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THE ABOLITION OF IMMIGRATION RESTRICTIONS AND THE PERFORMANCE OF FIRMS AND WORKERS: EVIDENCE FROM SWITZERLAND

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The present study builds on the insights of two papers that were concurrently written: Beerli and Peri (2018) and Ruffner and Siegenthaler (2017). The first paper analyzed the employment, wage, mobility and occupational specialization effects of the labor market liberalization in Switzerland. The second studied the firm-level impact of the same reform, establishing its effects on the employment of foreigners within firms and on size, productivity, innovation, location and staffing decisions of firms. We are indebted to the editor and three referees for unusually helpful comments. We also thank the following for their many helpful comments and suggestions on the prior versions of this paper: Andrea Ariu, Matthias Bannert, Christoph Basten, Richard Blundell, Marius Brülhart, David Card, Matz Dahlberg, Kathrin Degen, David Dorn, Christian Dustmann, Peter Egger, Sandro Favre, Ingrid Hägele, Dominik Hangartner, Fred Henneberger, Jennifer Hunt, Boris Kaiser, Daniel Kaufmann, Felix König, Johannes Kunz, Stephan Kyburz, Rafael Lalive, Andrea Lassmann, Guy Michaels, Tobias Müller, Dina Pomeranz, Kjell Salvanes, Lukas Schmid, Daphné Skandalis, Andrin Spescha, Uta Schönberg, Jan Stuhler, Jan-Egbert Sturm, Fabian Waldinger, Martin Wörter, Dean Yang, Josef Zweimüller, Alexandre Ziegler, and Fabrizio Zilibotti. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

The Abolition of Immigration Restrictions and the Performance of Firms and Workers: Evidence from Switzerland

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ABSTRACT

We study a reform that granted European cross-border workers free access to the Swiss labor market. Our Differences-in-Differences estimations leverage the fact that regions close to the border were affected more intensely and earlier. The greater availability of cross-border workers increased their employment but also wages and possibly employment of highly educated native workers although the new cross-border workers were also highly educated. The reason is a simultaneous increase in labor demand in skill-intensive firms: the reform increased the size, productivity, innovation performance of some incumbent firms, attracted new firms, and created opportunities for natives to pursue managerial jobs.

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1 Introduction

Policies that open the labor market to foreigners are often opposed by natives on the ground that they could harm their labor market opportunities. Yet, free mobility of workers means more opportunities for businesses to hire a wider variety of skills. Firms usually welcome a less restricted access to foreign workers.[1] If firms benefit from open borders through increased productivity and growth, this may counteract the effects of increased labor market competition and expand job opportunities for native workers. However, our knowledge on how immigration policies affect firms' success, and whether such effects shape the labor market effects of immigration, is very limited. This study attempts to extend our knowledge on the labor market effect of and firms' responses to opening the border.

To do this, we investigate the impact of an important reform: the gradual and eventually complete removal of all immigration restrictions for workers from the European Union (EU) in Switzerland when the latter introduced the principle of the "free movement of persons." This principle allows EU citizens to move freely within the territory of member states for the purpose of employment. When introducing it, Switzerland progressively removed all its existing legal barriers to immigration for EU workers. It also opened—fully and earlier—its labor market to European *cross-border workers* (CBW). CBW are individuals employed in Switzerland, who live in its neighboring countries (Italy, Germany, Austria and France) and commute across the border for work. CBW were already a sizable group in Swiss regions near the border prior to the policy changes. However, there were several administrative hurdles to hiring them. Among others, CBW were subject to a bureaucratic admission process that aimed at ensuring that firms only hire them if they did not find an equally qualified resident worker (the so-called priority requirement). Moreover, employment of CBW was limited

[1] According to a survey by BAK (2013), 75% of all employers in Switzerland, the country analyzed in this paper, consider access to foreign workers as "important," "very important," or even "indispensable" for their competitiveness and profits.

to a clearly defined set of municipalities close to the border—the so-called "border region."

These prior restrictions were gradually abolished during the course of a far-reaching reform announced in 1999. After partial liberalization measures in 2002, Switzerland removed the "priority requirement" in 2004. CBW thus gained free access to the Swiss labor markets close to the border. This measure increased the presence of CBW substantially. However, the increase occurred exclusively close to the border. In locations beyond 30 minutes driving distance from the border, employment of CBW remained negligible throughout the period of the reform. One reason is that employing CBW remained restricted to the "border region" until 2007. Another, more important reason is that CBW, as frequent commuters, are sensitive to geographical distance and rarely travel at a driving distance larger than 20-30 minutes. The same reform also increased the mobility of permanent immigrants from EU countries. But neither did they experience full liberalization in 2004, nor did their employers cluster close to the border.

In this paper, we thus analyze the consequences of permanently removing all restrictions on hiring cross-border commuters on Swiss workers and firms, taking advantage of the temporarily and geographically discontinuous change in CBW accessibility. We study the effects on workers mainly using data from of a large-scale employer survey conducted every second year between 1994 and 2010. The effects on firms are analyzed using micro-level panel data from the Swiss Business Censuses 1991–2011, covering the universe of establishments in Switzerland, and a series of innovation surveys conducted between 1996 and 2013. Empirically, we compare changes in outcomes in labor markets close to the border with changes in outcomes in labor markets further away from it, distinguishing a pre-treatment (before 1999), a transitional (1999–2003), and a free movement phase (2004 onward). This comparison may partly identify relative rather than absolute causal effects, as some of our evidence on firms suggests that certain effects close to the

border may have occurred at the expense of regions further away.[2]

We first show that, between 1999 and 2010, the labor market liberalization for CBW produced an increase of foreign workers equal to 10 percentage points of the total 1998 employment in municipalities within 15 minutes travel time to the border. This increase is mainly concentrated in the post-2004 period and mostly, but not entirely, attributable to increased employment of cross-border workers. We also observe an increase in employment of permanent resident immigrants, suggesting crowding-in of this group. We then document that the policy changes did not have a measurable negative effect on average employment or wages of native workers. In fact, we find evidence that the free movement policy increased wages of highly educated native workers by 4.6% and possibly their employment. As many of the incoming workers were highly-educated, too, these results cannot be rationalized by a simple model with high- and low-skilled labor in which immigration represents a pure supply shift (as in, e.g., Borjas, 2003). Our labor market findings are even more striking considering that CBW almost certainly generate weaker consumption-side effects (e.g. on demand for housing) than normal immigrants, as they do not reside in Switzerland. This attenuates one positive demand channel.

We argue that the greater availability of CBW produced positive effects on highly-educated natives because it prompted a simultaneous increase in labor demand. We present five pieces of evidence that support this conclusion. First, we show that the inflow of CBW was largest in high-tech manufacturing and the knowledge-intensive business service sector. Skilled workers are arguably important in these sectors, increasing the scope for demand effects that counteract wage pressure for natives. Indeed, we find that the increase in employment of foreign workers and the positive wage effects on natives are concentrated in these industries, and that the reform stimulated firm and productivity growth of incumbents in these sectors. Second,

[2]We will show, however, that mobility and relocation of native workers to areas far from the border was limited in response to the liberalization policy.

we show that the reform boosted firm expansion by relaxing prior constraints to recruit skilled workers. We observe substantial gains in labor productivity in firms that had reported to be constrained by a lack of specialized personnel before the reform. Third, we show that the free movement policy increased R&D employment, patent applications, and product innovations, mainly in firms that had suffered from lack of R&D workers before the reform. Fourth, we find evidence of capital adjustments: the share of new establishments increased by 4 percentage points in the regions closest to the border. Finally, these firm effects created opportunities for natives to grow professionally: their likelihood to work in top managerial positions increased. These transitions into high-paying management explain roughly one third of the positive wage effects for highly-educated natives. In sum, the increased supply of CBW was absorbed due to productivity growth, job growth and greater innovation performance in skill-intensive incumbent firms and the entry of new establishments.

Ours is one of the first studies exploiting changes in policies for cross-border commuters to study the effects of immigration.[3] The closest precursor to this paper is Dustmann et al. (2017) who analyze the strong increase in employment of Czech CBW in regions close to the border shortly after the opening of Germany's labor market to Czech workers in 1991—a policy that was revoked in 1993. Despite the similar policy change, Dustmann et al. (2017) find strikingly different results than we do. They show that the inflow of Czech workers had strong negative short-run effects on native employment and smaller but significant negative effects on native wages. We believe that these differences arise because of differences in the design of the policy and in economic circumstances. First, the Czech inflow was mainly composed of less-educated workers hired in relatively low-skill-intensive industries. In contrast, many new CBW in the Swiss case were highly skilled,

[3]The idea to exploit the changes in Switzerland's commuting policies in order to study the effects of immigration was also advanced in two policy reports by Henneberger and Ziegler (2011) and Losa et al. (2014).

and the beneficial effects on natives mainly arose in industries and firms that heavily depend on skilled workers. Second, the policy change in Germany was unexpected, temporary, and it affected a region that had not experienced significant immigrant inflows previously. In contrast, the Swiss policy change was permanent, announced early and phased-in gradually, and it had the strongest impact on regions which were exposed to foreign workers for decades. Arguably, Swiss firms were thus prepared to efficiently match the new workers to jobs, and they were ready to adjust to the reform. Indeed, contrary to Dustmann et al. (2017)[4], we find evidence of significant firm entry starting already during the transitional phase of the reform. Finally, the episode studied by Dustmann et al. (2017) took place after the fall of the Iron Curtain, when the Czech Republic underwent a major transition and Germany was in a recession, too. In contrast, Switzerland opened its labor market at a time when the unemployment rate was very low and demand for skilled workers high. Our results suggest that relaxing the constraint on the supply of skilled workers was a major economic boost especially for firms that suffered from lack of skilled labor.

The most important contribution of this paper is to highlight the importance of analyzing firms' responses to changes in the availability of labor in order to understand the labor market effects of (skilled) migration. Our labor market findings can only be rationalized in relation to firms' productivity, capital investment, innovation and specialization responses. So far, "there is very little tradition for considering firms in analyses of immigration" (Kerr et al., 2015, p. S148). In fact, we are unaware of any previous paper analyzing firm and labor market effects of immigration jointly. Our evaluation also has important advantages in terms of identification of causal effects compared to many previous studies in the literature. These studies are mainly based on the so-called area approach, and typically isolate supply-driven variation in immigration into regional labor markets by ap-

[4] Dustmann et al. (2017) only find a small impact on the entry of new firms that is concentrated in the non-tradeable sector.

plying a shift-share instrumental variable (as in Card, 2001).[5] The approach hinges on the assumption that historical immigrant settlement patterns are uncorrelated to the regional distribution of current unobserved labor demand shocks—an assumption that is not always plausible (see Jaeger et al., 2018). In our case, both the increase in the aggregate number of foreigners and its uneven regional impact are a direct consequence of the exogenous change in the commuting policy.

Another contribution of our paper is to rigorously evaluate the consequences of a policy that permanently removed all barriers to the labor market access of foreign workers. Our variation is thus different to the temporary push-driven surges in immigrants analyzed in many other papers (such as the Mariel Boatlift in Miami), and provides insights into the long-run effects of permanently changing immigration policies. Moreover, our findings inform policy makers about the potential economic benefits of free movement of workers. This is highly relevant against the background of mounting opposition to free labor mobility in Europe, which culminated in 2016 with Britain's decision to leave the EU.

Finally, our study contributes to the literature on the impacts of skilled immigration on productivity, innovation, and production technology in the receiving country (see Kerr et al., 2015, for an overview). Studies on the regional level (e.g., Hunt and Gauthier-Loiselle, 2010; Peri et al., 2015a,b) or that focus on inventors (Moser et al., 2014) tend to find positive impacts on productivity and innovation. Firm-level studies examining these links are still rare and reach conflicting conclusions (Doran et al., 2015; Dustmann and Glitz, 2015; Ghosh et al., 2014; Hunt and Gauthier-Loiselle, 2010; Kerr et al., 2015; Mitaritonna et al., 2017). To our knowledge, ours is the first study to directly link firms' performance to a policy change that fully and permanently opened the labor market for foreign workers.

[5] See Blau and Kahn (2012) and Lewis and Peri (2015) for overviews of this literature.

2 The immigration reform

The process of opening the Swiss labor market to citizens from the EU started with the signing of the bilateral agreements between the EU and Switzerland on June 21, 1999. The so called "Agreement on the Free Movement of Persons" (AFMP) introduced free worker mobility among the signing countries. The relevant details of this agreement were publicly announced in Switzerland in late 1998. The agreement was then discussed by the Swiss parliament. After the treaty had been signed, it required the approval of the Swiss electorate (which accepted it in a national referendum in May 2000 with an approval rate of 67.2%), the European parliament, and of each EU member state. The AFMP was enacted in June 2002—one-and-a-half years later than planned at the time of the first announcement. Given the timing of the reform, anticipatory effects of the reform are only possible from 1999 onward.[6] Similarly, given the political circumstances of the reform, it appears very unlikely that the local economic conditions of the regions most affected by the agreements were a consideration in the timing and the content of the treaty.[7]

Table 1 provides a simplified illustration of the stepwise introduction of free movement of persons. The table distinguishes three reform phases (the pre-reform, transition, and free movement phase) and two types of foreign workers: permanent resident immigrants and CBW. The shading of the table highlights the restrictiveness of the regulations for the respective worker

[6]The relevant details of the reform were not public knowledge before 1998 and the success of the negotiations was uncertain prior to a breakthrough achieved only in 1998. In fact, even in 1997 and early 1998, several members of the Swiss parliament expressed their concerns that the negotiations could fail.

[7]One reason is that it was the federal government, not the cantons, that negotiated the AFMP. Another reason is that introducing the free movement of workers was not championed by the Swiss government but a political concession to the EU. The AFMP was part of a package of agreements negotiated at the same time. The other agreements pertain to harmonizations in specialized fields such as air and land traffic, agriculture, research cooperation, and reducing non-tariff barriers to trade. At the beginning of the negotiations in 1993 that led to these agreements, the Swiss government tried to avoid a full-fledged version of free worker mobility. As the EU insisted on full labor mobility, a breakthrough in the negotiations was only reached when both parties agreed that the free labor mobility would be implemented step-wise and included further safety measures.

Table 1: The different phases of the introduction of free movement of workers

			Cross-border workers		Immigrants
Phase	**Year**	**Event**	*Border region*	*Non-border region*	*Both regions*
Pre-reform	1995		Admission process (priority requirement), further restrictions	No access	Admission process, annual quotas, further restrictions
	1996				
	1997				
	1998	Announcement			
Transition phase	1999	AFMP signed	Anticipatory effects possible		
	2000	Referendum			
	2001				
	2002	AFMP enacted	Abolition of further restrictions		Higher quotas, further changes[1]
	2003				
Free movement *phase in border region*	2004	Liberalization	***Free***		Abolition of admission process
	2005	in border region			
	2006				
	2007	Full liberalization		***Free***	***Free***
	2008				

[1] Extension of durations of several residency permits. Allowance of family reunion for most permit holders.

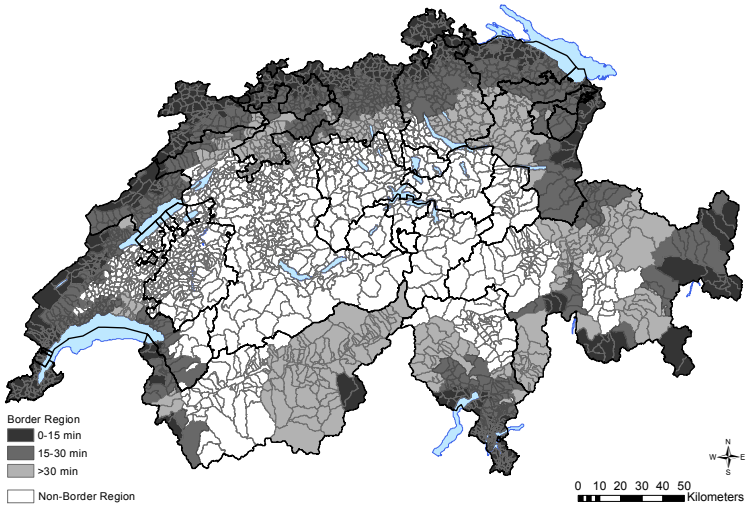
category. The table shows that permanent resident immigrants had been subject to yearly national quotas set by the federal government before the reform and to an admission process very similar to the one for CBW detailed below. These restrictions were removed from 2002 onward. Resident immigrants gained free access to the Swiss labor market with the abolition of the prior annual quotas on different residency permits in 2007.

Table 1 shows when and how the reform lifted the prior barriers on hiring and employing CBW. Importantly, these changes happened earlier than those for permanent immigrants—namely between 1999 and 2004—and only affected municipalities in the *border region* (BR). The reason is that employment of CBW remained restricted to BR until 2007, as it had been before the reform. Figure 1 illustrates the geographical split of Switzerland into the BR (in grey) and the rest of Switzerland, termed the non-border region (NBR, in white) henceforth. The BR had been defined in bilateral agreements between Switzerland and its neighboring countries signed between 1928 and 1973.[8] The frontier between BR and NBR is specific to these contracts. It does not follow cultural or religious border, nor cantonal or other political

[8]The contract between Germany and Switzerland was signed in 1970; those between Switzerland and France, Italy, and Austria were signed in 1946, 1928, and 1973 respectively. The frontier between BR and NBR remained unchanged in the course of the reform.

other than municipal borders.

Figure 1: Municipalities in the border and non-border region and travel distance to the border



Notes: Municipalities in the border region are indicated in three different shades of gray and those in the non-border region in white. Within the border region, we distinguish three regions according to their travel time by car to the nearest border crossing. The black lines denote cantonal borders. Note that border regions do not overlap completely with cantonal borders.

The liberalizations for CBW within the BR occurred in two steps. In the *transition phase* starting in 1999, cantonal offices, which were responsible for handling applications for CBW, first gained more discretion for doing so. Anecdotally, they exploited this to handle CBW applications in a less stringent manner.[9] Some restrictions were lifted starting in 2002. First, the recruitment area for CBW was expanded to the entire neighboring countries of Switzerland. Prior to 2002, Swiss firms could only hire CBW who had lived for at least six months in certain German, Italian, French or Austrian municipalities close to the border to Switzerland. Second, new cross-border permits were now generally valid for five years and no longer bound to a specific job. Before 2002, cross-border permits were formally limited to one year and ended with the termination of a work contract, which restricted

[9]Conversations with representatives from cantonal immigration offices revealed that there was a more relaxed handling of new CBW applications after 1999, and particularly after the national referendum on May 21, 2000, as it was clear that eventually CBW would be the first to gain unrestricted access to the BR.

the geographical and occupational mobility of CBW. Third, CBW were only required to commute to their place of residence weekly rather than daily as before. They were thus granted the right to search for housing in Switzerland.[10]

The *free movement phase* started mid-2004 when firms in the BR gained full and free access to CBW. Switzerland dropped completely the bureaucratic admission process for CBW that had been in place before. In particular, Swiss firms had to provide evidence that they had not found, "within an appropriate period of time," resident workers who were willing and capable of filling their vacancies. This regulation had aimed to ensure that firms gave priority to resident workers. The "priority requirement" created direct recruitment costs for firms by requiring them to go through a relatively lengthy admission process for CBW.[11] In June 2004, hiring CBW in the BR became as easy as hiring Swiss workers.

The number of CBW employed in the BR increased substantially in the years of the liberalization. Overall, the total number grew from 103'900 to 175'200 workers from 1998 to 2010. Importantly, this increase in CBW was strongly concentrated in labor markets very close to the border. Figure 2 uses data from the Swiss Earnings Structure Surveys to plot the share of cross-border workers in total employment separately for the BR and the NBR. Municipalities are grouped into bins of 5 minutes travel-time by car to the nearest border crossing. The figure shows that CBW were almost exclusively employed in municipalities in the BR between 0 and 30 minutes

[10] It appears, however, that only a small fraction of CBW switched to weekly commuting during our period of analysis (see Beerli and Peri, 2018, p. 8, for details).

[11] When hiring a CBW, firms had to prepare an application detailing the job requirements of their vacancy and the working and contract conditions offered. Moreover, firms had to provide proof that they had searched unsuccessfully for a worker within Switzerland for a certain number of weeks. The application had to be sent to the cantonal and federal migration offices. The processing of the application lasted about one to three months. The migration offices evaluated each application individually, notably by comparing the job requirements with information on the qualifications of residents registered as unemployed. Today, the direct costs for Swiss firms to recruit workers from outside the EU are estimated to be about ten to twenty times larger than those for recruiting EU workers (B,S,S. Volkswirtschaftliche Beratung, 2013). This is relevant, as hiring non-EU workers is regulated similarly today as hiring CBW before the reform.

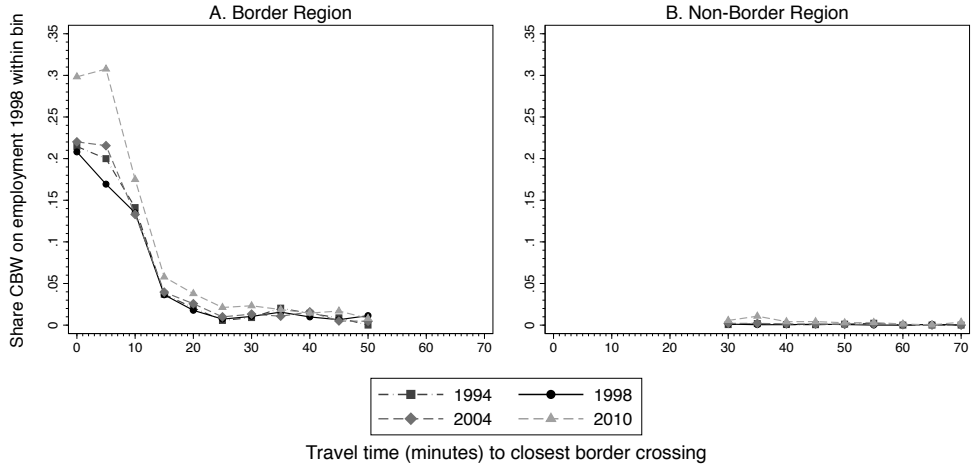
from the border, both before and after the reforms. This is because of limits to the distance that CBW are willing to commute. The figure also reveals the *change* in the employment share of CBW over time. The change was very small and sometimes even negative in the pre-liberalization period (i.e. between 1994 and 1998). During the transition period (1999–2003), the share increased slightly only in municipalities close to the border. Finally the increase in the share is largest in the free movement phase (i.e. between 2004 and 2010), and the larger, the closer to the border. It is essentially zero in municipalities that are more than 30 minutes away from the border. We also observe an increase in the employment share of CBW in the NBR (Panel B of Figure 2) between 2004 and 2010, but this increase is quantitatively very small, possibly because less than 2% of all establishments in the NBR are located within 30 minutes to the border. Due to the limited employment of CBW in the NBR, our empirical framework does not exploit the switch from no to free access for CBW in the NBR in 2007.

