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Train drain? Access to skilled foreign workers and firms' provision of training

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Train drain? Access to skilled foreign workers and firms' provision of training

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Abstract

Does better access to skilled workers reduce firms' willingness to provide general skills training to unskilled workers? We analyze how the gradual 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 skilled workers increased more in firms close to the border because they gained unrestricted access to cross-border workers from neighboring countries. Our Difference-in-Differences estimates suggest that firm-provided training and access to skilled workers are not necessarily substitutes: opening the borders did not have a statistically significant effect on apprenticeship provision. We show theoretically and empirically that the small impact was the consequence of two opposing effects: the greater availability of skilled workers reduced firms' incentive to train because the cost of hiring external labor fell. Positive impacts on firm growth worked in the opposite direction.

Keywords: labor demand; skilled immigration; firm-provided training; apprenticeships; vocational education and training; free movement of workers; cross-border workers; recruitment; immigration policy; labor mobility; hiring costs

JEL: J24, J63, M53

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1 Introduction

Does firms' willingness to provide general skills training depend on the supply of skilled 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 their internal workers. Indeed, 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 If present, such a trade-off would have important implications for policy makers. A case in point is immigration 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 and firms and governments in immigrants' source countries. Such shifts could reinforce a possible brain drain associated with free mobility of labor and may lead to opposition among residents against open borders.²

This paper provides, to the best of our knowledge, the first quasi-experimental evidence whether the local supply of skilled workers affects firms' provision of general training. We exploit an exogenous increase in the availability of skilled workers that resulted from the implementation of the Agreement on the Free Movement of unrestricted access to the Swiss labor market starting from 1999 onward. As shown by [Beerli et al. \(2021\)](#), firms near the border incidentally experienced a larger shock because the free movement policy abolished the pre-existing restrictions on hiring and employing cross-border workers from Switzerland's neighboring countries. Cross-border workers work almost exclusively in regions within 30 minutes' commuting time of the border. Thus, firms close to the border hired substantially more EU workers after the border opening than firms farther away from the border. Following [Beerli et al. \(2021\)](#), we thus study the policy's effects using a transparent

¹In the training model of [Stevens \(1994\)](#), firms train skilled workers to save on hiring costs for skilled workers in the external labor market. Similarly, in the search and matching models of [Shintoyo \(2008, 2010\)](#), the proportion of skilled workers in the unemployment pool is the central (endogenous) determinant of firm training. If skilled labor is abundant, firms prefer external recruitment to training. Existing empirical papers tend to support the notion of a trade-off between external recruitment and training. For instance, [Blatter et al. \(2012, 2016\)](#) document a positive correlation between the costs of recruiting workers from the external labour market and firms' provision of general skills training.

²Denmark, for example, defends its strict immigration policies with the argument that Danish firms should invest more in the training of native workers to cover their hiring needs (cf. [Hermann, 2019](#)).

Differences-in-Differences design that compares changes in firms’ provision of general skills training near the border—the treatment group—with changes in firms’ training farther away. We measure general skills training by looking at firms’ provision of apprenticeships for young adults.³

In the cross-section, apprenticeship provision depends strikingly on the distance to the nearest border crossing. Using data from the Swiss Business Censuses 1995–2008 that provide the employment and geo-coordinates for the universe of establishments in Switzerland, we find that firms located near the border are substantially less likely to train apprentices. In contrast, these firms are more likely to employ cross-border workers—and foreign workers in total—than firms farther away. However, these cross-sectional relationships do not appear to be causal. Our DiD estimates suggest that the free movement policy had a substantial and highly statistically significant positive impact on the employment of foreign workers in firms close to the border in general and among firms that train apprentices in particular. However, analogous effect estimates on the number of apprenticeship positions are statistically indistinguishable from zero, even among highly treated establishments within 15 minutes of travel time to the border. This conclusion holds in various robustness checks, including a matching approach that only compares establishments with similar pre-reform characteristics.

We interpret these results through the lens of a basic dynamic labor-demand model of training originally proposed by [Stevens \(1994\)](#). We extend this model to allow for skilled immigration. Central to firms’ dynamic decisions are the assumptions that firms face exogenous separations from skilled workers and costs for replacing them. Firms thus train unskilled workers today to save on hiring costs for skilled workers tomorrow, in line with empirical evidence that the supply of training increases with hiring costs (e.g., [Blatter et al., 2016](#)). We also assume that firms produce with a constant elasticity of substitution (CES) production function with two labor inputs: skilled immigrant workers and native workers. The CES framework allows the two labor inputs to be imperfect substitutes, a standard assumption in the economics of immigration (e.g., [Ottaviano and Peri, 2012](#)). In this

³Apprenticeships are an upper-secondary level education program combining formal schooling with work-based practical learning in firms ([OECD, 2004](#)). Apprenticeships in Switzerland are largely composed of general (i.e., transferable) skills ([Mueller and Schweri, 2015](#)). Therefore, many studies used the firms’ employment of apprentices to study firms’ willingness to provide general training, including the influential study by [Acemoglu and Pischke \(1998\)](#). See [Wolter and Ryan \(2011\)](#) for an overview of the economic literature on apprenticeship training.

framework, a greater availability of skilled immigrant workers does not necessarily reduce firms' provision of training because there are two countervailing effects. On the one hand, better access to suitable workers lowers the future savings in terms of hiring costs associated with training (*cost effect*). On the other hand, if native and immigrant workers are imperfect substitutes, the additional hiring of skilled foreign workers increases firms' demand for all types of workers through a reduction in wage costs (the *scale effect* familiar from standard labor demand theory).⁴

We then provide empirical evidence that the two mechanisms are at play, and counteract each other, in our setting. To this end, we exploit rich firm-level surveys in 2000, 2004, and 2009 on the costs, benefits, and motives of apprenticeship provision in Switzerland (Schwery et al., 2003; Muehleemann et al., 2007; Strupler and Wolter, 2012). Consistent with the cost effect, the free movement policy reduced firms' willingness to train to attract skilled workers because it is hard to find them elsewhere. Firms also became significantly less likely to train to save hiring costs for external workers. Consistent with the scale effect, the free movement policy had a quantitatively meaningful 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 we do not observe a positive effect on firm growth.

Our study contributes to the literature on immigration and firm-provided training in four 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 of firms.⁵ Our conceptual framework highlights that firms' cost-benefit considerations and production complementarities shape the effects of the supply of skilled workers on firms' provision of training. These findings provide the mirror image to the empirical evidence that companies are discouraged from

⁴An alternative transmission channel is that skilled foreign immigrants increase firms' size by increasing innovation output and productivity, and hence total factor productivity. Indeed, Beerli et al. (2021) provide evidence that this alternative channel may be at work in our empirical setting, too.

⁵Examples of observational studies that analyze the determinants of "make or buy" decisions are Bellmann et al. (2014) and Blatter et al. (2016). However, overview articles on why firms train such as Leuven (2005) and Bassanini et al. (2007) do not touch directly upon these decisions. Training models typically assume that firms are homogeneous in terms of cost structure as well as training and recruitment cost function. Hence, either all firms train or no firm does. Once we relax these restrictive assumptions, firms' optimal recruitment strategies may differ and consist of both hiring externally and supplying some internal training (Wolter and Ryan, 2011).

training because other companies might poach trained workers (e.g., Winkelmann, 1996; Cappelli, 2004; Mohrenweiser and Zwick, 2009; Muehlemann and Wolter, 2011; Mohrenweiser et al., 2019). Conceptually, these papers analyze changes in the number of firms competing for fixed number of workers, while we study changes in the number of skilled workers for an initially fixed number of firms.

Second, our study provides the first quasi-experimental evidence on how firms' willingness to train unskilled workers depends on immigration.⁶ The existing empirical studies on this relationship are observational and reach conflicting conclusions. The reported estimates suggest 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.⁷ Our main contribution relative to these papers is that we exploit quasi-experimental variation in the availability of skilled workers to identify the causal impact of better access to skilled foreign workers on training. We also contribute to this literature by providing direct evidence on two potential mechanisms underlying the effects of better access to skilled foreign workers on training provision. Our simple theoretical framework complements the model proposed by Mountford and Wadsworth (2019) by focusing on the within-firm effects of immigration on training. The model of Mountford and Wadsworth (2019) instead focuses on the between-firm (reallocation) effects.

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). Using Swiss business census data, they argue that the employment of cross-border workers significantly replaces firms' provision of apprenticeships. The difference in results relative to ours is due to the differences in research design. Aepli and Kuhn (2021) estimate the effect of cross-border workers on training by instrumenting firms' employment of cross-border workers with an establishment's distance from the border. The exclusion restriction of this instrumental variable strategy states that distance from the border affects firms'

⁶There is a larger literature that analyzes the effects of immigration on the educational choices of residents. Two recent examples are Bächli and Tsankova (2020) and Brunello et al. (2020).

⁷Using Australian data, Baker and Wooden (1992) report a negative correlation between immigration and in-house training. This association, however, disappears once the authors control for industry fixed effects. Mountford and Wadsworth (2019) find a negative association between immigration and the share of UK-born workers that receive on-the-job training in sector times occupation cells. Finally, Campo et al. (2018) find a positive impact of skilled immigration on on- and off-the-job training acquired by workers in the UK. Their identification strategy relies on a standard spatial shift-share instrumental variable approach following Card (2001). See Jaeger et al. (2018) for a critical assessment of this strategy.

provision of training *only* through the greater supply of cross-border workers near the border. Our DiD approach, which exploits quasi-experimental variation in the availability of cross-border workers, suggests that part of the differences in training behavior between firms close and farther away from the border are unrelated to the supply of cross-border workers.

Third, by focusing on firms’ provision of training, our study also adds a novel angle to recent studies looking at the effects of immigration on wages and employment (Mansour, 2010; Dustmann et al., 2017) and on firms (Beerli et al., 2021) by exploiting changes in commuting policies. 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. This approach contrasts the traditional shift-share instrumental variable approach to isolate supply-driven variation in regional immigration flows, which hinges on the assumption that historical immigrant concentrations across regions are uncorrelated with the current regional distribution of labor-demand shocks. This assumption is not always warranted (Jaeger et al., 2018). Another difference is that possible consumption-side effects of immigration (e.g., on demand for local housing) are muted in the case of commuting policies because cross-border workers do not relocate to the country they work. While this absence of reallocation likely increases the scope for displacement effects in the labor market, it also makes it easier to isolate the cost and scale effects highlighted by our demand-side framework.

Fourth, by linking our empirical results to the introduction of the free movement policy, our study has important implications for policy makers on the potential societal costs of training opportunities for young adults in the receiving country—particularly given growing opposition to the free movement of workers in several European countries. In Switzerland, with widespread fears that immigration may harm the economic opportunities of citizens, the majority of Swiss citizens voted for a 2014 referendum—the “stop mass migration” initiative—which was intended to restrict the free movement of persons. One argument in the pre-voting debate was that open borders incentivize firms to hire cheap labor from abroad (Blocher, 2011), thereby undermining firms’ willingness to invest in the training of unskilled citizens and reinforcing the brain drain caused by skilled migration.

