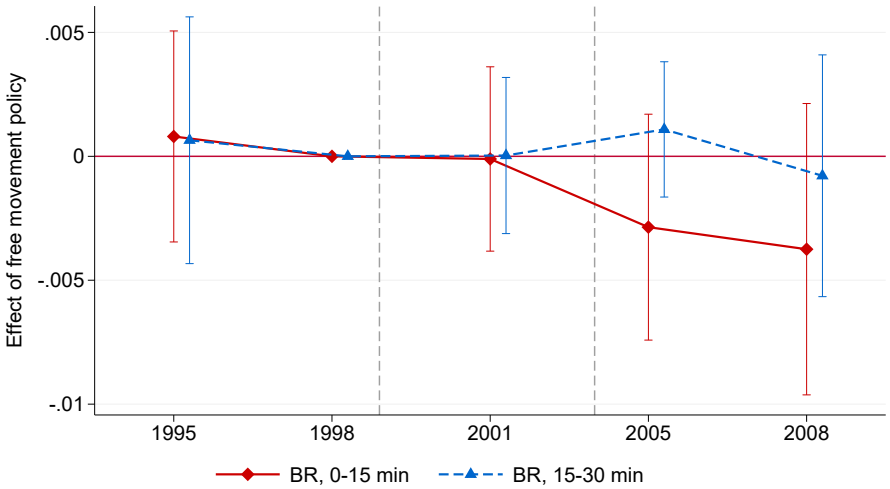
The event study in Figure 5 does not suggest that the greater availability of foreign workers affected firms' provision of apprenticeships. Relative to the two control groups, apprenticeships begin to decline slightly in highly treated establishments after 1998. By 2008, the negative point estimate amounts to 0.4 percent of total 1998 full-time equivalent

Figure 4: Timing of effect of free movement policy on foreign employment




*Notes:* The figure shows the effect of the free movement policy on establishments' employment of foreign workers using data from the Swiss business censuses 1995–2008. It plots the estimated policy effects and associated 95% confidence intervals using a generalization of our main DiD model (equation (1)) that estimates separate effects for each census year. We standardize the effects to 0 in 1998 by dropping the indicator for that year from the regression. The estimation sample comprises all establishments existing throughout 1995–2008. The dependent variable is full-time equivalent employment of foreign workers (cross-border workers plus foreign resident immigrant workers) relative to total full-time equivalent employment in 1998. We estimate effects for all highly treated establishments (within 15 minutes of the border, termed "BR, 0–15 min")) and slightly treated establishments (within 15–30 minutes of the border, termed "BR, 15–30 min"). The control group is establishments located more than 30 minutes away from the border. The regression is weighted using establishments' average employment pre-1999 as weight. We control for establishment and period fixed effects. Confidence intervals are clustered on the level of commuting zones.

Figure 5: Timing of effect of free movement policy on apprenticeship training




*Notes:* The figure shows the effect of the free movement policy on the establishments' provision of apprenticeships using data from the Swiss business censuses 1995–2008. It plots the estimated policy effects and associated 95% confidence intervals using a generalization of our main DiD model (equation (1)) that estimates separate effects for each census year. We standardize the effects to 0 in 1998 by dropping the indicator for that year from the regression. The estimation sample comprises all establishments existing throughout 1995–2008. The dependent variable is the number of apprentices relative to total full-time equivalent employment in 1998. We estimate effects for highly treated establishments (within 15 minutes of the border, "BR, 0–15 min") and slightly treated establishments (within 15–30 minutes of the border, "BR, 15–30 min"). The control group is establishments located more than 30 minutes away from the border. The regression accounts for establishment and period fixed effects and is weighted using establishments' average employment pre-1999 as weight. Confidence intervals are clustered on the level of commuting zones.

Table 3: Effect of free movement policy on foreign employment and apprenticeships

| VARIABLES | (1) OLS | (2) FE | (3) FE | (4) FE | (5) FE | (6) FE |
|---|---|---|---|---|---|---|
| ***A. Foreign workers / total FTE employment in 1998*** | | | | | | |
| $I(t = 2001) \cdot I(d_i \leq 15)$ | -0.002 | 0.015** | 0.014* | 0.022*** | 0.021*** | 0.012 |
| | (0.008) | (0.006) | (0.008) | (0.006) | (0.007) | (0.007) |
| $I(t = 2001) \cdot I(15 < d_i \leq 30)$ | 0.007 | 0.014** | 0.015*** | 0.017* | 0.020*** | 0.012 |
| | (0.005) | (0.006) | (0.005) | (0.009) | (0.006) | (0.007) |
| $I(t \geq 2005) \cdot I(d_i \leq 15)$ | 0.031*** | 0.043*** | 0.041*** | 0.045*** | 0.046*** | 0.042*** |
| | (0.011) | (0.009) | (0.010) | (0.011) | (0.011) | (0.010) |
| $I(t \geq 2005) \cdot I(15 < d_i \leq 30)$ | 0.012* | 0.015** | 0.015** | 0.016* | 0.018** | 0.014* |
| | (0.007) | (0.007) | (0.006) | (0.010) | (0.009) | (0.007) |
| Observations | 1,442,654 | 904,900 | 904,900 | 345,080 | 625,140 | 770,375 |
| Mean dep. variable in 1998 | .153 | .149 | .149 | .158 | .174 | .153 |
| ***B. Apprentices / total FTE employment in 1998*** | | | | | | |
| $I(t = 2001) \cdot I(d_i \leq 15)$ | 0.003 | -0.001 | -0.000 | -0.000 | 0.000 | -0.001 |
| | (0.002) | (0.002) | (0.002) | (0.003) | (0.002) | (0.002) |
| $I(t = 2001) \cdot I(15 < d_i \leq 30)$ | 0.001 | -0.000 | -0.001 | -0.000 | 0.001 | -0.001 |
| | (0.001) | (0.002) | (0.002) | (0.002) | (0.001) | (0.002) |
| $I(t \geq 2005) \cdot I(d_i \leq 15)$ | -0.005* | -0.004 | -0.002 | -0.005 | -0.003 | -0.004 |
| | (0.002) | (0.003) | (0.002) | (0.004) | (0.003) | (0.003) |
| $I(t \geq 2005) \cdot I(15 < d_i \leq 30)$ | -0.000 | -0.000 | -0.000 | -0.000 | 0.001 | -0.001 |
| | (0.002) | (0.002) | (0.002) | (0.003) | (0.003) | (0.002) |
| Observations | 1,442,654 | 904,900 | 904,900 | 345,080 | 625,140 | 770,375 |
| Mean dep. variable in 1998 | .044 | .061 | .061 | .161 | .057 | .061 |
| ***C. Training provision (0/1)*** | | | | | | |
| $I(t = 2001) \cdot I(d_i \leq 15)$ | -0.000 | -0.006 | -0.006* | -0.006 | -0.006 | -0.006* |
| | (0.002) | (0.004) | (0.003) | (0.009) | (0.004) | (0.004) |
| $I(t = 2001) \cdot I(15 < d_i \leq 30)$ | 0.003 | 0.002 | 0.002 | 0.007 | 0.002 | 0.002 |
| | (0.002) | (0.002) | (0.002) | (0.006) | (0.003) | (0.002) |
| $I(t \geq 2005) \cdot I(d_i \leq 15)$ | -0.006 | -0.007 | -0.006 | -0.007 | -0.007 | -0.007 |
| | (0.005) | (0.006) | (0.005) | (0.014) | (0.006) | (0.006) |
| $I(t \geq 2005) \cdot I(15 < d_i \leq 30)$ | 0.003 | 0.004 | 0.004 | 0.014* | 0.004 | 0.004 |
| | (0.003) | (0.003) | (0.003) | (0.007) | (0.003) | (0.003) |
| Observations | 1,878,844 | 904,900 | 904,900 | 345,080 | 625,140 | 770,375 |
| Mean dep. variable in 1998 | .172 | .247 | .247 | .650 | .235 | .247 |
| Control group | Both | Both | Both | Both | BR 30+ | NBR |
| Balanced sample | No | Yes | Yes | Yes | Yes | Yes |
| Period FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Region FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Establishment FE | No | Yes | Yes | Yes | Yes | Yes |
| Apprentice supply control | No | No | Yes | No | No | No |
| Training establ. only | No | No | No | Yes | No | No |

*Notes:* This table shows the effect of the free movement policy on foreign employment and the provision of apprenticeships using our main DiD model (equation (1)). The regressions are based on the business censuses (BC) 1995–2008. The dependent variable in panel A is full-time equivalent (FTE) employment of foreign workers relative to total full-time equivalent employment in 1998. The dependent variable in panel B is the number of apprentices relative to total full-time equivalent employment in 1998. The dependent variable in panel C is a dummy variable equal to 1 if an establishment trains apprentices. The main coefficient of interest is the interaction between firms within 15 minutes commuting time $d_i$ to the border and the free movement period, $I(t \geq 2005) \cdot I(d_i \leq 15)$. The control group in columns 1–4 is establishments located in the border region with more than 30 minutes travel time to the border (BR 30+) and establishments in the non-border region (NBR). Results for each control group separately are provided in columns 5 and 6. The sample in column 1 is all establishments in the BC. The "balanced sample" used in the remaining columns comprises all establishments existing throughout 1995–2008. The sample in columns 4 is additionally restricted to establishments that train at least once throughout 1995–2008. Regressions in panels A and B are weighted using establishments' average employment 1995 and 1998 as weight. The "apprentice supply control" used in column 3 is the number of graduates from lower secondary schools (11th grade) in the commuting zone (NUTS-III region). Standard errors are clustered on the level of commuting zones. ***, **, *, denote statistical significance at the 1%, 5%, and 10% level, respectively.

employment. However, the policy effects are not statistically significant at conventional levels. Moreover, in contrast to the sharp increase in the employment of foreign workers in the same establishments, the negative estimate emerges gradually over an extended period, which makes it hard to attribute it to the free movement policy. The estimated effects are also close to zero and statistically insignificant for slightly treated establishments in all census years.

The DiD estimates that correspond to the event study, presented in panel B of Table 3, confirm that the free movement policy had limited effects on the employment of apprentices. The exception is column 1, where the coefficient for highly treated establishments, $I(t \geq 2005) \cdot I(d_i \leq 15)$, is statistically significantly negative at the 10 percent significance level. However, since we include all establishments that exist in 1998 and do not control for establishment fixed effects, these results could be biased downward because of firm entry.[18] All other specifications in the table produce estimates that are close to zero and statistically insignificant. This holds if we control for the (potential) supply of apprentices in the local labor market ($Supply_{j(i),t}$, column 3) and if we estimate the model for both control groups separately (columns 5 and 6).

To put the estimated impact of the free movement policy on the number of apprenticeships in perspective, we can compare it to the effect on the hiring of foreign workers. Panel (a) of Figure 6 provides this comparison for highly treated establishments. We observe that the effect on apprentices is an order of magnitude smaller than the effect on foreign employment. Taken at face value, the estimates suggest that each foreign worker hired by highly treated establishments is associated with a decrease of about 0.09 apprenticeship positions. Alternatively, the 0.0037 percentage point effect on highly treated establishments in our main specification implies that the free movement policy reduced the number of apprenticeship positions by 6% in highly treated establishments, or 1100–1200 positions in total.[19]

Next, we analyze whether the free movement policy affected firms' selection into apprenticeship training. Did the policy reduce the probability that incumbent establishments start offering apprenticeships or increase the likelihood that they stop training? To analyze

[18] Beerli et al. (2021) show that the policy led to the entry of firms close to the border. New firms are, on average, smaller than incumbent firms. Smaller firms have a lower probability to train apprentices than larger firms (Muehlemann and Wolter, 2007). The entry of new firms thus likely reduces the percentage of firms that train in the highly treated region. This effect might lead us to overstate a possible negative effect of the free movement policy on the training of apprentices.

[19] Aepli and Kuhn (2021) present similar back-of-the-envelope calculations. They estimate that the growth in cross-border employment between 1995 and 2008 led to about 3500 fewer apprenticeship positions. These estimates are not directly comparable to ours since we quantify the effect of the free movement policy while Aepli and Kuhn (2021) focus on the growth in cross-border workers.

this question, Table 3, panel C, re-estimates the DiD model using a dummy that establishments provide at least one training position as dependent variable.[20] The (unweighted) regressions do not provide evidence for an impact of the policy on the probability to offer apprenticeship training. Overall, Table 3 provides little evidence that the policy's impact differed on the extensive and intensive margin.

Do these aggregate estimates hide heterogeneity between industries? Figure 7 shows separate estimates of the event study model for broad industries. The dependent variables are the number foreign workers (panel a) and apprentices (panel b) relative to total employment in 1998. The figure, first, shows that highly treated and control establishments display parallel pre-trends in employment of foreign workers and apprentices in all sectors between 1995 and 1998. Second, manufacturers, and in particular high-tech manufacturers, hired a particularly large number of additional foreigners because of the reform. Third, there is no statistically significant evidence for a displacement of apprenticeships in four of the five sectors.

The exception is the construction sector, which accounts for the negative point estimate on apprenticeship provision in the aggregate regressions. In this sector, we find statistically significant evidence at the 5 percent level that the free movement policy reduced apprenticeship provision.

Interestingly, the corresponding event study in Figure 7, panel a, reveals no policy effect on employment of foreign workers. Why was there an effect on apprenticeship provision, then? An explanation is provided in Table 7: the reform induced treated construction firms to hire additional cross-border workers but these appear to have crowded out not just apprentices, but also regular resident immigrants, which explains the null effect on total foreign employment.

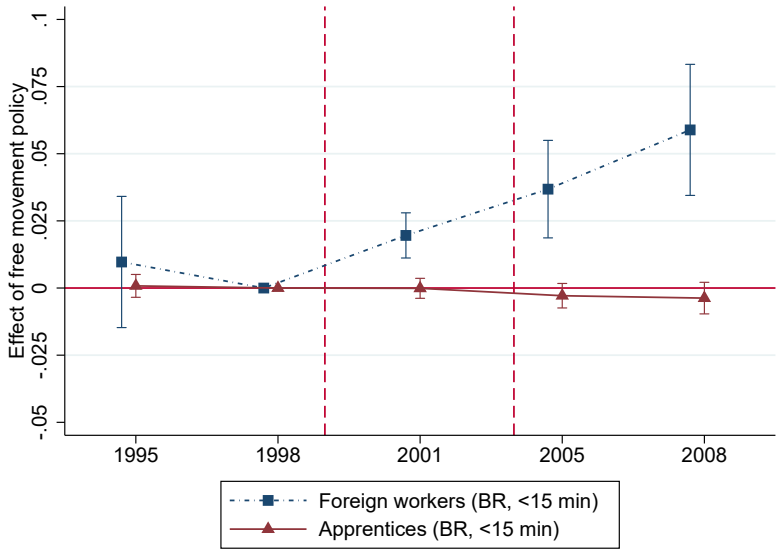
Overall, the census data suggest that the greater availability of cross-border workers did not reduce firms' willingness to provide apprenticeships. The exception seems to be the construction sector where cross-border workers may substitute for apprenticeship training. Analogous estimates of the policy's effect with the cost-benefit data also provide no evidence that the policy reduced firms' propensity to train apprentices on average (see Appendix Table B.7). We also find no evidence for negative effects on the apprentice share of highly treated firms if we differentiate the regressions by training occupation (Appendix Table B.8).[21]

[20] Indeed, the estimated effects on *slightly* treated establishments are *positive* in both periods and all specifications, and even statistically significantly so at the 10 percent level if we focus only on firms that train at least once (column 4).