The focus of our analysis, therefore, is on the BR. We distinguish three groups within the BR (see Figure 1). Municipalities and firms located 0–15 minutes away from the border are considered as strongly treated. Those between 15 and 30 minutes are considered as weakly or moderately treated. The municipalities over 30 minutes within the BR will form the main control group. For all our main outcomes of interest, we will also show that our results are very similar if we include municipalities in the NBR into the control group. Our approach to analyze the reform by partitioning Switzerland into three regions has the important advantage that it allows for a transparent empirical analysis of the data.[12]

Two features of the AFMP have important implications for the interpretation of our results. First, the AFMP also lifted all restrictions for Swiss residents to work as a CBW in neighboring countries. However, the

[12]In Beerli and Peri (2018) and Ruffner and Siegenthaler (2017), we show that finer intervals and differently defined regions, or exploiting the continuous nature of the travel distance, lead to very similar results.

Figure 2: Employment share of cross-border workers in distance bins



Notes: The figure plots the share of cross-border workers in total employment in 1994, 1998, 2004 and 2010 separately for the border region (Panel A) and the non-border region (Panel B). Municipalities are grouped into bins of 5 minutes according to their travel time by car to the next border crossing. Bins with very a small number of total workers are omitted, i.e. those with travel time above 50 minutes in the border region and those between 13 and 30 minutes in the non-border region. SESS data.

change in employment of CBW in Switzerland was about nine times larger than the change of CBW from Switzerland working in neighboring countries, so that increasing the access of foreign workers to the Swiss labor market was the main outcome of the reform.[13] Second, we interpret the reform as *increasing the availability of CBW* in regions close to the border. This interpretation does *not* just entail an increased employment share of CBW. It also encompasses possible reform effects on firms' recruitment costs through lower bureaucratic hurdles, and possible effects on CBW already working in Switzerland prior to the reform. These CBW may have benefitted from increased geographical and occupational mobility. As a consequence, our empirical specifications focus on the *reduced-form effects* of the reform. Our parameter of interest is the extent to which removing all prior restrictions for

[13]See Table A.1 in the appendix. The asymmetry reflects the fact that nominal wages and the cost of living are much higher in Switzerland, making it very unattractive to live in Switzerland while working abroad. Data from the Eurostat/OECD purchasing power parities (PPP) program suggest that consumer prices were between 23% (France) to 34% (Germany) lower in neighboring countries compared with Switzerland in 2009. Eurostat's labor cost survey in 2012 suggests that nominal wage costs per hour are between 33% (France) to 46% (Italy) lower in neighboring countries.

CBW affected outcomes of natives workers and resident firms in the regions most heavily affected by the reform.

3 Data and empirical strategy

3.1 Data

Our empirical analyses are based on three data sets. The main data source for our labor market analyses are the Swiss Earnings Structure Surveys (SESS) that have been conducted every two years by the Swiss Federal Statistical Office (FSO) since 1994. They are a stratified random sample of private and public firms with at least three full-time equivalent (FTE) workers from the manufacturing and service sectors, covering between 16.6% (1996) and 50% (2010) of total employment in Switzerland. The data include detailed information about workers, their wages and full-time equivalents, their demographic characteristics, and their place of work. Our sample includes individuals between 18 and 65 years of age, working in the private sector, and with non-missing information for nationality, place of work, education, wages, full-time equivalents, and some other basic demographics.[14] Based on information about workers' residency permits, we distinguish between *native workers*—those with Swiss nationality either born in Switzerland or naturalized—, foreign-born workers with a residency permit which we simply call immigrants I, and CBW. Using the SSES, we analyze the effects of the policy change on the number of cross-border and foreign-born workers as a share of total employment, and the effects on full-time equivalents and real hourly wages of natives, sometimes separately by workers' highest educational attainment. We define workers with tertiary education as being *highly-educated*. Workers with completed secondary education (such an apprenticeship program) and workers with primary education or less are defined as *lower educated*.[15]

[14] Appendix section B.1 contains a detailed discussion of the sample construction for the labor market analysis.

[15] There are good *a priori* reasons to show separate results for these two subgroups. This is the approach followed in Beerli and Peri (2018). For brevity and because of the

Our second data source consist of seven waves of the Swiss Business Censuses (BC) conducted in 1991, 1995, 1998, 2001, 2005, 2008, and 2011 by the FSO in October. The BC cover the *universe* of private and public establishments in Switzerland. Approximately 4 million employed persons in 389,000 workplaces are included in the census of 2008. The data are available as a panel dataset and provide us, among other information, with information on the size (FTE employment) and the exact geographical location (geographic coordinates) of all establishments in Switzerland. The data are very reliable. Until 2008, the censuses were based on mandatory surveys. In 2011, the census was constructed from register data.

The third data source is the innovation surveys (IS) of the KOF Swiss Economic Institute. These surveys were conducted among Swiss companies between 1996 and 2013 in seven waves. The quantitative variables refer to the year *prior* to each survey. All surveys are based on a representative sample of private-sector firms with at least five FTE employees. The surveys are disproportionately stratified with respect to firm size and two-digit industry affiliation. The IS provide very detailed information on the characteristics of the surveyed firms and a rich set of outcomes such as value added and the number of patent applications filed. However, the data cover only a relatively small sample of firms per wave, are potentially subject to reporting errors, and there is attrition and non-response because the surveys are voluntary (the average response rate across all surveys is 35%). In addition, the unit of observation is the firm, not the establishment. Both facts imply that the results with the IS may be affected by classical measurement error, especially for multi-establishment firms, as we have to assign these to a location depending on the location of the headquarter.[16] Indeed, our estimates with

similarity of the labor market results, we decided to pool the two subgroups in this version. Indeed, previous research suggests that these two subgroups are perfectly substitutable (Gerfin and Kaiser, 2010; Müller and Graf, 2015). In any case, the subgroup with less than secondary education is quite small (around 13% of employment in the BR).

[16] Appendix section B.2 provides detailed discussions on how we constructed our estimation sample for the two datasets and how we assign firms to BR and NBR.

the IS are not very precise, precluding us from drawing strong conclusions on the effect sizes on variables from this dataset.

3.2 Descriptive statistics

Panel A of Table A.2 in the appendix shows summary statistics for CBW and native workers in the BR of Switzerland using the SSES. We focus on the years 1998, 2010, and the change over this time period. Three features are noteworthy. First, while the share of less educated CBW exceeds the corresponding share among natives in 1998, we observe a large increase in the share of highly educated CBW in the 1998–2010 period (+12.6%). Second, the increase in CBW is largest in IT/R&D/business/real estate and, to a lesser extent, in the health sector, suggesting that the new CBW were highly qualified and professionals in science and technology. Third, employment of CBW grew mostly in occupations with high and intermediate pay, confirming the high skill levels of many new CBW.[17]

In Panel B of Table A.2, we regress log hourly wages of natives and CBW on an indicator for CBW and a series of controls. The first Mincer regression suggests that wages of CBW are on average -0.053 log points lower than those of observationally similar natives. The wage gap decreases to -0.016 log points when we only compare workers in the same year, establishment, and occupation hired in the same year. The results suggest relatively little wage discrimination against CBW and, at the same time, that CBW have comparable labor market skills as observationally similar natives.

Table A.3 in the appendix compares the characteristics of workers of the four regions that we are comparing in our empirical analysis: highly-treated regions (municipalities within 15 minutes to the border in the BR), weakly treated regions (15–30 minutes to the border in the BR), and the two control regions (municipalities in the BR with more than 30 minutes to the border

[17] Beerli et al. (2017) show that the increase in tertiary education among newly arriving immigrants in Switzerland between 1990–2010 is a response to long-term, technology-driven increase in the demand for skills increasing the relative wages and employment opportunities for this type of workers.

Table 2: Average firm characteristics prior to the reform, by region

Duration to border (in min)	Border region			Central region	Total
	≤15	*15–30*	*>30*		
	(1)	(2)	(3)	(4)	(5)
Firm age	45.3	45.5	46.0	51.3	47.1
Manufacturers (%)	0.49	0.47	0.53	0.51	0.49
Firms with R&D expendit. (%)	0.43	0.51	0.48	0.42	0.46
Share of exporters (%)	0.54	0.51	0.48	0.43	0.49
Export share in sales (%)	22.54	20.87	20.38	16.65	20.01
Firms with foreign owner (%)	0.12	0.14	0.09	0.09	0.11
FTE employment (ln)	3.89	4.05	3.84	3.83	3.92
Total sales (ln)	16.16	16.50	16.12	16.10	16.26
Share academics in workforce (%)	0.18	0.19	0.16	0.15	0.17
Wage per FTE worker (ln)	11.12	11.20	11.13	11.13	11.15
Value added per FTE worker (ln)	11.69	11.82	11.74	11.73	11.75
Firm-year observations	932	1,428	615	1,131	4,106

Notes: The table shows average firm characteristics in the border and non-border region. The border region is split into three groups depending on the travel duration between the firm's location and the nearest border crossing. The data are from the KOF innovation surveys 1996 and 1999. Entries represent averages per region of all firm-year observations in the surveys.

and the CR). The table suggests that the four groups are quite comparable in terms of their size, important average worker characteristics, workers' mean log hourly wages, and industry composition. Similarly, Table 2 shows that there are only small pre-reform differences in firms' characteristics across the regions using firm-level data from the IS. The regions are, for example, comparable in terms of average firm size, firm age, the average export share, and the share of firms with nonzero R&D expenditures.

3.3 Empirical specification and identification

Our basic empirical specifications focus on the effects of the greater availability of CBW in the BR during the *transition* phase (1999–2003) and the *free movement* phase (2004 onward). We build a treatment-control framework exploiting that the timing and intensity of regions' exposure to the reform depends upon its distance to the nearest border crossing. We implement this approach empirically by defining a dummy $Transition_t$ which is equal to one in the years $1999 \leq t < 2004$ and zero otherwise, and a dummy $Free_t$ which equals zero prior to 2004, and equals one in $t \geq 2004$. We then interact these two variables with indicators for the travel time d_i between unit i (a

municipality, an establishment, or a firm) and the nearest border crossing to one of Switzerland's neighboring countries: $I(d_i \leq 15)$ and $I(15 < d_i \leq 30)$ are one if a unit is located within 15 minutes or between 15 and 30 minutes travel time to the Swiss border, respectively.[18] Using these variables, we estimate the following DiD model for a generic outcome $y_{i,t}$ of municipality, firm or establishment i in year t:

$$\begin{aligned} y_{i,t} &= \beta_{d1}^{T}\left[Transition_t \times I(d_i \leq 15)\right] + \beta_{d2}^{T}\left[Transition_t \times I(15 < d_i \leq 30)\right] \\ &\quad + \beta_{d1}^{F}\left[Free_t \times I(d_i \leq 15)\right] + \beta_{d2}^{F}\left[Free_t \times I(15 < d_i \leq 30)\right] \\ &\quad + \alpha_i + \alpha_t + \gamma Controls_{i,t} + \epsilon_{m,t} \end{aligned} \tag{1}$$

In this model, $\beta_{d1}^{\text{phase}}$ and $\beta_{d2}^{\text{phase}}$ capture the impact of the reform on highly and slightly treated units, respectively, i.e. the differential evolution in the outcome $y_{i,t}$ in these groups during the transition phase and free movement phase relative to the control group. In our baseline specification, we limit our analysis to units in the BR, so that the control group are municipalities in the BR at more than 30 minutes from the border. As shown below, the results are very similar when adding municipalities in the NBR to the control group.[19] The term α_t represents year fixed effects absorbing the dummies $Transition_t$ and $Free_t$ and time variation common to all units such as common changes in aggregate prices and demand. α_i represent unit fixed effects that control for pre-existing differences between regions. Such differences could have been a direct consequence of the long-established cross-border policy that restricted the hiring of CBW to the BR. Finally, $Controls_{i,t}$ are time-varying control

[18]The travel distance to the border is computed using information on the location of establishments (BC) and firms (IS). Note that d_i is time-invariant even though some firms change location in our sample period. We avoid this by assigning firms to their location in 1998 or to the location that they are first observed if they do not exist in 1998. For the municipality-level specifications, we use the BC 1995 and 1998 to compute the employment-weighted average travel time to the border of the establishments in a municipality. See section B.2 in the appendix for further details.

[19]In this case, we exclude the very few establishments and municipalities located in the NBR within less than 30 minutes to the border. Although our results are insensitive to this exclusion, it appears conceptually preferable because these units may be affected by the switch from no to free access to CBW that occurred in 2007 (see Table 1).

variables that may affect labor demand in the municipalities.

The central identifying assumption of our empirical design is that we would have observed the same average change in outcomes within units in the three regions absent the reform. As always in a DiD, this "common trend" assumption cannot be tested. However, we will assess its plausibility in several ways. Most importantly, we assess flexibly how the outcomes in the three relevant regions evolve before and after the reform. To this end, we generalize equation (1) to an event study DiD model by interacting the two travel time indicators with a dummy for each year in the data, denoted as $I(year = t)$. In the case of the SESS, the model takes the following form:

$$\begin{aligned} y_{i,t} &= \alpha_i + \alpha_t + \sum_{t=1994}^{2010} \gamma_{d1,t} I(year = t) \times [I(d_i \leq 15)] \\ &\quad + \sum_{t=1994}^{2010} \gamma_{d2,t} I(year = t) \times [I(15 < d_i \leq 30)] + \delta Controls_{i,t} + \epsilon_{i,t} \end{aligned} \tag{2}$$

The estimates of the coefficients $\gamma_{d1,t}$ for each year between 1994 and 2010 are the parameters of interest. For $t \geq 1999$, they reveal policy effects on the most highly treated units. $\gamma_{d2,t}$ would reveal possible impacts on slightly treated units. As the impact of the policy should be zero prior to the date it was announced, we should find that $\gamma_{d1t} = 0$, for $t \leq 1998$. All effects are estimated relative to 1998, as we omit the indicator for the year 1998 in the summation.

In both regression models, the main threats to a causal interpretation of our estimates are arguably unobserved factors that are correlated with the timing of the reform and that affect regions differently depending on the distance to the border. Candidate confounding factors are simultaneous other reforms (e.g. due to changes in cantonal policies) and unobserved region-specific shocks to prices, demand, or productivity. We partially account for such factors by controlling for a full set of linear time trends at the level of NUTS-II regions. We also provide extensive evidence that unobserved

region- or industry-specific shocks, e.g. due to changes in trade flows, do not confound our results in the two robustness sections 5.2 and 7.5, focusing the labor market and firms, respectively.

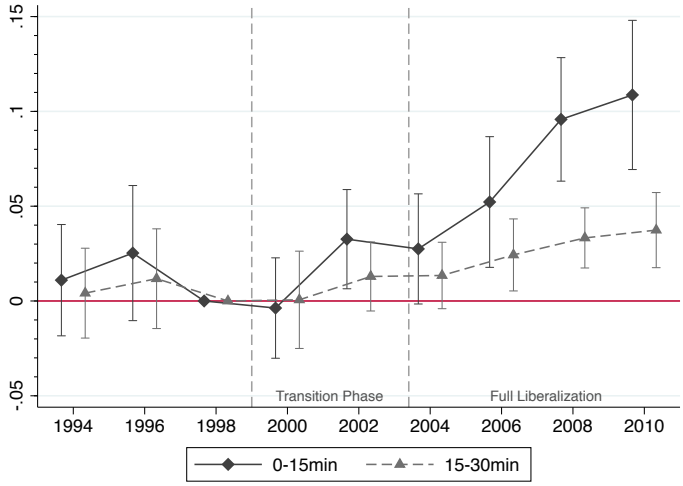
A final remark concerns inference. We cluster standard errors at the level of commuting zones (CZ), both in our municipality- and firm-level analysis. We thus allow for arbitrary cross-sectional dependence between units within the same CZ. In tables A.8 and A.13, we compare the standard errors based on this strategy with standard errors clustered at the unit (firm/municipality), two-digit industry, and cantonal level, and with standard errors based on the Spatial Heteroscedasticity and Autocorrelation Consistent (SHAC) variance estimator proposed by Conley (1999), also used by Dustmann et al. (2017). This estimator allows for correlation between areas that are geographically close but belong to different regional units. These alternative standard errors are often substantially smaller than our preferred ones. We thus view our inference as being conservative.

4 Policy and the intensity of immigration

It is a central requirement for the validity of our empirical strategy that regions close to the border were more strongly affected by the immigration reform. Figure 2 provides descriptive evidence that supports this idea. Using the SESS from 1994–2010, we now study the exact dynamics of the change in total immigrant exposure in municipalities located close to the Swiss border compared to those further away. Figure 3 plots the coefficients $\gamma_{d1,t}$ (diamonds) and $\gamma_{d2,t}$ (triangles) and their 95% confidence intervals of a regression as specified in equation (3) restricted to the BR. The dependent variable is the number of total immigrant workers (CBW plus resident immigrants) in municipality i and year t standardized by the total 1998 employment, $\frac{CBW_{i,t}+I_{i,t}}{Emp_{i,1998}}$.

Figure 3 reveals some important features in the evolution of immigrant exposure by region. First, in the pre-1999 period, none of the estimates

Figure 3: Event analysis of effect of free movement policy on share of immigrants on total employment



Notes: The figure plots the coefficients and the 95% confidence interval for the 0-15min distance bin of a regression based on equation (3) with the share of immigrants on total employment in 1998 as dependent variable. Regressions are weighted using the total workforce in 1998 in a cell and include NUTS-II trends and only the sample of municipalities in the BR. Standard errors are clustered on the CZ level. SESS data.

for any of the interactions is significantly different from zero at the 95% confidence level. There are thus no differences in the trends of the share of immigrants between municipalities close to the border and those far from it before the reform. Second, there is a mild upward trend in the immigrant share between 2000 and 2002 in the 0–15 minutes bin. The estimated increase in the immigrant share relative to 1998 is between two and three percentage points in 2002, suggesting a small reform effect on immigrant employment in the highly treated regions during the transition phase. Third and most importantly, the share of immigrants grows consistently in the 0–15 minutes bin after 2004, and to a lesser extent in the 15–30 minutes bin. By 2010, the reform increased the share of immigrants by 10 percentage points. The coefficient for the 15–30 minutes bin also increases after 2004 and reaches 3.7 percentage points in 2010. As expected, the estimates indicate that the reform effect was smaller in the slightly compared to the highly treated regions. Using our baseline DiD specification (1), the first two columns of Table 3 show that these results do not change much if we add municipalities of the NBR to the control group: the increase in the share of immigrants

is 5.6 (baseline) or 4.9 (including the NBR) percentage points in the free movement period. Note that in this specification, we compare the average outcome in the free movement period with the entire pre-reform period.

The regressions in columns 3–6 of Table 3 show how the aggregate inflow of immigrants can be decomposed into contributions from different immigrant subgroups. In columns 3 and 4, we decompose the total inflow into inflows from highly and lower educated immigrants. The results suggest that most of the increase in CBW is attributable to highly educated immigrants (3.8 of 5.6 percentage points). Complementing this picture, appendix table A.4 shows that the largest contribution to the overall growth in the immigrant share came from occupations with high pay, such as R&D workers, IT specialists, analysts and consultants, and to a lesser extent from those with middle pay. Column 5 shows that two thirds (3.8 of 5.6 percentage points) of the total increase in immigrants in the free movement phase close to the border can be attributed to inflow of CBW. We thus find that the greater availability of CBW did not offset the inflow of resident immigrants, $I_{i,t}$, but, to the contrary, led to a *crowding-in* of this group (see column 6 of Table 3).

In sum, we established that the free movement policy increased the supply of CBW and of other resident immigrants by about 10 percentage points by 2010 in municipalities within 0–15 minutes from the border. Many of these new CBW were highly educated. As expected, the increase in immigrants' supply was smaller 15–30 minutes from the border and was most pronounced after 2004, when the labor market in the BR was fully liberalized for CBW.

5 Labor market effects

5.1 Main results

In this section, we investigate whether the greater availability of (mainly highly educated) CBW depressed wages or employment opportunities of (highly-educated) natives. We analyze wage and employment outcomes of natives jointly as they represent different margins of adjustment to the shock,

Table 3: Decomposition of effect of free movement policy by immigrant type

Dependent variable: Number of immigrants by type relative to total employment 1998

	# total immigrants by education group					# resident
	all		high	lower	# CBW	immigrants
	(1)	(2)	(3)	(4)	(5)	(6)
$Transition_t \cdot I(d_i \leq 15)$	0.001	-0.001	0.007***	-0.006	-0.001	0.002
	(0.008)	(0.007)	(0.003)	(0.007)	(0.008)	(0.007)
$Transition_t \cdot I(15 < d_i \leq 30)$	0.002	0.000	0.004*	-0.002	0.002	-0.001
	(0.008)	(0.006)	(0.002)	(0.007)	(0.004)	(0.007)
$Free_t \cdot I(d_i \leq 15)$	0.056***	0.049***	0.038***	0.018**	0.038***	0.018**
	(0.014)	(0.013)	(0.007)	(0.009)	(0.013)	(0.008)
$Free_t \cdot I(15 < d_i \leq 30)$	0.022***	0.016**	0.011***	0.012*	0.011**	0.012*
	(0.008)	(0.007)	(0.004)	(0.006)	(0.005)	(0.007)
R-squared	0.517	0.488	0.548	0.464	0.545	0.332
Observations	9585	14598	9585	9585	9585	9585
# Clusters	72	105	72	72	72	72
Including NBR Sample		√				
Year/Area fixed effects	√	√	√	√	√	√
Nuts II trend	√	√	√	√	√	√

Notes: ***, **, *, denote statistical significance at the 1%, 5% and 10% level, respectively. Robust standard errors, clustered by commuting zone, are given in parentheses. $Transition_t$ is one for the period between 2000 and 2003, whereas $Free_t$ is one from year 2004 onward. $(d_i \leq x)$ and $(y < d_i \leq z)$ indicate whether a municipality is located less than x travel minutes or between y and z travel minutes from the next border crossing, respectively. Regressions are weighted using the total workforce in 1998 in a cell. SESS data.