This study is organized as follows. Section 2 presents a theoretical framework that guides our empirical analyses. Section 3 provides the institutional background on the reform process of the free movement policy 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 Theoretical framework

Our theoretical framework is based on a dynamic labor-demand model with adjustment costs augmented with firms' investment in training (Stevens, 1994). In the model, firms can recruit skilled workers either by hiring them externally tomorrow at certain hiring costs or by training unskilled workers internally today at certain training costs. The model formalizes the investment approach to firm-provided training (Oatey, 1970; Lindley, 1975; Merrilees, 1983). We extend the original model of Stevens (1994) to study the effects of a greater availability of skilled immigrant workers. We omit the discount rate because it is not affected by immigration, and we focus on only two periods $t \in \{0, 1\}$. These simplifications make the exposition easier.

The model has three types of workers: unskilled native workers T_t , skilled native workers N_t , and skilled immigrant workers I_t . Firms hire skilled workers $L_t = N_t + I_t$ in both periods, and they can choose to hire unskilled workers (T_t) in the first period to train them and thus have skilled workers for the second period. There are constant and exogenous voluntary quit rates for trained unskilled workers (γ) and skilled workers (δ). Firms' skilled employment in the second period, L_1 , therefore consists of the skilled workers remaining from the first period, the remaining trained unskilled workers from the first period T_0 (newly trained skilled workers), and skilled workers (X_1) newly hired from the external labor market:

$$L_1 = N_1 + I_1 \tag{1a}$$

$$\text{with } N_1 = (1 - \delta)N_0 + (1 - \gamma)T_0 + X_1^N \tag{1b}$$

$$\text{and } I_1 = (1 - \delta)I_0 + X_1^I \tag{1c}$$

Following Stevens (1994), we assume that unskilled native workers do not produce output in the training period. Thus, the firm's revenue function depends only on skilled labor ($R_t[L_t]$ with $R' > 0, R'' \leq 0$). The price is set to unity. The revenue function hence equals the production function. In line with the previous literature,

skilled native and skilled immigrant workers may be imperfect substitutes.⁸ Thus, we assume that firms produce with a constant elasticity of substitution (CES) production function with inputs I_t and N_t . The degree of imperfect substitutability between the two inputs is governed by the elasticity of substitution $\sigma = 1/(1 - \rho)$. The two labor inputs are perfect substitutes if $\sigma = \infty$. The production function also contains a parameter measuring total factor productivity (A_t) and a productivity parameter measuring the relative efficiency of skilled immigrant and native workers (α):

$$Y_t = R_t[N_t, I_t] = A_t(\alpha N_t^\rho + (1 - \alpha)I_t^\rho)^{\frac{1}{\rho}} \quad (2)$$

Firms face costs for hiring skilled workers and net costs for training unskilled workers. Both net training costs and hiring costs are quadratic, a mathematical requirement for a closed-form solution. Following [Stevens \(1994\)](#), we assume that the net training costs depend on the number of unskilled workers in training: $C_t[T_t] = \frac{1}{2}c_t T_t^2$. Net training costs are positive $c_t \geq 0$, which implies that training is never profitable per se.⁹ As is plausible in the empirical context studied—where professional associations provide wage recommendations for trainees from which few firms deviate strongly—trainees’ compensation is set exogenously and is part of the training costs.

The hiring costs are increasing in the number of hires of skilled workers X_t . We also assume that they decrease in the total supply of skilled labor in the labor market $L^s = L^N + L^I$. The assumption reflects that it is easier and faster to find workers with the required skills if the supply of skilled labor is larger, which lowers their hiring costs (as in [Chassamboulli and Peri, 2020](#)). Total hiring costs are thus expressed as $B_t[X_t] = \frac{1}{2}b_t[L^s]X_t^2$, where $b_t \geq 0$ and $b'_t[L^s] < 0$. In support of these assumptions, [Blatter et al. \(2012\)](#), [Blatter et al. \(2016\)](#), and [Muehleemann and Strupler Leiser \(2018\)](#) show that hiring costs for skilled workers in our context are substantial, convex, and increasing in labor market tightness.

⁸[Ottaviano and Peri \(2012\)](#) show that both native and immigrant workers with similar observable skills in the U.S. are imperfect substitutes. [Gerfin and Kaiser \(2010\)](#) provide similar results for Switzerland.

⁹This assumption rules out situations in which firms use training as a means of producing their output more cheaply (the so-called production approach to training, [Lindley, 1975](#)). However, the assumption accommodates situations where unskilled workers in training make up for a certain fraction of their training costs near the end of the training period. Studies on the costs and benefits of apprenticeship training in Switzerland suggest that investment and production motives are both important determinants of whether a firm participates in training apprentices ([Strupler and Wolter, 2012](#)).

In the first period, firms gain revenue from the production of their skilled workers, pay wages to these workers, incur training costs for unskilled workers, and have hiring costs for new skilled workers. Wages of both types of skilled workers, w_N and w_I , are set competitively. We assume that hiring costs are the same for skilled native and skilled immigrant workers, or that possible differences in hiring costs between the two skilled labor inputs are fully compensated by differences in wage levels. In the second period, firms incur hiring costs if they hire skilled workers. The profit-maximizing firms operate on a competitive product market. Thus, the profit optimization problem is:

$$\begin{aligned} \max_{T_0, N_0, I_0} & A_0(\alpha N_0^\rho + (1 - \alpha)I_0^\rho)^{\frac{1}{\rho}} - w_0^N N_0 - w_0^I I_0 - \frac{1}{2}c_0 T_0^2 - \frac{1}{2}b_0[L^s](N_0 + I_0)^2 \\ & + A_1(\alpha N_1^\rho + (1 - \alpha)I_1^\rho)^{\frac{1}{\rho}} - w_1^N N_1 - w_1^I I_1 - \frac{1}{2}b_1[L^s]X_1^2 \end{aligned} \quad (3)$$

where, in the first period, all skilled workers are new hires (i.e., $X_0 = L_0$). In the second period, the newly hired skilled workers fill the gap between the required skilled workers and the remaining skilled workers from the first period, together with the trained unskilled workers from the first period: $X_1 = (N_1 + I_1) - (1 - \delta)(N_0 + I_0) - (1 - \gamma)T_0$.

We solve the simple model in appendix A. Combining the first-order conditions for skilled native workers, skilled immigrant workers, and trainees shows that firms train unskilled workers to cover a fraction of the additional skilled workers needed in the future. In general, firms' training depends on the current and future employment of native and immigrant workers and the relative wages between skilled immigrant and skilled native workers. It also depends on the training costs relative to the hiring costs: the greater the training costs relative to the hiring costs, the larger the number of trainees. Firms do not train when the hiring costs approach zero. Conversely, when training costs are zero, firms train all additional skilled workers needed in period 1.

In this model, the exogenous variables affect the decision to train workers in period 0 as follows (see [Stevens, 1994](#), for an extended discussion):

$$T_0 = T\left(b_1[L^I + L^N]^+, c_0^-, \gamma^-, \delta^+ \alpha^-, \frac{w_0^N(L^I, L^N)^+}{w_0^I(L^I, L^N)}\right) \quad (4)$$

Therefore, the model suggests that a market-level increase in the supply of

skilled immigrant workers—an increase in L^I —has two opposing effects on firms’ willingness to train. On the one hand, it makes hiring additional skilled workers cheaper in period 1. This effect reduces the firms’ willingness to train in period 0 (cost effect). The size of this effect depends on the extent to which the greater supply of skilled immigrant workers affects hiring costs. On the other hand, an increase in L^I shifts the market-level labor supply curve of immigrants to the right, leading to a decrease in w_0^I .¹⁰ The reduction in wage costs induces firms to hire more workers overall and incentivizes them to train more unskilled workers. This scale effect depends on the degree of complementarity between skilled immigrant and skilled native workers (i.e., σ).

In sum, the simple framework predicts that if skilled native and skilled immigrant workers are perfect substitutes ($\sigma = \infty$), an increase in the availability of skilled immigrant workers decreases the firms’ willingness to train because of a reduction in future hiring costs. If skilled immigrants and natives are imperfect substitutes, the effect is unclear and depends on the strength of the cost and scale effects.

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 so-called 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

¹⁰We leave this channel implicit in the model. One approach to model it explicitly is to postulate aggregate labor supply schedules for skilled native and skilled immigrant workers that are increasing in the respective market-level wage. At the labor market level, a greater supply of skilled immigrant workers leads to a reduction in their relative wage (see, e.g., [Borjas, 2003](#)).

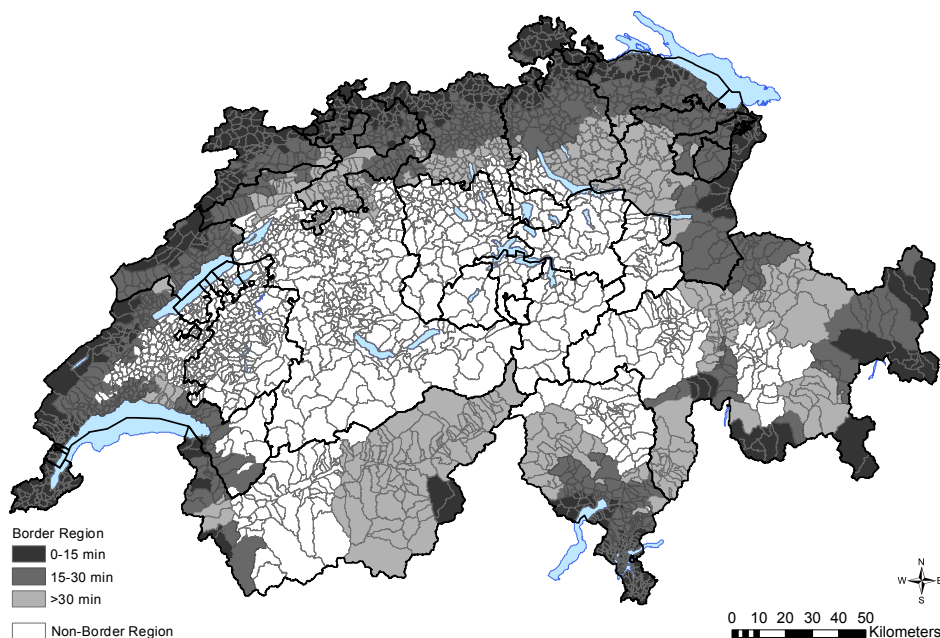
cross-border worker. This priority requirement was enforced through a government-controlled admission process. Firms had to prepare an application detailing the job requirements, the contract and working conditions, and had to demonstrate that they had searched unsuccessfully in Switzerland for a certain 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 legal 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 job mobility within Switzerland. The second legal 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.

Together, lifting these restrictions caused an unprecedented growth in the employment of cross-border workers. Likely because of natural limits in workers’ willingness to commute, this growth was concentrated in Swiss firms within approximately 30 minutes of travel distance to the border. As we show in Tables C.1 and C.2 in the appendix, many of the new cross-border workers had a tertiary degree,

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.

took up jobs in skilled occupations, and, in terms of economic sectors, began to work in manufacturing, IT, R&D, business services, real estate, and in the health sector. As a consequence, certain occupations with a high share of apprentices in employment experienced significant increases in the employment share of cross-border workers (see Appendix Figure C.1). Examples include clerical occupations, machine operators, occupations involving medical and social tasks, and occupations in the construction and retail sectors. Apprenticeship graduates were thus exposed to the free movement policy despite the high formal qualifications of the new cross-border workers.