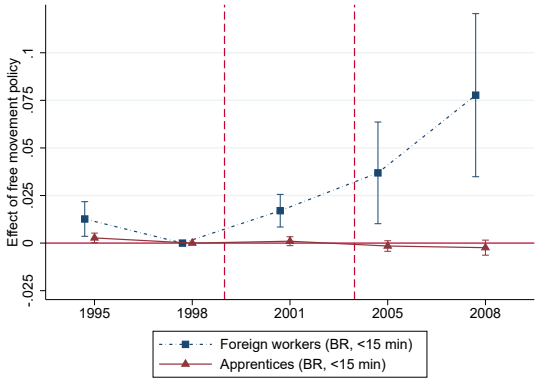
[21] There are two exceptions for slightly treated firms, where we observe a significant increase in the apprentice share for merchants in 2009 and a significant reduction in the apprentice share for polymechanics in 2004.

Figure 6: Effect of free movement policy on apprenticeship training: Comparison of effect size

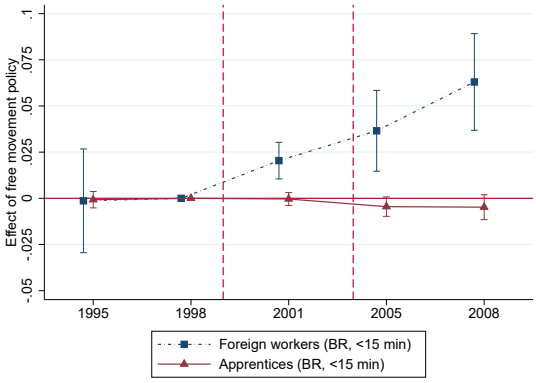
(a) Establishment-level estimates




(b) Municipality-level estimates



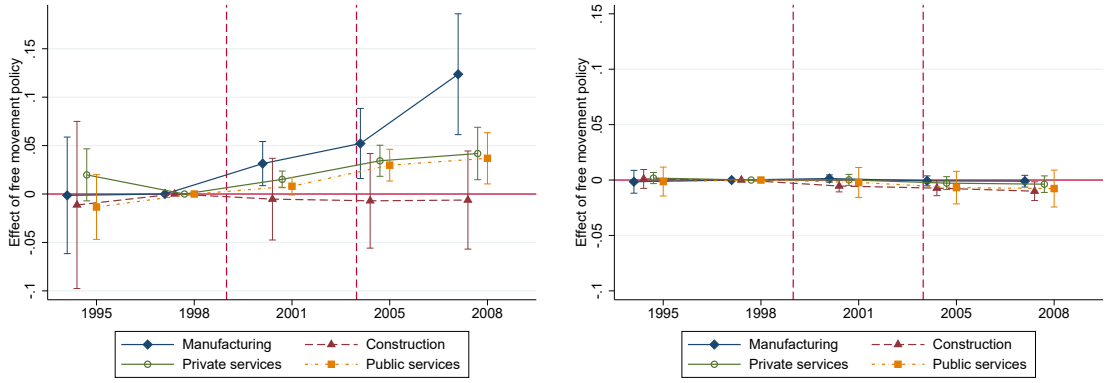

(c) Matched control group




*Notes:* The figure shows the effect of the free movement policy on employment of foreign workers and the provision of apprenticeships in highly treated units using data from the Swiss business censuses 1995–2008. It plots the estimated policy effects and associated 95% confidence intervals using a generalization of our main DiD model (equation (1)) that estimates separate effects for each census year. We focus on highly treated establishments (establishments within 15 minutes of the border) and standardize the effects to 0 in 1998 by dropping the indicator for that year from the regression. Panels (a) and (c) are estimated using establishment-level data. The dependent variables are (i) full-time equivalent employment of foreign workers relative to total full-time equivalent employment in 1998, and (ii) the number of apprentices relative to total employment in 1998. Panel (a) shows the results with our baseline approach based on a balanced panel of establishments existing throughout 1995–2008. Panel (b) shows analogous estimates if we use all establishments existing in a given year and aggregate the data on municipal level. Panel (c) shows our baseline establishment-level DiD regressions if we use a control group of non-treated establishments that we match to highly treated establishments using Mahalanobis (covariate) distance matching (see appendix A for details). The regressions are weighted using average employment pre-1999 as weight. In panel c, the weight is additionally multiplied with the number of times that a control establishment is matched to a highly treated establishment. All regressions account for period fixed effects and establishment (panels a and c) or region (panel b) fixed effects, respectively. Confidence intervals are clustered on the level of commuting zones.

Figure 7: Effect of free movement policy on employment of foreign workers and apprentices, by broad sector

(a) Foreign workers/employment in 1998 (b) Apprentices/employment in 1998




*Notes:* The figure shows the effect of the free movement policy on the establishments' employment of foreign workers (panel a) and apprentices (panel b), estimated by sector of activity. We use data from the Swiss business censuses 1995–2008. The figures plot the estimated policy effects and associated 95% confidence intervals using a generalization of our main DiD model (equation (1)) that estimates separate effects for each census year. We standardize the effects to 0 in 1998 by dropping the indicator for that year from the regression. The estimation sample comprises all establishments existing throughout 1995–2008. The dependent variables are the number foreign workers (panel a) and apprentices (panel b) relative to an establishment's total employment in 1998. We focus on highly treated establishments. The control group in all panels is establishments more than 30 minutes from the border. All regressions account for establishment and period fixed effects. The regressions are weighted using establishments' average employment pre-1999 as weight. Confidence intervals are clustered on the level of commuting zones.

## 5.4 Robustness

We now establish that these results are robust. Table 4 adds more demanding sets of fixed effects to our baseline model and presents the results of a few alternative specifications. The table shows that the estimates are comparable if we control for unobserved industry-specific shocks by including two-digit industry times year fixed effects (column 1)[22], and unobserved regional shocks by including NUTS-II region times year (column 2) and canton times year fixed effects (column 3). The latter model is identified only from comparing firms with different distances to the border located within the same of the 26 Swiss cantons.

The estimated effects are also similar if we restrict the sample to two-digit industries unaffected by either of the other bilateral agreements introduced along with the free movement policy (column 4)[23], if we use the somewhat smaller estimation sample and the

[22]The robustness to including industry-year and, in the case of the cost-benefit data, to including occupation-year fixed effects also limits concerns that our results are affected by a major revision of the federal act on vocational and professional education that took place in 2004. The reform concerned health, social, art, and agriculture and forestry occupations (BBT, 2003). The industry-year and occupation-year fixed effects thus likely absorb the possible reform effects on apprenticeship provision.

[23]The free movement policy was part of a package of bilateral agreements between the EU and negotiated at the same time as the agreement on the free movement of persons. One of these

weighting scheme of Beerli et al. (2021) (column 5), if we do not weight observations by establishment size (column 6), and if we do not winsorize the outcome variables (column 7).

Importantly, the main effect on the apprentice share (panel B) turns statistically significantly negative in columns 2 and 6. Yet, there are no *a priori* reasons to prefer those estimates over the others. Indeed, our baseline estimates (column 2 of Table 3) are close to the average of the estimates in Tables 3 and 4. Moreover, we find a positive, not negative, effect of the policy on employment of apprentices if we estimate our baseline regression with Poisson Pseudo Maximum Likelihood (PPML). PPML is often applied to analyze outcome variables such as employment of apprentices that take on a value of zero for many firms.[24]

All regressions presented so far only use firms that exist in 1998 because we normalize our main outcome variables with firm size in 1998. Our preferred specification is additionally restricted to firms existing throughout 1995–2008. We probe the relevance of this focus on surviving incumbents in Panel (b) of Figure 6 by aggregating the employment of *all* establishments in the census to the municipality level. We then estimate our DiD model using municipality-level data. These estimates incorporate the training behavior of entering and existing establishments in each municipality. Reassuringly, the resulting estimates confirm our baseline estimates. This holds for several other specifications using municipality level data, as shown in Appendix Table B.6. In particular, we find no evidence that the free movement policy affected the number and the share of establishments that train apprentices. As we show in Appendix Figure B.3, the results are also very similar if we aggregate the data to the level of commuting zones, limiting concerns that the main findings are driven by a subtle reallocation of apprentices between establishments or municipalities in the vicinity of the borders of our treatment and control regions.

Another potential concern with our results is that there are noteworthy differences in observed characteristics between treated and control firms prior to the free movement policy (see Table 1). These differences raise the possibility that unobserved shocks to these dimensions or dimensions correlated with them confound our estimates. We address this

agreements, for example, reduced non-tariff barriers to trade between Switzerland and the EU. This trade liberalization may have affected regions near the border more than the regions farther away. Beerli et al. (2021) thus use a proxy for exposure to these other agreements based on a classification by Buehler et al. (2011). Buehler et al. (2011) carefully assess the extent to which a specific two-digit industry was affected by these other agreements. Column 7 of Table 4 is restricted to non-affected two-digit industries.

[24]We present the average marginal effects from PPML estimations that use employment of foreigners and apprentices as dependent variables in Appendix Table B.5. Note that the baseline establishment fixed effects estimates in Table 3, panel B, are only identified from establishments that have a non-zero apprentice share at least once. In this group, only 35% of the firm-year observations are zero.

concern using a matching approach that makes treated and control establishments close to identical in several pre-reform characteristics, including those that are imbalanced in Table 1. We then run our DiD regressions using highly treated and control establishments that are similar *ex ante*, limiting concerns that unobserved shocks could affect treatment and control group differently *ex post*.

We use Mahalanobis covariate matching to match units (see appendix A for details). We match one control establishment to each highly treated establishment. We only match firms in the same two-digit industry and same firm size class that, in addition, agree in terms of indicators whether they trained apprentices pre-reform, employed foreign workers, and existed in 1991. Moreover, we make firms as similar as possible in terms of some other pre-reform characteristics, including indicators of public ownership and export and import status. Indeed, Appendix Table A.1 shows that the matched control group is, sometimes by construction, statistically indistinguishable from the treatment group along several important characteristics. We then run our baseline regression with this matched control sample by multiplying the employment weight by the number of times a control establishment is matched to a highly treated establishment. We loose 133 highly treated establishments without an exact match. Panel (c) of Figure 6 and Appendix Table A.2 present the results. We observe that the matching estimates confirm our baseline results in terms of sign and size of the effects.

Lastly, we assess the sensitivity of the main results to the distance thresholds that we use in our baseline specification. Figure 8 estimates the effects of the full liberalization of the Swiss labor market on foreign employment and apprenticeships using more fine-grained distance bins. The control group consists of firms in the non-border region and firms in the border region located at least 52.5 minutes away from the border. As expected, the increase in foreign employment is largest in establishments that are within 5 minutes of the border. The effect decreases close to linearly with increasing commuting distance to the border. It becomes close to zero and statistically insignificant in establishments located more than 30 minutes away from the border. Nevertheless, even in the two distance bins closest to the border where employment of foreigners grows most, we fail to find a statistically significant effect on apprenticeship training.

## 5.5 Mechanisms

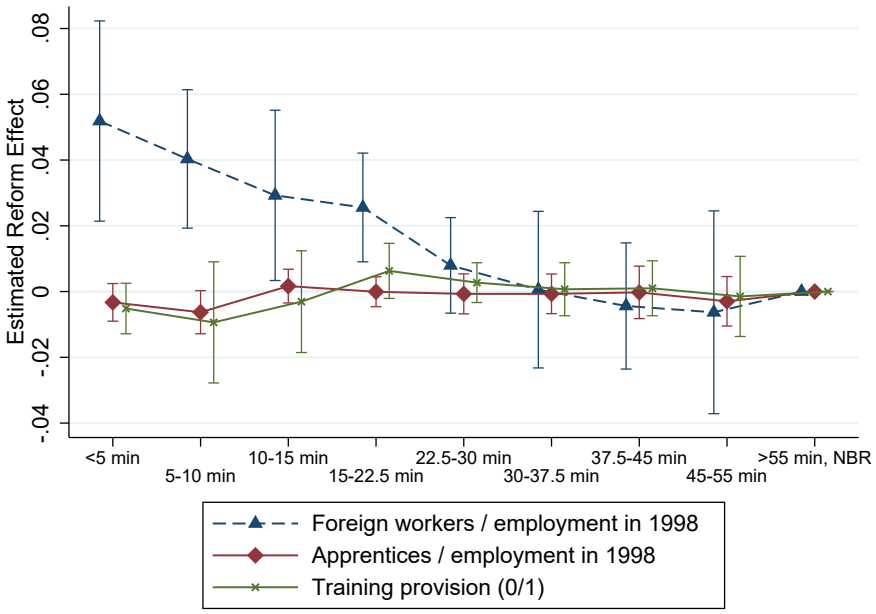
The previous sections suggest that a greater availability of foreign workers caused a strong growth in the number of skilled foreign workers but had no or at most a small negative impact on firms' provision of apprenticeship training. This section analyzes whether the two mechanisms highlighted in section 2—what we termed the scale and cost effects—