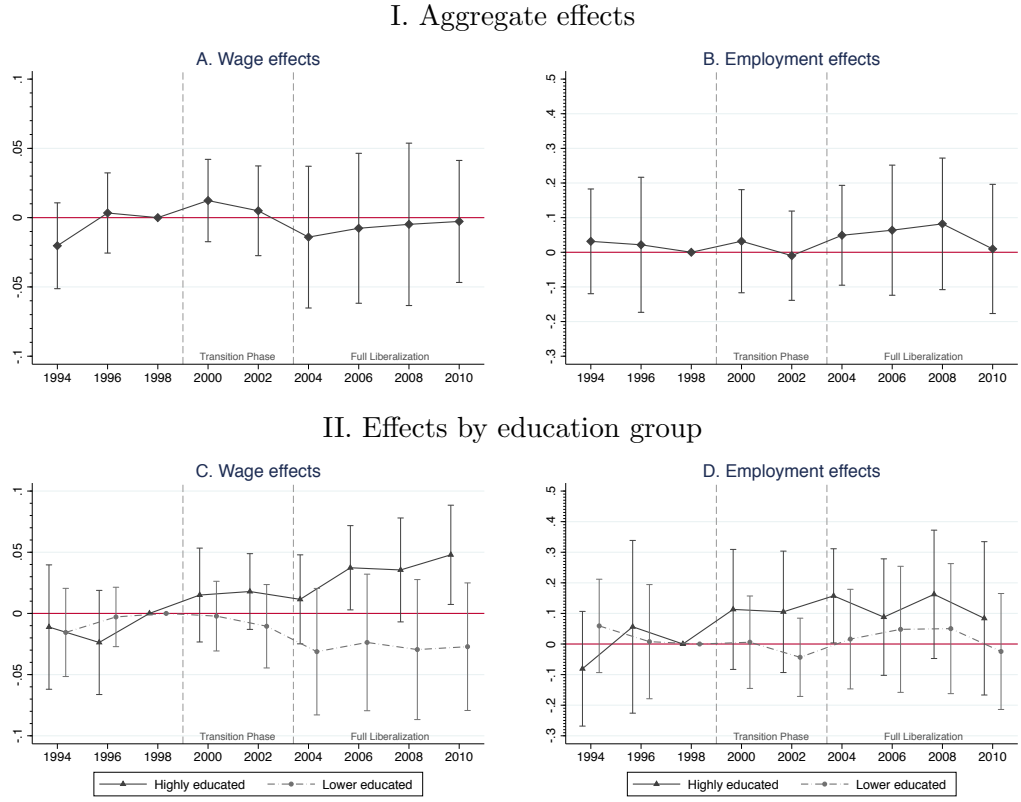
potentially heterogenous across groups due to group-specific labor supply elasticities or wage rigidities (see Dustmann et al., 2016, for a discussion).

Part I of Figure 4 provides first evidence of the average wage and employment effects. It plots the estimates of the year-interactions for the highly exposed regions (0–15 minutes) from equation (3) using average log hourly wages (Panel A) and log total native workers (Panel B) as dependent variables, respectively. Table A.5 presents the corresponding point estimates from our baseline DiD model (equation 1) using the logarithms of real hourly wages, total workers, and total full-time equivalent (FTE) workers as dependent variables. The table shows the results if we focus on the BR and if we include municipalities in the NBR to the control group.

Part I of Figure 4 shows that both natives' wages and employment evolved similarly in the treatment group and in the control group prior to 1999. This remains true in the reform period, too: the estimated reform effects are never significant for both outcomes. Consistently, most point estimates in Panel A of Table A.5 for average wages, employment and FTE workers are small and not statistically significant. Hence, we neither find evidence of a

negative effect on average native wages nor on native employment despite the substantial increase in immigrant employment in highly treated regions. Establishment-level regressions based on data from the business censuses strengthen this view: we do not find evidence that the substantial increase in employment of foreigners within establishments crowded out Swiss nationals (see Figure A.1 in the appendix).

Figure 4: Event analysis of effect of free movement policy on wages and employment of natives by education group



Notes: The figure plots the coefficients (and the 95% confidence interval) of the 0–15 minutes bin-year interactions of a regression based on equation (3) with the average log hourly wage (Panel A and C) and log total workers (Panel B and D) of an education group of native workers as dependent variable. Regressions are weighted using the number of natives in a cell and include NUTS-II trends. Standard errors are clustered on the CZ level. The sample includes only municipalities in the border region. SESS data.

In Panels C and D of Figure 4, we look at the impacts on highly and lower educated native workers separately. The estimates represent the "total" effect of immigrants on wages and employment of each education group of natives. They capture the impact on natives both from competition with immigrants with similar skills and from complementarity to those with dif-

ferent skills.[20] As documented above, the free movement policy produced a larger inflow of CBW with tertiary education relative to those with lower qualifications. Hence, the canonical "partial effects" model would imply downward pressure on wages and employment of highly-educated natives and possibly positive effects on less educated natives through complementarity.

This is not what we find. Rather, Panel C of Figure 4 suggests a *positive* effect on wages of highly educated natives that starts in the transition phase and grows to +4.5% in the free movement period. This positive wage effect is evident using both control groups—it is very similar in both specifications reported in Panel B of Table A.5—, robust, and economically meaningful: real wages of highly educated natives grew by only 3%, on average, in the BR between 1998 and 2010. Consistent with the view that highly educated natives gained from the reform, Table A.5 provides evidence that the free movement policy increased employment and FTE employment of highly educated natives in the high-treatment regions, and also in the slightly treated regions (the 15–30 minutes bin). On the other side, we find no statistically significant evidence that the reform had an effect on any of those outcomes of lower educated natives. The estimated employment effects by education group are imprecisely estimated but in general rule out strong negative employment effects. If anything, they are consistent with a crowding-in of highly educated native workers.

Overall, highly educated natives appear to have gained from the increased availability of mostly highly educated CBW. This evidence is difficult to explain in a labor market framework where immigration represents an increase in labor supply that meets a fixed labor demand. We will develop this point below. First, however, we discuss a series of important empirical checks for these labor market results.

[20] See Ottaviano and Peri (2012) for a more formal argument about the estimation of a total effect of immigrants aggregating all the direct competition and indirect complementarity effects from different skill groups.

5.2 Robustness of labor market results

Probably the most important concern in the causal interpretation of our DiD results is that the effects found are caused by differential trends in the outcomes across regions or by other unobserved factors. Potential confounders are changes in regional policies, unobserved demand and productivity shocks that occurred at the time of the reform in the treated and not in the control region due, for instance, to their different industrial composition. We address these concerns in several ways. In Panel B of Table A.7, we show that the estimated labor market effects are very close to the baseline estimates across the different outcomes if we include a Bartik (1991) variable, which controls for region-specific, sector-driven demand trends or shocks.[21] Similarly, Panel C shows that the results are similar when we control for unobserved region-specific shocks using an interaction between NUTS-II regions and year dummies. Below, we also present evidence that our results are indeed attributable to increased labor mobility rather than caused by other policies, e.g. one of the other bilateral agreements which were signed at the same time as the free movement of persons agreement (see section 7.5).

Another important concern with our labor market results is that they are affected by reform effects on the composition of native workers rather than by actual effects on workers that remain employed in the regions of interest. In particular, natives may respond to the greater availability of CBW by leaving the highly treated regions or by leaving the labor market, which would attenuate possible displacement effects (Borjas, 2006; Dustmann et al., 2017). It is, however, difficult to come up with consistent explanations how composition changes could explain our main result: the evidence that wages *and* employment of highly educated native workers increased in the highly treated regions. One possible explanation is a substantial relocation of high-wage natives from the control to the treatment regions. We investigate

[21] The basic intuition is to control for regional changes in employment or wages (by skill group) which are due to national-level changes in industries that are strongly represented in a particular region. See Appendix B.3 on the construction of this variable.

whether the reform influenced worker flows in and out of employment and in and out of regions in Appendix C. One caveat of the SESS data is that it does not allow to follow individual workers over time. For this reason, this analysis is based on a complementary data set, the Swiss Labor Force Surveys, the Swiss equivalent to the Current Population Survey. The SLFS allows us tracking workers' place of work and employment status across years. Overall, the estimates provide no evidence that the greater availability of CBW affected inflows or outflows of natives. In particular, we find no evidence for an increase in job-to-job transitions from the control to the treatment region. If anything, the point estimates suggest a small *reduction* in inflows into the highly treated regions.

Beerli and Peri (2018) provide several further robustness checks. Most importantly, the paper shows that the results are similar if we only compare changes in outcomes in municipalities in the highly affected area (0-15 min) with outcomes in matched control municipalities that are similar in terms of predetermined characteristics (an approach also followed by Dustmann et al., 2017). The labor market results are also similar if skills are measured using occupation (high-, middle- and low-paying occupations) instead of educational groups.

6 Theoretical framework

Our empirical findings show that the greater availability of CBW in the regions close to the border was associated with increased employment of CBW, especially highly educated ones. Despite this fact, reform led to an increase in wages and possibly employment of highly educated native workers. This is counter to interpreting the reform as a simple increase in the local supply of high-skilled workers within the frame of a canonical model where nothing else changes. A simple conceptual framework based on a local aggregate production function allows us discussing several plausible channels through which firms may have responded to the increased availability of CBW, gen-

erating the observed increase in demand for skilled natives.

Consider the production of an aggregate tradeable good Y_{ct} in local area c and in year t.[22] This good is obtained as a function of local productivity A_{ct}, skilled and unskilled local labor inputs, H_{ct}, L_{ct}, and their specific productivity/efficiency levels, θ_{ct}^H, θ_{ct}^L, and of physical capital K_{ct}. Such a production function can be written as follows:

$$Y_{ct} = A_{ct} F\left(\theta_{ct}^H H_{ct}, \theta_{ct}^L L_{ct}\right)^{\alpha} K_{ct}^{1-\alpha} \tag{3}$$

In equation (3), $F()$ is a constant return to scale labor aggregate consisting of skilled (H) and unskilled (L) labor. The parameter α is the elasticity of output to the labor aggregate. The two terms H_{ct} and L_{ct} each represent an aggregate of native (N) and foreign-born (F) workers. They can be written as $H_{ct} = H(H_{ct}^N, H_{ct}^F)$ and $L_{ct} = L(L_{ct}^N, L_{ct}^F)$ with the functions H and L exhibiting constant returns to scale.

Our results suggest that the main consequence of the opening of the border was to increase the availability of skilled CBW in regions close to the border. In our framework, this can be modelled as an increase in H_{ct}^F. The canonical model analyzes such a change assuming that *all other terms in the production function* (3) *are unchanged* and that *high skilled immigrants are perfect substitutes for high skilled natives* (such that $H_{ct} = H_{ct}^N + H_{ct}^F$). Under these assumptions, the marginal product of skilled workers, $MPH = \frac{\partial Y_{ct}}{\partial H_{ct}}$, is decreasing in H_{ct}^F, and an increase in H_{ct}^F lowers wages of native skilled workers—and/or employment, if wages are rigid. This is not what our empirical results indicate. The observed labor market effects of an increase in H_{ct}^F, instead, require that the greater availability of highly educated CBW had positive effects on (i) productivity, (ii) physical capital and/or (iii) skill-specific productivity. It is possible that the increase in H_{ct}^F affected labor demand through each of these components.

[22] We are assuming for simplicity that the price of the aggregate output Y_{ct}, a tradeable good, is not affected by local conditions and we standardize it to one.

First, there may be static and dynamic productivity effects from increasing the density of the workforce in general and the share of highly educated workers in particular in a location due to input sharing, labor pooling, or knowledge spillovers.[23] Similarly, an increase in H_{ct}^F may increase local productivity growth as highly qualified immigrants could be a direct input in the innovation process of firms, in the creation of new knowledge, or because they support the exchange of ideas (see Kerr and Lincoln, 2010; Peri et al., 2015b). In our framework, such effects can be modelled as $A_{ct} = A(h_{ct})$ where $h_{ct} = \frac{H_{ct}}{L_{ct}}$ and $\frac{\partial A}{\partial h_{ct}} > 0$. The productivity effects from the increase in H_{ct} would imply a positive effect on wages of *all* workers in the area.

Second, an increase in H_{ct}^F may attract firms and induce firm-creation and investment (Kerr et al., 2015; Olney, 2013). If capital and skilled workers are complements (Krusell et al., 2000), the resulting increase in physical capital in the area, K_{ct}, would mainly increase wages of skilled native workers.

Third, an increase in H_{ct}^F may trigger firms to adopt technologies that are relatively productive in the skills supplied by immigrants (e.g., Peri, 2012; Lewis, 2011). Similar effects could be caused by better skill to task allocation between natives and immigrants, which in turn may cause efficiency gains. Several studies have shown that skill-to-task specialization of immigrants and the response of natives can mediate the labor market effects of immigration (Peri and Sparber, 2009).[24] As we highlighted, many new CBW were employed in scientific and technical jobs. Highly educated Swiss workers thus have an incentive to move towards the higher end of managerial jobs. In our framework, greater task specialization would imply an increase in the skill-specific productivity parameter θ_{ct}^H, generating a positive effect on the marginal productivity of high-skilled natives. Moreover, if skilled CBW and

[23] Moretti (2004) and Diamond (2016), among others, show that a larger share of college educated workers increases labor productivity in US cities. Glaeser and Mare (2001) show that this may be in part due to dynamic local learning, Iranzo and Peri (2009) argue that this may be due to faster technological adoption.

[24] For instance, Cattaneo et al. (2015) for countries in the European Union and in Foged and Peri (2016) for Denmark have shown that native workers upgrade their occupations in response to immigration by moving into more complex types of jobs.

native specialize in different jobs, this would attenuate possible negative effects of H_{ct}^F on the marginal productivity of all native workers by reducing the substitutability between H_{ct}^N and H_{ct}^F (Ottaviano and Peri, 2012; Peri and Sparber, 2011).

The framework highlights that an increase in wages and employment of highly educated natives is consistent with a greater availability of skilled CBW if the latter generates productivity enhancing skill-to-task specialization, productivity effects, innovation, and firm entry or capital accumulation.[25] The next section examines the empirical relevance of these mechanisms at the firm level, as the underlying theoretical mechanisms operate at the firm level. We expect that many of the above mechanisms are stronger in firms that use the skills of CBW more intensively. This heterogeneity could be captured in our framework by firm-specific labor-aggregator functions $F_i(.;.)$ with different elasticity to H across firms. Specifically, we expect a stronger response to the increased availability of skilled CBW in firms with larger values of θ_{ct}^H, the "skill bias" in production.

7 Mechanisms

This section proceeds as follows. We first document that the positive demand effects of the reform were indeed largest in skill-intensive sectors (section 7.1). We then document that increased productivity (section 7.2), innovation (section 7.3), firm entry (section 7.4), and task specialization (section 7.6) were also important margins of adjustment, especially for firms with high demand of skilled workers.

Before turning to the results of these analyses, a couple of remarks on the firm-level regression models are warranted.[26] First, the unit of observa-

[25] Some possible effects of immigrants on demand for native workers are not captured by the framework above. Chassambouli and Palivos (2014) and others focus on the role of immigrants in stimulating job creation by firms in search models. If the surplus produced by immigrants for the firm is larger than that created by natives, their presence may encourage job creation for natives, too.

[26] Section B.2 in the appendix contains a detailed discussion on the construction of our firm-/establishment level estimation samples.

tion in the regressions is an establishment (in the Business Census, BC) or a firm (in the Innovation Surveys, IS). Consequently, the unit fixed effects of equations (1) and (3) represent establishment or firm fixed effects. They imply that we focus on within-firm or within-establishment variation. This is important, as the free movement policy led to the creation of new establishments in the heavily affected regions. The firm fixed effects thus help to isolate changes taking place within incumbent firms from those due to changes in the composition of firms.[27] Second, in order to ensure that our results are comparable to the labor market results, we focus on private-sector establishments and drop establishments with less than three FTE workers in 1998.[28] An additional benefit of dropping microfirms is that it helps to limit the non-trivial problems that arise in regressions based on log employment of very small firms.[29] Third, in order to get estimates representative for the average worker, regressions based on the BC use an establishment's average size over the sample as weight. However, in the IS, we prefer to show regressions that are not weighted by firm size.[30] Fourth, in both firm datasets, we discard a very small number of extreme outliers that strongly affect the precision (but not the size) of the estimates. Finally, our baseline strategy

[27]To give a concrete example, most new entrants are smaller than the average incumbent. The entry hence reduces *average* establishment size in highly affected regions. A regression without firm fixed effects would convolute the size effect due to firm entry with possible effects on incumbents. More restrictive alternatives to estimate effects on incumbents (such as a balancing the panel or focusing on a fixed cross-section of firms) lead to similar results (see Ruffner and Siegenthaler, 2017).

[28]In case an establishment is not present in 1998, we use establishment size in the first period that it is observed.

[29]In the BC, almost 55% of all establishments have fewer than 3 FTE worker. The main concern when including these establishments is a mechanical negative correlation between their initial size and subsequent growth (see Mata, 1994). Ruffner and Siegenthaler (2017) contains an extended discussion of this issue and regressions using FTE employment in levels rather than logs as dependent variable including these establishments with less than three FTEs, as the mechanical correlation is less pronounced in levels. This leads to quantitatively and qualitatively similar results.

[30]Firm-size weights give a large weight to very few, large multi-establishment firms. Large multi-establishment firms are also those firms whose location we measure with the largest error in this dataset, as discussed in section 3.1. If we weight observations by firm size in the IS, the effects have the same sign but are usually larger than those reported here. But they are also more sensitive to the choice of specification (see Ruffner and Siegenthaler, 2017).

remains to focus on firms in the BR. We present the results including firms in the NBR as controls in the robustness section (Panel A of Table A.12).

7.1 Effects by skill-intensity of industries

This section assesses a first prediction from our theoretical considerations: the complementarity and productivity effects arising from the inflow of skilled CBW should be stronger in industries that depend on skilled worker. To test this, we differentiate between low- and high-tech manufacturing and between knowledge-intensive (finance, business, human resource management) and more traditional service industries. We expect that high-tech manufacturers and knowledge-intensive services are more dependent on free access to CBW, many of whom work as scientists, engineers, or IT professionals.

We first show that the skill-intensive sectors were indeed responsible for the increase in employment of foreign workers in the highly treated regions. Panel A of Figure 5 provides establishment-level estimations of equation (3) using the BC. The dependent variable is the number foreign workers (i.e., resident immigrants plus CBW) as a share of FTE employment in 1998. Because the censuses in 1991 and 2011 do not contain information on workers' nationality, the estimations are based on the census waves 1995–2008 only. The results confirm that employment of foreigners increased most within high-tech manufacturers and in establishments in the knowledge-intensive service sector. The increase was much smaller in low-tech manufacturing and in more traditional service industries. Column 1 of Table 5 presents an estimate of the reform effect at the establishment-level, averaged across all industries, on the share of foreign employment using our baseline model (equation 1). This confirms disproportionate effect of the policy on the employment of foreigners close to the border found above.

We now test whether the evidence that the reform stimulated labor demand is most visible in the two skill-intensive sectors. As a first step, Table 4 reports separate wage regressions for the four sectors using our preferred municipal-level wage regressions. As expected, the wage gains of natives

indeed arise in the skill-intensive sectors. In the knowledge-intensive service sector, these wage gains only accrue to highly educated natives. In high-tech manufacturing, we observe wage gains for lower educated natives, suggesting complementarity between lower educated natives and CBW. In contrast, we find no wage increases in the two sectors that are not skill-intensive. In fact, there is some evidence for non-negligible wage losses among the lower educated natives in non-knowledge intensive service industries. This result suggests the potential for negative wage effects from a greater availability of CBW in sectors in which there is less scope for counteracting demand effects.