We analyze the effects of the policy by following Beerli et al. (2021)'s transparent Difference-in-Differences approach. We assign firms in Switzerland to one of four groups: Firms are *highly treated* if they are located within 15 minutes of the border within the “border region”, *slightly treated* if they are located 15–30 minutes of the border in the border region, and *control* firms if they are either located within *or* outside of the border region and located more than 30 minutes from the border.

Since neither control group is clearly more similar to the highly treated group in terms of observables, we generally pool the two control groups. We also show the most important results for the two possible control groups separately. This strategy does not leverage the 2007 abolition of the border region because the 2007 liberalizations affected very few firms.¹¹

Three further comments on the institutional setting are noteworthy. First, we allow for anticipation effects after the policy’s approval by the Swiss parliament. Indeed, Beerli et al. (2021) find 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. Second, we focus on the reduced-form effects of the policy throughout because we view it as an exogenous increase *in the availability of cross-border workers*. Hence, we do not present instrumental variable estimates that use the policy to instrument for the cross-border worker or immigrant share in a region. The reason is that the policy likely had effects on firms beyond those mediated through the increased employment of foreigners only.¹² Third, although the free movement policy allowed *Swiss* cross-border workers to work in Switzerland’s neighboring countries, we argue that our estimates 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.¹³ For the same reason, we view it as unlikely that the policy affected training decisions of Swiss firms because it increased the fear that foreign firms may poach the trained workers.

¹¹Almost 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 2 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.

¹²For 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 the instrumental variable strategy.

¹³Both 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).

3.2 Vocational education and training in Switzerland

In Switzerland, vocational education and training (VET) is the largest program at the upper-secondary education level. Every year, about two thirds of a cohort ending compulsory education start a VET track. Nine out of ten VET students take a dual VET track, where they spend about one to two days at a vocational school, obtaining formal education, and three to four days at a training firm, where they learn practical skills and acquire work experience. VET differs from active labor market programs or on-the-job training because it is better integrated into the educational system and more regulated. The federal act on vocational and professional education and training mandates the involvement of the federal government, professional organizations, and cantons and defines their roles (SERI, 2020).

Firms decide on their own whether to participate in training apprentices—providing apprenticeships is neither mandatory nor subsidized (Wolter et al., 2006).¹⁴ The programs last between three and four years. In this period, students follow a structured national curriculum, culminating in 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, trained apprentices are typically considered skilled workers (Mueller and Schweri, 2015). 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.

4 Empirical strategy

4.1 Data

Our empirical analyses are based on an establishment-level and a firm-level dataset. The first source is the Swiss business censuses from 1995, 1998, 2001, 2005, and 2008. The censuses provide information on the total employment and workforce composition, including the number of apprentices, of all private and public establishments in Switzerland in September. The censuses also contain the precise geo-coordinates

¹⁴As 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.

of each establishment. We use this information to compute the travel time by car to the nearest border crossing.¹⁵ We assign each establishment to its 1998 location throughout the whole sample to avoid biases from endogenous relocation of businesses.

We impose three sample restrictions: we drop establishments from the agricultural sector, which 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).

Our second dataset is three waves of an administrative and representative firm-level survey on the costs and benefits of apprenticeship training in Switzerland. These cross-sectional surveys were conducted in 2000, 2004, and 2009. They contain unusually detailed information on firms’ motives for training apprentices. We drop firms reporting in training occupations that are either available only in 2004 and 2009 or unavailable for non-training firms.¹⁶ We also drop observations if we are unsure about their assignment to the border or non-border region. We end up with 5,231 observations for non-training firms and 3,837 observations for training firms.

An important disadvantage of the cost-benefit surveys is that we cannot link firms across surveys. An important advantage is that they provide direct evidence of whether firms trade off apprenticeship training against hiring skilled workers externally. Firms were asked to assess whether they train to attract skilled workers, save hiring costs for external workers, save adjustment costs, or secure a talent pipeline in the sector/region. Table C.3 in the appendix provides the list of training motives levied in the surveys and an English translation of the exact survey questions. A nice feature of these data is that firms that did not train were asked to answer these questions in a hypothetical fashion, too. If firms train apprentices in several occupations, the surveys asked firms to answer the questions with reference

¹⁵We 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.

¹⁶The occupations are auto mechanic, carpenter, dental assistant, electronics technician, health specialist, logistics, medical practice assistant, painter, plumber, other three-year VET programs, and other four-year VET programs (in total, 3,663 observations).

to a chosen training occupation.

Table 1 provides descriptive statistics for the two datasets and compares the pre-reform characteristics of establishments in the four regions that we will compare in our DiD estimation: establishments within the border region up to 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 establishments in the highly treated region are somewhat larger than those in the two control groups. 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 to which we return later 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 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. But we also observe some relevant pre-treatment differences across regions. For instance, highly treated establishments employ more foreigners and are more likely to be exporters and importers. Given the 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 highly treated establishments in terms of these and other pre-treatment characteristics.

4.2 Regression model

We estimate the effect of the greater availability of skilled workers on firms' training behavior using the Difference-in-Differences (DiD) design by Beerli et al. (2021). The approach leverages the larger impact of lifting restrictions for cross-border workers on establishments near the border. We assign firms to one of three groups:

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

<i>Travel time to border</i>	Border region						Non-border region	
	≤ 15 min		15–30 min		>30 min			
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
A. Business census (all)								
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)
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 only)								
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)	0.19	(0.39)
Number of apprentices	0.34	(1.56)	0.41	(2.14)	0.46	(1.64)	0.44	(1.42)
Apprentice share	0.03	(0.15)	0.03	(0.12)	0.05	(0.13)	0.05	(0.15)
Attract skilled workers	3.82	(1.24)	3.86	(1.18)	3.75	(1.19)	3.89	(1.16)
Hiring cost	2.69	(1.15)	2.62	(1.12)	2.50	(1.13)	2.56	(1.12)
Risk of wrong decision	3.01	(1.22)	2.87	(1.20)	2.78	(1.21)	2.83	(1.17)
Replace unskilled workers	2.62	(1.17)	2.69	(1.22)	2.59	(1.24)	2.54	(1.17)
Adjustment cost	2.84	(1.25)	2.79	(1.22)	2.74	(1.19)	2.77	(1.15)
Hire the best	3.19	(1.24)	3.14	(1.24)	3.04	(1.26)	3.07	(1.19)
Avoid fluctuation	3.25	(1.23)	3.20	(1.16)	3.05	(1.20)	3.06	(1.16)
Qualify junior staff	3.84	(1.15)	3.78	(1.10)	3.67	(1.14)	3.71	(1.11)
Secure skilled workers	3.72	(1.23)	3.81	(1.23)	3.80	(1.21)	3.87	(1.17)
Firm size	16.90	(57.41)	15.81	(60.62)	11.66	(43.85)	12.95	(38.84)
Observations	725		1074		560		951	

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. Panel B focuses on establishments that train apprentices. Panel C shows descriptive statistics using the cost-benefit data in 2000. The training motives are measured on a 5-point Likert scale. The table shows the mean of the ordinal variable. Table C.3 provides the list of training motives levied in the surveys and an English translation of the exact survey questions.

establishments located within 15 minutes travel time to the nearest border crossing d_i within the border region, $I(d_i \leq 15)$, establishments located 15 to 30 minutes of the border within the border region, $I(15 < d_i \leq 30)$, and establishments more than 30 minutes of the border either within or outside the border region. We then interact these indicators with indicators of the survey years t . For the business censuses that took place in 1995, 1998, 2001, 2005, and 2008, we estimate variants of the following DiD model:

$$\begin{aligned}
y_{i,t} = & \beta_{high}^{t=2001} [I(d_i \leq 15) * I(t = 2001)] + \beta_{high}^{t \geq 2005} [I(d_i \leq 15) * I(t \geq 2005)] + \\
& \beta_{slight}^{t=2001} [I(15 < d_i \leq 30) * I(t = 2001)] + \beta_{slight}^{t \geq 2005} [I(15 < d_i \leq 30) * I(t \geq 2005)] \\
& + \alpha_i + \alpha_t + \epsilon_{i,t}
\end{aligned} \tag{5}$$

In this model, $y_{i,t}$, represents the training or employment outcome of firm i in year t . The β coefficients capture the DiD of this outcome for highly treated firms (β_{high}) and slightly treated (β_{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. The model controls for year fixed effects (α_t), which capture aggregate macroeconomic shocks common to all firms such as changes in aggregate prices and foreign demand, and establishments fixed effects (α_i), which control for the baseline effects of establishments' distance to the border (d_i) and other pre-existing differences between establishments and regions.

The central identifying assumption in our research design is that establishments in the three regions would have, on average, had the same *change* in 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.

While we cannot test the validity of the common trend assumption, we can assess its plausibility by assessing pre-treatment trends in outcomes. Thus, we also estimate an event study version of equation 5 that contains year-specific effects for

each census year except 1998 that 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 datasets 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, 2020; Bächli and Tsankova, 2020; Cristelli and Lissoni, 2020; Naguib, 2019), present evidence of common pre-trends for a variety of firm outcomes in our setting, including firm size, productivity, wages, worker composition, innovation, and patents, in some cases with data that goes back until the 1980s. In addition, section 5.4 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 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 skilled workers on the establishments' willingness to train unskilled workers.

5.1 Descriptive evidence

Figure 2 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 information on 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 border region establishments within 15 minutes to the border. The figure also shows that the growth in the employment share of cross-border workers between 1995 and 2008

is substantial and concentrated close to the border, too.

Panel (b) of Figure 2 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.

Taken together, Figure 2 shows that employment of cross-border workers and apprenticeship training are inversely related in the cross-section, suggesting that the two are substitutes. [Aepli and Kuhn \(2021\)](#) argue based on this cross-sectional relationship that cross-border workers displace apprentices. However, we do not observe a strong relationship between distance to the border and the over-time *changes* in the training and apprentice share in panels b) and c) of Figure 2 although there is a simultaneous growth in cross-border employment close to the border. It may thus be that other differences between firms near and farther from the border explain why firms near the border train less. 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](#)). We thus now present DiD estimates that account for time-invariant differences between firms close and farther from the border. They focus solely on changes in incumbent firms' propensity to train as a response to a large, exogenous increase in the availability of cross-border workers.

5.2 Effect on the employment of foreign workers

Using data from the business census 1995–2008, Figure 3 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 (5) 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

Figure 2: Employment of cross-border workers and apprentices by region



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, termed "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.

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). Finally, we weight the regression by firm

size prior to the policy change.¹⁷ We discuss how these specification choices affect our results in section 5.4.

Figure 3 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 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 similar trends in foreign workers' employment in the treatment and control groups in this period.

Panel A of Table 2 uses the same outcome but the simpler DiD model (equation (5)) to provide two additional insights. The main coefficient of interest is the interaction term $I(t \geq 2005) \cdot I(d_i \leq 15)$ that captures the policy effects on highly treated establishments after cross-border workers had unrestricted access to the border region. 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 also pertains to establishments that train apprentices. Estimating similar regression models by educational attainment and occupation, Beerli et al. (2021) show that these additional hirings of foreigners primarily consisted of *skilled*, tertiary-educated workers, consistent with the descriptive patterns in Appendix Table C.2.