Table 4: Main robustness checks

| VARIABLES | (1) Industry-period FE | (2) NUTS-II-period FE | (3) Canton-period FE | (4) Not exposed to bilaterals | (5) BRSP sample | (6) No weights | (7) Including outliers |
|---|---|---|---|---|---|---|---|
| ***A. Foreign workers / total FTE employment in 1998*** | | | | | | | |
| $I(t=2001)\cdot I(d_i \leq 15)$ | 0.011* | 0.017** | 0.024*** | 0.021** | 0.032*** | 0.010*** | 0.016* |
| | (0.006) | (0.007) | (0.009) | (0.009) | (0.006) | (0.003) | (0.009) |
| $I(t=2001)\cdot I(15 < d_i \leq 30)$ | 0.014*** | 0.020*** | 0.016*** | 0.021* | 0.024*** | 0.006** | 0.017** |
| | (0.005) | (0.005) | (0.004) | (0.011) | (0.007) | (0.003) | (0.008) |
| $I(t\geq 2005)\cdot I(d_i \leq 15)$ | 0.035*** | 0.038*** | 0.033** | 0.039*** | 0.079*** | 0.045*** | 0.045*** |
| | (0.007) | (0.008) | (0.014) | (0.010) | (0.017) | (0.011) | (0.010) |
| $I(t\geq 2005)\cdot I(15 < d_i \leq 30)$ | 0.015*** | 0.023*** | 0.019*** | 0.012 | 0.032*** | 0.014*** | 0.018** |
| | (0.005) | (0.006) | (0.006) | (0.012) | (0.010) | (0.003) | (0.008) |
| ***B. Apprentices / total FTE employment in 1998*** | | | | | | | |
| $I(t=2001)\cdot I(d_i \leq 15)$ | -0.001 | -0.001 | -0.000 | 0.001 | -0.005*** | -0.002 | 0.000 |
| | (0.002) | (0.002) | (0.001) | (0.002) | (0.002) | (0.001) | (0.004) |
| $I(t=2001)\cdot I(15 < d_i \leq 30)$ | 0.000 | -0.001 | 0.000 | 0.000 | -0.000 | 0.000 | -0.002 |
| | (0.002) | (0.001) | (0.001) | (0.002) | (0.001) | (0.001) | (0.003) |
| $I(t\geq 2005)\cdot I(d_i \leq 15)$ | -0.003 | -0.005*** | -0.000 | -0.002 | -0.005 | -0.006** | -0.006 |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.003) | (0.003) | (0.004) |
| $I(t\geq 2005)\cdot I(15 < d_i \leq 30)$ | 0.002 | -0.001 | -0.000 | -0.000 | 0.001 | 0.000 | -0.002 |
| | (0.002) | (0.001) | (0.002) | (0.003) | (0.002) | (0.002) | (0.003) |
| ***C. Training provision (0/1)*** | | | | | | | |
| $I(t=2001)\cdot I(d_i \leq 15)$ | -0.006 | -0.003 | -0.000 | -0.004 | -0.002 | | |
| | (0.004) | (0.003) | (0.003) | (0.004) | (0.018) | | |
| $I(t=2001)\cdot I(15 < d_i \leq 30)$ | 0.002 | 0.002 | 0.003 | 0.003 | 0.015 | | |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.010) | | |
| $I(t\geq 2005)\cdot I(d_i \leq 15)$ | -0.005 | -0.003 | 0.003 | 0.001 | 0.004 | | |
| | (0.006) | (0.004) | (0.003) | (0.005) | (0.021) | | |
| $I(t\geq 2005)\cdot I(15 < d_i \leq 30)$ | 0.005* | 0.003 | 0.003 | 0.007** | 0.022** | | |
| | (0.003) | (0.002) | (0.002) | (0.003) | (0.009) | | |
| Observations | 904,896 | 904,495 | 904,900 | 582,068 | 441,025 | 904,900 | 904,900 |
| Control group | Both | Both | Both | Both | Both | Both | Both |
| Balanced sample | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Establishment FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Period FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* This table assesses the robustness of the effect of the free movement policy on foreign employment and apprenticeship training using our main DiD model (equation (1)). The regressions are based on data from the business censuses 1995–2008. The estimation sample comprises all establishments existing throughout 1995–2008. The main coefficient of interest is the interaction between firms within 15 minutes commuting time $d_i$ to the border and the free movement period, $I(t \geq 2005) \cdot I(d_i \leq 15)$. The dependent variable in panel A is full-time equivalent (FTE) employment of foreign workers relative to total full-time equivalent employment in 1998. The dependent variable in panel B is the number of apprentices relative to total full-time equivalent employment in 1998. The dependent variable in panel C is a dummy variable equal to 1 if an establishment trains apprentices in a given census year. Regressions in panels A and B (except those in column 6) are weighted using establishments' average employment pre-1999 as weight. In columns 1–3, we control for (NACE rev. 1.1 two-digit) industry-period fixed effects (FE), NUTS-II-period FE, and canton-period FE, respectively. The regressions in column 4 are restricted to two-digit industries that are unaffected by the other bilateral agreements according to the classification by Buehler et al. (2011). Column 5 presents the results using the sample of establishments used in the analyses of Beerli et al. (2021) (BRSP). Column 6 does not weight observations in panels A and B by establishment size. Column 7 does not winsorize the outcomes in panels A and B at 99.99%. Standard errors are clustered by commuting zone. ***, **, *, denote statistical significance at the 1%, 5%, and 10% level, respectively.

Figure 8: Effect of free movement policy on apprenticeship training, by detailed duration to border




*Notes:* The figure shows the effect of the full liberalization of the Swiss labor market on establishments' employment of foreign workers and apprentices using data from the Swiss business censuses 1995–2008. It plots the estimated policy effects and associated 95% confidence intervals using a generalization of our main DiD model (equation (1)) that estimates separate effects for detailed distance groups. The control group consists of firms in the non-border region and firms located in the border region but at least 52.5 minutes away from the border. The estimation sample is all establishments existing throughout 1995–2008. The dependent variables are full-time equivalent employment of foreign workers (cross-border workers plus foreign resident immigrant workers) and employment of apprentices, both expressed relative to total full-time equivalent employment in 1998, and an indicator whether a firm trains at least one apprentice. The regressions with the first two outcomes are weighted using establishments' average employment pre-1999 as weight. The third regression (training provision) is unweighted. We control for establishment and period fixed effects. Confidence intervals are clustered on the level of commuting zones.

contribute to explain this finding. While other mechanisms such as effects on local consumption may play a role in explaining the empirical estimates, too, we show that the two mechanisms had an important and opposing influence on firms' propensity to train in our context.

### 5.5.1 The relevance of the cost effect

We first analyze the relevance of the cost effect. It reflects the notion that a larger supply of skilled foreign workers reduced firms' problems to find suitable skilled workers and hence their recruitment costs. By lowering the financial consequences of hiring trained workers externally, a larger supply of skilled workers reduces the future savings in terms of hiring costs associated with training. We study the importance of this mechanism by exploiting the unique quantitative and qualitative questions on firms' recruitment costs

and training motives in the cost-benefit surveys. Appendix Table B.3 provides the list of outcome variables and the corresponding survey questions that we study in this section.

We start by analyzing whether we find evidence that the policy affected the local supply of skilled workers in the cost-benefit data—a perquisite to study the cost effect with the data. The first columns of Table 5 present a series of DiD regression models that speak to this question. As in all regressions with the surveys, we cannot control for firm fixed effects because it is not a panel dataset. Instead, we make firms as comparable as possibly by flexibly controlling for firm size fixed effects[25], industry times year fixed effects (19 industries[26]), and occupation times year fixed effects. The occupation refers to the occupation for which the company completed the questionnaire (see section 4.1). As before, we cluster standard errors at the level of commuting zones.

Finally, we weight observations using the surveys' sampling weights. However, we topcode the weights at 100 because the original weights contain some large weights that give certain firms a 3000 times higher weight than other firms. As expected, this makes the results somewhat more robust and precise.[27]

Columns 1–3 of Table 5 suggest that the policy affected the supply of skilled workers in labor markets close to the border. The outcome variables in these columns are one if the firm reported staffing problems. Since the outcomes are binary, we present average marginal effects derived from probit regressions. The first column suggests that the policy reduced the chance that highly treated firms think that the local supply of workers is insufficiently qualified by 4 percentage points, or 18% relative to the sample mean of the outcome in 2000.

Consistently, highly treated firms were 6.4 percentage points, or 14 percent, less likely to report skill shortages in the 2004 survey (column 2). We find no statistically significant evidence that policy reduced the probability to report problems with work permits for foreigners although the point estimates have the expected sign and are economically sizeable (column 3). In sum, the free movement policy appears to have reduced problems to find suitable skilled workers on the external labor market.

Against this background, we now analyze whether it became cheaper to hire skilled

[25] We add separate dummies for four broad firm size categories: 0–9 workers, 10–49 workers, 50–99 workers, and 100+ workers.

[26] The 19 industries are construction, food product and beverage manufacturing, textile and apparel manufacturing, wood and paper product manufacturing, chemical manufacturing, metal products manufacturing, machinery and equipment manufacturing, electrical equipment manufacturing, other manufacturing, trade and repair, food and beverage service activities, transport and telecommunication, financial services and insurance, real estate, IT, education, human health and social work, public administration, and other services.

[27] Table B.10 and Table B.12 show that the results are similar if we use the original weights for both tables that are based on the cost-benefit data.

workers either because of decreased direct cost for recruiting them or because decreased costs associated with training them during the adaptation period. In Table 6, we analyze the number of job applicants that the firm typically interviews if it hires new skilled workers (column 1); the time it typically takes to conduct these interviews (column 2); the costs to advertise a typical position (column 3); the costs of external consultants that help fill the position (column 4); the time it takes to train new hires until they reach full productivity (column 5); the extent to which the performance of the new hires is reduced relative to a fully productive worker during the adaptation period (column 6); and the money typically spend on further training courses (column 7). The companies were instructed to provide these numbers for the focus occupation for which they also reported their training behavior.

We estimate the policy effect on these costs for all firms (panel A of Table 6) and for firms that hired at least one skilled workers (panel B). The reason is that firms only report the costs if they hired at least one skilled worker in the three years before the survey. However, the hiring of skilled workers may be an endogenous response to the free movement policy. In panel C, we also present results for the subset of firms that train apprentices, independent of whether they hired a skilled worker or not. In all panels, the outcome variables are set to zero for firms that spend no money on a certain cost components or that did not hire a skilled worker recently. To retain the zeros in the estimation, we focus on outcomes specified in levels or, in case of outlier-prone outcomes such as the costs denoted in Swiss francs, we apply the inverse hyperbolic sine transformation.

Panel A of Table 6 suggests that the policy reduced firms' costs to hire skilled workers. Column 4 suggests that the policy reduced the fee that highly treated firms have to pay to external consultants by more than 30%. Similarly, it reduced the adaptation period for skilled workers by approximately 0.3 months, or by 12% relative to the mean of 2.55 months of the outcome in 2000 (column 5). During this adaptation period, the hired workers were also approximately 2 percentage points (or 11 percent) less inferior than the typical, fully productive, skilled worker (column 6). The results suggest that companies found candidates who better fit their firm-specific needs.

Table 5: Effect of free movement policy on firms' perceived personnel problems and training motives

| | (1) Personnel problem Insufficient qualifications | (2) Personnel problem Skills shortage | (3) Personnel problem Work permits (foreigners) | (4) Training motive Attract skilled workers | (5) Training motive Hiring costs | (6) Training motive Avoid adjustment costs |
|---|---|---|---|---|---|---|
| $I(d_i \leq 15)$ | 0.053*** | 0.038 | 0.021 | 0.035 | 0.158*** | 0.100 |
| | (0.018) | (0.025) | (0.018) | (0.098) | (0.060) | (0.077) |
| $I(d_i \leq 15) \cdot I(t = 2004)$ | -0.044* | -0.064** | -0.022 | 0.050 | -0.009 | 0.054 |
| | (0.023) | (0.026) | (0.027) | (0.103) | (0.081) | (0.087) |
| $I(d_i \leq 15) \cdot I(t = 2009)$ | -0.041*** | -0.031 | -0.018 | -0.096 | -0.116* | -0.078 |
| | (0.011) | (0.025) | (0.012) | (0.090) | (0.060) | (0.073) |
| $I(15 < d_i < 30)$ | 0.036** | 0.015 | 0.001 | 0.088 | 0.111* | 0.108 |
| | (0.017) | (0.023) | (0.016) | (0.087) | (0.060) | (0.072) |
| $I(15 < d_i \leq 30) \cdot I(t = 2004)$ | -0.002 | -0.042 | -0.006 | -0.151** | 0.030 | 0.033 |
| | (0.015) | (0.032) | (0.022) | (0.071) | (0.065) | (0.086) |
| $I(15 < d_i \leq 30) \cdot I(t = 2009)$ | -0.036 | -0.035 | -0.005 | -0.083 | -0.075 | -0.057 |
| | (0.024) | (0.026) | (0.014) | (0.081) | (0.062) | (0.063) |
| Observations | 15233 | 15237 | 14297 | 15544 | 15544 | 15544 |
| Estimation method | Probit | Probit | Probit | Ordered probit | Ordered probit | Ordered probit |
| Industry x Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Occupation x Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm size FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Apprentice supply control | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table analyzes how the free movement policy affected perceived personnel problems and selected training motives of firms in the cost-benefit data. All DiD estimations are based on probit (columns 1–3) or ordered probit (columns 4–6) regressions using the cost-benefit surveys in 2000, 2004 and 2009. The probit regressions report average marginal effects. The dependent variables in columns 1–3 are dummy variables equal to one if a firm perceives a certain personnel problem at the time of the survey. The dependent variables in columns 4–6 are qualitative survey questions on the importance of certain training motives (5-point Likert scale, where 1 is not important and 5 is very important). Appendix Table B.3 provides a detailed explanation of these outcome variables and the corresponding survey questions. The treated groups are the firms located up to 15 minutes from the closest border within the border region ($I(d_i \leq 15)$) and the firms located 15 minutes to up to 30 minutes from the closest border within the border region ($I(15 < d_i \leq 30)$). The control group consists of firms located more than 30 minutes away from the closest border within the border region and outside of it. All regressions control for firm size fixed effects (4 groups), industry times year fixed effects, and occupation times year fixed effects (see text for details). The "apprentice supply control" is the number of graduates from lower secondary schools (11th grade) in the commuting zone (NUTS-III region). The estimations are weighted using the surveys' sampling weights capped at 100. Standard errors are clustered on the level of commuting zones. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table 6: Effect of free movement policy on recruitment and training cost