Table 4: Effect of the free movement policy on wage levels of natives by sector of employment

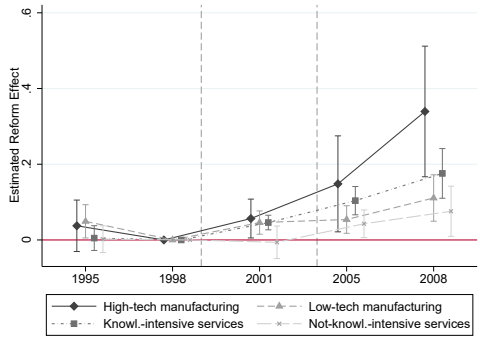
Dependent variable: mean log hourly wage of natives by education group

Category of employment	Manufacturing		Services	
	High-tech	Low-tech	Knowl.-intensive	Not-Knowl. intensive
	(1)	(2)	(3)	(4)
	A. All education groups			
$Free_t \cdot I(d_m \leq 15)$	0.050***	0.015	0.040**	-0.028
	(0.016)	(0.018)	(0.017)	(0.022)
$Free_t \cdot I(15 < d_m \leq 30)$	0.016	0.014	0.024*	-0.018
	(0.017)	(0.016)	(0.012)	(0.014)
	B. Highly educated			
$Free_t \cdot I(d_m \leq 15)$	0.001	-0.032	0.083***	0.031
	(0.015)	(0.034)	(0.017)	(0.020)
$Free_t \cdot I(15 < d_m \leq 30)$	0.030**	-0.011	0.029*	-0.014
	(0.014)	(0.024)	(0.016)	(0.029)
	C. Lower educated			
$Free_t \cdot I(d_m \leq 15)$	0.042***	0.020	0.009	-0.037*
	(0.014)	(0.018)	(0.017)	(0.020)
$Free_t \cdot I(15 < d_m \leq 30)$	0.005	0.016	0.008	-0.029**
	(0.017)	(0.017)	(0.012)	(0.014)
Year and area fixed effects	√	√	√	√
Nuts II trends	√	√	√	√

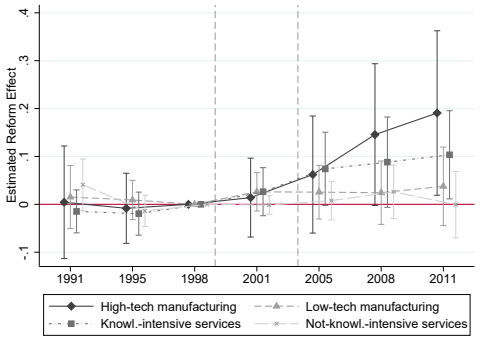
Notes: ***, **, *, denote statistical significance at the 1%, 5% and 10% level, respectively. Robust standard errors, clustered by commuting zone, are given in parentheses. $Free_t$ is one from year 2004 onward. $(d_i \leq x)$ and $(y < d_i \leq z)$ indicate whether a municipality is located less than x travel minutes or between y and z travel minutes from the next border crossing, respectively. Distance interactions with the transition phase are omitted for brevity. Regressions are weighted using the total number of natives in a cell. High-tech manufacturing is NACE Rev 1.1 industries 24, 29, 30, 31, 32, 33, 34 and 35 excluding 35.1. Low-tech manufacturers are the remainder manufacturing categories. Knowledge-intensive services are NACE Rev 1.1 industries 61, 62, 64, 65-67, 70-74, 80, 85, 92. Not knowledge-intensive services are the remainder service sector categories. SESS data.

Figure 5: Effect of free movement policy on the foreign employment share and establishment size, by broad sector (business censuses)

A. Effect on foreign employment share (1995–2008)



B. Effect on establishment size (1991–2011)



Notes: The figure plots the coefficients and the associated 95% confidence intervals for the 0-15 minutes bin of individual regressions based on equation (3) using private-sector establishment-level data from the BC. The regressions control for establishment fixed effects, year fixed effects, and NUTS-II trends. The sample is restricted to the BR. In Panel A, the dependent variables is FTE employment of foreigners as a share of total employment in 1998, estimated separately by establishments' broad sector of economic activity. In Panel B, the dependent variable is log FTE employment. The regressions are weighted using the average establishment size (in FTE). Standard errors are clustered by commuting zone.

As a second step, Panel B of Figure 5 tests for a disproportionate effect on firm expansion in the skill-intensive sectors. Consistent with our previous evidence, the results suggest that the policy changes had a much larger impact on the size of incumbent establishments in the two skill-intensive sectors. The size effect in these two sectors explains why the reform affected the size of the *average* incumbent establishment in the highly treated regions. In our baseline DiD model, this average effect amounts to 6.4% in the free movement period (Column 2 of Table 5). Our preferred estimations using the IS data yield reform effects on the size of incumbent firms (rather than establishments) that are even somewhat larger (see column 3 of Table 5).

7.2 Firm productivity

Our theoretical considerations suggest that the positive wage effects on highly educated natives may be due to productivity gains in Swiss firms close to the border. We assess this mechanism using data on firm-level sales and value added per FTE worker from the IS 1996–2013. Columns 4 and 5 of Table 5 present the results of firm-level regressions of equation (1) us-

Table 5: Effect of free movement policy on various firm outcomes

Dependent variable	Foreign share	Establ. size (FTE)	Firm size (FTE)	Sales	Productivity	Patent appl. 0/1	Patent appl. count
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$Transition_t * I(d_i \leq 15)$	0.023*	0.019	0.027	-0.001	-0.006	0.019	0.042
	(0.012)	(0.014)	(0.034)	(0.036)	(0.036)	(0.018)	(0.028)
$Transition_t * I(15 < d_i \leq 30)$	0.020***	0.033***	0.046	-0.004	-0.055	0.007	-0.016
	(0.007)	(0.011)	(0.030)	(0.034)	(0.034)	(0.014)	(0.029)
$Free_t * I(d_i \leq 15)$	0.107***	0.064***	0.098**	0.118**	0.036	0.066**	0.124**
	(0.018)	(0.021)	(0.047)	(0.050)	(0.035)	(0.027)	(0.048)
$Free_t * I(15 < d_i \leq 30)$	0.042***	0.058***	0.092*	0.050	-0.046	0.016	0.035
	(0.011)	(0.018)	(0.048)	(0.045)	(0.039)	(0.024)	(0.052)
Observations	474,739	674,523	9,467	8,660	7,276	9,243	9,107
R-squared	0.612	0.951	0.966	0.973	0.733	0.708	0.817
Dataset	BC	BC	IS	IS	IS	IS	IS
Sample period	95–08	91–11	95–12	95–12	95–12	95–12	95–12
Period effects	√	√	√	√	√	√	√
Firm/establishment effects	√	√	√	√	√	√	√
Nuts-II trends	√	√	√	√	√	√	√
Weights	√	√					
Number of clusters	73	73	73	73	72	73	73

Notes: ***, **, *, denote statistical significance at the 1%, 5% and 10% level, respectively. Standard errors are clustered by commuting zone. The table presents results of establishment- and firm-level DiD regressions using the BC (columns 1–2) and the IS (columns 3–7). All regressions account for establishment (BC) or firm (IS) fixed effects, period fixed effects, and linear trends per NUTS-II region. The dependent variable in column 1 is full-time equivalent (FTE) employment of foreigners as a share of total employment in 1998. The dependent variable in column 2 is log FTE employment. The dependent variables in columns 3–5 are firms' log FTE employment, log total sales, and log value added per FTE worker. The dependent variable in column 6 is a dummy equal to 1 if a firm filed at least one patent application in the three years preceding the survey. Column 7 uses the Inverse Hyperbolic Sine (IHS) of the number of patent applications. $Transition_t$ is a dummy equal to one between 1999 and 2003, whereas $Free_t$ is one from year 2004 onward. $I(d_i \leq x)$ and $I(y < d_i \leq z)$ indicate whether a firm is located less than x travel minutes or between y and z travel minutes from the next border crossing, respectively. The regressions in columns 1 and 2 are weighted using the average establishment size (in FTE) as weight.

ing the log of these variables as dependent variables. Column 4 suggests that the free movement policy increased sales of highly treated incumbent firms by almost 12%. We do not find statistically significant evidence that the liberalizations affected labor productivity of the average firm (column 5 of Table 5), however, despite its sizeable impact on sales. Figure 6 shows why. It compares the timing of the reform effects on highly treated incumbent firms in the IS using the flexible event study model (equation 3). We observe that the sales effect mirrors size and timing of the effect on FTE employment very closely. In the average firm, sales value added per FTE worker thus remained more or less constant.

According to our theoretical considerations, however, we mainly expect firm-level productivity gains in firms using skilled workers more intensively. Given the evidence of positive wage effects in skill-intensive sectors, Panel A

Figure 6: Effect on firm size, sales and labor productivity in the innovation surveys (IS)

Notes: The figure plots the coefficients and the associated 95% confidence intervals for the 0-15 minutes bin of individual regressions based on equation (3) using private-sector firm-level data from the IS 1996–2013. The (unweighted) regressions control for firm fixed effects, year fixed effects, and NUTS-II trends. The sample is restricted to the BR. The dependent variables are firms' log FTE employment, log total sales, and log value added per FTE worker. Standard errors are clustered by commuting zone.

of Table 6 explores heterogeneity by sector. In the first two columns, we estimate separate reform effects on sales and productivity by fully interacting indicator variables for certain characteristics of interest with all treatment indicators. In the table, we focus on the effects on the highly treated incumbent firms, captured by the interaction term on $Free_t * I(d_i \leq 15)$. Panel A provides evidence for evidence for positive effects on labor productivity in high-tech manufacturers and, somewhat weaker, knowledge-intensive business service firms. The other panels of Table 6 reveal positive productivity effects in larger firms—which had accounted for most of the employment of CBW before the reform (Panel B)—and in firms belonging to an industry that had a CBW share of at least 13% in 1998 in the BR according to the SSES. Overall, the results indeed suggest productivity gains in industries depending on the skills of CBW.

In Figure 7, we exploit that the IS data allow identifying two additional margins of firm-level heterogeneity. In Panel A, we identify firms that per-

Table 6: Heterogeneity of firm-level effects of free movement policy

Dependent variable	Sales	Productivity	Patent count
	(1)	(2)	(3)
Panel A: KIBS vs. high-tech vs. others			
$Free_t * I(d_i \leq 15)$	0.067	-0.034	0.132***
	(0.056)	(0.036)	(0.040)
$Free_t * I(d_i \leq 15)*$ KIBS	0.164	0.135**	-0.018
	(0.129)	(0.057)	(0.054)
$Free_t * I(d_i \leq 15)*$ High-tech manufacturer	0.120	0.225***	-0.025
	(0.091)	(0.066)	(0.087)
Panel B: Firm size			
$Free_t * I(d_i \leq 15)$	0.134**	-0.002	0.127***
	(0.059)	(0.042)	(0.045)
$Free_t * I(d_i \leq 15)* I(FTE \geq 100)$	-0.043	0.094***	-0.006
	(0.060)	(0.034)	(0.060)
Panel C: High vs. low CBW share			
$Free_t * I(d_i \leq 15)$	0.097*	-0.001	0.132***
	(0.050)	(0.032)	(0.047)
$Free_t * I(d_i \leq 15)*$ CBW share $\geq 13\%$	0.125	0.252**	-0.059
	(0.100)	(0.118)	(0.091)
Panel D: Export status			
$Free_t * I(d_i \leq 15)$	0.137**	-0.021	0.103**
	(0.054)	(0.044)	(0.039)
$Free_t * I(d_i \leq 15)*$ Exporter	-0.005	0.065	0.062
	(0.047)	(0.041)	(0.060)

Notes: ***, **, *, denote statistical significance at the 1%, 5% and 10% level, respectively. Standard errors are clustered by commuting zone. Each panel contains separate regressions of our baseline firm-level DiD model using the IS 1996–2013, augmented with one or several interactions between indicators for subgroups of firms and $Transition_t * I(d_i \leq 15)$, $Transition_t * I(15 < d_i \leq 30)$, $Free_t * I(d_i \leq 15)$, and $Free_t * I(15 < d_i \leq 30)$. In the table, we focus on the effects on highly treated firms in the free movement phase. In Panel A, the indicators refer to knowledge-intensive service industries (KIBS) and high-tech manufacturers (defined as NACE rev. 1.1 industries 24 and 29–35 excluding 35.1). In the other panels, the indicators refer to firm size (in terms of FTE employment, Panel B), whether the firm operates in an industry with a cross-border worker share of at least 13 % in 1998 in the BR (panel C), and firms' export share in sales (Panel D). All regressions account for firm fixed effects, period fixed effects, and linear trends per Nuts-II region. The dependent variable in column 1 is firms' log total sales. The dependent variable in column 2 is firms' log value added per FTE worker. The dependent variable in column 3 is the inverse hyperbolic sine of the number of patent application filed by the firm in the three years preceding the survey. $Free_t$ is a dummy equal to one from year 2004 onward. $I(d_i \leq x)$ indicate whether a firm is located less than x travel minutes from the next border crossing.

ceived to suffer from skill shortages *ex ante*. Firms in the IS were explicitly asked whether they think that their innovation efforts are negatively affected by a *"shortage of specialized personnel"*. We average the 5-point Likert scale survey item over the two survey waves prior to the reform for each firm and subsequently group them into three categories, from "no shortage" to "high

shortage".[31] As in Table 6, we augment equation (1) with interaction terms between the four treatment interactions and dummy variables associated with shortage. The figure shows that there is only weak evidence that the sales effect of the free movement policy was larger among highly treated firms that suffered from substantial skill shortages prior to the reform. However, as expected, we find strong evidence that these firms experienced substantial productivity gains with the greater availability of CBW.

In Panel B of Figure 7, we leverage a similar subjective survey question to differentiate between firms that differed in the extent to which they reported that their innovation activities were hampered by *"labor market regulation for foreigners"* prior to the policy changes. The figure suggests that relaxing this obstacle spurred productivity growth in firms that had perceived to be constrained by this regulation. Indeed, in appendix table A.16, we provide evidence that the reform reduced the probability that firms perceive to be hampered by this type of regulation, suggesting that the liberalizations reduced the costs of hiring foreigners close to the border.

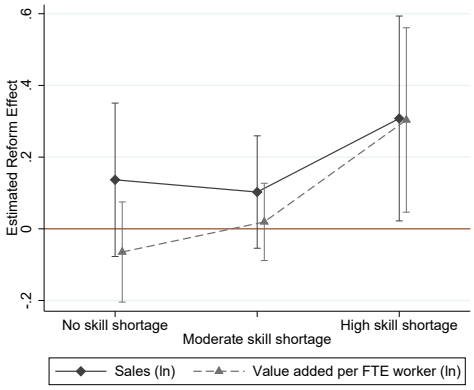
Consistent with our theoretical framework, our results thus suggest positive productivity effects of the reform on incumbent firms operating in skill-intensive industries and in industries that relied heavily on CBW before the reform, and in a distinct set[32] of incumbent firms that experienced skill shortages or felt hampered by labor market regulation for foreigners prior to the reform. Overall, these findings provide support for our theoretical prediction that the increased availability of CBW increased productivity of firms in high demand of the skill set of CBW. They also resemble the results from studies on the impacts of H-1B workers on US firms, which typically find that changes in the number of H-1B visas affect productivity in firms and regions that rely strongly on H-1B workers (Ghosh et al., 2014; Kerr

[31] In particular, firms that have "no shortage" are firms with a less than 2, "moderate shortage" firms have a value between 2 and 4, and "high shortage" firms have a value greater than or equal to 4.

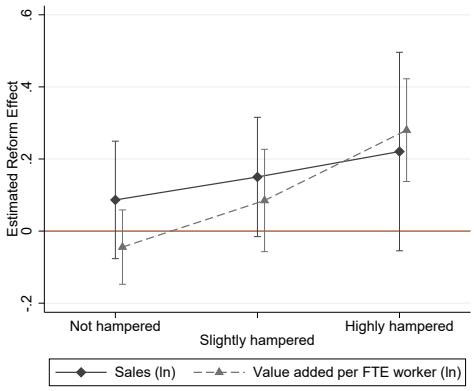
[32] There is almost no correlation between the industry-level indicators used in Panels A and C of Table 6 and the firm-level indicators used in Figure 7.

Figure 7: Effect of free movement policy by pre-reform problems to find skilled workers

A. By initial skill shortage



B. By perceived labor market regulation of foreigners



Notes: The figure illustrates regressions based on our baseline regression model (equation 1), augmented with interactions between our main treatment indicators and indicators of labor shortage prior to the policy change. The figure shows the coefficients and the associated 95% confidence intervals on the interaction terms between $Free_t \times I(d_i < 15)$ and these indicators. The regressions are based on firm-level data from the IS 1996–2013. We control for firm fixed effects, year fixed effects, and NUTS-II trends. The sample is restricted to the BR. The dependent variables are firms' log total sales and log value added per FTE worker. In Panel A, we differentiate firms that differed in the extent to which they reported that their innovation efforts were negatively affected by a shortage of specialized personnel. This information is taken from the IS 1996 and or 1999. Similarly, in Panel B, firms are differentiated by the extent to which their innovation efforts were negatively affected by "labor market regulation for foreigners" prior to the reform. Standard errors are clustered by commuting zone.

and Lincoln, 2010; Peri et al., 2015a).

7.3 Innovation

According to our theoretical framework, the positive effects of the reform on productivity and native wages may be the consequence of positive effects of highly skilled immigrants on the innovation activities of firms. This is the mechanism that we study in this section. We start by documenting that the additional immigrants indeed played an important role in the growth of research and development activities of firms close to the border. Using data from the SSES, Panel A of Figure 8 plots the share of researchers in total employment depending on the travel distance to the nearest border crossing.[33] The figure separates Swiss nationals, CBW, and all other foreigners (long- and short-term immigrants). It illustrates that CBW researchers represent

[33]The surveys allows distinguishes 24 different occupational categories. One category is "research and development". Using this information, we calculate the researcher share in total FTE employment in every establishment.

almost one third of all researchers in firms located within 15 minutes to the border. As for CBW in general, the presence of CBW researchers declines strongly with the distance to the nearest border crossing. The figure also documents a clear increase in the CBW researcher share between 2000 and 2010, concentrated close to the border. The figure suggests that the free movement of workers increased the R&D employment share.

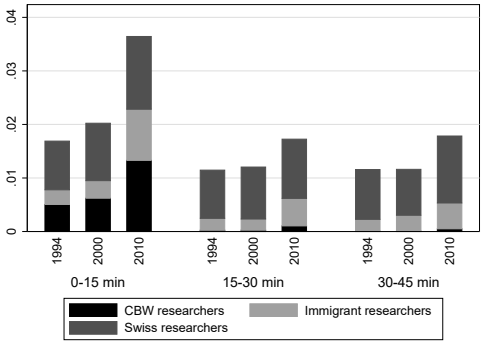
Our regressions' results are supportive of a causal effect of the reform on R&D employment and the R&D intensity of firms. At the municipal level, Table A.4 suggests that the reform increased the share of immigrant researchers in total employment by about 0.6 percentage points. Ruffner and Siegenthaler (2017) provide establishment-level regressions that suggest that the reform increased the share of researchers in total employment by about 1 percentage point. Interestingly, this increase in the R&D worker share is not only partly attributable to increased employment of CBW. Employment of other foreign R&D workers grew, too, suggesting complementarity between CBW and other foreign researchers. The evidence whether Swiss researchers were crowded in is mixed.

Did this increase in R&D employment translate into more inventions? This question is analyzed in columns 6 and 7 of Table 5 using data from the IS. In the surveys, firms are asked whether they filed a patent application in the three years before the survey, and if they did so, how many applications they filed. Column 6 uses this information in order to examine whether the reform affected the probability that a firm filed at least one application. Panel B of Figure 8 provides the corresponding event study results. In column 7, the dependent variable is the inverse hyperbolic sine (IHS) of the count of patent applications.[34] These regressions yield robust evidence

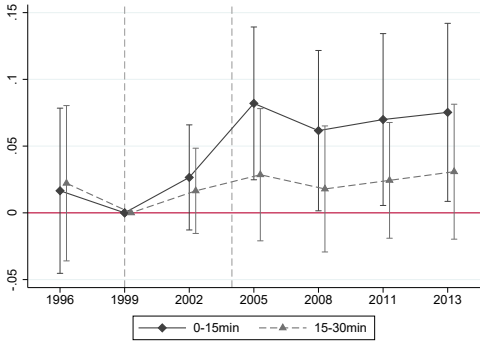
[34]The inverse hyperbolic sine (IHS) of the number of patents accounts for the substantial amount of firms without patents and the long right tail of the distribution. The IHS of outcome y is $IHS(y) = ln(y + \sqrt{1 + y^2})$. The estimated coefficients reflect the approximate percentage increase in y caused by the reform. As argued by Doran et al. (2015), using the IHS is attractive for innovation outcomes because it approximates the log of an outcome but has the advantage that it is defined at 0.

Figure 8: Effect of free movement policy on R&D employment and patenting

A. R&D employment share by distance to border in BR



B. Effect of free movement policy on probability of patent application



Notes: Panel A plots the employment share of workers in the occupation "research and development" by immigrant status and travel duration to the nearest border crossing, using data from the SSES in 1994, 2000, and 2010. Panel B plots the coefficients and the associated 95% confidence intervals for the 0-15 and 15-30 min distance bins of regressions based on equation (3) using firm-level data from the IS 1996–2013. The dependent variables is a dummy equal to 1 if a firm filed at least one patent application in the three years preceding the survey. The regression controls for firm fixed effects, year fixed effects, and NUTS-II trends. Standard errors are clustered by commuting zone. Both figures focus on private-sector firms in the BR.

of a sizeable positive reform effect on patent applications of highly treated firms.[35] The estimated effect on the probability to apply for a patent is 6.6 percentage points in the free movement phase (column 6 of Table 5). This positive effect can be observed across different groups of incumbent firms (as shown in column 3 of Table 6). Figure 8 shows that this effect arises mainly between 2002 and 2005 and that the trends in patent applications were similar before the reform and after 2005.

Table A.14 in the appendix adds further evidence that the reform stimulated firms' innovation performance. Using the IS, we show that the greater R&D efforts (both in terms of R&D employment and expenditures) and the higher propensity to invent resulted in a higher probability to realize product innovations and a higher share of innovative products in total sales. We also find that most of these effects are larger in firms that reported that their innovation efforts were affected by lack of R&D workers in the pre-treatment phase. Again, the information about lack of R&D personnel prior

[35] We find no effect on slightly treated firms, consistent with the fact that there are few CBW researchers in regions 15–30 minutes to the border throughout the sample period.

to the reform stem from a subjective survey question about obstacles to firms' innovation efforts in the IS 1996 and 1999.

In sum, we find positive effects from a greater access to CBW on innovation and patenting in incumbent firms, especially in firms that declared shortages of R&D workers before the policy implementation. These results add to an unsettled debate on whether inflows of skilled immigrants benefit high-skilled residents and scientists. Different studies on the impacts of H-1B workers or foreign-born scientists reach conflicting conclusions.[36] In particular, Borjas and Doran (2012) find that the strong influx of Russian mathematicians after the collapse of the Soviet Union had negative effects on publications and the academic positions of US mathematicians. As pointed out by Card and Peri (2016), immigrant and resident mathematicians likely compete for a fixed number of positions in the US in the short and medium run, which increases the scope for displacement. In the private sector context, however, as in the case of this paper, firms may react to the availability of R&D workers and increase the number of jobs for them in parallel. In fact, the next section provides evidence that firm growth may explain the absorption of the increased supply of CBW on the Swiss labor market more generally.