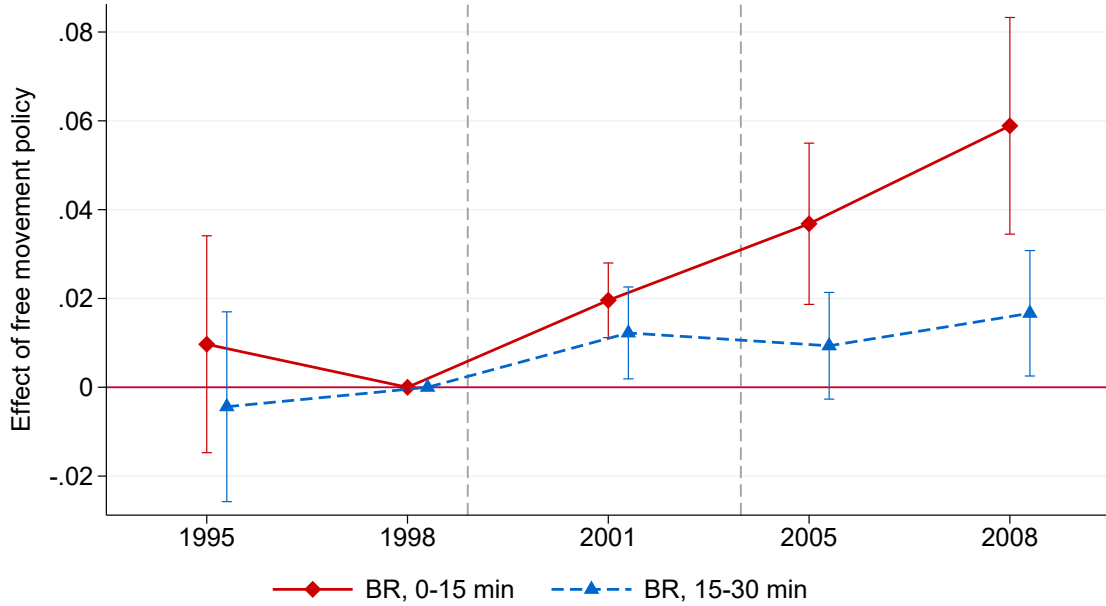
¹⁷Initial firm size is a firm's average full-time equivalent employment in the waves 1995 and 1998 of the census. In estimations that incorporate firms founded after 1998, we use a firm's size in the first census wave that the firm appears.

Table 2: 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 (0.008)	0.015** (0.006)	0.014* (0.008)	0.022*** (0.006)	0.021*** (0.007)	0.012 (0.007)
$I(t = 2001) \cdot I(15 < d_i \leq 30)$	0.007 (0.005)	0.014** (0.006)	0.015*** (0.005)	0.017* (0.009)	0.020*** (0.006)	0.012 (0.007)
$I(t \geq 2005) \cdot I(d_i \leq 15)$	0.031*** (0.011)	0.043*** (0.009)	0.041*** (0.010)	0.045*** (0.011)	0.046*** (0.011)	0.042*** (0.010)
$I(t \geq 2005) \cdot I(15 < d_i \leq 30)$	0.012* (0.007)	0.015** (0.007)	0.015** (0.006)	0.016* (0.010)	0.018** (0.009)	0.014* (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 (0.002)	-0.001 (0.002)	-0.000 (0.002)	-0.000 (0.003)	0.000 (0.002)	-0.001 (0.002)
$I(t = 2001) \cdot I(15 < d_i \leq 30)$	0.001 (0.001)	-0.000 (0.002)	-0.001 (0.002)	-0.000 (0.002)	0.001 (0.001)	-0.001 (0.002)
$I(t \geq 2005) \cdot I(d_i \leq 15)$	-0.005* (0.002)	-0.004 (0.003)	-0.002 (0.002)	-0.005 (0.004)	-0.003 (0.003)	-0.004 (0.003)
$I(t \geq 2005) \cdot I(15 < d_i \leq 30)$	-0.000 (0.002)	-0.000 (0.002)	-0.000 (0.002)	-0.000 (0.003)	0.001 (0.003)	-0.001 (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 (0.002)	-0.006 (0.004)	-0.006* (0.003)	-0.006 (0.009)	-0.006 (0.004)	-0.006* (0.004)
$I(t = 2001) \cdot I(15 < d_i \leq 30)$	0.003 (0.002)	0.002 (0.002)	0.002 (0.002)	0.007 (0.006)	0.002 (0.003)	0.002 (0.002)
$I(t \geq 2005) \cdot I(d_i \leq 15)$	-0.006 (0.005)	-0.007 (0.006)	-0.006 (0.005)	-0.007 (0.014)	-0.007 (0.006)	-0.007 (0.006)
$I(t \geq 2005) \cdot I(15 < d_i \leq 30)$	0.003 (0.003)	0.004 (0.003)	0.004 (0.003)	0.014* (0.007)	0.004 (0.003)	0.004 (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 (5)). 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 of 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 to 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.

Figure 3: Timing of effect of free movement policy on foreign employment



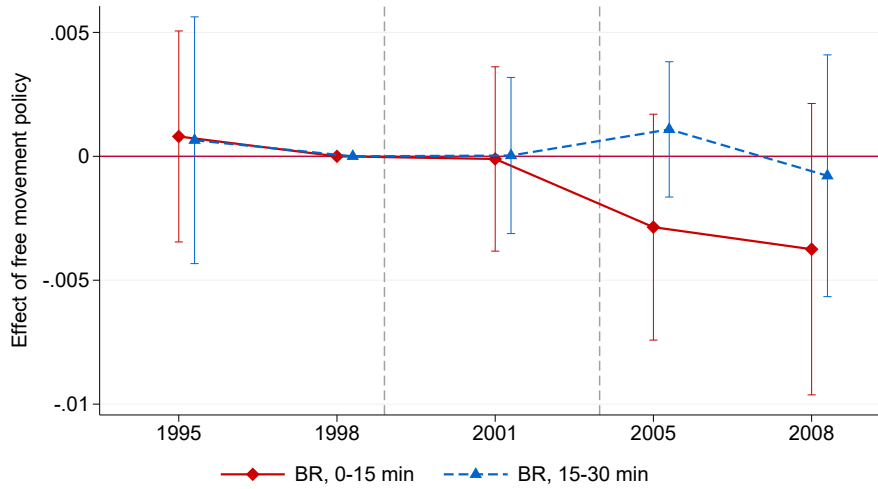
Notes: The figure shows the effect of the free movement policy on establishments' employment of foreign workers using data from the Swiss business censuses 1995–2008. It plots the estimated policy effects and associated 95% confidence intervals using a generalization of our main DiD model (equation (5)) 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 of 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.

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 it 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 2. Figure 4 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 specification of 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 4 does not suggest that the greater availability of skilled foreign workers affected firms' provision of apprenticeships. Relative to the two control groups, apprenticeships began to decline in highly treated establishments after 1998. By 2008, the negative point estimate amounts to 0.4 percent

Figure 4: 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 (5)) 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 of 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.

of total 1998 full-time equivalent 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 the relative decline in apprenticeship training 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 2, confirm that the free movement policy had limited effects on the employment of apprentices. The exception is column 1. The coefficient for highly treated establishment, $I(t \geq 2005) \cdot I(d_i \leq 15)$, is statistically significantly negative at the 10 percent significance level in this specification. 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

¹⁸Beerli 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

specifications in the table produce estimates that are close to zero and statistically insignificant. For instance, we get similar estimates if we absorb possible trend differences in the local supply of apprentices by adding a control variable that reflects the (potential) supply of apprentices in the local labor market.¹⁹ Columns 5 and 6 show that the findings do not depend on the control group either. Finally, there is no evidence that the policy affected the extensive margin—the probability that firms provide at least one training position. These results are shown in panel C of Table 2.²⁰ The dependent variable of these unweighted regressions is a dummy equal to one if an establishment engages in apprenticeship training..

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 5 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%, or approximately 1200 positions in total.²¹

Do these aggregate estimates hide heterogeneity between industries? Figure 6 shows separate estimates of our main event study model for broad industries. The figure illustrates that the negative point estimate of our aggregate model is primarily the result of highly treated establishments in the construction and service sectors. Indeed, we find statistically significant evidence at the 5 percent level that the free movement policy reduced apprenticeship provision in the construction sector. There is no statistically significant evidence for a displacement of apprenticeships in the

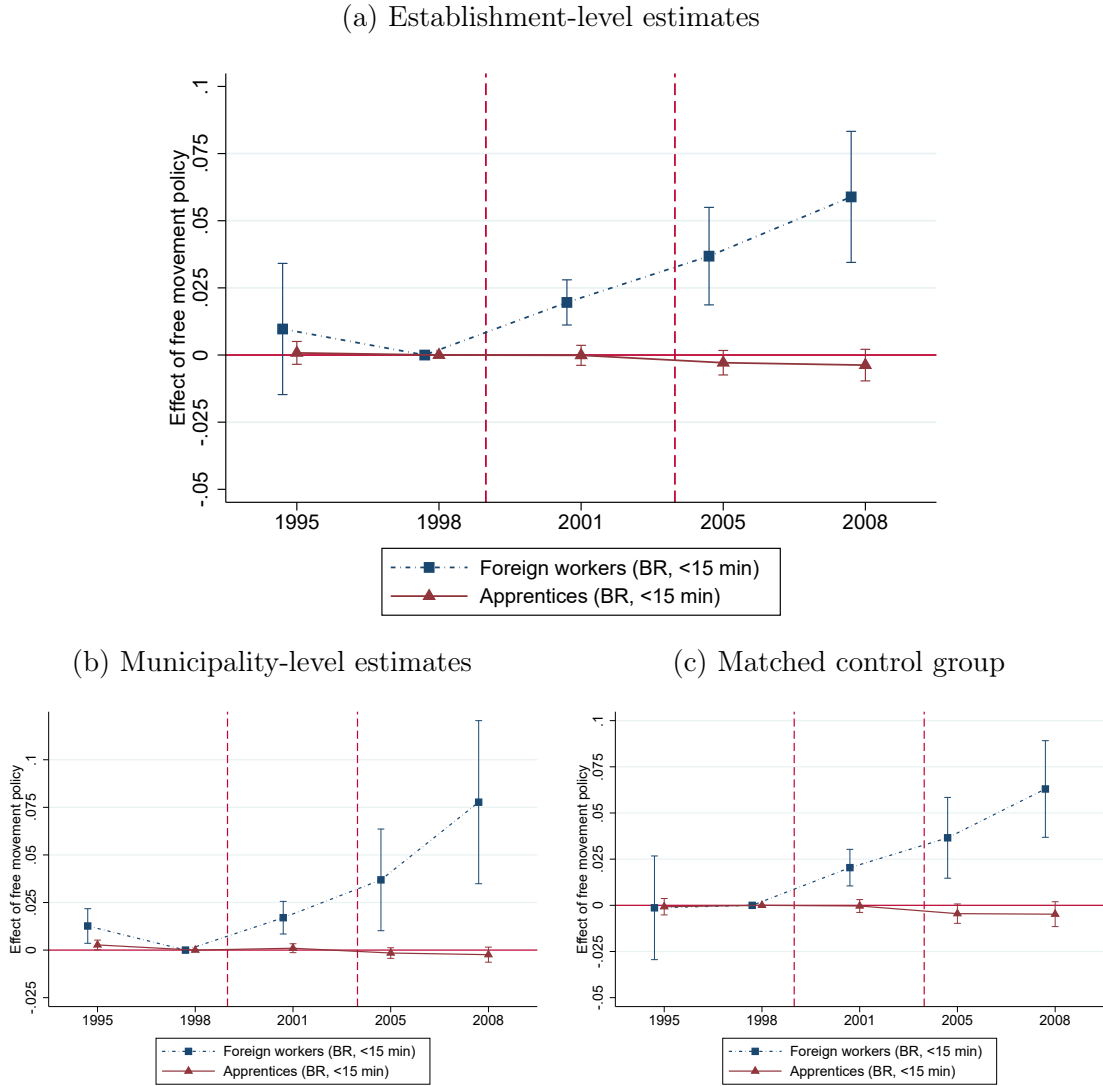
reduces the percentage of firms that train in the highly treated region. This effect might lead us to overstate a possible negative effect of the free movement policy on the training of apprentices.