| | (1) Interviews per job | (2) Interview time in hours | (3) Advertisement costs per job | (4) Costs of external consultant | (5) Adaptation period in months | (6) Shortfall in productivity | (7) Direct training costs |
|---|---|---|---|---|---|---|---|
| | | | ***A. All firms*** | | | | |
| $I(d_i \leq 15) \cdot I(t = 2004)$ | 0.034 | 0.587 | 0.064 | -0.171 | -0.198 | -0.018 | -0.005 |
| | (0.072) | (0.459) | (0.244) | (0.175) | (0.201) | (0.011) | (0.250) |
| $I(d_i \leq 15) \cdot I(t = 2009)$ | -0.059 | -0.120 | -0.217 | -0.320** | -0.335** | -0.026* | -0.012 |
| | (0.069) | (0.376) | (0.238) | (0.137) | (0.131) | (0.012) | (0.118) |
| $I(15 < d_i \leq 30) \cdot I(t = 2004)$ | -0.041 | -0.244 | -0.148 | -0.182 | -0.315* | -0.017* | 0.137 |
| | (0.084) | (0.402) | (0.327) | (0.128) | (0.171) | (0.009) | (0.133) |
| $I(15 < d_i \leq 30) \cdot I(t = 2009)$ | -0.043 | -0.591* | -0.220 | -0.189* | -0.273** | -0.024*** | 0.118 |
| | (0.055) | (0.320) | (0.170) | (0.102) | (0.110) | (0.006) | (0.156) |
| Observations | 15544 | 15544 | 15544 | 15544 | 15544 | 15544 | 15544 |
| | | | ***B. Hired skilled worker*** | | | | |
| $I(d_i \leq 15) \cdot I(t = 2004)$ | 0.124*** | 1.217 | 0.317 | -0.238 | -0.171 | -0.026* | 0.140 |
| | (0.042) | (0.751) | (0.200) | (0.308) | (0.244) | (0.014) | (0.436) |
| $I(d_i \leq 15) \cdot I(t = 2009)$ | 0.127** | 0.302 | 0.112 | -0.564** | -0.422 | -0.027* | 0.244 |
| | (0.051) | (0.658) | (0.341) | (0.243) | (0.255) | (0.014) | (0.215) |
| $I(15 < d_i \leq 30) \cdot I(t = 2004)$ | 0.093* | 0.096 | 0.166 | -0.175 | -0.235 | -0.010 | 0.497* |
| | (0.044) | (0.585) | (0.329) | (0.231) | (0.322) | (0.013) | (0.243) |
| $I(15 < d_i \leq 30) \cdot I(t = 2009)$ | 0.109** | -0.756 | 0.069 | -0.175 | -0.214 | -0.021** | 0.468 |
| | (0.049) | (0.567) | (0.225) | (0.216) | (0.226) | (0.010) | (0.323) |
| Observations | 7368 | 7368 | 7368 | 7368 | 7368 | 7368 | 7368 |
| | | | ***C. Training firms*** | | | | |
| $I(d_i \leq 15) \cdot I(t = 2004)$ | 0.146 | 1.848 | 0.546 | -0.342 | 0.061 | 0.021 | 0.112 |
| | (0.213) | (1.073) | (0.673) | (0.318) | (0.302) | (0.026) | (0.394) |
| $I(d_i \leq 15) \cdot I(t = 2009)$ | 0.158 | 0.205 | -0.056 | -0.799** | -0.363 | -0.003 | -0.123 |
| | (0.193) | (0.760) | (0.763) | (0.309) | (0.274) | (0.029) | (0.325) |
| $I(15 < d_i \leq 30) \cdot I(t = 2004)$ | 0.107 | -0.377 | 0.448 | 0.130 | 0.304 | 0.005 | 0.454 |
| | (0.128) | (0.501) | (0.670) | (0.178) | (0.196) | (0.008) | (0.388) |
| $I(15 < d_i \leq 30) \cdot I(t = 2009)$ | 0.114 | -1.248** | 0.210 | 0.027 | 0.122 | 0.006 | 0.387 |
| | (0.111) | (0.543) | (0.576) | (0.189) | (0.309) | (0.021) | (0.308) |
| Observations | 4553 | 4553 | 4553 | 4553 | 4553 | 4553 | 4553 |
| Distance FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry x Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Occupation x Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm size FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Apprentice supply control | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table analyzes how the free movement policy affected firms' recruitment and training costs. All DiD estimations are based on OLS regressions using the cost-benefit surveys in 2000, 2004 and 2009. The dependent variables are the following: the inverse hyperbolic sine (IHS) of the number of job applicants that the firm typically interviews if it hires new skilled workers (column 1), the time it typically takes to conduct these interviews (in hours, column 2), the IHS of the average job advertising costs (in Swiss francs, column 3) and for external consultants per successful new hire (in Swiss francs, column 4), the time it takes to train a newly hired skilled worker (in months, column 5), the extent to which the performance of the newly hired skilled workers is reduced relative to a fully productive worker during the adaptation period (ranging from 0 = 0% to 1 = 100% reduction, column 6), and the IHS of the average costs of direct training courses for newly hired skilled workers (in Swiss francs, column 7). See Appendix Table B.3 for details. These outcomes are set zero for firms that did not hire a skilled worker in the three years before the survey or if the firm did not incur the costs. Panel A focuses on all firms. Panel B focuses only on firms that hired a skilled worker. Panel C focuses on firm that train at least one apprentice. All regressions control for the baseline distance effects ($I(d_i \leq 15)$ and $I(15 < d_i \leq 30)$), firm size fixed effects (4 groups), industry times year fixed effects, and occupation times year fixed effects. The "apprentice supply control" is the number of graduates from lower secondary schools (11th grade) in the commuting zone (NUTS-III region). The estimations are weighted using the surveys' sampling weights capped at 100. Standard errors are clustered on the level of commuting zones. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

These findings are robust. They hold for both control groups, if we weight the regressions differently, or if we remove certain occupations only covered in certain survey years (see Appendix Tables B.12 and B.13). They also hold, qualitatively, for firms that hired at least one skilled worker (panel B of Table 6) or for training firms (panel C). The exception is column 1 of panel B. Firms that hired skilled workers interviewed around 12% more job applicants per job opening because of the open borders. One explanation for this result is that companies received more and better applications to their job postings, which may have prompted them to conduct additional job interviews.

Together, the results suggest that the policy meaningfully reduced firms' recruitment and training costs. Our theoretical considerations suggest that the lower costs to hire skilled workers may, in turn, have lowered firms' incentive to train workers (as in, e.g., Blatter et al., 2012, 2016; Wolter et al., 2006). Consistently, in the cost benefit surveys, many firms report that they train to "attract skilled workers because it is hard to find qualified workers on the external labor market" and to "save on the costs of hiring skilled workers on the external labor market" (see Table 1).

Did the free movement policy influence the likelihood that firms train because of these motives, as we may expect given the policy's effects on the costs to hire skilled workers? Columns 4–6 in Table 5 formally analyze whether the greater availability of foreign workers affected firms' training motives. We estimate equation (1) using ordered probit models because the motives were levied on a 5-point Likert scale. The regressions provide suggestive evidence that the free movement policy reduced highly treated firms' likelihood to train apprentices to save hiring costs for external workers (column 5). The estimate is statistically significant at the ten percent level. We also find some evidence that weakly treated firms became less likely to train to attract skilled workers because it is hard to find qualified workers externally (column 4). We find no statistically significant effect of the policy on the motive to save adjustment costs (column 6).[28]

Overall, these results support the notion that the free movement policy caused a cost effect that reduced treated firms' incentives to train apprentices. While many estimates are not very precise due to measurement error and relatively small sample sizes, the results suggest that the opening of the border may have had detrimental effects on firms' provision of apprenticeships through the cost effect.

Did a scale effect contribute to offset this negative pressure, as hypothesized in section 2? This question is analyzed next.

[28]The policies' effects on the six other training motives covered in the survey are presented in Appendix Table B.11.

### 5.5.2 The relevance of the scale effect

The scale effect may arise if foreign workers are imperfect substitutes of unskilled apprentices—if firms train because apprentices are unskilled productive workers—or of skilled apprenticeship graduates—if training is an investment for firms to secure skilled workers in the future. A key result of Beerli et al. (2021) in this regard is that the free movement policy neither lowered the employment nor the wages of the average Swiss native workers. On the contrary, the policy led to increased wages for highly educated native workers. More generally, the findings of Beerli et al. (2021) suggest that cross-border workers and skilled natives are imperfect substitutes. The scale effect could thus be important in our context.

A key sign of the scale effect is that heavily exposed establishments would create additional positions and should thus grow more than comparable non-treated establishments. We explore this prediction in Table 7, extending similar analyses by Beerli et al. (2021) to the context and the sample relevant in this paper.

The table contains four panels. The first two panels analyze the effects on employment of all foreign workers (cross-border workers plus regular immigrants, panel A) and for cross-border workers only (panel B). Information on the employment of cross-border workers is only available in the censuses of 1995, 2005, and 2008. The DiD estimates thus reflect the regional differences in the growth of cross-border employment in the 2005–2008 period relative to 1995. Panel C studies the effects on the number of apprentices. Finally, panel D provides estimates of the effect of the free movement policy on establishment size—the scale effect. We present these estimates separately by broad economic sector since we expect due to Figure 7 that the scale effect may be most prevalent in the manufacturing and private services sectors and more muted in the construction and public services sectors.

The table confirms this prediction. We find statistically significant evidence of a quantitatively meaningful scale effect of the free movement policy in manufacturing (column 1, panel D). The policy effect on establishment size is also economically sizable but marginally insignificant in the private service sector (column 3, panel D). The impacts are smaller and statistically insignificant in the two other sectors. The point estimate is negative in the construction sector, and the confidence intervals rule out large positive effects.

Does the scale effect result from imperfect substitution of cross-border workers and apprentices, or imperfect substitution of cross-border workers and apprenticeship graduates? To explore this question, Appendix Table B.9 uses the cost-benefit data to estimate the effect of the free movement policy separately for firms that incur net costs and net benefits of apprenticeship training. The aim is to differentiate the effect of the free movement policy between firms that may train according to the investment and production motive. The point estimates suggest that the free movement policy increased employment

of apprentices in firms following the investment approach. Some of the point estimate, particularly for slightly treated firms, are statistically significantly positive. In contrast, the point estimates are negative for highly treated firms following the production approach. Viewed through the lens of our conceptual framework, these results suggest that cross-border workers might be substitutes of apprentices but are complements of apprenticeship graduates.

Overall, we find evidence that an effect of immigration on firm growth may have contributed to offset the negative pressure on apprenticeship provision in the manufacturing and, possibly, the private service sector. There is evidence for a quantitatively meaningful displacement of apprenticeships in the construction sector where we find no evidence for a scale effect.

# 6 Conclusion

This study analyzes the effects of opening the Swiss labor market to cross-border workers on the employment of (skilled) foreign workers, the firms' provision of apprenticeships to unskilled native workers, firms' recruitment costs, and the training motives of firms. We exploit the step-wise implementation of the agreement on the free movement of persons, which affected firms near the Swiss border more than firms farther away.

We show that the free movement policy increased the number of foreign workers in the firms situated near the border that provide apprenticeships. This increase in foreign employment did not displace apprentices on aggregate. While the point estimate of our preferred specification suggests that ten additional cross-border workers replace 0.9 apprentice positions, the standard errors do not rule out a zero effect. Using unique data on the costs and benefits of training, we show that the policy reduced treated firms' costs to recruit skilled workers because it made it easier to find skilled workers that fit the firm-specific requirements. It also reduced firms' incentives to train apprentices to save on hiring costs for external skilled workers. A scale effect contributed to counteract the lower incentive to train apprentices: the free movement policy had a positive impact on establishment size in manufacturing and possibly in the private service sector, which allowed firms to hire more workers overall. We find evidence for a displacement of apprenticeship positions in the construction sector where we do not find evidence for a scale effect.

Our findings might suffer from two limitations. First, our estimates only capture general equilibrium effects if they affect firms close to the border more than firms farther away. However, the far-reaching reform might have affected all firms in Switzerland to some extent. Such effects are absorbed in our specification. Second, our data is limited to the period after 1995. Hence, we cannot show pre-trends for an extended pre-treatment

Table 7: Heterogeneity by broad economic sector

| VARIABLES | (1) FE Manufac-turing | (2) FE Construction sector | (3) FE Private services | (4) FE Public services |
|---|---|---|---|---|
| ***A. Foreign workers / total FTE employment in 1998*** | | | | |
| $I(t = 2001) \cdot I(d_i \leq 15)$ | 0.032* | 0.000 | 0.000 | 0.015 |
| | (0.017) | (0.018) | (0.013) | (0.010) |
| $I(t \geq 2005) \cdot I(d_i \leq 15)$ | 0.088*** | -0.001 | 0.023*** | 0.040*** |
| | (0.028) | (0.021) | (0.008) | (0.011) |
| ***B. Cross-border workers / total employment in 1995*** | | | | |
| $I(t \geq 2005) \cdot I(d_i \leq 15)$ | 0.100*** | 0.038* | 0.055*** | 0.089** |
| | (0.022) | (0.019) | (0.013) | (0.040) |
| ***C. Apprentices / total FTE employment in 1998*** | | | | |
| $I(t = 2001) \cdot I(d_i \leq 15)$ | 0.002 | -0.006** | -0.000 | -0.001 |
| | (0.003) | (0.003) | (0.002) | (0.006) |
| $I(t \geq 2005) \cdot I(d_i \leq 15)$ | -0.000 | -0.009** | -0.002 | -0.007 |
| | (0.004) | (0.004) | (0.002) | (0.006) |
| ***D. Log FTE employment*** | | | | |
| $I(t = 2001) \cdot I(d_i \leq 15)$ | 0.051* | 0.001 | 0.023 | 0.002 |
| | (0.028) | (0.025) | (0.016) | (0.012) |
| $I(t \geq 2005) \cdot I(d_i \leq 15)$ | 0.082** | -0.043 | 0.042 | 0.017 |
| | (0.038) | (0.026) | (0.029) | (0.015) |
| Control group | Both | Both | Both | Both |
| Balanced sample | Yes | Yes | Yes | Yes |
| Establishment FE | Yes | Yes | Yes | Yes |
| Period FE | Yes | Yes | Yes | Yes |

*Notes:* This table shows the effect of the free movement policy on foreign employment, employment of cross-border workers, apprenticeships, and establishment size of highly treated establishments using our main DiD model (equation (1)). The regressions are based on data from the business censuses 1995–2008 and estimated separately by sector. The estimation sample comprises all establishments existing throughout 1995–2008. The main coefficient of interest is the interaction between firms within 15 minutes commuting time $d_i$ to the border and the free movement period, $I(t \geq 2004) * I(d_i \leq 15)$. The effects on slightly treated firms ($I(15 < d_i \leq 30)$) are estimated but omitted here for brevity. The dependent variable in panel A is full-time equivalent (FTE) employment of foreign workers relative to total full-time equivalent employment in 1998. The dependent variable in panel B is the number of cross-border workers relative to total employment in 1998. This information is only available in the censuses 1995, 2005, and 2008, which explains why we do not estimate the effects in the transition phase ($t$ =2001). The dependent variable in panel C is the number of apprentices relative to total full-time equivalent employment in 1998. The dependent variable in panel D is establishments' log full-time equivalent employment. Regressions are weighted using establishments' average employment pre-1999 as weight. Standard errors are clustered by commuting zone. ***, **, *, denote statistical significance at the 1%, 5%, and 10% level, respectively.

period. While the number of foreigners employed at highly treated firms exhibits a clear break when the policy takes place, the evidence is less clear for the number of apprentices. Here, we observe a gradual excess decline in highly treated firms. The gradual trend could be caused by the policy but also by other factors, which cautions us from drawing firm conclusions from the negative point estimate on the training of apprentices.

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# Appendix

## A Results with the matched control group

This section presents estimates of the effect of the free movement policy on foreign employment and the provision of apprenticeships if we use control establishments matched to highly treated units based on Mahalanobis distance matching. Since the firms that we compare are thus similar along a set of observed pre-treatment variables such as industry affiliation and international exposure, these results are more robust to unobserved shocks that affect firms along the matched attributes or attributes correlated with the matched attributes.