7.4 Firm entry

Our theoretical considerations suggest that the greater availability of CBW—or even the possibility to be able to hire them—may lead to capital investments and attract new establishments, producing the employment and wage growth of highly educated workers that we observe.[37] Due to the early an-

[36]The results in Ghosh et al. (2014) and Kerr and Lincoln (2010) suggest that greater access to H-1B workers generally increases the size, productivity, and innovation performance of firms that rely heavily on H-1B visas. Doran et al. (2015), on the other hand, find that winning an additional H-1B worker in the H-1B lotteries of 2006 and 2007 increased firms' profits, had no effect on patenting and firm size and crowded out resident workers. Similarly, Kerr et al. (2015) find that hiring young skilled immigrants increases firms' skill intensity, but their evidence regarding firm size is inconclusive.

[37]Theoretically, we expect that firms' entry and location choices under nonzero profits depend upon the same quantities as those that affect firms' sales and profits (Combes and Gobillon, 2015). The prospect of hiring the right type of workers can be a strong

nouncement and permanent nature of the reform studied here, such capital adjustment may have happened simultaneously to the increase in the supply of workers.

We study the role of capital deepening mainly by focusing on entry and exit of establishments as observed in the BC data. In every census wave, an establishment is considered a new entrant (an exiter) if its establishment identifier is new (disappears). The estimations are run at the municipality level and cover the period 1991–2008 in the case of firm entry and the period 1991–2011 for firm exit.[38] The results are presented in Figure 9 and in Table A.15 in the appendix. The outcome variable in Panel A of the figure is the number of establishments entering a municipality between BC waves $t-1$ and t relative to the number of establishments in 1998. The regression is weighted by the number of establishments in a municipality in 1998.

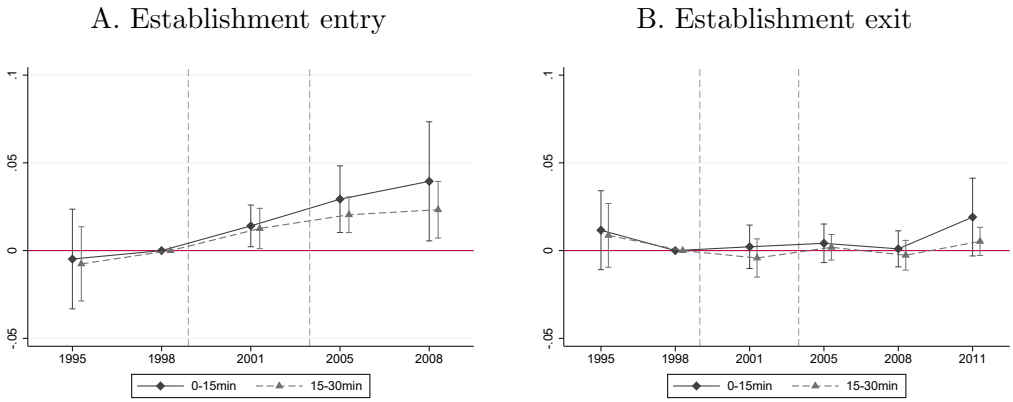
We find strong evidence of a positive effect of the reform on establishment entry. The figure suggests that the full liberalization increased the share of new establishments by roughly 4 percentage points in the highly treated region relative to 1998. This firm entry already starts during the transition phase and continues during the free movement phase. Columns 2–5 of Table A.15 show that the increase in firm-creation was most pronounced in high-tech manufacturing and in knowledge-intensive services, in line with our previous results. We find no evidence that the reform affected the number of establishment exits per municipality relative to the number of establishments in 1998 (see Panel B of Figure 9). This is reassuring, as it also limits the danger that our firm-level regressions are affected by biases due to selective attrition.[39]

attractor for firms and a key driver of agglomeration economies (as in Moretti, 2004).

[38]See reason for sample coverage and further comments on construction of these variables in Appendix B.2.

[39]The main concern is a possible survivorship bias. This bias could mean that we attribute too much—or not enough—of the reform effect to occur within firms rather than to the change in firms' composition. In Ruffner and Siegenthaler (2017), we present additional robustness checks that suggest that the results are not driven by a survivorship bias. Among others, we show that the estimated reform effects on the size of incumbent firms is positive and statistically different from 0 if we construct lower bounds on the

Figure 9: Effect of free movement policy on establishment entry and exit



Notes: The figure plots the coefficients and the associated 95% confidence intervals for the 0-15 and 15-30 min distance bins of a regression based on equation (3) using municipality-level data. In Panel A, the dependent variable is the number of new establishments in t as a fraction of the number of establishments in 1998. The estimation sample is based on the BC 1991–2008. In Panel B, the dependent variable is the number of establishments exiting between $t-1$ and t as a fraction of the number of establishments in 1998. The estimation sample is based on the BC 1991–2011. Regressions are weighted using the number of establishments in 1998 per municipality, include municipality and year fixed effects and NUTS-II trends, and are restricted to the BR. Standard errors are clustered by commuting zone.

In Ruffner and Siegenthaler (2017), we present two further findings that suggest that the reform affected firms' capital investment. First, we show that the greater availability of CBW affected the staffing decisions of multi-establishments firms. Comparing the growth of establishments within the same multi-establishment firm, we find larger growth rates of establishments close to the border as soon as the labor market is liberalized. Second, we present suggestive evidence that the reform led to a sizeable decrease in firms' propensity to outsource production and service tasks in the BR.

Overall, these results indicate that the greater access to CBW led to the creation of new establishments in the border regions. Importantly, the resulting increase in labor demand appears to have occurred simultaneously with the increase in the supply of CBW. The capital adjustment in the anticipatory phase of the reform likely were an important reason for the positive short-run effects on wages and employment of highly educated natives. At the same time, the results in this section raise the possibility that establishment entry in regions close to the border may have at least partly occurred *at the expense* of lower growth in other regions in the country. If firm simply

reform effect using a trimming procedure proposed by Lee (2009).

changed their location decisions, some of our effects could be relative rather than absolute. While we cannot rule this out, our results on the geographical mobility of workers does not suggest a significant displacement effect of workers between the border regions and regions further away from the border (see Appendix C).

7.5 Robustness of firm results

We perform a similar set of robustness checks regarding our firm results as in the case of the labor market results. We first address concerns that our estimates are confounded by unobserved region-, canton-, or industry-specific shocks. Panels B–D of Table A.12 show that our preferred firm-level results hold if we include full sets of industry-period, NUTS-II-period, and canton-period fixed effects. Moreover, we also checked directly whether there were other changes in the political and regulatory environment (e.g. taxes or market access to the EU) that changed differently between treated and control firms in our period of interest. To this end, we exploit that firms participating in the IS are asked whether a set of policy-related factors hampered their innovation efforts. We find no correlation between our variables of interest and any of these factors except one (see Table A.16): the probability that firms perceive that their innovation activities are hampered by "labor market regulation for foreigners," which is likely a direct consequence of the CBW liberalizations.

We conducted an extensive number of robustness checks to assure that the estimates of our reform effects are not confounded by potential effects from trade, changes in market access or more generally from the other bilateral agreements signed at the same time as the AFMP. One of these agreements is particularly relevant as it aimed at facilitating trade with the EU by reducing non-tariff barriers to trade. Although this agreement only affected specific groups of products, it may have spurred firm and productivity growth, and particularly so close to the border.

Several pieces of evidence suggest that neither trade flows, the exchange

rate, nor any of the other bilateral agreements drive our results.[40] First, the share of exporters (see Table 2) and importers do not differ much between our regions of interest, making it unlikely that a given trade shock affects the regions differently.[41] Second, we find very limited evidence that the reform effects on firms were larger for firms that export more (see Panel C of Table 6 and Figure A.2 in the appendix).[42] Third, our firm and labor market results tend to be even stronger if we exclude all two-digit industries that were directly affected by one of the other bilateral agreements (see Panel D of Table A.7 and Panel F of Table A.12).[43] Finally, Ruffner and Siegenthaler (2017) present the results of an identification strategy that does not rely on the comparison of firms that differ in their commuting distance to the border. Exploiting information on employment of CBW in establishments in 1995 from the BC, we show that the estimated reform effects on establishment size are similar if we only compare establishments that are located within the same municipality, but that differ in whether or not they employed CBW before the reform.

Admittedly, however, it is ultimately impossible to rule out that other unobserved factors also affect outcomes differentially across regions given the relatively long time horizon of our study. While we are confident that such factors are not the main cause of the effects that we find, we may have to

[40] In recent years, a strong real appreciation in the Swiss Franc relative to the Euro has increased cross-border shopping. This phenomenon mostly affects regions close to the border. However, in the period analyzed here, the exchange rate was comparatively constant. In Ruffner and Siegenthaler (2017), we also provide a series of robustness checks that show that our results are similar if we control directly for movements in the real exchange rate on the firm level.

[41] The BC in 2005 levied information on firms' import behavior. According to this data, the share of importers amounted to 27.4% in the high-treatment, 27.7% in the slightly treated, 23.4% in the control group in the BR, and 23.7% in the NBR.

[42] Indeed, we find no effect of the reform on firms' export share in sales if we use the export share as outcome in our baseline regression, suggesting that domestic sales grew in parallel to export sales in treated firms.

[43] We proxy exposure to these agreements using a classification by Bühler et al. (2011) who study how the trade liberalization caused by the bilateral agreements affected plant growth in Switzerland. The authors carefully assess the extent to which a specific two-digit industry was affected by the six other bilateral agreements next to the free movement agreement. The authors assign industries into three categories: not affected, affected, and strongly affected. In the table, we only keep non-affected industries.

be cautious in interpreting the exact effect sizes.

7.6 Moving up the management ladder

A last potential adjustment channel discussed by our theoretical framework suggests that the absorption of CBW and the observed labor market impact may be explained by a reallocation of natives across tasks that enhances their complementarity. As highly educated CBW were primarily employed in technical, scientific, and engineering jobs, highly educated Swiss workers have an incentive to move towards the higher end of managerial jobs (similar as in Peri and Sparber, 2011; Peri et al., 2015b). The findings above raises the possibility that the entry of new firms may have contributed to the scope for specialization and increased demand for managers. Such management positions require knowledge of local culture, laws and norms, and possibly a local network of contacts. Those are typically more accessible to natives than to foreigners.[44]

To analyse whether the reform affected natives' occupational selection we use the Swiss Labor Force Surveys (SLFS) 1996–2009.[45] One question in the SLFS identifies whether workers are in the top executive level ("Direktion/Geschäftsleitung") of a firm. We then estimate whether the free movement policy affected the number of native workers in executive boards. The results, presented in appendix table A.6, indeed suggest their share and numbers increased among highly educated natives. The estimated effect is sizeable: the likelihood for a native to hold a top-tier position increases by 18 percent relative to the pre-reform average. We do not find evidence for an effect on the share of lower educated natives working in top management positions. These findings suggest that high-qualified native workers were

[44] In Beerli and Peri (2018, Appendix Table A10), we show that CBW usually work in the language region where they speak the local language. Thus, language may not *per se* be a significant source of comparative advantage. Institutional and local knowledge, though, can be just as important in creating specialization and comparative advantages.

[45] We impose similar sample restrictions and use similar definitions regarding skill groups, worker characteristics, and geography as in the labor market analysis with the SSES data (see Appendix B.1).

more likely to become top-tier managers, possibly as a consequence of the imperfect substitutability with CBW and the scale, productivity and firm creation effects documented above. For Switzerland and using a very different identification strategy, Basten and Siegenthaler (forthcoming) also find that the immigrant inflows in the 2002–2011 period caused natives to move towards jobs with more managerial tasks.

Table 7: Effect of the free movement policy on wages of highly educated natives in different management ranks

Dependent variable: Average log hourly wages of highly educated natives in management ranks

	All highly	Wage by manag. rank		constant manag.
	educated	high and middle	low and no	rank shares
	(1)	(2)	(3)	(4)
$Free_t \cdot I(d_m \leq 15)$	0.045***	0.054***	0.014	0.033***
	(0.015)	(0.016)	(0.016)	(0.011)
$Free_t \cdot I(15 < d_m \leq 30)$	0.015	0.025*	-0.012	0.007
	(0.012)	(0.014)	(0.014)	(0.014)
Year and area fixed effects	√	√	√	√
Nuts II trend	√	√	√	√

Notes: ***, **, *, denote statistical significance at the 1%, 5% and 10% level, respectively. Robust standard errors, clustered by commuting zone, are given in parentheses. $Free_t$ is one from year 2004 onward. $(d_i \leq x)$ and $(y < d_i \leq z)$ indicate whether a municipality is located less than x travel minutes or between y and z travel minutes from the next border crossing, respectively. Distance interactions with the transition phase are omitted for brevity. Regressions are weighted using the total number of natives in a cell. The sample in column 1 includes all highly educated natives. In column 2 and 3 the sample of highly educated natives is split into those with a high/middle and low/no management rank. In column 4 the average wage in a cell is calculated keeping the share of managers among highly educated natives constant at its 1998 level between 2000-2010. SESS data.

Which part of the wage effect on the highly educated natives can be attributed to a larger share of natives in better-paying top management positions? We look at this question in Table 7 using the SSES, which contains information about the management rank associated to individuals' positions. The first column reproduces our baseline effects on the wage of highly educated native workers in the free movement period. The second and third column show the impact on wages in top management and in non-management positions, respectively. We see that the positive effect on highly educated natives is concentrated in top management positions corroborating that the reform increased the demand for this type of workers. Column 4 shows the wage increase among highly educated if the share of natives in top-management positions is kept constant at the pre-1999 level. This spec-

ification controls for the reallocation of natives towards top management. We see that the effect on the highly treated regions amounts to roughly 70% of that in column 1. This indicates that 30% of the wage growth of high-qualified natives arises because they move to management positions.

8 Conclusion

This study sheds light on the effects of opening the Swiss labor market for European CBW on the number and types of cross-border workers in Switzerland, wages and employment of native workers, and on employment, sales, productivity, innovation, and location decisions of Swiss firms. Empirically, we exploit that the sequential introduction of the free movement of persons affected Swiss regions close to the border earlier and more strongly due to the greater importance of CBW.

We show that the greater availability of CBW produced a progressive and significant increase in their employment in municipalities close to the border, but had very little effect beyond 30 minutes from the border. Despite of this, natives working in municipalities close the border did not experience, on average, any differential change in average log hourly wages, log employment, or log number of full-time equivalents after the liberalization relative to natives in similar municipalities further away from the border. Instead, we find evidence that wages and employment of highly educated natives increased as a consequence of the reform. These positive effects resulted from pushing some of the natives to managerial and high-paying occupations, from stimulating productivity and job growth in incumbent firms, from attracting new firms, and from promoting firms' innovation activities. Most of these effects are most prominent in skill-intensive sectors and firms in need of skilled workers. The permanent nature of the reform and its early announcement may have facilitated that these effects occurred at the same time as the CBW inflow took place, explaining why native workers experienced no deterioration of their labor market situation.

Our results have at least three important insights that future research could extend. First, they highlight the important role of firms in determining the labor market effects of immigration. The policy changes affected the dynamics of capital investment, firm growth and productivity, indicating that firms recognized the opportunities created from a better access to highly skilled CBW. These findings also emphasize the importance of analyzing the "total" rather than the "partial" effects of immigration, which encompass the variety of adjustment mechanisms that we document. Second, our findings corroborate claims of business leaders that opening the border for foreign workers can benefit firms' performance. There has been little systematic research whether and which firms profit from an unrestricted access to foreign workers. Third, our results suggest that the gradual and predictable implementation of the reform may have played a central role in enabling adjustments by firms that allowed absorbing the increased supply of CBW. We encourage further studies that focus on changes in immigration policies to gain insights how immigration policy can foster firms' success without harming the labor market opportunities of native workers.

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Appendix

For Online publication

A The Swiss labor market around the time of the reform

During the first half of the 1990s, Switzerland experienced a prolonged phase of economic stagnation. Employment fell by 3% between 1991 and 1996 and registered unemployment increased to 5% in the mid-1990s. This unemployment rate was high in a historical perspective. Switzerland had official unemployment rates of almost 0% during many years of the post-war era. Consequences of the restructuring process associated with the economic stagnation in the early 1990s were an increasingly human capital-intensive economy and changes in the occupational and industrial structure, leading to an increase in the relative demand for skills (Puhani, 2005).

The macroeconomic situation improved in the late 1990s, with GDP picking up and the official unemployment rate falling below 2% in 1998. In this recovery, Swiss firms increasingly reported that they struggle to find suitable skilled workers. At the same time, the skill mix of new immigrants improved substantially relative to earlier periods (Beerli et al., 2017). The macroeconomic situation worsened when the dot-com bubble burst. Switzerland entered a phase of economic stagnation between 2001 and mid-2003. Unemployment increased to 3.5%.

The stagnation phase ended towards the end of 2003. Switzerland entered a relatively extended boom phase with high GDP growth rates, falling unemployment, and very high employment growth relative to previous years. Even the Great Recession of 2007/2008 left only small marks in Switzerland. After a drop in 2009, the Swiss economy recovered fast and strongly. GDP grew at 3% in 2010, more than offsetting the fall in the year before. Employment growth also picked up substantially in 2010 after a stagnation in 2009.

Overall, the number of employees increased by 15.2% between 2003 and 2013, from 4.2 to 4.8 million persons. A large part of this increase in employment was attributable to increased employment of EU workers. Switzerland's growth in hours worked in this period was remarkable even in international perspective. For instance, Germany, for which the recent surge in employment has been the subject of several studies, had lower employment growth than Switzerland from 2002 to 2013. Remarkably, Switzerland had high employment growth despite solid real wage increases. Siegenthaler et al. (2016) dubbed this phenomenon the Swiss "job miracle".

B Data construction

Table B.1 provides an overview of the data sets, their samples, variables, and unit of analysis, as used in the labor market and the firm-level analysis, respectively.

B.1 Sample construction and variables used for labor market analysis

Swiss Earnings Structure Survey The analysis of the reform effects on immigration and on wages and employment of native workers is based on data from the Swiss Earnings Structure Survey (SESS). The SESS is a stratified random sample of private and public firms with at least 3 full-time equivalents from the manufacturing and service sectors. It is available in even years between 1994 and 2010 and covers between 16.6% (1996) and 50% (2010) of total employment in Switzerland. We restrict the sample and define the key variables as follows:

- *Sample restriction in the SESS*: The sample includes individuals with age between 18 and 65 years working in the private sector with non-missing information on nationality, place of work, education, wages, full-time equivalents, and other basic demographics. We only keep workers employed in private sector firms as the coverage of the public sector is not complete throughout our analysis period.

- *Definition of immigrants and natives*: The group we call resident immigrants hold either an L permit (4 to 12 months) or a B permit (1 to 6 years). *Cross-border workers* hold a G permit. *Natives* are individuals with Swiss nationality, either born in Switzerland or naturalized. The foreign-born individuals with a permanent residence permit (C permit) can be considered as *long-time immigrants*. This group excluded in our analysis, although they could reasonably be considered as native residents. We exclude them because some immigrants are likely to switch from an L or B permit to a C permit within our sample period. As we do not observe these changes in our data, we would have individuals that switch between immigrants and natives within the sample if we included long-time immigrants. Reassuringly, however, our labor market results are very similar if we count long-time immigrants as natives.

- *Construction of real hourly wages and full-time equivalent employment*: The dataset contains the gross monthly wage for each individual worker (in the month of October) in Swiss Francs. This measure includes social transfers, bonuses, and one-twelfth of additional yearly payments. We divide this measure by the number of hours worked in October, and use the consumer price index to deflate it into the *real hourly wage* of an individual worker at 2010 constant prices. When analyzing wage outcomes we trim our sample by excluding individuals with wages above the 99th percentile of real hourly wages in each year. We express *full-time equivalent (FTE) employment* as a fraction of the number of hours worked by a full-time worker, so that one unit is FTE.

- *Assignment to border region and driving time to border*: We use an official crosswalk from the Federal Statistical Office (FSO) to link zip codes of work places of workers in the SESS to municipalities. As the number of municipalities (and zip codes) changed over

time due to mergers, we use the municipality definition in year 2000 as a time-invariant unit. Observations with outdated zip codes that could not be linked (less than 0.3%) were dropped. We allocate municipalities to the *border region* and the *non-border region* as defined below for the firm-level analysis. Similarly, we use driving time to the nearest border crossing calculated for establishments d_i in the business census (BC) averaged at the municipality level as d_m using establishment employment in 1998 as weights.

Swiss Labor Force Survey Since we cannot track individuals across years in the SESS, we use the Swiss Labor Force Survey (SLFS) as a complementary data set to investigate flows in and out of regional employment (see table A.9, A.10 and A.11 in Appendix C). The SLFS is the equivalent of the US Current Population Survey and was conducted in the second quarter of the year in our period of interest. It covers roughly 17'000 individuals (or 0.5% of households) prior to 2002 and about 50'000 (1.5%) from 2002 onward. As information on their municipality of work is available from 1996, we use yearly data between 1996 and 2009. In this period, most individuals were interviewed up to five consecutive years. We consider, however, only individuals' information in two consecutive years as only this sample is of meaningful size. Using information on the labor force status, place of work and other individual and job characteristics in two consecutive years, we can decompose the change in total private sector employment of natives by education group G in municipality m as follows:

$$E^G_{m,t} - E^G_{m,t-1} = IN^G_{m,t-1,t} - OUT^G_{m,t-1,t} \tag{BA.1}$$

Individuals are considered as inflows to local employment in municipality m in year t, (i) if were employed in a municipality located in another segment of the border region (0-15, 15-30, >30 minutes) or the non-border region in $t-1$, (ii) if they were not employed (either unemployed or out of the labor force), in $t-1$ or (iii) if they were not in the sample. The latter group includes all individuals who were not in the SLFS or were in the SLFS but did not belong to group G, had another nationality status than native, did not work in the private sector or had missing values in any of these variables. Outflows of local employment in year $t-1$ to year t are coded analogously.