¹⁹The control variable measures the number of graduates from lower secondary schools (11th grade) in the commuting zone (NUTS-III region) in a given year.

²⁰Indeed, 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).

²¹Aepli 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.

Figure 5: Effect of free movement policy on apprenticeship training: Comparison of effect size

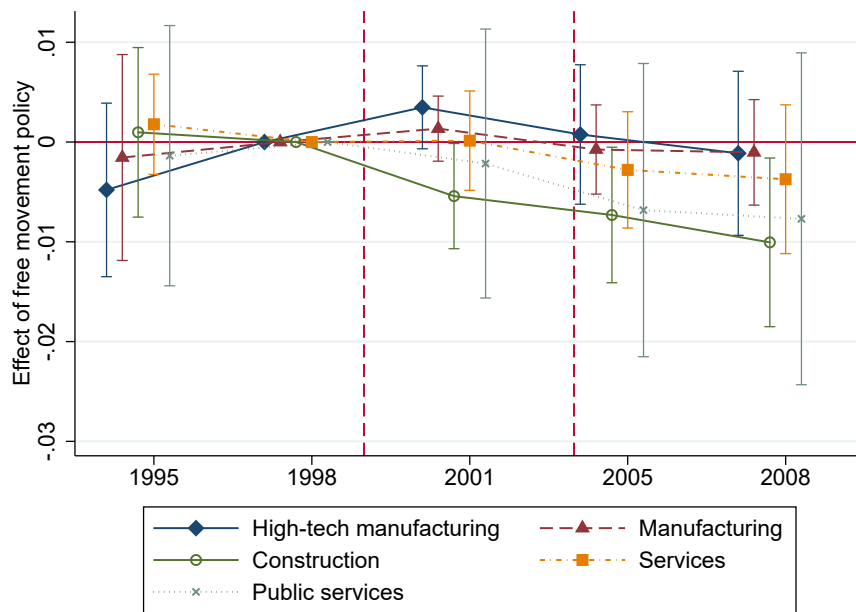


Notes: The figure shows the effect of the free movement policy on employment of foreign workers and the provision of apprenticeships in highly treated units using data from the Swiss business censuses 1995–2008. It plots the estimated policy effects and associated 95% confidence intervals using a generalization of our main DiD model (equation (5)) 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 B 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.

three other sectors. In all sectors, highly treated and control establishments display parallel pre-trends in the number of apprentices.

Overall, our evidence suggests that the greater availability of cross-border workers did not reduce firms' willingness to provide apprenticeships. We reach a similar conclusion if we analyze firms' training provision in the cost-benefit surveys.²² An exception is the construction sector where apprenticeship training and access to skilled foreign workers appear to be substitutes. Importantly, the point estimates also rule out (large) positive effects on the number of apprentices: apprentices do not appear to have profited from the free movement policy in contrast to tertiary-qualified workers, which experienced wage and possibly employment growth because of the policy (see Beerli et al., 2021).

Figure 6: Effect of free movement policy on apprenticeship training, by broad sector



Notes: The figure shows the effect of the free movement policy on the establishments' provision of apprenticeships estimated separately by broad economic sector of activity. We use 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 (5)) that estimates separate effects for each census year. The DiD estimates based on equation (5) are presented in panel C of Table 4. We standardize the effects to 0 in 1998 by dropping the indicator for that year from the regression. The estimation sample comprises of all establishments existing throughout 1995–2008. The dependent variable of each regression is the number of apprentices relative to 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.

²²Indeed, the point estimates presented in appendix table C.5 suggest that the free movement policy if anything increased firms' provision of apprenticeship training.

5.4 Robustness

This section assesses how robust these baseline results are. Table 3 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)²³, 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)²⁴, if we use the somewhat smaller estimation sample and the weighting scheme that Beerli et al. (2021) use (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 2) are close to the average of the estimates in Tables 2 and 3.

Since we normalize our main outcome variables with firm sine in 1998, all regressions presented so far only use firms that exist in 1998. Our preferred specification is additionally restricted to firms existing throughout 1995–2008. We probe the

²³The robustness to including industry-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 included health, social, art, and agriculture and forestry occupations in the VET system from 2004 onwards (BBT, 2003). Although this reform touched all regions in Switzerland at the same time, it could have region-specific effects because of differences in the occupational composition across regions. Since the reform affected occupations concentrated in certain industries, the industry-year fixed effects likely control for a large part of the possible reform effects. To further assess the robustness of our results to this major reform, we use the cost-benefit data and exclude all training occupations that are directly affected by the reform. Table C.6 thus disregards carpenters, computer scientists, electricians, retail specialists, and salesman. The results are similar to our baseline results (Table C.5) with this restriction.

²⁴The 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 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 3 is restricted to non-affected two-digit industries.

relevance of these restrictions in Panel (b) of Figure 5 by aggregating employment of *all* establishments in the census to the municipality level. We then rerun our DiD model on this level. Reassuringly, the resulting estimates are very similar to our baseline estimates. As we show in Appendix Table C.4, the municipality-level regressions also provide no evidence that the free movement policy affected the number and the share of establishments that train apprentices.

An important further 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 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 achieve these goals (see appendix B 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. 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 lose 133 highly treated establishments without an exact match. Panel (c) of Figure 5 presents the results. We observe that the matching estimates confirm our baseline results, presented in panel (a), in terms of sign and size of the effects.

5.5 Mechanisms

The previous sections suggest that a greater availability of skilled foreign workers caused a strong growth in the number of foreign workers but had no or at most a small negative impact on firms' provision of apprenticeship training. Our theoretical

Table 3: 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* (0.006)	0.017** (0.007)	0.024*** (0.009)	0.021** (0.009)	0.032*** (0.006)	0.010*** (0.003)	0.016* (0.009)
$I(t = 2001) \cdot I(15 < d_i \leq 30)$	0.014*** (0.005)	0.020*** (0.005)	0.016*** (0.004)	0.021* (0.011)	0.024*** (0.007)	0.006** (0.003)	0.017** (0.008)
$I(t \geq 2005) \cdot I(d_i \leq 15)$	0.035*** (0.007)	0.038*** (0.008)	0.033** (0.014)	0.039*** (0.010)	0.079*** (0.017)	0.045*** (0.011)	0.045*** (0.010)
$I(t \geq 2005) \cdot I(15 < d_i \leq 30)$	0.015*** (0.005)	0.023*** (0.006)	0.019*** (0.006)	0.012 (0.012)	0.032*** (0.010)	0.014*** (0.003)	0.018** (0.008)
B. Apprentices / total FTE employment in 1998							
$I(t = 2001) \cdot I(d_i \leq 15)$	-0.001 (0.002)	-0.001 (0.002)	-0.000 (0.001)	0.001 (0.002)	-0.005*** (0.002)	-0.002 (0.001)	0.000 (0.004)
$I(t = 2001) \cdot I(15 < d_i \leq 30)$	0.000 (0.002)	-0.001 (0.001)	0.000 (0.001)	0.000 (0.002)	-0.000 (0.001)	0.000 (0.001)	-0.002 (0.003)
$I(t \geq 2005) \cdot I(d_i \leq 15)$	-0.003 (0.002)	-0.005*** (0.002)	-0.000 (0.002)	-0.002 (0.002)	-0.005 (0.003)	-0.006** (0.003)	-0.006 (0.004)
$I(t \geq 2005) \cdot I(15 < d_i \leq 30)$	0.002 (0.002)	-0.001 (0.001)	-0.000 (0.002)	-0.000 (0.003)	0.001 (0.002)	0.000 (0.002)	-0.002 (0.003)
C. Training provision (0/1)							
$I(t = 2001) \cdot I(d_i \leq 15)$	-0.006 (0.004)	-0.003 (0.003)	-0.000 (0.003)	-0.004 (0.004)	-0.002 (0.018)		
$I(t = 2001) \cdot I(15 < d_i \leq 30)$	0.002 (0.002)	0.002 (0.002)	0.003 (0.002)	0.003 (0.002)	0.015 (0.010)		
$I(t \geq 2005) \cdot I(d_i \leq 15)$	-0.005 (0.006)	-0.003 (0.004)	0.003 (0.003)	0.001 (0.005)	0.004 (0.021)		
$I(t \geq 2005) \cdot I(15 < d_i \leq 30)$	0.005* (0.003)	0.003 (0.002)	0.003 (0.002)	0.007** (0.003)	0.022** (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 (5)). The regressions are based on data from the business censuses 1995–2008. The estimation sample comprises of 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.

framework can rationalize this finding if the new foreign workers caused a scale effect that offset the incentives to train less through the cost effect. This section provides evidence that both mechanisms played a role in our context.

5.5.1 The relevance of the scale effect

We start with the scale effect that arises if skilled foreign workers and native workers are imperfect substitutes. The scale effect implies that heavily exposed establishments should grow more than comparable non-treated establishments.

We explore this mechanism in Table 4, 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 know from Figure 6 that there is evidence for a displacement of apprentices in the construction sector—a finding that is confirmed in panel C of Table 4. We also observed a non-significant but economically meaningful negative effect in the public services. Thus, we expect that the scale effect may be most visible 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 sizeable 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 even negative in the construction sector, and the confidence intervals rule out large positive effects. Interestingly, panel A, column 2, suggests that highly treated establishments in the construction sector do not hire more foreign workers than control establishments. They do, however, hire more cross-border workers (column 2, panel B). These two regressions thus suggest that cross-border

workers substitute apprentices in this sector (column 2, panel C) as well as regular immigrants.

Overall, we find evidence in line with our theoretical model that the scale effect offset the negative pressure on apprenticeship provision in manufacturing and, possibly, the private service sector. There is evidence for a quantitatively meaningful displacement of apprenticeships in the construction sector, where the scale effect was absent.