To construct the matched sample, we only focus on the balanced sample of establishments existing in all years 1995–2008. We then match control establishments—establishments located more than 30 minutes away from the border in either region—to highly treated establishments—establishments located within 15 minutes to the border in the border region—using Mahalanobis distance matching. We only consider control units that completely agree with highly treated units in terms of indicators whether a firm trained apprentices, employed foreign workers, of two-digit industry affiliation (NACE rev. 1.1), of establishment size (in 4 groups), all measured in 1998, and whether a firm existed in 1991. These restrictions drop 133 highly treated establishments without corresponding control unit. We additionally match on the following pre-treatment covariates: (continuous) establishment size (in full-time equivalents) in 1998, the foreign employment share in 1995, and indicators whether the establishment is publicly owned, a single firm, a subsidiary, a headquarter (all in 1998), an exporter, and an importer (both in 1995). The same establishment can serve as control for several highly treated units. We randomly select one control establishment if there are several control units that have the same Mahalanobis distance score.

Table A.1 table provides descriptive statistics of certain pre-treatment characteristics of the highly treated establishments and the matched control group. The characteristics are measured in 1998 unless otherwise noted. The number of observations differs between the two groups because the average control establishment is matched to 2.5 treated units. However, we account for the unequal number of observations when calculating means and standard deviations in the other rows of columns 3–4 by weighting them by the number of times that an untreated establishment is matched to a treated establishment. The table shows that the matched control group is, sometimes by construction, statistically indistinguishable from the treatment group along some important pre-treatment characteristics, including the long-run growth in FTE employment between the business censuses 1991 and

1998. As expected, firms in the high-treatment group have a higher share of cross-border workers. As a consequence, they also have a somewhat higher share of foreigners in total employment. But since we match on these firm characteristics, all observed differences are considerable smaller than in our baseline sample (see Table 1).

Table A.2 shows the results if we use the matched control group to estimate our main DiD model.[29] The regressions are weighted by number of times that an untreated establishment is matched to a treated establishment. We additionally multiply this weight by establishments' average employment in the estimation period, topcoded at 500 FTE workers per establishment (as in our baseline regressions). Control establishments that are never matched are dropped. Reassuringly, the table shows that the effect estimates that we get are close to our baseline estimates in column 2 of Table 3, independent of whether we control for the potential supply of apprentices in the regional labor market (column 2 and 4) or not (columns 1 and 3).

[29]The corresponding event study is panel (b) of Figure 6 in the main text.

Table A.1: Pre-treatment characteristics of highly treated establishments and matched control group

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | **Highly treated** | | **Matched control** | | Difference in means | |
| Variable | Mean | SD | Mean | SD | Difference | SE |
| FTE employment | 11.286 | (56.671) | 10.453 | (43.263) | 0.833** | (0.413) |
| Existed in 1991 | 0.796 | (0.403) | 0.796 | (0.403) | 0.000 | (0.003) |
| Firm growth 1991–1998 in % | 11.684 | (69.511) | 11.137 | (69.378) | 0.547 | (0.635) |
| Employs foreigners (0/1) | 0.477 | (0.499) | 0.477 | (0.499) | -0.000 | (0.004) |
| Share of foreign workers | 0.230 | (0.315) | 0.201 | (0.291) | 0.029*** | (0.002) |
| Cross-border worker share (1995) | 0.075 | (0.168) | 0.004 | (0.038) | 0.072*** | (0.001) |
| Training firm (0/1) | 0.209 | (0.406) | 0.209 | (0.406) | -0.000 | (0.003) |
| Apprentice share | 0.044 | (0.111) | 0.046 | (0.112) | -0.001 | (0.001) |
| Manufacturer (0/1) | 0.114 | (0.318) | 0.114 | (0.318) | 0.000 | (0.003) |
| High-tech manufacturing (0/1) | 0.032 | (0.175) | 0.032 | (0.175) | -0.000 | (0.001) |
| Construction (0/1) | 0.086 | (0.280) | 0.086 | (0.280) | 0.000 | (0.002) |
| Publicly owned firm (0/1) | 0.124 | (0.330) | 0.123 | (0.329) | 0.001 | (0.003) |
| Exporter (0/1, 1995) | 0.163 | (0.369) | 0.157 | (0.364) | 0.006* | (0.003) |
| Importer (0/1, 1995) | 0.258 | (0.437) | 0.256 | (0.436) | 0.002 | (0.004) |
| Travel minutes to border | 7.129 | (3.544) | 47.567 | (13.661) | -40.438*** | (0.082) |
| Observations | 42,490 | | 17,068 | | 59,558 | |

*Notes:* The table shows mean and standard deviation of pre-treatment characteristics of highly treated establishments (establishments located within 15 minutes to the border in the border region, columns 1 and 2) and a matched control group of establishments located in one of the two control regions (establishments located more than 30 minutes to the border in the border region and establishments in the non-border region, columns 3 and 4). The characteristics are measured in 1998 unless otherwise noted. Columns 5 and 6 test whether the covariates are balanced in treated and control units. Details on the matching are given in the text. A given control observation may appear as a match for more than one treated observation. The average control establishment is matched to 2.5 treated units (hence the difference in the number of observations). Mean and standard deviation in columns 3–4 are weighted by the number of times that an untreated establishment is matched to a treated establishment. "Firm growth 1991–1998 in %" reflects an establishment's growth in FTE employment between 1991 and 1998, winsorized at the 1st and 99th percentile. ***, **, *, denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table A.2: Main results using matched control group

| VARIABLES | (1) FE Foreigners/ total FTEs 1998 | (2) FE Foreigners/ total FTEs 1998 | (3) FE Apprentices/ total FTEs 1998 | (4) FE Apprentices/ total FTEs 1998 |
|---|---|---|---|---|
| $I(t = 2001) \cdot I(d_i \leq 15)$ | 0.021*** | 0.020** | 0.000 | 0.000 |
| | (0.008) | (0.010) | (0.002) | (0.002) |
| $I(t \geq 2005) \cdot I(d_i \leq 15)$ | 0.050*** | 0.047*** | -0.004 | -0.003 |
| | (0.010) | (0.012) | (0.003) | (0.002) |
| Observations | 297,790 | 297,790 | 297,790 | 297,790 |
| R-squared | 0.576 | 0.576 | 0.605 | 0.605 |
| Control group | Matched | Matched | Matched | Matched |
| Firm FE | Yes | Yes | Yes | Yes |
| Period FE | Yes | Yes | Yes | Yes |
| Apprentice supply control | No | Yes | No | Yes |

*Notes:* This table shows the effect of the free movement policy on foreign employment and the provision of apprenticeships if we use control establishments matched to highly treated units based on Mahalanobis distance matching. The regressions are based on data from the business censuses (BC) 1995–2008. The dependent variable in columns 1–2 is full-time equivalent (FTE) employment of foreign workers relative to total full-time equivalent employment in 1998. The dependent variable in columns 3–4 is the number of apprentices relative to total full-time equivalent employment in 1998. The main coefficient of interest is the interaction between firms within 15 minutes commuting time $d_i$ to the border and the free movement period, $I(t \geq 2004) * I(d_i \leq 15)$. The control group is establishments located more than 30 minutes away from the border matched to highly treated units with Mahalanobis distance matching. Details on the matching are given in the associated text. The regressions are weighted by number of times that an untreated establishment is matched to a treated establishment. We then multiply this weight by establishments' average employment pre-1999. The "apprentice supply control" is the (estimated) number of graduates from lower secondary schools (11th grade) in the commuting zone (NUTS-III region). ***, **, *, denote statistical significance at the 1%, 5%, and 10% level, respectively.

# B Further Tables and Figures

Table B.1: Characteristics of recent apprenticeship graduates, natives, and cross-border workers in the border region, 1998 and 2010

| | 1998 | | | 2010 | | |
|---|---|---|---|---|---|---|
| Worker characteristics | Recent apprentices | Cross-border workers | Other natives | Recent apprentices | Cross-border workers | Other natives |
| *Demographic characteristics* | | | | | | |
| Mean age | 24.897 | 39.658 | 42.557 | 24.516 | 40.542 | 44.234 |
| Share male | 0.528 | 0.693 | 0.614 | 0.508 | 0.648 | 0.531 |
| Mean tenure | 3.558 | 9.472 | 10.573 | 2.816 | 7.284 | 9.827 |
| Mean log hourly real wage | 3.311 | 3.455 | 3.620 | 3.321 | 3.545 | 3.687 |
| Share tertiary educated | 0.000 | 0.153 | 0.235 | 0.000 | 0.291 | 0.344 |
| Share secondary educated | 1.000 | 0.513 | 0.588 | 1.000 | 0.485 | 0.541 |
| Share primary educated | 0.000 | 0.334 | 0.178 | 0.000 | 0.224 | 0.115 |
| *Occupations* | | | | | | |
| Manufacture | 0.125 | 0.286 | 0.111 | 0.105 | 0.210 | 0.070 |
| Construction | 0.092 | 0.121 | 0.053 | 0.102 | 0.097 | 0.039 |
| Machine operators | 0.069 | 0.064 | 0.058 | 0.070 | 0.072 | 0.051 |
| Define goal & strategy | 0.002 | 0.010 | 0.039 | 0.002 | 0.017 | 0.034 |
| Accounting, HR | 0.047 | 0.026 | 0.059 | 0.036 | 0.029 | 0.057 |
| Clerks | 0.105 | 0.023 | 0.073 | 0.064 | 0.028 | 0.057 |
| Other clerical occupations | 0.113 | 0.047 | 0.080 | 0.089 | 0.055 | 0.074 |
| Logistics, strategy department | 0.010 | 0.020 | 0.026 | 0.014 | 0.026 | 0.022 |
| Review, consult, certify | 0.049 | 0.012 | 0.058 | 0.041 | 0.033 | 0.069 |
| Retail | 0.126 | 0.060 | 0.101 | 0.155 | 0.069 | 0.091 |
| R&D | 0.008 | 0.038 | 0.017 | 0.006 | 0.049 | 0.023 |
| Analyze, program, operating | 0.019 | 0.034 | 0.030 | 0.020 | 0.041 | 0.032 |
| Plan, design | 0.036 | 0.030 | 0.045 | 0.032 | 0.034 | 0.032 |
| Transport | 0.031 | 0.069 | 0.062 | 0.036 | 0.046 | 0.043 |
| Security | 0.003 | 0.001 | 0.004 | 0.024 | 0.005 | 0.021 |
| Medical, social tasks | 0.063 | 0.036 | 0.053 | 0.078 | 0.068 | 0.087 |
| Manicure, cleaning | 0.015 | 0.014 | 0.016 | 0.024 | 0.021 | 0.032 |
| Education | 0.008 | 0.006 | 0.027 | 0.014 | 0.025 | 0.095 |
| Restaurants and hospitality | 0.042 | 0.057 | 0.035 | 0.064 | 0.056 | 0.039 |
| Culture, sport, information | 0.009 | 0.002 | 0.010 | 0.014 | 0.009 | 0.020 |
| Others | 0.015 | 0.010 | 0.025 | 0.010 | 0.009 | 0.011 |
| *Industries* | | | | | | |
| Agriculture/Fishing/Mining | 0.003 | 0.005 | 0.004 | 0.013 | 0.006 | 0.006 |
| Manufacturing | 0.215 | 0.461 | 0.268 | 0.167 | 0.361 | 0.172 |
| Utilities | 0.002 | 0.001 | 0.007 | 0.007 | 0.003 | 0.011 |
| Construction | 0.093 | 0.127 | 0.062 | 0.106 | 0.102 | 0.049 |
| Wholesale/Retail/Repair | 0.242 | 0.144 | 0.188 | 0.258 | 0.146 | 0.164 |
| Hotel/Restaurants | 0.045 | 0.055 | 0.034 | 0.061 | 0.048 | 0.033 |
| Transport/Communication | 0.062 | 0.064 | 0.086 | 0.056 | 0.056 | 0.063 |
| Financial Intermediation | 0.113 | 0.021 | 0.104 | 0.056 | 0.024 | 0.085 |
| Real Estate/R&D/IT | 0.114 | 0.056 | 0.112 | 0.114 | 0.117 | 0.123 |
| Education | 0.007 | 0.007 | 0.024 | 0.015 | 0.028 | 0.112 |
| Health | 0.073 | 0.042 | 0.083 | 0.106 | 0.088 | 0.138 |
| Personal Services | 0.031 | 0.016 | 0.028 | 0.042 | 0.024 | 0.045 |
| Number of Workers | 163,977 | 103,885 | 859,185 | 192,977 | 185,661 | 1,358,600 |

*Notes:* This table shows descriptive statistics of recent native apprenticeship graduates, native workers, cross-border workers in 1998 and their change between 1998 and 2010. The table is based on data from the Swiss Earnings Structure Surveys 1998 and 2010. We focus on workers in the border region aged 18–65 working in the private sector, with non-missing information for nationality, place of work, education, wages, and full-time equivalents. Native workers are Swiss nationals, either born in Switzerland or naturalized. Cross-border workers are identified based on their residency permit. Recent native apprenticeship graduates are Swiss workers aged 18–29 with an apprenticeship as highest degree.