Using individuals' average survey weight in the SLFS, we compute total group specific employment $E^G_{m,t}$ as well as total flow and their subcomponents. From these components we construct the number of inflow and outflow relative to the previous employment level in municipality m in year $t-1$ for the years 1996-2009, i.e. $IN^G_{m,t-1,t}/E^G_{m,t-1}$ (total inflow in column 1 and subcomponent (i)-(iii) in columns 2–4, respectively, in table A.9 and A.10) and outflows $OUT^G_{m,t-1,t}/E^G_{m,t-1}$ (total inflow in column 5 and subcomponent (i)-(iii) in columns 6–8, respectively, in table A.9 and A.10). In appendix table A.11 we further breakdown the component (ii) into individuals moving in from employment from the border region above 30 minutes travel time in column 1, the non-border region in column 2 or outflows to those two destinations in column 3 and 4, respectively.

In subsection 7.6, we exploit the rich information in the SLFS to construct the share of workers by education group G working in top executive boards ("Direktion/Geschäftsleitung")

of a firm. Similarly as for the analysis of in- and outflows we only use the years 1996-2009 in which we have information on the municipality of work of individuals.

B.2 Sample construction and variables used for firm-level analysis

Our firm-level estimation are based on the innovation surveys (IS) and the Swiss business censuses (BC). In the IS, the raw data contains answers for 1989, 2172, 2586, 2555, 2141, 2363, and 2034 firms for the seven years of the survey, representing an average response rate of 35%.[46] Moreover, the following comments on the construction of our analyses samples should be mentioned. Ruffner and Siegenthaler (2017) provide extensive sensitivity checks that show that our main results are not sensitive to imposing these sample restrictions:

- *Sample restrictions in the BC*: Our analyses with the BC are based on all firms that participated in the censuses 1991–2011. We exclude establishments from the agricultural sector as well as public sector firms, as these sectors are not covered in the other datasets used in the analysis. Since the censuses do no provide information on the split between foreign and Swiss workers in 1991 and 2011, the results on the foreign employment share are restricted to the 1995–2008 period. Moreover, the BC in 2011 is based on register data. Many variables available for the earlier waves are no longer available because of this change. Consequently, we update certain firm characteristics in 2011 using data from the same establishments in 2008. Our 2011 data thus contain only establishments that were already present in 2008.

- *Deletion of microfirms (BC and IS)*: In both firm-level datasets, we discard firms with less than 3 FTE in 1998 in order to conform with the sample restrictions in the SSES. If the respective firm is not present in 1998, we discard it if it has less than 3 FTE in the first wave that it is observed.

- *Outliers*: In both firm-level datasets, we discard a very small number of extreme outliers that have a strong leverage on the precision (not the point estimate) of the estimates. In the IS, we delete a small number of observations that report to have relocated from one year to another and at the same time report large changes in employment. Closer inspection of these cases revealed that most of them have implausibly large changes in sales and employment in one year. It is likely that some of these cases are due to changes in the reporting unit (e.g. from firm to establishment or vice versa). In the BC, we compute deviations from within-firm means in FTE employment and discard 23 establishments with observations that lie above the 99.9% quantile or below the 0.1% quantile of the distribution of this variable.

- *Assignment of municipalities to border and non-border region*: The border region is classified based on official documents of the Swiss Federal Statistical Office. In cases where no official documents were available, the classification is based on direct information gathered at cantonal statistical offices. The border region is slightly differently classified to

[46] The questionnaires can be downloaded from www.kof.ethz.ch/en/surveys.

previous studies (Losa et al., 2014) in the canton of Valais, based on information provided by the statistical office of the canton of Valais. All municipalities in the region Upper Valais and Lower Valais until Saint-Maurice (St-Gingolph, Port-Valais, Vouvry, Vionnaz, Collombey-Muraz, Monthey, Troistorrents, Val-d'Illiez, Champéry, Massongex, St-Maurice, Mex, Evionnaz, Salvan, Finhaut, Martigny-Combe, Orsières) are classified as border region. The other municipalities in the canton are classified as non-border region. The results are, however, not sensitive to the differential treatment of these municipalities compared to other studies.

- *Computation of distance to nearest border crossing*
  For each unit (establishment or firm), we construct the distance (travel duration) to the nearest border crossing (d_i) in minutes using information on the exact geographic coordinates of each establishment in the BC and based on the zipcode the questionnaire was sent to in the IS. We assign each establishment/firm to the location observed in 1998 (or to the location it is first observed if it is not present in 1998). The data on the location of border crossings in Switzerland necessary to construct d_i come from Henneberger and Ziegler (2011) and refer to the year 2010. We also use the BC 1995 and 1998 to compute an employment-weighted distance to the border for each municipality.

- *Assignment of units to border and non-border region*: In the BC, we assign establishments to the border and non-border region based on the municipality code of each establishment. In the KOF innovation data, we assign firms to the BR and CR depending upon the address the survey was sent to. Because the unit of observation is a firm and not an establishment in the IS, multi-establishment firms are assigned to a treatment or control group based on the location of their headquarters. In both datasets, we exclude a very small number of firms located in municipalities where we could not establish whether they belonged to the BR or CR.

- *Establishment entry and exit* In every wave of the BC, an establishment is considered as a new entrant if its establishment identifier is new. Exiting establishments are those whose identifiers disappears in the next BC wave. There are two reasons why we observe establishments with new establishment identifiers in the BC. The first is the actual creation of a new firm. The second is that a firm is created by a merger of incumbent firms. The former represents the large majority of cases. We count the number of entering and exiting establishments per municipality and BC wave to construct their share relative to the total number of establishments in a municipality in 1998. We analyse the effect on entrants using the years 1991-2008. For exiting establishments we use the years 1991-2011. We cannot use the census in 2011 in the firm entry analysis because this census uses a more encompassing definition of what counts as an establishment compared to the previous censuses. Therefore, many establishment entries between 2008 and 2011 result from the change in the definition, and we cannot identify those.

B.1 Overview of data sets used in labor market and firm analysis

Data source	Coverage and sample	Unit of analysis	Outcome variables	Dimension of heterogeneity	Distance to border
I. Labor market analysis					
Swiss Earnings Structure Survey (SESS)	• 1994–2010 (even years) • employees in private sector establishments • repeated cross-section	municipality	hourly wage, full-time equivalents, number of workers	permit-type, age, education, occupation, industry, management rank	employment weighted mean travel time to next border crossing
Swiss Labor Force Survey (SLFS)	• 1996–2009 (yearly) • employees in private sector establishments • rotating panel	municipality	executive board members, number of workers moving (i) in/out of regions, (ii) in/out of labor force (iii) in/out of sample	permit-type, age, education labor force status	employment weighted mean travel time to next border crossing
II. Establishment- and firm-level analysis					
Business Census (BC)	• 1991, 1995, 1998, 2001, 2005, 2008, 2011 • private sector establishments • panel of universe of establishments	establishment	number of establishments, size of establishments, share of foreign-born and Swiss workforce, entry, exit	industry, establishment size	travel time to next border crossing based on establishment geocode
Innovation Surveys (IS)	• 1996, 1999, 2002, 2005, 2008, 2011, 2013 • private sector establishments • panel of firms	firm	size, sales, export sales, labor productivity, innovation input, innovation output	industry, firm size, export share, pre-reform answers to questions on obstacles to their innovation efforts	travel time to next border crossing based on firm address

Notes: The table gives an overview of the main data sets used in for the labor market and the establishment/firm level analysis.

B.3 Construction of Bartik control

The Bartik control is a proxy for industry-driven local demand shocks. It absorbs local variation in employment or wages (by education group) resulting from national level changes of sectors which are strongly represented in a particular region. In other words, if, for instance, employment in a given industry increased (decreased) nationally, areas in which that industry represented a significant share of employment must have experienced a positive (negative) relative change in the demand for workers relative to those where that industry is not present. The Bartik control is defined at the level of the "commuting zone", which is an aggregation of municipalities often used to represent local labor markets. There are 106 commuting zones in the whole of Switzerland. We define the sector-driven employment growth for group G in a commuting zone cz in year t as:

$$\widetilde{EMP}^{G}_{cz,t} = \sum_{i \in \{1,50\}} \left(EMP^{G}_{i,cz,1994} \times \frac{EMP^{G}_{-cz,i,t}}{EMP^{G}_{-cz,i,1994}} \right) \tag{BA.2}$$

where $EMP^{G}_{i,cz,1994}$ is the employment level of group G (which could be, alternately, all workers or a specific education group of workers) in commuting zone cz and (2-digit) industry i in the earliest available year, 1994. $\frac{EMP^{G}_{-cz,i,t}}{EMP^{G}_{-cz,i,1994}}$ is the group employment growth factor between 1994 and year t for the industry nationally, excluding the commuting zone cz.[47]

When we consider the wage as outcome we use a Bartik measure also based on national wage growth:

$$\widetilde{w_{cz,t}} = \sum_{i \in \{1,50\}} s_{i,cz,1990} \left(w^{G}_{i,cz,1994} \times \frac{w^{G}_{-cz,i,t}}{w^{G}_{-cz,i,1994}} \right) \tag{BA.3}$$

where $w^{G}_{i,cz,1994}$ is the initial log hourly wage payed in (2-digit) industry i for education group G in commuting zone cz in the first available wave in 1994 and $\frac{w^{G}_{-cz,i,t}}{w^{G}_{-cz,i,1994}}$ measures industry wage growth for that group on the national level (excluding commuting zone cz). Wage growth is aggregated using each industry's employment share in 1990 $s_{cz,i,1990}$ taken from the national Census.

C Analysis of worker flows

To interpret the estimates of the reform effects on wages and employment by natives as causal, workers in the control group must not be affected by the inflow of CBW due to the reform. This condition would be violated if native workers responded to the inflow of CBW by moving from treated to the control municipalities or vice versa, hence questioning our assumption that the latter constitute a valid control group.[48]

[47] From the list of industries, we dropped the industry 'Recycling' which was not available in all years.

[48] In the case of flows from the treatment to the control region, employment would increase in the control region and wages would fall, attenuating (overstating) the effects on wage (employment) that the regional comparison in the DiD may detect (see discussion in Dustmann et al. (2017)). The absence of strong negative employment effects on any group of native workers in our case make this particular concern less plausible. However, flows of highly skilled natives in the reverse direction, from the control region to the treatment region as a response to the inflow of CBW, could be consistent with the positive wage and employment effects we find, if effects from human capital externalities outweigh competition

To investigate the importance of such worker flows, we use the Swiss Labor Force Survey, a complementary data set available yearly between 1996 and 2009. Most individuals in the SLFS were interviewed for two consecutive years. We exploit information on each worker's place of work and employment status in the previous year (next year) to total calculate the share of workers flowing into (flowing out) of local employment. We count all workers as flows (i) if they move in from or out to employment in one of the four regional groups to another (BR 0-15min, 15-30min, > 30min, or NBR), (ii) if they move in from or out to non-employment or (iii) if move in from or out of the sample.[49]

The estimates presented in Appendix Table A.9 show no differential changes in total in- or outflows (Column 1 and 4, respectively) or decomposed by origin (Column 2-4) and destination (Column 6-7) in both treatment regions compared to the BR further away than 30 min. Table A.10 shows similar estimates when the NBR is added to the control group. For highly educated natives, there is some reduction in inflows from non-employment, and for lower educated some increase in outflows to other regions. The evidence for *reduced* flows from highly educated non-employed into employment in the highly treated regions is hard to square with our evidence for positive employment effects. Moreover, the significance of these effects depends strongly on the choice of the control groups (Table A.9 vs Table A.10) and, thus, cannot be seen as very robust.

As a last check, we decompose in Table A.11 the workers flows into or out of the two treated regions (0-15min or 15-30min) from and to one of the two control groups, i.e. either the BR further away than 30 minutes (Column 1 and 3) or the NBR (Column 2 and 4).[50] The results show that there is no robust evidence for such worker flows between the treated regions to either of two control groups.

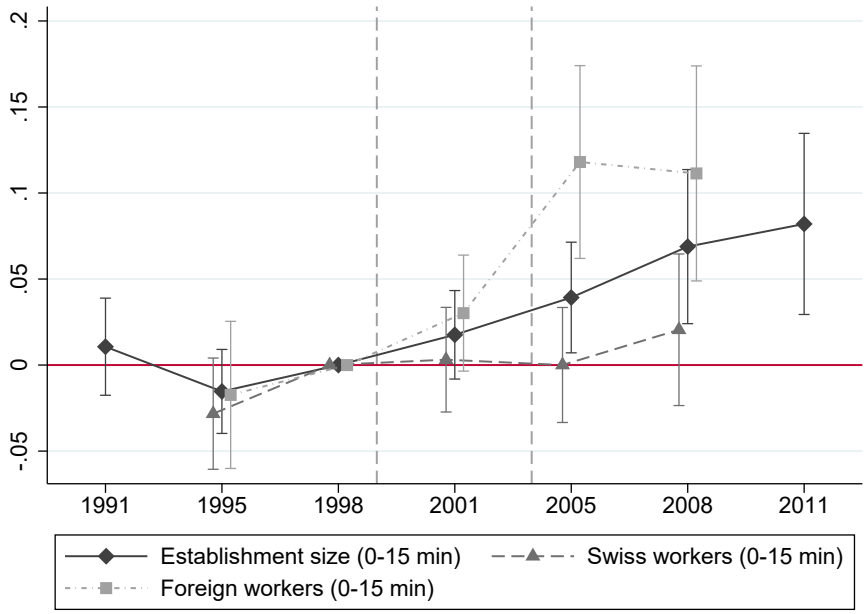
effects among highly skilled (see e.g. Moretti, 2004).

[49]The last category, for instance, includes workers that move to public sector employment, drop out of our age range 18-64, etc. See Appendix B.1 for details on construction of these variables.

[50]Note that when we analyze the flows between the treated regions and the BR +30min, the NBR serves as control group and vice versa when the flows to/from the NBR are analyzed.

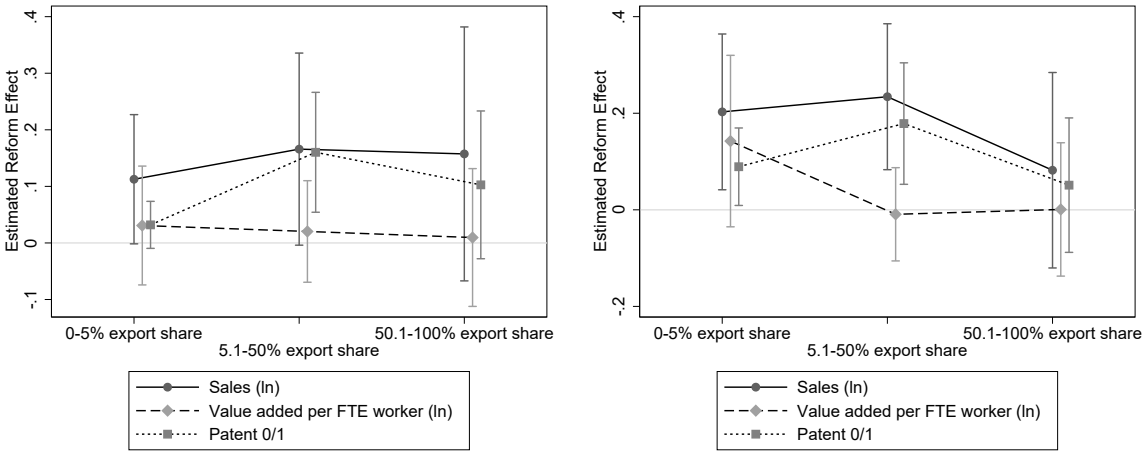
D Appendix figures

Figure A.1: Firm-level effects of free movement policy on total FTE employment and FTE by nationality of workers



Notes: The figure plots the coefficients and the associated 95% confidence intervals for the 0-15 minutes bin of regressions based on equation (3) using private-sector establishment-level data from the BC. The regressions control for establishment fixed effects, year fixed effects, and NUTS-II trends. The sample is restricted to the BR. The dependent variables is log full-time equivalent (FTE) employment and log FTE employment of Swiss nationals and foreigners, respectively. The regressions are weighted using the average establishment size (in FTE). Standard errors are clustered by commuting zone.

Figure A.2: Effect of free movement policy by firms' pre-reform export share



(a) All private sector firms

(b) Manufacturing only

Notes: The figure illustrates regressions based on our baseline regression model (equation (1)), augmented with interactions between our main treatment indicators and indicators based on firms' average export share in sales in the IS 1996 and 1999. The figure shows the coefficients and the associated 95% confidence intervals on the interaction terms between $Free_t \times I(d_i < 15)$ and these indicators. The regressions are based on firm-level data from the IS 1996–2013. We control for firm fixed effects, year fixed effects, and NUTS-II trends. The sample is restricted to the BR. The dependent variables are firms' log total sales, log value added per FTE worker, and the probability to file a patent application in the three years before the survey. Panel A uses our baseline firm sample in the IS. Panel B is restricted to manufacturing. Standard errors are clustered by commuting zone. The two subfigures show that the estimated reform effects are similar between firms with different initial export share. The exception is the patenting effect that is driven by firms with intermediate export share. This, however, results from the fact that the patenting effect is concentrated in manufacturing firms, which in Switzerland are more likely to export than the rest of the firms. If we focus on the manufacturing sector only, the patenting effect has no obvious relationship to firms' export status (panel B).

E Appendix tables

Table A.1: Cross-border workers residing in Switzerland and abroad

	3-Years Average, in Thousands					Average
	1999-2001	**2002-2004**	**2005-2007**	**2008-2010**	**2011-2013**	**Annual Change**
Swiss border workers working in Switzerland and living abroad	NA	NA	10	8	15	0.63
Foreign border workers working in Switzerland and living abroad	144	167	188	221	261	7.81
Swiss border workers working abroad and living in Switzerland	NA	6	9	9	10	0.4
Foreign border workers working abroad and living in Switzerland	NA	5	7	10	13	0.7

Notes: This table provides data on CBW on both sides of the border. In the three-year period from 2002 to 2004, 11,000 CBW living in Switzerland worked in neighboring countries. In the three-year period 2011–2013, the number had increased to 23,000 (+12,000). There were approximately 100,000 additional CBW working in Switzerland but living in neighboring countries in the same period. *Source*: Swiss Federal Statistical Office.

Table A.2: Characteristics of natives and cross-border workers in the border region, 1998 and 2010

	Natives			Cross-border workers		
Panel A: Worker characteristics	1998	2010	Change	1998	2010	Change
Demographic characteristics						
Share highly educated	0.200	0.262	0.062	0.153	0.279	0.126
Share lower educated	0.800	0.738	-0.062	0.847	0.721	-0.126
Share male	0.598	0.544	-0.054	0.693	0.660	-0.033
Mean age	39.7	41.2	1.5	39.7	40.5	0.8
Mean tenure	9.3	8.2	-1.1	9.5	7.2	-2.3
Mean log hourly real wage	3.566	3.598	0.032	3.455	3.534	0.079
Management positions						
Share top management	0.066	0.075	0.009	0.019	0.028	0.009
Share middle management	0.088	0.084	-0.004	0.052	0.063	0.011
Share lower management	0.238	0.205	-0.033	0.189	0.215	0.026
Share no management	0.608	0.636	0.028	0.739	0.694	-0.045
Occupation groups						
Share high-paying	0.242	0.267	0.025	0.159	0.233	0.074
Share middle-paying	0.394	0.365	-0.029	0.244	0.259	0.015
Share low-paying	0.364	0.368	0.004	0.597	0.508	-0.089
Industries						
Agriculture/Fishing/Mining	0.004	0.006	0.002	0.005	0.006	0.001
Manufacturing	0.265	0.205	-0.060	0.461	0.380	-0.081
Utilities	0.007	0.008	0.001	0.001	0.003	0.002
Construction	0.068	0.069	0.001	0.127	0.108	-0.019
Wholesale/Retail/Repair	0.203	0.210	0.007	0.144	0.153	0.009
Hotels/Restaurants	0.037	0.044	0.007	0.055	0.051	-0.004
Transport/Communication/Storage	0.062	0.048	-0.014	0.064	0.054	-0.010
Financial Intermediation	0.107	0.087	-0.020	0.021	0.024	0.003
Real Estate/R&D/IT/Business activities	0.113	0.141	0.028	0.056	0.122	0.066
Education	0.022	0.023	0.001	0.007	0.014	0.007
Health	0.083	0.118	0.035	0.042	0.061	0.019
Personal Services	0.029	0.041	0.012	0.016	0.024	0.008
No. Workers	1,023,236	1,256,986	233,750	103,863	175,206	71,343

Panel B: Relative wage gap natives vs. cross-border workers (2004–2010)		
	Coeff.	S.E.
(i) Municipality and year fixed effects	-0.055	(0.001)
(ii) Year × establishment × occupation fixed effects	-0.031	(0.001)
(iii) Year × establishment × occupation × tenure fixed effects	-0.015	(0.001)

Notes: Panel A compares characteristics and their change of native workers and cross-border workers between 1998 and 2010. Occupations are categorised into the high-, middle- and low-paying according the mean wage in 1998 (see Table A.4). Panel B reports the coefficient (and its standard error) of an identifier for cross-border workers from individual level regressions of the log hourly wage as dependent variable. The sample includes natives and CBW only, uses the years 2004–2010. All regression control for age, age squared, marital status, sex and three education groups (tertiary, secondary, primary or less). Row (i) additionally includes municipality and year fixed effects. Row (ii) further adds year-specific establishment fixed effects interacted with fixed effects for 24 occupations in the SESS. Row (iii) also adds interactions with tenure. The table is based on sample restrictions outlined in 3.1. This is the reason, the numbers of CBW (but not their overall growth) reported in this table deviates from the numbers of CBW taken from the official CBW register which are reported in section 2. SESS data.