5.5.2 The relevance of the cost effect

We now analyze the relevance of the cost effect, which is based on the notion that a larger supply of skilled foreign workers reduced firms' problems to find suitable skilled workers. By lowering the financial consequences to hire skilled 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 qualitative questions on firms' training motives in the cost-benefit surveys (see section 4.1). Appendix Table C.3 provides the list of training motives covered in the surveys.

Our first piece of evidence consistent with the cost effect is descriptive: one of the most important motives that Swiss firms train apprentices are to attract skilled workers because it is hard to find qualified personnel on the external labor market. Other relevant motives consistent with the cost effect are to train junior workers into skilled workers whose skills match the firms' needs exactly and to save on the cost of hiring skilled workers on the external labor market (see Table 1).

A second, descriptive piece of evidence comes from the observed changes in the importance of firms' training motives between 2000 and 2009. Appendix figure C.2 shows that attracting skilled workers, qualifying junior workers into skilled workers, and securing a talent pipeline in the sector/region became less important training motives between 2000 and 2009. Consistent with an effect of the free movement policy on these motives, the reductions are most pronounced for firms situated near the border. Similarly, the training motives to save hiring costs for external workers, to avoid the risk of a wrong hiring decisions, and to avoid fluctuation by hiring specialists whose skills match the needs very closely became less important for firms near the border while they became more important for firms farther away.

Table 5 formally analyzes whether the greater availability of skilled workers

Table 4: Heterogeneity by broad economic sector

	(1)	(2)	(3)	(4)
	FE	FE	FE	FE
VARIABLES	Manufac- turing	Construction sector	Private services	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 (5)). The regressions are based on data from the business censuses 1995–2008 and estimated separately by sector. The estimation sample comprises of 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.

affected firms’ training motives by making it easier for firms to find skilled workers in the external labor market. Since the motives were levied on a 5-point Likert scale, we estimate equation (5) using ordered probit models.²⁵ These regression do not contain firm fixed effects because the surveys do not track firms over time. Instead, we absorb firm size fixed effects²⁶, industry fixed effects (19 industries²⁷), and fixed effects per training occupation. We estimate heteroscedasticity-robust standard errors²⁸ and weight observations using firms’ sampling weights.

The estimates of the interaction term $I(d_i \leq 15) * I(t = 2009)$ provide strong evidence that the free movement policy influenced firms’ training motives in a way that is consistent with the cost effect. The policy had a statistically significant negative effect on the following five training motives: it reduced highly treated firms’ motives to train apprentices to attract skilled workers because it is hard to find qualified workers externally (column 1), to save hiring costs for external workers (column 2), to avoid the risk of wrong hiring decisions (column 3), to avoid high personnel fluctuation (column 7), and to qualify junior workers into skilled workers (column 8).

We can gain an intuition about the economic size of these effects if we compute predictive margins at a specific value of the 5-point survey questions. The policy, for example, reduced the probability that highly treated firms think that training to attract skilled workers is an important training motive by about 1.2 percentage points. The probability that it is a very important motive decreased by 8.3 percentage points. Similarly, the policy increased the probability that saving hiring costs

²⁵Appendix table C.7 shows that the results are similar if we use a linear probability model where the outcome is 1 if the firms consider the respective motive as important or very important and zero otherwise.

²⁶ We add separate dummies for four broad firm size categories: 0–9 workers, 10–49 workers, 50–99 workers, and 100+ workers.

²⁷The 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.

²⁸We do not estimate cluster-robust standard errors at the level of labor market (NUTS-III) regions because our cost-benefit data does not contain detailed information on the location of firms due to privacy concerns. While we know the exact distance to the border for each firm—this information was computed by the data provider using our crosswalk—, the highest-resolution geographical information that we have is the *canton* in which the firm is located. Table C.8 shows ordered probit estimates with standard errors clustered at the cantonal level. The estimated standard errors are *smaller* in this case. However, these standard errors may be biased because of the small number of cantons (26).

for external workers is not an important training motive by 7.3 percentage points. Interestingly, some of these effects manifest themselves already in the 2004 survey, conducted in autumn and thus only a few months after treated firms gained unrestricted access to cross-border workers. As expected, we observe similarly signed but generally smaller effects for slightly treated firms. We find no statistically significant effect of the policy on the motives to replace *unskilled* workers, save on adjustment costs, and secure a talent pipeline in the sector/region.

Overall, we interpret these findings as supportive for our view that the free movement policy caused a cost effect that reduced treated firms' incentives to train apprentices. Therefore, absent the scale effect demonstrated in the previous subsection, the opening of the border could have had detrimental effects on firms' provision of apprenticeships.

Table 5: Effect of the free movement policy on firms' training motives

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Oprobit Attract skilled workers	Oprobit Hiring cost	Oprobit Risk of wrong decision	Oprobit Replace unskilled workers	Oprobit Adjust- ment cost	Oprobit Hire the best	Oprobit Avoid fluc- tuation	Oprobit Qualify junior staff	Oprobit Secure skilled workers
$I(d_i \leq 15)$	0.044 (0.090)	0.218** (0.092)	0.280*** (0.100)	0.161* (0.089)	0.200** (0.095)	0.264*** (0.089)	0.271*** (0.093)	0.182** (0.092)	-0.113 (0.090)
$I(d_i \leq 15) \cdot I(t = 2004)$	-0.037 (0.139)	-0.112 (0.136)	-0.322** (0.138)	-0.149 (0.129)	-0.169 (0.135)	-0.317*** (0.133)	-0.361*** (0.135)	-0.231* (0.133)	0.012 (0.134)
$I(d_i \leq 15) \cdot I(t = 2009)$	-0.262** (0.130)	-0.286** (0.118)	-0.275** (0.115)	-0.202 (0.125)	-0.115 (0.126)	-0.117 (0.123)	-0.205* (0.123)	-0.262** (0.127)	0.086 (0.147)
$I(15 < d_i < 30)$	0.145* (0.075)	0.071 (0.078)	0.078 (0.083)	0.077 (0.086)	0.008 (0.082)	0.029 (0.080)	0.109 (0.079)	0.119 (0.080)	-0.089 (0.084)
$I(15 < d_i \leq 30) \cdot I(t = 2004)$	-0.373*** (0.110)	0.088 (0.112)	-0.035 (0.116)	-0.057 (0.116)	0.075 (0.111)	-0.039 (0.113)	-0.122 (0.112)	-0.250** (0.114)	-0.072 (0.116)
$I(15 < d_i \leq 30) \cdot I(t = 2009)$	-0.228** (0.116)	-0.077 (0.098)	-0.063 (0.100)	-0.034 (0.112)	0.043 (0.105)	-0.031 (0.113)	-0.179* (0.102)	-0.279** (0.117)	-0.046 (0.119)
Observations	9068	9068	9068	9068	9068	9068	9068	9068	9068
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Size FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table presents the estimation results of the free movement policy on the training motives of firms in the cost-benefit data. All DiD estimations are based on ordered probit regressions of aggregate cross-sections for firms in 2000, 2004 and 2009. The dependent variables “attract skilled workers”, “hiring cost”, “risk of wrong decision”, “replace unskilled workers”, adjustment costs, choose the best, avoid fluctuation, qualify junior staff, and “secure skilled workers” come from qualitative survey questions on the importance of these training motives (5-point Likert scale, where 1 is not important and 5 is very important). 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, industry, and occupation fixed effects (see text for details). ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively. All estimations use sample weights and robust standard errors.

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, 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 (skilled) 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. Consistent with our theoretical framework, the policy reduced treated firms' incentives to train apprentices to save on hiring costs for external skilled workers. A scale effect counteracted the lower incentive to train apprentices arising from this cost effect: the free movement policy had a positive impact on establishment size in manufacturing and possibly in the private service sector. This effect on firm size, possibly the result of imperfect substitution between skilled immigrant and skilled native workers, 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 do not capture possible general equilibrium effects. The far-reaching reform might have affected all firms in Switzerland to some extent. Such effects are absorbed in our specification, which focuses on the differences in the policy's effects between firms that were more and less exposed. Second, our data is limited to the period after 1995. Hence, we cannot show pre-trends for an extended pre-treatment 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 Solving the model

In this section, we derive the first-order condition of the firms' optimization problem sketched in section 2 of the main paper. Recall that the optimization problem reads as follows:

$$\begin{aligned} \max_{T_0, N_0, I_0} \quad & A_0(\alpha N_0^\rho + (1 - \alpha)I_0^\rho)^{\frac{1}{\rho}} - w_0^N N_0 - w_0^I I_0 - \frac{1}{2}c_0 T_0^2 - \frac{1}{2}b_0[L^s](N_0 + I_0)^2 \\ & + A_1(\alpha N_1^\rho + (1 - \alpha)I_1^\rho)^{\frac{1}{\rho}} - w_1^N N_1 - w_1^I I_1 - \frac{1}{2}b_1[L^s]X_1^2 \quad (\text{A.1}) \end{aligned}$$

The first-order condition for skilled native and immigrant workers are:

$$\begin{aligned} N_0 : \quad & A_0 \alpha N_0^{\rho-1} (\alpha N_0^\rho + (1 - \alpha)I_0^\rho)^{\frac{1-\rho}{\rho}} - w_0^N - b_0[L^s](N_0 + I_0) \\ & + (1 - \delta)b_1[L^s]((N_1 + I_1) - (1 - \delta)(N_0 + I_0) - (1 - \gamma)T_0) = 0 \quad (\text{A.2}) \end{aligned}$$

$$\begin{aligned} I_0 : \quad & A_0 (1 - \alpha)I_0^{\rho-1} (\alpha N_0^\rho + (1 - \alpha)I_0^\rho)^{\frac{1-\rho}{\rho}} - w_0^I - b_0[L^s](N_0 + I_0) \\ & + (1 - \delta)b_1[L^s]((N_1 + I_1) - (1 - \delta)(N_0 + I_0) - (1 - \gamma)T_0) = 0 \quad (\text{A.3}) \end{aligned}$$

The first-order condition for training unskilled apprentices reads as follows:

$$T_0 : -c_0 T_0 + (1 - \gamma)b_1[L^s]((N_1 + I_1) - (1 - \delta)(N_0 + I_0) - (1 - \gamma)T_0) = 0 \quad (\text{A.4})$$

To derive an expression for firms' employment of trainees, we follow Stevens' (1994) and define H_0 as a function of exogenous variables reflecting the ratio between hiring costs of skilled workers and training costs unskilled workers, i.e. $H_0 \equiv c_0/(b_1[L^s](1 - \gamma)^2)$ with $H_0 \geq 0$; $\frac{\partial H_0}{\partial b_1} \leq 0$; $\frac{\partial H_0}{\partial c_1} \geq 0$ because $c_t \geq 0$. Combining the three first-order conditions—the two for skilled workers (eq. 4 and eq. 5) and the first-order condition for training (eq. 6)—, and rearranging yields the following decision rule for the training of unskilled workers:

$$T_0 = \frac{1}{H_0 + 1}((N_1 + I_1) - (1 - \delta)I_0(G_0 + 1)) \quad (\text{A.5})$$

where we express the first-period use of skilled labor $N_0 + I_0$ as a function of I_0 and G_0 which relates the first-period wages of skilled native to those of skilled immigrant workers, that is, $G_0 \equiv (\frac{1-\alpha}{\alpha} \frac{w_0^N}{w_0^I})^{-\sigma}$.