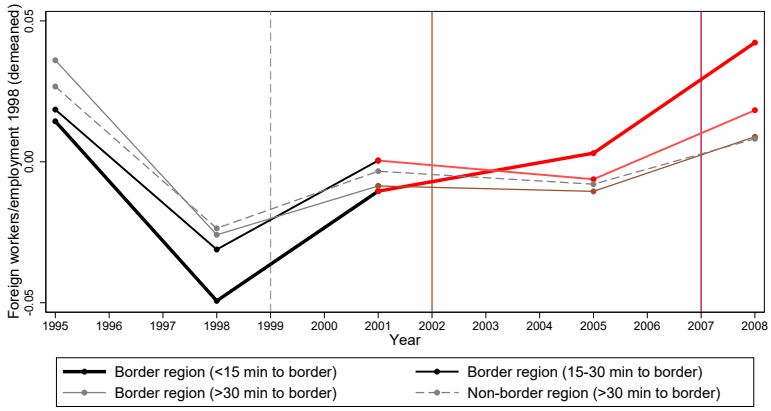
Table B.2: Employment shares of recent apprenticeship graduates and cross-border workers in the border region, 1998 and 2010

| | Recent apprentices | | | Cross-border workers | | |
|---|---|---|---|---|---|---|
| Employment share | 1998 | 2010 | Δ 2010 - 1998 | 1998 | 2010 | Δ 2010 -1998 |
| *Employment share by occupation* | | | | | | |
| Manufacture | 0.064 | 0.073 | 0.009 | 0.137 | 0.183 | 0.046 |
| Construction | 0.089 | 0.110 | 0.020 | 0.108 | 0.129 | 0.020 |
| Machine operators | 0.085 | 0.083 | -0.002 | 0.081 | 0.117 | 0.036 |
| Define goal & strategy | 0.005 | 0.005 | 0.000 | 0.025 | 0.056 | 0.031 |
| Accounting, HR | 0.076 | 0.052 | -0.024 | 0.040 | 0.056 | 0.015 |
| Clerks | 0.130 | 0.090 | -0.040 | 0.028 | 0.052 | 0.023 |
| Other clerical occupations | 0.113 | 0.085 | -0.028 | 0.045 | 0.066 | 0.021 |
| Logistics, strategy department | 0.035 | 0.044 | 0.009 | 0.073 | 0.102 | 0.029 |
| Review, consult, certify | 0.080 | 0.045 | -0.036 | 0.018 | 0.048 | 0.030 |
| Retail | 0.087 | 0.103 | 0.016 | 0.046 | 0.067 | 0.022 |
| R&D | 0.034 | 0.014 | -0.020 | 0.154 | 0.158 | 0.004 |
| Analyze, program, operating | 0.052 | 0.041 | -0.011 | 0.094 | 0.108 | 0.014 |
| Plan, design | 0.073 | 0.073 | 0.000 | 0.055 | 0.095 | 0.040 |
| Transport | 0.038 | 0.049 | 0.011 | 0.078 | 0.082 | 0.004 |
| Security | 0.065 | 0.094 | 0.029 | 0.052 | 0.027 | -0.025 |
| Medical, social tasks | 0.102 | 0.061 | -0.041 | 0.051 | 0.081 | 0.030 |
| Manicure, cleaning | 0.045 | 0.034 | -0.011 | 0.040 | 0.040 | 0.000 |
| Education | 0.029 | 0.013 | -0.016 | 0.019 | 0.029 | 0.010 |
| Restaurants and hospitality | 0.055 | 0.069 | 0.015 | 0.073 | 0.080 | 0.007 |
| Culture, sport, information | 0.072 | 0.049 | -0.022 | 0.016 | 0.042 | 0.026 |
| Others | 0.045 | 0.048 | 0.002 | 0.032 | 0.063 | 0.031 |
| *Employment share by education* | | | | | | |
| Share tertiary educated | 0.000 | 0.000 | 0.000 | 0.061 | 0.079 | 0.018 |
| Share secondary educated | 0.161 | 0.139 | -0.022 | 0.066 | 0.074 | 0.008 |
| Share primary educated | 0.000 | 0.000 | 0.000 | 0.104 | 0.108 | 0.004 |
| *Employment share by industry* | | | | | | |
| Agriculture/Fishing/Mining | 0.061 | 0.138 | 0.077 | 0.082 | 0.084 | 0.002 |
| Manufacturing | 0.055 | 0.058 | 0.002 | 0.115 | 0.167 | 0.052 |
| Utilities | 0.032 | 0.056 | 0.025 | 0.013 | 0.032 | 0.018 |
| Construction | 0.083 | 0.104 | 0.020 | 0.112 | 0.131 | 0.019 |
| Wholesale/Retail/Repair | 0.092 | 0.094 | 0.003 | 0.055 | 0.080 | 0.025 |
| Hotel/Restaurants | 0.059 | 0.077 | 0.017 | 0.072 | 0.085 | 0.013 |
| Transport/Communication/Storage | 0.053 | 0.055 | 0.001 | 0.051 | 0.074 | 0.022 |
| Financial Intermediation | 0.092 | 0.051 | -0.040 | 0.017 | 0.032 | 0.015 |
| Real Estate/R&D/IT/Business | 0.086 | 0.058 | -0.028 | 0.041 | 0.080 | 0.039 |
| Education | 0.028 | 0.012 | -0.016 | 0.027 | 0.028 | 0.001 |
| Health | 0.075 | 0.054 | -0.021 | 0.040 | 0.069 | 0.030 |
| Personal Services | 0.083 | 0.069 | -0.014 | 0.046 | 0.055 | 0.009 |
| Number of Workers | 163,977 | 192,977 | 29,000 | 119,962 | 217,649 | 97,687 |

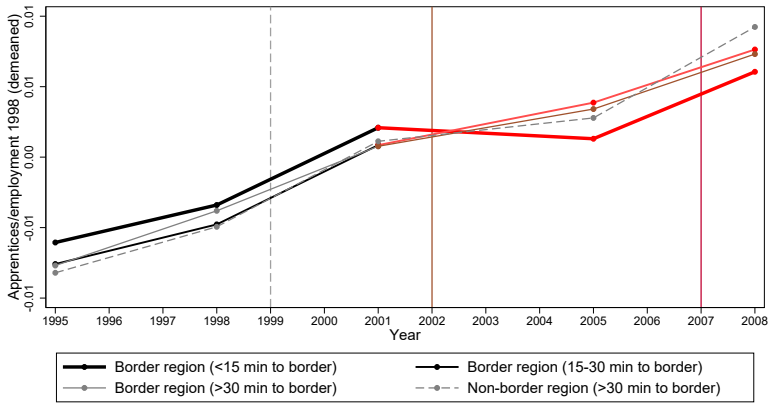
*Notes:* This table shows the share of recent apprentices and cross-border workers in total employment in the border region in 1998 and 2010. It uses data from the Swiss Earnings Structure Surveys 1998 and 2010. The employment share is the fraction that the two worker categories make up in the specific occupation/industry in the border region. We focus on workers aged 18–65 working in the private sector, with non-missing information for nationality, place of work, education, wages, and full-time equivalents. Cross-border workers are identified based on their residency permit. Recent native apprenticeship graduates are Swiss workers aged 18–29 with an apprenticeship as highest degree.

Figure B.1: Illustration of DiD identification strategy: establishment-demeaned main outcomes

(a) Foreign worker/total employment in 1998



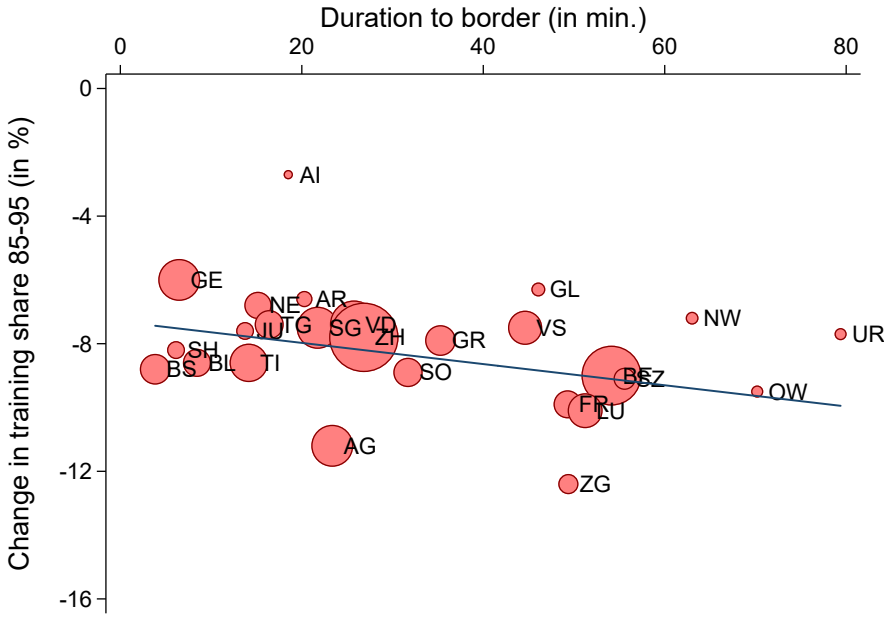

(b) Apprentice/total employment in 1998



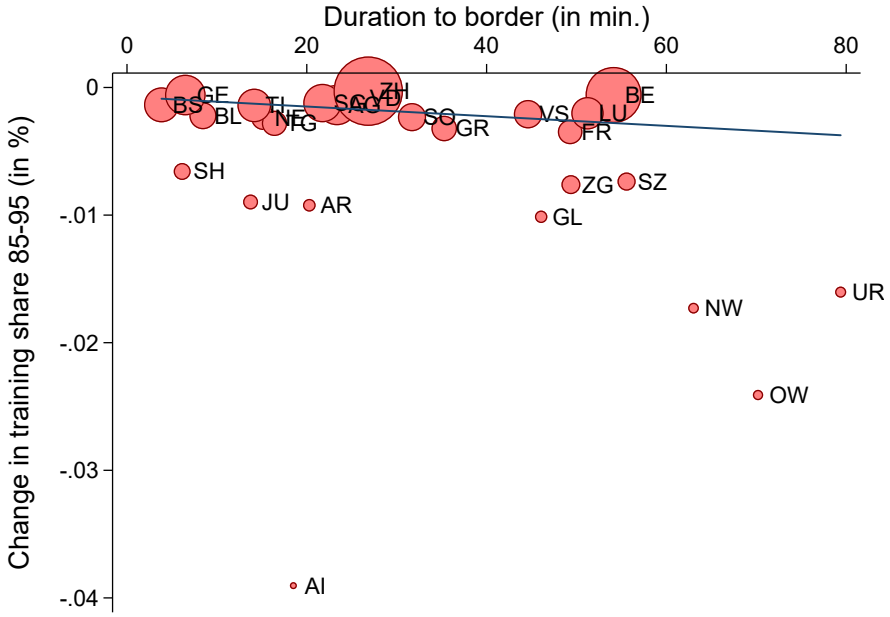

*Notes*: This figure shows establishments' employment foreign workers (panel a) and apprentices (panel b), expressed as a share of total employment in 1998, averaged for four groups: highly treated establishments (establishments within 15 minutes of the border), slightly treated establishments (establishments within 15–30 minutes of the border), establishments more than 30 minutes of the border within the border region, and establishments in the non-border region. We focus on a balanced sample of establishments comprising of all establishments existing in all business censuses 1995–2008. To mirror the establishment fixed effects regressions in our paper, we demean both variable with an establishment's average of the outcome over the entire 1995–2008 period. Formally, we focus on $y_{it} - \bar{y}_{it}$, where $\bar{y}_{it}$ is the mean outcome for the firm over the sample years 1995–2008. As in our regressions, we weight each establishment using an establishment's initial employment as weight.

Figure B.2: Long-run trends in firms' apprenticeship provision in Swiss cantons

A. Change in training share, 1985–1995

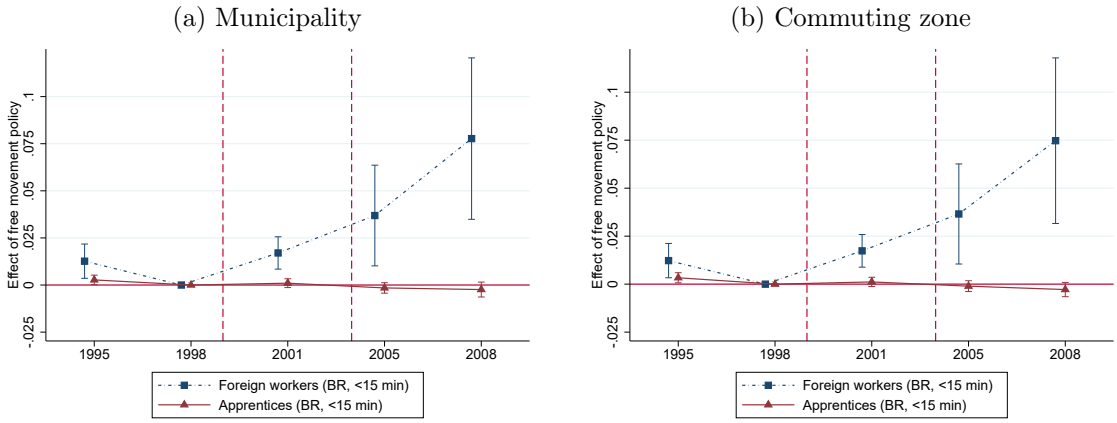



B. Change in apprentice share, 1985–1995




*Notes:* This figure plots a canton's travel duration to the border against the change in the share of firms engaged in apprenticeship training between 1985 and 1995 (panel a) and the change in the number of apprentices between 1985 and 1995, normalized with total employment in 1985 (panel b). Travel duration is measured as the employment-weighted average travel duration of the establishments in the canton. The size of each dot is proportional to a canton's number of establishments (panel a) or employment (panel b), respectively, in 1985. Source of the data for the training share is Mueller, B., & Schweri, J. (2012). Die Betriebe in der dualen Berufsbildung: Entwicklungen 1985 bis 2008: eine Analyse der Betriebszaehlungsdaten, durchgefuehrt durch das eidgenoessische Hochschulinstitut fuer Berufsbildung (EHB). Bundesamt fuer Statistik.

Figure B.3: Effect of free movement policy on foreign employment and apprenticeship training: commuting zone level estimates




*Notes:* The figure shows the effect of the free movement policy on employment of foreign workers and the provision of apprenticeships in highly treated units using data from the Swiss business censuses 1995–2008 aggregated to the municipality level (panel a) and the commuting zone level (106 MS regions, panel b). The figures plot the estimated policy effects and associated 95% confidence intervals using a generalization of our main DiD model (equation (1)) that estimates separate effects for each census year. We focus on highly treated municipalities/regions within 15 minutes of the border. In panel b, we assign each commuting zone to the treatment (control) region if the majority of employment is (not) in the border region. The dependent variables are (i) full-time equivalent employment of foreign workers relative to total full-time equivalent employment in 1998, and (ii) the number of apprentices relative to total employment in 1998. The regressions are weighted using average employment in 1998 as weight. All regressions account for period fixed effects and region fixed effects. Confidence intervals are clustered on the level of commuting zones.

Table B.3: Variables in the cost-benefit data

| Variable name | Question |
|---|---|
| *A. Recruitment cost* | |
| Hires of skilled workers | How many skilled workers did your company hire in the selected [focus] occupation or comparable occupations in the last three years? |
| Interviews per job | How many job applicants are typically interviewed if you hire skilled workers in the selected or a comparable occupation? |
| Interview time in hours | How long (in hours) does it take on average to interview a skilled worker (including preparation, conduct, and follow-up of interviews, administrative processing) for all involved employees? |
| Advertisement costs per job | What are the current average costs for job advertising in Swiss francs (press advertisements, inquiries at the public employment office, internal job advertisements, etc.) for a new skilled worker if it is recruited in the selected or a comparable occupation? |
| Costs of external consultants | Costs for external consultants per successful new hire in Swiss francs. |
| Adaptation period in months | How many months does the adaptation period last on average if your company recruits skilled workers in the selected occupation or a comparable occupation externally, assuming they are hired for a similar job as the trained apprentices in this occupation? |
| Shortfall in productivity | On average, how much lower (in percentage) is the performance of the externally hired skilled workers during the training period compared to an average skilled worker in your company? |
| Direct training costs | Do the externally recruited skilled workers, if employed in similar jobs as trained apprentices, normally take part in training courses? If yes, what are the costs of those courses per skilled worker in Swiss francs? |

| | |
|---|---|
| *B. Personnel problems* | Do you have any of the following personnel problems (Yes/No)? |
| Insufficient qualifications | Insufficient qualification/competencies of the skilled workers in the area |
| Skills shortage | Difficulties in finding suitable skilled workers on the labor market |
| Work permits for foreigners | Difficulties in obtaining work permits for suitable foreign workers |
| *C. Training motives* | **Training firm:** For your firm, how important are the following reasons to train apprentices (on a 5-point scale from 1 "not important" to 5 "very important")?<br>**Non-training firm:** Assuming that your firm meets all the requirements and decided to start training apprentices, how important would the following aspects of training apprentices be for your firm? |
| Attract skilled workers | Attracting skilled workers because it is hard to find qualified personnel on the external labor market |
| Hiring costs | Saving the costs of hiring personnel on the external labor market |
| Risk of wrong decision | Avoiding the risk of wrong hiring decisions that comes with external hiring |
| Replace unskilled workers | Replacing unskilled and semi-skilled workers with apprentices' work |
| Adjustment costs | Saving the money used for training external specialists (adjustment costs) |
| Hire the best | Having the opportunity to hire the "best" young person as apprentices |
| Avoid fluctuation | Avoiding high turnover by hiring specialists whose skills match the firm's needs very closely |
| Qualify junior staff | Training junior workers into skilled workers whose skills exactly match the firm's requirements |
| Secure skilled workers | Securing a talent pipeline in the sector/region |

*Notes:* The table presents the questions used in the cost-benefit survey to inquire the training motives of firms. Training firms got a direct question on their reason to train. Non-training firms are asked a hypothetical question if they immediately started with training. All firms rated the importance of each training motive on a 5-point Likert scale.