Table A.3: Characteristics of workers depending on travel time from the border, 1998

	Border Region			Non-Border-
Travel time to border crossing (minutes)	$0-15$min	$16-30$min	>30min	Region
Demographic characteristics				
Share highly educated	0.178	0.186	0.166	0.148
Share lower educated	0.822	0.814	0.834	0.852
Share male	0.608	0.629	0.614	0.605
Mean age	39.6	39.4	39.1	38.7
Mean tenure	9.3	8.9	8.9	9.1
Mean log hourly real wage	3.505	3.545	3.476	3.447
Industry shares				
Agriculture/Fishing/Mining	0.006	0.003	0.003	0.003
Manufacturing	0.317	0.272	0.299	0.259
Utilities	0.006	0.003	0.01	0.006
Construction	0.094	0.081	0.101	0.122
Wholesale/Retail/Repair	0.175	0.191	0.179	0.226
Hotels/Restaurants	0.047	0.055	0.071	0.081
Transport/Communication/Storage	0.054	0.059	0.05	0.056
Financial Intermediation	0.077	0.11	0.051	0.058
Real Estate/R&D/IT/Business activities	0.106	0.102	0.101	0.076
Education	0.018	0.019	0.025	0.014
Health	0.073	0.074	0.09	0.074
Personal Services	0.03	0.032	0.02	0.027
Mean travel minutes to border	6.61	23.574	37.956	53.081
No. municipalities	522	567	403	874
No. workers	501,660	674,040	287,722	497,469

Notes: Municipalities in the border region are categorized into three bins according to their travel time in minutes from to the next border crossing. SESS data 1998.

Table A.4: Effect on share of total immigrants in occupation groups relative to total employment in 1998

Dependent variable: Number of total immigrants with occupation relative to total employment in 1998

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A. High-paying occupations							
	Define goals & strategy in companies	Logistics, strategy departm.	Review, consult, certify	Invest	R&D	Analyse, program, operating	Plan, Design	Education
$Free_t \cdot I(d_m \leq 15)$	0.004***	0.002***	0.009***	0.002***	0.006**	0.004***	0.001*	0.003***
	(0.001)	(0.000)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
$Free_t \cdot I(15 < d_m \leq 30)$	0.000	0.000*	0.002*	-0.000	0.000	0.001**	0.001**	0.000
	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
	B. Middle-paying occupations							
	Machine Operators	Account-ing, HR	Clerks	Other clerical occ.	Security	Medical, nursing, & social tasks	Cultural, Entertain., Info., Sport	Other
$Free_t \cdot I(d_m \leq 15)$	0.005***	0.004***	0.002***	0.005***	0.001***	0.004***	0.001*	-0.008**
	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)	(0.003)
$Free_t \cdot I(15 < d_m \leq 30)$	0.002**	0.001***	0.001***	0.001*	0.000*	0.001	-0.000	0.001
	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)
	C. Low-paying occupations							
	Manufac-turing	Construction	Craft	Retail Retail	Transport	Manicure, laundary	Cleaning	Restauration
$Free_t \cdot I(d_m \leq 15)$	0.002	0.000	0.000	0.003***	0.001	0.001***	0.002*	0.002
	(0.005)	(0.003)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.004)
$Free_t \cdot I(15 < d_m \leq 30)$	0.003	0.000	-0.000	0.003***	0.001	0.000	0.001	-0.000
	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.004)
Observations	9585	9585	9585	9585	9585	9585	9585	9585
# Clusters	72	72	72	72	72	72	72	72

Notes: ***, **, *, denote statistical significance at the 1%, 5% and 10% level, respectively. Robust standard errors, clustered by commuting zone, are given in parentheses. $Free_t$ is one from year 2004 onward. The coefficients for the transition phase are included but not shown for brevity. $(d_i \leq x)$ and $(y < d_i \leq z)$ indicate whether a municipality is located less than x travel minutes or between y and z travel minutes from the next border crossing, respectively. Regressions are weighted using the total number of workers in 1998. The number of CBW in municipality m and year t is split into 24 different mutually exclusive and exhaustive occupations categories available in the SESS. Workers with missing occupation information are allocated to the category "other occupations". Occupations are categorised into the high-, middle- and low-paying according the mean wage in 1998. SESS data.

Table A.5: Effect of free movement policy on wage and employment of natives by education group

Dependent variable	Mean log hourly wages		Log # workers		Log # FTEs	
	(1)	(2)	(3)	(4)	(5)	(6)
			A. All education groups			
$Transition_t \cdot I(d_i \leq 15)$	0.014	0.005	-0.006	-0.024	0.008	-0.022
	(0.010)	(0.008)	(0.055)	(0.054)	(0.057)	(0.054)
$Transition_t \cdot I(15 < d_i \leq 30)$	0.019**	0.011	0.019	0.001	0.032	0.004
	(0.009)	(0.007)	(0.043)	(0.040)	(0.046)	(0.040)
$Free_t \cdot I(d_i \leq 15)$	-0.002	-0.005	0.037	0.002	0.040	-0.002
	(0.021)	(0.021)	(0.048)	(0.044)	(0.045)	(0.042)
$Free_t \cdot I(15 < d_i \leq 30)$	0.009	0.007	0.052	0.018	0.059	0.019
	(0.006)	(0.006)	(0.038)	(0.031)	(0.039)	(0.032)
Observations	11181	17225	11188	17234	11188	17234
# Clusters	72	106	72	106	72	106
			B. Highly educated			
$Transition_t \cdot I(d_i \leq 15)$	0.028**	0.025**	0.118	0.065	0.132*	0.075
	(0.014)	(0.010)	(0.074)	(0.062)	(0.075)	(0.065)
$Transition_t \cdot I(15 < d_i \leq 30)$	0.020	0.018	0.116*	0.061	0.122*	0.064
	(0.014)	(0.011)	(0.068)	(0.047)	(0.067)	(0.047)
$Free_t \cdot I(d_i \leq 15)$	0.045***	0.046***	0.133**	0.089*	0.163**	0.109**
	(0.015)	(0.013)	(0.065)	(0.049)	(0.064)	(0.050)
$Free_t \cdot I(15 < d_i \leq 30)$	0.015	0.017*	0.160**	0.110**	0.193***	0.137**
	(0.012)	(0.010)	(0.074)	(0.055)	(0.072)	(0.054)
Observations	8383	12764	8415	12805	8415	12805
# Clusters	72	106	72	106	72	106
			C. Lower educated			
$Transition_t \cdot I(d_i \leq 15)$	-0.000	-0.006	-0.040	-0.050	-0.028	-0.049
	(0.010)	(0.007)	(0.055)	(0.055)	(0.056)	(0.054)
$Transition_t \cdot I(15 < d_i \leq 30)$	0.008	0.003	-0.006	-0.014	0.008	-0.012
	(0.009)	(0.006)	(0.047)	(0.046)	(0.048)	(0.045)
$Free_t \cdot I(d_i \leq 15)$	-0.022	-0.023	0.004	-0.028	-0.003	-0.040
	(0.022)	(0.021)	(0.056)	(0.052)	(0.051)	(0.047)
$Free_t \cdot I(15 < d_i \leq 30)$	-0.006	-0.006	0.017	-0.014	0.014	-0.020
	(0.007)	(0.007)	(0.040)	(0.033)	(0.044)	(0.035)
Observations	11045	17016	11049	17021	11049	17021
# Clusters	72	106	72	106	72	106
Including NBR Sample		√		√		√
Year/Area fixed effects	√	√	√	√	√	√
Nuts II trend	√	√	√	√	√	√

Notes: ***, **, *, denote statistical significance at the 1%, 5% and 10% level, respectively. Robust standard errors, clustered by commuting zone, are given in parentheses. In column 1–2, the dependent variable is the mean log hourly real wage by education group. The dependent variable in column 3–4 is the log number of native workers by education group. In column 5–6 it is the log number of full-time equivalents. $Transition_t$ is one for the period between 2000 and 2003, whereas $Free_t$ is one from year 2004 onward. $(d_i \leq x)$ and $(y < d_i \leq z)$ indicate whether a municipality is located less than x travel minutes or between y and z travel minutes from the next border crossing, respectively. Regressions are weighted using the total number of natives in a cell. SESS data.

Table A.6: Effect of the free movement policy on share and number of natives in top tier management

Dependent variable	Share board members		Log (board members)	
			# Workers	# FTEs
	(1)	(2)	(3)	(4)
	A. All education groups			
$Free_t \cdot I(d_i \leq 15)$	0.020	0.027**	0.130	0.140
	(0.014)	(0.013)	(0.091)	(0.097)
$Free_t \cdot I(15 < d_i \leq 30)$	-0.005	0.001	0.066	0.097
	(0.012)	(0.010)	(0.101)	(0.112)
Mean Dep. Var. pre-period	0.218	0.214		
	B. Highly educated			
$Free_t \cdot I(d_i \leq 15)$	0.072*	0.069**	0.230*	0.199
	(0.039)	(0.034)	(0.121)	(0.126)
$Free_t \cdot I(15 < d_i \leq 30)$	-0.003	-0.006	0.109	0.077
	(0.032)	(0.028)	(0.130)	(0.139)
Mean Dep. Var. pre-period	0.386	0.388		
	C. Lower educated			
$Free_t \cdot I(d_i \leq 15)$	-0.006	0.008	-0.038	-0.009
	(0.014)	(0.013)	(0.094)	(0.100)
$Free_t \cdot I(15 < d_i \leq 30)$	-0.015	-0.004	-0.014	0.060
	(0.013)	(0.010)	(0.104)	(0.119)
Mean Dep. Var. pre-period	0.169	0.167		
Including NBR Sample		√		
Year/Area fixed effects	√	√	√	√
Nuts II trend	√	√	√	√

Notes: ***, **, *, denote statistical significance at the 1%, 5% and 10% level, respectively. Robust standard errors, clustered by commuting zone, are given in parentheses. The dependent variable in column 1–2 is the share of native workers who are board members within an education group. In column 3 and 4, the dependent variable is log number of natives board members or their full-time equivalents, respectively, by education group. $Free_t$ is one for municipalities in the border region after 2004. $(d_i \leq x)$ and $(y < d_i \leq z)$ indicate whether a municipality is located less than x travel minutes or between y and z travel minutes from the next border crossing, respectively. Distance interactions with the transition phase are omitted for brevity. Regressions are weighted using the total number of natives in a cell. The share of board members in each panel is computed using the pre-1999 average in each panel. SLFS data 1996-2010.

Table A.7: Main robustness checks for labor market analysis

Dependent variable	# immi-grants	Mean log hourly wages by educ. group			Log # full-time equivalents by educ. group		
	grants	all	high	lower	all	high	lower
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			A. Baseline with Nuts II trends				
$Free_t \cdot I(d_m \leq 15)$	0.056***	-0.002	0.045***	-0.022	0.040	0.163**	-0.003
	(0.014)	(0.021)	(0.015)	(0.022)	(0.045)	(0.064)	(0.051)
$Free_t \cdot I(15 < d_m \leq 30)$	0.022***	0.009	0.015	-0.006	0.059	0.193***	0.014
	(0.008)	(0.006)	(0.012)	(0.007)	(0.039)	(0.072)	(0.044)
Observations	9585	11181	8383	11045	11188	8415	11049
			B. Including Bartik				
$Free_t \cdot I(d_m \leq 15)$	0.056***	0.006	0.045***	-0.017	0.041	0.163**	-0.006
	(0.014)	(0.017)	(0.015)	(0.019)	(0.046)	(0.063)	(0.053)
$Free_t \cdot I(15 < d_m \leq 30)$	0.022***	0.014*	0.016	-0.001	0.059	0.193**	0.011
	(0.008)	(0.008)	(0.012)	(0.007)	(0.038)	(0.073)	(0.043)
Observations	9585	11181	8383	11045	11188	8415	11049
			C. Nuts II regions × year fixed effects				
$Free_t \cdot I(d_m \leq 15)$	0.060***	0.001	0.048***	-0.020	0.037	0.172**	-0.008
	(0.014)	(0.020)	(0.014)	(0.021)	(0.047)	(0.066)	(0.053)
$Free_t \cdot I(15 < d_m \leq 30)$	0.022***	0.008	0.015	-0.006	0.059	0.186**	0.015
	(0.008)	(0.007)	(0.013)	(0.007)	(0.039)	(0.075)	(0.044)
Observations	9585	11181	8383	11045	11188	8415	11049
			D. Excluding industries exposed to bilateral agreements				
$Free_t \cdot I(d_m \leq 15)$	0.044***	-0.001	0.051***	-0.021	0.144*	0.272***	0.107
	(0.015)	(0.020)	(0.013)	(0.019)	(0.084)	(0.089)	(0.100)
$Free_t \cdot I(15 < d_m \leq 30)$	0.020	0.017*	0.020	-0.003	0.105**	0.315***	0.048
	(0.013)	(0.009)	(0.013)	(0.010)	(0.047)	(0.084)	(0.051)
Observations	8802	10308	6896	10138	10315	6928	10140
Year and area fixed effects	√	√	√	√	√	√	√

Notes: ***, **, *, denote statistical significance at the 1%, 5% and 10% level, respectively. Robust standard errors, clustered by commuting zone, are given in parentheses. $Free_t$ is one for municipalities in the border region after 2004. $(d_i \leq x)$ and $(y < d_i \leq z)$ indicate whether a municipality is located less than x travel minutes or between y and z travel minutes from the next border crossing, respectively. Distance interactions with the transition phase are omitted for brevity. Regressions are weighted using the total number of natives in a cell. Panel A repeats estimates from the baseline specification as in Table A.5 including Nuts II regional trends. Panel B adds the Bartik measure, computed separately for wages (Column 2–4) and full-time equivalents (Column 1, 5–7) by education group, as control for sector-driven trends as specified in Appendix B.3. Panel C instead includes full interactions of fixed effects at the level of Nuts II regions and years instead of regional trends. In Panel D the sample includes only two-digit industries that are unaffected by the bilateral agreements according to a classification by Bühler et al. (2011). SESS data.

Table A.8: Labor market results with alternative computation of standard errors

Dependent variable	# immi-grants	Mean log hourly wages by educ. group			Log # full-time equivalents by educ. group		
	grants	all	high	lower	all	high	lower
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			A. Baseline				
$Free_t \cdot I(d_m \leq 15)$	0.056***	-0.002	0.045***	-0.022	0.040	0.163**	-0.003
	(0.014)	(0.021)	(0.015)	(0.022)	(0.045)	(0.064)	(0.051)
$Free_t \cdot I(15 < d_m \leq 30)$	0.022***	0.009	0.015	-0.006	0.059	0.193***	0.014
	(0.008)	(0.006)	(0.012)	(0.007)	(0.039)	(0.072)	(0.044)
Observations	9585	11181	8383	11045	11188	8415	11049
Number of clusters	72						
			B. SE clustered at municipality level				
$Free_t \cdot I(d_m \leq 15)$	0.056***	-0.002	0.045***	-0.022	0.040	0.163**	-0.003
	(0.012)	(0.019)	(0.013)	(0.020)	(0.039)	(0.066)	(0.047)
$Free_t \cdot I(15 < d_m \leq 30)$	0.022***	0.009	0.015	-0.006	0.059*	0.193***	0.014
	(0.009)	(0.007)	(0.011)	(0.007)	(0.034)	(0.060)	(0.037)
Observations	9585	11181	8383	11045	11188	8415	11049
Number of clusters	1065	1464	1271	1459	1464	1273	1459
			C. SE clustered at canton level				
$Free_t \cdot I(d_m \leq 15)$	0.056***	-0.002	0.045**	-0.022	0.040	0.163**	-0.003
	(0.016)	(0.023)	(0.016)	(0.024)	(0.047)	(0.059)	(0.058)
$Free_t \cdot I(15 < d_m \leq 30)$	0.022***	0.009	0.015**	-0.006	0.059	0.193**	0.014
	(0.007)	(0.006)	(0.006)	(0.009)	(0.036)	(0.070)	(0.044)
Observations	9585	11181	8383	11045	11188	8415	11049
Number of clusters	18	18	18	18	18	18	18
			D. SHAC variance				
$Free_t \cdot I(d_m \leq 15)$	0.056***	-0.002	0.045***	-0.022	0.040	0.163***	-0.003
	(0.016)	(0.016)	(0.014)	(0.016)	(0.027)	(0.053)	(0.031)
$Free_t \cdot I(15 < d_m \leq 30)$	0.022***	0.009	0.015**	-0.006	0.059**	0.193***	0.014
	(0.007)	(0.007)	(0.007)	(0.007)	(0.026)	(0.055)	(0.027)
Observations	9585	11181	8383	11045	11188	8415	11049
Year and area fixed effects	√	√	√	√	√	√	√

Notes: ***, **, *, denote statistical significance at the 1%, 5% and 10% level, respectively. $Free_t$ is one for municipalities in the border region after 2004. $(d_i \leq x)$ and $(y < d_i \leq z)$ indicate whether a municipality is located less than x travel minutes or between y and z travel minutes from the next border crossing, respectively. Distance interactions with the transition phase are omitted for brevity. Regressions are weighted using the total number of natives in a cell. Panel A repeats estimates from the baseline specification as in Table A.5 including Nuts II regional trends and with standard errors, clustered by commuting zone. In Panel B and C standard errors are clustered at the level of municipalities and Cantons, respectively. In Panel D, we report standard errors based on the Spatial Heteroscedasticity and Autocorrelation Consistent (SHAC) variance estimator initially proposed by Conley (1999) and recently advanced by Colella et al. (2018). This estimator allows for correlation between areas that are geographically close but belong to different regional units. Following Dustmann et al. (2017), we use a uniform kernel and a bandwidth of 100 kilometers. SESS data.

Table A.9: Effect of free movement policy on inflow and outflow of local employment by natives in the border region

	Employment share of inflow from				Employment share of outflow to			
Dependent variable	all origins	other regions	non-em ployment	out of sample	all desti-nations	other regions	non-em ployment	out of sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				A. All education groups				
$Free_t \cdot I(d_m \leq 15)$	0.014	0.001	0.003	0.009	-0.010	0.008	-0.003	-0.015
	(0.019)	(0.006)	(0.006)	(0.017)	(0.016)	(0.007)	(0.008)	(0.015)
$Free_t \cdot I(15 < d_m \leq 30)$	0.007	0.002	-0.001	0.006	-0.005	0.004	0.001	-0.010
	(0.015)	(0.005)	(0.006)	(0.014)	(0.013)	(0.006)	(0.006)	(0.014)
Mean dep. var.	0.582	0.029	0.044	0.509	0.384	0.021	0.029	0.333
Observations	8094	8094	8094	8094	8094	8094	8094	8094
				B. Highly educated				
$Free_t \cdot I(d_m \leq 15)$	-0.038	-0.022	-0.017**	0.002	-0.016	-0.008	0.006	-0.013
	(0.041)	(0.016)	(0.008)	(0.040)	(0.032)	(0.016)	(0.011)	(0.031)
$Free_t \cdot I(15 < d_m \leq 30)$	-0.051	-0.013	-0.017***	-0.021	-0.022	-0.014	0.007	-0.015
	(0.035)	(0.019)	(0.005)	(0.037)	(0.030)	(0.014)	(0.011)	(0.030)
Mean dep. var.	0.517	0.032	0.015	0.470	0.328	0.018	0.015	0.296
Observations	3420	3420	3420	3420	3420	3420	3420	3420
				C. Lower educated				
$Free_t \cdot I(d_m \leq 15)$	0.029	0.008	0.007	0.015	-0.009	0.012**	-0.006	-0.014
	(0.021)	(0.006)	(0.008)	(0.018)	(0.020)	(0.006)	(0.010)	(0.017)
$Free_t \cdot I(15 < d_m \leq 30)$	0.021	0.005	0.000	0.016	-0.009	0.008	0.000	-0.017
	(0.017)	(0.004)	(0.007)	(0.017)	(0.015)	(0.005)	(0.006)	(0.014)
Mean dep. var.	0.592	0.025	0.047	0.521	0.388	0.018	0.032	0.337
Observations	7254	7254	7254	7254	7254	7254	7254	7254
Year/Area fixed effects	√	√	√	√	√	√	√	√
Nuts II trend	√	√	√	√	√	√	√	√

Notes: ***, **, *, denote statistical significance at the 1%, 5% and 10% level, respectively. Robust standard errors, clustered by commuting zone, are given in parentheses. $Free_t$ is one from year 2004 onward. $(d_i \leq x)$ and $(y < d_i \leq z)$ indicate whether a municipality is located less than x travel minutes or between y and z travel minutes from the next border crossing, respectively. Distance interactions with the transition phase are omitted for brevity. Regressions are weighted using the total number of natives in a cell. In column 1, the dependent variable is total inflows in municipality m in year t (i.e. the number of workers employed who were not employed in the same distance bin or not in the sample in the previous year) relative to the total number of workers in this municipality in $t-1$. Total inflows are decomposed into inflows from employment in other distance bins (column 2), from non-employment (column 3) or from out of the sample (column 4), respectively, relative to total employment in $t-1$. In column 5, the dependent variable is total outflows from municipality m in year $t-1$ (i.e. the number of workers employed who will not be employed in the same distance bin or not in the sample in the next year) relative to the total number of workers in this municipality in $t-1$. Total outflows are decomposed into outflows to employment in other distance bins (column 6), to non-employment (column 7) or out of the sample (column 8), respectively, relative to total employment in $t-1$. See Appendix B.1 for details on the construction of flow variables. The sample only includes municipalities in the border region, i.e. municipalities farther away than 30 minutes constitute the control group. SLFS data 1996-2009.