Equation A.5 shows that firms train unskilled workers to cover a fraction $1/(1 + H_0)$ of the additionally needed skilled workers in the future. In general, the number of unskilled workers that firms train depends on current and future employment of native and immigrant workers and on the relative wages between skilled immigrant and skilled native workers (to an extent that is governed by the elasticity of substitution between the two skilled types). It also depends on the training costs relative to the hiring costs: the lower the training costs relative to the hiring costs, the larger the number of trainees and vice versa. Firms do not train when the hiring costs b_1 approach zero. Conversely, when training costs c_0 are zero, firms train all additional skilled workers needed for the future.

To derive the decision rule for skilled native and skilled immigrant workers, one can apply the standard CES solution and express the log relative demand for native and skilled workers as a function of the log ratio wages of skilled native to skilled immigrant workers:

$$\log \frac{N_0}{I_0} = \sigma * \left(\log \frac{\alpha}{1 - \alpha} - \log \frac{w_0^N}{w_0^I} \right). \quad (\text{A.6})$$

The employment decision of skilled workers, as in [Stevens \(1994\)](#), does not depend on the number of trained unskilled workers.

B 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 B.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. 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 B.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 2, 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).

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

Variable	(1)	(2)	(3)	(4)	(5)	(6)
	Highly treated Mean	SD	Matched control Mean	SD	Difference in means	SE
FTE employment	11.286	(56.671)	10.454	(43.263)	0.833**	(0.413)
Existed in 1991	0.796	(0.403)	0.796	(0.403)	0.000	(0.003)
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)	-40.437***	(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. ***, **, *, denote statistical significance at the 1%, 5%, and 10% level, respectively.

²⁹The corresponding event study is panel (b) of Figure 5 in the main text.

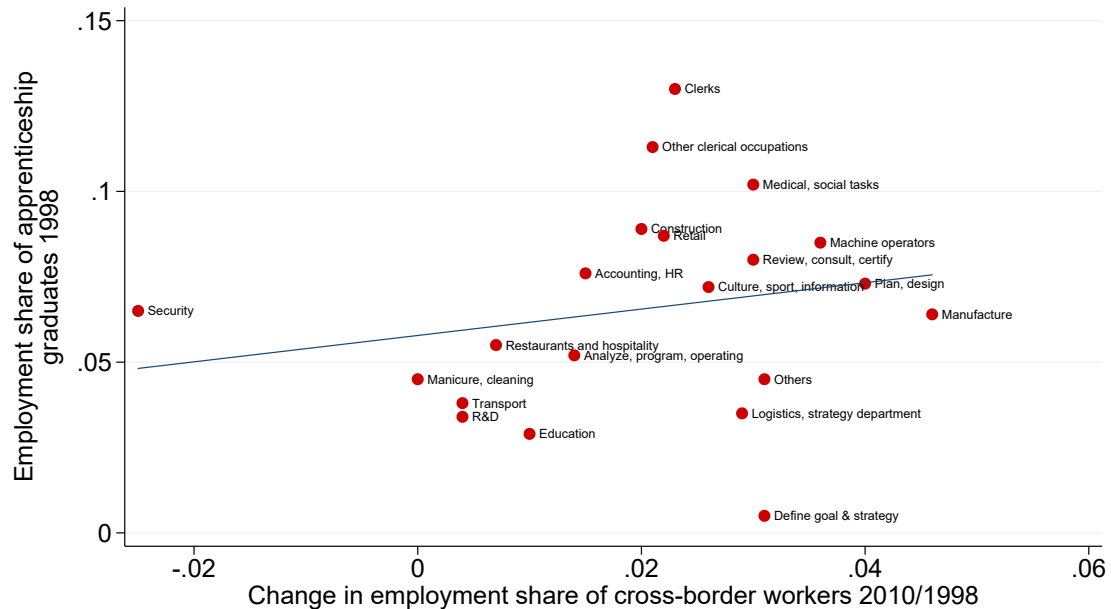
Table B.2: Main results using matched control group

VARIABLES	(1)	(2)	(3)	(4)
	FE	FE	FE	FE
	Foreigners/ total FTEs	Foreigners/ total FTEs	Apprentices/ total FTEs	Apprentices/ total FTEs
	1998	1998	1998	1998
$I(t = 2001) \cdot I(d_i \leq 15)$	0.021*** (0.008)	0.020** (0.010)	0.000 (0.002)	0.000 (0.002)
$I(t \geq 2005) \cdot I(d_i \leq 15)$	0.050*** (0.010)	0.047*** (0.012)	-0.004 (0.003)	-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.

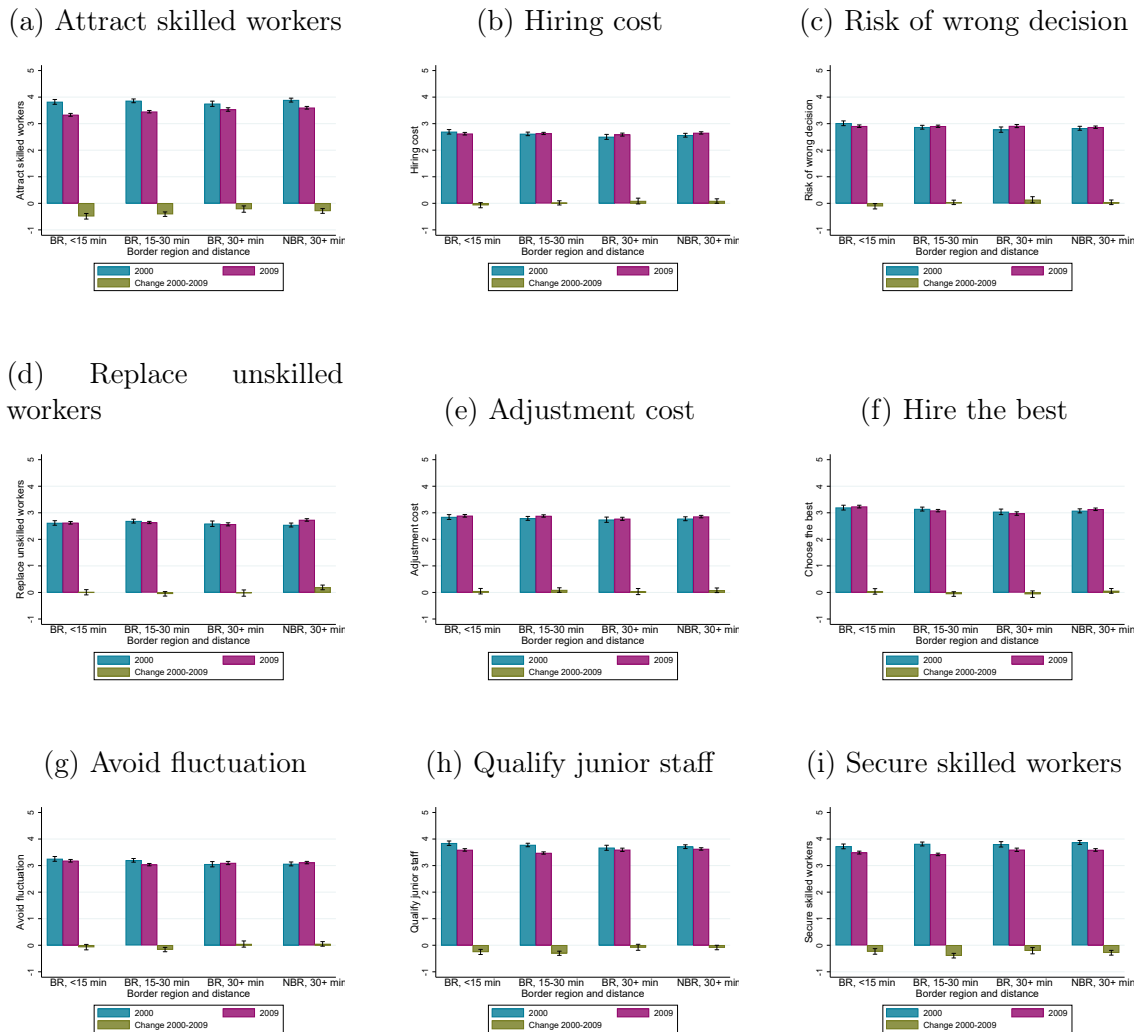
C Further Tables and Figures

Figure C.1: Employment share of apprentices in 1998 and change in employment share of cross-border workers, by occupation



Notes: This figure compares the employment share of recent (native) apprentices 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 C.2 in tabular form.

Figure C.2: Importance of firms' training motives, by region



Notes: This figure shows firms' motives to provide apprenticeships according to the cost benefit surveys in 2000 (blue) and 2009 (teal), as well as the change between 2000 and 2009 (green). We differentiate highly treated firms (firms within the border region within 15 minutes to the border), slightly treated firms (firms within the border region 15–30 minutes away from the border) and firms in the two control groups (firms within the border region more than 30 minutes away from the border and firms outside the border region).

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

Worker characteristics	1998			2010		
	Recent apprentices	Cross-border workers	Other natives	Recent apprentices	Cross-border workers	Other natives
<i>Demographic characteristics</i>						
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
<i>Occupations</i>						
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
<i>Industries</i>						
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 C.2: Employment shares of recent apprenticeship graduates and cross-border workers in the border region, 1998 and 2010

Employment share	Recent apprentices			Cross-border workers		
	1998	2010	Δ 2010 - 1998	1998	2010	Δ 2010 -1998
<i>Employment share by occupation</i>						
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
<i>Employment share by education</i>						
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
<i>Employment share by industry</i>						
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.