Table B.4: Relationship between changes in apprenticeship provision 1985–1995 and duration to the border at the cantonal level

| VARIABLES | (1) OLS Δ training share | (2) WLS Δ training share | (3) OLS Δ apprentices | (4) WLS Δ apprentices |
|---|---|---|---|---|
| Distance to border 15-30 min. | 0.792 | -0.293 | -0.004 | 0.001 |
| | (0.955) | (0.702) | (0.005) | (0.002) |
| Distance to border >30 min. | -0.825 | -1.240 | -0.005 | -0.001 |
| | (0.884) | (0.730) | (0.005) | (0.002) |
| Observations | 26 | 26 | 26 | 26 |
| R-squared | 0.150 | 0.143 | 0.042 | 0.066 |
| Weights | No | Establishments 85 | No | Employment 85 |

*Notes:* This table uses the canton-level data from Figure B.2 to relate the change in the share of firms engaged in apprenticeship training between 1985 and 1995 (columns 1 and 2) and the change in the number of apprentices between 1985 and 1995, normalized with total employment in 1985 (columns 3 and 4), to indicators of a canton's travel distance to the border. Columns 1 and 3 are unweighted, columns 2 and 4 use the total number of establishments and total employment in 1985 as weights, respectively. We report standard OLS standard errors. ***, **, *, denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table B.5: PPML estimates of the effect of free movement policy on employment of foreigners and apprentices

| VARIABLES | (1) PPML Foreign workers | (2) PPML Foreign workers | (3) PPML Foreign workers | (4) PPML Apprentices | (5) PPML Apprentices | (6) PPML Apprentices |
|---|---|---|---|---|---|---|
| $I(t=2001) \cdot I(d_i \leq 15)$ | 0.156** | 0.355*** | 0.398*** | 0.021 | 0.013 | 0.025 |
| | (0.067) | (0.102) | (0.145) | (0.055) | (0.061) | (0.059) |
| $I(t=2001) \cdot I(15 < d_i \leq 30)$ | 0.239*** | 0.446*** | 0.177 | 0.069** | 0.060 | 0.073* |
| | (0.072) | (0.099) | (0.127) | (0.034) | (0.044) | (0.038) |
| $I(t \geq 2005) \cdot I(d_i \leq 15)$ | 0.456*** | 0.623*** | | | | |
| | (0.125) | (0.183) | | | | |
| $I(t \geq 2005) \cdot I(15 < d_i \leq 30)$ | 0.242** | 0.388** | | | | |
| | (0.107) | (0.172) | | | | |
| Observations | 531,530 | 395,035 | 457,745 | 345,080 | 231,100 | 293,430 |
| Control group | Both | BR 30+ | NBR | Both | BR 30+ | NBR |
| Balanced sample | Yes | Yes | Yes | Yes | Yes | Yes |
| Establishment FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Period FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* This table shows the average marginal effects of the free movement policy on employment of foreign workers (columns 1–3) and apprentices (columns 4–6) using our main DiD model (equation (1)). We estimate the model using Pseudo Poisson Maximum Likelihood (PPML). The regressions are based on data from the business censuses (BC) 1995–2008. The dependent variables are full-time equivalent (FTE) employment of foreign workers and employment of apprentices. The effect of the free movement period is captured with a dummy variable equal to one in the census years after 2004. The main coefficient of interest is the interaction between firms within 15 minutes commuting time $d_i$ to the border and the free movement period, $I(t \geq 2005) \cdot I(d_i \leq 15)$. The control group is specified in the table footer. Standard errors are clustered on the level of commuting zones. ***, **, *, denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table B.6: Municipality-level estimates of the effect of free movement policy on immigrant employment and apprenticeship training

| VARIABLES | (1) FE Foreign workers/ FTEs 1998 | (2) FE Number of training firms | T es |
|---|---|---|---|
| $I(t=2001) \cdot I(d_i \leq 15)$ | 0.011** | 0.543 | |
| | (0.005) | (0.597) | |
| $I(t=2001) \cdot I(15 < d_i \leq 30)$ | 0.013** | 1.750* | |
| | (0.005) | (0.885) | |
| $I(t \geq 2005) \cdot I(d_i \leq 15)$ | 0.051*** | 0.656 | |
| | (0.017) | (0.704) | |
| $I(t \geq 2005) \cdot I(15 < d_i \leq 30)$ | 0.021*** | 2.674** | |
| | (0.006) | (1.162) | |
| Observations | 12,934 | 12,934 | |
| R-squared | 0.205 | 0.052 | |
| Number of municipalities | 2,594 | 2,594 | |
| Control group | Both | Both | |
| Municipality FE | Yes | Yes | |
| Period FE | Yes | Yes | |
| Weights | Employment | No | E |

*Notes:* This table shows the effect of the free movement policy on employment o of apprenticeships using our main DiD model (equation (1)). The regressions a censuses (BC) 1995–2008, aggregated to the municipality level. The dependen equivalent (FTE) employment of foreign workers (cross-border workers plus fo relative to total full-time equivalent employment in 1998 in the municipality. 2 is the number of establishments that train apprentices. The dependent va establishments with apprenticeships among all establishments. The dependent of apprentices working in a specific municipality relative to total full-time equ effect of the free movement period is captured with a dummy variable equal to i.e. the BC 2005 and 2008. The main coefficient of interest is the interaction commuting time $d_i$ to the border and the free movement period, $I(t \geq 2005)$ firms located in the border region with more than 30 minutes travel time to the region. Regressions in columns 1 and 4 are weighted using average municipa The regression in column 3 is weighted using the average number of establish Standard errors are clustered on the level of commuting zones. ***, **, *, denot 5%, and 10% level, respectively.

Table B.7: Effect of the free movement policy on firms' training behavior in the cost-benefit data

| VARIABLES | (1) Training firm | (2) Number of apprentices | (3) Apprentice share | (4) Number of apprentices | (5) Apprentice share |
|---|---|---|---|---|---|
| $I(d_{\text{i}} \leq 15)$ | -0.037 | -0.062 | -0.010 | -0.034 | -0.015 |
| | (0.038) | (0.054) | (0.011) | (0.051) | (0.014) |
| $I(d_{\text{i}} \leq 15) \cdot I(t = 2004)$ | -0.000 | -0.004 | 0.002 | -0.028 | 0.020 |
| | (0.031) | (0.049) | (0.011) | (0.060) | (0.033) |
| $I(d_{\text{i}} \leq 15) \cdot I(t = 2009)$ | 0.012 | 0.018 | 0.007 | 0.010 | 0.003 |
| | (0.021) | (0.028) | (0.006) | (0.051) | (0.021) |
| $I(15 < d_{\text{i}} < 30)$ | -0.023 | -0.029 | -0.008 | 0.008 | -0.014 |
| | (0.023) | (0.030) | (0.006) | (0.043) | (0.013) |
| $I(15 < d_{\text{i}} \leq 30) \cdot I(t = 2004)$ | 0.014 | 0.022 | 0.011 | 0.032 | 0.034 |
| | (0.029) | (0.045) | (0.013) | (0.069) | (0.023) |
| $I(15 < d_{\text{i}} \leq 30) \cdot I(t = 2009)$ | 0.031 | 0.065** | 0.017** | 0.136*** | 0.030 |
| | (0.019) | (0.030) | (0.006) | (0.045) | (0.023) |
| Observations | 15544 | 15544 | 14135 | 4553 | 3931 |
| Sample | All | All | All | Training | Training |
| Industry x Year FE | Yes | Yes | Yes | Yes | Yes |
| Occupation x Year FE | Yes | Yes | Yes | Yes | Yes |
| Firm Size FE | Yes | Yes | Yes | Yes | Yes |
| Apprentice supply control | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table estimates the effect of the free movement policy on the training behavior of firms in the cost-benefit data. All DiD estimations are based on OLS regressions using the cost-benefit surveys in 2000, 2004 and 2009. Columns 1–3 use all firms, columns 4 and 5 only firm that train at least one apprentice. The treated groups are the firms located up to 15 minutes from the closest border within the border region ($I(d_i \leq 15)$) and the firms located 15 minutes to up to 30 minutes from the closest border within the border region ($I(15 < d_i \leq 30)$). The control group consists of firms located more than 30 minutes away from the closest border within the border region and outside of it. All regressions control for firm size fixed effects (4 groups), industry times year fixed effects, and occupation times year fixed effects. The "apprentice supply control" is the number of graduates from lower secondary schools (11th grade) in the commuting zone (NUTS-III region). The estimations are weighted using the surveys' sampling weights capped at 100. Standard errors are clustered on the level of commuting zones. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table B.8: Effect of the free movement policy on apprentice share, by occupation

| VARIABLES | (1) Merchant | (2) Electrician | (3) Polymechanic | (4) Retail specialist | (5) Information technologist | (6) Chef | (7) Draughtsman | (8) Bricklayer |
|---|---|---|---|---|---|---|---|---|
| $I(d_{\text{i}} \leq 15) \cdot I(t = 2004)$ | 0.025 | -0.040 | -0.036 | 0.081 | 0.001 | -0.005 | 0.000 | 0.044 |
| | (0.021) | (0.112) | (0.070) | (0.060) | (0.026) | (0.033) | (0.079) | (0.029) |
| $I(d_{\text{i}} \leq 15) \cdot I(t = 2009)$ | 0.022 | -0.119 | -0.060 | 0.008 | -0.023 | 0.041 | -0.068 | 0.014 |
| | (0.019) | (0.090) | (0.035) | (0.044) | (0.019) | (0.028) | (0.079) | (0.032) |
| $I(15 < d_{\text{i}} \leq 30) \cdot I(t = 2004)$ | -0.003 | 0.077 | -0.066* | 0.027 | 0.009 | 0.013 | 0.088 | 0.151 |
| | (0.016) | (0.136) | (0.031) | (0.039) | (0.019) | (0.023) | (0.070) | (0.105) |
| $I(15 < d_{\text{i}} \leq 30) \cdot I(t = 2009)$ | 0.023* | 0.079 | -0.020 | -0.006 | -0.004 | 0.030 | 0.073 | 0.030 |
| | (0.013) | (0.109) | (0.030) | (0.047) | (0.026) | (0.025) | (0.081) | (0.022) |
| Observations | 4516 | 364 | 576 | 605 | 585 | 938 | 391 | 390 |
| Sample | All | All | All | All | All | All | All | All |
| Industry x Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm Size FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Apprentice supply control | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table estimates the effect of the free movement policy on the apprentice share in the cost-benefit data for each occupation with more than 300 observations and observed in all three years separately. All DiD estimations are based on OLS regressions using the cost-benefit surveys in 2000, 2004 and 2009. The treated groups are the firms located up to 15 minutes from the closest border within the border region ($I(d_i \leq 15)$) and the firms located 15 minutes to up to 30 minutes from the closest border within the border region ($I(15 < d_i \leq 30)$). The control group consists of firms located more than 30 minutes away from the closest border within the border region and outside of it. All regressions control for firm size fixed effects (4 groups), industry times year fixed effects, and occupation times year fixed effects (see text for details). The "apprentice supply control" is the number of graduates from lower secondary schools (11th grade) in the commuting zone (NUTS-III region). The estimations are weighted using the surveys' sampling weights capped at 100. Standard errors are clustered on the level of commuting zones. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table B.9: Effect of the free movement policy on firms' training behavior in the cost-benefit data: investment versus production motive

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | ***Investment*** | | ***Production*** | |
| VARIABLES | Number of apprentices | Apprentice share | Number of apprentices | Apprentice share |
| $I(d_i \leq 15)$ | -0.127 | 0.007 | 0.077 | 0.025 |
| | (0.075) | (0.021) | (0.088) | (0.022) |
| $I(d_i \leq 15) \cdot I(t = 2004)$ | 0.051 | -0.041 | -0.110 | 0.044 |
| | (0.086) | (0.039) | (0.112) | (0.052) |
| $I(d_i \leq 15) \cdot I(t = 2009)$ | 0.166* | 0.010 | -0.016 | 0.041* |
| | (0.089) | (0.033) | (0.110) | (0.022) |
| $I(15 < d_i < 30)$ | -0.124* | -0.008 | 0.060 | 0.022 |
| | (0.063) | (0.020) | (0.054) | (0.021) |
| $I(15 < d_i \leq 30) \cdot I(t = 2004)$ | 0.157* | -0.010 | 0.034 | 0.054 |
| | (0.079) | (0.024) | (0.092) | (0.032) |
| $I(15 < d_i \leq 30) \cdot I(t = 2009)$ | 0.187** | 0.042 | 0.048 | 0.013 |
| | (0.069) | (0.025) | (0.063) | (0.029) |
| Observations | 1602 | 1602 | 2329 | 2329 |
| Industry x Year FE | Yes | Yes | Yes | Yes |
| Occupation x Year FE | Yes | Yes | Yes | Yes |
| Firm Size FE | Yes | Yes | Yes | Yes |
| Apprentice supply control | Yes | Yes | Yes | Yes |