Table A.10: Effect of free movement policy on inflow and outflow of local employment by natives in the border and non-border region

	Employment share of inflow from				Employment share of outflow to			
Dependent variable	all origins	other regions	non-em ployment	out of sample	all desti-nations	other regions	non-em ployment	out of sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				A. All education groups				
$Free_t \cdot I(d_m \leq 15)$	-0.000	0.001	0.003	-0.004	-0.012	0.010*	-0.006	-0.016
	(0.018)	(0.005)	(0.005)	(0.017)	(0.015)	(0.005)	(0.007)	(0.014)
$Free_t \cdot I(15 < d_m \leq 30)$	-0.005	0.002	-0.002	-0.005	-0.007	0.006	-0.002	-0.011
	(0.014)	(0.004)	(0.005)	(0.013)	(0.011)	(0.004)	(0.004)	(0.012)
Mean dep. var.	0.576	0.026	0.042	0.507	0.377	0.018	0.029	0.331
Observations	12398	12398	12398	12398	12398	12398	12398	12398
				B. Highly educated				
$Free_t \cdot I(d_m \leq 15)$	-0.039	-0.015	-0.013	-0.011	-0.042	-0.001	-0.000	-0.041
	(0.045)	(0.011)	(0.008)	(0.044)	(0.026)	(0.012)	(0.008)	(0.026)
$Free_t \cdot I(15 < d_m \leq 30)$	-0.049	-0.005	-0.011**	-0.032	-0.045*	-0.005	0.001	-0.041
	(0.042)	(0.013)	(0.005)	(0.043)	(0.024)	(0.011)	(0.008)	(0.026)
Mean dep. var.	0.521	0.028	0.014	0.479	0.327	0.017	0.014	0.296
Observations	5043	5043	5043	5043	5043	5043	5043	5043
				C. Lower educated				
$Free_t \cdot I(d_m \leq 15)$	0.007	0.005	0.007	-0.005	-0.002	0.012**	-0.010	-0.005
	(0.019)	(0.005)	(0.007)	(0.017)	(0.017)	(0.005)	(0.009)	(0.016)
$Free_t \cdot I(15 < d_m \leq 30)$	0.002	0.002	-0.000	0.000	-0.002	0.007*	-0.003	-0.007
	(0.015)	(0.004)	(0.006)	(0.016)	(0.012)	(0.004)	(0.005)	(0.012)
Mean dep. var.	0.583	0.023	0.044	0.516	0.382	0.016	0.031	0.335
Observations	11183	11183	11183	11183	11183	11183	11183	11183
Year/Area fixed effects	√	√	√	√	√	√	√	√
Nuts II trend	√	√	√	√	√	√	√	√
Including NBR Sample	√	√	√	√	√	√	√	√

Notes: ***, **, *, denote statistical significance at the 1%, 5% and 10% level, respectively. Robust standard errors, clustered by commuting zone, are given in parentheses. $Free_t$ is one from year 2004 onward. $(d_i \leq x)$ and $(y < d_i \leq z)$ indicate whether a municipality is located less than x travel minutes or between y and z travel minutes from the next border crossing, respectively. Distance interactions with the transition phase are omitted for brevity. Regressions are weighted using the total number of natives in a cell. In column 1, the dependent variable is total inflows in municipality m in year t (i.e. the number of workers employed who were not employed in the same distance bin or not in the sample in the previous year) relative to the total number of workers in this municipality in $t-1$. Total inflows are decomposed into inflows from employment in other distance bins (column 2), from non-employment (column 3) or from out of the sample (column 4), respectively, relative to total employment in $t-1$. In column 5, the dependent variable is total outflows from municipality m in year $t-1$ (i.e. the number of workers employed who will not be employed in the same distance bin or not in the sample in the next year) relative to the total number of workers in this municipality in $t-1$. Total outflows are decomposed into outflows to employment in other distance bins (column 6), to non-employment (column 7) or out of the sample (column 8), respectively, relative to total employment in $t-1$. See Appendix B.1 for details on the construction of flow variables. The sample only includes municipalities in the border region and non-border region, i.e. municipalities farther away than 30 minutes and those in the non-border region constitute the control group. SLFS data 1996-2009.

Table A.11: Effect of free movement policy on inflow and outflow of local employment by natives

Dependent variable	Employment share inflow from		Employment share outflow to	
	BR > 30min	NBR	BR > 30min	NBR
	(1)	(2)	(3)	(4)
	A. All education groups			
$Free_t \cdot I(d_m \leq 15)$	0.001	0.002	0.003	0.004
	(0.002)	(0.004)	(0.003)	(0.004)
$Free_t \cdot I(15 < d_m \leq 30)$	0.002	0.002	0.001	0.003
	(0.002)	(0.003)	(0.003)	(0.003)
Mean dep. var.	0.007	0.006	0.005	0.005
Observations	10334	8094	10334	8094
	B. Highly educated			
$Free_t \cdot I(d_m \leq 15)$	-0.004	-0.014*	0.005	-0.001
	(0.006)	(0.008)	(0.006)	(0.007)
$Free_t \cdot I(15 < d_m \leq 30)$	0.003	-0.012	-0.004	-0.004
	(0.005)	(0.007)	(0.007)	(0.006)
Mean dep. var.	0.007	0.007	0.004	0.004
Observations	4281	3420	4281	3420
	C. Lower educated			
$Free_t \cdot I(d_m \leq 15)$	0.003	0.006	0.004	0.004
	(0.002)	(0.005)	(0.003)	(0.003)
$Free_t \cdot I(15 < d_m \leq 30)$	0.002	0.006	0.003	0.004
	(0.002)	(0.004)	(0.003)	(0.003)
Mean dep. var.	0.007	0.006	0.005	0.005
Observations	9315	7254	9315	7254
Control group: NBR	√		√	
Control group: BR > 30min		√		√
Year/Area fixed effects	√	√	√	√
Nuts II trend	√	√	√	√

Notes: ***, **, *, denote statistical significance at the 1%, 5% and 10% level, respectively. Robust standard errors, clustered by commuting zone, are given in parentheses. $Free_t$ is one from year 2004 onward. $(d_i \leq x)$ and $(y < d_i \leq z)$ indicate whether a municipality is located less than x travel minutes or between y and z travel minutes from the next border crossing, respectively. Distance interactions with the transition phase are omitted for brevity. Regressions are weighted using the total number of natives in a cell. In column 1, the dependent variable is number of workers moving into municipality m in year t who were previously employed in the border region farther away than 30 minutes relative to the total number of workers in this municipality in $t-1$. In column 2 the dependent variable measures inflows from the non-border region. Analogously, the dependent variables are outflows to employment in the border region father than 30 minutes (in Column 3) or the non-border region (in Column 4), respectively. The sample of control municipalities are those in the non-border region (in columns 1 and 3) and those in the border region farther away than 30 minutes (in column 2 and 4). SLFS data 1996-2009.

Table A.12: Main robustness checks for firm-level results

Dependent variable	Establ. size BC	Firm size IS	Sales	Produc-tivity	Patents 0/1
	(1)	(2)	(3)	(4)	(5)
Panel A: Including NBR					
$Free_t * I(d_i \leq 15)$	0.052***	0.060*	0.086**	0.039	0.055**
	(0.018)	(0.034)	(0.039)	(0.031)	(0.022)
$Free_t * I(15 < d_i \leq 30)$	0.047***	0.055	0.019	-0.043	0.005
	(0.014)	(0.034)	(0.032)	(0.034)	(0.017)
Including NBR	√	√	√	√	√
Panel B: Industry period					
$Free_t * I(d_i \leq 15)$	0.052**	0.080*	0.094**	0.034	0.068**
	(0.023)	(0.043)	(0.042)	(0.035)	(0.027)
$Free_t * I(15 < d_i \leq 30)$	0.053***	0.081*	0.040	-0.041	0.011
	(0.017)	(0.045)	(0.040)	(0.038)	(0.025)
Industry-period effects	√	√	√	√	√
Panel C: Nuts-II period					
$Free_t * I(d_i \leq 15)$	0.063***	0.111**	0.121**	0.040	0.065**
	(0.022)	(0.047)	(0.047)	(0.038)	(0.027)
$Free_t * I(15 < d_i \leq 30)$	0.057***	0.102**	0.061	-0.051	0.018
	(0.017)	(0.046)	(0.047)	(0.039)	(0.023)
NUTS-II-period effects	√	√	√	√	√
Panel D: Canton period					
$Free_t * I(d_i \leq 15)$	0.046*	0.172***	0.141**	0.005	0.085**
	(0.027)	(0.061)	(0.067)	(0.055)	(0.033)
$Free_t * I(15 < d_i \leq 30)$	0.038**	0.150***	0.100	-0.065	0.029
	(0.017)	(0.056)	(0.062)	(0.049)	(0.026)
Observations	674,520				
Canton-period effects	√	√	√	√	√
Panel E: Only incumbents					
$Free_t * I(d_i \leq 15)$	0.060***	0.098**	0.122**	0.017	0.071**
	(0.022)	(0.045)	(0.054)	(0.034)	(0.030)
$Free_t * I(15 < d_i \leq 30)$	0.056***	0.099**	0.041	-0.074**	0.021
	(0.018)	(0.043)	(0.043)	(0.037)	(0.025)
Only firms existing in 1998	√	√	√	√	√
Panel F: Unexposed to Bilaterals					
$Free_t * I(d_i \leq 15)$	0.075***	0.100*	0.137**	0.052	0.040*
	(0.027)	(0.059)	(0.061)	(0.039)	(0.024)
$Free_t * I(15 < d_i \leq 30)$	0.070***	0.094	0.085	-0.009	-0.015
	(0.021)	(0.057)	(0.054)	(0.046)	(0.027)
Unexposed to Bilateral Agreements	√	√	√	√	√

Cluster-robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Each panel contains separate regressions of our baseline firm/establishment-level DiD model using the BC (column 1) and the IS (columns 2–5). The regressions account for establishment (BC) or firm (IS) fixed effecs, period fixed effects, and linear trends per NUTS-II region. The dependent variable in column 1 is establishments' log FTE employment. The dependent variable in column 2 is firms' log FTE employment. The dependent variable in column 3 is firms' log total sales. The dependent variable in column 4 is firms' log value added per FTE worker. The dependent variable in column 5 is a dummy equal to 1 if a firm filed at least one patent application in the three years preceding the survey. $Free_t$ is a dummy variable equal to one from year 2004 onward. $I(d_i \leq x)$ and $I(y < d_i \leq z)$ indicate whether a firm is located less than x travel minutes or between y and z travel minutes from the next border crossing, respectively. The variables capturing the transition effects are included in the regression but omitted for brevity. Standard errors are clustered by commuting zone. The estimations in column 1 is weighted using establishments' average size (in FTE). In Panel A, we include firms/establishments from the non-border region into the control group; in Panels B, C, and D, we control for industry-period FE, NUTS-II-period FE, and linear time trends per canton. The regressions in Panel E are restricted to firms/establishments existing in 1998. The regressions in Panel F are restricted to two-digit industries that are unaffected by the bilateral agreements according to a classification by Bühler et al. (2011).

Table A.13: Firm results with alternative standard errors

Dependent variable	FTE (ln, IS)	Sales (ln, IS)	Value added per FTE	Patents 0/1
	(1)	(2)	(3)	(4)
Panel A: Commuting zone				
$Free_t * I(d_i \leq 15)$	0.099**	0.119**	0.036	0.066**
	(0.047)	(0.050)	(0.035)	(0.027)
$Free_t * I(15 < d_i \leq 30)$	0.093*	0.052	-0.046	0.016
	(0.048)	(0.045)	(0.039)	(0.024)
Observations	11,240	10,405	8,935	11,007
Number of clusters	73	73	73	73
Panel B: Firm				
$Free_t * I(d_i \leq 15)$	0.099**	0.119***	0.036	0.066***
	(0.041)	(0.046)	(0.038)	(0.023)
$Free_t * I(15 < d_i \leq 30)$	0.093**	0.052	-0.046	0.016
	(0.039)	(0.041)	(0.037)	(0.022)
Number of clusters	4663	4411	3973	4609
Panel C: Canton				
$Free_t * I(d_i \leq 15)$	0.099**	0.119***	0.036	0.066**
	(0.045)	(0.029)	(0.044)	(0.025)
$Free_t * I(15 < d_i \leq 30)$	0.093**	0.052**	-0.046	0.016
	(0.045)	(0.019)	(0.045)	(0.024)
Number of clusters	23	23	22	23
Panel D: Industry				
$Free_t * I(d_i \leq 15)$	0.099***	0.119***	0.036	0.066***
	(0.037)	(0.040)	(0.039)	(0.023)
$Free_t * I(15 < d_i \leq 30)$	0.093***	0.052	-0.046	0.016
	(0.034)	(0.043)	(0.033)	(0.024)
Number of clusters	48	48	47	48
Panel E: SHAC variance				
$Free_t * I(d_i \leq 15)$	0.099***	0.119***	0.036	0.066***
	(0.024)	(0.015)	(0.029)	(0.018)
$Free_t * I(15 < d_i \leq 30)$	0.093***	0.052	-0.046*	0.016
	(0.031)	(0.033)	(0.028)	(0.015)

Robust standard errors in parentheses

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

Notes: All panels contain separate regressions of our baseline models based on the IS data (see Table 5 for information). In Panel A, standard errors are clustered on the level of commuting zone (our baseline strategy). In Panels B, C, and D standard errors are clustered on the firm, cantonal and two-digit industry (NACE rev. 1.1) level, respectively. In Panel E, we report standard errors based on the Spatial Heteroscedasticity and Autocorrelation Consistent (SHAC) variance estimator proposed by Conley (1999). This estimator allows for correlation between areas that are geographically close but belong to different regional units. Following Dustmann et al. (2017), we use a uniform kernel and a bandwidth of 100 kilometers.

Table A.14: Effect of free movement policy on different innovation outcomes by pre-reform shortage of R&D workers

Dependent variable	R&D inputs			R&D outputs			
	R&D activity 0/1	R&D workers IHS	R&D expend. IHS	Patent appl. 0/1	Process innov. 0/1	Product innov. 0/1	Sales share new/impr. products
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$Free_t * I(d_i \leq 15)$	-0.033	-0.059	-0.605	0.085**	0.011	-0.063	-0.034
	(0.039)	(0.099)	(0.572)	(0.035)	(0.047)	(0.048)	(0.029)
$Free_t * I(d_i \leq 15) * R\&D_i^{short}$	0.111*	0.368**	1.529**	0.072*	-0.031	0.217***	0.106**
	(0.066)	(0.144)	(0.688)	(0.043)	(0.068)	(0.063)	(0.052)
$Free_t * I(15 < d_i \leq 30)$	-0.007	0.121	0.201	0.036	-0.088*	-0.013	-0.030
	(0.037)	(0.086)	(0.493)	(0.030)	(0.047)	(0.033)	(0.028)
$Free_t * I(15 < d_i \leq 30) * R\&D_i^{short}$	0.001	-0.124	-0.556	0.054	0.088	0.060	-0.006
	(0.049)	(0.113)	(0.730)	(0.042)	(0.062)	(0.062)	(0.059)
Observations	4,929	4,444	4,324	4,866	4,947	4,947	2,784
R-squared	0.031	0.026	0.017	0.020	0.051	0.027	0.039
Number of firms	1,543	1,497	1,464	1,540	1,543	1,543	1,235
Firm effects	√	√	√	√	√	√	√
Period effects	√	√	√	√	√	√	√
Nuts II trend	√	√	√	√	√	√	√

Cluster-robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The table presents results of firm-level DiD regressions using the IS 1996–2013. All regressions account for firm fixed effects, period fixed effects, and linear trends per NUTS-II region. The dependent variable in column 1 is a dummy equal to one if a firm reports to have R&D activity. The dependent variables in columns 2 and 3 are the Inverse Hyperbolic Sines (IHS) of the number of R&D workers and R&D expenditures, respectively. The dependent variable in column 4 is a dummy equal to 1 if a firm filed at least one patent application in the three years preceding the survey. The dependent variables in columns 5 and 6 are dummies equal to one if a firm reports to have had process or product innovation in the three years preceding the survey. Process innovation refers to the implementation of a new or significantly improved production or delivery method. A product innovation is defined as the introduction of a good or service that is either new or a substantially improved version of a prior good or service. The dependent variable in column 1 is the firms' sales share of new or significantly improved products. $Free_t$ is a dummy equal to one from year 2004 onward. The variables capturing the transition effects are included in the regression but omitted for brevity. $I(d_i \leq x)$ and $I(y < d_i \leq z)$ indicate whether a firm is located less than x travel minutes or between y and z travel minutes from the next border crossing, respectively. $R\&D_i^{shortage}$ is a dummy equal to 1 if a firm reported substantial problems in finding R&D workers in either one or the two IS in 1996 and 1999 (i.e. if the average of the corresponding original Likert scale survey item is at least 4), or 0 otherwise. Standard errors are clustered by commuting zone.

Table A.15: Effect of free movement policy on establishment entry and exit

Dependent variable	Entry					Exit
		Manufacturing		Services		
Industry category	all	Low-tech	High-tech	Knowl.-intensive	Not-Knowl. intensive	all
	(1)	(2)	(3)	(4)	(5)	(6)
$Transition_t * I(d_{it} \leq 15)$	0.016***	0.013	0.042**	0.018	0.008*	-0.004
	(0.006)	(0.010)	(0.017)	(0.012)	(0.004)	(0.009)
$Transition_t * I(15 < d_{it} \leq 30)$	0.016**	0.006	0.008	0.040***	-0.001	-0.009
	(0.007)	(0.008)	(0.017)	(0.013)	(0.005)	(0.008)
$Free_t * I(d_{it} \leq 15)$	0.037***	0.033***	0.056***	0.048***	0.022***	0.002
	(0.010)	(0.010)	(0.019)	(0.014)	(0.007)	(0.007)
$Free_t * I(15 < d_{it} \leq 30)$	0.026***	0.012	0.020	0.041***	0.017***	-0.003
	(0.005)	(0.009)	(0.012)	(0.009)	(0.004)	(0.005)
Observations	8,157	7,284	5,135	7,602	8,055	9,764
R-squared	0.386	0.136	0.075	0.170	0.342	0.422
Number of municipalities	1,636	1,457	1,027	1,521	1,615	1,636
Municipality effects	√	√	√	√	√	√
Period effects	√	√	√	√	√	√
Nuts-II trend	√	√	√	√	√	√
Number of clusters	73	72	71	73	73	73

Cluster-robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The table studies whether the immigration reform affected establishment entry and exit. All estimations are run at the municipality level using BC data and are restricted to the BR. All regressions account for municipality fixed effects, period fixed effects, and linear trends per NUTS-II region. The dependent variable in columns 1–5 is the number of new establishments in t as a fraction of the number of establishments in 1998 in the sector. The estimation sample is based on the BC 1991–2008. The dependent variable in columns 6 is the number of establishments exiting between $t-1$ and t as a fraction of the number of establishments in 1998 in the sector. The sample is based on the BC 1991–2011 in this case. $Transition_t$ is a dummy equal to one between 1999 and 2003, whereas $Free_t$ is one from year 2004 onward. $I(d_i \leq x)$ and $I(y < d_i \leq z)$ indicate whether a firm is located less than x travel minutes or between y and z travel minutes from the next border crossing, respectively. Regressions are weighted using the municipality-specific number of establishments in the sector in 1998 as the weight. Standard errors are clustered by commuting zone.

Table A.16: The reform and firms' perceptions of policy-related obstacles to innovation

VARIABLES	(1) FE Insufficient external funding	(2) FE Taxes	(3) FE Regulation in domestic market	(4) FE Labor market regulation for foreigners	(5) FE Environm. regulation	(6) FE Construction laws	(7) FE Restricted access to EU	(8) FE Insufficient public support for research	(9) FE Insufficient public support for technology
Panel A: Baseline									
$Transition_t * I(d_i \leq 15)$	-0.048	0.016	-0.018	-0.004	-0.008	0.002	-0.001	0.017	-0.015
	(0.037)	(0.029)	(0.026)	(0.027)	(0.032)	(0.041)	(0.042)	(0.026)	(0.018)
$Transition_t * I(15 < d_i \leq 30)$	-0.050	0.010	0.012	0.012	-0.010	0.017	-0.040	0.038	0.027
	(0.032)	(0.028)	(0.029)	(0.025)	(0.030)	(0.039)	(0.039)	(0.031)	(0.017)
$Free_t * I(d_i \leq 15)$	-0.009	0.026	-0.026	-0.022	0.003	-0.023	0.001	0.024	-0.006
	(0.034)	(0.027)	(0.028)	(0.023)	(0.034)	(0.036)	(0.039)	(0.028)	(0.023)
$Free_t * I(15 < d_i \leq 30)$	0.001	0.031	0.036	-0.016	0.014	-0.026	-0.037	0.034	0.040*
	(0.029)	(0.025)	(0.027)	(0.022)	(0.032)	(0.034)	(0.037)	(0.030)	(0.021)
Observations	9,103	9,102	7,815	7,813	7,813	7,815	7,818	7,274	7,273
Panel B: Including NBR									
$Transition_t * I(d_i \leq 15)$	-0.022	0.004	-0.020	-0.035	0.010	-0.001	0.031	-0.013	-0.028*
	(0.031)	(0.026)	(0.022)	(0.023)	(0.026)	(0.031)	(0.030)	(0.018)	(0.016)
$Transition_t * I(15 < d_i \leq 30)$	-0.024	-0.002	0.010	-0.018	0.008	0.016	-0.009	0.009	0.014
	(0.024)	(0.024)	(0.026)	(0.019)	(0.024)	(0.027)	(0.025)	(0.022)	(0.014)
$Free_t * I(d_i \leq 15)$	0.015	0.002	-0.041*	-0.054***	0.000	0.002	0.033	-0.003	-0.029
	(0.028)	(0.022)	(0.021)	(0.020)	(0.029)	(0.028)	(0.028)	(0.020)	(0.020)
$Free_t * I(15 < d_i \leq 30)$	0.023	0.006	0.021	-0.045**	0.016	0.002	-0.007	0.008	0.018
	(0.022)	(0.020)	(0.021)	(0.018)	(0.026)	(0.025)	(0.023)	(0.021)	(0.017)
Observations	12,610	12,608	10,840	10,839	10,838	10,840	10,843	10,101	10,101

Robust standard errors in parentheses

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

Notes: The table shows regressions of our baseline DiD model. In the IS 1996–2011, firms were asked whether different policy-related factors had a negative effect on their innovation efforts. We built 9 dummy variables from the original Likert-scale survey items. The dependent variables are dummies equal to 1 if a firm gave a value of 4 or 5 to a specific innovation obstacle. Panel A is based on firms in the BR only. Panel B includes firms in the NBR into the control group. All regressions control for firm fixed effects, year fixed effects, and NUTS-II trends. $Transition_t$ is a dummy equal to one between 1999 and 2003, whereas $Free_t$ is one from year 2004 onward. $I(d_i \leq x)$ and $I(y < d_i \leq z)$ indicate whether a firm is located less than x travel minutes or between y and z travel minutes from the next border crossing, respectively. Standard errors are clustered by commuting zone.

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