Table C.3: Questions on firms' training motives in the cost-benefit data

Variable name	Question
	<p>Training firm: For your firm, how important are the following reasons to train apprentices?</p> <p>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?</p>
Attract skilled workers	Attracting skilled workers because it is hard to find qualified personnel on the external labor market
Hiring cost	Saving cost to hire 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 cost	Saving the money used for training external specialists (adjustment cost)
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 C.4: 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	(3) FE Training firms/ establishments	(4) FE Apprentices/ FTEs 1998
$I(t = 2001) \cdot I(d_i \leq 15)$	0.011** (0.005)	0.543 (0.597)	-0.001 (0.002)	-0.000 (0.001)
$I(t = 2001) \cdot I(15 < d_i \leq 30)$	0.013** (0.005)	1.750* (0.885)	0.003 (0.002)	0.001 (0.001)
$I(t \geq 2005) \cdot I(d_i \leq 15)$	0.051*** (0.017)	0.656 (0.704)	-0.006 (0.005)	-0.003* (0.002)
$I(t \geq 2005) \cdot I(15 < d_i \leq 30)$	0.021*** (0.006)	2.674** (1.162)	0.003 (0.002)	0.001 (0.002)
Observations	12,934	12,934	12,934	12,934
R-squared	0.205	0.052	0.299	0.226
Number of municipalities	2,594	2,594	2,594	2,594
Control group	Both	Both	Both	Both
Municipality FE	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes
Weights	Employment	No	Establishments	Employment

Notes: This table shows the effect of the free movement policy on employment of foreign workers and the provision of apprenticeships using our main DiD model (equation (5)). The regressions are based on data from the business censuses (BC) 1995–2008, aggregated to the municipality level. The dependent variable in column 1 is full-time equivalent (FTE) employment of foreign workers (cross-border workers plus foreign resident immigrant workers) relative to total full-time equivalent employment in 1998 in the municipality. The dependent variable in column 2 is the number of establishments that train apprentices. The dependent variable in column 3 is the share of establishments with apprenticeships among all establishments. The dependent variable in column 4 is the number of apprentices working in a specific municipality relative to total full-time equivalent employment in 1998. The effect of the free movement period is captured with a dummy variable equal to one in the census years after 2004, i.e., the BC 2005 and 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 control group is firms located in the border region with more than 30 minutes travel time to the border and firms in the non-border region. Regressions in columns 1 and 4 are weighted using average municipal employment pre-1999 as weight. The regression in column 3 is weighted using the average number of establishments in a municipality as weight. Standard errors are clustered on the level of commuting zones. ***, **, *, denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table C.5: Effect of the free movement policy on firms' training behavior

VARIABLES	All firms			Training firms	
	(1) FE Training firm	(2) FE Number of apprentices	(3) FE Apprentice share	(4) FE Number of apprentices	(5) FE Apprentice share
$I(d_i \leq 15)$	-0.093*** (0.029)	-0.137*** (0.043)	-0.022* (0.011)	-0.031 (0.055)	0.002 (0.030)
$I(d_i \leq 15) \cdot I(t = 2004)$	0.034 (0.043)	0.030 (0.062)	0.004 (0.016)	-0.041 (0.078)	-0.000 (0.037)
$I(d_i \leq 15) \cdot I(t = 2009)$	0.069 (0.057)	0.127 (0.093)	0.020 (0.016)	0.138 (0.092)	-0.024 (0.033)
$I(15 < d_i < 30)$	-0.075*** (0.027)	-0.096** (0.040)	-0.022** (0.009)	0.009 (0.048)	-0.010 (0.018)
$I(15 < d_i \leq 30) \cdot I(t = 2004)$	0.020 (0.037)	0.038 (0.055)	0.013 (0.014)	0.061 (0.067)	0.032 (0.028)
$I(15 < d_i \leq 30) \cdot I(t = 2009)$	0.056 (0.045)	0.072 (0.073)	0.030** (0.013)	0.092 (0.066)	0.014 (0.024)
Observations	9068	9068	9041	3837	3837
Year FE	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Firm Size FE	Yes	Yes	Yes	Yes	Yes

Notes: The table presents the estimation results of the free movement policy on the training behavior of firms in the cost-benefit data. The first three DiD estimations are based on OLS regressions of aggregate cross-sections for all firms in 2000, 2004 and 2009. The latter two DiD estimations are based on OLS regressions of aggregate cross-sections for training firms in 2000, 2004 and 2009. The dependent variable in column 1 is 0 for firms not training apprentices and 1 otherwise. The dependent variable in columns 2 and 4 is 0 for firms not training apprentices and the number of apprentices otherwise. The dependent variable in columns 3 and 5 is 0 for firms not training apprentices and else the fraction of apprentices among all workers. 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. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively. All estimations include sample weights and robust standard errors.

Table C.6: Effect of the free movement policy on firms' training behavior, excluding occupations affected by the 2004 educational reform

VARIABLES	All firms			Training firms	
	(1) FE Training firm	(2) FE Number of apprentices	(3) FE Apprentice share	(4) FE Number of apprentices	(5) FE Apprentice share
$I(d_i \leq 15)$	-0.093*	-0.146*	-0.002	-0.088	0.056
	(0.051)	(0.075)	(0.023)	(0.106)	(0.077)
$I(d_i \leq 15) \cdot I(t = 2004)$	-0.017	-0.049	-0.016	-0.050	-0.008
	(0.073)	(0.105)	(0.032)	(0.138)	(0.092)
$I(d_i \leq 15) \cdot I(t = 2009)$	0.028	0.052	0.004	0.091	0.000
	(0.071)	(0.109)	(0.030)	(0.152)	(0.081)
$I(15 < d_i < 30)$	-0.086*	-0.132**	-0.027*	-0.044	-0.030
	(0.046)	(0.065)	(0.015)	(0.086)	(0.033)
$I(15 < d_i \leq 30) \cdot I(t = 2004)$	0.018	0.025	0.014	0.044	0.053
	(0.065)	(0.093)	(0.023)	(0.120)	(0.051)
$I(15 < d_i \leq 30) \cdot I(t = 2009)$	0.032	0.063	0.018	0.052	0.036
	(0.062)	(0.091)	(0.020)	(0.119)	(0.044)
Observations	2580	2580	2571	1148	1148
Year FE	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Firm Size FE	Yes	Yes	Yes	Yes	Yes

Notes: The table presents the estimation results of the free movement policy on the training behavior of firms in the cost-benefit data for occupations not affected by the educational reform in 2003. The first three DiD estimations are based on OLS regressions of aggregate cross-sections for all firms in 2000, 2004 and 2009. The latter two DiD estimations are based on OLS regressions of aggregate cross-sections for training firms in 2000, 2004 and 2009. The dependent variable in column 1 is 0 for firms not training apprentices and 1 otherwise. The dependent variable in columns 2 and 4 is 0 for firms not training apprentices and the number of apprentices otherwise. The dependent variable in columns 3 and 5 is 0 for firms not training apprentices and else the fraction of apprentices among all workers. 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. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively. All estimations include sample weights and robust standard errors.

Table C.7: Effect of the free movement policy on firms' training motives using a linear probability model

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	FE	FE	FE	FE	FE	FE	FE	FE	FE
	Attract skilled workers	Hiring cost	Risk of wrong decision	Replace unskilled workers	Adjustment cost	Hire the best	Avoid fluctuation	Qualify junior staff	Secure skilled workers
$I(d_i \leq 15)$	0.023 (0.035)	0.057 (0.036)	0.108*** (0.040)	0.032 (0.036)	0.088** (0.040)	0.070* (0.040)	0.119*** (0.040)	0.087** (0.038)	-0.042 (0.036)
$I(d_i \leq 15) \cdot I(t = 2004)$	-0.049 (0.057)	-0.059 (0.052)	-0.145*** (0.056)	-0.026 (0.053)	-0.093 (0.058)	-0.047 (0.059)	-0.161*** (0.059)	-0.125** (0.057)	0.048 (0.055)
$I(d_i \leq 15) \cdot I(t = 2009)$	-0.125** (0.059)	-0.078* (0.044)	-0.138*** (0.048)	-0.063 (0.050)	-0.036 (0.063)	-0.032 (0.060)	-0.097 (0.065)	-0.096* (0.054)	0.005 (0.054)
$I(15 < d_i < 30)$	0.041 (0.030)	0.012 (0.031)	0.027 (0.034)	0.031 (0.032)	-0.010 (0.034)	-0.008 (0.035)	0.030 (0.036)	0.037 (0.035)	-0.025 (0.032)
$I(15 < d_i \leq 30) \cdot I(t = 2004)$	-0.139*** (0.046)	0.008 (0.044)	0.013 (0.049)	-0.011 (0.046)	0.027 (0.048)	-0.026 (0.051)	-0.081 (0.051)	-0.087* (0.050)	-0.050 (0.047)
$I(15 < d_i \leq 30) \cdot I(t = 2009)$	-0.094** (0.045)	-0.022 (0.039)	-0.029 (0.043)	-0.050 (0.044)	0.048 (0.048)	-0.026 (0.048)	-0.071 (0.052)	-0.105** (0.046)	-0.043 (0.045)
Observations	9068	9068	9068	9068	9068	9068	9068	9068	9068
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Size FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table presents the estimation results of the free movement policy on the training motives of firms in the cost-benefit data. All DiD estimations are based on linear probability regressions of aggregate cross-sections for all firms in 2000, 2004 and 2009. The dependent variables attract skilled workers, hiring cost, risk of wrong decision, replace unskilled workers, adjustment costs, choose the best, avoid fluctuation, qualify junior staff, and secure skilled workers come from qualitative survey questions on the importance of these training motives (5-point Likert scale, where 1 is not important and 5 is very important). For the linear probability model we create dummies for the motives being important when the values are 4 and 5. 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. ***, **, *, denote statistical significance at the 1%, 5%, and 10% level, respectively. All estimations include sample weights and robust standard errors.

Table C.8: Effect of the free movement policy on firms' training motives (SE clustered at cantonal level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Oprobit Attract skilled workers	Oprobit Hiring cost	Oprobit Risk of wrong decision	Oprobit Replace unskilled workers	Oprobit Adjust- ment cost	Oprobit Hire the best	Oprobit Avoid fluc- tuation	Oprobit Qualify junior staff	Oprobit Secure skilled workers
$I(d_i \leq 15)$	0.044 (0.082)	0.218** (0.107)	0.280* (0.144)	0.161* (0.095)	0.200* (0.118)	0.264*** (0.099)	0.271** (0.105)	0.182 (0.116)	-0.113 (0.078)
$I(d_i \leq 15) \cdot I(t = 2004)$	-0.037 (0.106)	-0.112 (0.155)	-0.322** (0.147)	-0.149 (0.133)	-0.169 (0.149)	-0.317*** (0.090)	-0.361** (0.149)	-0.231* (0.136)	0.012 (0.129)
$I(d_i \leq 15) \cdot I(t = 2009)$	-0.262** (0.129)	-0.286** (0.123)	-0.275* (0.162)	-0.202** (0.091)	-0.115 (0.139)	-0.117 (0.136)	-0.205* (0.118)	-0.262* (0.135)	0.086 (0.118)
$I(15 < d_i < 30)$	0.145** (0.064)	0.071 (0.059)	0.078 (0.073)	0.077 (0.106)	0.008 (0.063)	0.029 (0.080)	0.109 (0.071)	0.119* (0.062)	-0.089 (0.069)
$I(15 < d_i \leq 30) \cdot I(t = 2004)$	-0.374*** (0.079)	0.088 (0.057)	-0.035 (0.059)	-0.058 (0.119)	0.075 (0.070)	-0.039 (0.067)	-0.122 (0.087)	-0.250*** (0.074)	-0.072 (0.098)
$I(15 < d_i \leq 30) \cdot I(t = 2009)$	-0.228** (0.112)	-0.077 (0.089)	-0.063 (0.092)	-0.034 (0.140)	0.043 (0.070)	-0.031 (0.119)	-0.179 (0.113)	-0.279* (0.156)	-0.046 (0.135)
Observations	9048	9048	9048	9048	9048	9048	9048	9048	9048
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Size FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table presents the estimation results of the free movement policy on the training motives of firms in the cost-benefit data. All DiD estimations are based on ordered probit regressions with clusters on the cantonal level of aggregate cross-sections for all firms in 2000, 2004 and 2009. All DiD estimations are based on ordered probit regressions of aggregate cross-sections for all firms in 2000, 2004 and 2009. The dependent variables attract skilled workers, hiring cost, risk of wrong decision, replace unskilled workers, adjustment costs, choose the best, avoid fluctuation, qualify junior staff, and secure skilled workers come from qualitative survey questions on the importance of these training motives (5-point Likert scale, where 1 is not important and 5 is very important). 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. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively. All estimations include sample weights and robust standard errors.