*Notes:* The table estimates the effect of the free movement policy on the training behavior of firms, separately for firms that train apprentices according to the investment and production motives. Firms train according to the investment (production) motive if the estimated net costs of training are negative (non-negative). All DiD estimations are based on OLS regressions using the cost-benefit surveys in 2000, 2004 and 2009. The treated groups are the firms located up to 15 minutes from the closest border within the border region ($I(d_i \leq 15)$) and the firms located 15 minutes to up to 30 minutes from the closest border within the border region ($I(15 < d_i \leq 30)$). The control group consists of firms located more than 30 minutes away from the closest border within the border region and outside of it. All regressions control for firm size fixed effects (4 groups), industry times year fixed effects, and occupation times year fixed effects (see text for details). The "apprentice supply control" is the number of graduates from lower secondary schools (11th grade) in the commuting zone (NUTS-III region). The estimations are weighted using the surveys' sampling weights capped at 100. Standard errors are clustered on the level of commuting zones. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table B.10: Effect of free movement policy on firms' perceived personnel problems and training motives (original survey weights)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Personnel problem Insufficient applications | Personnel problem Skills shortage | Personnel problem Work permits (foreigners) | Tra mo Att ski wor |
| $I(d_i \leq 15)$ | 0.069*** | 0.050* | 0.022 | 0. |
| | (0.020) | (0.030) | (0.020) | (0. |
| $I(d_i \leq 15) \cdot I(t = 2004)$ | -0.022 | -0.065* | -0.014 | 0. |
| | (0.027) | (0.034) | (0.032) | (0. |
| $I(d_i \leq 15) \cdot I(t = 2009)$ | -0.069*** | -0.008 | -0.017 | -0. |
| | (0.022) | (0.040) | (0.015) | (0. |
| $I(15 < d_i < 30)$ | -0.029 | 0.027 | 0.005 | 0. |
| | (0.022) | (0.024) | (0.016) | (0. |
| $I(15 < d_i \leq 30) \cdot I(t = 2004)$ | 0.002 | -0.037 | -0.006 | -0. |
| | (0.014) | (0.033) | (0.024) | (0. |
| $I(15 < d_i \leq 30) \cdot I(t = 2009)$ | -0.038* | -0.045 | -0.006 | -0. |
| | (0.021) | (0.034) | (0.014) | (0. |
| Observations | 16332 | 15237 | 14297 | 15 |
| Estimation method | Probit | Probit | Probit | Ord pr |
| Industry x Year FE | Yes | Yes | Yes | Y |
| Occupation x Year FE | Yes | Yes | Yes | Y |
| Firm size FE | Yes | Yes | Yes | Y |
| Apprentice supply control | Yes | Yes | Yes | Y |

*Notes:* The table analyzes how the free movement policy affected perceived perso motives of firms in the cost-benefit data. All DiD estimations are based on prob (columns 4–6) regressions using the cost-benefit surveys in 2000, 2004 and 20 average marginal effects. The dependent variables in columns 1–3 are dumm perceives a certain personnel problem at the time of the survey. The depen qualitative survey questions on the importance of certain training motives (5 important and 5 is very important). Appendix Table B.3 provides a detailed exp and the corresponding survey questions. The treated groups are the firms locate border within the border region ($I(d_i \leq 15)$) and the firms located 15 minutes t minutes away from the closest border within the border region and outside of size fixed effects (4 groups), industry times year fixed effects, and occupation t details). The apprentice supply control is the number of graduates from low in the commuting zone (NUTS-III region). The estimations are weighted usin sampling weights. Standard errors are clustered on the level of commuting z significance at the 1%, 5%, and 10% level, respectively.

Table B.11: Effect of the free movement policy on firms' training motives

| VARIABLES | (1) Attract skilled workers | (2) Hiring costs | (3) Risk of wrong decision | (4) Replace unskilled workers | (5) Adjust-ment costs | (6) Hire the best | (7) Avoid fluc-tuation | (8) Qualify junior staff | (9) Secure skilled workers |
|---|---|---|---|---|---|---|---|---|---|
| $I(d_i \leq 15)$ | 0.035 | 0.158*** | 0.196** | 0.045 | 0.100 | 0.093 | 0.131 | 0.155* | -0.054 |
| | (0.098) | (0.060) | (0.097) | (0.110) | (0.077) | (0.067) | (0.080) | (0.091) | (0.074) |
| $I(d_i \leq 15) \cdot I(t = 2004)$ | 0.050 | -0.009 | -0.077 | 0.089 | 0.054 | -0.052 | -0.040 | -0.151 | 0.119 |
| | (0.103) | (0.081) | (0.086) | (0.094) | (0.087) | (0.061) | (0.061) | (0.092) | (0.086) |
| $I(d_i \leq 15) \cdot I(t = 2009)$ | -0.096 | -0.116* | -0.132 | -0.008 | -0.078 | -0.001 | -0.018 | -0.108 | 0.102** |
| | (0.090) | (0.060) | (0.086) | (0.081) | (0.073) | (0.059) | (0.039) | (0.093) | (0.045) |
| $I(15 < d_i < 30)$ | 0.088 | 0.111* | 0.081 | 0.100 | 0.108 | 0.081 | 0.105 | 0.096 | 0.030 |
| | (0.087) | (0.060) | (0.073) | (0.113) | (0.072) | (0.065) | (0.065) | (0.063) | (0.048) |
| $I(15 < d_i \leq 30) \cdot I(t = 2004)$ | -0.151** | 0.030 | 0.023 | -0.025 | 0.033 | -0.067 | -0.101* | -0.168** | -0.034 |
| | (0.071) | (0.065) | (0.080) | (0.114) | (0.086) | (0.079) | (0.055) | (0.081) | (0.072) |
| $I(15 < d_i \leq 30) \cdot I(t = 2009)$ | -0.083 | -0.075 | -0.027 | -0.080 | -0.057 | -0.061 | -0.067* | -0.123*** | -0.023 |
| | (0.081) | (0.062) | (0.063) | (0.102) | (0.063) | (0.069) | (0.041) | (0.048) | (0.042) |
| Observations | 15544 | 15544 | 15544 | 15544 | 15544 | 15544 | 15544 | 15544 | 15544 |
| Industry x Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Occupation x Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm Size FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Apprentice supply control | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table analyzes how the free movement policy affected the training motives of firms in the cost-benefit data. All DiD estimations are based on ordered probit regressions using the cost-benefit surveys in 2000, 2004 and 2009. The dependent variables are qualitative survey questions on the importance of certain training motives (5-point Likert scale, where 1 is not important and 5 is very important). Appendix Table B.3 provides a detailed explanation of the outcome variables and the corresponding survey questions. The treated groups are the firms located up to 15 minutes from the closest border within the border region ($I(d_i \leq 15)$) and the firms located 15 minutes to up to 30 minutes from the closest border within the border region ($I(15 < d_i \leq 30)$). The control group consists of firms located more than 30 minutes away from the closest border within the border region and outside of it. All regressions control for firm size fixed effects (4 groups), industry times year fixed effects, and occupation times year fixed effects (see text for details). The "apprentice supply control" is the number of graduates from lower secondary schools (11th grade) in the commuting zone (NUTS-III region). The estimations are weighted using the surveys' sampling weights capped at 100. Standard errors are clustered on the level of commuting zones. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table B.12: Robustness checks: effects on recruitment and training cost

| | (1) Interviews per job | (2) Interview time in hours | (3) Advertisement costs per job | (4) Costs of external consultant | (5) Adaptation period in months | (6) Shortfall in productivity | (7) Direct training costs |
|---|---|---|---|---|---|---|---|
| ***A. Occupations that are available in all years*** | | | | | | | |
| $I(d_i \leq 15) \cdot I(t = 2004)$ | 0.092 | 0.565 | 0.232 | -0.115 | -0.156 | -0.009 | 0.008 |
| | (0.088) | (0.517) | (0.262) | (0.165) | (0.249) | (0.012) | (0.304) |
| $I(d_i \leq 15) \cdot I(t = 2009)$ | -0.059 | -0.027 | -0.197 | -0.316** | -0.335** | -0.025* | -0.012 |
| | (0.071) | (0.390) | (0.248) | (0.132) | (0.121) | (0.012) | (0.133) |
| $I(15 < d_i \leq 30) \cdot I(t = 2004)$ | -0.003 | -0.217 | -0.050 | -0.163 | -0.185 | -0.010 | 0.184 |
| | (0.102) | (0.433) | (0.332) | (0.119) | (0.173) | (0.011) | (0.163) |
| $I(15 < d_i \leq 30) \cdot I(t = 2009)$ | -0.049 | -0.509 | -0.242 | -0.193* | -0.282** | -0.026*** | 0.135 |
| | (0.056) | (0.323) | (0.164) | (0.098) | (0.121) | (0.006) | (0.163) |
| Observations | 14535 | 14535 | 14535 | 14535 | 14535 | 14535 | 14535 |
| ***B. Original weights*** | | | | | | | |
| $I(d_i \leq 15) \cdot I(t = 2004)$ | 0.033 | 0.550 | 0.131 | -0.188 | -0.163 | -0.023* | 0.072 |
| | (0.080) | (0.563) | (0.230) | (0.203) | (0.192) | (0.013) | (0.260) |
| $I(d_i \leq 15) \cdot I(t = 2009)$ | -0.003 | 0.347 | -0.076 | -0.295* | -0.214 | -0.019 | 0.212 |
| | (0.095) | (0.482) | (0.245) | (0.146) | (0.151) | (0.017) | (0.188) |
| $I(15 < d_i \leq 30) \cdot I(t = 2004)$ | -0.061 | -0.187 | -0.196 | -0.195 | -0.389* | -0.017 | 0.130 |
| | (0.090) | (0.471) | (0.345) | (0.152) | (0.196) | (0.010) | (0.148) |
| $I(15 < d_i \leq 30) \cdot I(t = 2009)$ | -0.026 | -0.509 | -0.061 | -0.164 | -0.102 | -0.018*** | 0.162 |
| | (0.060) | (0.383) | (0.175) | (0.118) | (0.152) | (0.006) | (0.208) |
| Observations | 15544 | 15544 | 15544 | 15544 | 15544 | 15544 | 15544 |
| ***C. No weights*** | | | | | | | |
| $I(d_i \leq 15) \cdot I(t = 2004)$ | 0.011 | 0.076 | 0.021 | -0.160 | -0.261* | -0.009 | -0.332* |
| | (0.047) | (0.397) | (0.133) | (0.199) | (0.128) | (0.007) | (0.187) |
| $I(d_i \leq 15) \cdot I(t = 2009)$ | -0.099* | -0.555 | -0.358* | -0.430* | -0.417** | -0.020* | -0.142 |
| | (0.050) | (0.408) | (0.188) | (0.210) | (0.142) | (0.010) | (0.146) |
| $I(15 < d_i \leq 30) \cdot I(t = 2004)$ | -0.001 | -0.464 | -0.087 | -0.108 | -0.230* | -0.010 | 0.016 |
| | (0.053) | (0.422) | (0.222) | (0.106) | (0.117) | (0.007) | (0.118) |
| $I(15 < d_i \leq 30) \cdot I(t = 2009)$ | -0.028 | -0.900*** | -0.370** | -0.310** | -0.262** | -0.020** | -0.102 |
| | (0.033) | (0.293) | (0.126) | (0.123) | (0.091) | (0.007) | (0.132) |
| Observations | 15544 | 15544 | 15544 | 15544 | 15544 | 15544 | 15544 |
| Distance FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry x Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Occupation x Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm size FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Apprentice supply control | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table assess the robustness of the effect of the free movement policy on recruitment and training costs (panel A of Table 6). Panel A focuses on firms reporting for a (focus) occupation covered in all three survey waves. The estimation is weighted using the surveys' sampling weights capped at 100. Panel B uses all firms and the original (uncapped) sampling weights. Panel C focuses on all firms but does not weight the regression. See the notes of Table 6 for details on the dependent and independent variables. Standard errors are clustered on the level of commuting zones. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table B.13: Effects on recruitment and training cost, by control group

| | (1) Interviews per job | (2) Interview time in hours | (3) Advertisement costs per job | (4) Costs of external consultant | (5) Adaptation period in months | (6) Shortfall in productivity | (7) Direct training costs |
|---|---|---|---|---|---|---|---|
| ***A. Only firms in non-border region as control group*** | | | | | | | |
| $I(d_i \leq 15) \cdot I(t = 2004)$ | 0.106 | 0.782 | 0.123 | 0.039 | -0.164 | -0.011 | -0.180 |
| | (0.062) | (0.457) | (0.226) | (0.164) | (0.235) | (0.009) | (0.261) |
| $I(d_i \leq 15) \cdot I(t = 2009)$ | -0.063 | -0.126 | -0.331 | -0.279* | -0.369** | -0.028* | -0.109 |
| | (0.080) | (0.359) | (0.245) | (0.157) | (0.129) | (0.014) | (0.098) |
| $I(15 < d_i \leq 30) \cdot I(t = 2004)$ | 0.017 | -0.135 | -0.109 | -0.013 | -0.283 | -0.012* | -0.034 |
| | (0.061) | (0.601) | (0.261) | (0.147) | (0.218) | (0.007) | (0.137) |
| $I(15 < d_i \leq 30) \cdot I(t = 2009)$ | -0.049 | -0.588* | -0.339** | -0.150 | -0.306** | -0.026*** | 0.012 |
| | (0.052) | (0.328) | (0.150) | (0.126) | (0.134) | (0.008) | (0.169) |
| Observations | 13026 | 13026 | 13026 | 13026 | 13026 | 13026 | 13026 |
| ***B. Only firms further away than 30 minutes from the border as control group*** | | | | | | | |
| $I(d_i \leq 15) \cdot I(t = 2004)$ | -0.079 | 0.269 | -0.040 | -0.469** | -0.272 | -0.028 | 0.223 |
| | (0.121) | (0.671) | (0.383) | (0.204) | (0.251) | (0.020) | (0.252) |
| $I(d_i \leq 15) \cdot I(t = 2009)$ | -0.054 | -0.195 | -0.048 | -0.370** | -0.259 | -0.023 | 0.157 |
| | (0.086) | (0.476) | (0.333) | (0.144) | (0.213) | (0.014) | (0.190) |
| $I(15 < d_i \leq 30) \cdot I(t = 2004)$ | -0.149 | -0.493 | -0.268 | -0.416** | -0.395 | -0.026 | 0.377** |
| | (0.139) | (0.483) | (0.503) | (0.142) | (0.222) | (0.018) | (0.158) |
| $I(15 < d_i \leq 30) \cdot I(t = 2009)$ | -0.037 | -0.664 | -0.047 | -0.230* | -0.205 | -0.020* | 0.294 |
| | (0.090) | (0.406) | (0.304) | (0.117) | (0.168) | (0.010) | (0.183) |
| Observations | 11158 | 11158 | 11158 | 11158 | 11158 | 11158 | 11158 |
| Distance FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry x Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Occupation x Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm size FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Apprentice supply control | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* The table shows whether the effect of the free movement policy on recruitment and training costs (panel A of Table 6) depends on the control group. The control group in panel A is firms located outside of the border region. The control group in panel B is firms located further away than 30 minutes from the next border crossing. See notes of Table 6 for details on the dependent and independent variables. The estimations are weighted using the surveys' sampling weights capped at 100. Standard errors are clustered on the level of commuting zones. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.